THE Alexander Ziwet

PHILOSOPHY

OF THE

INDUCTIVE SCIENCES,

FOUNDED UPON THEIR HISTORY.

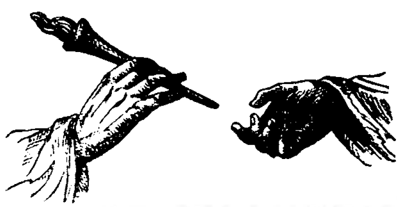
BY WILLIAM WHEWELL, D.D.,

MASTER OF TRINITY COLLEGE, CAMBRIDGE.

A NEW EDITION,

WITH CORRECTIONS AND ADDITIONS, AND
AN APPENDIX, CONTAINING
PHILOSOPHICAL ESSAYS PREVIOUSLY PUBLISHED.

IN TWO VOLUMES.



Λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις.

VOLUME THE FIRST.

LONDON:
JOHN W. PARKER, WEST STRAND.

M.DCCC.XLVII.

TO THE

Rev. ADAM SEDGWICK, M.A.,

SENIOR FELLOW OF TRINITY COLLEGE,

WOODWARDIAN PROFESSOR OF GEOLOGY IN THE UNIVERSITY OF CAMBRIDGE, AND PREBENDARY OF NORWICH.

My dear Sedgwick,

When I showed you the last sheet of my *History of the Inductive Sciences* in its transit through the press, you told me that I ought to add a paragraph or two at the end, by way of Moral to the story; and I replied that the Moral would be as long as the story itself. The present work, the *Moral* which you then desired, I have, with some effort, reduced within a somewhat smaller compass than I then spoke of; and I cannot dedicate it to any one with so much pleasure as to you.

It has always been my wish that, as far and as long as men might know anything of me by my writings, they should hear of me along with the friends with whom I have lived, whom I have loved, and by whose conversation I have been animated to hope that I too might add something to the literature of our country. There is no one whose name has, on such grounds, a better claim than yours to stand in the front of a work, which has been the subject of my labours for no small portion of our long period of friendship. But there is another reason which gives a peculiar propriety to this dedication of my Philosophy to you. I have little doubt that if your life had not been absorbed in struggling with many of the most difficult problems of a difficult science, you would have been my fellow-labourer or master in the work which I have here undertaken. The same spirit which dictated your vigorous protest against some of the errours which I also attempt to expose, would have led you, if your thoughts had been

more free, to take a leading share in that Reform of Phil
which all who are alive to such errours, must see to be i
dispensable. To you I may most justly inscribe a work
contains a criticism of the fallacies of the ultra-Lockian s

I will mention one other reason which enters into the s
tion with which I place your name at the head of my Phil
By doing so, I may consider myself as dedicating it to the
to which we both belong, to which we both owe so much
that we are, and in which we have lived together so long
happily; and *that*, be it remembered, the College of Bacon
Newton. That College, I know, holds a strong place in you
tions, as in mine; and among many reasons, not least
account;—we believe that sound and enduring philosop
finds there a congenial soil and a fostering shelter. If t
trines which the present work contains be really true an
able, my unhesitating trust is, that they will spread gr
from these precincts to every part of the land.

That this office of being the fosterer and diffuser of tr
ever belong to our common Nursing Mother, and that
dear Sedgwick, may long witness and contribute to the
ficial influences, is the hearty wish of

Yours affectionately,

W. WHEW

Trinity College, May 1, 1840.

PREFACE

TO THE

SECOND EDITION.

In the Preface to the first edition of this work, it was stated that the work was intended as an application of the plan of Bacon's *Novum Organon* to the present condition of Physical Science. Such an undertaking, it was there said, plainly belongs to the present generation. Bacon only divined how sciences might be constructed; we can trace, in their history, how their construction has taken place. However sagacious were his conjectures, it may be expected that they will be further illustrated by facts which we know to have really occurred. However large were his anticipations, the actual progress of science since his time may aid in giving comprehensiveness to our views. And with respect to the methods by which science is to be promoted,—the structure and operation of the *Organ* by which truth is to be collected from nature,—we know that, though Bacon's general maxims still guide and animate philosophical enquirers yet that his views, in their detail, have all turned out inapplicable: the technical parts of his method failed in his hands, and are forgotten among the cultivators of science. It cannot be an unfit task, at the present day, to endeavour to extract from the actual past progress of science, the elements of a more effectual and substantial Method of Discovery. The advances have, during the last three centuries, been made physical sciences;—in Astronomy, in Physics, in mistry, in Natural History, in Physiology;—the allowed by all to be real, to be great, to be str may it not be, then, that these steps of progres in them something alike?—that in each advancing ment there is some common process, some commo ciple?—that the organ by which discoveries hav made has had something uniform in its structu working? If this be so, and if we can, by attenc the past history of science, discover something common element and common process in all disco we shall have a *Philosophy of Science*, such as ou may naturally hope for:—we shall have the *New* of Bacon, *renovated* according to our advanced ir tual position and office.

It was with the view to such a continuatio extension of Bacon's design, that I undertook th vey of the *History of Science* which I have gi another work; and that analysis of the advance c science which the present work contains. Of th trines promulgated by Bacon, none has more com remained with us, as a stable and valuable trut his declaration that true knowledge is to be ol from Facts by *Induction:* and in order to denote start at once from the point to which Bacon thus I have, both in the *History* and in the *Philosophy*, the sciences with which I have to do, *the Inducti ences.* By treating of the Physical Sciences only I speak of the Inductive Sciences in the descrip my design, I do not, (as I have already elsewhere said*) intend to deny the character of Inductive Sciences to many other branches of knowledge, as for instance, Ethnology, Glossology, Political Economy, and Psychology. But I think it will be allowed that by taking, as I have done, the Physical Sciences alone, in which the truths established are universally assented to, and regarded with comparative calmness, we are better able to discuss the formal conditions and general processes of scientific discovery, than we could do if we entangled ourselves among subjects where the interest is keener and the truth more controverted. Perhaps a more exact description of the present work would be, *The Philosophy of the Inductive Sciences, founded upon the History of the principal Physical Sciences.*

I am well aware how much additional interest and attractiveness are given to speculations concerning the progress of human knowledge, when we include in them, as examples of such knowledge, views on subjects of politics, morals, beauty in art and literature, and the like. Prominent instances of the effect of this mode of treating such subjects have recently appeared. But I still think that the real value and import of Inductive Philosophy, even in its application to such subjects, are best brought into view by making the progress of political, and moral and *callesthetical*† truth a subject of consideration apart from physical science.

It can hardly happen that a work which treats of Methods of Scientific Discovery shall not seem to fail in

* *Hist. Ind. Sci.* Second Edition. Note to the Introduction.

† See Vol. II. On the Language of Science, Aphorism, XVII.

the positive results which it offers. For an Ar covery is not possible. At each step of the pr science, are needed invention, sagacity, genius;— which no Art can give. We may hope in vain, hoped, for an organ which shall enable all men to c scientific truths, as a pair of compasses enables to construct exact circles*. The practical resul Philosophy of Science must, we are persuaded, b classification and analysis than precept and me think however that the methods of discover I have to recommend, though gathered from survey of scientific history, as to subject an time, than, (so far as I am aware,) has been e attempted, are quite as definite and practical others which have been proposed; with the gre tional advantage of being the methods by which discoveries in physical science really have bee This may be said, for instance, of *the Method of tion*, and *the Method of Natural Classification* of Book XIII. Chap. VIII.; and in a narrower the *Method of Curves, the Method of Means, the of Least Squares*, and the *Method of Residues* of in Chap. VII. of the same Book. Also the on the *Use of Hypotheses* and on the *Tests of Hy* (Book XI. Chap. V.) point out features which n usual course of discovery.

But undoubtedly one of the principal lesson results from the views here given is that sciences may be expected to advance by differen of procedure, according to their present conditi

* *Nov. Org.* Lib. I. Aph. 61.

that, in many of these sciences, an *Induction* performed by any of the methods just referred to, is not the step which we may expect to see next made. Several of the sciences may not be in a condition which fits them for such a *Colligation of Facts*, (to use the phraseology to which the succeeding analysis has led me. See B. XI. C. I). The Facts may, at the present time, require to be more fully observed, or the Idea by which they are to be colligated may require to be more fully unfolded.

But in this point also, our speculations are far from being barren of practical results. The Philosophy of each Science, as given in the present work, affords us means of discerning whether that which is needed for the further progress of the Science has its place in the Observations, or in the Ideas, or in the union of the two. If Observations be wanted, the *Methods of Observation* given in Book XIII. Chap. II. may be referred to; if those who are to make the next discoveries need, for that purpose, a developement of their Ideas, the modes in which such a developement has usually taken place are treated of in Chapters III. and IV. of that Book.

Perhaps one of the most prominent points of this work is the attempt to show the place which discussions concerning Ideas have had in the progress of science. The metaphysical aspect of each of the physical sciences is very far from being, as some have tried to teach, an aspect which it passes through previously to the most decided progress of the science. On the contrary, the metaphysical is a necessary part of the inductive movement. This, which is evidently so by the nature of the

case, is proved by a copious collection of histor
dences in the first ten Books of the present work.
Books contain an account of the principal philo
controversies which have taken place in all the
sciences, from Mathematics to Physiology; an
controversies, which must be called *metaphysica*
thing be so called, have been conducted by the
discoverers in each science, and have been an
part of the discoveries made. Physical discover
differed from barren speculators, not by having
physics in their heads, but by having *good* met
while their adversaries had bad; and by bindi
metaphysics to their physics, instead of keeping
asunder. I trust that the ten Books of which
spoken are of some value, even as a series of an
a number of remarkable controversies; but I can
ceive how any one, after reading these Books,
to see that there is in progressive science a meta
as well as a physical element;—ideas, as well as
thoughts, as well as things:—in short, that the
mental Antithesis, for which I contend, is the
abundantly and strikingly exemplified.

On the subject of this doctrine of a Fun
Analysis, which our knowledge always involve
venture here to add a remark, which looks be
domain of the physical sciences. This doctrine
to throw light upon Moral and Political Philos
less than upon Physical. In Morality, in Legisl
National Polity, we have still to do with the op
and combination of two Elements;—of Facts an
of History, and an Ideal Standard of Action;

character and position, and of the aims which are placed above the Actual. Each of these is in conflict with the other; each modifies and moulds the other. We can never escape the control of the first; we must ever cease to strive to extend the sway of the second. In these cases, indeed, the Ideal Element assumes a new form. It includes the Idea of Duty. The opposition, the action and re-action, the harmony at which we must ever aim, and can never reach, are between what *is* and what *ought* to be;—between the past or present Fact, and the Supreme Idea. The Idea can never be independent of the Fact, but the Fact must ever be drawn towards the Idea. The History of Human Societies, and of each Individual, is by the moral philosopher, regarded in reference to this Antithesis; and thus both Public and Private Morality becomes an actual progress towards an Ideal Form; or ceases to be a moral reality.

I have made very slight alterations in the first edition, except that the First Book is remodelled with a view of bringing out more clearly the basis of the work;—this doctrine of the Fundamental Antithesis of Philosophy. This doctrine, and its relation to the rest of the work, have become more clear in the years which have elapsed since the first edition.

A separate Essay, in which this doctrine was explained, and a few other Essays previously published in various forms, and containing discussions of special points belonging to the scheme of philosophy here delivered, have attracted some notice, both in this and in other countries. I have therefore added them as an Appendix to the present edition.

I have added a few Notes, in answer to arguments brought against particular parts of this work. I have written these in what I have elsewhere called an *impersonal* manner; wishing to avoid controversy, so far as justice to philosophical Truth will allow me to do so.

I have not given any detailed reply to the criticisms of this work which occur in Mr. Mill's *System of Logic.* The consideration of these criticisms would be interesting to me, and I think would still further establish the doctrines which I have here delivered. But such a discussion would involve me in a critique of Mr. Mill's work; which if I were to offer to the world, I should think it more suitable to publish separately.

More than one of my critics has expressed an opinion that when I published this work, I had not given due attention to the *Cours de Philosophie Positive* of M. Comte. I had, and have, an opinion of the value of M. Comte's speculations very different from that entertained by my monitors. I had in the former edition discussed, and, as I conceive, confuted, some of M. Comte's leading doctrines*. In order further to show that I had not lightly passed over those portions of M. Comte's work which had then appeared, I now publish† an additional portion of a critique of the work which, though I had written, I excluded from the former edition. This is printed exactly as it existed in manuscript at the period of that publication. To return to the subject and to take it up in all its extent, would be an undertaking out of the range of a new edition of my published work.

* B. XI. c. vii. B. XIII. c. iv. † B. XII. c. xvi.

Bacon delivered his philosophy in Aphorisms;—a series of Sentences which profess to exhibit rather the results of thought than the process of thinking. A mere Aphoristic Philosophy unsupported by reasoning, is not suited to the present time. No writer upon such subjects can expect to be either understood or assented to, beyond the limits of a narrow school, who is not prepared with good arguments as well as magisterial decisions upon the controverted points of philosophy. But it may be satisfactory to some readers to see the Philosophy, to which in the present work we are led, presented in the Aphoristic form. I have therefore placed a Series of Aphorisms at the end of the work. In the former edition these, by being placed at the beginning of the work, might mislead the reader; seeming to some, perhaps, to be put forwards as the grounds, not as the results, of our philosophy. I have also prefixed an analysis of the work, in the form of a Table of Contents to each volume.

In that part of the second volume which treats of the Language of Science, I have made a few alterations and additions, tending to bring my recommendations into harmony with the present use of the best scientific works.

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PART I.

OF IDEAS.

Quæ adhuc inventa sunt in Scientiis, ea hujusmodi sunt ut Notionibus Vulgaribus fere subjaceant: ut vero ad interiora et remotiora Naturæ penetretur, necesse est ut tam Notiones quam Axiomata magis certâ et munitâ viâ a particularibus abstrahantur; atque omnino melior et certior intellectûs adoperatio in usum veniat.

Bacon, *Nov. Org.*, Lib. i. Aphor. xviii.

BOOK I.

OF IDEAS IN GENERAL.

CHAPTER I.

INTRODUCTION.

THE PHILOSOPHY OF SCIENCE, if the phrase were to be understood in the comprehensive sense which most naturally offers itself to our thoughts, would imply nothing less than a complete insight into the essence and conditions of all real knowledge, and an exposition of the best methods for the discovery of new truths. We must narrow and lower this conception, in order to mould it into a form in which we may make it the immediate object of our labours with a good hope of success; yet still it may be a rational and useful undertaking, to endeavour to make some advance towards such a Philosophy, even according to the most ample conception of it which we can form. The present work has been written with a view of contributing, in some measure, however small it may be, towards such an undertaking.

But in this, as in every attempt to advance beyond the position which we at present occupy, our hope of success must depend mainly upon our being able to profit, to the fullest extent, by the progress already made. We may best hope to understand the nature and conditions of real knowledge, by studying the nature and conditions of the most certain and stable portions of knowledge which we already possess: and we are most likely to learn the best methods of discovering truth, by examining how truths, now universally recognized, have really been discovered. Now there do exist among us doctrines of solid and acknowledged certainty, and truths of which the discovery has been received with universal applause. These constitute what we commonly term *Sciences;* and of these bodies of exact and enduring knowledge, we have within our reach so large and varied a collection, that we may examine them, and the history of their formation, with a good prospect of deriving from the study such instruction as we seek. We may best hope to make some progress towards the Philosophy of Science, by employing ourselves upon THE PHILOSOPHY OF THE SCIENCES.

The *Sciences* to which the name is most commonly and unhesitatingly given, are those which are concerned about the material world; whether they deal with the celestial bodies, as the sun and stars, or the earth and its products, or the elements; whether they consider the differences which prevail among such objects, or their origin, or their mutual operation. And in all these Sciences it is familiarly understood and assumed, that their doctrines are obtained by a common process of collecting general truths from particular observed facts, which process is termed *Induction.* It is further assumed that both in these and in other provinces of knowledge, so long as this process is duly and legitimately performed, the results will be real substantial truth. And although this process, with the conditions under which it is legitimate, and the general laws of the formation of Sciences, will hereafter be subjects of discussion in this work, I shall at present so far adopt the assumption of which I speak, as to give to the Sciences from which our lessons are to be collected the name of *Inductive* Sciences. And thus it is that I am led to designate my work as THE PHILOSOPHY OF THE INDUCTIVE SCIENCES.

The views respecting the nature and progress of knowledge, towards which we shall be directed by such a course of inquiry as I have pointed out, though derived from those portions of human knowledge which are more peculiarly and technically termed *Sciences*, will by no means be confined, in their bearing, to the domain of such Sciences as deal with the material world, nor even to the whole range of Sciences now existing. On the contrary, we shall be led to believe that the nature of truth is in all subjects the same, and that its discovery involves, in all cases, the like conditions. On one subject of human speculation after another, man's knowledge assumes that exact and substantial character which leads us to term it *Science;* and in all these cases, whether inert matter or living bodies, whether permanent relations or successive occurrences, be the subject of our attention, we can point out certain universal characters which belong to truth, certain general laws which have regulated its progress among men. And we naturally expect that, even when we extend our range of speculation wider still, when we contemplate the world within us as well as the world without us, when we consider the thoughts and actions of men as well as the motions and operations of unintelligent bodies, we shall still find some general analogies which belong to the essence of truth, and run through the whole intellectual universe. Hence we have reason to trust that a just Philosophy of the Sciences may throw light upon the nature and extent of our knowledge in every department of human speculation. By considering what is the real import of our acquisitions, where they are certain and definite, we may learn something respecting the difference between true knowledge and its precarious or illusory semblances; by examining the steps by which such acquisitions have been made, we may discover the conditions under which truth is to be obtained; by tracing the boundary-line between our knowledge and our ignorance, we may ascertain in some measure the extent of the powers of man's understanding.

But it may be said, in such a design there is nothing new; these are objects at which inquiring men have often before aimed. To determine the difference between real and imaginary knowledge, the conditions under which we arrive at truth, the range of the powers of the human mind, has been a favourite employment of speculative men from the earliest to the most recent times. To inquire into the original, certainty, and compass of man's knowledge, the limits of his capacity, the strength and weakness of his reason, has been the professed purpose of many of the most conspicuous and valued labours of the philosophers of all periods up to our own day. It may appear, therefore, that there is little necessity to add one more to these numerous essays; and little hope that any new attempt will make any very important addition to the stores of thought upon such questions, which have been accumulated by the profoundest and acutest thinkers of all ages.

To this I reply, that without at all disparaging the value or importance of the labours of those who have previously written respecting the foundations and conditions of human knowledge, it may still be possible to add something to what they have done. The writings of all great philosophers, up to our own time, form a series which is not yet terminated. The books and systems of philosophy which have, each in its own time, won the admiration of men, and exercised a powerful influence upon their thoughts, have had each its own part and functions in the intellectual history of the world; and other labours which shall succeed these may also have their proper office and useful effect. We may not be able to do much, and yet still it may be in our power to effect something. Perhaps the very advances made by former inquirers may have made it possible for us, at present, to advance still further. In the discovery of truth, in the developement of man's mental powers and privileges, each generation has its assigned part; and it is for us to endeavour to perform our portion of this perpetual task of our species. Although the terms which describe our undertaking may be the same which have often been employed by previous writers to express their purpose, yet our position is different from theirs, and thus the result may be different too. We have, as they had, to run our appropriate course of speculation with the exertion of our best powers; but our course lies in a more advanced part of the great line along which Philosophy travels from age to age. However familiar and old, therefore, be the design of such a work as this, the execution may have, and if it be performed in a manner suitable to the time, will have, something that is new and not unimportant.

Indeed, it appears to be absolutely necessary, in order to check the prevalence of grave and pernicious errour, that the doctrines which are taught concerning the foundations of human knowledge and the powers of the human mind, should be from time to time revised and corrected or extended. Erroneous and partial views are promulgated and accepted; one portion of the truth is insisted upon to the undue exclusion of another; or principles true in themselves are exaggerated till they produce on men's minds the effect of falsehood. When evils of this kind have grown to a serious height, a *Reform* is requisite. The faults of the existing systems must be remedied by correcting what is wrong, and supplying what is wanting. In such cases, all the merits and excellencies of the labours of the preceding times do not supersede the necessity of putting forth new views suited to the emergency which has arrived. The new form which errour has assumed makes it proper to endeavour to give a new and corresponding form to truth. Thus the mere progress of time, and the natural growth of opinion from one stage to another, leads to the production of new systems and forms of philosophy. It will be found, I think, that some of the doctrines now most widely prevalent respecting the foundations and nature of truth are of such a kind that a Reform is needed. The present age seems, by many indications, to be called upon to seek a sounder Philosophy of Knowledge than is now current among us. To contribute towards such a Philosophy is the object of the present work. The work is, therefore, like all works which take into account the most recent forms of speculative doctrine, invested with a certain degree of novelty in its aspect and import, by the mere time and circumstances of its appearance.

But, moreover, we can point out a very important peculiarity by which this work is, in its design, distinguished from preceding essays on like subjects; and this difference appears to be of such a kind as may well entitle us to expect some substantial addition to our knowledge as the result of our labours. The peculiarity of which I speak has already been announced;—it is this: that we purpose to collect our doctrines concerning the nature of knowledge, and the best mode of acquiring it, from a contemplation of the Structure and History of those Sciences (the Material Sciences), which are universally recognized as the clearest and surest examples of knowledge and of discovery. It is by surveying and studying the whole mass of such Sciences, and the various steps of their progress, that we now hope to approach to the true Philosophy of Science.

Now this, I venture to say, is a new method of pursuing the philosophy of human knowledge. Those who have hitherto endeavoured to explain the nature of knowledge, and the process of discovery, have, it is true, often illustrated their views by adducing special examples of truths which they conceived to be established, and by referring to the mode of their establishment. But these examples have, for the most part, been taken at random, not selected according to any principle or system. Often they have involved doctrines so precarious or so vague that they confused rather than elucidated the subject; and instead of a single difficulty,— What is the nature of Knowledge? these attempts at illustration introduced two,—What was the true analysis of the Doctrines thus adduced? and,—Whether they might safely be taken as types of real Knowledge?

This has usually been the case when there have been adduced, as standard examples of the formation of human knowledge, doctrines belonging to supposed sciences other than the material sciences; doctrines, for example, of Political Economy, or Philology, or Morals, or the Philosophy of the Fine Arts. I am very far from thinking that, in regard to such subjects, there are no important truths hitherto established: but it would seem that those truths which have been obtained in these provinces of knowledge, have not yet been fixed by means of distinct and permanent phraseology, and sanctioned by universal reception, and formed into a connected system, and traced through the steps of their gradual discovery and establishment, so as to make them instructive examples of the nature and progress of truth in general. Hereafter we trust to be able to show that the progress of moral, and political, and philological, and other knowledge, is governed by the same laws as that of physical science. But since, at present, the former class of subjects are full of controversy, doubt, and obscurity, while the latter consist of undisputed truths clearly understood and expressed, it may be considered a wise procedure to make the latter class of doctrines the basis of our speculations. And on the having taken this course, is, in a great measure, my hope founded, of obtaining valuable truths which have escaped preceding inquirers.

But it may be said that many preceding writers on the nature and progress of knowledge have taken their examples abundantly from the Physical Sciences. It would be easy to point out admirable works, which have appeared during the present and former generations, in which instances of discovery, borrowed from the Physical Sciences, are introduced in a manner most happily instructive. And to the works in which this has been done, I gladly give my most cordial admiration. But at the same time I may venture to remark that there still remains a difference between my design and theirs: and that I use the Physical Sciences as exemplifications of the general progress of knowledge in a manner very materially different from the course which is followed in works such as are now referred to. For the conclusions stated in the present work, respecting knowledge and discovery, are drawn from *a connected and systematic survey of the whole range of Physical Science and its History;* whereas, hitherto, philosophers have contented themselves with adducing detached examples of scientific doctrines, drawn from one or two departments of science. So long as we select our examples in this arbitrary and limited manner, we lose the best part of that philosophical instruction, which the sciences are fitted to afford when we consider them as all members of one series, and as governed by rules which are the same for all. Mathematical and chemical truths, physical and physiological doctrines, the sciences of classification and of causation, must alike be taken into our account, in order that we may learn what are the general characters of real knowledge. When our conclusions assume so comprehensive a shape that they apply to a range of subjects so vast and varied as these, we may feel some confidence that they represent the genuine form of universal and permanent truth. But if our exemplification is of a narrower kind, it may easily cramp and disturb our philosophy. We may, for instance, render our views of truth and its evidence so rigid and confined as to be quite worthless, by founding them too much on the contemplation of mathematical truth. We may overlook some of the most important steps in the general course of discovery, by fixing our attention too exclusively upon some one conspicuous group of discoveries, as, for instance, those of Newton. We may misunderstand the nature of physiological discoveries, by attempting to force an analogy between them and discoveries of mechanical laws, and by not attending to the intermediate sciences which fill up the vast interval between these extreme terms in the series of material sciences. In these and in many other ways, a partial and arbitrary reference to the material sciences in our inquiry into human knowledge may mislead us; or at least may fail to give us those wider views, and that deeper insight, which should result from a systematic study of the whole range of sciences with this particular object.

The design of the following work, then, is to form a Philosophy of Science, by analyzing the substance and examining the progress of the existing body of the sciences. As a preliminary to this undertaking, a survey of the history of the sciences was necessary. This, accordingly, I have already performed; and the result of the labour thus undertaken has been laid before the public as a *History of the Inductive Sciences*.

In that work I have endeavoured to trace the steps by which men acquired each main portion of that knowledge on which they now look with so much confidence and satisfaction. The events which that History relates, the speculations and controversies which are there described, and discussions of the same kind, far more extensive, which are there omitted, must all be taken into our account at present, as the prominent and standard examples of the circumstances which attend the progress of knowledge. With so much of real historical fact before us, we may hope to avoid such views of the processes of the human mind as are too partial and limited, or too vague and loose, or too abstract and unsubstantial, to represent fitly the real forms of discovery and of truth.

Of former attempts, made with the same view of tracing the conditions of the progress of knowledge, that of Bacon is perhaps the most conspicuous: and his labours on this subject were opened by his book on the *Advancement of Learning*, which contains, among other matter, a survey of the then existing state of knowledge. But this review was undertaken rather with the object of ascertaining in what quarters future advances were to be hoped for, than of learning by what means they were to be made. His examination of the domain of human knowledge was conducted rather with the view of discovering what remained undone, than of finding out how so much had been done. Bacon's survey was made for the purpose of tracing the boundaries, rather than of detecting the principles of knowledge. "I will now attempt," he says*, "to make a general and faithful perambulation of learning, with an inquiry what parts thereof lie fresh and waste, and not improved and converted by the industry of man; to the end that such a plot made and recorded to memory, may both minister

* *Advancement of Learning*, b. i. p. 74.

light to any public designation, and also serve to excite voluntary endeavours." Nor will it be foreign to our scheme also hereafter to examine with a like purpose the frontier-line of man's intellectual estate. But the object of our perambulation in the first place, is not so much to determine the extent of the field, as the sources of its fertility. We would learn by what plan and rules of culture, conspiring with the native forces of the bounteous soil, those rich harvests have been produced which fill our garners. Bacon's maxims, on the other hand, respecting the mode in which he conceived that knowledge was thenceforth to be cultivated, have little reference to the failures, still less to the successes, which are recorded in his Review of the learning of his time. His precepts are connected with his historical views in a slight and unessential manner. His Philosophy of the Sciences is not collected from the Sciences which are noticed in his survey. Nor, in truth, could this, at the time when he wrote, have easily been otherwise. At that period, scarce any branch of physics existed as a science, except Astronomy. The rules which Bacon gives for the conduct of scientific researches are obtained, as it were, by divination, from the contemplation of subjects with regard to which no sciences as yet were. His instances of steps rightly or wrongly made in this path, are in a great measure cases of his own devising. He could not have exemplified his Aphorisms by references to treatises then extant, on the laws of nature; for the constant burden of his exhortation is, that men up to his time had almost universally followed an erroneous course. And however we may admire the sagacity with which he pointed the way along a better path, we have this great advantage over him;—that we can interrogate the many travellers who since his time have journeyed on this road. At the present day, when we have under our notice so many sciences, of such wide extent, so well established; a Philosophy of the Sciences ought, it must seem, to be founded, not upon conjecture, but upon an examination of many instances;—should not consist of a few vague and unconnected maxims, difficult and doubtful in their application, but should form a system of which every part has been repeatedly confirmed and verified.

This accordingly it is the purpose of the present work to attempt. But I may further observe, that as my hope of making any progress in this undertaking is founded upon the design of keeping constantly in view the whole result of the past history and present condition of science, I have also been led to draw my lessons from my examples in a manner more systematic and regular, as appears to me, than has been done by preceding writers. Bacon, as I have just said, was led to his maxims for the promotion of knowledge by the sagacity of his own mind, with little or no aid from previous examples. Succeeding philosophers may often have gathered useful instruction from the instances of scientific truths and discoveries which they adduced, but their conclusions were drawn from their instances casually and arbitrarily. They took for their moral any which the story might suggest. But such a proceeding as this cannot suffice for us, whose aim is to obtain a consistent body of philosophy from a contemplation of the whole of Science and its History. For our purpose it is necessary to resolve scientific truths into their conditions and ingredients, in order that we may see in what manner each of these has been and is to be provided, in the cases which we may have to consider. This accordingly is necessarily the first part of our task:—*to analyze Scientific Truth into its Elements.* This attempt will occupy the earlier portion of the present work; and will necessarily be somewhat long, and perhaps, in many parts, abstruse and uninviting. The risk of such an inconvenience is inevitable; for the inquiry brings before us many of the most dark and entangled questions in which men have at any time busied themselves. And even if these can now be made clearer and plainer than of yore, still they can be made so only by means of mental discipline and mental effort. Moreover this analysis of scientific truth into its elements contains much, both in its principles and in its results, different from the doctrines most generally prevalent among us in recent times: but on that very account this analysis is an essential part of the doctrines which I have now to lay before the reader: and I must therefore crave his indulgence towards any portion of it which may appear to him obscure or repulsive.

There is another circumstance which may tend to make the present work less pleasing than others on the same subject, in the nature of the examples of human knowledge to which I confine myself; all my instances being, as I have said, taken from the material sciences. For the truths belonging to these sciences are, for the most part, neither so familiar nor so interesting to the bulk of readers as those doctrines which belong to some other subjects. Every general proposition concerning politics or morals at once stirs up an interest in men's bosoms, which makes them listen with curiosity to the attempts to trace it to its origin and foundation. Every rule of art or language brings before the mind of cultivated men subjects of familiar and agreeable thought, and is dwelt upon with pleasure for its own sake, as well as on account of the philosophical lessons which it may convey. But the curiosity which regards the truths of physics or chemistry, or even of physiology and astronomy, is of a more limited and less animated kind.

Hence, in the mode of inquiry which I have prescribed to myself, the examples which I have to adduce will not amuse and relieve the reader's mind as much as they might do, if I could allow myself to collect them from the whole field of human knowledge. They will have in them nothing to engage his fancy, or to warm his heart. I am compelled to detain the listener in the chilly air of the external world, in order that we may have the advantage of full daylight.

But although I cannot avoid this inconvenience, so far as it is one, I hope it will be recollected how great are the advantages which we obtain by this restriction. We are thus enabled to draw all our conclusions from doctrines which are universally allowed to be eminently certain, clear, and definite. The portions of knowledge to which I refer are well known, and well established among men. Their names are familiar, their assertions uncontested. Astronomy and Geology, Mechanics and Chemistry, Optics and Acoustics, Botany and Physiology, are each recognized as large and substantial collections of undoubted truths. Men are wont to dwell with pride and triumph on the acquisitions of knowledge which have been made in each of these provinces; and to speak with confidence of the certainty of their results. And all can easily learn in what repositories these treasures of human knowledge are to be found. When, therefore, we begin our inquiry from such examples, we proceed upon a solid foundation. With such a clear ground of confidence, we shall not be met with general assertions of the vagueness and uncertainty of human knowledge; with the question, What truth is, and How we are to recognize it; with complaints concerning the hopelessness and unprofitableness of such researches. We have, at least, a definite problem before us. We have to examine the structure and scheme, not of a shapeless mass of incoherent materials, of which we doubt whether it be a ruin or a natural wilderness, but of a fair and lofty palace, still erect and tenanted, where hundreds of different apartments belong to a common plan, where every generation adds something to the extent and magnificence of the pile. The certainty and the constant progress of science are things so unquestioned, that we are at least engaged in an intelligible inquiry, when we are examining the grounds and nature of that certainty, the causes and laws of that progress.

To this enquiry, then, we now proceed. And in entering upon this task, however our plan or our principles may differ from those of the eminent philosophers who have endeavoured, in our own or in former times, to illustrate or enforce the philosophy of science, we most willingly acknowledge them as in many things our leaders and teachers. Each reform must involve its own peculiar principles, and the result of our attempts, so far as they lead to a result, must be, in some respects, different from those of former works. But we may still share with the great writers who have treated this subject before us, their spirit of hope and trust, their reverence for the dignity of the subject, their belief in the vast powers and boundless destiny of man. And we may once more venture to use the words of hopeful exhortation, with which the greatest of those who have trodden this path encouraged himself and his followers when he set out upon his way.

"Concerning ourselves we speak not; but as touching the matter which we have in hand, this we ask;—that men deem it not to be the setting up an Opinion, but the performing of a Work: and that they receive this as a certainty; that we are not laying the foundations of any sect or doctrine, but of the profit and dignity of mankind. Furthermore, that being well disposed to what shall advantage themselves, and putting off factions and prejudices, they take common counsel with us, to the end that being by these our aids and appliances freed and defended from wanderings and impediments, they may lend their hands also to the labours which remain to be performed: and yet further, that they be of good hope; neither imagine to themselves this our Reform as something of infinite dimension, and beyond the grasp of mortal man, when in truth it is the end and true limit of infinite errour; and is by no means unmindful of the condition of mortality and humanity, not confiding that such a thing can be carried to its perfect close in the space of one single age, but assigning it as a task to a succession of generations."

Chapter II.

OF THE FUNDAMENTAL ANTITHESIS OF PHILOSOPHY.

Sect. 1.—*Thoughts and Things.*

In order that we may do something towards determining the nature and conditions of human knowledge, (which I have already stated as the purpose of this work,) I shall have to refer to an antithesis or opposition, which is familiar and generally recognized, and in which the distinction of the things opposed to each other is commonly considered very clear and plain. I shall have to attempt to make this opposition sharper and stronger than it is usually conceived, and yet to shew that the distinction is far from being so clear and definite as it is usually assumed to be: I shall have to point the contrast, yet shew that the things which are contrasted cannot be separated:—I must explain that the antithesis is constant and essential, but yet that there is no fixed and permanent line dividing its members. I may thus appear, in different parts of my discussion, to be proceeding in opposite directions, but I hope that the reader who gives me a patient attention will see that both steps lead to the point of view to which I wish to lead him.

The antithesis or opposition of which I speak is denoted, with various modifications, by various pairs of terms: I shall endeavour to show the connexion of these different modes of expression, and I will begin with that form which is the simplest and most idiomatic.

The simplest and most idiomatic expression of the antithesis to which I refer is that in which we oppose to each other THINGS and THOUGHTS. The opposition is familiar and plain. Our Thoughts are something which belongs to ourselves; something which takes place within us; they are what *we* think; they are actions of our minds. Things, on the contrary, are something different from ourselves and independent of us; something which is without us; they *are;* we see them, touch them, and thus know that they exist; but we do not make them by seeing or touching them, as we make our *Thoughts* by thinking them; we are passive, and *Things* act upon our organs of perception.

Now what I wish especially to remark is this: that in all human KNOWLEDGE both Thoughts and Things are concerned. In every part of my knowledge there must be some *thing* about which I know, and an internal act of *me* who know. Thus, to take simple yet definite parts of our knowledge, if I know that a solar year consists of 365 days, or a lunar month of 30 days, I know something about the sun or the moon; namely, that those objects perform certain revolutions and go through certain changes, in those numbers of days; but I count such numbers and conceive such revolutions and changes by acts of my own thoughts. And both these elements of my knowledge are indispensable. If there were not such external Things as the sun and the moon I could not have any knowledge of the progress of time as marked by them. And however regular were the motions of the sun and moon, if I could not count their appearances and combine their changes into a cycle, or if I could not understand this when done by other men, I could not know anything about a year or a month. In the former case I might be conceived as a human being, possessing the human powers of thinking and reckoning, but kept in a dark world with nothing to mark the progress of existence. The latter is the case of brute animals, which see the sun and moon, but do not know how many days make a month or a year, because they have not human powers of thinking and reckoning.

The two elements which are essential to our knowledge in the above cases, are necessary to human knowledge in all cases. In all cases, Knowledge implies a combination of Thoughts and Things. Without this combination, it would not be Knowledge. Without Thoughts, there could be no connexion; without Things, there could be no reality. Thoughts and Things are so intimately combined in our Knowledge, that we do not look upon them as distinct. One single act of the mind involves them both; and their contrast disappears in their union.

But though Knowledge requires the union of these two elements, Philosophy requires the separation of them, in order that the nature and structure of Knowledge may be seen. Therefore I begin by considering this separation. And I now proceed to speak of another way of looking at the antithesis of which I have spoken;

and which I may, for the reasons which I have just mentioned, call the FUNDAMENTAL ANTITHESIS OF PHILOSOPHY.

SECT. 2.—*Necessary and Experiential Truths.*

MOST persons are familiar with the distinction of *necessary* and *contingent* truths. The former kind are Truths which cannot but be true; as that 19 and 11 make 30;—that parallelograms upon the same base and between the same parallels are equal:—that all the angles in the same segment of a circle are equal. The latter are Truths which *it happens* (*contingit*) are true; but which, for any thing which we can see, might have been otherwise; as that a lunar month contains 30 days, or that the stars revolve in circles round the pole. The latter kind of Truths are learnt by experience, and hence we may call them *Truths of Experience*, or, for the sake of convenience, *Experiential* Truths, in contrast with Necessary Truths.

Geometrical propositions are the most manifest examples of Necessary Truths. All persons who have read and understood the elements of geometry, know that the propositions above stated (that parallelograms upon the same base and between the same parallels are equal; that all the angles in the same segment of a circle are equal,) are necessarily true; not only they *are* true, but they *must be* true. The meaning of the terms being understood, and the proof being gone through, the truth of the propositions must be assented to. We learn these propositions to be true by demonstrations deduced from definitions and axioms; and when we have thus learnt them, we see that they could not be otherwise. In the same manner, the truths which concern numbers are necessary truths: 19 and 11 not only *do* make 30, but *must* make that number, and cannot make anything else.

In the same manner, it is a necessary truth that half the sum of two numbers added to half their difference is equal to the greater number.

It is easy to find examples of Experiential Truths;—propositions which we know to be true, but know by experience only. We know, in this way, that salt will dissolve in water; that plants cannot live without light;—in short, we know in this way all that we do know in chemistry, physiology, and the material sciences in general. I take the *Sciences* as my examples of human knowledge, rather than the common truths of daily life, or moral or political truths; because, though the latter are more generally interesting, the former are much more definite and certain, and therefore better starting-points for our speculations, as I have already said. And we may take elementary astronomical truths as the most familiar examples of Experiential Truths in the domain of science.

With these examples, the distinction of Necessary and Experiential Truths is, I hope, clear. The former kind, we see to be true by thinking about them, and see that they could not be otherwise. The latter kind, men could never have discovered to be true without looking at them; and having so discovered them, still no one will pretend to say they might not have been otherwise. For aught we can see, the astronomical truths which express the motions and periods of the sun, moon and stars, might have been otherwise. If we had been placed in another part of the solar system, our experiential truths respecting days, years, and the motions of the heavenly bodies, would have been other than they are, as we know from astronomy itself.

It is evident that this distinction of Necessary and Experiential Truths involves the same antithesis which we have already considered;—the antithesis of Thoughts and Things. Necessary Truths are derived from our own Thoughts: Experiential Truths are derived from our observation of Things about us. The opposition of Necessary and Experiential Truths is another aspect of the Fundamental Antithesis of Philosophy.

SECT. 3.—*Deduction and Induction.*

I HAVE already stated that geometrical truths are established by demonstrations *deduced* from definitions and axioms. The term *Deduction* is specially applied to such a course of demonstration of truths from definitions and axioms. In the case of the parallelograms upon the same base and between the same parallels, we prove certain triangles to be equal, by supposing them placed so that their two bases have the same extremities; and hence, referring to an Axiom respecting straight lines, we infer that the bases coincide. We combine these equal triangles with other equal spaces, and in this way make up both the one and the other of the parallelograms, in such a manner as to shew that they are equal. In this manner, going on step by step, deducing the equality of the triangles from the axiom, and the equality of the parallelograms from that of the triangles, we travel to the conclusion. And this process of successive deduction is the scheme of all geometrical proof. We begin with Definitions of the notions which we reason about, and with Axioms, or self-evident truths, respecting these notions; and we get, by reasoning from these, other truths which are demonstratively evident; and from these truths again, others of the same kind, and so on. We begin with our own Thoughts, which supply us with Axioms to start from; and we reason from these, till we come to propositions which are applicable to the Things about us; as for instance, the propositions respecting circles and spheres are applicable to the motions of the heavenly bodies. This is *Deduction*, or *Deductive Reasoning.*

Experiential truths are acquired in a very different way. In order to obtain such truths, we begin with Things. In order to learn how many days there are in a year, or in a lunar month, we must begin by observing the sun and the moon. We must observe their changes day by day, and try to make the cycle of change fit into some notion of number which we supply from our own Thoughts. We shall find that a cycle of 30 days *nearly* will fit the changes of phase of the moon;—that a cycle of 365 days nearly will fit the changes of daily motion of the sun. Or, to go on to experiential truths of which the discovery comes within the limits of the history of science—we shall find (as Hipparchus *found*) that the unequal motion of the sun among the stars, such as observation shews it to be, may be fitly represented by the notion of an *eccentric*;—a circle in which the sun has an equable annual motion, the spectator not being in the center of the circle. Again, in the same manner, at a later period, Kepler started from more exact observations of the sun, and compared them with a supposed motion in a certain ellipse; and was able to shew that, not a circle about an eccentric point, but an ellipse, supplied the mode of conception which truly agreed with the motion of the sun about the earth; or rather, as Copernicus had already shewn, of the earth about the sun. In such cases, in which truths are obtained by beginning from observation of external things and by finding some notion with which the Things, as observed, agree, the truths are said to be obtained by *Induction*. The process is an *Inductive Process*.

The contrast of the Deductive and Inductive process is obvious. In the former, we proceed at each step from general truths to particular applications of them;

in the latter, from particular observations to a general truth which includes them. In the former case we may be said to reason *downwards*, in the latter case, *upwards;* for general notions are conceived as standing above particulars. Necessary truths are proved, like arithmetical sums, by adding together the portions of which they consist. An inductive truth is proved, like the guess which answers a riddle, by its agreeing with the facts described. Demonstation is irresistible in its effect on the belief, but does not produce surprize, because all the steps to the conclusion are exhibited, before we arrive at the conclusion. Inductive inference is not demonstrative, but it is often more striking than demonstrative reasoning, because the intermediate links between the particulars and the inference are not shown. Deductive truths are the results of relations among our own Thoughts. Inductive Truths are relations which we discern among existing Things; and thus, this opposition of Deduction and Induction is again an aspect of the Fundamental Antithesis already spoken of.

SECT. 4.—*Theories and Facts.*

GENERAL experiential Truths, such as we have just spoken of, are called *Theories*, and the particular observations from which they are collected, and which they include and explain, are called *Facts*. Thus Hipparchus's doctrine, that the sun moves in an eccentric about the earth, is *his Theory* of the Sun, or the *Eccentric Theory.* The doctrine of Kepler, that the Earth moves in an Ellipse about the Sun, is *Kepler's Theory* of the Earth, the Elliptical Theory. Newton's doctrine that this elliptical motion of the Earth about the Sun is produced and governed by the Sun's attraction upon the Earth, is the *Newtonian* theory, the *Theory of Attraction.* Each of these Theories was accepted, because it included, connected and explained the *Facts*; the Facts being, in the two former cases, the motions of the Sun as observed; and in the other case, the elliptical motion of the Earth as known by Kepler's Theory. This antithesis of *Theory* and *Fact* is included in what has just been said of Inductive Propositions. A Theory is an Inductive Proposition, and the Facts are the particular observations from which, as I have said, such Propositions are inferred by Induction. The Antithesis of Theory and Fact implies the fundamental Antithesis of Thoughts and Things; for a Theory (that is, a true Theory) may be described as a Thought which is contemplated distinct from Things and seen to agree with them; while a Fact is a combination of our Thoughts with Things in so complete agreement that we do not regard them as separate.

Thus the antithesis of Theory and Fact involves the antithesis of Thoughts and Things, but is not identical with it. Facts involve Thoughts, for we know Facts only by thinking about them. The Fact that the year consists of 365 days; the Fact that the month consists of 30 days, cannot be known to us, except we have the Thoughts of Time, Number and Recurrence. But these Thoughts are so familiar, that we have the Fact in our mind as a simple Thing without attending to the Thought which it involves. When we mould our Thoughts into a Theory, we consider the Thought as distinct from the Facts; but yet, though distinct, not independent of them; for it is a true Theory only by including and agreeing with the Facts.

SECT. 5.—*Ideas and Sensations.*

WE have just seen that the antithesis of Theory and Fact, although it involves the antithesis of Thoughts and Things, is not identical with it. There are other modes of expression also, which involve the same Fundamental Antithesis, more or less modified. Of these, the pair of words which in their relations appear to separate the members of the antithesis most distinctly are *Ideas* and *Sensations*. We see and hear and touch external things, and thus perceive them by our senses; but in perceiving them, we connect the impressions of sense according to relations of space, time, number, likeness, cause, &c. Now some at least of these kinds of connexion, as space, time, number, may be contemplated distinct from the things to which they are applied; and so contemplated, I term them *Ideas*. And the other element, the impressions upon our senses which they connect, are called *Sensations*.

I term space, time, cause, &c., *Ideas*, because they are general relations among our sensations, apprehended by an act of the mind, not by the senses simply. These relations involve something beyond what the senses alone could furnish. By the sense of sight we see various shades and colours and shapes before us, but the *outlines* by which they are separated into distinct objects of definite forms, are the work of the mind itself. And again, when we conceive visible things, not only as surfaces of a certain form, but as *solid bodies*, placed at various distances in space, we again exert an act of the mind upon them. When we see a body move, we see it move in a path or *orbit*, but this orbit is not itself seen; it is constructed by the mind. In like manner when we see the motions of a needle towards a magnet, we do not *see* the attraction or force which produces the effects; but we infer the force, by having in our minds the Idea of Cause. Such acts of thought, such *Ideas*, enter into our perceptions of external things.

But though our perceptions of external things involve some act of the mind, they must involve something else besides an act of the mind. If we must exercise an act of thought in order to see force exerted, or orbits described by bodies in motion, or even in order to see bodies existing in space, and to distinguish one kind of object from another, still the act of thought alone does not make the bodies. There must be something besides, *on which* the thought is exerted. A colour, a form, a sound, are not produced by the mind, however they may be moulded, combined, and interpreted by our mental acts. A philosophical poet has spoken of

> All the world
> Of eye and ear, both what they half create,
> And what perceive.

But it is clear, that though they *half* create, they do not wholly create: there must be an external world of colour and sound to give impressions to the eye and ear, as well as internal powers by which we perceive what is offered to our organs. The mind is in some way passive as well as active: there are objects without as well as faculties within;—Sensations, as well as acts of Thought.

Indeed this is so far generally acknowledged, that according to common apprehension, the mind is passive *rather* than active in acquiring the knowledge which it receives concerning the material world. Its sensations are generally considered more distinct than its operations. The world without is held to be more clearly real than the faculties within. That there is something different from ourselves, something external to us, something independent of us, something which no act of our minds can make or can destroy, is held by all men to be at least as evident, as that our minds can exert any effectual process in modifying and appreciating the impressions made upon them. Most persons are more likely to doubt whether the mind be always actively applying Ideas to the objects which it perceives, than whether it perceive them passively by means of Sensations.

But yet a little consideration will show us that an activity of the mind, and an activity according to certain Ideas, is requisite in all our knowledge of external objects. We see objects, of various solid forms, and at various distances from us. But we do not thus perceive them by sensation alone. Our visual impressions cannot, of themselves, convey to us a knowledge of solid form, or of distance from us. Such knowledge is inferred from what we see:—inferred by conceiving the objects as existing in space, and by applying to them the Idea of Space. Again:—day after day passes, till they make up a year: but we do not know that the days are 365, except we count them; and thus apply to them our Idea of Number. Again:—we see a needle drawn to a magnet: but, in truth, the *drawing* is what we cannot see. We see the needle move, and infer the attraction, by applying to the fact our Idea of Force, as the cause of motion. Again:—we see two trees of different kinds; but we cannot know that they are so, except by applying to them our Idea of the resemblance and difference which makes kinds. And thus Ideas, as well as Sensations, necessarily enter into all our knowledge of objects: and these two words express, perhaps more exactly than any of the pairs before mentioned, that Fundamental Antithesis, in the union of which, as I have said, all knowledge consists.

Sect 6.—*Reflexion and Sensation.*

It will hereafter be my business to show what the Ideas are, which thus enter into our knowledge; and how each Idea has been, as a matter of historical fact, introduced into the Science to which it especially belongs. But before I proceed to do this, I will notice some other terms, besides the phrases already noticed; which have a reference, more or less direct, to the Fundamental Antithesis of Ideas and Sensations. I will mention some of these, in order that if they should come under the reader's notice, he may not be perplexed as to their bearing upon the view here presented to him.

The celebrated doctrine of Locke, that all our "Ideas," (that is, in his use of the word, all our objects of thinking,) come from Sensation or Reflexion, will naturally occur to the reader as connected with the antithesis of which I have been speaking. But there is a great difference between Locke's account of Sensation and Reflexion, and our view of Sensation and Ideas. He is speaking of the origin of our knowledge;—we, of its nature and composition. He is content to say that all the knowledge which we do not receive directly by Sensation, we obtain by Reflex Acts of the mind, which make up his Reflexion. But we hold that there is no Sensation without an act of the mind, and that the mind's activity is not only reflexly exerted upon itself, but directly upon objects, so as to perceive in them connexions and relations which are not Sensations. He is content to put together, under the name of Reflexion, everything in our knowledge which is not Sensation: we are to attempt to analyze all that is not Sensation; not only to say it consists of Ideas, but to point out what those Ideas are, and to show the mode in which each of them enters into our knowledge. His purpose was, to prove that there are no Ideas, except the reflex acts of the mind: our endeavour will be to show that the acts of the mind, both direct and reflex, are governed by certain Laws, which may be conveniently termed Ideas. His procedure was, to deny that any knowledge could be derived from the mind alone: our course will be, to show that in every part of our most certain and exact knowledge, those who have added to our knowledge in every age have referred to principles which the mind itself supplies. I do not say that my view is contrary to his: but it is altogether different from his. If I grant that all our knowledge comes from Sensation and Reflexion, still my task then is only begun; for I want further to determine, in each science, what portion comes, not from mere Sensation, but from those Ideas by the aid of which either Sensation or Reflexion can lead to Science.

Locke's use of the word "idea" is, as the reader will perceive, different from ours. He uses the word, as he says, which "serves best to stand for whatsoever is the object of the understanding when a man thinks." "I have used it," he adds, "to express whatever is meant by *phantasm*, *notion*, *species*, or whatever it is to which the mind can be employed about in thinking." It might be shown that this separation of the *mind itself* from the ideal *objects* about which it is employed in thinking, may lead to very erroneous results. But it may suffice to observe that we use the word *Ideas*, in the manner already explained, to express that element, supplied by the mind itself, which must be combined with Sensation in order to produce knowledge. For us, Ideas are not Objects of Thought, but rather Laws of Thought. Ideas are not synonymous with Notions; they are Principles which give to our Notions whatever they contain of truth. But our use of the term *Idea* will be more fully explained hereafter.

SECT. 7—*Subjective and Objective.*

THE Fundamental Antithesis of Philosophy of which I have to speak has been brought into great prominence in the writings of modern German philosophers, and has conspicuously formed the basis of their systems. They have indicated this antithesis by the terms *subjective* and *objective*. According to the technical language of old writers, a thing and its qualities are described as *subject* and *attributes*; and thus a man's faculties and acts are attributes of which he is the *subject*. The mind is the *subject* in which ideas inhere. Moreover, the man's faculties and acts are employed upon external *objects;* and from objects all his sensations arise. Hence the part of a man's knowledge which belongs to his own mind, is *subjective:* that which flows in upon him from the world external to him, is *objective*. And as in man's contemplation of nature, there is always some act of thought which depends upon himself, and some matter of thought which is independent of him, there is, in every part of his knowledge, a subjective and an objective element. The combination of the two elements, the subjective or ideal, and the objective or observed, is necessary, in order to give us any insight into the laws of nature. But different persons, according to their mental habits and constitution, may be inclined to dwell by preference upon the one or the other of these two elements. It may perhaps interest the reader to see this difference of intellectual character illustrated in two eminent men of genius of modern times, Göthe and Schiller.

Göthe himself gives us the account to which I refer, in his history of the progress of his speculations concerning the Metamorphosis of Plants; a mode of viewing their structure by which he explained, in a very striking and beautiful manner, the relations of the different parts of a plant to each other; as has been narrated in the *History of the Inductive Sciences*. Göthe felt a delight in the passive contemplation of nature, unmingled with the desire of reasoning and theorizing; a delight such as naturally belongs to those poets who merely embody the images which a fertile genius suggests, and do not mix with these pictures, judgments and reflexions of their own. Schiller, on the other hand, both by his own strong feeling of the value of a moral purpose in poetry, and by his adoption of a system of metaphysics in which the subjective element was made very prominent, was well disposed to recognize fully the authority of ideas over external impressions.

Göthe for a time felt a degree of estrangement towards Schiller, arising from this contrariety in their views and characters. But on one occasion they fell into discussion on the study of natural history; and Göthe endeavoured to impress upon his companion his persuasion that nature was to be considered, not as composed of detached and incoherent parts, but as active and alive, and unfolding herself in each portion, in virtue of principles which pervade the whole. Schiller objected that no such view of the objects of natural history had been pointed out by observation, the only guide which the natural historians recommended; and was disposed on this account to think the whole of their study narrow and shallow. "Upon this," says Göthe, "I expounded to him, in as lively a way as I could, the metamorphosis of plants, drawing on paper for him, as I proceeded, a diagram to represent that general form of a plant which shows itself in so many and so various transformations. Schiller attended and understood; and, accepting the explanation, he said, 'This is not observation, but an idea.' I replied," adds Göthe, "with some degree of irritation; for the point which separated us was most luminously marked by this expression: but I smothered my vexation, and merely said, 'I was happy to find that I had got ideas without knowing it; nay, that I saw them before my eyes.'" Göthe then goes on to say, that he had been grieved to the very soul by maxims promulgated by Schiller, that no observed fact ever could correspond with an idea. Since he himself loved best to wander in the domain of external observation, he had been led to look with repugnance and hostility upon anything which professed to depend upon ideas. "Yet," he observes, "it occurred to me that if my Observation was identical with his Idea, there must be some common ground on which we might meet." They went on with their mutual explanations, and became intimate and lasting friends. "And thus," adds the poet, "by means of that mighty and interminable controversy between *object* and *subject*, we two concluded an alliance which remained unbroken, and produced much benefit to ourselves and others."

The general diagram of a plant, of which Göthe here speaks, must have been a combination of lines and marks expressing the relations of position and equivalence among the elements of vegetable forms, by which so many of their resemblances and differences may be explained. Such a symbol is not an Idea in that general sense in which we propose to use the term, but is a particular modification of the general Ideas of symmetry, developement, and the like; and we shall hereafter see, according to the phraseology which we shall explain in the next chapter, how such a diagram might express the *ideal conception* of a plant.

The antithesis of *subjective* and *objective* is very familiar in the philosophical literature of Germany and France; nor is it uncommon in any age of our own literature. But though efforts have recently been made to give currency among us to this phraseology, it has not been cordially received, and has been much complained of as not of obvious meaning. Nor is the complaint without ground: for when we regard the mind as the *subject* in which ideas inhere, it becomes for us an *object*, and the antithesis vanishes. We are not so much accustomed to use *subject* in this sense, as to make it a proper contrast to *object*. The combination "*ideal* and *objective*," would more readily convey to a modern reader the opposition which is intended between the ideas of the mind itself, and the objects which it contemplates around it.

To the antitheses already noticed—Thoughts and Things; Necessary and Experiential Truths; Deduction and Induction; Theory and Fact; Ideas and Sensations; Reflexion and Sensation; Subjective and Objective; we may add others, by which distinctions depending more or less upon the fundamental antithesis have been denoted. Thus we speak of the *internal* and *external* sources of our knowledge; of the world *within* and the world *without* us; of *Man* and *Nature*. Some of the more recent metaphysical writers of Germany have divided the universe into the *Me* and the *Not-me* (Ich and Nicht-ich). Upon such phraseology we may observe, that to have the fundamental antithesis of which we speak really understood, is of the highest consequence to philosophy, but that little appears to be gained by expressing it in any novel manner. The most weighty part of the philosopher's task is to analyze the operations of the mind; and in this task, it can aid us but little to call it, instead of the *mind*, the *subject*, or the *me*.

SECT. 8.—*Matter and Form.*

THERE are some other ways of expressing, or rather of illustrating, the fundamental antithesis, which I may briefly notice. The antithesis has been at different times presented by means of various images. One of the most ancient of these, and one which is still very instructive, is that which speaks of Sensations as the *Matter*, and Ideas as the *Form*, of our knowledge; just as ivory is the matter, and a cube the form, of a die. This comparison has the advantage of showing that two elements of an antithesis which cannot be separated in fact, may yet be advantageously separated in our reasonings. For Matter and Form cannot by any means be detached from each other. All matter must have some form; all form must be the form of some material thing. If the ivory be not a cube, it must have a spherical or some other form. And the cube, in order to be a cube, must be of some material;—if not of ivory, of wood, or stone, for instance. A figure without matter is merely a geometrical conception;—a modification of the idea of space. Matter without figure is a mere abstract term; —a supposed union of certain sensible qualities which, so insulated from others, cannot exist. Yet the distinction of Matter and Form is real; and, as a subject of contemplation, clear and plain. Nor is the distinction by any means useless. The speculations which treat of the two subjects, Matter and Figure, are very different. Matter is the subject of the sciences of Mechanics and Chemistry; Figure, of Geometry. These two classes of Sciences have quite different sets of principles. If we refuse to consider the Matter and the Form of bodies separately, because we cannot exhibit Matter and Form separately, we shut the door to all philosophy on such subjects. In like manner, though Sensations and Ideas are necessarily united in all our knowledge, they can be considered as distinct; and this distinction is the basis of all philosophy concerning knowledge.

This illustration of the relation of Ideas and Sensations may enable us to estimate a doctrine which has been put forwards at various times. In a certain school of speculators there has existed a disposition to derive all our Ideas from our Sensations, the term *Idea* being, in this school, used in its wider sense, so as to include all modifications and limitations of our Fundamental Ideas. The doctrines of this school have been summarily expressed by saying that "Every Idea is a transformed Sensation." Now, even supposing this assertion to be exactly true, we easily see, from what has been said, how little we are likely to answer the ends of philosophy by putting forward such a maxim as one of primary importance. For we might say, in like manner, that every statue is but a transformed block of marble, or every edifice but a collection of transformed stones. But what would these assertions avail us, if our object were to trace the rules of art by which beautiful statues were formed, or great works of architecture erected? The question naturally occurs, What is the nature, the principle, the law of this Transformation? In what faculty resides the transforming power? What train of ideas of beauty, and symmetry, and stability, in the mind of the statuary or the architect, has produced those great works which mankind look upon as among their most valuable possessions;—the Apollo of the Belvidere, the Parthenon, the Cathedral of Cologne? When this is what we want to know, how are we helped by learning that the Apollo is of Parian marble, or the Cathedral of basaltic stone? We must know much more than this, in order to acquire any insight into the principles of statuary or of architecture. In like manner, in order that we may make any progress in the philosophy of knowledge, which is our purpose, we must endeavour to learn something further respecting ideas than that they are transformed sensations, even if they were this.

But, in reality, the assertion that our ideas are transformed sensations, is erroneous as well as frivolous. For it conveys, and is intended to convey, the opinion that our sensations have one form which properly belongs to them; and that, in order to become ideas, they are converted into some other form. But the truth is, that our sensations, of themselves, without some act of the mind, such as involves what we have termed an Idea, have no form. We cannot see one object without the idea of space; we cannot see two without the idea of resemblance or difference; and space and difference are not sensations. Thus, if we are to employ the metaphor of Matter and Form, which is implied in the expression to which I have referred, our sensations, from their first reception, have their Form not *changed*, but *given* by our Ideas. Without the relations of thought which we here term *Ideas*, the sensations are matter without form. Matter without form cannot exist: and in like manner sensations cannot become perceptions of objects, without some formative power of the mind. By the very act of being received as perceptions, they have a formative power exercised upon them, the operation of which might be expressed, by speaking of them, not as *transformed*, but simply as *formed*;—as invested with form, instead of being the mere formless material of perception. The word *inform*, according to its Latin etymology, at first implied this process by which matter is invested with form. Thus Virgil* speaks of the thunderbolt as *informed* by the hands of Brontes, and Steropes, and Pyracmon. And Dryden introduces the word in another place:—

> Let others better mould the running mass
> Of metals, or *inform* the breathing brass.

Even in this use of the word, the form is something superior to the brute manner, and gives it a new significance and purpose. And hence the term is again used

* Ferrum exercebant vasto Cyclopes in Antro
Brontesque Steropesque et nudus membra Pyracmon;
His informatum manibus, jam parte polita
Fulmen erat.—*Æn.* viii. 424.

to denote the effect produced by an intelligent principle of a still higher kind:—

> . . . . . . He *informed*
> This ill-shaped body with a daring soul.

And finally even the soul itself, in its original condition, is looked upon as matter, when viewed with reference to education and knowledge, by which it is afterwards moulded; and hence these are, in our language, termed *information*. If we confine ourselves to the first of these three uses of the term, we may correct the erroneous opinion of which we have just been speaking, and retain the metaphor by which it is expressed, by saying, that ideas are not *transformed*, but *informed* sensations.

SECT. 9.—*Man the Interpreter of Nature.*

THERE is another image by which writers have represented the acts of thought through which knowledge is obtained from the observation of the external world. Nature is the Book, and Man is the *Interpreter*. The facts of the external world are marks, in which man discovers a meaning, and so reads them. Man is the Interpreter of Nature, and Science is the right Interpretation. And this image also is, in many respects, instructive. It exhibits to us the necessity of both elements;—the marks which man has to look at, and the knowledge of the alphabet and language which he must possess and apply before he can find any meaning in what he sees. Moreover this image presents to us, as the ideal element, an activity of the mind of that very kind which we wish to point out. Indeed the illustration is rather an example than a comparison of the composition of our knowledge. The letters and symbols which are presented to the Interpreter are really objects of sensation: the notion of letters as signs of words, the notion of connexions among words by which they have meaning, really are among our Ideas;—*Signs* and *Meaning* are Ideas, supplied by the mind, and added to all that sensation can disclose in any collection of visible marks. The Sciences are not figuratively, but really, Interpretations of Nature. But this image, whether taken as example or comparison, may serve to show both the opposite character of the two elements of knowledge, and their necessary combination, in order that there may be knowledge.

This illustration may also serve to explain another point in the conditions of human knowledge which we shall have to notice:—namely, the very different degrees in which, in different cases, we are conscious of the mental act by which our sensations are converted into knowledge. For the same difference occurs in reading an inscription. If the inscription were entire and plain, in a language with which we were familiar, we should be unconscious of any mental act in reading it. We should seem to collect its meaning by the sight alone. But if we had to decipher an ancient inscription, of which only imperfect marks remained, with a few entire letters among them, we should probably make several suppositions as to the mode of reading it, before we found any mode which was quite successful; and thus, our guesses, being separate from the observed facts, and at first not fully in agreement with them, we should be clearly aware that the conjectured meaning, on the one hand, and the observed marks on the other, were distinct things, though these two things would become united as elements of one act of knowledge when we had hit upon the right conjecture.

SECT. 10.—*The Fundamental Antithesis inseparable.*

THE illustration just referred to, as well as other ways of considering the subject, may help us to get over a difficulty which at first sight appears perplexing. We have spoken of the common opposition of *Theory* and *Fact* as important, and as involving what we have called the Fundamental Antithesis of Philosophy. But after all, it may be asked, Is this distinction of Theory and Fact really tenable? Is it not often difficult to say whether a special part of our knowledge is a Fact or a Theory? Is it a Fact or a Theory that the stars revolve round the pole? Is it a Fact or a Theory that the earth is a globe revolving on its axis? Is it a Fact or a Theory that the earth travels in an ellipse round the sun? Is it a Fact or a Theory that the sun attracts the earth? Is it a Fact or a Theory that the loadstone attracts the needle? In all these cases, probably some persons would answer one way, and some persons the other. There are many persons by whom the doctrine of the globular form of the earth, the doctrine of the earth's elliptical orbit, the doctrine of the sun's attraction on the earth, would be called *theories*, even if they allowed them to be true theories. But yet if each of these propositions be true, is it not a *fact?* And even with regard to the simpler facts, as the motion of the stars round the pole, although this may be a Fact to one who has watched and measured the motions of the stars, one who has not done this, and who has only carelessly looked at these stars from time to time, may naturally speak of the circles which the astronomer makes them describe as Theories. It would seem, then, that we cannot in such cases expect general assent, if we say, *This is a Fact and not a Theory*, or, *This is a Theory and not a Fact.* And the same is true in a vast range of cases. It would seem, therefore, that we cannot rest any reasoning upon this distinction of Theory and Fact; and we cannot avoid asking whether there is any real distinction in this antithesis, and if so, what it is.

To this I reply: the distinction between Theory (that is, true Theory) and Fact, is this: that in Theory the Ideas are considered as distinct from the Facts: in Facts, though Ideas may be involved, they are not, in our apprehension, separated from the sensations. In a Fact, the Ideas are applied so readily and familiarly, and incorporated with the sensations so entirely, that we do not see *them*, we see *through them*. A person who carefully notes the motion of a star all night, sees the circle which it describes, as he sees the star, though the circle is, in fact, a result of his own Ideas. A person who has in his mind the measures of different lines and countries on the earth's surface, and who can put them together into one conception, finds that they can make no figure but a globular one: to him, the earth's globular form is a Fact, as much as the square form of his chamber. A person to whom the grounds of believing the earth to travel round the sun are as familiar as the grounds for believing the movements of the mail-coaches in this country, looks upon the former event as a Fact, just as he looks upon the latter events as Facts. And a person who, knowing the Fact of the earth's annual motion, refers it distinctly to its mechanical cause, conceives the sun's attraction as a Fact, just as he conceives as a Fact, the action of the wind which turns the sails of a mill. He cannot *see* the force in either case; he supplies it out of his own Ideas. And thus, a true Theory is a Fact; a Fact is a familiar Theory. That which is a Fact under one aspect, is a Theory under another. The most recondite Theories when firmly established are Facts: the simplest Facts involve something of the nature of Theory. Theory and Fact correspond, in a certain degree, with Ideas and Sensations, as to the nature of their opposition. But the Facts are Facts, so far as the Ideas have been combined with the Sensations and absorbed in them: the Theories are Theories, so far as the Ideas are kept distinct from the Sensations, and so far as it is considered still a question whether those can be made to agree with these.

We may, as I have said, illustrate this matter by considering man as *interpreting* the phenomena which he sees. He often interprets without being aware that he does so. Thus when we see the needle move towards the magnet, we assert that the magnet exercises an attractive force on the needle. But it is only by an interpretative act of our own minds that we ascribe this motion to attraction. That, in this case, a force is exerted—something of the nature of the pull which we could apply by our own volition—is our interpretation of the phenomena; although we may be conscious of the act of interpretation, and may then regard the attraction as a Fact.

Nor is it in such cases only that we interpret phenomena in our own way, without being conscious of what we do. We see a tree at a distance, and judge it to be a chestnut or a lime; yet this is only an inference from the colour or form of the mass according to preconceived classifications of our own. Our lives are full of such unconscious interpretations. The farmer recognizes a good or a bad soil; the artist a picture of a favourite master; the geologist a rock of a known locality, as we recognize the faces and voices of our friends; that is, by judgments formed on what we see and hear; but judgments in which we do not analyze the steps, or distinguish the inference from the appearance. And in these mixtures of observation and inference, we speak of the judgment thus formed, as a Fact directly observed.

Even in the case in which our perceptions appear to be most direct, and least to involve any interpretations of our own,—in the simple process of seeing,—who does not know how much we, by an act of the mind, add to that which our senses receive? Does any one fancy that he sees a solid cube? It is easy to show that the solidity of the figure, the relative position of its faces and edges to each other, are inferences of the spectator; no more conveyed to his conviction by the eye alone, than they would be if he were looking at a painted representation of a cube. The scene of nature is a picture without depth of substance, no less than the scene of art; and in the one case as in the other, it is the mind which, by an act of its own, discovers that colour and shape denote distance and solidity. Most men are unconscious of this perpetual habit of reading the language of the external world, and translating as they read. The draughtsman, indeed, is compelled, for his purposes, to return back in thought from the solid bodies which he has inferred, to the shapes of surface which he really sees. He knows that there is a mask of theory over the whole face of nature, if it be *theory* to infer more than we *see*. But other men, unaware of this masquerade, hold it to be a fact that they see cubes and spheres, spacious apartments and winding avenues. And these things are facts to them, because they are unconscious of the mental operation by which they have penetrated nature's disguise.

And thus, we still have an intelligible distinction of Fact and Theory, if we consider Theory as a conscious, and Fact as an unconscious inference, from the phenomena which are presented to our senses.

But still, Theory and Fact, Inference and Perception, Reasoning and Observation, are antitheses in none of which can we separate the two members by any fixed and definite line.

Even the simplest terms by which the antithesis is expressed cannot be separated. Ideas and Sensations, Thoughts and Things, Subject and Object, cannot in any case be applied absolutely and exclusively. Our Sensations require Ideas to bind them together, namely, Ideas of space, time, number, and the like. If not so bound together, Sensations do not give us any apprehension of Things or Objects. All Things, all Objects, must exist in space and in time—must be one or many. Now space, time, number, are not Sensations or Things. They are something different from, and opposed to Sensations and Things. We have termed them Ideas. It may be said they are *Relations* of Things, or of Sensations. But granting this form of expression, still a *Relation* is not a Thing or a Sensation; and therefore we must still have another and opposite element, along with our Sensations. And yet, though we have thus these two elements in every act of perception, we cannot designate any portion of the act as absolutely and exclusively belonging to one of the elements. Perception involves Sensation, along with Ideas of time, space, and the like; or, if any one prefers the expression, we may say, Perception involves Sensations along with the apprehension of Relations. Perception is Sensation, along with such Ideas as make Sensation into an apprehension of Things or Objects.

And as Perception of Objects implies Ideas,—as Observation implies Reasoning;—so, on the other hand, Ideas cannot exist where Sensation has not been; Reasoning cannot go on when there has not been previous Observation. This is evident from the necessary order of developement of the human faculties. Sensation necessarily exists from the first moments of our existence, and is constantly at work. Observation begins before we can suppose the existence of any Reasoning which is not involved in Observation. Hence, at whatever period we consider our Ideas, we must consider them as having been already engaged in connecting our Sensations, and as having been modified by this employment. By being so employed, our Ideas are unfolded and defined; and such developement and definition cannot be separated from the Ideas themselves. We cannot conceive space, without boundaries or forms; now Forms involve Sensations. We cannot conceive time, without events which mark the course of time; but events involve Sensations. We cannot conceive number, without conceiving things which are numbered; and Things imply sensations. And the forms, things, events, which are thus implied in our Ideas, having been the objects of Sensation constantly in every part of our life, have modified, unfolded, and fixed our Ideas, to an extent which we cannot estimate, but which we must suppose to be essential to the processes which at present go on in our minds. We cannot say that Objects create Ideas; for to perceive Objects we must already have Ideas. But we may say, that Objects and the constant Perception of Objects have so far modified our Ideas, that we cannot, even in thought, separate our Ideas from the perception of Objects.

We cannot say of any Ideas, as of the Idea of space, or time, or number, that they are absolutely and exclusively Ideas. We cannot conceive what space, or time, or number, would be in our minds, if we had never perceived any Thing or Things in space or time. We cannot conceive ourselves in such a condition as never to have perceived any Thing or Things in space or time. But, on the other hand, just as little can we conceive ourselves becoming acquainted with space and time or numbers as objects of Sensation. We cannot reason without having the operations of our minds affected by previous Sensations; but we cannot conceive Reasoning to be merely a series of Sensations. In order to be used in Reasoning, Sensation must become Observation; and, as we have seen, Observation already involves Reasoning. In order to be connected by our Ideas, Sensations must be Things or Objects, and Things or Objects already include Ideas. And thus, none of the terms by which the fundamental antithesis is expressed can be absolutely and exclusively applied.

I will make a remark suggested by the views which have thus been presented. Since, as we have just seen, none of the terms which express the fundamental antithesis can be applied absolutely and exclusively, the absolute application of the antithesis in any particular case can never be a conclusive or immoveable principle. This remark is the more necessary to be borne in mind, as the terms of this antithesis are often used in a vehement and peremptory manner. Thus we are often told that such a thing is *a Fact;* A FACT and not a Theory, with all the emphasis which, in speaking or writing, tone or italics or capitals can give. We see from what has been said, that when this is urged, before we can estimate the truth, or the value of the assertion, we must ask, to whom is it a Fact? what habits of thought, what previous information, what Ideas does it imply, to conceive the Fact as a Fact? Does not the apprehension of the Fact imply assumptions which may with equal justice be called Theory, and which are perhaps false Theory? in which case, the Fact is no Fact. Did not the ancients assert it as a Fact, that the earth stood still, and the stars moved? and can any Fact have stronger apparent evidence to justify persons in asserting it emphatically than this had?

These remarks are by no means urged in order to shew that no Fact can be certainly known to be true; but only, to shew that no Fact can be certainly shown to be a Fact, merely by calling it a Fact, however emphatically. There is by no means any ground of general skepticism with regard to truth, involved in the doctrine of the necessary combination of two elements in all our knowledge. On the contrary, Ideas are requisite to the essence, and Things to the reality of our knowledge in every case. The proportions of Geometry and Arithmetic are examples of knowledge respecting our Ideas of space and number, with regard to which there is no room for doubt. The doctrines of Astronomy are examples of truths not less certain respecting the Facts of the external world.

SECT. 11.—*Successive Generalization.*

IN the preceding pages we have been led to the doctrine, that though, in the Antithesis of Theory and Fact, there is involved an essential opposition; namely the opposition of the thoughts within us and the phenomena without us; yet that we cannot distinguish and define the members of this antithesis separately. Theories become Facts, by becoming certain and familiar: and thus, as our knowledge becomes more sure and more extensive, we are constantly transferring to the class of facts, opinions which were at first regarded as theories.

Now we have further to remark, that in the progress of human knowledge respecting any branch of speculation, there may be *several* such steps in succession, each depending upon and including the preceding. The theoretical views which one generation of discoverers establishes, become the facts from which the next generation advances to new theories. As men rise from the particular to the general, so, in the same manner, they rise from what is general to what is more general. Each induction supplies the materials of fresh inductions; each generalization, with all that it embraces in its circle, may be found to be but one of many circles, comprehended within the circuit of some wider generalization.

This remark has already been made, and illustrated, in the *History of the Inductive Sciences**; and, in truth, the whole of the history of science is full of suggestions and exemplifications of this course of things. It may be convenient, however, to select a few instances which may further explain and confirm this view of the progress of scientific knowledge.

The most conspicuous instance of this succession is to be found in that science which has been progressive from the beginning of the world to our own times, and which exhibits by far the richest collection of successive discoveries: I mean Astronomy. It is easy to see that each of these successive discoveries depended on those antecedently made, and that in each, the truths which were the highest point of the knowledge of one age were the fundamental basis of the efforts of the age which came next. Thus we find, in the days of Greek discovery, Hipparchus and Ptolemy combining and explaining the particular *facts* of the motion of the sun, moon, and planets, by means of the *theory* of epicycles and eccentrics;—a highly important step, which gave an intelligible connexion and rule to the motions of each of these luminaries. When these cycles and epicycles, thus truly representing the apparent motions of the heavenly bodies, had accumulated to an inconvenient amount, by the discovery of many inequalities in the observed motions, Copernicus showed that their effects might all be more simply included, by making the sun the center of motion of the planets, instead of the earth. But in this new view, he still retained the epicycles and eccentrics which governed the motion of each body. Tycho Brahe's observations, and Kepler's calculations,

* *Hist. Inductive Sciences*, B. VII. c. ii. Sect. 5.

showed that, besides the vast number of facts which the epicyclical theory could account for, there were some which it would not exactly include, and Kepler was led to the persuasion that the planets move in ellipses. But this view of motion was at first conceived by Kepler as a modification of the conception of epicycles. On one occasion he blames himself for not sooner seeing that such a modification was possible. "What an absurdity on my part!" he cries*; "as if libration in the diameter of the epicycle might not come to the same thing as motion in the ellipse." But again; Kepler's *laws* of the elliptical motion of the planets were established; and these laws immediately became the *facts* on which the mathematicians had to found their mechanical theories. From these facts, Newton, as we have related, proved that the central force of the sun retains the planets in their orbits, according to the law of the inverse square of the distance. The same *law* was shown to prevail in the gravitation of the earth. It was shown, too, by induction from the motions of Jupiter and Saturn, that the planets attract each other; by calculations from the figure of the earth, that the parts of the earth attract each other; and, by considering the course of the tides, that the sun and moon attract the waters of the ocean. And all these curious discoveries being established as *facts*, the subject was ready for another step of generalization. By an unparalleled rapidity in the progress of discovery in this case, not only were all the inductions which we have first mentioned made by one individual, but the new advance, the higher flight, the closing victory, fell to the lot of the same extraordinary person.

The attraction of the sun upon the planets, of the moon upon the earth, of the planets on each other, of the parts of the earth on themselves, of the sun and moon

* *Hist. Inductive Sciences,* B. v. c. iv. Sect. 3.

upon the ocean;—all these truths, each of itself a great discovery, were included by Newton in the higher *generalization*, of the universal gravitation of matter, by which each particle is drawn to each other according to the law of the inverse square: and thus this long advance from discovery to discovery, from truths to truths, each justly admired when new, and then rightly used as old, was closed in a worthy and consistent manner, by a truth which is the most worthy admiration, because it includes all the researches of preceding ages of Astronomy.

We may take another example of a succession of this kind from the history of a science, which, though it has made wonderful advances, has not yet reached its goal, as physical astronomy appears to have done, but seems to have before it a long prospect of future progress. I now refer to Chemistry, in which I shall try to point out how the preceding discoveries afforded the materials of the succeeding; although this subordination and connexion is, in this case, less familiar to men's minds than in Astronomy, and is, perhaps, more difficult to present in a clear and definite shape. Sylvius saw, in the facts which occur, when an acid and an alkali are brought together, the evidence that they neutralize each other. But cases of neutralization, and acidification, and many other effects of mixture of the ingredients of bodies, being thus viewed as *facts*, had an aspect of unity and law given them by Geoffroy and Bergman*, who introduced the *conception* of the Chemical Affinity or Elective Attraction, by which certain elements select other elements, as if by preference. That combustion, whether a chemical union or a chemical separation of ingredients, is of the same nature with acidification, was the doctrine of Beccher

* *Hist. Inductive Sciences*, B. xiv. c. iii.

and Stahl, and was soon established as a truth which must form a part of every succeeding physical theory. That the rules of affinity and chemical composition may include gaseous elements, was established by Black and Cavendish. And all these truths, thus brought to light by chemical discoverers,—affinity, the identity of acidification and combustion, the importance of gaseous elements,—along with all the facts respecting the weight of ingredients and compounds which the balance disclosed,—were taken up, connected, and included as *particulars* in the oxygen *theory* of Lavoisier. Again, the results of this theory, and the quantity of the several ingredients which entered into each compound—(such results, for the most part, being now no longer mere theoretical speculations, but recognized facts)—were the *particulars* from which Dalton derived that wide law of chemical combination which we term the Atomic *Theory*. And this law, soon generally accepted among chemists, is already in its turn become one of the *facts* included in Faraday's *Theory* of the identity of Chemical Affinity and Electric Attraction.

It is unnecessary to give further exemplifications of this constant ascent from one step to a higher;—this perpetual conversion of true theories into the materials of other and wider theories. It will hereafter be our business to exhibit, in a more full and formal manner, the mode in which this principle determines the whole scheme and structure of all the most exact sciences. And thus, beginning with the facts of sense, we gradually climb to the highest forms of human knowledge, and obtain from experience and observation a vast collection of the most wide and elevated truths.

There are, however, truths of a very different kind, to which we must turn our attention, in order to pursue our researches respecting the nature and grounds of our knowledge. But before we do this, we must notice one more feature in that progress of science which we have already in part described.

CHAPTER III.

OF TECHNICAL TERMS.

1. IT has already been stated that we gather knowledge from the external world, when we are able to apply, to the facts which we observe, some ideal conception, which gives unity and connexion to multiplied and separate perceptions. We have also shown that our conceptions, thus verified by facts, may themselves be united and connected by a new bond of the same nature; and that man may thus have to pursue his way from truth to truth through a long progression of discoveries, each resting on the preceding, and rising above it.

Each of these steps, in succession, is recorded, fixed, and made available, by some peculiar form of words; and such words, thus rendered precise in their meaning, and appropriated to the service of science, we may call *Technical Terms*. It is in a great measure by inventing such Terms that men not only best express the discoveries they have made, but also enable their followers to become so familiar with these discoveries, and to possess them so thoroughly, that they can readily use them in advancing to ulterior generalizations.

Most of our ideal conceptions are described by exact and constant words or phrases, such as those of which we here speak. We have already had occasion to employ many of these. Thus we have had instances of technical Terms expressing geometrical conceptions, as *Ellipsis*, *Radius Vector*, *Axis*, *Plane*, the Proportion of the *Inverse Square*, and the like. Other Terms have described mechanical conceptions, as *Accelerating Force* and *Attraction*. Again, chemistry exhibits (as do all sciences) a series of Terms which mark the steps of our progress. The views of the first real founders of the science are recorded by the Terms which are still in use, *Neutral Salts*, *Affinity*, and the like. The establishment of Dalton's theory has produced the use of the word *Atom* in a peculiar sense, or of some other word, as *Proportion*, in a sense equally technical. And Mr. Faraday has found it necessary, in order to expound his electro-chemical theory, to introduce such terms as *Anode* and *Cathode*, *Anīon* and *Cathīon*.

2. I need not adduce any further examples, for my object at present is only to point out the use and influence of such language: its rules and principles I shall hereafter try, in some measure, to fix. But what we have here to remark is, the extraordinary degree in which the progress of science is facilitated, by thus investing each new discovery with a compendious and steady form of expression. These terms soon become part of the current language of all who take an interest in speculation. However strange they may sound at first, they soon grow familiar in our ears, and are used without any effort, or any recollection of the difficulty they once involved. They become as common as the phrases which express our most frequent feelings and interests, while yet they have incomparably more precision than belongs to any terms which express feelings; and they carry with them, in their import, the results of deep and laborious trains of research. They convey the mental treasures of one period to the generations that follow; and laden with this, their precious freight, they sail safely across gulfs of time in which empires have suffered shipwreck, and

the languages of common life have sunk into oblivion. We have still in constant circulation among us the Terms which belong to the geometry, the astronomy, the zoology, the medicine of the Greeks, and the algebra and chemistry of the Arabians. And we can in an instant, by means of a few words, call to our own recollection, or convey to the apprehension of another person, phenomena and relations of phenomena in optics, mineralogy, chemistry, which are so complex and abstruse, that it might seem to require the utmost subtlety of the human mind to grasp them, even if that were made the sole object of its efforts. By this remarkable effect of Technical Language, we have the results of all the labours of past times not only always accessible, but so prepared that we may (provided we are careful in the use of our instrument) employ what is really useful and efficacious for the purpose of further success, without being in any way impeded or perplexed by the length and weight of the chain of past connexions which we drag along with us.

By such means,—by the use of the Inductive Process, and by the aid of Technical Terms,—man has been constantly advancing in the path of scientific truth. In a succeeding part of this work we shall endeavour to trace the general rules of this advance, and to lay down the maxims by which it may be most successfully guided and forwarded. But in order that we may do this to the best advantage, we must pursue still further the analysis of knowledge into its elements; and this will be our employment in the first part of the work.

CHAPTER IV.

OF NECESSARY TRUTHS.

1. EVERY advance in human knowledge consists, as we have seen, in adapting new ideal conceptions to ascertained facts, and thus in superinducing the Form upon the Matter, the active upon the passive processes of our minds. Every such step introduces into our knowledge an additional portion of the ideal element, and of those relations which flow from the nature of Ideas. It is, therefore, important for our purpose to examine more closely this element, and to learn what the relations are which may thus come to form part of our knowledge. An inquiry into those Ideas which form the foundations of our sciences;—into the reality, independence, extent, and principal heads of the knowledge which we thus acquire;—is a task on which we must now enter, and which will employ us for several of the succeeding Books.

In this inquiry our object will be to pass in review all the most important Fundamental Ideas which our sciences involve; and to prove more distinctly in reference to each, what we have already asserted with regard to all, that there are everywhere involved in our knowledge acts of the mind as well as impressions of sense; and that our knowledge derives, from these acts, a generality, certainty, and evidence which the senses could in no degree have supplied. But before I proceed to do this in particular cases, I will give some account of the argument in its general form.

We have already considered the separation of our knowledge into its two elements,—Impressions of Sense and Ideas,—as evidently indicated by this; that all knowledge possesses characters which neither of these elements alone could bestow. Without our ideas, our sensations could have no connexion; without external impressions, our ideas would have no reality; and thus both ingredients of our knowledge must exist.

2. There is another mode in which the distinction of the two elements of knowledge appears, as I have already said: (C. I. Sect. 2.) namely in the distinction of *necessary* and *contingent* or *experiential* truths. For of these two classes of truths, the difference arises from this;—that the one class derives its nature from the one, and the other from the other, of the two elements of knowledge. I have already stated briefly the difference of these two kinds of truths:—namely, that the former are truths which, we see, must be true:—the latter are true, but so far as we can see, might be otherwise. The former are true necessarily and universally: the latter are learnt from experience and limited by experience. Now with regard to the former kind of truths, I wish to show that the universality and necessity which distinguish them can by no means be derived from experience; that these characters do in reality flow from the ideas which these truths involve; and that when the necessity of the truth is exhibited in the way of logical demonstration, it is found to depend upon certain fundamental principles, (Definitions and Axioms,) which may thus be considered as expressing, in some measure, the essential characters of our ideas. These fundamental principles I shall afterwards proceed to discuss and to exhibit in each of the principal departments of science.

I shall begin by considering Necessary Truths more fully than I have yet done. As I have already said, necessary truths are those in which we not only learn that the proposition *is* true, but see that it *must be* true; in which the negation of the truth is not only false, but impossible; in which we cannot, even by an effort of imagination, or in a supposition, conceive the reverse of that which is asserted.

3. That there are such truths cannot be doubted. We may take, for example, all relations of number. Three and Two added together make Five. We cannot conceive it to be otherwise. We cannot, by any freak of thought, imagine Three and Two to make Seven.

It may be said that this assertion merely expresses what we mean by our words; that it is a matter of definition; that the proposition is an identical one.

But this is by no means so. The definition of Five is not Three and Two, but Four and One. How does it appear that Three and Two is the same number as Four and One? It is evident that it is so; but *why* is it evident?—not because the proposition is identical; for if that were the reason, all numerical propositions must be evident for the same reason. If it be a matter of definition that 3 and 2 make 5, it must be a matter of definition that 39 and 27 make 66. But who will say that the definition of 66 is 39 and 27? Yet the magnitude of the numbers can make no difference in the ground of the truth. How do we know that the product of 13 and 17 is 4 less than the product of 15 and 15? We see that it is so, if we perform certain operations by the rules of arithmetic; but how do we know the truth of the rules of arithmetic? If we divide 123375 by 987 according to the process taught us at school, how are we assured that the result is correct, and that the number 125 thus obtained is really the number of times one number is contained in the other?

The correctness of the rule, it may be replied, can be rigorously demonstrated. It can be shewn that the process must inevitably give the true quotient.

Certainly this can be shown to be the case. And precisely because it *can* be shown that the result must be true, we have here an example of a necessary truth; and this truth, it appears, is not *therefore* necessary because it is itself evidently identical, however it may be possible to prove it by reducing it to evidently identical propositions. And the same is the case with all other numerical propositions; for, as we have said, the nature of all of them is the same.

Here, then, we have instances of truths which are not only true, but demonstrably and necessarily true. Now such truths are, in this respect at least, altogether different from truths, which, however certain they may be, are learnt to be so only by the evidence of observation, interpreted, as observation must be interpreted, by our own mental faculties. There is no difficulty in finding examples of these merely observed truths. We find that sugar dissolves in water, and forms a transparent fluid, but no one will say that we can see any reason beforehand why the result *must* be so. We find that all animals which chew the cud have also the divided hoof; but could any one have predicted that this would be universally the case? or supposing the truth of the rule to be known, can any one say that he cannot conceive the facts as occurring otherwise? Water expands when it crystallizes, some other substances contract in the same circumstances; but can any one know that this will be so otherwise than by observation? We have here propositions *rigorously* true, (we will assume,) but can any one say they are *necessarily* true? These, and the great mass of the doctrines established by induction, are actual, but so far as we can see, accidental laws; results determined by some unknown selection, not demonstrable consequences of the essence of things, inevitable and perceived to be inevitable. According to the phraseology which has been frequently used by philosophical writers, they are *contingent*, not necessary truths.

It is requisite to insist upon this opposition, because no insight can be obtained into the true nature of knowledge, and the mode of arriving at it, by any one who does not clearly appreciate the distinction. The separation of truths which are learnt by observation, and truths which can be seen to be true by a pure act of thought, is one of the first and most essential steps in our examination of the nature of truth, and the mode of its discovery. If any one does not clearly comprehend this distinction of necessary and contingent truths, he will not be able to go along with us in our researches into the foundations of human knowledge; nor, indeed, to pursue with success any speculation on the subject. But, in fact, this distinction is one that can hardly fail to be at once understood. It is insisted upon by almost all the best modern, as well as ancient, metaphysicians*, as of primary importance. And if any person does not fully apprehend, at first, the different kinds of truth thus pointed out, let him study, to some extent, those sciences which have necessary truth for their subject, as geometry, or the properties of numbers, so as to obtain a familiar acquaintance with such truth; and he will then hardly fail to see how different the evidence of the propositions which occur in these sciences, is from the evidence of the facts which are merely learnt from experience. That the year goes through its course in 365 days, can only be known by observation of the sun or stars: that 365 days is 52 weeks and a day, it requires no experience, but only a little thought to perceive. That bees build their cells in the form of hexagons, we cannot know without looking at them; that regular hexagons may be arranged so as to fill space, may be proved with the utmost rigour, even if there were not in existence such a thing as a material hexagon.

4. As I have already said, one mode in which we may express the difference of necessary truths and truths

* Aristotle, Dr. Whately, Dugald Stewart, &c.

of experience, is, that necessary truths are those of which we cannot distinctly conceive the contrary. We can very readily conceive the contrary of experiential truths. We can conceive the stars moving about the pole or across the sky in any kind of curves with any velocities; we can conceive the moon always appearing during the whole month as a luminous disk, as she might do if her light were inherent and not borrowed. But we cannot conceive one of the parallelograms on the same base and between the same parallels larger than the other; for we find that, if we attempt to do this, when we separate the parallelograms into parts, we have to conceive one triangle larger than another, both having all their parts equal; which we cannot conceive at all, if we conceive the triangles distinctly. We make this impossibility more clear by conceiving the triangles to be placed so that two sides of the one coincide with two sides of the other; and it is then seen, that in order to conceive the triangles unequal, we must conceive the two bases which have the same extremities both ways, to be different lines, though both straight lines. This it is impossible to conceive: we assent to the impossibility as an axiom, when it is expressed by saying, that two straight lines cannot inclose a space; and thus we cannot distinctly conceive the contrary of the proposition just mentioned respecting parallelograms.

But it is necessary, in applying this distinction, to bear in mind the terms of it;—that we cannot *distinctly* conceive the contrary of a necessary truth. For in a certain loose, indistinct way, persons conceive the contrary of necessary geometrical truths, when they erroneously conceive false propositions to be true. Thus, Hobbes erroneously held that he had discovered a means of geometrically doubling the cube, as it is called, that is, finding two mean proportionals between two given lines; a problem which cannot be solved by plane geometry. Hobbes not only proposed a construction for this purpose, but obstinately maintained that it was right, when it had been proved to be wrong. But then, the discussion showed how indistinct the geometrical conceptions of Hobbes were; for when his critics had proved that one of the lines in his diagram would not meet the other in the point which his reasoning supposed, but in another point near to it; he maintained, in reply, that one of these points was large enough to include the other, so that they might be considered as the same point. Such a mode of conceiving the opposite of a geometrical truth, forms no exception to the assertion, that this opposite cannot be distinctly conceived.

In like manner, the indistinct conceptions of children and of rude savages do not invalidate the distinction of necessary and experiential truths. Children and savages make mistakes even with regard to numbers; and might easily happen to assert that 27 and 38 are equal to 63 or 64. But such mistakes cannot make arithmetical truths cease to be necessary truths. When any person conceives these numbers and their addition distinctly, by resolving them into parts, or in any other way, he sees that their sum is necessarily 65. If, on the ground of the possibility of children and savages conceiving something different, it be held that this is not a necessary truth, it must be held on the same ground, that it is not a necessary truth that 7 and 4 are equal to 11; for children and savages might be found so unfamiliar with numbers as not to reject the assertion that 7 and 4 are 10, or even that 4 and 3 are 6, or 8. But I suppose that no persons would on such grounds hold that these arithmetical truths are truths known only by experience.

5. I have taken examples of necessary truths from the properties of number and space; but such truths exist no less in other subjects, although the discipline of thought which is requisite to perceive them distinctly, may not be so usual among men with regard to the sciences of mechanics and hydrostatics, as it is with regard to the sciences of geometry and arithmetic. Yet every one may perceive that there are such truths in mechanics. If I press the table with my hand, the table presses my hand with an equal force: here is a self-evident and necessary truth. In any machine, constructed in whatever manner to increase the force which I can exert, it is certain that what I gain in force I must lose in the velocity which I communicate. This is not a contingent truth, borrowed from and limited by observation; for a man of sound mechanical views applies it with like confidence, however novel be the construction of the machine. When I come to speak of the ideas which are involved in our mechanical knowledge, I may, perhaps, be able to bring more clearly into view the necessary truth of general propositions on such subjects. That reaction is equal and opposite to action, is as necessarily true as that two straight lines cannot inclose a space; it is as impossible theoretically to make a perpetual motion by mere mechanism as to make the diagonal of a square commensurable with the side.

6. Necessary truths must be *universal* truths. If any property belong to a right-angled triangle *necessarily*, it must belong to *all* right-angled triangles. And it shall be proved in the following Chapter, that truths possessing these two characters, of Necessity and Universality, cannot possibly be the mere results of experience.

Chapter V.

OF EXPERIENCE.

1. I here employ the term Experience in a more definite and limited sense than that which it possesses in common usage; for I restrict it to matters belonging to the domain of science. In such cases, the knowledge which we acquire, by means of experience, is of a clear and precise nature; and the passions and feelings and interests, which make the lessons of experience in practical matters so difficult to read aright, no longer disturb and confuse us. We may, therefore, hope, by attending to such cases, to learn what efficacy experience really has, in the discovery of truth.

That from *experience* (including intentional experience, or *observation*,) we obtain much knowledge which is highly important, and which could not be procured from any other source, is abundantly clear. We have already taken several examples of such knowledge. We know by experience that animals which ruminate are cloven-hoofed; and we know this in no other manner. We know, in like manner, that all the planets and their satellites revolve round the sun from west to east. It has been found by experience that all meteoric stones contain chrome. Many similar portions of our knowledge might be mentioned.

Now what we have here to remark is this;—that in no case can experience prove a proposition to be *necessarily* or *universally* true. However many instances we may have observed of the truth of a proposition, yet if it be known merely by observation, there is nothing to assure us that the next case shall not be an exception to the rule. If it be strictly true that every ruminant animal yet known has cloven hoofs, we still cannot be sure that some creature will not hereafter be discovered which has the first of these attributes without having the other. When the planets and their satellites, as far as Saturn, had been all found to move round the sun in one direction, it was still possible that there might be other such bodies not obeying this rule; and, accordingly, when the satellites of Uranus were detected, they appeared to offer an exception of this kind. Even in the mathematical sciences, we have examples of such rules suggested by experience, and also of their precariousness. However far they may have been tested, we cannot depend upon their correctness, except we see some reason for the rule. For instance, various rules have been given, for the purpose of pointing out *prime numbers;* that is, those which cannot be divided by any other number. We may try, as an example of such a rule, this one—any odd power of the number two, diminished by one. Thus the third power of two, diminished by one, is seven; the fifth power, diminished by one, is thirty-one; the seventh power so diminished is one hundred and twenty-seven. All these are prime numbers: and we might be led to suppose that the rule is universal. But the next example shows us the fallaciousness of such a belief. The ninth power of two, diminished by one, is five hundred and eleven, which is not a prime, being divisible by seven.

Experience must always consist of a limited number of observations. And, however numerous these may be, they can show nothing with regard to the infinite number of cases in which the experiment has not been made. Experience being thus unable to prove a fact to be universal, is, as will readily be seen, still more incapable of proving a truth to be necessary. Experience cannot, indeed, offer the smallest ground for the necessity of a proposition. She can observe and record what has happened; but she cannot find, in any case, or in any accumulation of cases, any reason for what *must* happen. She may see objects side by side; but she cannot see a reason why they must ever be side by side. She finds certain events to occur in succession; but the succession supplies, in its occurrence, no reason for its recurrence. She contemplates external objects; but she cannot detect any internal bond, which indissolubly connects the future with the past, the possible with the real. To learn a proposition by experience, and to see it to be necessarily true, are two altogether different processes of thought.

2. But it may be said, that we do learn by means of observation and experience many universal truths; indeed, all the general truths of which science consists. Is not the doctrine of universal gravitation learnt by experience? Are not the laws of motion, the properties of light, the general principles of chemistry, so learnt? How, with these examples before us, can we say that experience teaches no universal truths?

To this we reply, that these truths can only be known to be general, not universal, if they depend upon experience alone. Experience cannot bestow that universality which she herself cannot have, and that necessity of which she has no comprehension. If these doctrines *are* universally true, this universality flows from the *ideas* which we apply to our experience, and which are, as we have seen, the real sources of necessary truth. How far these ideas can communicate their universality and necessity to the results of experience, it will hereafter be our business to consider. It will then appear, that when the mind collects from observation truths of a wide and comprehensive kind, which approach to the simplicity and universality of the truths of pure science; she gives them this character by throwing upon them the light of her own Fundamental Ideas.

But the truths which we discover by observation of the external world, even when most strikingly simple and universal, are not necessary truths. Is the doctrine of universal gravitation necessarily true? It was doubted by Clairaut (so far as it refers to the moon), when the progression of the apogee in fact appeared to be twice as great as the theory admitted. It has been doubted, even more recently, with respect to the planets, their mutual perturbations appearing to indicate a deviation from the law. It is doubted still, by some persons, with respect to the double stars. But suppose all these doubts to be banished, and the law to be universal; is it then proved to be necessary? Manifestly not: the very existence of these doubts proves that it is not so. For the doubts were dissipated by reference to observation and calculation, not by reasoning on the nature of the law. Clairaut's difficulty was removed by a more exact calculation of the effect of the sun's force on the motion of the apogee. The suggestion of Bessel, that the intensity of gravitation might be different for different planets, was found to be unnecessary, when Professor Airy gave a more accurate determination of the mass of Jupiter. And the question whether the extension of the law of the inverse square to the double stars be true, (one of the most remarkable questions now before the scientific world,) must be answered, not by any speculations concerning what the laws of attraction must necessarily be, but by carefully determining the actual laws of the motion of these curious objects, by means of the observations such as those which Sir John Herschel has collected for that purpose, by his unexampled survey of both hemispheres of the sky. And since the extent of this truth is thus to be determined by reference to observed facts, it is clear that no mere accumulation of them can make its universality certain, or its necessity apparent.

Thus no knowledge of the necessity of any truths can result from the observation of what really happens. This being clearly understood, we are led to an important inquiry.

The characters of universality and necessity in the truths which form part of our knowledge, can never be derived from experience, by which so large a part of our knowledge is obtained. But since, as we have seen, we really do possess a large body of truths which are necessary, and because necessary, therefore universal, the question still recurs, from what source these characters of universality and necessity are derived.

The answer to this question we will attempt to give in the next chapter.

Chapter VI.

OF THE GROUNDS OF NECESSARY TRUTHS.

1. To the question just stated, I reply, that the necessity and universality of the truths which form a part of our knowledge, are derived from the *Fundamental Ideas* which those truths involve. These ideas entirely shape and circumscribe our knowledge; they regulate the active operations of our minds, without which our passive sensations do not become knowledge. They govern these operations, according to rules which are not only fixed and permanent, but which may be expressed in plain and definite terms; and these rules, when thus expressed, may be made the basis of demonstrations by which the necessary relations imparted to our knowledge by our Ideas may be traced to their consequences in the most remote ramifications of scientific truth.

These enunciations of the necessary and evident conditions imposed upon our knowledge by the Fundamental Ideas which it involves, are termed *Axioms*. Thus the Axioms of Geometry express the necessary conditions which result from the Idea of Space; the Axioms of Mechanics express the necessary conditions which flow from the Ideas of Force and Motion; and so on.

2. It will be the office of several of the succeeding Books of this work to establish and illustrate in detail what I have thus stated in general terms. I shall there pass in review many of the most important fundamental ideas on which the existing body of our science depends; and I shall endeavour to show, for each such idea in succession, that knowledge involves an active as well as a passive element; that it is not possible without an act of the mind, regulated by certain laws. I shall further attempt to enumerate some of the principal fundamental relations which each idea thus introduces into our thoughts, and to express them by means of definitions and axioms, and other suitable forms.

I will only add a remark or two to illustrate further this view of the ideal grounds of our knowledge.

3. To persons familiar with any of the demonstrative sciences, it will be apparent that if we state all the Definitions and Axioms which are employed in the demonstrations, we state the whole basis on which those reasonings rest. For the whole process of demonstrative or deductive reasoning in any science, (as in geometry, for instance,) consists entirely in combining some of these first principles so as to obtain the simplest propositions of the science; then combining these so as to obtain other propositions of greater complexity; and so on, till we advance to the most recondite demonstrable truths; these last, however, intricate and unexpected, still involving no principles except the original definitions and

axioms. Thus, by combining the Definition of a triangle, and the Definitions of equal lines and equal angles, namely, that they are such as when applied to each other, coincide, with the Axiom respecting straight lines (that two such lines cannot inclose a space,) we demonstrate the equality of triangles, under certain assumed conditions. Again, by combining this result with the Definition of parallelograms, and with the Axiom that if equals be taken from equals the wholes are equal, we prove the equality of parallelograms between the same parallels and upon the same base. From this proposition, again, we prove the equality of the square on the hypotenuse of a triangle to the squares on the two sides containing the right angle. But in all this there is nothing contained which is not rigorously the result of our geometrical Definitions and Axioms. All the rest of our treatises of geometry consists only of terms and phrases of reasoning, the object of which is to connect those first principles, and to exhibit the effects of their combination in the shape of demonstration.

4. This combination of first principles takes place according to the forms and rules of *Logic*. All the steps of the demonstration may be stated in the shape in which logicians are accustomed to exhibit processes of reasoning in order to show their conclusiveness, that is, in *Syllogisms*. Thus our geometrical reasonings might be resolved into such steps as the following:—

All straight lines drawn from the centre of a circle to its circumference are equal:

But the straight lines AB, AC, are drawn from the centre of a circle to its circumference:

Therefore the straignt lines AB, AC, are equal.

Each step of geometrical, and all other demonstrative reasoning, may be resolved into three such clauses as these; and these three clauses are termed respectively, the *major premiss*, the *minor premiss*, and the *conclusion;* or, more briefly, the *major*, the *minor*, and the *conclusion.*

The principle which justifies the reasoning when exhibited in this syllogistic form, is this:—that a truth which can be asserted as generally, or rather as universally true, can be asserted as true also in each particular case. The *minor* only asserts a certain particular case to be an example of such conditions as are spoken of in the *major;* and hence the conclusion, which is true of the major by supposition, is true of the minor by consequence; and thus we proceed from syllogism to syllogism, in each one employing some general truth in some particular instance. Any proof which occurs in geometry, or any other science of demonstration, may thus be reduced to a series of processes, in each of which we pass from some general proposition to the narrower and more special propositions which it includes. And this process of deriving truths by the mere combination of general principles, applied in particular hypothetical cases, is called *deduction;* being opposed to *induction*, in which, as we have seen, (Chap. I. Sect. 3.) a new general principle is introduced at every step.

5. Now we have to remark that, this being so, however far we follow such deductive reasoning, we can never have, in our conclusion any truth which is not virtually included in the original principles from which the reasoning started. For since at any step we merely take out of a general proposition something included in it, while at the preceding step we have taken this general proposition out of one more general, and so on perpetually, it is manifest that our last result was really included in the principle or principles with which we began. I say *principles*, because, although our logical conclusion can only exhibit the legitimate issue of our

first principles, it may, nevertheless, contain the result of the combination of several such principles, and may thus assume a great degree of complexity, and may appear so far removed from the parent truths, as to betray at first sight hardly any relationship with them. Thus the proposition which has already been quoted respecting the squares on the sides of a right-angled triangle, contains the results of many elementary principles; as, the definitions of parallels, triangle, and square; the axioms respecting straight lines, and respecting parallels; and, perhaps, others. The conclusion is complicated by containing the effects of the combination of all these elements; but it contains nothing, and can contain nothing, but such elements and their combinations.

This doctrine, that logical reasoning produces no new truths, but only unfolds and brings into view those truths which were, in effect, contained in the first principles of the reasoning, is assented to by almost all who, in modern times, have attended to the science of logic. Such a view is admitted both by those who defend, and by those who depreciate the value of logic. "Whatever is established by reasoning, must have been contained and virtually asserted in the premises*." "The only truth which such propositions can possess consists in conformity to the original principles."

In this manner the whole substance of our geometry is reduced to the Definitions and Axioms which we employ in our elementary reasonings; and in like manner we reduce the demonstrative truths of any other science to the definitions and axioms which we there employ.

6. But in reference to this subject, it has sometimes been said that demonstrative sciences do in reality depend upon Definitions only; and that no additional kind of

* Whateley's *Logic*, pp. 237, 238.

principle, such as we have supposed Axioms to be, is absolutely required. It has been asserted that in geometry, for example, the source of the necessary truth of our propositions is this, that they depend upon definitions alone, and consequently merely state the identity of the same thing under different aspects.

That in the sciences which admit of demonstration, as geometry, mechanics, and the like, Axioms as well as Definitions are needed, in order to express the grounds of our necessary convictions, must be shown hereafter by an examination of each of these sciences in particular. But that the propositions of these sciences, those of geometry for example, do not merely assert the identity of the same thing, will, I think, be generally allowed, if we consider the assertions which we are enabled to make. When we declare that "a straight line is the shortest distance between two points," is this merely an identical proposition? the definition of a straight line in another form? Not so: the definition of a straight line involves the notion of form only, and does not contain anything about magnitude; consequently, it cannot contain anything equivalent to "shortest." Thus the propositions of geometry are not merely identical propositions; nor have we in their general character anything to countenance the assertion, that they are the results of definitions alone. And when we come to examine this and other sciences more closely, we shall find that axioms, such as are usually in our treatises made the fundamental principles of our demonstrations, neither have ever been, nor can be, dispensed with. Axioms, as well as Definitions, are in all cases requisite, in order properly to exhibit the grounds of necessary truth.

7. Thus the real logical basis of every body of demonstrated truths are the Definitions and Axioms which are the first principles of the reasonings. But when we are

arrived at this point, the question further occurs, what is the ground of the truth of these Axioms? It is not the logical, but the philosophical, not the formal, but the real foundation of necessary truth, which we are seeking. Hence this inquiry necessarily comes before us, What is the ground of the Axioms of Geometry, of Mechanics, and of any other demonstrable science?

The answer which we are led to give, by the view which we have taken of the nature of knowledge, has already been stated. The ground of the axioms belonging to each science is the *Idea* which the axiom involves. The ground of the Axioms of Geometry is the *Idea of Space:* the ground of the Axioms of Mechanics is the *Idea of Force*, of *Action* and *Reaction*, and the like. And hence these Ideas are Fundamental Ideas; and since they are thus the foundations, not only of demonstration but of truth, an examination into their real import and nature is of the greatest consequence to our purpose.

8. Not only the Axioms, but the Definitions which form the basis of our reasonings, depend upon our Fundamental Ideas. And the Definitions are not arbitrary definitions, but are determined by a necessity no less rigorous than the Axioms themselves. We could not think of geometrical truths without conceiving a circle; and we could not reason concerning such truths without defining a circle in some mode equivalent to that which is commonly adopted. The Definitions of parallels, of right angles, and the like, are quite as necessarily prescribed by the nature of the case, as the Axioms which these Definitions bring with them. Indeed we may substitute one of these kinds of principles for another. We cannot always put a Definition in the place of an Axiom; but we may always find an Axiom which shall take the place of a Definition. If we assume a proper Axiom respecting straight lines, we need no Definition

of a straight line. But in whatever shape the principle appear, as Definition or as Axiom, it has about it nothing casual or arbitrary, but is determined to be what it is, as to its import, by the most rigorous necessity, growing out of the Idea of Space.

9. These principles,—Definitions, and Axioms,—thus exhibiting the primary developements of a fundamental idea, do in fact express the idea, so far as its expression in words forms part of our science. They are different views of the same body of truth; and though each principle, by itself, exhibits only one aspect of this body, taken together they convey a sufficient conception of it for our purposes. The Idea itself cannot be fixed in words; but these various lines of truth proceeding from it, suggest sufficiently to a fitly-prepared mind, the place where the idea resides, its nature, and its efficacy.

It is true that these principles,—our elementary Definitions and Axioms,—even taken altogether, express the Idea incompletely. Thus the Definitions and Axioms of Geometry, as they are stated in our elementary works, do not fully express the Idea of Space as it exists in our minds. For, in addition to these, other Axioms, independent of these, and no less evident, can be stated; and are in fact stated when we come to the Higher Geometry. Such, for instance, is the Axiom of Archimedes —that a curve line which joins two points is less than a broken line which joins the same points and includes the curve. And thus the Idea is disclosed but not fully revealed, imparted but not transfused, by the use we make of it in science. When we have taken from the fountain so much as serves our purpose, there still remains behind a deep well of truth, which we have not exhausted, and which we may easily believe to be inexhaustible.

Chapter VII.

THE FUNDAMENTAL IDEAS ARE NOT DERIVED FROM EXPERIENCE.

1. By the course of speculation contained in the last three Chapters, we are again led to the conclusion which we have already stated, that our knowledge contains an ideal element, and that this element is not derived from experience. For we have seen that there are propositions which are known to be necessarily true; and that such knowledge is not, and cannot be, obtained by mere observation of actual facts. It has been shown, also, that these necessary truths are the results of certain fundamental ideas, such as those of space, number, and the like. Hence it follows inevitably that these ideas and others of the same kind are not derived from experience. For these ideas possess a power of infusing into their developements that very necessity which experience can in no way bestow. This power they do not borrow from the external world, but possess by their own nature. Thus we unfold out of the Idea of Space the propositions of geometry, which are plainly truths of the most rigorous necessity and universality. But if the idea of space were merely collected from observation of the external world, it could never enable or entitle us to assert such propositions: it could never authorize us to say that not merely some lines, but *all* lines, not only have, but *must* have, those properties which geometry teaches. Geometry in every proposition speaks a language which experience never dares to utter; and indeed of which she but half comprehends the meaning. Experience sees that the assertions are true, but she sees not how profound and absolute is their truth. She unhesitatingly assents to the laws which geometry delivers, but she does

not pretend to see the origin of their obligation. She is always ready to acknowledge the sway of pure scientific principles as a matter of fact, but she does not dream of offering her opinion on their authority as a matter of right; still less can she justly claim to be herself the source of that authority.

David Hume asserted*, that we are incapable of seeing in any of the appearances which the world presents anything of necessary connexion; and hence he inferred that our knowledge cannot extend to any such connexion. It will be seen from what we have said that we assent to his remark as to the fact, but we differ from him altogether in the consequence to be drawn from it. Our inference from Hume's observation is, not the truth of his conclusion, but the falsehood of his premises;—not that, therefore, we can know nothing of natural connexion, but that, therefore, we have some other source of knowledge than experience:—not, that we can have no idea of connexion or causation, because, in his language, it cannot be the copy of an impression; but that since we have such an idea, our ideas are not the copies of our impressions.

Since it thus appears that our fundamental ideas are not acquired from the external world by our senses, but have some separate and independent origin, it is important for us to examine their nature and properties, as they exist in themselves; and this it will be our business to do through a portion of the following pages. But it may be proper first to notice one or two objections which may possibly occur to some readers.

2. It may be said that without the use of our senses, of sight and touch, for instance, we should never have any idea of space; that this idea, therefore, may properly be said to be derived from those senses. And to this I

* *Essays*, Vol. II. p. 70.

reply, by referring to a parallel instance. Without light we should have no perception of visible figure; yet the power of perceiving visible figure cannot be said to be derived from the light, but resides in the structure of the eye. If we had never seen objects in the light, we should be quite unaware that we possessed a power of vision; yet we should not possess it the less on that account. If we had never exercised the senses of sight and touch (if we can conceive such a state of human existence) we know not that we should be conscious of an idea of space. But the light reveals to us at the same time the existence of external objects and our own power of seeing. And in a very similar manner, the exercise of our senses discloses to us, at the same time, the external world, and our own ideas of space, time, and other conditions, without which the external world can neither be observed nor conceived. That light is necessary to vision, does not, in any degree, supersede the importance of a separate examination of the laws of our visual powers, if we would understand the nature of our own bodily faculties and the extent of the information they can give us. In like manner, the fact that intercourse with the external world is necessary for the conscious employment of our ideas, does not make it the less essential for us to examine those ideas in their most intimate structure, in order that we may understand the grounds and limits of our knowledge. Even before we see a single object, we have a faculty of vision; and in like manner, if we can suppose a man who has never contemplated an object in space or time, we must still assume him to have the faculties of entertaining the ideas of space and time, which faculties are called into play on the very first occasion of the use of the senses.

3. In answer to such remarks as the above, it has sometimes been said that to assume separate faculties in

the mind for so many different processes of thought, is to give a mere verbal explanation, since we learn nothing concerning our idea of space by being told that we have a faculty of forming such an idea. It has been said that this course of explanation leads to an endless multiplication of elements in man's nature, without any advantage to our knowledge of his true constitution. We may, it is said, assert man to have a faculty of walking, of standing, of breathing, of speaking; but what, it is asked, is gained by such assertions? To this I reply, that we undoubtedly have such faculties as those just named; that it is by no means unimportant to consider them; and that the main question in such cases is, whether they are separate and independent faculties, or complex and derivative ones; and, if the latter be the case, what are the simple and original faculties by the combination of which the others are produced. In walking, standing, breathing, for instance, a great part of the operation can be reduced to one single faculty; the voluntary exercise of our muscles. But in breathing this does not appear to be the whole of the process. The operation is, in part at least, involuntary; and it has been held that there is a certain sympathetic action of the nerves, in addition to the voluntary agency which they transmit, which is essential to the function. To determine whether or no this sympathetic faculty is real and distinct, and if so, what are its laws and limits, is certainly a highly philosophical inquiry, and well deserving the attention which has been bestowed upon it by eminent physiologists. And just of the same nature are the inquiries with respect to man's intellectual constitution, on which we propose to enter. For instance, man has a faculty of apprehending time, and a faculty of reckoning numbers: are these distinct, or is one faculty derived from the other? To analyze the various combinations of our ideas and observations into the original faculties which they involve; to show that these faculties are original, and not capable of further analysis: to point out the characters which mark these faculties and lead to the most important features of our knowledge;—these are the kind of researches on which we have now to enter, and these, we trust, will be found to be far from idle or useless parts of our plan. If we succeed in such attempts, it will appear that it is by no means a frivolous or superfluous step to distinguish separate faculties in the mind. If we do not learn much by being told that we have a faculty of forming the idea of space, we at least, by such a commencement, circumscribe a certain portion of the field of our investigations, which, we shall afterwards endeavour to show, requires and rewards a special examination. And though we shall thus have to separate the domain of our philosophy into many provinces, these are, as we trust it will appear, neither arbitrarily assigned, nor vague in their limits, nor infinite in number.

Chapter VIII.

OF THE PHILOSOPHY OF THE SCIENCES.

We proceed, in the ensuing Books, to the closer examination of a considerable number of those Fundamental Ideas on which the sciences, hitherto most successfully cultivated, are founded. In this task, our objects will be to explain and analyze such Ideas so as to bring into view the Definitions and Axioms, or other forms, in which we may clothe the conditions to which our speculative knowledge is subjected. I shall also try to prove, for some of these Ideas in particular, what has been already urged respecting them in general, that they are not derived from observation, but necessarily impose their conditions upon that knowledge of which observation supplies the materials. I shall further, in some cases, endeavour to trace the history of these Ideas as they have successively come into notice in the progress of science; the gradual developement by which they have arrived at their due purity and clearness; and, as a necessary part of such a history, I shall give a view of some of the principal controversies which have taken place with regard to each portion of knowledge.

An exposition and discussion of the Fundamental Ideas of each Science may, with great propriety, be termed the PHILOSOPHY OF such SCIENCE. These ideas contain in themselves the elements of those truths which the science discovers and enunciates; and in the progress of the sciences, both in the world at large and in the mind of each individual student, the most important steps consist in apprehending these ideas clearly, and in bringing them into accordance with the observed facts. I shall, therefore, in a series of Books, treat of the *Philosophy of the Pure Sciences*, the *Philosophy of the Mechanical Sciences*, the *Philosophy of Chemistry*, and the like, and shall analyze and examine the ideas which these sciences respectively involve.

In this undertaking, inevitably somewhat long, and involving many deep and subtle discussions, I shall take, as a chart of the country before me, by which my course is to be guided, the scheme of the sciences which I was led to form by travelling over the history of each in order*. Each of the sciences of which I then narrated the progress, depends upon several of the Fundamental Ideas of which I have to speak: some of these Ideas are peculiar to one field of speculation, others are common to more. A previous enumeration of Ideas thus collected

* *History of the Inductive Sciences.*

may serve both to show the course and limits of this part of our plan, and the variety of interest which it offers.

I shall, then, successively, have to speak of the Ideas which are the foundation of Geometry and Arithmetic, (and which also regulate all sciences depending upon these, as Astronomy and Mechanics;) namely, the Ideas of *Space*, *Time*, and *Number:*

Of the Ideas on which the Mechanical Sciences (as Mechanics, Hydrostatics, Physical Astronomy) more peculiarly rest; the ideas of *Force* and *Matter*, or rather the idea of *Cause*, which is the basis of these:

Of the Ideas which the Secondary Mechanical Sciences (Acoustics, Optics, and Thermotics) involve; namely, the Ideas of the *Externality* of objects, and of the *Media* by which we perceive their qualities:

Of the Ideas which are the basis of Mechanico-chemical and Chemical Science; *Polarity*, *Chemical Affinity*, and *Substance;* and the Idea of *Symmetry*, a necessary part of the Philosophy of Crystallography:

Of the Ideas on which the Classificatory Sciences proceed (Mineralogy, Botany, and Zoology); namely, the Ideas of *Resemblance*, and of its gradations, and of *Natural Affinity:*

Finally, of those Ideas on which the Physiological Sciences are founded; the Ideas of separate Vital Powers, such as *Assimilation* and *Irritability;* and the Idea of *Final Cause.*

We have, besides these, the Palætiological Sciences, which proceed mainly on the conception of *Historical Causation.*

It is plain that when we have proceeded so far as this, we have advanced to the verge of those speculations which have to do with mind as well as body. The extension of our philosophy to such a field, if it can be justly so extended, will be one of the most important results of our researches; but on that very account we must fully study the lessons which we learn in those fields of speculation where our doctrines are most secure, before we venture into a region where our principles will appear to be more precarious, and where they are inevitably less precise.

We now proceed to the examination of the above Ideas, and to such essays towards the philosophy of each Science as this course of investigation may suggest.

BOOK II.

THE PHILOSOPHY OF THE PURE SCIENCES.

CHAPTER I.

OF THE PURE SCIENCES.

1. ALL external objects and events which we can contemplate are viewed as having relations of Space, Time, and Number; and are subject to the general conditions which these Ideas impose, as well as to the particular laws which belong to each class of objects and occurrences. The special laws of nature, considered under the various aspects which constitute the different sciences, are obtained by a mixed reference to experience and to the fundamental ideas of each science. But besides the sciences thus formed by the aid of special experience, the conditions which flow from those more comprehensive ideas first mentioned, Space, Time, and Number, constitute a body of science, applicable to objects and changes of all kinds, and deduced without recurrence being had to any observation in particular. These sciences, thus unfolded out of ideas alone, unmixed with any reference to the phenomena of matter, are hence termed *Pure* Sciences. The principal sciences of this class are Geometry, Theoretical Arithmetic, and Algebra considered in its most general sense, as the investigation of the relations of space and number by means of general symbols.

2. These Pure Sciences were not included in our survey of the history of the sciences, because they are not *inductive* sciences. Their progress has not consisted in collecting laws from phenomena, true theories from observed facts, and more general from more limited laws; but in tracing the consequences of the ideas themselves, and in detecting the most general and intimate analogies and connexions which prevail among such conceptions as are derivable from the ideas. These sciences have no principles besides definitions and axioms, and no process of proof but *deduction;* this process, however, assuming here a most remarkable character; and exhibiting a combination of simplicity and complexity, of rigour and generality, quite unparalleled in other subjects.

3. The universality of the truths, and the rigour of the demonstrations of these pure sciences, attracted attention in the earliest times; and it was perceived that they offered an exercise and a discipline of the intellectual faculties, in a form peculiarly free from admixture of extraneous elements. They were strenuously cultivated by the Greeks, both with a view to such a discipline, and from the love of speculative truth which prevailed among that people: and the name *mathematics*, by which they are designated, indicates this their character of *disciplinal* studies.

4. As has already been said, the ideas which these sciences involve extend to all the objects and changes which we observe in the external world; and hence the consideration of mathematical relations forms a large portion of many of the sciences which treat of the phenomena and laws of external nature, as Astronomy, Optics, and Mechanics. Such sciences are hence often termed *Mixed Mathematics*, the relations of space and number being, in these branches of knowledge, combined with principles collected from special observation;

while Geometry, Algebra, and the like subjects, which involve no result of experience, are called *Pure Mathematics.*

5. Space, time, and number, may be conceived as *forms* by which the knowledge derived from our sensations is moulded, and which are independent of the differences in the *matter* of our knowledge, arising from the sensations themselves. Hence the sciences which have these ideas for their subject may be termed *Formal Sciences.* In this point of view, they are distinguished from sciences in which, besides these mere formal laws by which appearances are corrected, we endeavour to apply to the phenomena the idea of cause, or some of the other ideas which penetrate further into the principles of nature. We have thus, in the History, distinguished Formal Astronomy and Formal Optics from Physical Astronomy and Physical Optics.

We now proceed to our examination of the Ideas which constitute the foundation of these formal or pure mathematical sciences, beginning with the Idea of Space.

CHAPTER II.

OF THE IDEA OF SPACE.

1. BY speaking of space as an Idea, I intend to imply, as has already been stated, that the apprehension of objects as existing in space, and of the relations of position, &c., prevailing among them, is not a consequence of experience, but a result of a peculiar constitution and activity of the mind, which is independent of all experience in its origin, though constantly combined with experience in its exercise.

That the idea of space is thus independent of experience, has already been pointed out in speaking of ideas in general: but it may be useful to illustrate the doctrine further in this particular case.

I assert, then, that space is not a notion obtained by experience. Experience gives us information concerning things without us: but our apprehending them *as* without us, takes for granted their existence in space. Experience acquaints us what are the form, position, magnitude of particular objects: but that they *have* form, position, magnitude, presupposes that they are in space. We cannot derive from appearances, by the way of observation, the habit of representing things to ourselves as in space; for no single act of observation is possible any otherwise than by beginning with such a representation, and conceiving objects as already existing in space.

2. That our mode of representing space to ourselves is not derived from experience, is clear also from this:—that through this mode of representation we arrive at propositions which are rigorously universal and necessary. Propositions of such a kind could not possibly be obtained from experience; for experience can only teach us by a limited number of examples, and therefore can never securely establish a universal proposition: and again, experience can only inform us that anything is so, and can never prove that it must be so. That two sides of a triangle are greater than the third is a universal and necessary geometrical truth: it is true of all triangles; it is true in such a way that the contrary cannot be conceived. Experience could not prove such a proposition. And experience has not proved it; for perhaps no man ever made the trial as a means of removing doubts: and no trial could, in fact, add in the smallest degree to the certainty of this truth. To seek for proof of geometrical propositions by an appeal to observation proves nothing in reality, except that the person who has recourse to such grounds has no due apprehension

of the nature of geometrical demonstration. We have heard of persons who convinced themselves by measurement that the geometrical rule respecting the squares on the sides of a right-angled triangle was true: but these were persons whose minds had been engrossed by practical habits, and in whom the speculative developement of the idea of space had been stifled by other employments. The practical trial of the rule may illustrate, but cannot prove it. The rule will of course be confirmed by such trial, because what is true in general is true in particular: but the rule cannot be proved from any number of trials, for no accumulation of particular cases makes up a universal case. To all persons who can see the force of any proof, the geometrical rule above referred to is as evident, and its evidence as independent of experience, as the assertion that sixteen and nine make twenty-five. At the same time, the truth of the geometrical rule is quite independent of numerical truths, and results from the relations of space alone. This could not be if our apprehension of the relations of space were the fruit of experience: for experience has no element from which such truth and such proof could arise.

3. Thus the existence of necessary truths, such as those of geometry, proves that the idea of space from which they flow, is not derived from experience. Such truths are inconceivable on the supposition of their being collected from observation; for the impressions of sense include no evidence of necessity. But we can readily understand the necessary character of such truths, if we conceive that there are certain necessary conditions under which alone the mind receives the impressions of sense. Since these conditions reside in the constitution of the mind, and apply to every perception of an object to which the mind can attain, we easily see that their rules must include, not only all that has been, but all that can

be, matter of experience. Our sensations can each convey no information except about itself; each can contain no trace of another additional sensation; and thus no relation and connexion between two sensations can be given by the sensations themselves. But the mode in which the mind perceives these impressions as objects, may and will introduce necessary relations among them: and thus by conceiving the idea of space to be a condition of perception in the mind, we can conceive the existence of necessary truths, which apply to all perceived objects.

4. If we consider the impressions of sense as the mere materials of our experience, such materials may be accumulated in any quantity and in any order. But if we suppose that this matter has a certain form given it, in the act of being accepted by the mind, we can understand how it is that these materials are subject to inevitable rules;—how nothing can be perceived exempt from the relations which belong to such a form. And since there are such truths applicable to our experience, and arising from the nature of space, we may thus consider space as a *form* which the materials given by experience necessarily assume in the mind; as an arrangement derived from the perceiving mind, and not from the sensations alone.

5. Thus this phrase,—that space is a *form* belonging to our perceptive power,—may be employed to express that we cannot perceive objects as in space, without an operation of the mind as well as of the senses—without active as well as passive faculties. This phrase, however, is not necessary to the exposition of our doctrines. Whether we call the conception of space a condition of perception, a form of perception, or an idea, or by any other term, it is something originally inherent in the mind perceiving, and not in the objects perceived. And

it is because the apprehension of all objects is thus subjected to certain mental conditions, forms or ideas, that our knowledge involves certain inviolable relations and necessary truths. The principles of such truths, so far as they regard space, are derived from the idea of space, and we must endeavour to exhibit such principles in their general form. But before we do this, we may notice some of the conditions which belong, not to our Ideas in general, but to this Idea of Space in particular.

CHAPTER III.

OF SOME PECULARITIES OF THE IDEA OF SPACE.

1. SOME of the Ideas which we shall have to examine involve conceptions of certain relations of objects, as the idea of Cause and of Likeness; and may appear to be suggested by experience, enabling us to *abstract* this general relation from particular cases. But it will be seen that Space is not such a general conception of a relation. For we do not speak of *Spaces* as we speak of Causes and Likenesses, but of Space. And when we speak of *spaces*, we understand by the expression, parts of one and the same identical everywhere-extended Space. We conceive a Universal Space; which is not made up of these partial spaces as its component parts, for it would remain if these were taken away; and these cannot be conceived without presupposing absolute space. Absolute Space is essentially one; and the complication which exists in it, and the conception of various spaces, depends merely upon boundaries. Space must, therefore, be, as we have said, not a general conception abstracted from particulars, but a universal mode of representation, altogether independent of experience.

2. Space is infinite. We represent it to ourselves as an infinitely great magnitude. Such an idea as that of Likeness or Cause, is, no doubt, found in an infinite number of particular cases, and so far includes these cases. But these ideas do not include an infinite number of cases as parts of an infinite whole. When we say that all bodies and partial spaces exist *in* infinite space, we use an expression which is not applied in the same sense to any cases except those of Space and Time.

3. What is here said may appear to be a denial of the real existence of space. It must be observed, however, that we do not deny, but distinctly assert, the existence of space as a real and necessary condition of all objects perceived; and that we not only allow that objects are seen external to us, but we found upon the fact of their being so seen, our view of the nature of space. If, however, it be said that we deny the reality of space as an object or thing, this is true. Nor does it appear easy to maintain that space exists as a thing, when it is considered that this thing is infinite in all its dimensions; and, moreover, that it is a thing, which, being nothing in itself, exists only that other things may exist in it. And those who maintain the real existence of space, must also maintain the real existence of time in the same sense. Now two infinite things, thus really existing, and yet existing only as other things exist in them, are notions so extravagant that we are driven to some other mode of explaining the state of the matter.

4. Thus space is not an object of which we perceive the properties, but a form of our perception; not a thing which affects our senses, but an idea to which we conform the impressions of sense. And its peculiarities appear to depend upon this, that it is not only a form of sensation, but of *intuition;* that in reference to space, we not only perceive but *contemplate* objects. We see

objects in space, side by side, exterior to each other; space, and objects in so far as they occupy space, have parts exterior to other parts; and have the whole thus made up by the juxtaposition of parts. This mode of apprehension belongs only to the ideas of space and time. Space and Time are made up of parts, but Cause and Likeness are not apprehended as made up of parts. And the term *intuition* (in its rigorous sense) is applicable only to that mode of contemplation in which we thus look at objects as made up of parts, and apprehend the relations of those parts at the same time and by the same act by which we apprehend the objects themselves.

5. As we have said, space limited by *boundaries* gives rise to various conceptions which we have often to consider. Thus limited, space assumes *form* or *figure;* and the variety of conceptions thus brought under our notice is infinite. We have every possible form of line, straight line, and curve; and of curves an endless number;—circles, parabolas, hyperbolas, spirals, helices. We have plane surfaces of various shapes,—parallelograms, polygons, ellipses; and we have solid figures,—cubes, cones, cylinders, spheres, spheroids, and so on. All these have their various properties, depending on the relations of their boundaries; and the investigation of their properties forms the business of the science of Geometry.

6. Space has three dimensions, or directions in which it may be measured; it cannot have more or fewer. The simplest measurement is that of a straight line, which has length alone. A surface has both length and breadth: and solid space has length, breadth, and thickness or depth. The origin of such a difference of dimensions will be seen if we reflect that each portion of space has a boundary, and is extended both *in* the direction in which its boundary extends, and also in a direction *from* its boundary; for otherwise it would not be a boundary.

A point has no dimensions. A line has but one dimension,—the distance from its boundary, or its *length*. A plane, bounded by a straight line, has the dimension which belongs to this line, and also has another dimension arising from the distance of its parts from this boundary line; and this may be called *breadth*. A solid, bounded by a plane, has the dimensions which this plane has; and has also a third dimension, which we may call *height* or *depth*, as we consider the solid extended above or below the plane; or *thickness*, if we omit all consideration of up and down. And no space can have any dimensions which are not resoluble into these three.

We may now proceed to consider the mode in which the idea of space is employed in the formation of Geometry.

Chapter IV.

OF THE DEFINITIONS AND AXIOMS WHICH RELATE TO SPACE.

1. The relations of space have been apprehended with peculiar distinctness and clearness from the very first unfolding of man's speculative powers. This was a consequence of the circumstance which we have just noticed, that the simplest of these relations, and those on which the others depend, are seen by intuition. Hence, as soon as men were led to speculate concerning the relations of space, they assumed just principles, and obtained true results. It is said that the science of *geometry* had its origin in Egypt, before the dawn of the Greek philosophy: but the knowledge of the early Egyptians (exclusive of their mythology) appears to have been purely practical; and, probably, their geometry consisted only in some maxims of *land-measuring*, which is what the term implies. The Greeks of the time of

Plato, had, however, not only possessed themselves of many of the most remarkable elementary theorems of the science; but had, in several instances, reached the boundary of the science in its elementary form; as when they proposed to themselves the problems of doubling the cube and squaring the circle.

But the deduction of these theorems by a systematic process, and the primary exhibition of the simplest principles involved in the idea of space, which such a deduction requires, did not take place, so far as we are aware, till a period somewhat later. The *Elements of Geometry* of Euclid, in which this task was performed, are to this day the standard work on the subject: the author of this work taught mathematics with great applause at Alexandria, in the reign of Ptolemy Lagus, about 280 years before Christ. The principles which Euclid makes the basis of his system have been very little simplified since his time; and all the essays and controversies which bear upon these principles, have had a reference to the form in which they are stated by him.

2. *Definitions.*—The first principles of Euclid's geometry are, as the first principles of any system of geometry must be, definitions and axioms respecting the various ideal conceptions which he introduces; as straight lines, parallel lines, angles, circles, and the like. But it is to be observed that these definitions and axioms are very far from being arbitrary hypotheses and assumptions. They have their origin in the idea of space, and are merely modes of exhibiting that idea in such a manner as to make it afford grounds of deductive reasoning. The axioms are necessary consequences of the conceptions respecting which they are asserted; and the definitions are no less necessary limitations of conceptions; not requisite in order to arrive at this or that

consequence; but necessary in order that it may be possible to draw any consequences, and to establish any general truths.

For example, if we rest the end of one straight staff upon the middle of another straight staff, and move the first staff into various positions, we, by so doing, alter the angles which the first staff makes with the other to the right hand and to the left. But if we place the staff in that special position in which these two angles are equal, each of them is a right angle, according to Euclid; and this is the *definition* of a right angle, except that Euclid employs the abstract conception of straight lines, instead of speaking, as we have done, of staves. But this selection of the case in which the two angles are equal is not a mere act of caprice; as it might have been if he had selected a case in which these angles are unequal in any proportion. For the consequences which can be drawn concerning the cases of unequal angles, do not lead to general truths, without some reference to that peculiar case in which the angles are equal: and thus it becomes necessary to single out and define that special case, marking it by a special phrase. And this definition not only gives complete and distinct knowledge what a right angle is, to any one who can form the conception of an angle in general; but also supplies a principle from which all the properties of right angles may be deduced.

3. *Axioms.*—With regard to other conceptions also, as circles, squares, and the like, it is possible to lay down definitions which are a sufficient basis for our reasoning, so far as such figures are concerned. But, besides these definitions, it has been found necessary to introduce certain axioms among the fundamental principles of geometry. These are of the simplest character; for instance, that two straight lines cannot cut each

other in more than one point, and an axiom concerning parallel lines. Like the definitions, these axioms flow from the Idea of Space, and present that idea under various aspects. They are different from the definitions; nor can the definitions be made to take the place of the axioms in the reasoning by which elementary geometrical properties are established. For example, the definition of parallel straight lines is, that they are such as, however far continued, can never meet: but, in order to reason concerning such lines, we must further adopt some axiom respecting them: for example, we may very conveniently take this axiom; that two straight lines which cut one another are not both of them parallel to a third straight line*. The definition and the axiom are seen to be inseparably connected by our intuition of the properties of space; but the axiom cannot be proved from the definition, by any rigorous deductive demonstration. And if we were to take any other definition of two parallel straight lines, (as that they are both perpendicular to a third straight line,) we should still, at some point or other of our progress, fall in with the same difficulty of demonstratively establishing their properties without some further assumption.

4. Thus the elementary properties of figures, which are the basis of our geometry, are necessary results of our Idea of Space; and are connected with each other by the nature of that idea, and not merely by our hypotheses and constructions. Definitions and axioms must be combined, in order to express this idea so far as the purposes of demonstrative reasoning require. These verbal enunciations of the results of the idea cannot be made to depend on each other by logical consequence; but have a mutual dependence of a more intimate kind,

* This axiom is simpler and more convenient than that of Euclid. It is employed by the late Professor Playfair in his *Geometry*.

which words cannot fully convey. It is not possible to resolve these truths into certain *hypotheses*, of which all the rest shall be the necessary logical consequence. The necessity is not hypothetical, but intuitive. The axioms require not to be granted, but to be seen. If any one were to assent to them without seeing them to be true, his assent would be of no avail for purposes of reasoning: for he would be also unable to see in what cases they might be applied. The clear possession of the Idea of Space is the first requisite for all geometrical reasoning; and this clearness of idea may be tested by examining whether the axioms offer themselves to the mind as evident.

5. The necessity of ideas added to sensations, in order to produce knowledge, has often been overlooked or denied in modern times. The ground of necessary truth which ideas supply being thus lost, it was conceived that there still remained a ground of necessity in definitions;—that we might have necessary truths, by asserting especially what the definition implicity involved in general. It was held, also, that this was the case in geometry:—that all the properties of a circle, for instance, were implicitly contained in the definition of a circle. That this alone is not the ground of the necessity of the truths which regard the circle,—that we could not in this way unfold a definition into proportions, without possessing an intuition of the relations to which the definition led,—has already been shown. But the insufficiency of the above account of the grounds of necessary geometrical truth appeared in another way also. It was found impossible to lay down a system of definitions out of which alone the whole of geometrical truth could be evolved. It was found that axioms could not be superseded. No definition of a straight line could be given which rendered the axiom concerning

straight lines superfluous. And thus it appeared that the source of geometrical truths was not definition alone; and we find in this result a confirmation of the doctrine which we are here urging, that this source of truth is to be found in the form or conditions of our perception;—in the idea which we unavoidably combine with the impressions of sense;—in the activity, and not in the passivity of the mind*.

6. This will appear further when we come to consider the mode in which we exercise our observation upon the relations of space. But we may, in the first place, make a remark which tends to show the connexion between our conception of a straight line, and the axiom which is made the foundation of our reasonings concerning space. The axiom is this;—that two straight lines, which have both their ends joined, cannot have the intervening parts separated so as to inclose a space. The necessity of this axiom is of exactly the same kind as the necessity of the definition of a right angle, of which we have already spoken. For as the line standing on another makes *right angles* when it makes the angles on the two sides of it equal; so a line is a *straight line* when it makes the two portions of space, on the two sides of it, similar. And as there is only a single position of the line first mentioned, which can make the angles equal, so there is only a single form of a line which can make the spaces near the line similar on one side and on the other: and therefore there cannot be two straight lines, such as the axiom describes,

* I formerly stated views similar to these in some "Remarks" appended to a work which I termed *The Mechanical Euclid*, published in 1837. These Remarks, so far as they bear upon the question here discussed, were noticed and controverted in No. 135 of the *Edinburgh Review*. As an examination of the reviewer's objections may serve further to illustrate the subject, I shall annex to this chapter an answer to the article to which I have referred.

which, between the same limits, give two different boundaries to space thus separated. And thus we see a reason for the axiom. Perhaps this view may be further elucidated if we take a leaf of paper, double it, and crease the folded edge. We shall thus obtain a straight line at the folded edge; and this line divides the surface of the paper, as it was originally spread out, into two similar spaces. And that these spaces are similar so far as the fold which separates them is concerned, appears from this;—that these two parts coincide when the paper is doubled. And thus a fold in a sheet of paper at the same time illustrates the definition of a straight line according to the above view, and confirms the axiom that two such lines cannot enclose a space.

If the separation of the two parts of space were made by any other than a straight line; if, for instance, the paper were cut by a concave line; then, on turning one of the parts over, it is easy to see that the edge of one part being concave one way, and the edge of the other part concave the other way, these two lines would enclose a space. And each of them would divide the whole space into two portions which were not similar; for one portion would have a concave edge, and the other a convex edge. Between any two points, there might be innumerable lines drawn, some, convex one way, and some, convex the other way; but the straight line is the line which is not convex either one way or the other; it is the single medium standard from which the others may deviate in opposite directions.

Such considerations as these show sufficiently that the singleness of the straight line which connects any two points is a result of our fundamental conceptions of space. But yet the above conceptions of the similar form of the two parts of space on the two sides of a line, and of the form of a line which is intermediate among all other forms, are of so vague a nature, that they cannot fitly be made the basis of our elementary geometry; and they are far more conveniently replaced, as they have been in almost all treatises of geometry, by the axiom, that two straight lines cannot inclose a space.

7. But we may remark that, in what precedes, we have considered space only under one of its aspects:—as a plane. The sheet of paper which we assumed in order to illustrate the nature of a straight line, was supposed to be perfectly *plane* or *flat*: for otherwise, by folding it, we might obtain a line not straight. Now this assumption of a plane appears to take for granted that very conception of a straight line which the sheet was employed to illustrate; for the definition of a plane given in the Elements of Geometry is, that it is a surface on which lie all straight lines drawn from one point of the surface to another. And thus the explanation above given of the nature of a straight line,—that it divides a plane space into similar portions on each side,—appears to be imperfect or nugatory.

To this we reply, that the explanation must be rendered complete and valid by deriving the conception of a plane from considerations of the same kind as those which we employed for a straight line. Any portion of solid space may be divided into two portions by surfaces passing through any given line or boundaries. And these surfaces may be convex either on one side or on the other, and they admit of innumerable changes from being convex on one side to being convex on the other in any degree. So long as the surface is convex either way, the two portions of space which it separates are not similar, one having a convex and the other a concave boundary. But there is a certain intermediate position of the surface, in which position the two portions of space which it divides have their boundaries exactly similar.

In this position, the surface is neither convex nor concave, but plane. And thus a plane surface is determined by this condition—of its being that single surface which is the intermediate form among all convex and concave surfaces by which solid space can be divided,—and of its separating such space into two portions, of which the boundaries, though they are the same surface in two opposite positions, are exactly similar.

Thus a plane is the simplest and most symmetrical boundary by which a solid can be divided; and a straight line is the simplest and most symmetrical boundary by which a plane can be separated. These conceptions are obtained by considering the boundaries of an interminable space, capable of imaginary division in every direction. And as a limited space may be separated into two parts by a plane, and a plane again separated into two parts by a straight line, so a line is divided into two portions by a point, which is the common boundary of the two portions; the end of the one and the beginning of the other portion having itself no magnitude, form, or parts.

8. The geometrical properties of planes and solids are deducible from the first principles of the Elements, without any new axioms; the definition of a plane above quoted,—that all straight lines joining its points lie in the plane,—being a sufficient basis for all reasoning upon these subjects. And thus, the views which we have presented of the nature of space being verbally expressed by means of certain definitions and axioms, become the groundwork of a long series of deductive reasoning, by which is established a very large and curious collection of truths, namely, the whole science of Elementary Plane and Solid Geometry.

This science is one of indispensable use and constant reference, for every student of the laws of nature; for the relations of space and number are the alphabet in which those laws are written. But besides the interest and importance of this kind which geometry possesses, it has a great and peculiar value for all who wish to understand the foundations of human knowledge, and the methods by which it is acquired. For the student of geometry acquires, with a degree of insight and clearness which the unmathematical reader can but feebly imagine, a conviction that there are necessary truths, many of them of a very complex and striking character; and that a few of the most simple and self-evident truths which it is possible for the mind of man to apprehend, may, by systematic deduction, lead to the most remote and unexpected results.

In pursuing such philosophical researches as that in which we are now engaged, it is of great advantage to the speculator to have cultivated to some extent the study of geometry; since by this study he may become fully aware of such features in human knowledge as those which we have mentioned. By the aid of the lesson thus learned from the contemplation of geometrical truths, we have been endeavouring to establish those further doctrines;—that these truths are but different aspects of the same Fundamental Idea, and that the grounds of the necessity which these truths possess reside in the Idea from which they flow, this Idea not being a derivative result of experience, but its primary rule. When the reader has obtained a clear and satisfactory view of these doctrines, so far as they are applicable to our knowledge concerning space, he has, we may trust, overcome the main difficulty which will occur in following the course of the speculations now presented to him. He is then prepared to go forwards with us; to see over how wide a field the same doctrines are applicable: and how rich and various a harvest of knowledge springs from these seemingly scanty principles.

But before we quit the subject now under our consideration, we shall endeavour to answer some objections which have been made to the views here presented; and shall attempt to illustrate further the active powers which we have ascribed to the mind.

CHAPTER V.

OF SOME OBJECTIONS WHICH HAVE BEEN MADE TO THE DOCTRINES STATED IN THE PREVIOUS CHAPTER*.

THE *Edinburgh Review*, No. CXXXV., contains a critique on a work termed *The Mechanical Euclid*, in which opinions were delivered to nearly the same effect as some of those stated in the last chapter, and in Chapter XI. of the First Book. Although I believe that there are no arguments used by the reviewer to which the answers will not suggest themselves in the mind of any one who has read with attention what has been said in the preceding chapters (except, perhaps, one or two remarks which have reference to mechanical ideas), it may serve to

* In order to render the present chapter more intelligible, it may be proper to state briefly the arguments which gave occasion to the review. After noticing Stewart's assertions, that the certainty of mathematical reasoning arises from its depending upon definitions, and that mathematical truth is hypothetical; I urged,—that no one has yet been able to construct a system of mathematical truths by the aid of definitions alone; that a definition would not be admissible or applicable except it agreed with a distinct conception in the mind; that the definitions which we employ in mathematics are not arbitrary or hypothetical, but necessary definitions; that if Stewart had taken as his examples of axioms the peculiar geometrical axioms, his assertions would have been obviously erroneous; and that the real foundation of the truths of mathematics is the Idea of Space, which may be expressed (for purposes of demonstration) partly by definitions and partly by axioms.

illustrate the subject if I reply to the objections directly, taking them as the reviewer has stated them.

1. I had dissented from Stewart's assertion that mathematical truth is hypothetical, or depends upon arbitrary definitions; since we understand by an hypothesis a supposition, not only which we may make, but may abstain from making, or may replace by a different supposition; whereas the definitions and hypotheses of geometry are necessarily such as they are, and cannot be altered or excluded. The reviewer (p. 84), informs us that he understands Stewart, when he speaks of hypotheses and definitions being the foundation of geometry, to speak of the hypothesis that real objects correspond to our geometrical definitions. "*If* a crystal be an exact hexahedron, the geometrical properties of the hexahedron may be predicated of that crystal." To this I reply, —that such hypotheses as this are the grounds of our applications of geometrical truths to real objects, but can in no way be said to be the foundation of the truths themselves;—that I do not think that the sense which the reviewer gives was Stewart's meaning;—but that if it was, this view of the use of mathematics does not at all affect the question which both he and I proposed to discuss, which was, the ground of mathematical certainty. I may add, that whether a crystal be an exact hexahedron, is a matter of observation and measurement, not of definition. I think the reader can have no difficulty in seeing how little my doctrine is affected by the connexion on which the reviewer thus insists. I have asserted that the proposition which affirms the square on the diagonal of a rectangle to be equal to the squares on two sides, does not rest upon arbitrary hypotheses; the objector answers, that the proposition that the square on the diagonal *of this page* is equal to the squares on the sides, depends upon the arbitrary hypothesis that the page is a rect-

angle. Even if this fact were a matter of arbitrary hypothesis, what could it have to do with the general geometrical proposition? How could a single fact, observed or hypothetical, affect a universal and necessary truth, which would be equally true if the fact were false? If there be nothing arbitrary or hypothetical in geometry till we come to such steps in its application, it is plain that the truths themselves are not hypothetical; which is the question for us to decide.

2. The reviewer then (p. 85), considers the doctrine that axioms as well as definitions are the foundations of geometry; and here he strangely narrows and confuses the discussion by making himself the advocate of Stewart, instead of arguing the question itself. I had asserted that some axioms are necessary as the foundations of mathematical reasoning, in addition to the definitions. If Stewart did not intend to discuss this question, I had no concern with what he had said about axioms. But I had every reason to believe that this was the question which Stewart did intend to discuss. I conceive there is no doubt that he intended to give an opinion upon the grounds of mathematical reasoning in general. For he begins his discussions (*Elements*, Vol. II., p. 38) by contesting Reid's opinion on this subject, which is stated generally; and he refers again to the same subject, asserting in general terms, that the first principles of mathematics are not axioms but definitions. If, then, afterwards, he made his proof narrower than his assertion;—if having declared that no axioms are necessary, he afterwards limited himself to showing that seven out of twelve of Euclid's axioms are barren truisms, it was no concern of mine to contest this assertion, which left my thesis untouched. I had asserted that the proper geometrical axioms (that two straight lines cannot inclose a space, and the axiom about parallel lines) are indispensable in

geometry. What account the reviewer gives of these axioms we shall soon see; but if Stewart allowed them to be axioms necessary to geometrical reasoning, he overturned his own assertion as to the foundations of such reasoning; and if he said nothing decisive about these axioms, which are the points on which the battle must turn, he left his assertion altogether unproved; nor was it necessary for me to pursue the war into a barren and unimportant corner, when the metropolis was surrendered. The reviewer's exultation that I have not contested the first seven axioms is an amusing example of the self-complacent zeal of advocacy.

3. But let us turn to the material point,—the proper geometrical axioms. What is the reviewer's account of these? Which side of the alternative does he adopt? Do they depend upon the definitions, and is he prepared to show the dependence? Or are they superfluous, and can he erect the structure of geometry without their aid? One of these two courses, it would seem, he must take. For we both begin by asserting the excellence of geometry as an example of demonstrated truth. It is precisely this attribute which gives an interest to our present inquiry. How, then, does the reviewer explain this excellence on his views? How does he reckon the foundation courses of the edifice which we agree in considering as a perfect example of intellectual building?

I presume I may take, as his answer to this question, his hypothetical statement of what Stewart would have said, (p. 87,) on the supposition that there had been, among the foundations of geometry, self-evident indemonstrable truths: although it is certainly strange that the reviewer should not venture to make up his mind as to the truth or falsehood of this supposition. If there were such truths they would be, he says, "legitimate filiations" of the definitions. They would be involved in the definitions. And again he speaks of the foundation of the geometrical doctrine of parallels as a flaw, and as a truth which requires, but has not received demonstration. And yet again, he tells us that each of these supposed axioms (Euclid's twelfth, for instance), is "merely an indication of the point at which geometry fails to perform that which it undertakes to perform" (p. 91); and that in reality her truths are not yet demonstrated. The amount of this is, that the geometrical axioms are to be held to be *legitimate filiations* of the definitions, because though certainly true, they cannot be proved from the definitions; that they are involved in the definitions, although they cannot be evolved out of them; and that rather than admit that they have any other origin than the definitions, we are to proclaim that geometry has failed to perform what she undertakes to perform.

To this I reply—that I cannot understand what is meant by "legitimate filiations" of principles, if the phrase not mean consequences of such principles established by rigorous and formal demonstrations;—that the reviewer, if he claims any real signification for his phrase, must substantiate the meaning of it by such a demonstration; he must establish his "legitimate filiation" by a genealogical table in a satisfactory form. When this cannot be done, to assert, notwithstanding, that the propositions are involved in the definitions, is a mere begging the question; and to excuse this defect by saying that geometry fails to perform what she has promised, is to calumniate the character of that science which we profess to make our standard, rather than abandon an arbitrary and unproved assertion respecting the real grounds of her excellence. I add, further, that if the doctrine of parallel lines, or any other geometrical doctrine of which we see the truth, with the most perfect insight of its necessity, have not hitherto received demonstration to the satisfaction of any school of reasoners, the defect must arise from their erroneous views of the nature of demonstrations, and the grounds of mathematical certainty.

4. I conceive, then, that the reviewer has failed altogether to disprove the doctrine that the axioms of geometry are necessary as a part of the foundations of the science. I had asserted further that these axioms supply what the definitions leave deficient; and that they, along with definitions, serve to present the idea of space under such aspects that we can reason logically concerning it. To this the reviewer opposes (p. 96) the common opinion that a perfect definition is a complete explanation of a name, and that the test of its perfection is, that we may substitute the definition for the name wherever it occurs. I reply, that my doctrine, that a definition expresses a part, but not the whole, of the essential characters of an idea, is certainly at variance with an opinion sometimes maintained, that a definition merely explains a word, and should explain it so fully that it may always replace it. The error of this common opinion may, I think, be shown from considerations such as these;—that if we undertake to explain one word by several, we may be called upon, on the same ground, to explain each of these several by others, and that in this way we can reach no limit nor resting-place;—that in point of fact, it is not found to lead to clearness, but to obscurity, when in the discussion of general principles, we thus substitute definitions for single terms;—that even if this be done, we cannot reason without conceiving what the terms mean; —and that, in doing this, the relations of our conceptions, and not the arbitrary equivalence of two forms of expression, are the foundations of our reasoning.

5. The reviewer conceives that some of the so-called axioms are really definitions. The axiom, that "magnitudes which coincide with each other, that is, which fill

the same space, are equal," is a definition of geometrical *equality:* the axiom, that "the whole is greater than its part," is a definition of *whole* and *part.* But surely there are very serious objections to this view. It would seem more natural to say, if the former axiom is a definition of the word *equal*, that the latter is a definition of the word *greater*. And how can one short phrase define two terms? If I say, "the heat of summer is greater than the heat of winter," does this assertion define anything, though the proposition is perfectly intelligible and distinct? I think, then, that this attempt to reduce these axioms to definitions is quite untenable.

6. I have stated that a definition can be of no use, except we can conceive the possibility and truth of the property connected with it; and that if we do conceive this, we may rightly begin our reasonings by stating the property as an axiom; which Euclid does, in the case of straight lines and of parallels. The reviewer inquires, (p. 92,) whether I am prepared to extend this doctrine to the case of circles, for which the reasoning is usually rested upon the definition;—whether I would replace this definition by an axiom, asserting the possibility of such a circle. To this I might reply, that it is not at all incumbent upon me to assent to such a change; for I have all along stated that it is indifferent whether the fundamental properties from which we reason be exhibited as definitions or as axioms, provided their necessity be clearly seen. But I am ready to declare that I think the form of our geometry would be not at all the worse, if, instead of the usual definition of a circle,—"that it is a figure contained by one line, which is called the circumference, and which is such, that all straight lines drawn from a certain point within the circumference are equal to one another,"—we were to substitute an axiom and a definition, as follows:—

Axiom. If a line be drawn so as to be at every point equally distant from a certain point, this line will return into itself, or will be *one* line including a space.

Definition. The space is called a *circle*, the line the *circumference*, and the point the *center*.

And this being done, it would be true, as the reviewer remarks, that geometry cannot stir *one* step without resting on an axiom. And I do not at all hesitate to say, that the above axiom, expressed or understood, is no less necessary than the definition, and is tacitly assumed in every proposition into which circles enter.

7. I have, I think, now disposed of the principal objections which bear upon the proper axioms of geometry. The principles which are stated as the first seven axioms of Euclid's *Elements*, need not, as I have said, be here discussed. They are principles which refer, not to Space in particular, but to Quantity in general: such, for instance, as these; "If equals be added to equals the wholes are equal;"—"If equals be taken from equals the remainders are equal." But I will make an observation or two upon them before I proceed.

Both Locke and Stewart have spoken of these axioms as barren truisms: as propositions from which it is not possible to deduce a single inference: and the reviewer asserts that they are not first principles, but laws of thought. (p. 88.) To this last expression I am willing to assent; but I would add, that not only these, but all the principles which express the fundamental conditions of our knowledge, may with equal propriety be termed laws of thought; for these principles depend upon our ideas, and regulate the active operations of the mind, by which coherence and connexion are given to its passive impressions. But the assertion that no conclusions can be drawn from simple axioms, or laws of human thought, which regard quantity, is by no means true. The whole of arithmetic,—for instance, the rules for the multiplication and division of large numbers, for finding a common measure, and, in short, a vast body of theory respecting numbers,—rests upon no other foundation than such axioms as have been just noticed, that if equals be added to equals the wholes will be equal. And even when Locke's assertion, that from these axioms no truths can be deduced, is modified by Stewart and the reviewer, and limited to *geometrical* truths, it is hardly tenable (although, in fact, it matters little to our argument whether it is or no). For the greater part of the Seventh Book of Euclid's *Elements*, (on Commensurable and Incommensurable Quantities,) and the Fifth Book, (on Proportion,) depend upon these axioms, with the addition only of the definition or axiom (for it may be stated either way) which expresses the idea of proportionality in numbers. So that the attempt to disprove the necessity and use of axioms, as principles of reasoning, fails even when we take those instances which the opponents consider as the more manifestly favourable to their doctrine.

8. But perhaps the question may have already suggested itself to the reader's mind, of what use can it be formally to state such principles as these, (for example, that if equals be added to equals the wholes are equal,) since, whether stated or no, they will be assumed in our reasoning? And how can such principles be said to be necessary, when our proof proceeds equally well without any reference to them? And the answer is, that it is precisely because these are the common principles of reasoning, which we naturally employ without specially contemplating them, that they require to be separated from the other steps and formally stated, when we *analyze* the demonstrations which we have obtained In every mental process many principles are combined

and abbreviated, and thus in some measure concealed and obscured. In analyzing these processes, the combination must be resolved, and the abbreviation expanded, and thus the appearance is presented of a pedantic and superfluous formality. But that which is superfluous for proof, is necessary for the analysis of proof. In order to exhibit the conditions of demonstration distinctly, they must be exhibited formally. In the same manner, in demonstration we do not usually express every step in the form of a syllogism, but we see the grounds of the conclusiveness of a demonstration, by resolving it into syllogisms. Neither axioms nor syllogisms are necessary for conviction; but they are necessary to display the conditions under which conviction becomes inevitable. The application of a single one of the axioms just spoken of is so minute a step in the proof, that it appears pedantic to give it a marked place; but the very essence of demonstration consists in this, that it is composed of an indissoluble succession of such minute steps. The admirable circumstance is, that by the accumulation of such apparently imperceptible advances, we can in the end make so vast and so sure a progress. The completeness of the analysis of our knowledge appears in the smallness of the elements into which it is thus resolved. The minuteness of any of these elements of truth, of axioms for instance, does not prevent their being as essential as others which are more obvious. And any attempt to assume one kind of element only, when the course of our analysis brings before us two or more kinds, is altogether unphilosophical. Axioms and definitions are the proximate constituent principles of our demonstrations; and the intimate bond which connects together a definition and an axiom on the same subject is not truly expressed by asserting the latter to be derived from the former. This bond of connexion exists in the mind of the reasoner, in his conception of *that* to which both definition and axiom refer, and consequently in the general Fundamental Idea of which that conception is a modification.

Chapter VI.

OF THE PERCEPTION OF SPACE.

1. According to the views above explained, certain of the impressions of our senses convey to us the perception of objects as existing in space; inasmuch as by the constitution of our minds we cannot receive those impressions otherwise than in a certain form, involving such a manner of existence. But the question deserves to be asked, *What* are the impressions of sense by which we thus become acquainted with space and its relations? And as we have seen that this idea of space implies an act of the mind as well as an impression on the sense, what manifestations do we find of this activity of the mind, in our observation of the external world?

It is evident that sight and touch are the senses by which the relations of space are perceived, principally or entirely. It does not appear that an odour, or a feeling of warmth or cold, would, independently of experience, suggest to us the conception of a space surrounding us. But when we *see* objects, we see that they are extended and occupy space; when we *touch* them, we feel that they are in a space in which we also are. We have before our eyes any object, for instance, a board covered with geometrical diagrams; and we distinctly perceive, by vision, those lines of which the relations are the subjects of our mathematical reasoning. Again, we see before us a solid object, a cubical box for instance; we see that it is within reach; we stretch out the hand and perceive by the touch that it has sides, edges, corners, which we had already perceived by vision.

2. Probably most persons do not generally apprehend that there is any material difference in these two cases;—that there are any different acts of mind concerned in perceiving by sight a mathematical diagram upon paper, and a solid cube lying on a table. Yet it is not difficult to show that, in the latter case at least, the perception of the shape of the object is not immediate. A very little attention teaches us that there is an act of judgment as well as a mere impression of sense requisite, in order that we may see any solid object. For there is no visible appearance which is inseparably connected with solidity. If a picture of a cube be rightly drawn in perspective and skilfully shaded, the impression upon the sense is the same as if it were a real cube. The picture may be mistaken for a solid object. But it is clear that, in this case, the solidity is given to the object by an act of mental judgment. All that is seen is outline and shade, figures and colours on a flat board. The solid angles and edges, the relation of the faces of the figure by which they form a cube, are matters of inference. This, which is evident in the case of the pictured cube, is true in all vision whatever. We see a scene before us on which are various figures and colours, but the eye cannot see more. It sees length and breadth, but no third dimension. In order to know that there are solids, we must infer as well as see. And this we do readily and constantly; so familiarly, indeed, that we do not perceive the operation. Yet we may detect this latent process in many ways; for instance, by attending to cases in which the habit of drawing such inferences misleads us. Most persons have experienced this delusion in looking at a scene in a theatre, and especially that kind of scene which is called a *diorama*, when the

interior of a building is represented. In these cases, the perspective representations of the various members of the architecture and decoration impress us almost irresistibly with the conviction that we have before us a space of great extent and complex form, instead of a flat painted canvass. Here, at least, the space is our own creation, but yet here, it is manifestly created by the same act of thought as if we were really in the palace or the cathedral of which the halls and aisles thus seem to inclose us. And the act by which we thus create space of three dimensions out of visible extent of length and breadth, is constantly and imperceptibly going on. We are perpetually interpreting in this manner the language of the visible world. From the appearances of things which we directly see, we are constantly inferring that which we cannot directly see,—their distance from us, and the position of their parts.

3. The characters which we thus interpret are various. They are, for instance, the visible forms, colours, and shades of the parts, understood according to the maxims of perspective; (for of perspective every one has a practical knowledge, as every one has of grammar;) the effort by which we fix both our eyes on the same object, and adjust each eye to distinct vision; and the like. The right interpretation of the information which such circumstances give us respecting the true forms and distances of things, is gradually learned; the lesson being begun in our earliest infancy, and inculcated upon us every hour during which we use our eyes. The completeness with which the lesson is mastered is truly admirable; for we forget that our conclusion is obtained indirectly, and mistake a judgment on evidence for an intuitive perception. We see the breadth of the street, as clearly and readily as we see the house on the other side of it; and we see the house

to be square, however obliquely it be presented to us. This, however, by no means throws any doubt or difficulty on the doctrine that in all these cases we do interpret and infer. The rapidity of the process, and the unconsciousness of the effort, are not more remarkable in this case than they are when we understand the meaning of the speech which we hear, or of the book which we read. In these latter cases we merely hear noises or see black marks; but we make, out of these elements, thought and feeling, without being aware of the act by which we do so. And by an exactly similar process we see a variously-coloured expanse, and collect from it a space occupied by solid objects. In both cases the act of interpretation is become so habitual that we can hardly stop short at the mere impression of sense.

4. But yet there are various ways in which we may satisfy ourselves that these two parts of the process of seeing objects are distinct. To separate these operations is precisely the task which the artist has to execute in making a drawing of what he sees. He has to recover the consciousness of his real and genuine sensations, and to discern the lines of objects as they appear. This at first he finds difficult; for he is tempted to draw what he knows of the forms of visible objects, and not what he sees: but as he improves in his art, he learns to put on paper what he sees only, separated from what he infers, in order that thus the inference, and with it a conception like that of the reality, may be left to the spectator. And thus the natural process of vision is the habit of seeing that which cannot be seen; and the difficulty of the art of drawing consists in learning not to see more than is visible.

5. But again; even in the simplest drawing we exhibit something which we do not see. However

slight is our representation of objects, it contains something which we create for ourselves. For we draw an *outline*. Now an outline has no existence in nature. There are no visible lines presented to the eye by a group of figures. We separate each figure from the rest, and the boundary by which we do this is the outline of the figure; and the like may be said of each member of every figure. A painter of our own times has made this remark in a work upon his art*. "The effect which natural objects produce upon our sense of vision is that of a number of parts, or distinct masses of form and colour, and not of lines. But when we endeavour to represent by painting the objects which are before us, or which invention supplies to our minds, the first and the simplest means we resort to is this picture, by which we separate the form of each object from those that surround it, marking its boundary, the extreme extent of its dimensions in every direction, as impressed on our vision: and this is termed drawing its outline."

6. Again, there are other ways in which we see clear manifestations of the act of thought by which we assign to the parts of objects their relations in space, the impressions of sense being merely subservient to this act. If we look at a medal through a glass which inverts it, we see the figures upon it become concave depressions instead of projecting convexities; for the light which illuminates the nearer side of the convexity will be transferred to the opposite side by the apparent inversion of the medal, and will thus imply a hollow in which the side nearest the light gathers the shade. Here our decision as to which part is nearest to us, has reference to the side from which the light comes. In other cases the decision is more spontaneous. If we draw black outlines, such as represent the edges of a cube seen

* Phillips *On Painting*.

in perspective, certain of the lines will cross each other; and we may make this cube appear to assume two different positions, by determining in our own mind that the lines which belong to one end of the cube shall be understood to be before or to be behind those which they cross. Here an act of the will, operating upon the same sensible image, gives us two cubes, occupying two entirely different positions. Again, many persons may have observed that when a windmill in motion at a distance from us, (so that the outline of the sails only is seen,) stands obliquely to the eye, we may, by an effort of thought, make the obliquity assume one or the other of two positions; and as we do this, the sails, which in one instance appear to turn from right to left, in the other case turn from left to right. A person a little familiar with this mental effort, can invert the motion as often as he pleases, so long as the conditions of form and light do not offer a manifest contradiction to either position.

Thus we have these abundant and various manifestations of the activity of the mind, in the process by which we collect from vision the relations of solid space of three dimensions. But we must further make some remarks on the process by which we perceive mere visible figure; and also, on the mode in which we perceive the relations of space by the touch; and first, of the latter subject.

7. The opinion above illustrated, that our sight does not give us a direct knowledge of the relations of solid space, and that this knowledge is acquired only by an inference of the mind, was first clearly taught by the celebrated Bishop Berkeley*, and is a doctrine now generally assented to by metaphysical speculators.

But does the sense of *touch* give us directly a knowledge of space? This is a question which has attracted considerable notice in recent times; and new light has

* *Theory of Vision.*

been thrown upon it in a degree which is very remarkable, when we consider that the philosophy of perception has been a prominent subject of inquiry from the earliest times. Two philosophers, advancing to this inquiry from different sides, the one a metaphysician, the other a physiologist, have independently arrived at the conviction that the long current opinion, according to which we acquire a knowledge of space by the sense of touch, is erroneous. And the doctrine which they teach instead of the ancient errour, has a very important bearing upon the principle which we are endeavouring to establish,—that our knowledge of space and its properties is derived rather from the active operations than from the passive impressions of the percipient mind.

Undoubtedly the persuasion that we acquire a knowledge of form by the touch is very obviously suggested by our common habits. If we wish to know the form of any body in the dark, or to correct the impressions conveyed by sight, when we suspect them to be false, we have only, it seems to us, at least at first, to stretch forth the hand and touch the object; and we learn its shape with no chance of error. In these cases, form appears to be as immediate a perception of the sense of touch, as colour is of the sense of sight.

8. But is this perception really the result of the passive sense of touch merely? Against such an opinion Dr. Brown, the metaphysician of whom I speak, urges* that the feeling of touch alone, when any object is applied to the hand, or any other part of the body, can no more convey the conception of form or extension, than the sensation of an odour or a taste can do, except we have already some knowledge of the relative position of the parts of our bodies; that is, except we are already in possession of an idea of space, and have, in our minds,

* *Lectures*, Vol. I. p. 459, (1824).

referred our limbs to their positions; which is to suppose the conception of form already acquired.

9. By what faculty then do we originally acquire our conceptions of the relations of position? Brown answers by the *muscular sense;* that is, by the conscious exertions of the various muscles by which we move our limbs. When we feel out the form and position of bodies by the hand, our knowledge is acquired, not by the mere touch of the body, but by perceiving the course the fingers must take in order to follow the surface of the body, or to pass from one body to another. We are conscious of the slightest of the volitions by which we thus feel out form and place; we know whether we move the finger to the right or left, up or down, to us or from us, through a large or a small space; and all these conscious acts are bound together and regulated in our minds by an idea of an extended space in which they are performed. That this idea of space is not borrowed from the sight, and transferred to the muscular feelings by habit, is evident. For a man born blind can feel out his way with his staff, and has his conceptions of position determined by the conditions of space, no less than one who has the use of his eyes. And the muscular consciousness which reveals to us the position of objects and parts of objects, when we feel them out by means of the hand, shews itself in a thousand other ways, and in all our limbs: for our habits of standing, walking, and all other attitudes and motions, are regulated by our feeling of our position and that of surrounding objects. And thus, we cannot touch any object without learning something respecting its position; not that the sense of touch directly conveys such knowledge; but we have already learnt, from the muscular sense, constantly exercised, the position of the limb which the object thus touches.

10. The justice of this distinction will, I think, be assented to by all persons who attend steadily to the process itself, and might be maintained by many forcible reasons. Perhaps one of the most striking evidences in its favour is that, as I have already intimated, it is the opinion to which another distinguished philosopher, Sir Charles Bell, has been led, reasoning entirely upon physiological principles. From his researches it resulted that besides the nerves which convey the impulse of the will from the brain to the muscle, by which every motion of our limbs is produced, there is another set of nerves which carry back to the brain a sense of the condition of the muscle, and thus regulate its activity; and give us the consciousness of our position and relation to surrounding objects. The motion of the hand and fingers, or the consciousness of this motion, must be combined with the sense of touch properly so called, in order to make an inlet to the knowledge of such relations. This consciousness of muscular exertion, which he has called a sixth sense*, is our guide, Sir C. Bell shows, in the common practical government of our motions; and he states that having given this explanation of perception as a physiological doctrine, he had afterwards with satisfaction seen it confirmed by Dr. Brown's speculations.

11. Thus it appears that our consciousness of the relations of space is inseparably and fundamentally connected with our own actions in space. We perceive only while we act; our sensations require to be interpreted by our volitions. The apprehension of extension and figure is far from being a process in which we are inert and passive. We draw lines with our fingers; we construct surfaces by curving our hands; we generate spaces by the motion of our arms. When the geometer bids us form lines, or surfaces, or solids by motion, he intends his

* *Bridgewater Treatise*, p. 195. *Phil. Trans.* 1826, Pt. II., p. 167.

injunction to be taken as hypothetical only; we need only conceive such motions. But yet this hypothesis represents truly the origin of our knowledge; we perceive spaces by motion at first, as we conceive spaces by motion afterwards:—or if not always by actual motion, at least by potential. If we perceive the length of a staff by holding its two ends in our two hands without running the finger along it, this is because by habitual motion we have already acquired a measure of the distance of our hands in any attitude of which we are conscious. Even in the simplest case, our perceptions are derived not from the touch, but from the sixth sense; and this sixth sense at least, whatever may be the case with the other five, implies an active mind along with the passive sense.

12. Upon attentive consideration, it will be clear that a large portion of the perceptions respecting space which appear at first to be obtained by sight alone, are, in fact, acquired by means of this sixth sense. Thus we consider the visible sky as a single surface surrounding us and returning into itself, and thus forming a hemisphere. But such a mode of conceiving an object of vision could never have occurred to us, if we had not been able to turn our heads, to follow this surface, to pursue it till we find it returning into itself. And when we have done this, we necessarily present it to ourselves as a concave inclosure within which we are. The sense of sight alone, without the power of muscular motion, could not have led us to view the sky as a vault or hemisphere. Under such circumstances, we should have perceived only what was presented to the eye in one position; and if different appearances had been presented in succession, we could not have connected them as parts of the same picture, for want of any perception of their relative position. They would have been so many detached and incoherent visual sensations. The muscular sense con-

nects their parts into a whole, making them to be only different portions of one universal scene*.

13. These considerations point out the fallacy of a very curious representation made by Dr. Reid, of the convictions to which man would be led, if he possessed vision without the sense of touch. To illustrate this subject, Reid uses the fiction of a nation whom he terms the *Idomenians*, who have no sense except that of sight. He describes their notions of the relations of space as being entirely different from ours. The axioms of their geometry are quite contradictory to our axioms. For example, it is held to be self-evident among them that two straight lines which intersect each other once, must intersect a second time; that the three angles of any triangle are *greater* than two right angles; and the like. These paradoxes are obtained by tracing the relations of lines on the surface of a concave sphere, which surrounds the spectator, and on which all visible appearances may be supposed to be presented to him. But from what is said above it appears that the notion of such a sphere, and such a connexion of visible objects which are seen in different directions, cannot be arrived at by sight alone.

* It has been objected to this view, that we might obtain a conception of the sky as a hemisphere, by being ourselves turned round, (as on a music-stool, for instance,) and thus seeing in succession all parts of the sky. But this assertion I conceive to be erroneous. By being thus turned round, we should see a number of pictures which we should put together as parts of a plane picture; and when we came round to the original point, we should have no possible means of deciding that it *was* the *same* point: it would appear only as a *repetition* of the picture. That sight, of itself, can give us only a plane picture, the doctrine of Berkeley, appears to be indisputable; and, no less so, the doctrine that it is the consciousness of our own action in space which puts together these pictures so that they cover the surface of a solid body. We can see length and breadth with our eyes, but we must thrust out our arm towards the flat surface, in order that we may, in our thoughts, combine a third dimension with the other two.

When the spectator combines in his conception the relations of long-drawn lines and large figures, as he sees them by turning his head to the right and to the left, upwards and downwards, he ceases to be an Idomenian. And thus our conceptions of the properties of space, derived through the exercise of one mode of perception, are not at variance with those obtained in another way; but all such conceptions, however produced or suggested, are in harmony with each other; being, as has already been said, only different aspects of the same idea.

14. If our perceptions of the position of objects around us do not depend on the sense of vision alone, but on the muscular feeling brought into play when we turn our head, it will obviously follow that the same is true when we turn the eye instead of the head. And thus we may learn the form of objects, not by looking at them with a fixed gaze, but by following the boundary of them with the eye. While the head is held perfectly still, the eye can rove along the outlines of visible objects, scrutinize each point in succession, and leap from one point to another; each such act being accompanied by a muscular consciousness which makes us aware of the direction in which the look is travelling. And we may thus gather information concerning the figures and places which we trace out with the visual ray, as the blind man learns the forms of things which he traces out with his staff, being conscious of the motions of his hand.

15. This view of the mode in which the eye perceives position, which is thus supported by the analogy of other members employed for the same purpose, is further confirmed by Sir Charles Bell by physiological reasons. He teaches us that* when an object is seen we employ two senses: there is an impression on the retina; but we receive also the idea of position or relation in

* *Phil. Trans.*, 1823. On the Motions of the Eye.

space, which it is not the office of the retina to give, by our consciousness of the efforts of the voluntary muscles of the eye: and he has traced in detail the course of the nerves by which these muscles convey their information. The constant *searching* motion of the eye, as he terms it*, is the means by which we become aware of the position of objects about us.

16. It is not to our present purpose to follow the physiology of this subject; but we may notice that Sir C. Bell has examined the special circumstances which belong to this operation of the eye. We learn from him that the particular point of the eye which thus traces the forms of visible objects is a part of the retina which has been termed the *sensible spot;* being that part which is *most distinctly* sensible to the impressions of light and colour. This part, indeed, is not a spot of definite size and form, for it appears that proceeding from a certain point of the retina, the distinct sensibility diminishes on every side by degrees. And the searching motion of the eye arises from the desire which we instinctively feel of receiving upon the sensible spot the image of the object to which the attention is directed. We are uneasy and

* *Bridgewater Treatise,* p. 282. I have adopted, in writing the above, the views and expressions of Sir Charles Bell. The essential part of the doctrine there presented is, that the eye constantly makes efforts to turn, so that the image of an object to which our attention is drawn, shall fall upon a certain particular point of the retina; and that when the image falls upon any other point, the eye turns away from this oblique into the direct position. Other writers have maintained that the eye thus turns, not because the point on which the image falls in direct vision is the *most sensible* point, but that it is the point of *greatest distinctness* of vision. They urge that a small star, which disappears when the eye is turned full upon it, may often be seen by looking a little away from it: and hence, they infer that the parts of the retina removed from the spot of direct vision, are more sensible than it is. The facts are very curious, however they be explained, but they do not disturb the doctrine delivered in the text.

impatient till the eye is turned so that this is effected. And as our attention is transferred from point to point of the scene before us, the eye, and this point of the eye in particular, travel along with the thoughts; and the muscular sense, which tells us of these movements of the organ of vision, conveys to us a knowledge of the forms and places which we thus successively survey.

17. How much of activity there is in the process by which we perceive the outlines of objects appears further from the language by which we describe their forms. We apply to them not merely adjectives of form, but verbs of motion. An abrupt hill *starts* out of the plain; a beautiful figure has a *gliding* outline. We have

> The windy summit, wild and high,
> Roughly *rushing* on the sky.

These terms express the course of the eye as it follows the lines by which such forms are bounded and marked. In like manner another modern poet* says of Soracte, that it

> From out the plain
> *Heaves* like a long-swept wave about to break,
> And on the curl *hangs pausing.*

Thus the muscular sense, which is inseparably connected with an act originating in our own mind, not only gives us all that portion of our perceptions of space in which we use the sense of touch, but also, at least in a great measure, another large portion of such perceptions, in which we employ the sense of sight. As we have before seen that our *knowledge* of solid space and its properties is not conceivable in any other way than as the result of a mental act, governed by conditions depending on its own nature; so it now appears that our *perceptions* of visible figure are not obtained without an act performed under the same conditions. The sensations of touch and sight are subordinated to an idea which is

* Byron, *Ch. Har.* VI., st. 75.

the basis of our speculative knowledge concerning space and its relations; and this same idea is disclosed to our consciousness by its practically regulating our intercourse with the external world.

By considerations such as have been adduced and referred to, it is proved beyond doubt, that in a great number of cases our knowledge of form and position is acquired from the muscular sense, and not from sight directly:—for instance, in all cases in which we have before us objects so large and prospects so extensive that we cannot see the whole of them in one position of the eye*.

We now quit the consideration of the properties of Space, and consider the Idea of Time.

Chapter VII.

OF THE IDEA OF TIME.

1. Respecting the Idea of Time, we may make several of the same remarks which we made concerning

* The expression in the first edition was "large objects and extensive spaces." In the text as now given, I state a definite size and extent, within which the sight by itself can judge of position and figure.

The doctrine that we require the assistance of the muscular sense to enable us to perceive space of three dimensions, is not at all inconsistent with this other doctrine, that within the space which is seen by the fixed eye, we perceive the relative positions of points directly by vision, and that, consequently, we have a perception of *visible figure*.

Sir Charles Bell has said, (*Phil. Trans.* 1823, p. 181,) "It appears to me that the utmost ingenuity will be at a loss to devise an explanation of that power by which the eye becomes acquainted with the position and relation of objects, if the sense of muscular activity be excluded which accompanies the motion of the eyeball." But surely we should have no difficulty in perceiving the relation of the sides and angles of a small triangle, placed before the eye, even if the muscles of the eyeball were severed. This subject is resumed B. IV. c. ii. sect. 11.

the idea of space, in order to shew that it is not borrowed from experience, but is a bond of connexion among the impressions of sense, derived from a peculiar activity of the mind, and forming a foundation both of our experience and of our speculative knowledge.

Time is not a notion obtained by experience. Experience, that is, the impressions of sense and our consciousness of our thoughts, gives us various perceptions; and different successive perceptions considered together exemplify the notion of change. But this very connexion of different perceptions,—this successiveness,—presupposes that the perceptions exist *in time*. That things happen either together, or one after the other, is intelligible only by assuming time as the condition under which they are presented to us.

Thus time is a necessary condition in the presentation of all occurrences to our minds. We cannot conceive this condition to be taken away. We can conceive time to go on while nothing happens in it; but we cannot conceive anything to happen while time does not go on.

It is clear from this that time is not an impression derived from experience, in the same manner in which we derive from experience our information concerning the objects which exist, and the occurrences which take place in time. The objects of experience can easily be conceived to be, or not to be:—to be absent as well as present. Time always is, and always is present, and even in our thoughts we cannot form the contrary supposition.

2. Thus time is something distinct from the *matter* or substance of our experience, and may be considered as a necessary *form* which that matter (the experience of change) must assume, in order to be an object of contemplation to the mind. Time is one of the necessary

conditions under which we apprehend the information which our senses and consciousness give us. By considering time as a form which belongs to our power of apprehending occurrences and changes, and under which alone all such experience can be accepted by the mind, we explain the necessity, which we find to exist, of conceiving all such changes as happening in time; and we thus see that time is not a property perceived as existing in objects, or as conveyed to us by our senses; but a condition impressed upon our knowledge by the constitution of the mind itself; involving an act of thought as well as an impression of sense.

3. We showed that space is an idea of the mind, or form of our perceiving power, independent of experience, by pointing out that we possess necessary and universal truths concerning the relations of space, which could never be given by means of experience; but of which the necessity is readily conceivable, if we suppose them to have for their basis the constitution of the mind. There exist also respecting number, many truths absolutely necessary, entirely independent of experience and anterior to it; and so far as the conception of number depends upon the idea of time, the same argument might be used to show that the idea of time is not derived from experience, but is a result of the native activity of the mind: but we shall defer all views of this kind till we come to the consideration of Number.

4. Some persons have supposed that we obtain the notion of time from the perception of motion. But it is clear that the perception of motion, that is, change of place, presupposes the conception of time, and is not capable of being presented to the mind in any other way. If we contemplate the same body as being in different places at different times, and connect these observations, we have the conception of motion, which thus presup-

poses the necessary conditions that existence in time implies. And thus we see that it is possible there should be necessary truths concerning all motion, and consequently, concerning those motions which are the objects of experience; but that the source of this necessity is the Ideas of time and space, which, being universal conditions of knowledge residing in the mind, afford a foundation for necessary truths.

Chapter VIII.

OF SOME PECULIARITIES OF THE IDEA OF TIME.

1. The Idea of Time, like the Idea of Space, offers to our notice some characters which do not belong to our fundamental ideas generally, but which are deserving of remark. These characters are, in some respects, closely similar with regard to time and to space, while, in other respects, the peculiarities of these two ideas are widely different. We shall point out some of these characters.

Time is not a general *abstract* notion collected from experience; as, for example, a certain general conception of the relations of things. For we do not consider particular *times* as examples of Time in general, (as we consider particular causes to be examples of Cause,) but we conceive all particular times to be parts of a single and endless Time. This continually-flowing and endless time is what offers itself to us when we contemplate any series of occurrences. All actual and possible times exist as Parts, in this original and general Time. And since all particular times are considered as derivable from time in general, it is manifest that the notion of time in general cannot be derived from the notions of particular times. The notion of time in general is therefore not a general conception gathered from experience.

2. Time is infinite. Since all actual and possible times exist in the general course of time, this general time must be infinite. All limitation merely divides, and does not terminate, the extent of absolute time. Time has no beginning and no end; but the beginning and the end of every other existence takes place in it.

3. Time, like space, is not only a form of perception, but of *intuition*. We contemplate events as taking place *in* time. We consider its parts as added to one another, and events as filling a larger or smaller extent of such parts. The time which any event takes up is the sum of all such parts, and the relation of the same to time is fully understood when we can clearly see what portions of time it occupies, and what it does not. Thus the relation of known occurrences to time is perceived by intuition; and time is a form of intuition of the external world.

4. Time is conceived as a quantity of one dimension; it has great analogy with a line, but none at all with a surface or solid. Time may be considered as consisting of a series of instants, which are before and after one another; and they have no other relation than this, of *before* and *after*. Just the same would be the case with a series of points taken along a line; each would be after those on one side of it, and before those on another. Indeed the analogy between time, and space of one dimension, is so close, that the same terms are applied to both ideas, and we hardly know to which they originally belong. Times and lines are alike called *long* and *short*; we speak of the *beginning* and *end* of a line; of a *point* of time, and of the *limits* of a portion of duration.

5. But, as has been said, there is nothing in time which corresponds to more than one dimension in space,

and hence nothing which has any obvious analogy with figure. Time resembles a line indefinitely extended both ways; all partial times are portions of this line; and no mode of conceiving time suggests to us a line making any angle with the original line, or any other combination which might give rise to figures of any kind. The analogy between time and space, which in many circumstances is so clear, here disappears altogether. Spaces of two and of three dimensions, planes and solids, have nothing to which we can compare them in the conceptions arising out of time.

6. As figure is a conception solely appropriate to space, there is also a conception which peculiarly belongs to time, namely, the conception of recurrence of times similarly marked; or, as it may be termed, *rhythm*, using this word in a general sense. The term rhythm is most commonly used to designate the recurrence of times marked by the syllables of a verse, or the notes of a melody: but it is easy to see that the general conception of such a recurrence does not depend on the mode in which it is impressed upon the sense. The forms of such recurrence are innumerable. Thus in such a line as

> Quádrupedánte putrém sonitú quatit úngula cámpum,

we have alternately one long or forcible syllable, and two short or light ones, recurring over and over. In like manner in our own language, in the line

> At the clóse of the dáy when the hámlet is stíll,

we have two light and one strong syllable repeated four times over. Such repetition is the essence of versification. The same kind of rhythm is one of the main elements of music, with this difference only, that in music the forcible syllables are made so for the purposes of rhythm by their length only or principally; for example, if either of the above lines were imitated by a melody in the most simple and obvious manner, each strong syllable would occupy exactly twice as much time as two of the weaker ones. Something very analogous to such rhythm may be traced in other parts of poetry and art, which we need not here dwell upon. But in reference to our present subject, we may remark that by the introduction of such rhythm, the flow of time, which appears otherwise so perfectly simple and homogeneous, admits of an infinite number of varied yet regular modes of progress. All the kinds of versification which occur in all languages, and the still more varied forms of recurrence of notes of different lengths, which are heard in all the varied strains of melodies, are only examples of such modifications, or configurations as we may call them, of time. They involve relations of various portions of time, as figures involve relations of various portions of space. But yet the analogy between rhythm and figure is by no means very close; for in rhythm we have relations of quantity alone in the parts of time, whereas in figure we have relations not only of quantity, but of a kind altogether different,—namely, of position. On the other hand, a *repetition* of similar elements, which does not necessarily occur in figures, is quite essential in order to impress upon us that measured progress of time of which we here speak. And thus the ideas of time and space have each its peculiar and exclusive relations; position and figure belonging only to space, while repetition and rhythm are appropriate to time.

7. One of the simplest forms of recurrence is *alternation*, as when we have alternate strong and slight syllables. For instance,—

Awáke, aríse, or bé for éver fáll'n.

Or without any subordination, as when we reckon numbers, and call them in succession, *odd*, *even*, *odd*, *even*.

8. But the simplest of all forms of recurrence is that which has no variety;—in which a series of units, each considered as exactly similar to the rest, succeed each other; as *one*, *one*, *one*, and so on. In this case, however, we are led to consider each unit with reference to all that have preceded; and thus the series *one*, *one*, *one*, and so forth, becomes *one*, *two*, *three*, *four*, *five*, and so on; a series with which all are familiar, and which may be continued without limit.

We thus collect from that repetition of which time admits, the conception of *Number*.

9. The relations of position and figure are the subject of the science of geometry; and are, as we have already said, traced into a very remarkable and extensive body of truths, which rests for its foundations on axioms involved in the Idea of Space. There is, in like manner, a science of great complexity and extent, which has its foundation in the Idea of Time. But this science, as it is usually pursued, applies only to the conception of Number, which is, as we have said, the simplest result of repetition. This science is *Theoretical Arithmetic*, or the speculative doctrine of the properties and relations of numbers; and we must say a few words concerning the principles which it is requisite to assume as the basis of this science.

CHAPTER IX.

OF THE AXIOMS WHICH RELATE TO NUMBER.

1. THE foundations of our speculative knowledge of the relations and properties of Number, as well as of Space, are contained in the mode in which we represent to ourselves the magnitudes which are the subjects of our reasonings. To express these foundations in axioms in the

case of number, is a matter requiring some consideration, for the same reason as in the case of geometry; that is, because these axioms are principles which we assume as true, without being aware that we have made any assumption; and we cannot, without careful scrutiny, determine when we have stated, in the form of axioms, all that is necessary for the formation of the science, and no more than is necessary. We will, however, attempt to detect the principles which really must form the basis of theoretical arithmetic.

2. Why is it that three and two are equal to four and one? Because if we look at five things of any kind, we *see* that it is so. The five are four and one; they are also three and two. The truth of our assertion is involved in our being able to conceive the number five at all. We perceive this truth by *intuition*, for we cannot see, or imagine we see, five things, without perceiving also that the assertion above stated is true.

But how do we state in words this fundamental principle of the doctrine of numbers? Let us consider a very simple case. If we wish to show that seven and two are equal to four and five, we say that seven are four and three, *therefore* seven and two are four and three and two; and because three and two are five, this is four and five. Mathematical reasoners justify the first inference (marked by the conjunctive word *therefore*), by saying that "When equals are added to equals the wholes are equal," and that thus, since seven is equal to three and four, if we add two to both, seven and two are equal to four and three and two.

3. Such *axioms* as this, that when equals are added to equals the wholes are equal, are, in fact, expressions of the general condition of intuition, by which a whole is contemplated as made up of parts, and as identical with the aggregate of the parts. And a yet more gene-

ral form in which we might more adequately express this conditon of intuition would be this; that "Two magnitudes are equal when they can be divided into parts which are equal, each to each." Thus in the above example, seven and two are equal to four and five, because each of the two sums can be divided into the parts, four, three, and two.

4. In all these cases, a person who had never seen such axioms enunciated in a verbal form would employ the same reasoning as a practised mathematician, in order to satisfy himself that the proposition was true. The steps of the reasoning, being seen to be true by intuition, would carry an entire conviction, whether or not the argument were made verbally complete. Hence the axioms may appear superfluous, and on this account such axioms have often been spoken contemptuously of as empty and barren assertions. In fact, however, although they cannot supply the deficiency of the clear intuition of number and space in the reasoner himself, and although when he possesses such a faculty, he will reason rightly if he have never heard of such axioms, they still have their place properly at the beginning of our treatises on the science of quantity; since they express, as simply as words can express, those conditions of the intuition of magnitudes on which all reasoning concerning quantity must be based; and are necessary when we want, not only to see the truth of the elementary reasonings on these subjects, but to put such reasonings in a formal and logical shape.

5. We have considered the above-mentioned axioms as the basis of all arithmetical operations of the nature of addition. But it is easily seen that the same principle may be carried into other cases; as for instance, multiplication, which is merely a repeated addition, and admits of the same kind of evidence. Thus

five times three are equal to three times five; why is this? If we arrange fifteen things in five rows of three, it is seen by looking, or by imaginary looking, which is intuition, that they may also be taken as three rows of five. And thus the principle that those wholes are equal which can be resolved into the same partial magnitudes, is immediately applicable in this as in the other case.

6. We may proceed to higher numbers, and may find ourselves obliged to use artificial nomenclature and notation in order to represent and reckon them; but the reasoning in these cases also is still the same. And the usual artifice by which our reasoning in such instances is assisted is, that the number which is the root of our scale of notation (which is *ten* in our usual system), is alternately separated into parts and treated as a single thing. Thus 47 and 35 are 82; for 47 is four tens and seven; 35 is three tens and five; whence 47 and 35 are seven tens and twelve; that is, 7 tens, 1 ten, and 2; which is 8 tens and 2, or 82. The like reasoning is applicable in other cases. And since the most remote and complex properties of numbers are obtained by a prolongation of a course of reasoning exactly similar to that by which we thus establish the most elementary propositions, we have, in the principles just noticed, the foundation of the whole of Theoretical Arithmetic.

Chapter X.

OF THE PERCEPTION OF TIME AND NUMBER.

1. Our perception of the passage of time involves a series of acts of memory. This is easily seen and assented to, when large intervals of time and a complex train of occurrences are concerned. But since memory is requi-

site in order to apprehend time in such cases, we cannot doubt that the same faculty must be concerned in the shortest and simplest cases of succession; for it will hardly be maintained that the process by which we contemplate the progress of time is different when small and when large intervals are concerned. If memory be absolutely requisite to connect two events which begin and end a day, and to perceive a tract of time between them, it must be equally indispensable to connect the beginning and end of a minute, or a second; though in this case the effort may be smaller, and consequently more easily overlooked. In common cases, we are unconscious of the act of thought by which we recollect the preceding instant, though we perceive the effort when we recollect some distant event. And this is analogous to what happens in other instances. Thus, we walk without being conscious of the volitions by which we move our muscles; but, in order to leap, a distinct and manifest exertion of the same muscles is necessary. Yet no one will doubt that we walk as well as leap by an act of the will exerted through the muscles; and in like manner, our consciousness of small as well as large intervals of time involves something of the nature of an act of memory.

2. But this constant and almost imperceptible kind of memory, by which we connect the beginning and end of each instant as it passes, may very fitly be distinguished in common cases from manifest acts of recollection, although it may be difficult or impossible to separate the two operations in general. This perpetual and latent kind of memory may be termed a *sense of successiveness;* and must be considered as an internal sense by which we perceive ourselves existing in time, much in the same way as by our external and muscular sense we perceive ourselves existing in space. And both our

internal thoughts and feelings, and the events which take place around us, are apprehended as objects of this internal sense, and thus as taking place in time.

3. In the same manner in which our interpretation of the notices of the muscular sense implies the power of moving our limbs, and of touching at will this object or that; our apprehension of the relations of time by means of the internal sense of successiveness implies a power of recalling what has past, and of retaining what is passing. We are able to seize the occurrences which have just taken place, and to hold them fast in our minds so as mentally to measure their distance in time from occurrences now present. And thus, this sense of successiveness, like the muscular sense with which we have compared it, implies activity of the mind itself, and is not a sense passively receiving impressions.

4. The conception of *Number* appears to require the exercise of the same sense of succession. At first sight, indeed, we seem to apprehend Number without any act of memory, or any reference to time: for example, we look at a horse, and see that his legs are four; and this we seem to do at once, without reckoning them. But it is not difficult to see that this seeming instantaneousness of the perception of small numbers is an illusion. This resembles the many other cases in which we perform short and easy acts so rapidly and familiarly that we are unconscious of them; as in the acts of seeing, and of articulating our words. And this is the more manifest, since we begin our acquaintance with number by counting even the smallest numbers. Children and very rude savages must use an effort to reckon even their five fingers, and find a difficulty in going further. And persons have been known who were able by habit, or by a peculiar natural aptitude, to count by dozens as rapidly as common persons can by units. We may conclude,

therefore, that when we appear to catch a small number by a single glance of the eye, we do in fact count the units of it in a regular, though very brief succession. To count requires an act of memory. Of this we are sensible when we count very slowly, as when we reckon the strokes of a church-clock; for in such a case we may forget in the intervals of the strokes, and *miscount*. Now it will not be doubted that the nature of the process in counting is the same whether we count fast or slow. There is no definite speed of reckoning at which the faculties which it requires are changed; and therefore memory, which is requisite in some cases, must be so in all*.

The act of counting, (*one*, *two*, *three*, and so on,) is the foundation of all our knowledge of number. The intuition of the relations of number involves this act of counting; for, as we have just seen, the conception of number cannot be obtained in any other way. And thus the whole of theoretical arithmetic depends upon an act of the mind, and upon the conditions which the exercise of that act implies. These have been already explained in the last chapter.

5. But if the apprehension of number be accompanied by an act of the mind, the apprehension of *rhythm* is so still more clearly. All the forms of versification and the *measures* of melodies are the creations of man, who thus realizes in words and sounds the forms of recurrence which rise within his own mind. When we hear in a

* I have considered Number as involving the exercise of the sense of succession, because I cannot draw any line between those cases of large numbers, in which, the process of counting being performed, there is a manifest apprehension of succession; and those cases of small numbers, in which we seem to see the number at one glance. But if any one holds Number to be apprehended by a direct act of intuition, as Space and Time are, this view will not disturb the other doctrines delivered in the text.

quiet scene any rapidly-repeated sound, as those made by the hammer of the smith or the saw of the carpenter, every one knows how insensibly we throw these noises into a rhythmical form in our own apprehension. We do this even without any suggestion from the sounds themselves. For instance, if the beats of a clock or watch be ever so exactly alike, we still reckon them alternately tick-*tack*, tick-*tack*. That this is the case, may be proved by taking a watch or clock of such a construction that the returning swing of the pendulum is silent, and in which therefore all the beats are rigorously alike: we shall find ourselves still reckoning its sounds as tick-*tack*. In this instance it is manifest that the rhythm is entirely of our own making. In melodies, also, and in verses in which the rhythm is complex, obscure, and difficult, we perceive something is required on our part; for we are often incapable of contributing our share, and thus lose the sense of the measure altogether. And when we consider such cases, and attend to what passes within us when we catch the measure, even of the simplest and best-known air, we shall no longer doubt that an act of our own thoughts is requisite in such cases, as well as impressions on the sense. And thus the conception of this peculiar modification of time, which we have called *rhythm*, like all the other views which we have taken of the subject, shows that we must, in order to form such conceptions, supply a certain idea by our own thoughts, as well as merely receive by senses, whether external or internal, the impressions of appearances and collections of appearances.

NOTE TO CHAPTER X.

I HAVE in the last ten chapters described Space, Time, and Number by various expressions, all intended to point out their office as exemplifying the Ideal Element of human knowledge. I have called them *Funda-*

mental Ideas; *Forms of Perception*; *Forms of Intuition*; and perhaps other names. I might add yet other phrases. I might say that the properties of Space, Time, and Number are *Laws of the Mind's Activity* in apprehending what is. For the mind cannot apprehend any thing or event except conformably to the properties of space, time, and number. It is not only that it *does* not, but it *can* not: and this impossibility shows that the law is a law of the mind, and not of objects extraneous to the mind.

— It is usual for some of those who reject the doctrines here presented to say that the axioms of geometry, and of other sciences, are obtained by Induction from facts constantly presented by experience. But I do not see how Induction can prove that a proposition *must* be true. The only intelligible usage of the word *Induction* appears to me to be, that in which it is applied to a proposition which, being separable from the facts in our apprehension, and being compared with them, is seen to agree with them. But in the cases now spoken of, the proposition is not separable from the facts. We cannot infer by induction that two straight lines cannot inclose a space, because we cannot contemplate special cases of two lines inclosing a space, in which it remains to be determined whether or not the proposition, that both are straight, is true.

I do not deny that the activity of the mind by which it perceives objects and events as related according to the laws of space, time, and number, is awakened and developed by being constantly exercised; and that we cannot imagine a stage of human existence in which the powers have not been awakened and developed by such exercise. In this way, experience and observation are necessary conditions and prerequisites of our apprehension of geometrical (and other) axioms. We cannot see the truth of these axioms without some experience, because we cannot see any thing, or be human beings, without some experience. This might be expressed by saying that such truths are acquired necessarily *in the course of* all experience; but I think it is very undesirable to apply, to such a case, the word *Induction*, of which it is so important to us to keep the scientific meaning free from confusion. Induction cannot give demonstrative proofs, as I have already stated in Book I. C. ii. sect. 3, and therefore cannot be the ground of necessary truths.

Another expression which may be used to describe the Fundamental Ideas here spoken of is suggested by the language of a very profonnd and acute Review of the former edition. The Reviewer holds that we pass from special experiences to universal truths in virtue of "the inductive propensity—the irresistible impulse of the mind to generalize *ad infinitum*." I have already given reasons why I cannot adopt the former expression; but I do not see why space, time, number, cause, and the rest, may not be termed *different forms* of the *impulse of the mind to generalize.* If we put together all the Fundamental Ideas as results of the Generalizing Impulse, we must still separate them as different modes of action of that Impulse, showing themselves in various characteristic ways in the axioms and modes of reasoning which belong to different sciences. The Generalizing Impulse in one case proceeds according to the Idea of Space; in another, according to the Idea of Mechanical Cause; and so in other subjects.

CHAPTER XI.

OF MATHEMATICAL REASONING.

1. *Discursive Reasoning.*—WE have thus seen that our notions of space, time, and their modifications, necessarily involve a certain activity of the mind; and that the conditions of this activity form the foundations of those sciences which have the relations of space, time, and number, for their object. Upon the fundamental principles thus established, the various sciences which are included in the term *Pure Mathematics*, (Geometry, Algebra, Trigonometry, Conic Sections, and the rest of the Higher Geometry, the Differential Calculus, and the like,) are built up by a series of reasonings. These reasonings are subject to the rules of Logic, as we have already remarked; nor is it necessary here to dwell long on the nature and rules of such processes. But we may here notice that such processes are termed *discursive*, in opposition to the operations by which we acquire our fundamental principles, which are, as we have seen, *intuitive.* This opposition was formerly very familiar to our writers; as Milton,—

> . . . Thus the soul reason receives,
> Discursive or intuitive.—*Paradise Lost*, v. 438.

For in such reasonings we obtain our conclusions, not by looking at our conceptions steadily in one view, which is *intuition*, but by passing from one view to another, like those who run from place to place (*discursus*). Thus a straight line may be at the same time a side of a triangle and a radius of a circle: and in the first proposition of Euclid a line is considered, first in one of these relations, and then in the other, and thus the sides of a certain triangle are proved to be equal. And by this "discourse of reason," as by our older writers it was termed, we set forth from those axioms which we perceive by intuition, travel securely over a vast and varied region, and become possessed of a copious store of mathematical truths.

2. *Technical Terms of Reasoning.*—The reasoning of mathematics, thus proceeding from a few simple principles to many truths, is conducted according to the rules of Logic. If it be necessary, mathematical proofs may be reduced to logical forms, and expressed in Syllogisms, consisting of major, minor, and conclusion. But in most cases the syllogism is of that kind which is called by logical writers an *Enthymeme;* a word which implies something existing in the thoughts only, and which designates a syllogism in which one of the premises is understood, and not expressed. Thus we say in a mathematical proof, "because the point C is the center of the circle AB, AC is equal to BC;" not stating the *major*,—that all lines drawn from the center of a circle to the circumference are equal; or introducing it only by a transient reference to the definition of a circle. But the enthymeme is so constantly used in all habitual forms of reasoning, that it does not occur to us as being anything peculiar in mathematical works.

The propositions which are proved to be generally true are termed *Theorems:* but when anything is required to be done, as to draw a line or a circle under given conditions, this proposition is a *Problem.* A theorem requires demonstration; a problem, solution. And for both purposes the mathematician usually makes a *Construction*. He directs us to draw certain lines, circles, or other curves, on which is to be founded his demonstration that his theorem is true, or that his problem is solved. Sometimes, too, he establishes some *Lemma*, or preparatory proposition, before he proceeds to his main task; and often he deduces from his demonstration some conclusion in addition to that which was the professed object of his proposition; and this is termed a *Corollary*.

These technical terms are noted here, not as being very important, but in order that they may not sound strange and unintelligible if we should have occasion to use some of them. There is, however, one technical distinction more peculiar, and more important.

3. *Geometrical Analysis and Synthesis.*—In geometrical reasoning such as we have described, we introduce at every step some new consideration; and it is by combining all these considerations, that we arrive at the conclusion, that is, the demonstration of the proposition. Each step tends to the final result, by exhibiting some part of the figure under a new relation. To what we have already proved, is added something more; and hence this process is called *Synthesis*, or *putting together*. The proof flows on, receiving at every turn new contributions from different quarters; like a river fed and augmented by many tributary streams. And each of these tributaries flows from some definition or axiom as its fountain, or is itself formed by the union of smaller rivulets which have sources of this kind. In descending along its course, the synthetical proof gathers all these accessions into one common trunk, the proposition finally proved.

But we may proceed in a different manner. We may begin from the formed river, and ascend to its sources. We may take the proposition of which we require a proof, and may examine what the supposition of its truth implies. If this be true, then something else may be seen to be true; and from this, something else, and so on. We may often, in this way, discover of what simpler propositions our theorem or solution is compounded, and may resolve these in succession, till we come to some proposition which is obvious. This is geometrical *Analysis*. Having succeeded in this analytical process, we may invert it; and may descend again from the simple and known propositions, to the proof of a theorem, or the solution of a problem, which was our starting-place.

This process resembles, as we have said, tracing a river to its sources. As we ascend the stream, we perpetually meet with bifurcations; and some sagacity is needed to enable us to see which, in each case, is the main stream: but if we proceed in our research, we exhaust the unexplored valleys, and finally obtain a clear knowledge of the place whence the waters flow. *Analytical* is sometimes confounded with *symbolical* reasoning, on which subject we shall make a remark in the next chapter. The object of that chapter is to notice certain other fundamental principles and ideas, not included in those hitherto spoken of, which we find thrown in our way as we proceed in our mathematical speculations. It would detain us too long, and involve us in subtle and technical disquisitions, to examine fully the grounds of these principles; but the Mathematics hold so important a place in relation to the inductive sciences, that I shall briefly notice the leading ideas which the ulterior progress of the subject involves.

Chapter XII.

OF THE FOUNDATIONS OF THE HIGHER MATHEMATICS.

1. *The Idea of a Limit.*—The general truths concerning relations of space which depend upon the axioms and definitions contained in Euclid's *Elements*, and which involve only properties of straight lines and circles, are termed Elementary Geometry: all beyond this belongs to the Higher Geometry. To this latter province appertain, for example, all propositions respecting the lengths of any portions of curve lines; for these cannot be obtained by means of the principles of the Elements alone. Here then we must ask to what other principles the geometer has recourse, and from what source these are drawn. Is there any origin of geometrical truth which we have not yet explored?

The *Idea of a Limit* supplies a new mode of establishing mathematical truths. Thus with regard to the length of any portion of a curve, a problem which we have just mentioned; a curve is not made up of straight lines, and therefore we cannot by means of any of the doctrines of elementary geometry measure the length of any curve. But we may make up a figure nearly resembling any curve by putting together many short straight lines, just as a polygonal building of very many sides may nearly resemble a circular room. And in order to approach nearer and nearer to the curve, we may make the sides more and more small, more and more numerous. We may then possibly find some mode of measurement, some relation of these small lines to other lines, which is not disturbed by the multiplication of the sides, however far it be carried. And thus, we may do what is equivalent to measuring the curve itself; for by multiplying the sides we may approach more and more closely to the curve till no appreciable difference remains. The curve line is the *Limit* of the polygon; and in this process we proceed on the *Axiom*, that "What is true up to the limit is true at the limit."

This mode of conceiving mathematical magnitudes is of wide extent and use; for every curve may be considered as the limit of some polygon; every varied magnitude, as the limit of some aggregate of simpler forms; and thus the relations of the elementary figures enable us to advance to the properties of the most complex cases.

A Limit is a peculiar and fundamental conception, the use of which in proving the propositions of the Higher Geometry cannot be superseded by any combination of other hypotheses and definitions*. The axiom just noticed, that what is true up to the limit is true at the limit, is involved in the very conception of a limit: and this principle, with its consequences, leads to all the results which form the subject of the higher mathematics, whe-

* This assertion cannot be fully proved and illustrated without a reference to mathematical reasonings which would not be generally intelligible. I have shown the truth of the assertion in my *Thoughts on the Study of Mathematics*, annexed to the *Principles of English University Education*. The proof is of this kind:—The ultimate equality of an arc of a curve and the corresponding periphery of a polygon, when the sides of the polygon are indefinitely increased in number, is *evident*. But this truth cannot be proved from any other axiom. For if we take the supposed axiom, that a curve is always less than the including broken line, this is not true, except with a condition; and in tracing the import of this condition, we find its necessity becomes evident only when we introduce a reference to a Limit. And the same is the case if we attempt to supersede the notion of a Limit in proving any other simple and evident proposition in which that notion is involved. Therefore these evident truths are *self*-evident, *in virtue of the Idea of a Limit.*

ther proved by the consideration of evanescent triangles, by the processes of the Differential Calculus, or in any other way.

The ancients did not expressly introduce this conception of a Limit into their mathematical reasonings; although in the application of what is termed the *Method of Exhaustions*, (in which they show how to *exhaust* the *difference* between a polygon and a curve, or the like,) they were in fact proceeding upon an obscure apprehension of principles equivalent to those of the Method of Limits. Yet the necessary fundamental principle not having, in their time, been clearly developed, their reasonings were both needlessly intricate and imperfectly satisfactory. Moreover they were led to put in the place of axioms, assumptions which were by no means self-evident; as when Archimedes assumed, for the basis of his measure of the circumference of the circle, the proposition that a circular arch is necessarily less than two lines which inclose it, joining its extremities. The reasonings of the older mathematicians, which professed to proceed upon such assumptions, led to true results in reality, only because they were guided by a latent reference to the limiting case of such assumptions. And this latent employment of the conception of a Limit, reappeared in various forms during the early period of modern mathematics; as for example, in the *Method* of *Indivisibles* of Cavalleri, and the *Characteristic Triangle* of Barrow; till at last, Newton distinctly referred such reasonings to the conception of a Limit, and established the fundamental principles and processes which that conception introduces, with a distinctness and exactness which required little improvement to make it as unimpeachable as the demonstrations of geometry. And when such processes as Newton thus deduced from the conception of a Limit are represented by means of general algebraical symbols instead of geometrical diagrams, we have then before us the *Method of Fluxions*, or the *Differential Calculus;* a mode of treating mathematical problems justly considered as the principal weapon by which the splendid triumphs of modern mathematics have been achieved.

2. *The Use of General Symbols.*—The employment of algebraical symbols, of which we have just spoken, has been another of the main instruments to which the successes of modern mathematics are owing. And here again the processes by which we obtain our results depend for their evidence upon a fundamental conception, —the conception of *arbitrary symbols* as the *Signs* of quantity and its relations; and upon a corresponding axiom, that "The interpretation of such symbols must be perfectly general." In this case, as in the last, it was only by degrees that mathematicians were led to a just apprehension of the grounds of their reasoning. For symbols were at first used only to represent numbers considered with regard to their numerical properties; and thus the science of Algebra was formed. But it was found, even in cases belonging to common algebra, that the symbols often admitted of an interpretation which went beyond the limits of the problem, and which yet was not unmeaning, since it pointed out a question closely analogous to the question proposed. This was the case, for example, when the answer was a *negative quantity;* for when Descartes had introduced the mode of representing curves by means of algebraical relations among the symbols of the *co-ordinates*, or distances of each of their points from fixed lines, it was found that negative quantities must be dealt with as not less truly significant than positive ones. And as the researches of mathematicians proceeded, other cases also were found, in which the symbols, although destitute of meaning according to

the original conventions of their institution, still pointed out truths which could be verified in other ways; as in the cases in which what are called *impossible quantities* occur. Such processes may usually be confirmed upon other principles, and the truth in question may be established by means of a demonstration in which no such seeeming fallacies defeat the reasoning. But it has also been shown in many such cases, that the process in which some of the steps appear to be without real meaning, does in fact involve a valid proof of the proposition. And what we have here to remark is, that this is not true accidentally or partially only, but that the results of systematic symbolical reasoning must *always* express general truths, by their nature, and do not, for their justification, require each of the steps of the process to represent some definite operation upon quantity. *The absolute universality of the interpretation of symbols* is the fundamental principle of their use. This has been shown very ably by Dr. Peacock in his *Algebra*. He has there illustrated, in a variety of ways, this principle: that "If general symbols express an identity when they are supposed to be of any special nature, they must also express an identity when they are general in their nature." And thus, this universality of symbols is a principle in addition to those we have already noticed; and is a principle of the greatest importance in the formation of mathematical science, according to the wide generality which such science has in modern times assumed.

3. *Connexion of Symbols and Analysis.*—Since in our symbolical reasoning our symbols thus reason for us, we do not necessarily here, as in geometrical reasoning, go on adding carefully one known truth to another, till we reach the desired result. On the contrary, if we have a theorem to prove or a problem to solve which can be

brought under the domain of our symbols, we may at once state the given but unproved truth, or the given combination of unknown quantities, in its symbolical form. After this first process, we may then proceed to trace, by means of our symbols, what other truth is involved in the one thus stated, or what the unknown symbols must signify; resolving step by step the symbolical assertion with which we began, into others more fitted for our purpose. The former process is a kind of *synthesis*, the latter is termed *analysis*. And although symbolical reasoning does not necessarily imply such analysis; yet the connexion is so familiar, that the term *analysis* is frequently used to designate symbolical reasoning.

CHAPTER XIII.

THE DOCTRINE OF MOTION.

1. *Pure Mechanism.*—THE doctrine of Motion, of which we have here to speak, is that in which motion is considered quite independently of its cause, force; for all consideration of force belongs to a class of ideas entirely different from those with which we are here concerned. In this view it may be termed the *pure* doctrine of motion, since it has to do solely with space and time, which are the subjects of pure mathematics. (See C. I. of this Book.) Although the doctrine of motion in connexion with force, which is the subject of mechanics, is by far the most important form in which the consideration of motion enters into the formation of our sciences, the Pure Doctrine of Motion, which treats of space, time, and velocity, might be followed out so as to give rise to a very considerable and curious body of science. Such a science is the science

of Mechanism, independent of force, and considered as the solution of a problem which may be thus enunciated: "To communicate any given motion from a first mover to a given body." The science which should have for its object to solve all the various cases into which this problem would ramify, might be termed *Pure Mechanism*, in contradistinction to *Mechanics Proper*, or *Machinery*, in which Force is taken into consideration. The greater part of the machines which have been constructed for use in manufactures have been practical solutions of some of the cases of this problem. We have also important contributions to such a science in the works of mathematicians; for example, the various investigations and demonstrations which have been published respecting the form of the Teeth of Wheels, and Mr. Babbage's memoir* on the Language of Machinery. There are also several works which contain collections of the mechanical contrivances which have been invented for the purpose of transmitting and modifying motion, and these works may be considered as treatises on the science of Pure Mechanism. But this science has not yet been reduced to the systematic simplicity which is desirable, nor indeed generally recognized as a separate science. It has been confounded, under the common name of *Mechanics*, with the other science, Mechanics Proper, or Machinery, which considers the effect of *force* transmitted by mechanism from one part of a material combination to another. For example, the *Mechanical Powers*, as they are usually termed, (the Lever, the Wheel and Axle, the Inclined Plane, the Wedge, and the Screw,) have almost always been treated with reference to the relation between the *Power* and the *Weight*, and not primarily as a mode of changing the velocity and kind

* *On a Method of expressing by Signs the Action of Machinery.* Phil. Trans., 1826, p. 250.

of the motion. The science of pure motion has not generally been separated from the science of motion viewed with reference to its causes.

Recently, indeed, the necessity of such a separation has been seen by those who have taken a philosophical view of science. Thus this necessity has been urged by M. Ampère, in his *Essai sur la Philosophie des Sciences* (1834): "Long," he says, (p. 50), "before I employed myself upon the present work, I had remarked that it is usual to omit, in the beginning of all books treating of sciences which regard motion and force, certain considerations which, duly developed, must constitute a special science: of which science certain parts have been treated of, either in memoirs or in special works; such, for example, as that of Carnot upon Motion considered geometrically, and the essay of Lanz and Betancourt upon the Composition of Machines." He then proceeds to describe this science nearly as we have done, and proposes to term it *Kinematics* (*Cinématique*), from κίνημα, motion.

2. *Formal Astronomy.*—I shall not attempt here further to develop the form which such a science must assume. But I may notice one very large province which belongs to it. When men had ascertained the apparent motions of the sun, moon, and stars, to a moderate degree of regularity and accuracy, they tried to conceive in their minds some mechanism by which these motions might be produced; and thus they in fact proposed to themselves a very extensive problem in *Kinematics*. This, indeed, was the view originally entertained of the nature of the science of astronomy. Thus Plato in the seventh Book of his *Republic**, speaks of astronomy as the doctrine of the motion of solids, meaning thereby, spheres. And the same was a proper description of the science till the time of Kepler, and even later: for

* P. 528.

Kepler endeavoured in vain to conjoin with the knowledge of the motions of the heavenly bodies, those true mechanical conceptions which converted formal into physical astronomy*.

The astronomy of the ancients admitted none but uniform circular motions, and could therefore be completely cultivated by the aid of their elementary geometry. But the pure science of motion might be extended to all motions, however varied as to the speed or the path of the moving body. In this form it must depend upon the doctrine of limits; and the fundamental principle of its reasonings would be this: That velocity is measured by the Limit of the *space* described, considered with reference to the *time* in which it is described. I shall not further pursue this subject; and in order to complete what I have to say respecting the Pure Sciences, I have only a few words to add respecting their bearing on Inductive Science in general.

CHAPTER XIV.

OF THE APPLICATION OF MATHEMATICS TO THE INDUCTIVE SCENCES.

1. ALL objects in the world which can be made the subjects of our contemplation are subordinate to the conditions of Space, Time, and Number; and on this account, the doctrines of pure mathematics have most numerous and extensive applications in every department of our investigations of nature. And there is a peculiarity in these Ideas, which has caused the mathematical sciences to be, in all cases, the first successful efforts of the awakening speculative powers of nations at

* *Hist. Ind. Sc.*, II. 130.

the commencement of their intellectual progress. Conceptions derived from these Ideas are, from the very first, perfectly precise and clear, so as to be fit elements of scientific truths. This is not the case with the other conceptions which form the subjects of scientific inquiries. The conception of *statical force*, for instance, was never presented in a distinct form till the works of Archimedes appeared: the conception of *accelerating force* was confused, in the mind of Kepler and his contemporaries, and only became clear enough for purposes of sound scientific reasoning in the succeeding century: the just conception of chemical *composition* of elements gradually, in modern times, emerged from the erroneous and vague notions of the ancients. If we take works published on such subjects before the epoch when the foundations of the true science were laid, we find the knowledge not only small, but worthless. The writers did not see any evidence in what we now consider as the axioms of the science; nor any inconsistency where we now see self-contradiction. But this was never the case with speculations concerning space and number. From their first rise, these were true as far as they went. The Geometry and Arithmetic of the Greeks and Indians, even in their first and most scanty form, contained none but true propositions. Men's intuitions upon these subjects never allowed them to slide into error and confusion; and the truths to which they were led by the first efforts of their faculties, so employed, form part of the present stock of our mathematical knowledge.

2. But we are here not so much concerned with mathematics in their pure form, as with their application to the phenomena and laws of nature. And here also the very earliest history of civilization presents to us some of the most remarkable examples of man's success in his attempts to attain to science. Space and

time, position and motion, govern all visible objects; but by far the most conspicuous examples of the relations which arise out of such elements, are displayed by the ever-moving luminaries of the sky, which measure days, and months, and years, by their motions, and man's place on the earth by their position. Hence the sciences of space and number were from the first cultivated with peculiar reference to Astronomy. I have elsewhere* quoted Plato's remark,—that it is absurd to call the science of the relations of space *geometry*, the measure of the earth, since its most important office is to be found in its application to the heavens. And on other occasions also it appears how strongly he, who may be considered as the representative of the scientific and speculative tendencies of his time and country, had been impressed with the conviction, that the formation of a science of the celestial motions must depend entirely upon the progress of mathematics. In the Epilogue to the *Dialogue on the Laws*†, he declares mathematical knowledge to be the first and main requisite for the astronomer, and describes the portions of it which he holds necessary for astronomical speculators to cultivate. These seem to be, Plane Geometry, Theoretical Arithmetic, the Application of Arithmetic to planes and to solids, and finally the doctrine of Harmonics. Indeed the bias of Plato appears to be rather to consider mathematics as the essence of the science of astronomy, than as its instrument; and he seems disposed, in this as in other things, to disparage observation, and to aspire after a science founded upon demonstration alone. "An astronomer," he says in the same place, "must not be like Hesiod and persons of that kind, whose astronomy consists in noting the settings and risings of the stars; but he must be one who

* *Hist. Ind. Sc.*, B. III. c. ii.

† *Epinomis*, p. 990.

understands the revolutions of the celestial spheres, each performing its proper cycle."

A large portion of the mathematics of the Greeks, so long as their scientific activity continued, was directed towards astronomy. Besides many curious propositions of plane and solid Geometry, to which their astronomers were led, their Arithmetic, though very inconvenient in its fundamental assumptions, was cultivated to a great extent; and the science of Trigonometry, in which problems concerning the relations of space were resolved by means of tables of numerical results previously obtained, was created. Menelaus of Alexandria wrote six Books on Chords, probably containing methods of calculating Tables of these quantities; such Tables were familiarly used by the later Greek astronomers. The same author also wrote three Books on Spherical Trigonometry, which are still extant.

3. The Greeks, however, in the first vigour of their pursuit of mathematical truth, at the time of Plato and soon after, had by no means confined themselves to those propositions which had a visible bearing on the phenomena of nature; but had followed out many beautiful trains of research, concerning various kinds of figures, for the sake of their beauty alone; as for instance in their doctrine of Conic Sections, of which curves they had discovered all the principal properties. But it is curious to remark, that these investigations, thus pursued at first as mere matters of curiosity and intellectual gratification, were destined, two thousand years later, to play a very important part in establishing that system of the celestial motions which succeeded the Platonic scheme of cycles and epicycles. If the properties of the conic sections had not been demonstrated by the Greeks, and thus rendered familiar to the mathematicians of succeeding ages, Kepler would probably

not have been able to discover those laws respecting the orbits and motions of the planets which were the occasion of the greatest revolution that ever happened in the history of science.

4. The Arabians, who, as I have elsewhere said, added little of their own to the stores of science which they received from the Greeks, did however make some very important contributions in those portions of pure mathematics which are subservient to astronomy. Their adoption of the Indian mode of computation by means of the Ten Digits, 1, 2, 3, 4, 5, 6, 7, 8, 9, 0, and by the method of Local Values, instead of the cumbrous sexagesimal arithmetic of the Greeks, was an improvement by which the convenience and facility of numerical calculations were immeasurably augmented. The Arabians also rendered several of the processes of trigonometry much more commodious, by using the Sine of an arc instead of the Chord; an improvement which Albategnius appears to claim for himself*; and by employing also the Tangents of arcs, or, as they called them†, *upright shadows.*

5. The constant application of mathematical knowledge to the researches of Astronomy, and the mutual influence of each science on the progress of the other, has been still more conspicuous in modern times. Newton's Method of Prime and Ultimate Ratios, which we have already noticed as the first correct exposition of the doctrine of a Limit, is stated in a series of Lemmas, or preparatory theorems, prefixed to his *Treatise on the System of the World.* Both the properties of curve lines and the doctrines concerning force and motion, which he had to establish, required that the common mathematical methods should be methodized and extended. If Newton had not been a most expert and in-

* Delambre, Ast., *M. A.*, p. 12. † *Ibid.*, p. 17.

ventive mathematician, as well as a profound and philosophical thinker, he could never have made any one of those vast strides in discovery of which the rapid succession in his work strikes us with wonder*. And if we see that the great task begun by him, goes on more slowly in the hands of his immediate successors, and lingers a little before its full completion, we perceive that this arises, in a great measure, from the defect of the mathematical methods then used. Newton's synthetical modes of investigation, as we have elsewhere observed, were an instrument†, powerful indeed in his mighty hand, but too ponderous for other persons to employ with effect. The countrymen of Newton clung to it the longest, out of veneration for their master; and English cultivators of physical astronomy were, on that very account, left behind the progress of mathematical science in France and Germany, by a wide interval, which they have only recently recovered. On the Continent, the advantages offered by a familiar use of symbols, and by attention to their symmetry and other relations, were accepted without reserve. In this manner the Differential Calculus of Leibnitz, which was in its origin and signification identical with the Method of Fluxions of Newton, soon surpassed its rival in the extent and generality of its application to problems. This Calculus was applied to the science of mechanics, to which it, along with the symmetrical use of co-ordinates, gave a new form; for it was soon seen that the most difficult problems might in general be reduced to finding integrals, which is the reciprocal process of that by which differentials are found; so that all difficulties of physical astronomy were reduced to difficulties of symbolical calculation, these, indeed, being often sufficiently stubborn. Clairaut, Euler, and D'Alembert employed the increased

* *Hist. Ind. Sc.*, B. VII. c. ii. † *Ib.*, p. 175.

resources of mathematical science upon the Theory of the Moon, and other questions relative to the system of the world; and thus began to pursue such inquiries in the course in which mathematicians are still labouring up to the present day. This course was not without its checks and perplexities. We have elsewhere quoted* Clairaut's expression when he had obtained the very complex differential equations which contain the solution of the problem of the moon's motion: "Now integrate them who can!" But in no very long time they were integrated, at least approximately; and the methods of approximation have since then been improved; so that now, with a due expenditure of labour, they may be carried to any extent which is thought desirable. If the methods of astronomical observation should hereafter reach a higher degree of exactness than they now profess, so that irregularities in the motions of the sun, moon, and planets, shall be detected which at present escape us, the mathematical part of the theory of universal gravitation is in such a condition that it can soon be brought into comparison with the newly-observed facts. Indeed at present the mathematical theory is in advance of such observations. It can venture to suggest what may afterwards be detected, as well as to explain what has already been observed. This has happened recently; for Professor Airy has calculated the law and amount of an inequality depending upon the mutual attraction of the Earth and Venus; of which inequality (so small is it,) it remains to be determined whether its effect can be traced in the series of astronomical observations.

6. As the influence of mathematics upon the progress of astronomy is thus seen in the cases in which theory and observation confirm each other, so this influence appears in another way, in the very few cases in which the

* *Hist. Ind. Sc.*, B. VI. c. vi. sect. 7.

facts have not been fully reduced to an agreement with theory. The most conspicuous case of this kind is the state of our knowledge of the Tides. This is a portion of astronomy: for the Newtonian theory asserts these curious phenomena to be the result of the attraction of the sun and moon. Nor can there be any doubt that this is true, as a general statement; yet the subject is up to the present time a blot on the perfection of the theory of universal gravitation; for we are very far from being able in this, as in the other parts of astronomy, to show that theory will exactly account for the time, and magnitude, and all other circumstances of the phenomenon at every place on the earth's surface. And what is the portion of our mathematics which is connected with this solitary signal defect in astronomy? It is the mathematics of the Motion of Fluids; a portion in which extremely little progress has been made, and in which all the more general problems of the subject have hitherto remained entirely insoluble. The attempts of the greatest mathematicians, Newton, Maclaurin, Bernoulli, Clairaut, Laplace, to master such questions, all involve some gratuitous assumption, which is introduced because the problem cannot otherwise be mathematically dealt with: these assumptions confessedly render the result defective, and how defective, it is hard to say. And it was probably precisely the absence of a theory which could be reasonably expected to agree with the observations, which made Observations of this very curious phenomenon, the Tides, to be so much neglected as till very recently they were. Of late years such observations have been pursued, and their results have been resolved into empirical laws, so that the rules of the phenomena have been ascertained, although the dependence of these rules upon the lunar and solar forces has not been shown. Here then we have a portion of our knowledge relating to facts undoubtedly dependent upon universal gravitation, in which Observation has outstripped Theory in her progress, and is compelled to wait till her usual companion overtakes her. This is a position of which Mathematical Theory has usually been very impatient, and we may expect that she will be no less so in the present instance.

7. It would be easy to show from the history of other sciences, for example, Mechanics and Optics, how essential the cultivation of pure mathematics has been to their progress. The parabola was already familiar among mathematicians when Galileo discovered that it was the theoretical path of a Projectile; and the extension and generalization of the Laws of Motion could never have been effected, unless the Differential and Integral Calculus had been at hand, ready to trace the results of every hypothesis which could be made. D'Alembert's mode of expressing the Third Law of Motion in its most general form*, if it did not prove the law, at least reduced the application of it to analytical processes which could be performed in most of those cases in which they were needed. In many instances the demands of mechanical science suggested the extension of the methods of pure analysis. The problem of Vibrating Strings gave rise to the Calculus of Partial Differences, which was still further stimulated by its application to the motions of fluids and other mechanical problems. And we have in the writings of Lagrange and Laplace other instances equally remarkable of new analytical methods, to which mechanical problems, and especially cosmical problems, have given occasion.

8. The progress of Optics as a science has, in like manner, been throughout dependent upon the progress of pure mathematics. The first rise of geometry was fol-

* *Hist. Ind. Sci.*, B. VI. c. vi. sect. 7.

lowed by some advances, slight ones no doubt, in the doctrine of Reflection and in Perspective. The law of Refraction was traced to its consequences by means of Trigonometry, which indeed was requisite to express the law in a simple form. The steps made in Optical science by Descartes, Newton, Euler, and Huyghens, required the geometrical skill which those philosophers possessed. And if Young and Fresnel had not been, each in his peculiar way, persons of eminent mathematical endowments, they would not have been able to bring the Theory of Undulations and Interferences into a condition in which it could be tested by experiments. We may see how unexpectedly recondite parts of pure mathematics may bear upon physical science, by calling to mind a circumstance already noticed in the History of Science*;—that Fresnel obtained one of the most curious confirmations of the theory (the laws of Circular Polarization by reflection) through an interpretation of an algebraical expression, which, according to the original conventional meaning of the symbols, involved an impossible quantity. We have already remarked, that in virtue of the principle of the generality of symbolical language, such an interpretation may often point out some real and important analogy.

9. From this rapid sketch it may be seen how important an office in promoting the progress of the physical sciences belongs to mathematics. Indeed in the progress of many sciences, every step has been so intimately connected with some advance in mathematics, that we can hardly be surprized if some persons have considered mathematical reasoning to be the most essential part of such sciences; and have overlooked the other elements which enter into their formation. How erro-

* *Hist. Ind. Sci.*, B. IX. c. xiii. sect. 2.

neous this view is we shall best see by turning our attention to the other Ideas besides those of space, number, and motion, which enter into some of the most conspicuous and admired portions of what is termed exact science; and by showing that the clear and distinct developement of such Ideas is quite as necessary to the progress of exact and real knowledge as an acquaintance with arithmetic and geometry.

BOOK III.

THE PHILOSOPHY OF THE MECHANICAL SCIENCES.

CHAPTER I.

OF THE MECHANICAL SCIENCES.

IN the History of the Sciences, that class of which we here speak occupies a conspicuous and important place; coming into notice immediately after those parts of astronomy which require for their cultivation merely the ideas of space, time, motion, and number. It appears from our History, that certain truths concerning the *equilibrium* of bodies were established by Archimedes;—that, after a long interval of inactivity, his principles were extended and pursued further in modern times:—and that to these doctrines concerning equilibrium and the forces which produce it, (which constitute the science *Statics*,) were added many other doctrines concerning the *motions* of bodies, considered also as produced by forces, and thus the science of *Dynamics* was produced. The assemblage of these sciences composes the province of *Mechanics*. Moreover, philosophers have laboured to make out the laws of the equilibrium of *fluid* as well as solid bodies; and hence has arisen the science of *Hydrostatics*. And the doctrines of Mechanics have been found to have a most remarkable bearing upon the motions of the heavenly bodies; with reference to which, indeed, they were at first principally studied. The explanation of those cosmical facts by means of mechanical principles and their consequences, forms the science of *Physical Astronomy*. These are the principal examples of mechanical science; although some other portions of Physics, as Magnetism and Electrodynamics, introduce mechanical doctrines very largely into their speculations.

Now in all these sciences we have to consider *Forces*. In all mechanical reasonings forces enter, either as producing motion, or as prevented from doing so by other forces. Thus force, in its most general sense, is the *cause* of motion, or of tendency to motion; and in order to discover the principles on which the mechanical sciences truly rest, we must examine the nature and origin of our knowledge of Causes.

In these sciences, however, we have not to deal with Cause in its more general acceptation, in which it applies to all kinds of agency, material or immaterial;—to the influence of thought and will, as well as of bodily pressure and attractive force. Our business at present is only with such causes as immediately operate upon matter. We shall nevertheless, in the first place, consider the nature of Cause in its most general form; and afterwards narrow our speculations so as to direct them specially to the mechanical sciences.

CHAPTER II.

OF THE IDEA OF CAUSE.

1. WE see in the world around us a constant succession of causes and effects connected with each other. The laws of this connexion we learn in a great measure from experience, by observation of the occurrences which present themselves to our notice, succeeding one another.

But in doing this, and in attending to this succession of appearances, of which we are aware by means of our senses, we supply from our own minds the Idea of Cause. This Idea, as we have already shown with respect to other Ideas, is not derived from experience, but has its origin in the mind itself;—is introduced into our experience by the active, and not by the passive part of our nature.

By Cause we mean some quality, power, or efficacy, by which a state of things produces a succeeding state. Thus the motion of bodies from rest is produced by a cause which we call *Force:* and in the particular case in which bodies fall to the earth, this force is termed *Gravity.* In these cases, the Conceptions of Force and Gravity receive their meaning from the Idea of Cause which they involve: for Force is conceived as the Cause of Motion. That this Idea of Cause is not derived from experience, we prove (as in former cases) by this consideration: that we can make assertions, involving this idea, which are rigorously necessary and universal; whereas knowledge derived from experience can only be true as far as experience goes, and can never contain in itself any evidence whatever of its necessity. We assert that "Every event must have a cause:" and this proposition we know to be true, not only probably, and generally, and as far as we can see: but we cannot suppose it to be false in any single instance. We are as certain of it as of the truths of arithmetic or geometry. We cannot doubt that it must apply to all events past and future, in every part of the universe, just as truly as to those occurrences which we have ourselves observed. *What* causes produce what effects;—what is the cause of any particular event;—what will be the effect of any peculiar process;—these are points on which experience may enlighten us. Observation and experience may be requisite, to enable us to judge respecting such matters. But that every event has *some* cause, Experience cannot prove any more than she can disprove. She can add nothing to the evidence of the truth, however often she may exemplify it. This doctrine, then, cannot have been acquired by her teaching; and the Idea of Cause, which the doctrine involves, and on which it depends, cannot have come into our minds from the region of observation.

2. That we do, in fact, apply the Idea of Cause in a more extensive manner than could be justified, if it were derived from experience only, is easily shown. For from the principle that everything must have a cause, we not only reason concerning the succession of the events which occur in the progress of the world, and which form the course of experience; but we infer that the world itself must have a cause;—that the chain of events connected by common causation, must have a First Cause of a nature different from the events themselves. This we are entitled to do, if our Idea of Cause be independent of, and superior to, experience: but if we have no Idea of Cause except such as we gather from experience, this reasoning is altogether baseless and unmeaning.

3. Again; by the use of our powers of observation, we are aware of a succession of appearances and events. But none of our senses or powers of external observation can detect in these appearances the power or quality which we call Cause. Cause is that which connects one event with another; but no sense or perception discloses to us, or can disclose, any connexion among the events which we observe. We see that one occurrence follows another, but we can never see anything which shows that one occurrence *must* follow another. We have already noticed*, that this truth has been urged by metaphy-

* Book I., chap. xiii.

sicians in modern times, and generally assented to by those who examine carefully the connexion of their own thoughts. The arguments are, indeed, obvious enough. One ball strikes another and causes it to move forwards. But by what compulsion? Where is the necessity? If the mind can see any circumstance in this case which makes the result inevitable, let this circumstance be pointed out. But, in fact, there is no such discoverable necessity; for we can conceive this event not to take place at all. The struck ball may stand still, for aught we can see. "But the laws of motion will not allow it to do so." Doubtless they will not. But the laws of motion are learnt from experience, and therefore can prove no necessity. Why should not the laws of motion be other than they are? Are they necessarily true? That they are necessarily such as do actually regulate the impact of bodies, is at least no obvious truth; and therefore this necessity cannot be, in common minds, the ground of connecting the impact of one ball with the motion of another. And assuredly, if this fail, no other ground of such necessary connexion can be shown. In this case, then, the events are not seen to be necessarily connected. But if this case, where one ball moves another by impulse, be not an instance of events exhibiting a necessary connexion, we shall look in vain for any example of such a connexion. There is, then, no case in which events can be observed to be necessarily connected: our idea of causation, which implies that the event is necessarily connected with the cause, cannot be derived from observation.

4. But it may be said, we have not any such Idea of Cause, implying necessary connexion with effect, and a quality by which this connexion is produced. We see nothing but the succession of events; and by *cause* we mean nothing but a certain succession of events;—name-

ly, a constant, unvarying succession. Cause and effect are only two events of which the second invariably follows the first. We delude ourselves when we imagine that our idea of causation involves anything more than this.

To this I reply by asking, what then is the meaning of the maxim above quoted, and allowed by all to be universally and necessarily true, that every event must have a cause? Let us put this maxim into the language of the explanation just noticed; and it becomes this:—"Every event must have a certain other event invariably preceding it." But why must it? Where is the necessity? Why must like events always be preceded by like, except so far as other events interfere? That there is such a necessity, no one can doubt. All will allow that if a stone ascend because it is thrown upwards in one case, a stone which ascends in another case has also been thrown upwards, or has undergone some equivalent operation. All will allow that in this sense, every kind of event must have some other specific kind of event preceding it. But this turn of men's thoughts shows that they see in events a connexion which is not mere succession. They see in cause and effect, not merely what does, often or always, precede and follow, but what *must* precede and follow. The events are not only conjoined, they are connected. The cause is more than the prelude, the effect is more than the sequel, of the fact. The cause is conceived not as a mere occasion; it is a power, an efficacy, which has a real operation.

5. Thus we have drawn from the maxim, that Every Effect must have a Cause, arguments to show that we have an Idea of Cause which is not borrowed from experience, and which involves more than mere succession. Similar arguments might be derived from any other

maxims of universal and necessary validity, which we can obtain concerning Cause: as, for example, the maxims that Causes are measured by their Effects, and that Reaction is equal and opposite to Action. These maxims we shall soon have to examine; but we may observe here, that the necessary truth which belongs to them, shows that they, and the Ideas which they involve, are not the mere fruits of observation; while their meaning, including, as it does, something quite different from the mere conception of succession of events, proves that such a conception is far from containing the whole import and signification of our Idea of Cause.

The progress of the opinions of philosophers on the points discussed in this chapter, has been one of the most remarkable parts of the history of Metaphysics in modern times: and I shall therefore briefly notice some of its features.

Chapter III.

MODERN OPINIONS RESPECTING THE IDEA OF CAUSE.

1. Towards the end of the seventeenth century there existed in the minds of many of the most vigorous and active speculators of the European literary world, a strong tendency to ascribe the whole of our Knowledge to the teaching of Experience. This tendency, with its consequences, including among them the reaction which was produced when the tenet had been pushed to a length manifestly absurd, has exercised a very powerful influence upon the progress of metaphysical doctrines up to the present time. I proceed to notice some of the most prominent of the opinions which have thus ob-

tained prevalence among philosophers, so far as the Idea of Cause is concerned.

Locke was one of the metaphysicians who produced the greatest effect in diffusing this opinion, of the exclusive dependence of our knowledge upon experience. Agreeably to this general system, he taught* that our ideas of Cause and Effect are got from observation of the things about us. Yet notwithstanding this tenet of his, he endeavoured still to employ these ideas in reasoning on subjects which are far beyond all limits of experience: for he professed to prove, from our idea of Causation, the existence of the Deity†.

Hume noticed this obvious inconsistency; but declared himself unable to discover any remedy for a defect so fatal to the most important parts of our knowledge. He could see, in our belief of the succession of cause and effect, nothing but the habit of associating in our minds what had often been associated in our experience. He therefore maintained that we could not, with logical propriety, extend our belief of such a succession to cases entirely distinct from all those of which our experience consisted. We see, he said, an actual *conjunction* of two events; but we can in no way detect a necessary *connexion;* and therefore we have no means of inferring cause from effect, or effect from cause‡. The only way in which we recognize Cause and Effect in the field of our experience, is as an unfailing Sequence: we look in vain for anything which can assure us of an infallible Consequence. And since experience is the only source of our knowledge, we cannot with any justice assert that the world in which we live must necessarily have had a cause.

2. This doctrine, taken in conjunction with the known

* *Essay on the Human Understanding*, B. II. c. xxvi. † B. IV. c. x.

‡ Hume's *Phil. of the Human Mind*, Vol. I. p. 94.

skepticism of its author on religious points, produced a considerable fermentation in the speculative world. The solution of the difficulty thus thrown before philosophers, was by no means obvious. It was vain to endeavour to find in experience any other property of a Cause, than a constant sequence of the effect. Yet it was equally vain to try to persuade men that they had no idea of Cause; or even to shake their belief in the cogency of the familiar arguments concerning the necessity of an original cause of all that is and happens. Accordingly these hostile and apparently irreconcilable doctrines,—the indispensable necessity of a cause of every event, and the impossibility of our knowing such a necessity,—were at last allowed to encamp side by side. Reid, Beattie, and others, formed one party, who showed how widely and constantly the idea of a cause pervades all the processes of the human mind: while another sect, including Brown, and apparently Stewart, maintained that this idea is always capable of being resolved into a constant sequence; and these latter reasoners tried to obviate the dangerous and shocking inferences which some persons might try to draw from their opinion, by declaring the maxim that "Every event must have a cause," to be an instinctive law of belief, or a fundamental principle of the human mind*.

3. While this series of discussions was going on in Britain, a great metaphysical genius in Germany was unravelling the perplexity in another way. Kant's speculations originated, as he informs us, in the trains of thought to which Hume's writings gave rise; and the *Kritik der Reinen Vernunft*, or *Examination of the Pure Reason*, was published in 1787, with the view of showing the true nature of our knowledge.

* Stewart's *Active Powers*, Vol. I. p. 347. Brown's *Lectures*, Vol. I. p. 115.

Kant's solution of the difficulties just mentioned differs materially from that above stated. According to Brown *, succession observed and cause inferred,—the memory of past conjunctions of events and the belief of similar future conjunctions,—are facts, independent, so far as we can discover, but inseparably combined by a law of our mental nature. According to Kant, causality is an inseparable condition of our experience: a connexion in events is requisite to our apprehending them *as* events. Future occurrences must be connected by causation as the past have been, because we cannot think of past, present, and future, without such connexion. We cannot fix the mind upon occurrences, without including these occurrences in a series of causes and effects. The relation of Causation is a condition under which we think of events, as the relations of space are a condition under which we see objects.

4. On a subject so abstruse, it is not easy to make our distinctions very clear. Some of Brown's illustrations appear to approach very near to the doctrine of Kant. Thus he says †, "The *form* of bodies is the relation of their elements to each other in space,—the *power* of bodies is their relation to each other in time." Yet notwithstanding such approximations in expression, the Kantian doctrine appears to be different from the views of Stewart and Brown, as commonly understood. According to the Scotch philosophers, the cause and the effect are two things, connected in our minds by a law of our nature. But this view requires us to suppose that we can conceive the law to be absent, and the course of events to be unconnected. If we can understand what is the special force of this law, we must be able to imagine what the case would be if the law were non-existing. We must be able to conceive a mind which does not connect

* *Lect.*. Vol. I. p. 114. † *Lect.*, I. p. 127.

effects with causes. The Kantian doctrine, on the other hand, teaches that we cannot imagine events liberated from the connexion of cause and effect: this connexion is a condition of our conceiving any real occurrences: we cannot think of a real sequence of things, except as involving the operation of causes. In the Scotch system, the past and the future are in their nature independent, but bound together by a rule; in the German system, they share in a common nature and mutual relation, by the act of thought which makes them past and future. In the former doctrine cause is a tie which binds; in the latter it is a character which pervades and shapes events. The Scotch metaphysicians only assert the *universality* of the relation; the German attempts further to explain its *necessity*.

This being the state of the case, such illustrations as that of Dr. Brown quoted above, in which he represents *cause* as a relation of the same kind with *form*, do not appear exactly to fit his opinions. Can the relations of figure be properly said to be connected with each other by a law of our nature, or a tendency of our mental constitution? Can we ascribe it to a law of our thoughts, that we believe the three angles of a triangle to be equal to two right angles? If so, we must give the same reason for our belief that two straight lines cannot inclose a space; or that three and two are five. But will any one refer us to an ultimate law of our constitution for the belief that three and two are five? Do we not see that they are so, as plainly as we see that they are three and two? Can we imagine laws of our constitution abolished, so that three and two shall make something different from five;—so that an inclosed space shall lie between two straight lines;—so that the three angles of a plane triangle shall be greater than two right angles? We cannot conceive this. If the numbers *are* three and two; if the lines *are* straight; if the triangle *is* a rectilinear triangle, the consequences are inevitable. We cannot even imagine the contrary. We do not want a law to direct that things should be what they are. The relation, then, of cause and effect, being of the same kind as the necessary relations of figure and number, is not properly spoken of as established in our minds by a special law of our constitution: for we reject that loose and inappropriate phraseology which speaks of the relations of figure and number as "determined by laws of belief."

5. In the present work, we accept and adopt, as the basis of our inquiry concerning our knowledge, the existence of necessary truths concerning causes, as there exist necessary truths concerning figure and number. We find such truths universally established and assented to among the cultivators of science, and among speculative men in general. All mechanicians agree that reaction is equal and opposite to action, both when one body presses another, and when one body communicates motion to another. All reasoners join in the assertion, not only that every observed change of motion has had a cause, but that every change of motion must have a cause. Here we have certain portions of substantial and undoubted knowledge. Now the essential point in the view which we must take of the idea of cause is this,—that our view must be such as to form a solid basis for our knowledge. We have, in the Mechanical Sciences, certain universal and necessary truths on the subject of causes. Now any view which refers our belief in causation to mere experience or habit, cannot explain the possibility of such necessary truths, since experience and habit can never lead to a perception of necessary connexion. But a view which teaches us to acknowledge axioms concerning cause, as we acknow-

ledge axioms concerning space, will lead us to look upon the science of mechanics as equally certain and universal with the science of geometry; and will thus materially affect our judgment concerning the nature and claims of our scientific knowledge.

Axioms concerning Cause, or concerning Force, which as we shall see, is a modification of Cause, will flow from an Idea of Cause, just as axioms concerning space and number flow from the ideas of space and number or time. And thus the propositions which constitute the science of Mechanics prove that we possess an idea of cause, in the same sense in which the propositions of geometry and arithmetic prove our possession of the ideas of space and of time or number.

6. The idea of cause, like the ideas of space and time, is a part of the *active* powers of the mind. The relation of cause and effect is a relation or condition under which events are apprehended, which relation is not given by observation, but supplied by the mind itself. According to the views which explain our apprehension of cause by reference to habit, or to a supposed law of our mental nature, causal connexion is a consequence of agencies which the mind passively obeys; but according to the view to which we are led, this connexion is a result of faculties which the mind actively exercises. And thus the relation of cause and effect is a condition of our apprehending successive events, a part of the mind's constant and universal activity, a source of necessary truths; or, to sum all this in one phrase, a Fundamental Idea.

CHAPTER IV.

OF THE AXIOMS WHICH RELATE TO THE IDEA OF CAUSE.

1. *Causes are abstract Conceptions.*—WE have now to express, as well as we can, the fundamental character of that Idea of Cause, of which we have just proved the existence. This may be done, at least for purposes of reasoning, in this as in former instances, by means of axioms. I shall state the principal axioms which belong to this subject, referring the reader to his own thoughts for the axiomatic evidence which belongs to them.

But I must first observe, that in order to express general and abstract truths concerning cause and effect, these terms, *cause* and *effect*, must be understood in a general and abstract manner. When one event gives rise to another, the first *event* is, in common language, often called the cause, and the second the effect. Thus the meeting of two billiard balls may be said to be the cause of one of them turning aside out of the path in which it was moving. For our present purposes, however, we must not apply the term cause to such occurrences as this meeting and turning, but to a certain conception, *force*, abstracted from all such special events, and considered as a quality or property by which one body affects the motion of the other. And in like manner in other cases, cause is to be conceived as some abstract quality, power, or efficacy, by which change is produced; a quality not identical with the events, but disclosed by means of them. Not only is this abstract mode of conceiving force and cause useful in expressing the fundamental principles of science; but it supplies us with the only mode by which such principles can be stated in a general manner, and made to lead to substantial truth and real knowledge.

Understanding *cause*, therefore, in this sense, we proceed to our Axioms.

2. First Axiom. *Nothing can take place without a Cause.*

Every event, of whatever kind, must have a Cause in the sense of the term which we have just indicated; and that it must, is a universal and necessary proposition to which we irresistibly assent as soon as it is understood. We believe each appearance to come into existence,—we conceive every change to take place,—not only with something preceding it, but something by which it is made to be what it is. An effect without a cause;—an event without a preceding condition involving the efficacy by which the event is produced;—are suppositions which we cannot for a moment admit. That the connexion of effect with cause is universal and necessary, is a universal and constant conviction of mankind. It persists in the minds of all men, undisturbed by all the assaults of sophistry and skepticism; and, as we have seen in the last chapter, remains unshaken, even when its foundations seem to be ruined. This axiom expresses, to a certain extent, our Idea of Cause; and when that idea is clearly apprehended, the axiom requires no proof, and indeed admits of none which makes it more evident. That notwithstanding its simplicity, it is of use in our speculations, we shall hereafter see; but in the first place, we must consider the other axioms belonging to this subject.

3. Second Axiom. *Effects are proportional to their Causes, and Causes are measured by their Effects.*

We have already said that *cause* is that quality or power, in the circumstances of each case, by which the effect is produced; and this power, an abstract property of the condition of things to which it belongs, can in no way fall directly under the cognizance of the senses. Cause, of whatever kind, is not apprehended as including objects and events which share its nature by being co-extensive with certain portions of it, as space and time are. It cannot therefore, like them, be measured by repetition of its own parts, as space is measured by repetition of inches, and time by repetition of minutes. Causes may be greater or less; as, for instance, the force of a man is greater than the force of a child. But how much is the one greater than the other? How are we to compare the abstract conception, force, in such cases as these?

To this, the obvious and only answer is, that we must compare causes by means of their effects;—that we must compare force by something which force can do. The child can lift one fagot; the man can lift ten such fagots: we have here a means of comparison. And whether or not the rule is to be applied in this manner, that is, by the number of the things operated on, (a question which we shall have to consider hereafter,) it is clear that this form of rule, namely, a reference to some effect or other as our measure, is the right, because the only possible form. The cause determines the effect. The cause being the same, the effect must be the same. The connexion of the two is governed by a fixed and inviolable rule. It admits of no ambiguity. Every degree of intensity in the cause has some peculiar modification of the effect corresponding to it. Hence the effect is an unfailing index of the amount of the cause; and if it be a measurable effect, gives a measure of the cause. We can have no other measure; but we need no other, for this is exact, sufficient, and complete.

It may be said, that various effects are produced by the same cause. The sun's heat melts wax and expands quicksilver. The force of gravity causes bodies to move downwards if they are free, and to press down upon their supports if they are supported. Which of the effects is to be taken as the measure of heat, or of gravity, in these cases? To this we reply, that if we had merely different states of the same cause to compare, any of the effects might be taken. The sun's heat on different days might be measured by the expansion of quicksilver, or by the quantity of wax melted. The force of gravity, if it were different at different places, might be measured by the spaces through which a given weight would bend an elastic support, or by the spaces through which a body would fall in a given time. All these measures are consistent with the general character of our idea of cause.

4. *Limitation of the Second Axiom.*—But there may be circumstances in the nature of the case which may further determine the kind of effect which we must take for the measure of the cause. For example, if causes are conceived to be of such a nature as to be capable of addition, the effects taken as their measure must conform to this condition. This is the case with mechanical causes. The weights of two bodies are the causes of the pressure which they exert downwards; and these weights are capable of addition. The weight of the two is the sum of the weight of each. We are therefore not at liberty to say that weights shall be measured by the spaces through which they bend a certain elastic support, except we have first ascertained that the whole weight bends it through a space equal to the sum of the inflections produced by the separate weights. Without this precaution, we might obtain inconsistent results. Two weights, each of the magnitude 3 as measured by their effects, might, if we took the inflections of a spring for the effects, be together equal to 5 or to 7 by the same kind of measurement. For the inflection produced by two weights of 3 might, for aught we can see beforehand, be more or less than twice as great as the inflection

produced by one weight of 3. That forces are capable of addition, is a condition which limits, and, as we shall see, in some cases rigorously fixes, the kind of effects which are to be taken as their measures.

Causes which are thus capable of addition are to be measured by the repeated addition of equal quantities. Two such causes are *equal* to each other when they produce exactly the same effect. So far our axiom is applied directly. But these two causes can be *added* together; and being thus added, they are *double* of one of them; and the cause composed by addition of *three* such, is *three* times as great as the first; and so on for any measure whatever. By this means, and by this means only, we have a complete and consistent measure of those causes which are so conceived as to be subject to this condition of being added and multiplied.

Causes are, in the present chapter, to be understood in the widest sense of the term; and the axiom now under our consideration applies to them, whenever they are of such a nature as to admit of any measure at all. But the cases which we have more particularly in view are *mechanical* causes, the causes of the motion and of the equilibrium of bodies. In these cases, forces are conceived as capable of addition; and what has been said of the measure of causes in such cases, applies peculiarly to mechanical forces. Two weights, placed together, may be considered as a single weight, equal to the *sum* of the two. Two pressures, pushing a body in the same direction at the same point, are identical in all respects with some single pressure, their *sum*, pushing in like manner; and this is true whether or not they put the body in motion. In the cases of mechanical forces, therefore, we take some certain effect, velocity generated or weight supported, which may fix the *unit* of force; and we then measure all other forces by the successive repetition of this unit, as we measure all spaces by the successive repetition of our unit of lineal measure.

But these steps in the formation of the science of Mechanics will be further explained, when we come to follow our axioms concerning cause into their application in that science. At present we have, perhaps, sufficiently explained the axiom that causes are measured by their effects, and we now proceed to a third axiom, also of great importance.

5. Third Axiom. *Reaction is equal and opposite to Action.*

In the case of mechanical forces, the action of a cause often takes place by an operation of one body upon another; and in this case, the action is always and inevitably accompanied by an *opposite* action. If I press a stone with my hand, the stone presses my hand in return. If one ball strike another and put it in motion, the second ball diminishes the motion of the first. In these cases the operation is mutual; the Action is accompanied by a Reaction. And in all such cases the Reaction is a force of exactly the same nature as the Action, exerted in an opposite direction. A pressure exerted upon a body at rest is resisted and balanced by another pressure; when the pressure of one body puts another in motion, the body, though it yields to the force, nevertheless exerts upon the pressing body a force like that which it suffers.

Now the axiom asserts further, that this Reaction is *equal*, as well as opposite, to the Action. For the Reaction is an effect of the Action, and is determined by it. And since the two, Action and Reaction, are forces of the same nature, each may be considered as cause and as effect; and they must, therefore, determine each other by a common rule. But this consideration leads necessarily to their equality: for since the rule is mutual, if we could for an instant suppose the Reaction to be less than the Action, we must, by the same rule, suppose the Action to be less than the Reaction. And thus Action and Reaction, in every such case, are rigorously equal to each other.

It is easily seen that this axiom is not a proposition which is, or can be, proved by experience; but that its truth is anterior to special observation, and depends on our conception of Action and Reaction. Like our other axioms, this has its source in an Idea; namely, the Idea of Cause, under that particular condition in which cause and effect are mutual. The necessary and universal truth which we cannot help ascribing to the axiom, shows that it is not derived from the stores of experience, which can never contain truths of this character. Accordingly, it was asserted with equal confidence and generality by those who did not refer to experience for their principles, and by those who did. Leonicus Tomæus, a commentator of Aristotle, whose work was published in 1552, and therefore at a period when no right opinions concerning mechanical reaction were current, at least in his school, says, in his remarks on the Author's Questions concerning the communication of motion, that "Reaction is equal and contrary to Action." The same principle was taken for granted by all parties, in all the controversies concerning the proper measure of force, of which we shall have to speak: and would be rigorously true, as a law of motion, whichever of the rival interpretations of the measure of the term "Action" we were to take.

6. *Extent of the Third Axiom.*—It may naturally be asked whether this third Axiom respecting causation extends to any other cases than those of mechanical action, since the notion of Cause in general has certainly a much wider extent. For instance, when a hot body

heats a cold one, is there necessarily an equal reaction of the second body upon the first? Does the snowball cool the boy's hand exactly as much as the hand heats the snow? To this we reply, that, in every case in which one body acts upon another by its physical qualities, there must be some reaction. No body can affect another without being itself also affected. But in any physical change the *action* exerted is an abstract term which may be variously understood. The hot hand may *melt* a cold body, or may *warm* it: which kind of effect is to be taken as action? This remains to be determined by other considerations.

In all cases of physical change produced by one body in another, it is generally possible to assume such a meaning of action, that the reaction shall be of the same nature as the action; and when this is done, the third axiom of causation, that reaction is equal to action, is universally true. Thus if a hot body heat a cold one, the change may be conceived as the transfer of a certain substance, *heat* or *caloric*, from the first body to the second. On this supposition, the first body *loses* just as much heat as the other *gains;* action and reaction are equal. But if the reaction be of a different kind to the action we can no longer apply the axiom. If a hot body *melt* a cold one, the latter *cools* the former: here, then, is reaction; but so long as the action and reaction are stated in this form, we cannot assert any equality between them.

In treating of the secondary mechanical sciences, we shall see further in what way we may conceive the physical action of one body upon another, so that the same axioms which are the basis of the science of Mechanics shall apply to changes not at first sight manifestly mechanical.

The three axioms of causation which we have now stated are the fundamental maxims of all reasoning concerning causes as to their quantities; and it will be shown in the sequel that these axioms form the basis of the science of Mechanics, determining its form, extent, and certainty. We must, however, in the first place, consider how we acquire those conceptions upon which the axioms now established are to be employed.

CHAPTER V.

OF THE ORIGIN OF OUR CONCEPTIONS OF FORCE AND MATTER.

1. *Force.*—WHEN the faculties of observation and thought are developed in man, the idea of causation is applied to those changes which we see and feel in the state of rest and motion of bodies around us. And when our abstract conceptions are thus formed and named, we adopt the term *Force*, and use it to denote that property which is the cause of motion produced, changed, or prevented. This conception is, it would seem, mainly and primarily suggested by our consciousness of the exertions by which we put bodies in motion. The Latin and Greek words for *Force*, *Vis*, Fἰς, were probably, like all abstract terms, derived at first from some sensible object. The original meaning of the Greek word was a *muscle* or *tendon*. Its first application as an abstract term is accordingly to muscular force.

> *Δεύτερος αὖτ' Αἴας πολὺ μείζονα λᾶαν ἀείρας*
> *ἧκ' ἐπιδινήσας, ἐπέρεισε δὲ* FῖN' *ἀπελεθρον.*
>
> Then Ajax a far heavier stone upheaved,
> He whirled it, and impressing Force intense
> Upon the mass, dismist it.

The property by which bodies affect each other's motions, was naturally likened to that energy which we exert upon them with similar effect: and thus the labouring horse, the rushing torrent, the descending weight, the elastic bow, were said to exert force. Homer* speaks of the *force* of the river, Fὶς ποταμοῖο; and Hesiod† of the *force* of the north wind, Fὶς ἀνέμου βορέαο.

Thus man's general notion of force was probably first suggested by his muscular exertions, that is, by an act depending upon that muscular sense, to which, as we have already seen, the perception of space is mainly due. And this being the case, it will be easily understood that the *Direction* of the force thus exerted is perceived by the muscular sense, at the same time that the force itself is perceived; and that the direction of any other force is understood by comparison with force which man must exert to produce the same effect, in the same manner as force itself is so understood.

This abstract notion of Force long remained in a very vague and obscure condition, as may be seen by referring to the History for the failures of attempts at a science of force and motion, made by the ancients and their commentators in the middle ages. By degrees, in modern times, we see the scientific faculty revive. The conception of Force becomes so far distinct and precise that it can be reasoned upon in a consistent manner, with demonstrated consequences; and a genuine science of Mechanics comes into existence. The foundations of this science are to be found in the Axioms concerning causation which we have already stated; these axioms being interpreted and fixed in their application by a constant reference to observed facts, as we shall show. But we must, in the first place, consider further those primary processes of observation by which we acquire the first materials of thought on such subjects.

2. *Matter.*—The conception of Force, as we have said,

* *Il.* XXI. † *Op. et D.*

arises with our consciousness of our own muscular exertions. But we cannot imagine such exertions without also imagining some bodily substance against which they are exercised. If we press, we press something: if we thrust or throw, there must be something to resist the thrust or to receive the impulse. Without body, muscular force cannot be exerted and force in general is not conceivable.

Thus Force cannot exist without *Body* on which it acts. The two conceptions, Force and Matter, are coexistent and correlative. Force implies resistance; and the force is effective only when the resistance is called into play. If we grasp a stone, we have no hold of it till the closing of the hand is resisted by the solid texture of the stone. If we push open a gate, we must surmount the opposition which it exerts while turning on its hinges. However slight the resistance be, there must be some resistance, or there would be no force. If we imagine a state of things in which objects do not resist our touch, they must also cease to be influenced by our strength. Such a state of things we sometimes imagine in our dreams; and such are the poetical pictures of the regions inhabited by disembodied spirits. In these, the figures which appear are conspicuous to the eye, but impalpable like shadow or smoke; and as they do not resist the corporeal impressions, so neither do they obey them. The spectator tries in vain to strike or to grasp them.

> Et ni cana vates tenues sine corpore vitas
> Admoneat volitare cavâ sub imagine formæ,
> Irruat ac frustra ferro diverberet umbras.

> The Sibyl warns him that there round him fly
> Bodiless things, but substance to the eye;
> Else had he pierced those shapes with life-like face,
> And smitten, fierce, the unresisting space.

Neque illum
Prensantem nequicquam umbras et multa volentem
Dicere, preterea vidit.

He grasps her form, and clutches but the shade.

Such may be the circumstances of the unreal world of dreams, or of poetical fancies approaching to dreams: for in these worlds our imaginary perceptions are bound by no rigid conditions of force and reaction. In such cases, the mind casts off the empire of the idea of cause, as it casts off even the still more familiar sway of the ideas of space and time. But the character of the material world in which we live when awake is, that we have at every instant and at every place, force operating on matter and matter resisting force.

3. *Solidity.*—From our consciousness of muscular exertion, we derive, as we have seen, the conception of force, and with that also the conception of matter. We have already shown, in a former chapter, that the same part of our frame, the muscular system, is the organ by which we perceive extension and the relations of space. Thus the same organ gives us the perception of body as resisting force, and as occupying space; and by combining these conditions we have the conception of *solid* extended bodies. In reality, this resistance is inevitably presented to our notice in the very facts from which we collect the notion of extension. For the action of the hand and arm by which we follow the forms of objects, implies that we apply our fingers to their surface; and we are stopped there by the resistance which the body offers. This resistance is precisely that which is requisite in order to make us conscious of our muscular effort*. Neither touch, nor any other mere passive sensation, could produce the perception of extent, as we have already urged: nor could the muscular sense lead to such

* Brown's *Lectures*, I. 466.

a perception, except the extension of the muscles were felt to be resisted. And thus the perception of resistance enters the mind along with the perception of extended bodies. All the objects with which we have to do are not only extended but solid.

This sense of the term *solidity*, (the general property of all matter,) is different to that in which we oppose *solidity* to *fluidity*. We may avoid ambiguity by opposing *rigid* to *fluid* bodies. By solid bodies, as we now speak of them, we mean only such as resist the pressure which we exert, so long as their parts continue in their places. By fluid bodies, we mean those whose parts are, by a slight pressure, removed out of their places. A drop of water ceases to prevent the contact of our two hands, not by ceasing to have solidity in this sense, but by being thrust out of the way. If it could remain in its place, it could not cease to exercise its resistance to our pressure, except by ceasing to be matter altogether.

The perception of solidity, like the perception of extension, implies an act of the mind, as well as an impression of the senses: as the perception of extension implies the idea of space, so the perception of solidity implies the idea of action and reaction. That an Idea is involved in our knowledge on this subject appears, as in other instances, from this consideration, that the convictions of persons, even of those who allow of no ground of knowledge but experience, do in fact go far beyond the possible limits of experience. Thus Locke says*, that "the bodies which we daily handle hinder by an *insurmountable* force the approach of the parts of our hands that press them." Now it is manifest that our observation can never go to this length. By our senses we can only perceive that bodies resist the greatest actual forces that we exert upon them. But our conception of force

* *Essay*, B. II. c. 4.

carries us further: and since, so long as the body is there to receive the action of the force, it must suffer the whole of that action, and must react as much as it suffers: it is therefore true, that so long as the body remains there, the force which is exerted upon it can never surmount the resistance which the body exercises. And thus this doctrine, that bodies resist the intrusion of other bodies by an insurmountable force, is, in fact, a consequence of the axiom that the reaction is always equal to the action.

4. *Inertia.*—But this principle of the equality of action and reaction appears also in another way. Not only when we exert force upon bodies at rest, but when, by our exertions, we put them in motion, they react. If we set a large stone in motion, the stone resists; for the operation requires an effort. By increasing the effort, we can increase the effect, that is, the motion produced; but the resistance still remains. And the greater the stone moved, the greater is the effort requisite to move it. There is, in every case, a resistance to motion, which shows itself, not in preventing the motion, but in a reciprocal force, exerted backwards upon the agent by which the motion is produced. And this resistance resides in each portion of matter, for it is increased as we add one portion of matter to another. We can push a light boat rapidly through the water; but we may go on increasing its freight, till we are barely able to stir it. This property of matter, then, by which it resists the reception of motion, or rather by which it reacts and requires an adequate force in order that any motion may result, is called its inertness, or *inertia*. That matter has such a property, is a conviction flowing from that idea of a reaction equal and opposite to the action, which the conception of all force involves. By what laws this inertia depends on the magnitude, form, and material of

the body, must be the subject of our consideration hereafter. But that matter has this inertia, in virtue of which, as the matter is greater, the velocity which the same effort can communicate to it is less, is a principle inseparable from the notion of matter itself.

Hermann says that Kepler first introduced this "most significant word" *inertia*. Whether it is to be found in earlier writers I know not; Kepler certainly does use it familiarly in those attempts to assign physical reasons for the motions of the planets which were among the main occasions of the discovery of the true laws of mechanics. He assumes the slowness of the motions of the planets to increase, (other causes remaining the same,) as the inertia increases; and though, even in this assumption, there is an errour involved, (if we adopt that interpretation of the term *inertia* to which subsequent researches led,) the introduction of such a word was one step in determining and expressing those laws of motion which depend on the fundamental principle of the equality of action and reaction.

5. We have thus seen, I trust in a satisfactory manner, the origin of our conceptions of Force, Matter, Solidity, and Inertness. It has appeared that the organ by which we obtain such conceptions is that very muscular frame, which is the main instrument of our perceptions of space; but that, besides bodily sensations, these ideal conceptions, like all the others which we have hitherto considered, involve also an habitual activity of the mind, giving to our sensations a meaning which they could not otherwise possess. And among the ideas thus brought into play, is an idea of action with an equal and opposite reaction, which forms a foundation for universal truths to be hereafter established respecting the conceptions thus obtained.

We must now endeavour to trace in what manner these fundamental principles and conceptions are unfolded by means of observation and reasoning, till they become an extensive yet indisputable science.

Chapter VI.

OF THE ESTABLISHMENT OF THE PRINCIPLES OF STATICS.

1. *Object of the Chapter.*—In the present and the succeeding chapters we have to show how the general axioms of Causation enable us to construct the science of Mechanics. We have to consider these axioms as moulding themselves, in the first place, into certain fundamental mechanical principles, which are of evident and necessary truth in virtue of their dependence upon the general axioms of Causation; and thus as forming a foundation for the whole structure of the science;—a system of truths no less necessary than the fundamental principles, because derived from these by rigorous demonstration.

This account of the construction of the science of Mechanics, however generally treated, cannot be otherwise than technical in its details, and will probably be imperfectly understood by any one not acquainted with Mechanics as a mathematical science.

I cannot omit this portion of my survey without rendering my work incomplete; but I may remark that the main purpose of it is to prove, in a more particular manner, what I have already declared in general, that there are, in Mechanics no less than in Geometry, fundamental principles of axiomatic evidence and necessity; —that these principles derive their axiomatic character from the Idea which they involve, namely the Idea of

Cause;—and that through the combination of principles of this kind, the whole science of Mechanics, including its most complex and remote results, exists as a body of solid and universal truths.

2. *Statics and Dynamics.*—We must first turn our attention to a technical distinction of Mechanics into two portions, according as the forces about which we reason produce rest, or motion; the former portion is termed *Statics*, the latter *Dynamics*. If a stone fall, or a weight put a machine in motion, the problem belongs to Dynamics; but if the stone rest upon the ground, or a weight be merely supported by a machine, without being raised higher, the question is one of Statics.

3. *Equilibrium.*—In Statics, forces *balance* each other, or keep each other *in equilibrium*. And forces which directly balance each other, or keep each other in equilibrium, are necessarily and manifestly equal. If we see two boys pull at two ends of a rope so that neither of them in the smallest degree prevails over the other, we have a case in which two forces are in equilibrium. The two forces are evidently equal, and are a statical exemplification of action and reaction, such as are spoken of in the third axiom concerning causes. Now the same exemplification occurs in every case of equilibrium. No point or body can be kept at rest except in virtue of opposing forces acting upon it; and these forces must always be equal in their opposite effect. When a stone lies on the floor, the weight of the stone downwards is opposed and balanced by an equal pressure of the floor upwards. If the stone rests on a slope, its tendency to slide is counteracted by some equal and opposite force, arising, it may be, from the resistance which the sloping ground opposes to any motion along its surface. Every case of rest is a case of equilibrium:

every case of equilibrium is a case of equal and opposite forces.

The most complex frame-work on which weights are supported, as the roof of a building, or the cordage of a machine, are still examples of equilibrium. In such cases we may have many forces all combining to balance each other; and the equilibrium will depend on various conditions of direction and magnitude among the forces. And in order to understand what are these conditions, we must ask, in the first place, what we understand by the magnitude of such forces;—what is the measure of statical forces.

4. *Measure of Statical Forces.*—At first we might expect, perhaps, that since statical forces come under the general notion of Cause, the mode of measuring them would be derived from the second axiom of Causation, that causes are measured by their effects. But we find that the application of this axiom is controlled by the limitation which we noticed, after stating that axiom; namely, the condition that the causes shall be capable of addition. Further, as we have seen, a statical force produces no other effect than this, that it balances some other statical force; and hence the measure of statical forces is necessarily dependent upon their balancing, that is, upon the equality of action and reaction.

That *statical forces are capable of addition* is involved in our conception of such forces. When two men pull at a rope in the same direction, the forces which they exert are added together. When two heavy bodies are put into a basket suspended by a string, their weights are added, and the sum is supported by the string.

Combining these considerations, it will appear that the measure of statical forces is necessarily given at once by the fundamental principle of the equality of action and reaction. Since two opposite forces which balance each other are equal, each force is measured by that which it balances; and since forces are capable of addition, a force of any magnitude is measured by adding together a proper number of such equal forces. Thus a heavy body which, appended to some certain elastic branch of a tree, would bend it down through one inch, may be taken as a unit of weight. Then if we remove this first body, and find a second heavy body which will also bend the branch through the same space, this is also a unit of weight; and in like manner we might go on to a third and a fourth equal body; and adding together the two, or the three, or the four heavy bodies, we have a force twice, or three times, or four times the unit of weight. And with such a collection of heavy bodies, or *weights*, we can readily measure all other forces; for the same principle of the equality of action and reaction leads at once to this maxim, that any statical force is measured by the weight which it would support.

As has been said, it might at first have been supposed that we should have to apply, in this case, the axiom that causes are measured by their effects in another manner; that thus, if that body were a unit of weight which bent the bough of a tree through one inch, *that* body would be *two* units which bent it through *two* inches, and so on. But, as we have already stated, the measures of weight must be subject to this condition, that they are susceptible of being added: and therefore we cannot take the deflexion of the bough for our measure, till we have ascertained, that which experience alone can teach us, that under the burden of two equal weights, the deflexion will be twice as great as it is with one weight, which is not true, or at least is neither obviously nor necessarily true. In this, as in all other cases, although causes must be measured by their effects, we learn from experience only how the effects are to be interpreted, so as to give a true and consistent measure.

With regard, however, to the measure of statical force, and of weight, no difficulty really occurred to philosophers from the time when they first began to speculate on such subjects; for it was easily seen that if we take any uniform material, as wood, or stone, or iron, portions of this which are geometrically equal, must also be equal in statical effect; since this was implied in the very hypothesis of a uniform material. And a body ten times as large as another of the same substance, will be of ten times the weight. But before men could establish by reasoning the conditions under which weights would be in equilibrium, some other principles were needed in addition to the mere measure of forces. The principles introduced for this purpose still resulted from the conception of equal action and reaction; but it required no small clearness of thought to select them rightly, and to employ them successfully. This, however, was done, to a certain extent, by the Greeks; and the treatise of Archimedes *On the Center of Gravity*, is founded on principles which may still be considered as the genuine basis of statical reasoning. I shall make a few remarks on the most important principle among those which Archimedes thus employs.

5. *The Center of Gravity.*—The most important of the principles which enter into the demonstration of Archimedes is this: that "Every body has a center of gravity;" meaning by the center of gravity, a point at which the whole matter of the body may be supposed to be collected, to all intents and purposes of statical reasoning. This principle has been put in various forms by succeeding writers: for instance, it has been thought sufficient to assume a case much simpler than the general one; and to assert that two *equal* bodies have their

center of gravity in the point midway between them. It is to be observed, that this assertion not only implies that the two bodies will *balance* upon a support placed at that midway point, but also, that they will exercise, upon such a support, a *pressure equal to their sum;* for this point being the center of gravity, the whole matter of the two bodies may be conceived to be collected there, and therefore the whole weight will press there. And thus the principle in question amounts to this, that *when two equal heavy bodies are supported on the middle point between them, the pressure upon the support is equal to the sum of the weights of the bodies.*

A clear understanding of the nature and grounds of this principle is of great consequence: for in it we have the foundation of a large portion of the science of Mechanics. And if this principle can be shown to be necessarily true, in virtue of our Fundamental Ideas, we can hardly doubt that there exist many other truths of the same kind, and that no sound view of the evidence and extent of human knowledge can be obtained, so long as we mistake the nature of these, its first principles.

The above principle, that the pressure on the support is equal to the sum of the bodies supported, is often stated as an axiom in the outset of books on Mechanics. And this appears to be the true place and character of this principle, in accordance with the reasonings which we have already urged. The axiom depends upon our conception of action and reaction. That the two weights are supported, implies that the supporting force must be equal to the force or weight supported.

In order further to show the foundation of this principle, we may ask the question:—If it be not an axiom, deriving its truth from the fundamental conception of equal action and reaction, which equilibrium always implies, what is the origin of its certainty? The principle is never for an instant denied or questioned: it is taken for granted, even before it is stated. No one will doubt that it is not only true, but true with the same rigour and universality as the axioms of Geometry. Will it be said, that it is borrowed from experience? Experience could never prove a principle to be universally and rigorously true. Moreover, when from experience we prove a proposition to possess great exactness and generality, we approach by degrees to this proof: the conviction becomes stronger, the truth more secure, as we accumulate trials. But nothing of this kind is the case in the instance before us. There is no gradation from less to greater certainty;—no hesitation which precedes confidence. From the first, we know that the axiom is exactly and certainly true. In order to be convinced of it, we do not require many trials, but merely a clear understanding of the assertion itself.

But in fact, not only are trials not necessary to the proof, but they do not strengthen it. Probably no one ever made a trial for the purpose of showing that the pressure upon the support is equal to the sum of the two weights. Certainly no person with clear mechanical conceptions ever wanted such a trial to convince him of the truth; or thought the truth clearer after the trial had been made. If to such a person, an experiment were shown which seemed to contradict the principle, his conclusion would be, not that the principle was doubtful, but that the apparatus was out of order. Nothing can be less like collecting truth from experience than this.

We maintain, then, that this equality of mechanical action and reaction, is one of the principles which do not flow from, but regulate our experience. To this principle, the facts which we observe must conform; and we cannot help interpreting them in such a manner that they shall be exemplifications of the principle. A mechanical pressure not accompanied by an equal and opposite pressure, can no more be given by experience, than two unequal right angles. With the supposition of such inequalities, space ceases to be space, force ceases to be force, matter ceases to be matter. And this equality of action and reaction, considered in the case in which two bodies are connected so as to act on a single support, leads to the axiom which we have stated above, and which is one of the main foundations of the science of Mechanics.

6. *Oblique Forces.*—By the aid of this axiom and a few others, the Greeks made some progress in the science of Statics. But after a short advance, they arrived at another difficulty, that of Oblique Forces, which they never overcame; and which no mathematician mastered till modern times. The unpublished manuscripts of Leonardo da Vinci, written in the fifteenth century, and the works of Stevinus and Galileo, in the sixteenth, are the places in which we find the first solid grounds of reasoning on the subject of forces acting obliquely to each other. And mathematicians, having thus become possessed of all the mechanical principles which are requisite in problems respecting equilibrium, soon framed a complete science of Statics. Succeeding writers presented this science in forms variously modified; for it was found, in Mechanics as in Geometry, that various propositions might be taken as the starting points; and that the collection of truths which it was the mechanician's business to include in his course, might thus be traversed by various routes, each path offering a series of satisfactory demonstrations. The fundamental conceptions of force and resistance, like those of space and number, could be contemplated under different aspects, each of which might be made the basis of axioms, or of principles employed as axioms. Hence the

grounds of the truth of Statics may be stated in various ways; and it would be a task of some length to examine all these completely, and to trace them to their Fundamental Ideas. This I shall not undertake here to do; but the philosophical importance of the subject makes it proper to offer a few remarks on some of the main principles involved in the different modes of presenting Statics as a rigorously demonstrated science.

7. *A Force may be supposed to act at any Point of its Direction.*—It has been stated in the history of Mechanics*, that Leonardo da Vinci and Galileo obtained the true measure of the effect of oblique forces, by reasonings which were, in substance, the same. The principle of these reasonings is that expressed at the head of this paragraph; and when we have a little accustomed ourselves to contemplate our conceptions of force, and its action on matter, in an abstract manner, we shall have no difficulty in assenting to the principle in this general form. But it may, perhaps, be more obvious at first in a special case.

If we suppose a wheel, moveable about its axis, and carrying with it in its motion a weight, (as, for example, one of the wheels by means of which the large bells of a church are rung,) this weight may be supported by means of a rope (not passing along the circumference of the wheel, as is usual in the case of bells,) but fastened to one of the spokes of the wheel. Now the principle which is enunciated above asserts, that if the rope pass in a straight line across several of the spokes of the wheel, it makes no difference in the mechanical effect of the force applied, for the purpose of putting the bell in motion, to *which* of these spokes the rope is *fastened*. In each case, the fastening of the rope to the wheel merely serves to enable the force to produce motion about the centre;

* *Hist. Ind. Sci.*, B. VI. c. i. sect. 2. and Note (A).

and so long as the force acts in the same line, the effect is the same, at whatever point of the rope the line of action finishes.

This axiom very readily aids us in estimating the effect of oblique forces. For when a force acts on one of the arms of a lever at any oblique angle, we suppose another arm projecting from the centre of motion, like another spoke of the same wheel, so situated that it is perpendicular to the force. This arm we may, with Leonardo, call the *virtual lever;* for, by the axiom, we may suppose the force to act where the line of its direction meets this arm; and thus we reduce the case to that in which the force acts perpendicularly on the arm.

The ground of this axiom is, that matter, in Statics, is necessarily conceived as *transmitting* force. That force can be transmitted from one place to another, by means of matter;—that we can push with a rod, pull with a rope,—are suppositions implied in our conceptions of force and matter. Matter is, as we have said, that which receives the impression of force, and the modes just mentioned, are the simplest ways in which that impression operates. And since, in any of these cases, the force might be resisted by a reaction equal to the force itself, the reaction in each case would be equal, and, therefore, the action in each case is necessarily equal; and thus the forces must be transmitted, from one point to another, without increase or diminution.

This property of matter, of transmitting the action of force, is of various kinds. We have the coherence of a rope which enables us to pull, and the rigidity of a staff, which enables us to push with it in the direction of its length; and again, the same staff has a rigidity of another kind, in virtue of which we can use it as a lever; that is, a rigidity to resist flexure, and to transmit the force which turns a body round a fulcrum. There is, further, the rigidity by which a solid body resists *twisting*. Of these kinds of rigidity, the first is that to which our axiom refers; but in order to complete the list of the elementary principles of Statics, we ought also to lay down axioms respecting the other kinds of rigidity*. These, however, I shall not here state, as they do not involve any new principle. Like the one just considered, they form part of our fundamental conception of matter; they are not the results of any experience, but are the hypotheses to which we are irresistibly led, when we would liberate our reasonings concerning force and matter from a dependence on the special results of experience. We cannot even conceive (that is, if we have any clear mechanical conceptions at all) the force exerted by the point of a staff and resisting the force which we steadily impress on the head of it, to be different from the impressed force.

8. *Forces may have equivalent Forces substituted for them. The Parallelogram of Forces.*—It has already been observed, that in order to prove the doctrines of Statics, we may take various principles as our starting points, and may still find a course of demonstration by which the leading propositions belonging to the subject may be established. Thus, instead of beginning our reasonings, as in the last section we supposed them to commence, with the case in which forces act upon different points of the same body in the same line of force, and counteract each other in virtue of the intervening matter by which the effect of force is transferred from one point to another, we may suppose different forces to act at the same point, and may thus commence our reasonings with a case in which we have to contemplate force, without having to take into our account

* Such axioms are given in a little work (*The Mechanical Euclid*) which I published on the Elements of Mechanics.

the resistance or rigidity of matter. Two statical forces, thus acting at a mathematical point, are equivalent, in all respects, to some single force acting at the same point; and would be kept in equilibrium by a force equal and opposite to that single force. And the rule by which the single force is derived from the two, is commonly termed *the parallelogram of forces;* the proposition being this,—That if the two forces be represented in magnitude and direction by the two sides of a parallelogram, the resulting force will be represented in the same manner by the diagonal of the parallelogram. This proposition has very frequently been made, by modern writers, the commencement of the science of Mechanics: a position for which, by its simplicity, it is well suited; although, in order to deduce from it the other elementary propositions of the science, as, for instance, those respecting the lever, we require the axiom stated in the last section.

9. *The Parallelogram of Forces is a necessary Truth.* —In the series of discussions in which we are here engaged, our main business is to ascertain the nature and grounds of the certainty of scientific truths. We have, therefore, to ask whether this proposition, the parallelogram of forces, be a necessary truth; and if so, on what grounds its necessity ultimately rests. We shall find that this, like the other fundamental doctrines of Statics, justly claims a demonstrative certainty. Daniel Bernoulli, in 1726, gave the first proof of this important proposition on pure statical principles; and thus, as he says*, "proved that statical theorems are not less necessarily true than geometrical are." If we examine this proof of Bernoulli, in order to discover what are the principles on which it rests, we shall find that the reasoning employs in its progress such axioms as this;—That if from forces which are in equilibrium at a point

* *Comm. Petrop.* Vol. I.

be taken away other forces which are in equilibrium at the same point, the remainder will be in equilibrium; and generally;—That if forces can be resolved into other equivalent forces, these may be separated, grouped, and recombined, in any new manner, and the result will still be identical with what it was at first. Thus in Bernoulli's proof, the two forces to be compounded are represented by P and Q; P is resolved into two other forces, X and U; and Q into two others, Y and V, under certain conditions. It is then assumed that these forces may be grouped into the pairs X, Y, and U, V: and when it has been shown that X and Y are in equilibrium, they may, by what has been said, be removed, and the forces, P, Q, are equivalent to U, V; which, being in the same direction by the course of the construction, have a result equal to their sum.

It is clear that the principles here assumed are genuine axioms, depending upon our conception of the nature of equivalence of forces, and upon their being capable of addition and composition. If the forces P, Q, be *equivalent* to forces X, U, Y, V, they are equivalent to these forces added and compounded in any order; just as a geometrical figure is, by our conception of space, equivalent to its parts added together in any order. The apprehension of forces as having magnitude, as made up of parts, as capable of composition, leads to such axioms in Statics, in the same manner as the like apprehension of space leads to the axioms of Geometry. And thus the truths of Statics, resting upon such foundations, are independent of experience in the same manner in which geometrical truths are so.

The proof of the parallelogram of forces thus given by Daniel Bernoulli, as it was the first, is also one of the most simple proofs of that proposition which have been devised up to the present day. Many other demonstrations, however, have been given of the same proposition. Jacobi, a German mathematician, has collected and examined eighteen of these*. They all depend either upon such principles as have just been stated; That forces may in every way be replaced by those which are equivalent to them;—or else upon those previously stated, the doctrine of the lever, and the transfer of a force from one point to another of its direction. In either case, they are necessary results of our statical conceptions, independent of any observed laws of motion, and indeed, of the conception of actual motion altogether.

There is another class of alleged proofs of the parallelogram of forces, which involve the consideration of the motion produced by the forces. But such reasonings are, in fact, altogether irrelevant to the subject of Statics. In that science, forces are not measured by the motion which they produce, but by the forces which they will balance, as we have already seen. The combination of two forces employed in producing motion in the same body, either simultaneously or successively, belongs to that part of Mechanics which has motion for its subject, and is to be considered in treating of the laws of motion. The composition of motion, (as when a man moves in a ship while the ship moves through the water,) has constantly been confounded with the composition of force. But though it has been done by very eminent mathematicians, it is quite necessary for us to keep the two subjects distinct, in order to see the real nature of the evidence of truth in either case. The conditions of equilibrium of two forces on a lever, or of three forces at

* These are by the following mathematicians; D. Bernoulli (1726); Lambert (1771); Scarella (1756); Venini (1764); Araldi (1806); Wachter (1815); Kæstner; Marini; Eytelwein; Salimbeni; Duchayla; two different proofs by Foncenex (1760); three by D'Alembert; and those of Laplace and M. Poisson.

a point, can be established without any reference whatever to any motions which the forces might, under *other* circumstances, produce. And because this can be done, to do so is the only scientific procedure. To prove such propositions by any other course, would be to support truth by extraneous and inconclusive reasons; which would be foreign to our purpose, since we seek not only knowledge, but the grounds of our knowledge.

10. *The Center of gravity seeks the lowest place.*—The principles which we have already mentioned afford a sufficient basis for the science of Statics in its most extensive and varied applications; and the conditions of equilibrium of the most complex combinations of machinery may be deduced from these principles with a rigour not inferior to that of geometry. But in some of the more complex cases, the results of long trains of reasoning may be foreseen, in virtue of certain maxims which appear to us self-evident, although it may not be easy to trace the exact dependence of these maxims upon our fundamental conceptions of force and matter. Of this nature is the maxim now stated;—That in any combination of matter any how supported, the Center of Gravity will descend into the lowest position which the connexion of the parts allows it to assume by descending. It is easily seem that this maxim carries to a much greater extent the principle which the Greek mathematicians assumed, that every body has a Center of Gravity, that is, a point in which, if the whole matter of the body be collected, the effect will remain unchanged. For the Greeks asserted this of a single rigid mass only; whereas, in the maxim now under our notice, it is asserted of any masses, connected by strings, rods, joints, or in any manner. We have already seen that more modern writers on mechanics, desirous of assuming as fundamental no wider principles than are absolutely necessary,

have not adopted the Greek axiom in all its generality, but have only asserted that two *equal* weights have a center of gravity midway between them. Yet the principle that every body, however irregular, has a center of gravity, and will be supported if that center is supported, and not otherwise, is so far evident, that it might be employed as a fundamental truth, if we could not resolve it into any simpler truths: and, historically speaking, it was assumed as evident by the Greeks. In like manner the still wider principle, that a collection of bodies, as, for instance, a flexible chain hanging upon one or more supports, has a center of gravity; and that this point will descend to the lowest possible situation, as a single body would do, has been adopted at various periods in the history of mechanics; and especially at conjunctures when mathematical philosophers have had new and difficult problems to contend with. For in almost every instance it has only been by repeated struggles that philosophers have reduced the solution of such problems to a clear dependence upon the most simple axioms.

11. *Stevinus's Proof for Oblique Forces.*—We have an example of this mode of dealing with problems, in Stevinus's mode of reasoning concerning the Inclined Plane; which, as we have stated in the History of Mechanics, was the first correct published solution of that problem. Stevinus supposes a loop of chain, or a loop of string loaded with a series of equal balls at equal distances, to hang over the Inclined Plane; and his reasoning proceeds upon this assumption,—That such a loop so hanging will find a certain position in which it will rest: for otherwise, says he*, its motion must go on for ever, which is absurd. It may be asked how this absurdity of a perpetual motion appears; and it will perhaps be added, that although the impossibility of a machine

* Stevin. *Statique*, Livre I., prop. 19.

with such a condition may be proved as a remote result of mechanical principles, this impossibility can hardly be itself recognized as a self-evident truth. But to this we may reply, that the impossibility is really evident in the case contemplated by Stevinus; for we cannot conceive a loop of chain to go on through all eternity, sliding round and round upon its support, by the effect of its own weight. And the ground of our conviction that this cannot be, seems to be this consideration; that when the chain moves by the effect of its weight, we consider its motion as the result of an effort to reach some certain position, in which it can rest; just as a single ball in a bowl moves till it comes to rest at the lowest point of the bowl. Such an effect of weight in the chain, we may represent to ourselves by conceiving all the matter of the chain to be collected in one single point, and this single heavy point to hang from the support in some way or other, so as fitly to represent the mode of support of the chain. In whatever manner this heavy point (the center of gravity of the chain) be supported and controlled in its movements, there will still be some position of rest which it will seek and find. And thus there will be some corresponding position of rest for the chain; and the interminable shifting from one position to another, with no disposition to rest in any position, cannot exist.

Thus the demonstration of the property of the Inclined Plane by Stevinus, depends upon a principle which, though far from being the simplest of those to which the case can be reduced, is still both true and evident: and the evidence of this principle, depending upon the assumption of a center of gravity, is of the same nature as the evidence of the Greek statical demonstrations, the earliest real advances in the science.

12. *Principle of Virtual Velocities.*—We have referred above to an assertion often made, that we

may, from the simple principles of Mechanics, demonstrate the impossibility of a perpetual motion. In reality, however, the simplest proof of that impossibility, in a machine acted upon by weight only, arises from the very maxim above stated, that the center of gravity seeks and finds the lowest place; or from some similar proposition. For if, as is done by many writers, we profess to prove the impossibility of a perpetual motion by means of that proposition which includes the conditions of equilibrium, and is called the *Principle of Virtual Velocities**, we are under the necessity of first proving in a general manner that principle. And if this be done by a mere enumeration of cases, (as by taking those five cases which are called the *Mechanical Powers,*) there may remain some doubts whether the enumeration of possible mechanical combinations be complete. Accordingly, some writers have attempted independent and general proofs of the Principle of Virtual Velocities; and these proofs rest upon assumptions of the same nature as that now under notice. This is, for example, the case with Lagrange's proof, which depends upon what he calls the *Principle of Pulleys.* For this principle is,—That a weight any how supported, as by a string passing round any number of pulleys any how placed, will be at rest then only, when it cannot get lower by any small motion of the pulleys. And thus the maxim that a weight will descend if it can, is assumed as the basis of this proof.

There is, as we have said, no need to assume such principles as these for the foundation of our mechanical science. But it is, on various accounts, useful to direct our attention to those cases in which truths, apprehended at first in a complex and derivative form, have afterwards been reduced to their simpler elements;—in which, also, sagacious and inventive men have fixed upon those

* See *Hist. Ind. Sci.*, B. VI. c. ii. sect. 4.

truths as self-evident, which now appear to us only certain in virtue of demonstration. In these cases we can hardly doubt that such men were led to assert the doctrines which they discovered, not by any capricious conjecture or arbitrary selection, but by having a keener and deeper insight than other persons into the relations which were the object of their contemplation; and in the science now spoken of, they were led to their assumptions by possessing clearly and distinctly the conceptions of mechanical cause and effect,—action and reaction,—force, and the nature of its operation.

13. *Fluids press Equally in all Directions.*—The doctrines which concern the equilibrium of fluids depend on principles no less certain and simple than those which refer to the equilibrium of solid bodies; and the Greeks, who, as we have seen, obtained a clear view of some of the principles of Statics, also made a beginning in the kindred subject of Hydrostatics. We still possess a treatise of Archimedes *On Floating Bodies*, which contains correct solutions of several problems belonging to this subject, and of some which are by no means easy. In this treatise, the fundamental assumption is of this kind: "Let it be assumed that the nature of a fluid is such, that the parts which are less pressed yield to those which are more pressed." In this assumption or axiom it is implied that a pressure exerted upon a fluid in one direction produces a pressure in another direction; thus, the weight of the fluid which arises from a downward force produces a lateral pressure against the sides of the containing vessel. Not only does the pressure thus diverge from its original direction into all other directions, but the pressure, is in all directions exactly equal, an equal extent of the fluid being taken. This principle, which was involved in the reasoning of Archimedes, is still to the present day the basis of all hydrostatical treatises, and is

expressed, as above, by saying that *fluids press equally in all directions.*

Concerning this, as concerning previously-noticed principles, we have to ask whether it can rightly be said to be derived from experience. And to this the answer must still be, as in the former cases, that the proposition is not one borrowed from experience in any usual or exact sense of the phrase. I will endeavour to illustrate this. There are many elementary propositions in physics, our knowledge of which indisputably depends upon experience; and in these cases there is no difficulty in seeing the evidence of this dependence. In such cases, the *experiments* which prove the law are prominently stated in treatises upon the subject: they are given with exact measures, and with an account of the means by which errors were avoided: the experiments of more recent times have either rendered more certain the law originally asserted, or have pointed out some correction of it as requisite: and the names, both of the discoverers of the law and of its subsequent reformers, are well known. For instance, the proposition that "The elastic force of air varies as the density," was first proved by Boyle, by means of operations of which the detail is given in his *Defence* of his *Pneumatical Experiments**; and by Marriotte in his *Traité de l'Equilibre des Liquides*, from whom it has generally been termed Marriotte's law. After being confirmed by many other experimenters, this law was suspected to be slightly inaccurate, and a commission of the French Academy of Sciences was appointed, consisting of several distinguished philosophers†, to ascertain the truth or falsehood of this suspicion.

* Shaw's *Boyle*, Vol. II. p. 671.

† The members were Prony, Arago, Ampère, Girard, and Dulong. The experiments were extended to a pressure of twenty-seven atmospheres; and in no instance did the difference between the observed

The result of their investigations appeared to be, that the law is exact, as nearly as the inevitable inaccuracies of machinery and measures will allow us to judge. Here we have an example of a law which is of the simplest kind and form; and which yet is not allowed to rest upon its simplicity or apparent probability, but is rigorously tested by experience. In this case, the assertion, that the law depends upon experience, contains a reference to plain and notorious passages in the history of science.

Now with regard to the principle that fluids press equally in all directions, the case is altogether different. It is, indeed, often asserted in works on hydrostatics, that the principle is collected from experience, and sometimes a few experiments are described as exhibiting its effect; but these are such as to illustrate and explain, rather than to prove, the truth of the principle: they are never related to have been made with that exactness of precaution and measurement, or that frequency of repetition, which are necessary to establish a purely experimental truth. Nor did such experiments occur as important steps in the history of science. It does not appear that Archimedes thought experiment necessary to confirm the truth of the law as he employed it: on the contrary, he states it in exactly the same shape as the axioms which he employs in statics, and even in geometry; namely, as an assumption. Nor does any intelligent student of the subject find any difficulty in assenting to this fundamental principle of hydrostatics as soon as it is propounded to him. Experiment was not requisite for its discovery; experiment is not necessary for its proof at present; and we may add, that experiment,

and calculated elasticity amount to one-hundredth of the whole; nor did the difference appear to increase with the increase of pressure.—Fechner, *Repertorium*, I. 110.

though it may make the proposition more readily intelligible, can add nothing to our conviction of its truth when it is once understood.

14. *Foundation of the above Axiom.*—But it will naturally be asked, What then is the ground of our conviction of this doctrine of the equal pressure of a fluid in all directions? And to this I reply, that the reasons of this conviction are involved in our idea of a fluid, which is considered as matter, and therefore as capable of receiving, resisting, and transmitting force according to the general conception of matter; and which is also considered as matter which has its parts perfectly moveable among one another. For it follows from these suppositions, that if the fluid be confined, a pressure which thrusts in one side of the containing vessel, may cause any other side to bulge outwards, if there be a part of the surface which has not strength to resist this pressure from within. And that this pressure, when thus transferred into a direction different from the original one, is not altered in intensity, depends upon this consideration; that any difference in the two pressures would be considered as a defect of *perfect* fluidity, since the fluidity would be still more complete, if this entire and undiminished transmission of pressure in all directions were supposed. If, for instance, the lateral pressure were less than the vertical, this could be conceived no other way than as indicating some rigidity or adhesion of the parts of the fluid. When the fluidity is perfect, the two pressures which act in the two different parts of the fluid exactly balance each other: they are the action and the reaction; and must hence be equal by the same necessity as two directly opposite forces in statics.

But it may be urged, that even if we grant that this conception of a perfect fluid, as a body which has its parts perfectly moveable among each other, leads us necessarily to the principle of the equality of hydrostatic pressure in all directions, still this conception itself is obtained from experience, or suggested by observation. And to this we may reply, that the conception of a fluid, as contemplated in mechanical theory, cannot be said to be derived from experience, except in the same manner as the conception of a solid and rigid body may be said to be acquired by experience. For if we imagine a vessel full of small, smooth spherical balls, such a collection of balls would approach to the nature of a fluid, in having its parts moveable among each other; and would approach to perfect fluidity, as the balls became smoother and smaller. And such a collection of balls would also possess the statical properties of a fluid; for it would transmit pressure out of a vertical into a lateral (or any other) direction, in the same manner as a fluid would do. And thus a collection of solid bodies has the same property which a fluid has; and the science of Hydrostatics borrows from experience no principles beyond those which are involved in the science of Statics respecting solids. And since in this latter portion of science, as we have already seen, none of the principles depend for their evidence upon any special experience, the doctrines of Hydrostatics also are not proved by experience, but have a necessary truth borrowed from the relations of our ideas.

It is hardly to be expected that the above reasoning will, at first sight, produce conviction in the mind of the reader, except he have, to a certain extent, acquainted himself with the elementary doctrines of the science of Hydrostatics as usually delivered; and have followed, with clear and steady apprehension, some of the trains of reasoning by which the pressures of fluids are determined; as, for instance, the explanation of what is called *the Hydrostatic Paradox*. The necessity of such a discipline in order that the reader may enter fully into this part of our speculations, naturally renders them less popular; but this disadvantage is inevitable in our plan. We cannot expect to throw light upon philosophy by means of the advances which have been made in the mathematical and physical sciences, except we really understand the doctrines which have been firmly established in those sciences. This preparation for philosophizing may be somewhat laborious; but such labour is necessary if we would pursue speculative truth with all the advantages which the present condition of human knowledge places within our reach.

We may add, that the consequences to which we are directed by the preceding opinions, are of very great importance in their bearing upon our general views respecting human knowledge. I trust to be able to show, that some important distinctions are illustrated, some perplexing paradoxes solved, and some large anticipations of the future extension of our knowledge suggested, by means of the conclusions to which the preceding discussions have conducted us. But before I proceed to these general topics, I must consider the foundations of some of the remaining portions of Mechanics.

Chapter VII.

OF THE ESTABLISHMENT OF THE PRINCIPLES OF DYNAMICS.

1. In the History of Mechanics, I have traced the steps by which the three Laws of Motion and the other principles of mechanics were discovered, established, and extended to the widest generality of form and application. We have, in these laws, examples of principles which were, historically speaking, obtained by reference

to experience. Bearing in mind the object and the result of the preceding discussions, we cannot but turn with much interest to examine these portions of science; to inquire whether there be any real difference in the grounds and nature between the knowledge thus obtained, and those truths which we have already contemplated; and which, as we have seen, contain their own evidence, and do not require proof from experiment.

2. *The First Law of Motion.*—The first law of motion is, that *When a body moves not acted upon by any force, it will go on perpetually in a straight line, and with a uniform velocity.* Now what is the real ground of our assent to this proposition? That it is not at first sight a self-evident truth, appears to be clear; since from the time of Aristotle to that of Galileo the opposite assertion was held to be true; and it was believed that all bodies in motion had, by their own nature, a constant tendency to move more and more slowly, so as to stop at last. This belief, indeed, is probably even now entertained by most persons, till their attention is fixed upon the arguments by which the first law of motion is established. It is, however, not difficult to lead any person of a speculative habit of thought to see that the retardation which constantly takes place in the motion of all bodies when left to themselves, is, in reality, the effect of extraneous forces which destroy the velocity. A top ceases to spin because the friction against the ground and the resistance of the air gradually diminish its motion, and not because its motion has any internal principle of decay or fatigue. This may be shown, and was, in fact, shown by Hooke before the Royal Society, at the time when the laws of motion were still under discussion, by means of experiments in which the weight of the top is increased, and the resistance to motion offered by its support, is diminished; for by such contrivances

its motion is made to continue much longer than it would otherwise do. And by experiments of this nature, although we can never remove the whole of the external impediments to continued motion, and although, consequently, there will always be some retardation; and an end of the motion of a body left to itself, however long it may be delayed, must at last come; yet we can establish a conviction that if all resistance could de removed, there would be no diminution of velocity, and thus the motion would go on for ever.

If we call to mind the axioms which we formerly stated, as containing the most important conditions involved in the idea of Cause, it will be seen that our conviction in this case depends upon the first axiom of Causation, that nothing can happen without a cause. Every change in the velocity of the moving body must have a cause; and if the change can, in any manner, be referred to the presence of other bodies, these are said to exert *force* upon the moving body: and the conception of force is thus evolved from the general idea of cause. *Force is any cause which has motion, or change of motion, for its effect*; and thus, all the change of velocity of a body which can be referred to extraneous bodies,—as the air which surrounds it, or the support on which it rests,—is considered as the effect of forces; and this consideration is looked upon as explaining the difference between the motion which really takes places in the experiment, and that motion which, as the law asserts, would take place if the body were not acted on by any forces.

Thus the truth of the first law of motion depends upon the axiom that no change can take place without a cause; and follows from the definition of force, if we suppose that there can be none but an *external* cause of change. But in order to establish the law, it was necessary further to be assured that there is no *internal* cause of change of velocity belonging to all matter whatever, and operating in such a manner that the mere progress of time is sufficient to produce a diminution of velocity in all moving bodies. It appears from the history of mechanical science, that this latter step required a reference to observation and experiment; and that the first law of motion is so far, historically at least, dependent upon our experience.

But notwithstanding this historical evidence of the need which we have of a reference to observed facts, in order to place this first law of motion out of doubt, it has been maintained by very eminent mathematicians and philosophers, that the law is, in truth, evident of itself, and does not really rest upon experimental proof. Such, for example, is the opinion of D'Alembert*, who offers what is called an *à priori* proof of this law; that is, a demonstration derived from our ideas alone. When a body is put in motion, either, he says, the cause which puts it in motion at first, suffices to make it move one foot, or the continued action of the cause during this foot is requisite for the motion. In the first case, the same reason which made the body proceed to the end of the first foot will hold for its going on through a second, a third, a fourth foot, and so on for any number. In the second case, the same reason which made the force continue to act during the first foot, will hold for its acting, and therefore for the body moving during each succeeding foot. And thus the body, once beginning to move, must go on moving for ever.

It is obvious that we might reply to this argument, that the reasons for the body proceeding during each succeeding foot may not necessarily be all the same; for among these reasons may be the time which has elapsed; and thus the velocity may undergo a change as the time

* *Dynamique.*

proceeds: and we require observation to inform us that it does not do so.

Professor Playfair has presented nearly the same argument, although in a different and more mathematical form*. If the velocity change, says he, it must change according to some expression of calculation depending upon the time, or, in mathematical language, must be a *function* of the time. If the velocity diminish as the time increases, this may be expressed by stating the velocity in each case as a certain number, from which another quantity, or *term,* increasing as the time increases, is subtracted. But, Playfair adds, there is no condition involved in the nature of the case, by which the *coefficients*, or numbers which are to be employed, along with the number representing the time, in calculating this second term, can be determined to be of one magnitude rather than of any other. Therefore he infers there can be no such coefficients, and that the velocity is in each case equal to some constant number, independent of the time; and is therefore the same for all times.

In reply to this we may observe, that the circumstance of *our not seeing* in the nature of the case anything which determines for us the coefficients above spoken off, cannot prove that they have not some certain value *in nature.* We do not see in the nature of the case anything which should determine a body to fall sixteen feet in a second of time, rather than one foot or one hundred feet: yet in fact the space thus run through by falling bodies is determined to a certain magnitude. It would be easy to assign a mathematical expression for the velocity of a body, implying that one-hundredth of the velocity, or any other fraction, is lost in each second†:

* *Outlines*, &c., p. 26.

† This would be the case, if, t being the number of seconds elapsed,

and where is the absurdity of supposing such an expression really to represent the velocity?

Most modern writers on mechanics have embraced the opposite opinion, and have ascribed our knowledge of this first law of motion to experience. Thus M. Poisson, one of the most eminent of the mathematicians who have written on this subject, says*, "We cannot affirm *à priori* that the velocity communicated to a body will not become slower and slower of itself, and end by being entirely extinguished. It is only by experience and induction that this question can be decided."

Yet it cannot be denied that there is much force in those arguments by which it is attempted to shew that the First Law of Motion, such as we find it, is more consonant to our conceptions than any other would be. The Law, as it exists, is the most simple that we can conceive. Instead of having to determine by experiments what is the law of the natural change of velocity, we find the Law to be that it does not change at all. To a certain extent, the Law depends upon the evident axiom, that no change can take place without a cause. But the question further occurs, whether the mere lapse of time may not be a cause of change of velocity. In order to ensure this, we have recourse to experiment; and the result is that time alone does not produce any such change. In addition to the conditions of change which we collect from our own Ideas, we ask of Experience what other conditions and circumstances she has to offer; and the answer is, that she can point out none. When we have removed the alterations which external causes, in

and C some constant quantity, the velocity were expressed by this mathematical formula,

$$C\left(\frac{99}{100}\right)^t.$$

* Poisson, *Dynamique*. Ed. 2, Art. 113.

our very conception of them, occasion, there are no longer any alterations. Instead of having to guide ourselves by experience, we learn that on this subject she has nothing to tell us. Instead of having to take into account a number of circumstances, we find that we have only to reject all circumstances. The velocity of a body remains unaltered by time alone, of whatever kind the body itself be.

But the doctrine that time alone is not a cause of change of velocity in any body is further recommended to us by this consideration;—that time is conceived by us not as a cause, but only as a condition of other causes producing their effects. Causes operate *in* time; but it is only when the cause exists, that the lapse of time can give rise to alterations. When therefore all external causes of change of velocity are supposed to be removed, the velocity must continue identical with itself, whatever the time which elapses. An eternity of negation can produce no positive result.

Thus, though the discovery of the First Law of Motion was made, historically speaking, by means of experiment, we have now attained a point of view in which we see that it might have been certainly known to be true independently of experience. This law in its ultimate form, when completely simplified and steadily contemplated, assumes the character of a self-evident truth. We shall find the same process to take place in other instances. And this feature in the progress of science will hereafter be found to suggest very important views with regard both to the nature and prospects of our knowledge.

3. *Gravity is a Uniform Force.*—We shall find observations of the same kind offering themselves in a manner more or less obvious, with regard to the other principles of Dynamics. The determination of the laws

according to which bodies fall downwards by the common action of gravity, has already been noticed in the History of Mechanics*, as one of the earliest positive advances in the doctrine of motion. These laws were first rightly stated by Galileo, and established by reasoning and by experiment, not without dissent and controversy. The amount of these doctrines is this: That gravity is a uniform accelerating force; such a *uniform force* having this for its character, that it makes *the velocity increase in exact proportion to the time of motion*. The relation which the spaces described by the body bear to the times in which they are described, is obtained by mathematical deduction from this definition of the force.

The clear Definition of a uniform accelerating force, and the Proposition that gravity is such a force, were co-ordinate and contemporary steps in this discovery. In defining accelerating force, reference, tacit or express, was necessarily made to the second of the general axioms respecting causation,—That causes are measured by their effects. Force, in the cases now under our notice, is conceived to be, as we have already stated, (p. 217,) any cause which, acting from without, changes the motion of a body. It must, therefore, in this acceptation, be measured by the magnitude of the changes which are produced. But *in what manner* the changes of motion are to be employed as the measures of force, is learnt from observation of the facts which we see taking place in the world. Experience *interprets* the axiom of causation, from which otherwise we could not deduce any real knowledge. We may assume, in virtue of our general conceptions of force, that under the same circumstances, a greater change of motion implies a greater force producing it; but what are we to expect when the

* *Hist. Ind. Sci.*, B. VI. c. ii. sect. 2.

circumstances change? The weight of a body makes it fall from rest at first, and causes it to move more quickly as it descends lower. We may express this by saying, that gravity, the universal force which makes all terrestrial bodies fall when not supported, by its continuous action first *gives* velocity to the body when it has none, and afterwards *adds* velocity to that which the body already has. But how is the velocity added proportioned to the velocity which already exists? Force acting on a body at rest, and on a body in motion, appears under very different conditions;—how are the effects related? Let the force be conceived to be in both cases the *same*, since force is conceived to depend upon the extraneous bodies, and not upon the condition of the moving mass itself. But the force being the same, the effects may still be different. It is at first sight conceivable that the body, acted upon by the same gravity, may receive a less addition of velocity when it is already moving in the direction in which this gravity impels it; for if we ourselves push a body forwards, we can produce little additional effect upon it when it is already moving rapidly away from us. May it not be true, in like manner, that although gravity be always the same force, its effect depends upon the velocity which the body under its influence already possesses?

Observation and reasoning combined, as we have said, enabled Galileo to answer these questions. He asserted and proved that we may consistently and properly measure a force by the velocity which is by it generated in a body, in some certain time, as one second; and further, that if we adopt this measure, gravity will be a force of the same value under all circumstances of the body which it affects; since it appeared that, in fact, a falling body does receive equal increments of velocity in equal times from first to last.

If it be asked whether we could have known, anterior to, or independent of, experiment, that gravity is a uniform force in the sense thus imposed upon the term; it appears clear that we must reply, that we could not have attained to such knowledge, since other laws of the motion of bodies downwards are easily conceivable, and nothing but observation could inform us that one of these laws does not prevail in fact. Indeed, we may add, that the assertion that the force of gravity is uniform, is so far from being self-evident, that it is not even true; for gravity varies according to the distance from the center of the earth; and although this variation is so small as to be, in the case of falling bodies, imperceptible, it negatives the rigorous uniformity of the force as completely, though not to the same extent, as if the weight of a body diminished in a marked degree, when it was carried from the lower to the upper room of a house. It cannot, then, be a truth independent of experience, that gravity is uniform.

Yet, in fact, the assertion that gravity is uniform was assented to, not only before it was proved, but even before it was clearly understood. It was readily granted by all, that bodies which fall freely are *uniformly* accelerated; but while some held the opinion just stated, that uniformly accelerated motion is that in which the velocity increases in proportion to the *time*, others maintained, that *that* is uniformly accelerated motion, in which the velocity increases in proportion to the *space;* so that, for example, a body in falling vertically through twenty feet should acquire twice as great a velocity as one which falls through ten feet.

These two opinions are both put forward by the interlocutors of Galileo's Dialogue on this subject*. And the latter supposition is rejected, the author showing,

* *Dialogo*, III. p. 95.

not that it is inconsistent with experience, but that it is impossible in itself: inasmuch as it would inevitably lead to the conclusion, that the fall through a large and a small vertical space would occupy exactly the same time.

Indeed, Galileo assumes his definition of uniformly accelerated motion as one which is sufficiently recommended by its own simplicity. "If we attend carefully," he says, "we shall find that no mode of increase of velocity is more simple than that which adds equal increments in equal times. Which we may easily understand if we consider the close affinity of time and motion: for as the uniformity of motion is defined by the equality of spaces described in equal times, so we may conceive the uniformity of acceleration to exist when equal velocities are added in equal times."

Galileo's mode of supporting his opinion, that bodies falling by the action of gravity are thus uniformly accelerated, consists, in the first place, in adducing the maxim that nature always employs the most simple means*. But he is far from considering this a decisive argument. "I," says one of his speakers, "as it would be very unreasonable in me to gainsay this or any other definition which any author may please to make, since they are all arbitrary, may still, without offence, doubt whether such a definition, conceived and admitted in the abstract, fits, agrees, and is verified in that kind of accelerated motion which bodies have when they descend naturally."

The experimental proof that bodies, when they fall downwards, are uniformly accelerated, is (by Galileo) derived from the inclined plane; and therefore assumes the proposition, that if such uniform acceleration prevail in vertical motion, it will also hold when a body is compelled to describe an oblique rectilinear path. This pro-

* *Dialogo*, III. p. 91.

position may be shown to be true, if (assuming by anticipation the Third Law of Motion, of which we shall shortly have to speak,) we introduce the conception of a uniform statical force as the cause of uniform acceleration. For the force on the inclined plane bears a constant proportion to the vertical force, and this proportion is known from statical considerations. But in the work of which we are speaking, Galileo does not introduce this abstract conception of force as the foundation of his doctrines. Instead of this, he proposes, as a postulate sufficiently evident to be made the basis of his reasonings, That bodies which descend down inclined planes of different inclinations, but of the same vertical height, all acquire the same velocity*. But when this postulate has been propounded by one of the persons of the dialogue, another interlocutor says, "You discourse very probably; but besides this likelihood, I wish to augment the probability so far, that it shall be almost as complete as a necessary demonstration." He then proceeds to describe a very ingenious and simple experiment, which shows that when a body is made to swing upwards at the end of a string, it attains to the same height, whatever is the path it follows, so long as it starts from the lowest point with the same velocity. And thus Galileo's postulate is experimentally confirmed, so far as the force of gravity can be taken as an example of the forces which the postulate contemplates: and conversely, gravity is proved to be a uniform force, so far as it can be considered clear that the postulate is true of uniform forces.

When we have introduced the conception and definition of accelerating force, Galileo's postulate, that bodies descending down inclined planes of the same vertical height, acquire the same velocity, may, by a

* *Dialogo*, III. p. 36.

few steps of reasoning, be demonstrated to be true of uniform forces: and thus the proof that gravity, either in vertical or oblique motion, is a uniform force, is confirmed by the experiment above mentioned; as it also is, on like grounds, by many other experiments, made upon inclined planes and pendulums.

Thus the propriety of Galileo's conception of a uniform force, and the doctrine that gravity is a uniform force, were confirmed by the same reasonings and experiments. We may make here two remarks; *First*, that the conception, when established and rightly stated, appears so simple as hardly to require experimental proof; a remark which we have already made with regard to the First Law of Motion: and *Second*, that the discovery of the real law of nature was made by assuming propositions which, without further proof, we should consider as very precarious, and as far less obvious, as well as less evident, than the law of nature in its simple form.

4. *The Second Law of Motion.*—When a body, instead of falling downwards from rest, is thrown in any direction, it describes a curve line, till its motion is stopped. In this, and in all other cases in which a body describes a curved path in free space, its motion is determined by the Second Law of Motion. The law, in its general form, is as follows:—When a body is thus cast forth and acted upon by a force in a direction transverse to its motion, the result is, That *there is combined with the motion with which the body is thrown, another motion, exactly the same as that which the same force would have communicated to a body at rest.*

It will readily be understood that the basis of this law is the axiom already stated, that effects are measured by their causes. In virtue of this axiom, the effect of gravity acting upon a body in a direction transverse to its motion, must measure the accelerative or deflective force of gravity under those circumstances. If this effect vary with the varying velocity and direction of the body thus acted upon, the deflective force of gravity also will vary with those circumstances. The more simple supposition is, that the deflective force of gravity is the same, whatever be the velocity and direction of the body which is subjected to its influence: and this is the supposition which we find to be verified by facts. For example, a ball let fall from the top of a ship's upright mast, when she is sailing steadily forward, will fall at the foot of the mast, just as if it were let fall while the ship were at rest; thus showing that the motion which gravity gives to the ball is compounded with the horizontal motion which the ball shares with the ship from the first. This general and simple conception of motions as *compounded* with one another, represents, it is proved, the manner in which the motion produced by gravity modifies any other motion which the body may previously have had.

The discussions which terminated in the general reception of this Second Law of Motion among mechanical writers, were much mixed up with the arguments for and against the Copernican system, which system represented the earth as revolving upon its axis. For the obvious argument against this system was, that if each point of the earth's surface were thus in motion from west to east, a stone dropt from the top of a tower would be left behind, the tower moving away from it: and the answer was, that by this law of motion, the stone would have the earth's motion impressed upon it, as well as that motion which would arise from its gravity to the earth; and that the motion of the stone relative to the tower would thus be the same as if both earth and tower were at rest. Galileo further urged, as a presumption in favour of the opinion that the two motions,—the circular motion arising from the rotation of the earth, and the downward motion arising from the gravity of the stone, would be compounded in the way we have described, (neither of them disturbing or diminishing the other,)—that the first motion was in its own nature not liable to any change or diminution*, as we learn from the First Law of Motion. Nor was the subject lightly dismissed. The experiment of the stone let fall from the top of the mast was made in various forms by Gassendi; and in his Epistle, *De Motu impresso a Motore translato*, the rule now in question is supported by reference to these experiments. In this manner, the general truth, the Second Law of Motion, was established completely and beyond dispute.

But when this law had been proved to be true in a general sense, with such accuracy as rude experiments, like those of Galileo and Gassendi, would admit, it still remained to be ascertained (supposing our knowledge of the law to be the result of experience alone,) whether it were true with that precise and rigorous exactness which more refined modes of experimenting could test. We so willingly believe in the simplicity of laws of nature, that the rigorous accuracy of such a law, known to be at least approximately true, was taken for granted, till some ground for suspecting the contrary should appear. Yet calculations have not been wanting which might confirm the law as true to the last degree of accuracy. Laplace relates (*Syst. du Monde*, livre iv., chap. 16,) that at one time he had conceived it possible that the effect of gravity upon the moon might be slightly modified by the moon's direction and velocity; and that in this way an explanation might be found for the moon's *acceleration* (a deviation of her observed from her calculated place, which long perplexed mathematicians). But it was after some time discovered that this feature in the moon's motion arose from another cause; and the second law of

* *Dialogo*, II. p. 114.

motion was confirmed as true in the most rigorous sense.

Thus we see that although there were arguments which might be urged in favour of this law, founded upon the necessary relations of ideas, men became convinced of its truth only when it was verified and confirmed by actual experiment. But yet in this case again, as in the former ones, when the law had been established beyond doubt or question, men were very ready to believe that it was not a mere result of observation,—that the truth which it contained was not derived from experience,—that it might have been assumed as true in virtue of reasonings anterior to experience,—and that experiments served only to make the law more plain and intelligible, as visible diagrams in geometry serve to illustrate geometrical truths; our knowledge not being (they deemed) in mechanics, any more than in geometry, borrowed from the senses. It was thought by many to be self-evident, that the effect of a force in any direction cannot be increased or diminished by any motion transverse to the direction of the force which the body may have at the same time: or, to express it otherwise, that if the motion of the body be compounded of a horizontal and vertical motion, the vertical motion alone will be affected by the vertical force. This principle, indeed, not only has appeared evident to many persons, but even at the present day is assumed as an axiom by many of the most eminent mathematicians. It is, for example, so employed in the *Mécanique Céleste* of Laplace, which may be looked upon as the standard of mathematical mechanics in our time; and in the *Mécanique Analytique* of Lagrange, the most consummate example which has appeared of subtilty of thought on such subjects, as well as of power of mathematical generalization*. And

* I may observe that the rule that we may *compound* motions, as

thus we have here another example of that circumstance which we have already noticed in speaking of the First Law of Motion, (Art. 2 of this Chapter,) and of the Law that Gravity is a uniform Force, (Art. 3); namely, that the law, though historically established by experiments, appears, when once discovered and reduced to its most simple and general form, to be self-evident. I am the more desirous of drawing attention to this feature in various portions of the history of science, inasmuch as it will be found to lead to some very extensive and important views, hereafter to be considered.

5. *The Third Law of Motion.*—We have, in the definition of Accelerating Force, a measure of Forces, so far as they are concerned in producing motion. We had before, in speaking of the principles of statics, defined the measure of Forces or Pressures, so far as they are employed in producing equilibrium. But these two aspects of Force are closely connected; and we require a law which shall lay down the rule of their connexion. By the same kind of muscular exertion by which we

the Law supposes, is involved in the step of *resolving* them; which is done in the passage to which I refer (*Méc. Analyt.* Ptie. I., sect. i. art. 3, p. 225). "Si on conçoit que la mouvement d'un corps et les forces qui le sollicitent soient *decomposées* suivant trois lignes droites perpendiculaires entre elles, on pourra considérer séparément les mouvemens et les forces relatives à chacun a de ces trois directions. Car à cause de la perpendicularité des directions il est visible que chacun de ces mouvemens partiels peut être regardé comme indépendant des deux autres, et qu'il ne peut recevoir d'alteration que de la part de la force qui agit dans la direction de ce mouvement; l'on peut conclure que ces trois mouvements doivent suivre, chacun en particulier, les lois des mouvemens rectilignes accélérés ou retardés par les forces données." Laplace makes the same assumption in effect, (*Méc. Cél.* P. I., liv. i., art. 7,) by resolving the forces which act upon a point in three rectangular directions, and reasoning separately concerning each direction. But in his mode of treating the subject is involved a principle which belongs to the Third Law of Motion, namely, the doctrine that the velocity is as the force, of which we shall have to speak elsewhere.

can support a heavy stone, we can also put it in motion. The question then occurs, how is the rate and manner of its motion determined? The answer to this question is contained in the Third Law of Motion, and it is to this effect: that the *Momentum* which any pressure produces in the mass in a given time is proportional to the pressure. By *Momentum* is meant the product of the numbers which express the velocity and the mass of the body: and hence, if the mass of the body be the same in the instances which we compare, the rule is,—That *the velocity is as the force which produces it;* and this is one of the simplest ways of expressing the Third Law of Motion.

In agreement with our general plan, we have to ask, What is the ground of this rule? What is the simplest and most satisfactory form to which we can reduce the proof of it? Or, to take an instance; if a double pressure be exerted against a given mass, so disposed as to be capable of motion, why must it produce twice the velocity in the same time?

To answer this question, suppose the double pressure to be resolved into two single pressures: one of these will produce a certain velocity; and the question is, why an equal pressure, acting upon the same mass, will produce an equal velocity *in addition* to the former? Or, stating the matter otherwise, the question is, why each of the two forces will produce its separate effect, unaltered by the simultaneous action of the other force?

This statement of the case makes it seem to approach very near to such cases as are included in the Second Law of Motion, and therefore it might appear that this Third Law has no grounds distinct from the Second. But it must be recollected that the word *force* has a different meaning in this case and in that; in this place it signifies *pressure;* in the statement of the Second Law its import was *accelerative* or *deflective force*, measured by the velocity or deflexion generated. And thus the Third Law of Motion, so far as our reasonings yet go, appears to rest on a foundation different from the Second.

Accordingly, that part of the Third Law of Motion which we are now considering, that the velocity generated is as the force, was obtained, in fact, by a separate train of research. The first exemplification of this law which was studied by mathematicians, was the motion of bodies upon inclined planes: for the force which urges a body down an inclined plane is known by statics, and hence the velocity of its descent was to be determined. Galileo originally* in his attempts to solve this problem of the descent of a body down an inclined plane, did not proceed from the principle which we have stated, (the determination of the force which acts down the inclined plane from statical considerations,) obvious as it may seem; but assumed, as we have already seen, a proposition apparently far more precarious;—namely, that a body sliding down a smooth inclined plane acquires always the same velocity, so long as the *vertical* height fallen through is the same. And this conjecture, (for at first it was nothing more than a conjecture,) he confirmed by an ingenious experiment; in which bodies acquired or lost the same velocity by descending or ascending through the same height, although their paths were different in other respects.

This was the form in which the doctrine of the motion of bodies down inclined planes was at first presented in Galileo's *Dialogues* on the Science of Motion. But his disciple Viviani was dissatisfied with the assumption thus introduced; and in succeeding editions of the *Dialogues*, the apparent chasm in the reasoning was much narrowed, by making the proof depend upon a principle

* *Dial. della Sc. Nuov.* III., p. 96. See *Hist. Ind. Sci.* B. VI. c. ii. sect. 5.

nearly identical with the third law of motion as we have just stated it. In the proof thus added, "We are agreed," says the interlocutor*, "that in a moving body the impetus, energy, momentum, or propension to motion, is as great as is the force or least resistance which suffices to sustain it;" and the impetus or momentum, in the course of the proof, being taken to be as the velocity produced in a given time, it is manifest that the principle so stated amounts to this; that the velocity produced is as the statical force. And thus this law of motion appears, in the school of Galileo, to have been suggested and established at first by experiment, but afterwards confirmed and demonstrated by *à priori* considerations.

We see, in the above reasoning, a number of abstract terms introduced which are not, at first at least, very distinctly defined, as *impetus*, *momentum*, &c. Of these, *momentum* has been selected, to express that quantity which, in a moving body, measures the statical force impressed upon the body. This quantity is, as we have just seen, proportional to the velocity in a given body. It is also, in different bodies, proportional to the mass of the body. This part of the third law of motion follows from our conception of matter in general as consisting of parts capable of addition. A double pressure must be required to produce the same velocity in a double mass; for if the mass be halved, each half will require an equal pressure; and the addition, both of the pressures and of the masses, will take place without disturbing the effects.

The measure of the quantity of matter of a body considered as affecting the velocity which pressure produces in the body, is termed its *inertia*, as we have already stated. (p. 190.) Inertia is the property by which a

* *Dialogo*, p. 104.

large mass of matter requires a greater force than a small mass, to give it an equal velocity. It belongs to each portion of matter; and portions of inertia are added whenever portions of matter are added. Hence *inertia is as the quantity of matter;* which is only another way of expressing this third law of motion, so far as quantity of matter is concerned.

But how do we know the quantity of matter of a body? We may reply, that we take the weight as the measure of the quantity of matter: but we may then be again asked, how it appears that the weight is proportional to the inertia; which it must be, in order that the quantity of matter may be proportional to both one and the other. We answer, that this appears to be true experimentally, because all bodies fall with equal velocities by gravity, when the known causes of difference are removed. The observations of falling bodies, indeed, are not susceptible of much exactness: but experiments leading to the same result, and capable of great precision, were made upon pendulums by Newton; as he relates in his *Principia*, Book III., prop. 6. They all agreed, he says, with perfect accuracy: and thus the weight and the inertia are proportional in all cases, and therefore each proportional to the quantity of matter as measured by the other.

The conception of inertia, as we have already seen in chapter V., involves the notion of action and reaction; and thus the laws which involve inertia depend upon the idea of mutual causation. The rule, that the velocity is as the force, depends upon the principle of causation, that the effect is proportional to the cause; the effect being here so estimated as to be consistent both with the other laws of motion and with experiment.

But here, as in other cases, the question occurs again; Is experiment really requisite for the proof of this law? If we look to authorities, we shall be not a little embarrassed to decide. D'Alembert is against the necessity of experimental proof. "Why," says he*, "should we have recourse to this principle employed, at the present day, by everybody, that the force is proportional to the velocity? . . . a principle resting solely upon this vague and obscure axiom, that the effect is proportional to the cause. We shall not examine here," he adds, "if this principle is necessarily true; we shall only avow that the proofs which have hitherto been adduced do not appear to us unexceptionable: nor shall we, with some geometers, adopt it as a purely contingent truth; which would be to ruin the certainty of mechanics, and to reduce it to be nothing more than an experimental science. We shall content ourselves with observing," he proceeds, "that certain or doubtful, clear or obscure, it is useless in mechanics, and consequently ought to be banished from the science." Though D'Alembert rejects the third law of motion in this form, he accepts one of equivalent import, which appears to him to possess axiomatic certainty; and this procedure is in consistence with the course which he takes, of claiming for the science of mechanics more than mere experimental truth. On the contrary, Laplace considers this third law as established by experiment. "Is the force," he says†, "proportioned to the velocity? This," he replies, "we cannot know *à priori*, seeing that we are in ignorance of the nature of moving force: we must therefore, for this purpose, recur to experience; for all which is not a necessary consequence of the few data we have respecting the nature of things, is, for us, only a result of observation." And again he says‡, "Here, then, we have two laws of motion,—the law of inertia [the first law of motion], and the law of the force proportional to

* *Dynamique*, Pref. p. x. † *Méc. Cél.* p. 15. ‡ P. 18.

the velocity,—which are given by observation. They are the most natural and the most simple laws which we can imagine, and without doubt they flow from the very nature of matter; but this nature being unknown, they are, for us, only observed facts: the only ones, however, which mechanics borrows from experience."

It will appear, I think, from the views given in this and several other parts of the present work, that we cannot with justice say that we have very "few data respecting the nature of things," in speculating concerning the laws of the universe; since all the consequences which flow from the relations of our fundamental ideas, necessarily regulate our knowledge of things, so far as we have any such knowledge. Nor can we say that the nature of matter is unknown to us, in any sense in which we can conceive knowledge as possible. The nature ot matter is no more unknown than the nature of space or of number. In our conception of matter, as of space and of number, are involved certain relations, which are the necessary groundwork of our knowledge; and anything which is independent of these relations, is not unknown, but inconceivable.

It must be already clear to the reader, from the phraseology employed by these two eminent mathematicians, that the question respecting the formation of the third law of motion can only be solved by a careful consideration of what we mean by observation and experience, nature and matter. But it will probably be generally allowed, that, taking into account the explanations already offered of the necessary conditions of experience and of the conception of inertia, this law of motion, that the inertia is as the quantity of matter, is almost or altogether self-evident.

6. *Action and Reaction are Equal in Moving Bodies.* —When we have to consider bodies as acting upon one

another, and influencing each other's motions, the third law of motion is still applied; but along with this, we also employ the general principle that action and reaction are equal and opposite. Action and reaction are here to be understood as momentum produced and destroyed, according to the measure of action established by the Third Law of Motion: and the cases in which this principle is thus employed form so large a portion of those in which the third law of motion is used, that some writers (Newton at the head of them) have stated the equality of action and reaction as the third law of motion.

The third law of motion being once established, the equality of action and reaction, in the sense of momentum gained and lost, necessarily follows. Thus, if a weight hanging by a string over the edge of a smooth level table draw another weight along the table, the hanging weight moves more slowly than it would do if not so connected, and thus loses velocity by the connexion; while the other weight gains by the connexion all the velocity which it has, for if left to itself it would rest. And the pressures which restrain the descent of the first body and accelerate the motion of the second, are equal at all instants of time, for each of these pressures is the tension of the string: and hence, by the third law of motion, the momentum gained by the one body, and the momentum lost by the other in virtue of the action of this string, are equal. And similar reasoning may be employed in any other case where bodies are connected.

The case where one body does not push or draw, but *strikes* another, appeared at first to mechanical reasoners to be of a different nature from the others; but a little consideration was sufficient to show that a blow is, in fact, only a short and violent pressure; and that, therefore, the general rule of the equality of momentum lost and gained applies to this as well as to the other cases.

Thus, in order to determine the case of the direct action of bodies upon one another, we require no new law of motion. The equality of action and reaction, which enters necessarily into every conception of mechanical operation, combined with the measure of action as given by the third law of motion, enables us to trace the consequences of every case, whether of pressure or of impact.

7. *D'Alembert's Principle.*—But what will be the result when bodies do not act directly upon each other, but are *indirectly* connected in any way by levers, strings, pulleys, or in any other manner, so that one part of the system has a mechanical advantage over another? The result must still be determined by the principle that action and reaction balance each other. The action and reaction, being pressures in one sense, must balance each other by the laws of statics, for these laws determine the equilibrium of pressure. Now action and reaction, according to their measures in the Third Law of Motion, are momentum gained and lost, when the action is direct; and except the indirect action introduce some modification of the law, they must have the same measure still. But, in fact, we cannot well conceive any modification of the law to take place in this case; for direct action is only one (the ultimate) case of indirect action. Thus if two heavy bodies act at different points of a lever, the action of each on the other is indirect; but if the two points come together, the action becomes direct. Hence the rule must be that which we have already stated; for if the rule were false for indirect action, it would also be false for direct action, for which case we have shown it to be true. And thus we obtain the general principle, that in any system of bodies which act on each other, action and reaction, estimated by momentum gained and lost, balance each other according

to the laws of equilibrium. This principle, which is so general as to supply a key to the solution of all possible mechanical problems, is commonly called *D'Alembert's Principle.* The experimental proofs which convinced men of the truth of the Third Law of Motion were, many or most of them, proofs of the law in this extended sense. And thus the proof of D'Alembert's Principle, both from the idea of mechanical action and from experience, is included in the proof of the law already stated.

8. *Connexion of Dynamical and Statical Principles.*—The principle of equilibrium of D'Alembert just stated, is the law which he would substitute for the Third Law of Motion; and he would thus remove the necessity for an independent proof of that law. In like manner, the Second Law of Motion is by some writers derived from the principle of the composition of statical forces; and they would thus supersede the necessity of a reference to experiment in that case. Laplace takes this course, and thus, as we have seen, rests only the First and Third Law of Motion upon experience. Newton, on the other hand, recognizes the same connexion of propositions, but for a different purpose; for he derives the composition of statical forces from the Second Law of Motion.

The close connexion of these three principles, the composition of (statical) forces, the composition of (accelerating) forces with velocities, and the measure of (moving) forces by velocities, cannot be denied; yet it appears to be by no means easy to supersede the necessity of independent proofs of the two last of these principles. Both may be proved or illustrated by experiment: and the experiments which prove the one are different from those which establish the other. For example, it appears by easy calculations, that when we apply our principles to the oscillations of a pendulum, the Second Law is proved by the fact, that the oscillations take place at the same rate in an east and west, and in a north and south direction: under the same circumstances, the Third Law is proved by our finding that the time of a small oscillation is proportional to the square root of the length of a pendulum; and similar differences might be pointed out in other experiments, as to their bearing upon the one law or the other.

9. *Mechanical Principles become gradually more simple and more evident.*—I will again point out in general two circumstances which I have already noticed in particular cases of the laws of motion.—Truths are often at first assumed in a form which is far from being the most obvious or simple;—and truths once discovered are gradually simplified, so as to assume the appearance of self-evident truths.

The former circumstance is exemplified in several of the instances which we have had to consider. The assumption that a perpetual motion is impossible preceded the knowledge of the first law of motion. The assumed equality of the velocities acquired down two inclined planes of the same height, was afterwards reduced to the third law of motion by Galileo himself. In the History*, we have noted Huyghens's assumption of the equality of the actual descent and potential ascent of the center of gravity: this was afterwards reduced by Herman and the Bernoullis, to the statical equivalence of the solicitations of gravity and the vicarious solicitations of the effective forces which act on each point; and finally to the principle of D'Alembert, which asserts that the motions gained and lost balance each other.

This assertion of principles which now appear neither obvious nor self-evident, is not to be considered as a groundless assumption on the part of the discoverers by

* B. VI. c. V. sect. 2.

whom it was made. On the contrary, it is evidence of the deep sagacity and clear thought which were requisite in order to make such discoveries. For these results are really rigorous consequences of the laws of motion in their simplest form: and the evidence of them was probably present, though undeveloped, in the minds of the discoverers. We are told of geometrical students, who, by a peculiar aptitude of mind, perceived the evidence of some of the more advanced propositions of geometry without going through the introductory steps. We must suppose a similar aptitude for mechanical reasonings, which, existing in the minds of Stevinus, Galileo, Newton, and Huyghens, led them to make those assumptions which finally resolved themselves into the laws of motion.

We may observe further, that the simplicity and evidence which the laws of mechanics have at length assumed, are much favoured by the usage of words among the best writers on such subjects. Terms which originally, and before the laws of motion were fully known, were used in a very vague and fluctuating sense, were afterwards limited and rendered precise, so that assertions which at first appear identical propositions become distinct and important principles. Thus *force*, *motion*, *momentum*, are terms which were employed, though in a loose manner, from the very outset of mechanical speculation. And so long as these words retained the vagueness of common language, it would have been a useless and barren truism to say that "the momentum is proportional to the force," or that "a body loses as much motion as it communicates to another." But when "momentum" and "quantity of motion" are defined to mean the product of mass and velocity, these two propositions immediately become distinct statements of the third law of motion and its consequences. In like manner, the assertion that "gravity is a uniform force" was assented to, before it was settled what a uniform force was; but this assertion only became significant and useful when that point had been properly determined. The statement that "when different motions are communicated to the same body their effects are compounded," becomes the second law of motion, when we define what composition of motions is. And the same process may be observed in other cases.

And thus we see how well the form which science ultimately assumes is adapted to simplify knowledge. The definitions which are adopted, and the terms which become current in precise senses, produce a complete harmony between the matter and the form of our knowledge; so that truths which were at first unexpected and recondite, became familiar phrases, and after a few generations sound, even to common ears, like identical propositions.

10. *Controversy of the Measure of Force.*—In the History of Mechanics*, we have given an account of the controversy which, for some time, occupied the mathematicians of Europe, whether the forces of bodies in motion should be reckoned proportional to the velocity, or to the square of the velocity. We need not here recall the events of this dispute; but we may remark, that its history, as a metaphysical controversy, is remarkable in this respect, that it has been finally and completely settled; for it is now agreed among mathematicians that both sides were right, and that the results of mechanical action may be expressed with equal correctness by means of *momentum* and of *vis viva*. It is, in one sense, as D'Alembert has said†, a dispute about words; but we are not

* B. VI. c. V. sect. 2.

† D'Alembert has also remarked (*Dynamique*, Pref. xxi.,) that this controversy "shows how little justice and precision there is in the pretended axiom that causes are proportional to their effects." But

to infer that, on that account, it was frivolous or useless; for such disputes are one principal means of reducing the principles of our knowledge to their utmost simplicity and clearness. The terms which are employed in the science of mechanics are now liberated for ever, in the minds of mathematicians, from that ambiguity which was the battle-ground in the war of the *vis viva*.

But we may observe that the real reason of this controversy was exactly that tendency which we have been noticing;—the disposition of man to assume in his speculations certain general propositions as true, and to fix the sense of terms so that they shall fall in with this truth. It was agreed, on all hands, that in the mutual action of bodies the same quantity of force is always preserved; and the question was, by which of the two measures this rule could best be verified. We see, therefore, that the dispute was not concerning a definition merely, but concerning a definition combined with a general proposition. Such a question may be readily conceived to have been by no means unimportant; and we may remark, in passing, that such controversies, although they are commonly afterwards stigmatized as quarrels about words and definitions, are, in reality, events of considerable consequence in the history of science; since they dissipate all ambiguity and vagueness in the use of terms, and bring into view the conditions under which the fundamental principles of our knowledge can be most clearly and simply presented.

It is worth our while to pause for a moment on the prospect that we have thus obtained, of the advance of

this reflection is by no means well founded. For since both measures are true, it appears that causes may be *justly* measured by their effects, even when very different kinds of effects are taken. That the axiom does not point out one *precise* measure, till illustrated by experience or by other considerations, we grant: but the same thing occurs in the application of other axioms also.

knowledge, as exemplified in the history of Mechanics. The general transformation of our views from vague to definite, from complex to simple, from unexpected discoveries to self-evident truths, from seeming contradictions to identical propositions, is very remarkable, but it is by no means peculiar to our subject. The same circumstances, more or less prominent, more or less developed, appear in the history of other sciences, according to the point of advance which each has reached. They bear upon very important doctrines respecting the prospects, the limits, and the very nature of our knowledge. And though these doctrines require to be considered with reference to the whole body of science, yet the peculiar manner in which they are illustrated by the survey of the history of Mechanics, on which we have just been engaged, appears to make this a convenient place for introducing them to the reader.

Chapter VIII.

OF THE PARADOX OF UNIVERSAL PROPOSITIONS OBTAINED FROM EXPERIENCE.

1. It was formerly stated* that experience cannot establish any universal or necessary truths. The number of trials which we can make of any proposition is necessarily limited, and observation alone cannot give us any ground of extending the inference to untried cases. Observed facts have no visible bond of necessary connexion, and no exercise of our senses can enable us to discover such connexion. We can never acquire from a mere observation of facts, the right to assert that a proposition is true in all cases, and that it could not be otherwise than we find it to be.

* B. I., c. V. Of Experience.

Yet, as we have just seen in the history of the laws of motion, we may go on collecting our knowledge from observation, and enlarging and simplifying it, till it approaches or attains to complete universality and seeming necessity. Whether the laws of motion, as we now know them, can be rigorously traced to an absolute necessity in the nature of things, we have not ventured absolutely to pronounce. But we have seen that some of the most acute and profound mathematicians have believed that, for these laws of motion, or some of them, there was such a demonstrable necessity compelling them to be such as they are, and no other. Most of those who have carefully studied the principles of Mechanics will allow that some at least of the primary laws of motion approach very near to this character of necessary truth; and will confess that it would be difficult to imagine any other consistent scheme of fundamental principles. And almost all mathematicians will allow to these laws an absolute universality; so that we may apply them without scruple or misgiving, in cases the most remote from those to which our experience has extended. What astronomer would fear to refer to the known laws of motion, in reasoning concerning the double stars; although these objects are at an immeasurably remote distance from that solar system which has been the only field of our observation of mechanical facts? What philosopher, in speculating respecting a magnetic fluid, or a luminiferous ether, would hesitate to apply to it the mechanical principles which are applicable to fluids of known mechanical properties? When we assert that the quantity of motion in the world cannot be increased or diminished by the mutual actions of bodies, does not every mathematician feel convinced that it would be an unphilosophical restriction to limit this proposition to such modes of action as we have tried?

Yet no one can doubt that, in historical fact, these laws were collected from experience. That such is the case, is no matter of conjecture. We know the time, the persons, the circumstances, belonging to each step of each discovery. I have, in the History, given an account of these discoveries; and in the previous chapters of the present work, I have further examined the nature and the import of the principles which were thus brought to light.

Here, then, is an apparent contradiction. Experience, it would seem, has done that which we had proved that she cannot do. She has led men to propositions, universal at least, and to principles which appear to some persons necessary. What is the explanation of this contradiction, the solution of this paradox? Is it true that Experience can reveal to us universal and necessary truths? Does she possess some secret virtue, some unsuspected power, by which she can detect connexions and consequences which we have declared to be out of her sphere? Can she see more than mere appearances, and observe more than mere facts? Can she penetrate, in some way, to the nature of things?—descend below the surface of phenomena to their causes and origins, so as to be able to say what can and what can not be;—what occurrences are partial, and what universal? If this be so, we have indeed mistaken her character and powers; and the whole course of our reasoning becomes precarious and obscure. But, then, when we return upon our path we cannot find the point at which we deviated, we cannot detect the false step in our deduction. It still seems that by experience, strictly so called, we cannot discover necessary and universal truths. Our senses can give us no evidence of a necessary connexion in phenomena. Our observation must be limited, and cannot testify concerning anything which is beyond its limits. A general view of our faculties appears to prove it to be impossible that men should do what the history of the science of mechanics shows that they have done.

2. But in order to try to solve this Paradox, let us again refer to the History of Mechanics. In the cases belonging to that science, in which propositions of the most unquestionable universality, and most approaching to the character of necessary truths, (as, for instance, the laws of motion,) have been arrived at, what is the source of the axiomatic character which the propositions thus assume? The answer to this question will, we may hope, throw some light on the perplexity in which we appear to be involved.

Now the answer to this inquiry is, that the laws of motion borrow their axiomatic character from their being merely *interpretations* of the Axioms of Causation. Those axioms, being exhibitions of the Idea of Cause under various aspects, are of the most rigorous universality and necessity. And so far as the laws of motion are exemplifications of those axioms, these laws must be no less universal and necessary. How these axioms are to be understood;—in what sense *cause* and *effect*, *action* and *reaction*, are to be taken, experience and observation did, in fact, teach inquirers on this subject; and without this teaching, the laws of motion could never have been distinctly known. If two forces act together, each must produce its effect, by the axiom of causation; and, therefore, the effects of the separate forces must be *compounded*. But a long course of discussion and experiment must instruct men of what kind this *composition* of forces is. Again; action and reaction must be equal; but much thought and some trial were needed to show what *action* and *reaction* are. Those metaphysicians who enunciated Laws of motion without reference to experience, propounded only such laws as were vague and inapplicable. But yet these persons manifested the indestructible conviction, belonging to man's speculative nature, that there exist Laws of motion, that is, universal formulæ, connecting the causes and effects when motion takes place. Those mechanicians, again, who, observed facts involving equilibrium and motion, and stated some narrow rules, without attempting to ascend to any universal and simple principle, obtained laws no less barren and useless than the metaphysicians; for they could not tell in what new cases, or whether in any, their laws would be verified;—they needed a more general rule, to show them the limits of the rule they had discovered. They went wrong in each attempt to solve a new problem, because their interpretation of the terms of the axioms, though true, perhaps, in certain cases, was not right in general.

Thus Pappus erred in attempting to interpret as a case of the lever, the problem of supporting a weight upon an inclined plane; thus Aristotle erred in interpreting the doctrine that the weight of bodies is the cause of their fall; thus Kepler erred in interpreting the rule that the velocity of bodies depends upon the force; thus Bernoulli* erred in interpreting the equality of action and reaction upon a lever in motion. In each of these instances, true doctrines, already established, (whether by experiment or otherwise,) were erroneously applied. And the error was corrected by further reflection, which pointed out that another mode of interpretation was requisite, in order that the axiom which was appealed to in each case might retain its force in the most general sense. And in the reasonings which avoided or corrected such errors, and which led to substantial general truths, the object of the speculator always was to give to the acknowledged maxims which the Idea of Cause suggested, such a signification as should be con-

* *Hist. Ind. Sci.*, B. VI. c. v. sect. 2.

sistent with their universal validity. The rule was not accepted as particular at the outset, and afterwards generalized more and more widely; but from the very first, the universality of the rule was assumed, and the question was, how it should be understood so as to be universally true. At every stage of speculation, the law was regarded as a general law. This was not an aspect which it gradually acquired, by the accumulating contributions of experience, but a feature of its original and native character. *What* should happen universally, experience might be needed to show: but that what happened should happen *universally*, was implied in the nature of knowledge. The universality of the laws of motion was not gathered from experience, however much the laws themselves might be so.

3. Thus we obtain the solution of our Paradox, so far as the case before us is concerned. The laws of motion borrow their *form* from the Idea of Causation, though their *matter* may be given by experience: and hence they possess a universality which experience cannot give. They are certainly and universally valid; and the only question for observation to decide is, how they are to be understood. They are like general mathematical formulæ, which are known to be true, even while we are ignorant what are the unknown quantities which they involve. It must be allowed, on the other hand, that so long as these formulæ are not interpreted by a real study of nature, they are not only useless but prejudicial; filling men's minds with vague general terms, empty maxims, and unintelligible abstractions, which they mistake for knowledge. Of such perversion of the speculative propensities of man's nature, the world has seen too much in all ages. Yet we must not, on that account, despise these forms of truth, since without them, no general knowledge is possible. Without general terms,

and maxims, and abstractions, we can have no science, no speculation; hardly, indeed, consistent thought or the exercise of reason. The course of real knowledge is, to obtain from thought and experience the right interpretation of our general terms, the real import of our maxims, the true generalizations which our abstractions involve.

4. If it be asked, How Experience is able to teach us to interpret aright the general terms which the Axioms of Causation involve;—whence she derives the light which she is to throw on these general notions; the answer is obvious;—namely, that the relations of causation are the *conditions* of Experience;—that the general notions are *exemplified* in the particular cases of which she takes cognizance. The events which take place about us, and which are the objects of our observation, we cannot conceive otherwise than as subject to the laws of cause and effect. Every event must have a cause;—Every effect must be determined by its cause;—these maxims are true of the phenomena which form the materials of our experience. It is precisely to them, that these truths apply. It is in the world which we have before our eyes, that these propositions are universally verified; and it is therefore by the observation of what we see, that we must learn how these propositions are to be understood. Every fact, every experiment, is an example of these statements; and it is therefore by attention to and familiarity with facts and experiments, that we learn the signification of the expressions in which the statements are made; just as in any other case we learn the import of language by observing the manner in which it is applied in known cases. Experience is the interpreter of nature; it being understood that she is to make her interpretation in that comprehensive phraseology which is the genuine language of science.

5. We may return for an instant to the objection, that experience cannot give us general truths, since, after any number of trials confirming a rule, we may, for aught we can foresee, have one which violates the rule. When we have seen a thousand stones fall to the ground, we may see one which does not fall under the same apparent circumstances. How then, it is asked, can experience teach us that *all* stones, rigorously speaking, will fall if unsupported? And to this we reply, that it is not true that we can conceive one stone to be suspended in the air, while a thousand others fall, without believing some peculiar cause to support it; and that, therefore, such a supposition forms no exception to the law, that gravity is a force by which *all* bodies are urged downwards. Undoubtedly we can conceive a body, when dropt or thrown, to move in a line quite different from other bodies: thus a certain missile* used by the natives of Australia, and lately brought to this country, when thrown from the hand in a proper manner, describes a curve, and returns to the place from whence it was thrown. But did any one, therefore, even for an instant suppose that the laws of motion are different for this and for other bodies? On the contrary, was not every person of a speculative turn immediately led to inquire how it was that the known causes which modify motion, the resistance of the air and the other causes, produced in this instance so peculiar an effect? And if the motion had been still more unaccountable, it would not have occasioned any uncertainty whether it were consistent with the agency of gravity and the laws of motion. If a body suddenly alter its direction, or move in any other unexpected manner, we never doubt that there is a cause of the change. We may continue quite ignorant of the nature of this cause, but this ignorance

* Called the Bo-me-rang.

never occasions a moment's doubt that the cause exists and is exactly suited to the effect. And thus experience can prove or discover to us general rules, but she can never prove that general rules do not exist. Anomalies, exceptions, unexplained phenomena, may remind us that we have much still to learn, but they can never make us suppose that truths are not universal. We may observe facts that show us we have not fully understood the meaning of our general laws, but we can never find facts which show our laws to have no meaning. Our experience is bound in by the limits of cause and effect, and can give us no information concerning any region where that relation does not prevail. The whole series of external occurrences and objects, through all time and space, exists only, and is conceived only, as subject to this relation; and therefore we endeavour in vain to imagine to ourselves when and where and how exceptions to this relation may occur. The assumption of the connexion of cause and effect is essential to our experience, as the recognition of the maxims which express this connexion is essential to our knowledge.

6. I have thus endeavoured to explain in some measure how, at least in the field of our mechanical knowledge, experience can discover universal truths, though she cannot give them their universality; and how such truths, though borrowing their form from our ideas, cannot be understood except by the actual study of external nature. And thus with regard to the laws of motion, and other fundamental principles of Mechanics, the analysis of our ideas and the history of the progress of the science well illustrate each other.

If the paradox of the discovery of universal truths by experience be thus solved in one instance, a much wider question offers itself to us;—How far the difficulty, and how far the solution, are applicable to other subjects. It is easy to see that this question involves most grave and extensive doctrines with regard to the whole compass of human knowledge: and the views to which we have been led in the present Book of this work are, we trust, fitted to throw much light upon the general aspect of the subject. But after discussions so abstract, and perhaps obscure, as those in which we have been engaged for some chapters, I willingly postpone to a future occasion an investigation which may perhaps appear to most readers more recondite and difficult still. And we have, in fact, many other special fields of knowledge to survey, before we are led by the order of our subject, to those general questions and doctrines, those antitheses brought into view and again resolved, which a view of the whole territory of human knowledge suggests, and by which the nature and conditions of knowledge are exhibited.

Before we quit the subject of mechanical science we shall make a few remarks on another doctrine which forms part of the established truths of the science, namely, the doctrine of universal gravitation.

Chapter IX.

OF THE ESTABLISHMENT OF THE LAW OF UNIVERSAL GRAVITATION.

The doctrine of universal gravitation is a feature of so much importance in the history of science that we shall not pass it by without a few remarks on the nature and evidence of the doctrine.

1. To a certain extent the doctrine of the attraction of bodies according to the law of the inverse square of the distance, exhibits in its progress among men the

same general features which we have noticed in the history of the laws of motion. This doctrine was maintained *à priori* on the ground of its simplicity, and asserted positively, even before it was clearly understood:—notwithstanding this anticipation, its establishment on the ground of facts was a task of vast labour and sagacity:—when it had been so established in a general way, there occurred at later periods, an occasional suspicion that it might be approximately true only:—these suspicions led to further researches, which showed the rule to be rigorously exact:—and at present there are mathematicians who maintain, not only that it is true, but that it is a necessary property of matter. A very few words on each of these points will suffice.

2. I have shown in the *History of Science**, that the attraction of the sun according to the inverse square of the distance, had been divined by Bullialdus, Hooke, Halley, and others, before it was proved by Newton. Probably the reason which suggested this conjecture was, that gravity might be considered as a sort of emanation; and that thus, like light or any other effect diffused from a center, it must follow the law just stated, the efficacy of the force being weakened in receding from the center, exactly in proportion to the space through which it is diffused. It cannot be denied that such a view appears to be strongly recommended by analogy.

When it had been proved by Newton that the planets were really retained in their elliptical orbits by a central force, his calculations also showed that the above-stated *law* of the force must be at least very approximately correct, since otherwise the aphelia of the orbits could not be so nearly at rest as they were. Yet when it seemed as if the motion of the moon's apogee could not be accounted for without some new supposition, the *à*

* B. VII. c. i.

priori argument in favour of the inverse square did not prevent Clairaut from trying the hypothesis of a small term added to that which expressed the ancient law: but when, in order to test the accuracy of this hypothesis, the calculation of the motion of the moon's apogee was pushed to a greater degree of exactness than had been obtained before, it was found that the new term vanished of itself; and that the inverse square now accounted for the whole of the motion. And thus, as in the case of the second law of motion, the most scrupulous examination terminated in showing the simplest rule to be rigorously true.

3. Similar events occurred in the history of another part of the law of gravitation: namely, that the attraction is proportional to the quantity of matter attracted. This part of the law may also be thus stated, That the weight of bodies arising from gravity is proportional to their inertia; and thus, that the accelerating force on all bodies under the same circumstances is the same. Newton made experiments which proved this with regard to terrestrial bodies; for he found that, at the end of equal strings, balls of all substances, gold, silver, lead, glass, wood, &c., oscillated in equal times*. But a few years ago, doubts arose among the German astronomers whether this law was rigorously true with regard to the planetary bodies. Some calculations appeared to prove, that the attraction of Jupiter as shown by the perturbations which he produces in the small planets Juno, Vesta, and Pallas, was different from the attraction which he exerts on his own satellites. Nor did there appear to these philosophers anything inconceivable in the supposition that the attraction of a planet might be thus *elective*. But when Mr. Airy obtained a more exact determination of the mass of Jupiter, as

* *Prin.* Lib. III., Prop. 6.

indicated by his effect on his satellites, it was found that this suspicion was unfounded; and that there was, in this case, no exception to the universality of the rule, that this cosmical attraction is in the proportion of the attracted mass.

4. Again: when it had thus been shown that a mutual attraction of parts, according to the law above mentioned, prevailed throughout the extent of the solar system, it might still be doubted whether the same law extended to other regions of the universe. It might have been perhaps imagined that each fixed star had its peculiar law of force. But the examination of the motions of double stars about each other, by the two Herschels and others, appears to show that these bodies describe ellipses as the planets do; and thus extends the law of the inverse squares to parts of the universe immeasurably distant from the whole solar system.

5. Since every doubt which has been raised with regard to the universality and accuracy of the law of gravitation, has thus ended in confirming the rule, it is not surprizing that men's minds should have returned with additional force to those views which had at first represented the law as a necessary truth, capable of being established by reason alone. When it had been proved by Newton that gravity is really a *universal* attribute of matter as far as we can learn, his pupils were not content without maintaining it to be an *essential* quality. This is the doctrine held by Cotes in the preface to the second edition of the *Principia* (1712): "Gravity," he says, "is a primary quality of bodies, as extension, mobility, and impenetrability are." But Newton himself by no means went so far. In his second Letter to Bentley (1693), he says: "You sometimes speak of gravity as essential and inherent to matter; pray do not ascribe that notion to me. The cause of gravity,"

he adds, "I do not pretend to know, and would take more time to consider of it."

Cotes maintains his opinion by urging, that we learn by *experience* that all bodies possess gravity, and that we do not learn in any other way that they are extended, moveable, or solid. But we have already seen, that the ideas of space, time, and reaction, on which depend extension, mobility, and solidity, are not results, but conditions, of experience. We cannot conceive a body except as extended; we cannot conceive it to exert mechanical action except with some kind of solidity. But so far as our conceptions of body have hitherto been developed, we find no difficulty in conceiving two bodies which do not attract each other.

6. Newton lays down, in the second edition of the *Principia*, this "Rule of Philosophizing" (Book III.); that "The qualities of bodies which cannot be made more or less intense, and which belong to all bodies on which we are able to make experiments, are to be held to be qualities of all bodies in general." And this Rule is cited in the sixth Proposition of the Third Book of the *Principia*, (Cor. 2,) in order to prove that gravity, proportional to the quantity of matter, may be asserted to be a quality of all bodies universally. But we may remark that a Rule of Philosophizing, itself of precarious authority, cannot authorize us in ascribing universality to an empirical result. Geometrical and statical properties are seen to be necessary, and *therefore* universal: but Newton appears disposed to assert a like universality of gravity, quite unconnected with any necessity. It would be a very inadequate statement, indeed a false representation, of statical truth, if we were to say, that because every body which has hitherto been tried *has been found* to have a center of gravity, we venture to assert that all bodies whatever have a center of gravity.

And if we are ever able to assert the absolute universality of the law of gravitation, we shall have to rest this truth upon the clearer developement of our ideas of matter and force; not upon a Rule of Philosophizing, which, till otherwise proved, must be a mere rule of prudence, and which the opponent may refuse to admit.

7. Other persons, instead of asserting gravity to be in its own nature essential to matter, have made hypotheses concerning some mechanism or other, by which this mutual attraction of bodies is produced*. Thus the Cartesians ascribed to a vortex the tendency of bodies to a center; Newton himself seems to have been disposed to refer this tendency to the elasticity of an ether; Le Sage propounded a curious hypothesis, in which this attraction is accounted for by the impulse of infinite streams of particles flowing constantly through the universe in all directions. In these speculations, the force of gravity is resolved into the pressure or impulse of solids or fluids. On the other hand, hypotheses have been propounded, in which the solidity, and other physical qualities of bodies, have been explained by representing the bodies as a collection of points, from which points, repulsive, as well as attractive, forces emanate. This view of the constitution of bodies was maintained and developed by Boscovich, and is hence termed "Boscovich's Theory:" and the discussion of it will more properly come under our review at a future period, when we speak of the question whether bodies are made up of atoms. But we may observe, that Newton himself appears to have inclined, as his followers certainly did, to this mode of contemplating the physical properties of bodies. In his Preface to the *Principia*, after speaking of the central forces which are exhibited

* See Vince, *Observations on the Hypothesis respecting Gravitation*, and the Critique of that work, *Edinb. Rev.* Vol. XIII.

in cosmical phenomena, he says: "Would that we could derive the other phenomena of Nature from mechanical principles by the same mode of reasoning. For many things move me, so that I suspect all these phenomena may depend upon certain forces, by which the particles of bodies, through causes not yet known, are either impelled to each other and cohere according to regular figures, or are repelled and recede from each other: which forces being unknown, philosophers have hitherto made their attempts upon nature in vain."

8. But both these hypotheses;—that by which cohesion and solidity are reduced to attractive and repulsive forces, and that by which attraction is reduced to the impulse and pressure of media;—are hitherto merely modes of representing mechanical laws of nature; and cannot, either of them, be asserted as possessing any evident truth or peremptory authority to the exclusion of the other. This consideration may enable us to estimate the real weight of the difficulty felt in assenting to the mutual attraction of bodies not in contact with each other; for it is often urged that this attraction of bodies at a distance is an absurd supposition.

The doctrine is often thus stigmatized, both by popular and by learned writers. It was long received as a maxim in philosophy (as Monboddo informs us*), that a body cannot act *where* it is not, any more than *when* it is not. But to this we reply, that time is a necessary condition of our conception of causation, in a different manner from space. The action of force can only be conceived as taking place in a succession of moments, in each of which cause and effect immediately succeed each other: and thus the interval of time between a cause and its remote effect is filled up by a continuous succession of events connected by the same chain of causation. But

* *Ancient Metaphysics*, Vol. II. p. 175.

in space, there is no such visible necessity of continuity; the action and reaction may take place at a distance from each other; all that is necessary being that they be equal and opposite.

Undoubtedly the existence of attraction is rendered more acceptable to common apprehension by supposing some intermediate machinery,—a cord, or rod, or fluid, —by which the forces may be conveyed from one point to another. But such images are rather fitted to satisfy those prejudices which arise from the earlier application of our ideas of force, than to exhibit the real nature of those ideas. If we suppose two bodies to pull each other by means of a rod or a cord, we only suppose, in addition to those equal and opposite forces acting upon the two bodies which forces are alone essential to mutual attraction, a certain power of resisting transverse pressure at every point of the intermediate line: which additional supposition is entirely useless, and quite unconnected with the essential conditions of the case. When the Newtonians were accused of introducing into philosophy an unknown cause which they termed *attraction*, they justly replied that they knew as much respecting attraction as their opponents did about impulse. In each case we have a knowledge of the conception in question so far as we clearly apprehend it under the conditions of those axioms of mechanical causation which form the basis of our science on such subjects.

Having thus examined the degree of certainty and generality to which our knowledge of the law of universal gravitation has been carried, by the progress of mechanical discovery and speculation up to the present time, we might proceed to the other branches of science, and examine in like manner their grounds and conditions. But before we do this, it will be worth our while to attend for a moment to the effect which the progress of mechanical ideas among mathematicians and mechanical philosophers has produced upon the minds of other persons, who share only in an indirect and derivative manner in the influence of science.

Chapter X.

OF THE GENERAL DIFFUSION OF CLEAR MECHANICAL IDEAS.

1. We have seen how the progress of knowledge upon the subject of motion and force has produced, in the course of the world's history, a great change in the minds of acute and speculative men; so that such persons can now reason with perfect steadiness and precision upon subjects on which, at first, their thoughts were vague and confused; and can apprehend, as truths of complete certainty and evidence, laws which it required great labour and time to discover. This *complete* developement and clear manifestation of mechanical ideas has taken place only among mathematicians and philosophers. But yet a progress of thought upon such subjects,—an advance from the obscure to the clear, and from errour to truth,—may be traced in the world at large, and among those who have not directly cultivated the exact sciences. This diffused and collateral influence of science manifests itself, although in a wavering and fluctuating manner, by various indications, at various periods of literary history. The opinions and reasonings which are put forth upon mechanical subjects, and above all, the adoption, into common language, of terms and phrases belonging to the prevalent mechanical systems, exhibit to us the most profound discoveries and speculations of philosophers in their effect upon more common

and familiar trains of thought. This effect is by no means unimportant, and we shall point out some examples of such indications as we have mentioned.

2. The discoveries of the ancients in speculative mechanics were, as we have seen, very scanty; and hardly extended their influence to the unmathematical world. Yet the familiar use of the term "center of of gravity" preserved and suggested the most important part of what the Greeks had to teach. The other phrases which they employed, as *momentum*, *energy*, *virtue*, *force*, and the like, never had any exact meaning, even among mathematicians; and therefore never, in the ancient world, became the means of suggesting just habits of thought. I have pointed out, in the History of Science, several circumstances which appear to denote the general confusion of ideas which prevailed upon mechanical subjects during the times of the Roman empire. I have there taken as one of the examples of this confusion, the fable narrated by Pliny and others concerning the echineïs, a small fish, which was said to stop a ship merely by sticking to it*. This story was adduced as betraying the absence of any steady apprehension of the equality of action and reaction; since the fish, except it had some immoveable obstacle to hold by, must be pulled forward by the ship, as much as it pulled the ship backward. If the writers who speak of this wonder had shown any perception of the necessity of a reaction, either produced by the rapid motion of the fish's fins in the water, or in any other way, they would not be chargeable with this confusion of thought; but from their expressions it is, I think, evident that they saw no such necessity†. Their idea of mechanical action

* *Hist. Ind. Sci.* B. IV. c. i. sect. 2.

† See Prof. Powell, *On the Nature and Evidence of the Laws of Motion. Reports of the Ashmolean Society.* Oxford. 1837. Professor

was not sufficiently distinct to enable them to see the absurdity of supposing an intense pressure with no obstacle for it to exert itself against.

3. We may trace, in more modern times also, indications of a general ignorance of mechanical truths. Thus the phrase of shooting at an object "point-blank," implies the belief that a cannon-ball describes a path of which the first portion is a straight line. This error was corrected by the true mechanical principles which Galileo and his followers brought to light; but these principles made their way to popular notice, principally in consequence of their application to the motions of the solar system, and to the controversies which took place respecting those motions. Thus by far the most powerful argument against the reception of the Copernican system of the universe, was that of those who asked, Why a stone dropt from a tower was not left behind by the motion of the earth? The answer to this question, now universally familiar, involves a reference to the true doctrine of the composition of motions. Again; Kepler's persevering and strenuous attempts* to frame a physical theory of the universe were frustrated by his ignorance of the first law of motion, which informs us that a body will retain its velocity without any maintaining force. He proceeded upon the supposition that the sun's force was requisite to *keep up* the motion of the planets,

Powell has made an objection to my use of this instance of confusion of thought; the remark in the text seems to me to justify what I said in the History. As an evidence that the fish was not supposed to produce its effect by its muscular power acting on the water, we may take what Pliny says, *Nat. Hist.*, xxxii. 1, "Domat mundi rabiem, nullo suo labore; non retinendo, aut alio modo quam adhærendo:" and also what he states in another place (ix. 41,) that when it is preserved in pickle, it may be used in recovering gold which has fallen into a deep well. All this implies adhesion alone, with no conception of reaction.

* *Hist. Ind. Sci.*, B. v. c. iv., and B. VII. c. i.

as well as to deflect and modify it; and he was thus led to a system which represented the sun as carrying round the planets in their orbits by means of a *vortex*, produced by his revolution. The same neglect of the laws of motion presided in the formation of Descartes' system of vortices. Although Descartes had enunciated in words the laws of motion, he and his followers showed that they had not the practical habit of referring to these mechanical principles; and dared not trust the planets to move in free space without some surrounding machinery to support them*.

4. When at last mathematicians, following Newton, had ventured to consider the motion of each planet as a mechanical problem not different in its nature from the motion of a stone cast from the hand; and when the solution of this problem and its immense consequences had become matters of general notoriety and interest; the new views introduced, as is usual, new terms, which soon became extensively current. We meet with such phrases as "flying off in the tangent," and "deflexion from the tangent;" with antitheses between "centripetal" and "centrifugal force," or between "projectile" and "central force." "Centers of force," "disturbing forces," "perturbations," and "perturbations of higher orders," are not unfrequently spoken of: and the expression "to gravitate," and the term "universal gravitation," acquired a permanent place in the language.

Yet for a long time, and even up to the present day, we find many indications that false and confused apprehensions on such subjects are by no means extirpated.

* I have, in the History, applied to Descartes the character which Bacon gives to Aristotle, "Audax simul et pavidus:" though he was bold enough to enunciate the laws of motion without knowing them aright, he had not the courage to leave the planets to describe their orbits by the agency of those laws, without the machinery of contact.

Arguments are urged against the mechanical system of the universe, implying in the opponents an absence of all clear mechanical notions. Many of this class of writers retrograde to Kepler's point of view. This is, for example, the case with Lord Monboddo, who, arguing on the assumption that force is requisite to maintain, as well as to deflect motion, produced a series of attacks upon the Newtonian philosophy; which he inserted in his *Ancient Metaphysics*, published in 1779 and the succeeding years. This writer (like Kepler), measures force by the velocity which the body *has**, not by that which its *gains*. Such a use of language would prevent our obtaining any laws of motion at all. Accordingly, the author, in the very next page to that which I have just quoted, abandons this measure of force, and, in curvilinear motion, measures force by "the fall from the extremity of the arc." Again; in his objections to the received theory, he denies that curvilinear motion is compounded, although his own mode of considering such motion assumes this composition in the only way in which it was ever intended by mathematicians. Many more instances might be adduced to show that a want of cultivation of the mechanical ideas rendered this philosopher incapable of judging of a mechanical system.

The following extract from the *Ancient Metaphysics*, may be sufficient to show the value of the author's criticism on the subjects of which we are now speaking. His object is to prove that there do not exist a centripetal and a centrifugal force in the case of elliptical motion. "Let any man move in a circular or elliptical line described to him; and he will find no tendency in himself either to the center or from it, much less both. If indeed he attempt to make the motion with great velocity, or if he do it carelessly and inattentively, he

* *Anc. Met.* Vol. II. B. v. c. vi., p. 413.

may go out of the line, either towards the center or from it: but this is to be ascribed, not to the nature of the motion, but to our infirmity; or perhaps to the animal form, which is more fitted for progressive motion in a right line than for any kind of curvilinear motion. But this is not the case with a sphere or spheroid, which is equally adapted to motion in all directions*." We need hardly remind the reader that the manner in which a man running round a small circle, finds it necessary to lean inwards, in order that there may be a centripetal inclination to counteract the centrifugal force, is a standard example of our mechanical doctrines; and this fact (quite familiar in practice as well as theory,) is in direct contradiction of Lord Monboddo's assertion.

5. A similar absence of distinct mechanical thought appears in some of the most celebrated metaphysicians of Germany. I have elsewhere noted† the opinion expressed by Hegel, that the glory which belongs to Kepler has been unjustly transferred to Newton; and I have suggested, as the explanation of this mode of thinking, that Hegel himself, in the knowledge of mechanical truth, had not advanced beyond Kepler's point of view. Persons who possess conceptions of space and number, but who have not learnt to deal with ideas of force and causation, may see more value in the discoveries of Kepler than in those of Newton. Another exemplification of this state of mind may be found in Mr. Schelling's speculations; for instance, in his *Lectures on the Method of Academical Study*. In the twelfth Lecture, on the Study of Physics and Chemistry, he says, (p. 266,) "What the mathematical natural philosophy has done for the knowledge of the laws of the universe since the time that they were discovered by his (Kepler's) godlike genius, is,

* *Anc. Met.*, Vol. I. B. ii. c. 19, p. 264.

† *Hist. Ind. Sci.*, B. VII. c. ii. sect. 5.

as is well known, this: it has attempted a construction of those laws which, according to its foundations, is altogether empirical. We may assume it as a general rule, that in any proposed construction, that which is not a pure general form cannot have any scientific import or truth. The foundation from which the centrifugal motion of the bodies of the world is derived, is no necessary form, it is an empirical fact. The Newtonian attractive force, even if it be a necessary assumption for a merely reflective view of the subject, is still of no significance for the Reason, which recognizes only absolute relations. The grounds of the Keplerian laws can be derived, without any empirical appendage, purely from the doctrine of Ideas, and of the two Unities, which are in themseves one Unity, and in virtue of which each being, while it is absolute in itself, is at the same time in the absolute, and reciprocally."

It will be observed, that in this passage our mechanical laws are objected to because they are not necessary results of our ideas; which, however, as we have seen, according to the opinion of some eminent mechanical philosophers, they are. But to assume this evident necessity as a condition of every advance in science, is to mistake the last, perhaps unattainable step, for the first, which lies before our feet. And, without inquiring further about "the Doctrine of the two Unities," or the manner in which from that doctrine we may deduce the Keplerian laws, we may be well convinced that such a doctrine cannot supply any sufficient reason to induce us to quit the inductive path by which all scientific truth up to the present time has been acquired.

6. But without going to schools of philosophy opposed to the Inductive School, we may find many loose and vague habits of thinking on mechanical subjects among the common classes of readers and reasoners. And

there are some familiar modes of employing the phraseology of mechanical science, which are, in a certain degree, chargeable with inaccuracy, and may produce or perpetuate confusion. Among such cases we may mention the way in which the centripetal and centrifugal forces, and also the projectile and central forces of the planets, are often compared or opposed. Such antitheses sometimes proceed upon the false notion that the two members of these pairs of forces are of the same kind: whereas on the contrary the *projectile* force is a hypothetical impulsive force which may, at some former period, have caused the motion to begin; while the *central* force is an actual force, which must act continuously and during the whole time of the motion, in order that the motion may go on in the curve. In the same manner the *centrifugal* force is not a distinct force in a strict sense, but only a certain result of the first law of motion, measured by the portion of *centripetal* force which counteracts it. Comparisons of quantities so heterogeneous imply confusion of thought, and often suggest baseless speculations and imagined reforms of the received opinions.

7. I might point out other terms and maxims, in addition to those already mentioned, which, though formerly employed in a loose and vague manner, are now accurately understood and employed by all just thinkers; and thus secure and diffuse a right understanding of mechanical truths. Such are *momentum*, *inertia*, *quantity of matter*, *quantity of motion*; that *force is proportional to its effects*; that *action and reaction are equal*; that *what is gained in force by machinery is lost in time*; that *the quantity of motion in the world cannot be either increased or diminished*. When the expression of the truth thus becomes easy and simple, clear and convincing, the meanings given to words and phrases by

discoverers glide into the habitual texture of men's reasonings, and the effect of the establishment of true mechanical principles is felt far from the school of the mechanician. If these terms and maxims are understood with tolerable clearness, they carry the influence of truth to those who have no direct access to its sources. Many an extravagant project in practical machinery, and many a wild hypothesis in speculative physics, has been repressed by the general currency of such maxims as we have just quoted.

8. Indeed so familiar and evident are the elementary truths of mechanics when expressed in this simple form, that they are received as truisms; and men are disposed to look back with surprize and scorn at the speculations which were carried on in neglect of them. The most superficial reasoner of modern times thinks himself entitled to speak with contempt and ridicule of Kepler's hypothesis concerning the physical causes of the celestial motions: and gives himself credit for intellectual superiority, because he sees, as self-evident, what such a man could not discover at all. It is well for such a person to recollect, that the real cause of his superior insight is not the pre-eminence of his faculties, but the successful labours of those who have preceded him. The language which he has learnt to use unconsciously, has been adapted to, and moulded on, ascertained truths. When he talks familiarly of "accelerating forces" and "deflexions from the tangent," he is assuming that which Kepler did not know, and which it cost Galileo and his disciples so much labour and thought to establish. Language is often called an instrument of thought; but it is also the nutriment of thought; or rather, it is the atmosphere in which thought lives: a medium essential to the activity of our speculative power, although invisible and imperceptible in its operation; and an element

modifying, by its qualities and changes, the growth and complexion of the faculties which it feeds. In this way the influence of preceding discoveries upon subsequent ones, of the past upon the present, is most penetrating and universal, though most subtle and difficult to trace. The most familiar words and phrases are connected by imperceptible ties with the reasonings and discoveries of former men and distant times. Their knowledge is an inseparable part of ours; the present generation inherits and uses the scientific wealth of all the past. And this is the fortune, not only of the great and rich in the intellectual world: of those who have the key to the ancient storehouses, and who have accumulated treasures of their own;—but the humblest inquirer, while he puts his reasonings into words, benefits by the labours of the greatest discoverers. When he counts his little wealth, he finds that he has in his hands coins which bear the image and superscription of ancient and modern intellectual dynasties; and that in virtue of this possession, acquisitions are in his power, solid knowledge within his reach, which none could ever have attained to, if it were not that the gold of truth, once dug out of the mine, circulates more and more widely among mankind.

9. Having so fully examined, in the preceding instances, the nature of the progress of thought which science implies, both among the peculiar cultivators of science, and in that wider world of general culture which receives only an indirect influence from scientific discoveries, we shall not find it necessary to go into the same extent of detail with regard to the other provinces of human knowledge. In the case of the Mechanical Sciences, we have endeavoured to show, not only that Ideas are requisite in order to form into a science the Facts which nature offers to us, but that we can advance,

almost or quite, to a complete identification of the Facts with the Ideas. In the sciences to which we now proceed, we shall not seek to fill up the chasm by which Facts and Ideas are separated; but we shall endeavour to detect the Ideas which our knowledge involves, to show how essential these are; and in some respects to trace the mode in which they have been gradually developed among men.

10. The motions of the heavenly bodies, their laws, their causes, are among the subjects of the first division of the Mechanical Sciences; and of these sciences we formerly sketched the history, and have now endeavoured to exhibit the philosophy. If we were to take any other class of motions, *their* laws and causes might give rise to sciences which would be mechanical sciences in exactly the same sense in which Physical Astronomy is so. The phenomena of magnets, of electrical bodies, of galvanical apparatus, seem to form obvious materials for such sciences; and if they were so treated, the philosophy of such branches of knowledge would naturally come under our consideration at this point of our progress.

But on looking more attentively at the sciences of Electricity, Magnetism, and Galvanism, we discover cogent reasons for transferring them to another part of our arrangement; we find it advisable to associate them with Chemistry, and to discuss their principles when we can connect them with the principles of chemical science. For though the first steps and narrower generalizations of these sciences depend upon mechanical ideas, the highest laws and widest generalizations which we can reach respecting them, involve chemical relations. The progress of these portions of knowledge is in some respects opposite to the progress of Physical Astronomy. In this, we begin with phenomena which appear to indicate peculiar and various qualities in the bodies which we consider, (namely, the heavenly bodies,) and we find in the end that all these qualities resolve themselves into one common mechanical property, which exists alike in all bodies and parts of bodies. On the contrary, in studying magnetical and electrical laws, we appear at first to have a single extensive phenomenon, attraction and repulsion: but in our attempts to generalize this phenomenon, we find that it is governed by conditions depending upon something quite separate from the bodies themselves, upon the presence and distribution of peculiar and transitory agencies; and, so far as we can discover, the general laws of these agencies are of a *chemical* nature, and are brought into action by peculiar properties of special substances. In cosmical phenomena, everything, in proportion as it is referred to mechanical principles, tends to simplicity,—to permanent uniform forces,—to one common, positive, property. In magnetical and electrical appearances, on the contrary, the application of mechanical principles leads only to a new complexity, which requires a new explanation; and this explanation involves changeable and various forces,—gradations and oppositions of qualities. The doctrine of the universal gravitation of matter is a simple and ultimate truth, in which the mind can acquiesce and repose. We rank gravity among the mechanical attributes of matter, and we see no necessity to derive it from any ulterior properties. Gravity belongs to matter, independent of any conditions. But the *conditions* of magnetic or electrical activity require investigation as much as the *laws* of their action. Of these conditions no mere mechanical explanation can be given; we are compelled to take along with us chemical properties and relations also: and thus magnetism, electricity, galvanism, are *mechanico-chemical sciences.*

11. Before considering these, therefore, I shall treat

of what I shall call *Secondary Mechanical Sciences;* by which expression I mean the sciences depending upon certain qualities which our senses discover to us in bodies;—*Optics*, which has visible phenomena for its subject; *Acoustics*, the science of hearing; the doctrine of *Heat*, a quality which our touch recognizes: to this last science I shall take the liberty of sometimes giving the name *Thermotics*, analogous to the names of the other two. If our knowledge of the phenomena of Smell and Taste had been successfully cultivated and systematized, the present part of our work would be the place for the philosophical discussion of those sensations as the subjects of science.

The branches of knowledge thus grouped in one class involve common Fundamental Ideas, from which their principles are derived in a mode analogous, at least in a certain degree, to the mode in which the principles of the mechanical sciences are derived from the fundamental ideas of causation and reaction. We proceed now to consider these Fundamental Ideas, their nature, development, and consequences.

ADDITIONAL NOTE TO CHAPTER IV.—ON THE AXIOMS WHICH RELATE TO THE IDEA OF CAUSE.

THE Axiom that *Reaction is equal and opposite to Action*, may appear to be at variance with a maxim concerning Cause which is commonly current; namely, that the "Cause precedes Effect, and Effect follows Cause." For it may be said, if A, the Action, and R, the Reaction, can be considered as mutually the cause of each other, A must precede R, and yet must follow it, which is impossible. But to this I reply, that in those cases of direct Causation to which the maxim applies, the Cause and Effect are not successive, but simultaneous. If I press against some obstacle, the obstacle resists and returns the pressure at the instant it is exerted, not after any interval of time, however small. The common maxim, that the effect follows the cause, has arisen from the practice of considering, as examples of cause and effect, not instantaneous forces or causes, and the instantaneous changes which they produce; but taking, instead of this latter, the *cumulative* effects produced in the course of time, and compared with like results occurring without the action of the cause. Thus, if we alter the length of a clock-pendulum, this change produces, as its effect, a subsequent change of rate in the clock: because the rate is measured by the accumulated effects of the pendulum's gravity, before and after the change. But the pendulum produces its mechanical effect upon the escapement, at the moment of its contact, and each wheel upon the next, at the moment of *its* contact. As has been said in a Review of this work, "The time lost in cases of indirect physical causation is consumed in the movements which take place among the parts of the mechanism in action, by which the active forces so transformed into momentum are transported over intervals of space to new points of action, the motion of matter in such cases being regarded as a mere carrier of force." (*Quarterly Rev.*, No. cxxxv., p. 212.) See this subject further treated in a Memoir entitled, "Discussion of the Question:—Are Cause and Effect Successive or Simultaneous?" in the *Memoirs of the Cambridge Philosophical Society*, Vol. VII. Part iii.

ADDITIONAL NOTE TO CHAPTER VI., SECT. 5.—ON THE CENTER OF GRAVITY.

To the doctrine that mechanical principles, such as the one here under consideration (that the pressure on the point of support is equal to the sum of the weights), are derived from our Ideas, and do not flow from but regulate our experience, objections are naturally made by those who assert all our knowledge to be derived from experience. How, they ask, can we know the properties of pressures, levers and the like, except from experience? What but experience can possibly inform us that a force applied transversely to a lever will have any tendency to turn the lever on its center? This cannot be, except we suppose in the lever tenacity, rigidity and the like, which are qualities known only by experience. And it is obvious that this line of argument might be carried on through the whole subject.

My answer to this objection is a remark of the same kind as one which I have made respecting the Ideas of Space, Time, and Number, in a Note at the end of Chapter x. of the last Book. The mind, in apprehending events as causes and effects, is governed by Laws of its own Activity; and these Laws govern the results of the mind's action;

and make these results conform to the Axioms of Causation. But this activity of the mind is awakened and developed by being exercised; and in dealing with the examples of cause and effect here spoken of, (namely, pressure and resistance, force and motion,) the mind's activity is necessarily governed also by the bodily powers of perception and action. We are human beings only in so far as we have existed in space and time, and of our human faculties, developed by our existence in space and time, space and time are necessary conditions. In like manner, we are human beings only in so far as we have bodies, and bodily organs; and our bodies necessarily imply material objects external to us. And hence our human faculties, developed by our bodily existence in a material world, have the conditions of matter for their necessary Laws. I have already said (Chap. v.) that our conception of Force arises with our consciousness of our own muscular exertions;—that Force cannot be conceived without Resistance to exercise itself upon;—and that this resistance is supplied by Matter. And thus the conception of Matter, and of the most general modes in which Matter receives, resists, and transmits force, are parts of our constitution which, though awakened and unfolded by our being in a material world, are not distinguishable from the original structure of the mind. I do not ascribe to the mind Ideas which it would have, even if it had no intercourse with the world of space, time, and matter; because we cannot imagine a mind in such a state. But I attempt to point out and classify those Conditions of all Experience, to which the intercourse of all minds with the material world has necessarily given rise in all. Truths *thus* necessarily acquired in the course of all experience, cannot be said to be learnt *from experience*, in the same sense in which particular facts, at definite times, are learnt from experience, learnt by some persons and not by others, learnt with more or less of certainty. These latter *special truths of experience* will be very important subjects of our consideration; but our whole chance of discussing them with any profit depends upon our keeping them distinct from the necessary and universal conditions of experience. Here, as everywhere, we must keep in view the fundamental antithesis of Ideas and Facts.

BOOK IV.

THE PHILOSOPHY OF THE SECONDARY MECHANICAL SCIENCES.

CHAPTER I.

OF THE IDEA OF A MEDIUM AS COMMONLY EMPLOYED.

1. *Of Primary and Secondary Qualities.*—IN the same way in which the mechanical sciences depend upon the Idea of Cause, and have their principles regulated by the development of that Idea, it will be found that the sciences which have for their subject Sound, Light, and Heat, depend for *their* principles upon the Fundamental Idea of Media by means of which we perceive those qualities. Like the idea of cause, this idea of a medium is unavoidably employed, more or less distinctly, in the common, unscientific operations of the understanding; and is recognized as an express principle in the earliest speculative essays of man. But here also, as in the case of the mechanical sciences, the developement of the idea, and the establishment of the scientific truths which depend upon it, was the business of a succeeding period, and was only executed by means of long and laborious researches, conducted with a constant reference to experiment and observation.

Among the most prominent manifestations of the influence of the idea of a medium of which we have now to speak, is the distinction of the qualities into *primary*, and *secondary* qualities. This distinction has been constantly spoken of in modern times: yet it has often been a subject of discussion among metaphysicians whether there be really such a distinction, and what the true difference is. Locke states it thus*: original or primary qualities of bodies are "such as are utterly inseparable from the body in what estate soever it may be,—such as sense constantly finds in every particle of matter which has bulk enough to be perceived, and the mind finds inseparable from every particle of matter, though less than to make itself singly perceived by our senses:" and he enumerates them as solidity, extension, figure, motion or rest, and number. Secondary qualities, on the other hand, are such "which in truth are nothing in the objects themselves, but powers to produce various sensations in us by their primary qualities, *i. e.*, by the bulk, figure, texture, and motion of their insensible parts, as colours, sounds, tastes, &c."

Dr. Reid†, reconsidering this subject, puts the difference in another way. There is, he says, a real foundation for the distinction of primary and secondary qualities, and it is this: "That our senses give us a direct and distinct notion of the primary qualities, and inform us what they are in themselves; but of the secondary qualities, our senses give us only a relative and obscure notion. They inform us only that they are qualities that affect us in a certain manner, that is, produce in us a certain sensation; but as to what they are in themselves, our senses leave us in the dark."

Dr. Brown‡ states the distinction somewhat otherwise. We give the name of matter, he observes, to that which has extension and resistance: these, therefore, are primary qualities of matter, because they compose our

* *Essay*, B. II. ch. viii. s. 9, 10. † *Essays*, B. II. c. xvii.

‡ *Lectures*, II. 12.

definition of it. All other qualities are secondary, since they are ascribed to bodies only because we find them associated with the primary qualities which form our notion of those bodies.

It is not necessary to criticize very strictly these various distinctions. If it were, it would be easy to find objections to them. Thus Locke, it may be observed, does not point out any *reason* for believing that his secondary qualities are produced by the primary. How are we to learn that the colour of a rose arises from the bulk, figure, texture, and motion of its particles? Certainly our senses do not teach us this; and in what other way, on Locke's principles, can we learn it? Reid's statement is not more free from the same objection. How does it appear that our notion of Warmth is relative to our own sensations more than our notion of Solidity? And if we take Brown's account, we may still ask whether our selection of certain qualities to form our idea and definition of matter be arbitrary and without reason? If it be, how can it make a real distinction? if it be not, what is the reason?

I do not press these objections, because I believe that any of the above accounts of the distinction of primary and secondary qualities is right in the main, however imperfect it may be. The difference between such qualities as Extension and Solidity on the one hand, and Colour or Fragrance on the other, is assented to by all, with a conviction so firm and indestructible, that there must be some fundamental principle at the bottom of the belief, however difficult it may be to clothe the principle in words. That successive efforts to express the real nature of the difference were made by men so clear-sighted and acute as those whom I have quoted, even if none of them are satisfactory, shows how strong and how deeply-seated is the perception of truth which impels us to such attempts.

The most obvious mode of stating the difference of primary and secondary qualities, as it naturally offers itself to speculative minds, appears to be that employed by Locke, slightly modified. Certain of the qualities of bodies, as their bulk, figure, and motion, are perceived immediately in the bodies themselves. Certain other qualities as sound, colour, heat, are perceived by means of some medium. Our conviction that this is the case is spontaneous and irresistible; and this difference of qualities immediately and mediately perceived is the distinction of primary and secondary qualities. We proceed further to examine this conviction.

2. *The Idea of Externality.*—In reasoning concerning the secondary qualities of bodies, we are led to assume the bodies to be external to us, and to be perceived by means of some medium intermediate between us and them. These assumptions are fundamental conditions of perception, inseparable from it even in thought.

That objects are *external* to us, that they are *without* us, that they have *outness*, is as clear as it is that these words have any meaning at all. This conviction is, indeed, involved in the exercise of that faculty by which we perceive all things as existing in space; for by this faculty we place ourselves and other objects in one common space, and thus they are exterior to us. It may be remarked that this apprehension of objects as external to us, although it assumes the idea of space, is far from being implied in the idea of space. The objects which we contemplate are considered as existing in space, and by that means become invested with certain mutual relations of position; but when we consider them as existing without us, we make the additional step of supposing *ourselves* and the objects to exist in one common space. The question respecting the Ideal Theory of Berkeley has been mixed up with the recognition of this condition of the externality of objects. That philosopher maintained, as is well known, that the perceptible qualities of bodies have no existence except in a perceiving mind. This system has often been understood as if he had imagined the world to be a kind of optical illusion, like the images which we see when we shut our eyes, appearing to be without us, though they are only in our organs; and thus this Ideal System has been opposed to a belief in an external world. In truth, however, no such opposition exists. The Ideal System is an attempt to explain the mental process of perception, and to get over the difficulty of mind being affected by matter. But the author of that system did not deny that objects were perceived under the conditions of space and mechanical causation;—that they were external and material so far as those words describe perceptible qualities. Berkeley's system, however visionary or erroneous, did not prevent his entertaining views as just, concerning optics or acoustics, as if he had held any other doctrine of the nature of perception.

But when Berkeley's theory was understood as a denial of the existence of objects without us, how was it answered? If we examine the answers which are given by Reid and other philosophers to this hypothesis, it will be found that they amount to this: that objects *are* without us, since we *perceive* that they are so; that we perceive them to be external, by the same act by which we perceive them to be objects. And thus, in this stage of philosophical inquiry, the externality of objects is recognized as one of the inevitable conditions of our perception of them; and hence the Idea of Externality is adopted as one of the necessary foundations of all reasoning concerning all objects whatever.

3. *Sensation by a Medium.*—Objects, as we have just seen, are necessarily apprehended as without us; and in general, as removed from us by a great or small distance.

Yet they affect our bodily senses; and this leads us irresistibly to the conviction that they are perceived by means of something intermediate. Vision, or hearing, or smell, or the warmth of a fire, must be communicated to us by some medium of sensation. This unavoidable belief appears in all attempts, the earliest and the latest alike, to speculate upon such subjects. Thus, for instance, Aristotle says*, "Seeing takes place in virtue of some action which the sentient organ suffers: now it cannot suffer action from the colour of the object directly: the only remaining possible case then is, that it is acted upon by an intervening Medium; there must then be an intervening Medium." "And the same may be said," he adds, "concerning sounding and odorous bodies; for these do not produce sensation by touching the sentient organ, but the intervening Medium is acted on by the sound or the smell, and the proper organ, by the Medium....In sound the Medium is air; in smell we have no name for it." In the sense of taste, the necessity of a Medium is not at first so obviously seen, because the object tasted is brought into contact with the organ; but a little attention convinces us that the taste of a solid body can only be perceived when it is conveyed in some liquid vehicle. Till the fruit is crushed, and till its juices are pressed out, we do not distinguish its flavour. In the case of heat, it is still more clear that we are compelled to suppose some invisible fluid, or other means of communication, between the distant body which warms us and ourselves.

It may appear to some persons that the assumption of an intermedium between the object perceived and the sentient organ results from the principles which form the basis of our mechanical reasonings,—that every change must have a cause, and that bodies can act upon

* Περὶ Ψυχῆς. II. 7.

each other only by contact. It cannot be denied that this principle does offer itself very naturally as the ground of our belief in media of sensation; and it appears to be referred to for this purpose by Aristotle in the passage quoted above. But yet we cannot but ask, Does the principle, that matter produces its effect by contact only, manifestly apply here? When we so apply it, we include *sensation* among the *effects* which material contact produces;—a case so different from any merely mechanical effect, that the principle, so employed, appears to acquire a new signification. May we not, then, rather say that we have here a new axiom,—That sensation implies a material cause immediately acting on the organ,—than a new application of our former proposition,—That all mechanical change implies contact?

The solution of this doubt is not of any material consequence to our reasonings; for whatever be the ground of the assumption, it is certain that we do assume the existence of media by which the sensations of sight, hearing, and the like, are produced; and it will be seen shortly that principles inseparably connected with this assumption are the basis of the sciences now before us.

This assumption makes its appearance in the physical doctrines of all the schools of philosophy. It is exhibited perhaps most prominently in the tenets of the Epicureans, who were materialists, and extended to all kinds of causation the axiom of the existence of a corporeal mechanism by which alone the effect is produced. Thus, according to them, vision is produced by certain images or material films which flow from the object, strike upon the eyes, and so become sensible. This opinion is urged with great detail and earnestness by Lucretius, the poetical expositor of the Epicurean creed among the Romans. His fundamental conviction of the necessity of a material medium is obviously the basis of his reasoning, though he attempts to show the existence of such a medium by facts. Thus he argues *, that by shouting loud we make the throat sore; which shows, he says, that the voice must be material, so that it can hurt the passage in coming out.

> Haud igitur dubium est quin voces verbaque constent
> Corporeis e principiis ut lædere possint.

4. *The Process of Perception of Secondary Qualities.*—The likenesses or representatives of objects by which they affect our senses were called by some writers *species*, or *sensible species*, a term which continued in use till the revival of science. It may be observed that the conception of these species as films cast off from the object, and retaining its shape, was different, as we have seen, from the view which Aristotle took, though it has sometimes been called the Peripatetic doctrine †. We may add that the expression was latterly applied to express the supposition of an emanation of any kind, and implied little more than that supposition of a medium of which we are now speaking. Thus Bacon, after reviewing the phenomena of sound, says ‡, "Videntur motus soni fieri per *species spirituales:* ita enim loquendum donec certius quippiam inveniatur."

Though the fundamental principles of several sciences depend upon the assumption of a medium of perception, these principles do not at all depend upon any special view of the process of our perceptions. The mechanism of that process is a curious subject of consideration; but it belongs to physiology, more properly than either to metaphysics, or to those branches of physics of which we are now speaking. The general nature of the process is the same for all the senses. The object affects the appropriate intermedium; the medium, through the proper

* Lib. IV. 529 † Brown, Vol. II. p. 98.
‡ *Hist. Son. et Aud.*, Vol. IX. p. 87.

organ, the eye, the ear, the nose, affects the nerves of the particular sense; and, by these, in some way, the sensation is conveyed to the mind. But to treat the *impression* upon the nerves as the *act* of sensation which we have to consider, would be to mistake our object, which is not the constitution of the human body, but of the human mind. It would be to mistake one link for the power which holds the end of the chain. No anatomical analysis of the corporeal conditions of vision, or hearing, or feeling warm, is necessary to the sciences of Optics, or Acoustics, or Thermotics.

Not only is this physiological research an extraneous part of our subject, but a partial pursuit of such a research may mislead the inquirer. We perceive objects *by means of* certain media, and *by means of* certain impressions on the nerves: but we cannot with propriety say that we perceive either the media or the impressions on the nerves. What person in the act of seeing is conscious of the little coloured spaces on the retina? or of the motions of the bones of the auditory apparatus whilst he is hearing? Surely, no one. This may appear obvious enough, and yet a writer of no common acuteness, Dr. Brown, has put forth several very strange opinions, all resting upon the doctrine that the coloured spaces on the retina are the *objects* which we perceive; and there are some supposed difficulties and paradoxes on the same subject which have become quite celebrated (as upright vision with inverted images), arising from the same confusion of thought.

As the consideration of the difficulties which have arisen respecting the philosophy of perception may serve still further to illustrate the principles on which we necessarily reason respecting the secondary qualities of bodies, I shall here devote a few pages to that subject.

Chapter II.

ON PECULIARITIES IN THE PERCEPTIONS OF THE DIFFERENT SENSES.

1. We cannot doubt that we perceive all secondary qualities by means of immediate impressions made, through the proper medium of sensation, upon our organs. Hence all the senses are sometimes vaguely spoken of as modifications of the sense of feeling. It will, however, be seen, on reflection, that this mode of speaking identifies in words things which in our conceptions have nothing in common. No impression on the organs of touch can be conceived as having any resemblance to colour or smell. No effort, no ingenuity, can enable us to describe the impressions of one sense in terms borrowed from another.

The senses have, however, each its peculiar powers, and these powers may be in some respects compared, so as to show their leading resemblances and differences, and the characteristic privileges and laws of each. This is what we shall do as briefly as possible.

Sect. I.—*Prerogatives of Sight.*

The sight distinguishes colours, as the hearing distinguishes tones; the sight estimates degrees of brightness, the ear, degrees of loudness; but with several resemblances, there are most remarkable differences between these two senses.

2. *Position.*—The sight has this peculiar prerogative, that it apprehends the *place* of its objects directly and primarily. We see *where* an object is at the same instant that we see what it is. If we see two objects, we see their relative position. We cannot help perceiving

that one is above or below, to the right or to the left of the other, if we perceive them at all.

There is nothing corresponding to this in sound. When we hear a noise, we do not necessarily assign a place to it. It may easily happen that we cannot tell from which side a thunder-clap comes. And though we often can judge in what direction a voice is heard, this is a matter of secondary impression, and of inference from concomitant circumstances, not a primary fact of sensation. The judgments which we form concerning the position of sounding bodies are obtained by the conscious or unconscious comparison of the impressions made on the two ears, and on the bones of the head in general; they are not inseparable conditions of hearing. We may hear sounds, and be uncertain whether they are "above, around, or underneath!" but the moment any thing visible appears, however unexpected, we can say, "see *where* it comes!"

Since we can see the relative position of things, we can see *figure*, which is but the relative position of the different parts of the boundary of the object. And thus the whole visible world exhibits to us a scene of various shapes, coloured and shaded according to their form and position, but each having relations of position to all the rest; and altogether, entirely filling up the whole range which the eye can command.

3. *Distance.*—The distance of objects from us is no matter of immediate perception, but is a judgment and inference formed from our sensations, in the same way as our judgment of position by the ear. That this is so, was most distinctly shown by Berkeley, in his *New Theory of Vision.* The elements on which we form our judgment are, the effort by which we fix both eyes on the same object, the effort by which we adjust each eye to distinct vision, and the known forms, colours, and

parts of objects, as compared with their appearance. The right interpretation of the information which these circumstances give us respecting the true distances and forms of things, is gradually learnt by experience, the lesson being begun in our earliest infancy, and inculcated upon us every hour during which we use our eyes. The completeness with which the lesson is learnt is truly admirable; for we forget that our conclusion is obtained indirectly, and mistake a judgment on evidence for an intuitive perception. This, however, is not more surprizing than the rapidity and unconsciousness of effort with which we understand the meaning of the speech that we hear, or the book that we read. In both cases, the habit of interpretation is become as familiar as the act of perception. And this is the case with regard to vision. We see the breadth of the street as clearly and readily as we see the house on the other side of it. We see the house to be square, however obliquely it be presented to us. Indeed the difficulty is, to recover the consciousness of our real and original sensations;—to discover what is the apparent relation of the lines which appear before us. As we have already said, in the common process of vision we suppose ourselves to see that which cannot be seen; and when we would make a picture of an object, the difficulty is to represent what is visible and no more.

But perfect as is our habit of interpreting what we perceive, we could not interpret if we did not perceive. If the eye did not apprehend visible position, it could not infer actual position, which is collected from visible position as a consequence: if we did not see apparent figure, we could not arrive at any opinion concerning real form. The perception of place, which is the prerogative of the eye, is the basis of all its other superiority.

The precision with which the eye can judge of apparent position is remarkable. If we had before us two stars distant from each other by one-twentieth of the moon's diameter, we could easily decide the apparent direction of the one from the other, as above or below, to the right or left. Yet eight millions of stars might be placed in the visible hemisphere of the sky at such distances from each other; and thus the eye would recognize the relative position in a portion of its range not greater than one eight-millionth of the whole. Such is the accuracy of the sense of vision in this respect; and, indeed, we might with truth have stated it much higher. Our judgment of the position of distant objects in a landscape depends upon features far more minute than the magnitude we have here described.

As our object is to point out principally the differences of the senses, we do not dwell upon the delicacy with which we distinguish tints and shades, but proceed to another sense.

SECT. II.—*Prerogatives of Hearing.*

THE sense of hearing has two remarkable prerogatives; it can perceive a definite and peculiar relation between certain tones, and it can clearly perceive two tones together; in both these circumstances it is distinguished from vision, and from the other senses.

4. *Musical Intervals.*—We perceive that two tones have, or have not, certain definite relations to each other, which we call *Concords*: one sound is a *Fifth*, an *Octave*, &c., above the other. And when this is the case, our perception of the relation is extremely precise. It is easy to perceive when a fifth is out of tune by one-twentieth of a tone; that is, by one-seventieth of itself. To this there is nothing analogous in vision. Colours have certain vague relations to one another; they look well together, by contrast or by resemblance; but this

is an indefinite, and in most cases a casual and variable feeling. The relation of *complementary* colours to one another, as of red to green, is somewhat more definite; but still, has nothing of the exactness and peculiarity which belongs to a musical concord. In the case of the two sounds, there is an exact point at which the relation obtains; when by altering one note we pass this point, the concord does not gradually fade away, but instantly becomes a discord; and if we go further still, we obtain another concord of quite a different character.

We learn from the theory of sound that concords occur when the times of vibration of the notes have exact simple ratios; an octave has these times as 1 to 2; a fifth, as 2 to 3. According to the undulatory theory of light, such ratios occur in colours, yet the eye is not affected by them in any peculiar way. The times of the undulations of certain red and certain violet rays are as 2 to 3, but we do not perceive any peculiar harmony or connexion between those colours.

5. *Chords*.—Again, the ear has this prerogative, that it can apprehend two notes together, yet distinct. If two notes, distant by a fifth from each other, are sounded on two wind instruments, both they and their musical relation are clearly perceived. There is not a mixture, but a concord, an interval. In colours, the case is otherwise. If blue and yellow fall on the same spot, they form green; the colour is simple to the eye; it can no more be decomposed by the vision than if it were the simple green of the prismatic spectrum: it is impossible for us, by sight, to tell whether it is so or not.

These are very remarkable differences of the two senses: two colours can be compounded into an apparently simple one; two sounds cannot: colours pass into each other by gradations and intermediate tints; sounds pass from one concord to another by no gradations: the most intolerable discord is that which is near a concord. We shall hereafter see how these differences affect the *scales* of sound and of colour.

6. *Rhythm.*—We might remark, that as we see objects in *space*, we hear sounds in *time*; and that we thus introduce an arrangement among sounds which has several analogies with the arrangement of objects in space. But the conception of time does not seem to be peculiarly connected with the sense of hearing; a faculty of apprehending tone and time, or in musical phraseology *tune* and *rhythm*, are certainly very distinct. I shall not, therefore, here dwell upon such analogies.

The other Senses have not any peculiar prerogatives, at least none which bear on the formation of science. I may, however, notice, in the feeling of heat, this circumstance; that it presents us with two opposites, heat and cold, which graduate into each other. This is not quite peculiar, for vision also exhibits to us white and black, which are clearly opposites, and which pass into each other by the shades of gray.

Sect. III.—*The Paradoxes of Vision.*

7. *First Paradox of Vision. Upright Vision.*—All our senses appear to have this in common;—That they act by means of organs, in which a bundle of nerves receives the impression of the appropriate medium of the sense. In the construction of these organs there are great differences and peculiarities, corresponding, in part at least, to the differences in the information given. Moreover, in some cases, as we have noted in the case of audible position and visible distance, that which seems to be a perception is really a judgment founded on perceptions of which we are not directly aware. It will be seen, therefore, that with respect to the peculiar powers of each sense, it may be asked;—whether they can be

explained by the construction of the peculiar organ;—whether they are acquired judgments and not direct perceptions;—or whether they are inexplicable in either of these ways, and cannot, at present at least, be resolved into anything but conditions of the intellectual act of perception.

Two of these questions with regard to vision, have been much discussed by psychological writers: the cause of our seeing objects upright by inverted images on the retina; and of our seeing single with two such images.

Physiologists have very completely explained the exquisitely beautiful mechanism of the eye, considered as analogous to an optical instrument; and it is indisputable that by means of certain transparent lenses and humours, an inverted image of the objects which are looked at is formed upon the *retina*, or fine network of nerve, with which the back of the eye is lined. We cannot doubt that the impression thus produced on these nerves is essential to the act of vision; and so far as we consider the nerves themselves to feel or perceive by contact, we may say that they perceive this image, or the affections of light which it indicates. But we cannot with any propriety say that *we* perceive, or that our mind perceives, this image; for we are not conscious of it, and none but anatomists are aware of its existence: we perceive *by means of* it.

A difficulty has been raised, and dwelt upon in a most unaccountable manner, arising from the neglect of this obvious distinction. It has been asked, how is it that we see an object, a man for instance, upright, when the immediate object of our sensation, the image of the man on our retina, is inverted? To this we must answer, that we see him upright *because* the image is inverted; that the inverted image is the necessary means of seeing an upright object. This is granted, and where then is the difficulty? Perhaps it may be put thus: How is it that we do not judge the man to be inverted, since the sensible image is so? To this we may reply, that we have no notion of *upright* or inverted, except that which is founded on experience, and that all our experience, without exception, must have taught us that such a sensible image belongs to a man who is in an upright position. Indeed, the contrary judgment is not conceivable; a man is upright whose head is upwards and his feet downwards. But what are the sensible images of *upwards* and *downwards?* Whatever be our standard of up and down, the sensible representation of *up* will be an image moving on the retina towards the lower side, and the sensible representation of *down* will be a motion towards the upper side. The head of the man's image is towards the image of the sky, its feet are towards the image of the ground; how then should it appear otherwise than upright? Do we expect that the whole world should appear inverted? Be it so: but if the whole be inverted, how is the relation of the parts altered? Do we expect that we should think our own persons in particular inverted? This cannot be, for we look at them as we do at other objects. Do we expect that things should appear to fall upwards? Surely not. For what do we know of upwards, except that it is the direction in which bodies do *not* fall? In short, the whole of this difficulty, though it has in no small degree embarrassed metaphysicians, appears to result from a very palpable confusion of ideas; from an attempt at comparison of what *we* see, with that which the retina feels, as if they were separately presentable. It is a sufficient explanation to say, that we do not see the image on the retina, but see by *means* of it. The perplexity does not require much more skill to disentangle, than it does to see that a word written in *black* ink, may signify *white* *.

8. *Second Paradox of Vision. Single Vision.*—(1.) *Small or Distant Objects.*—The other difficulty, why with two images on the retina we see only one object, is of a much more real and important kind. This effect is manifestly limited by certain circumstances of a very precise nature; for if we direct our eyes at an object which is very near the eye, we see all other objects double. The fact is not, therefore, that we are incapable of receiving two impressions from the two images, but that, under certain conditions, the two impressions form one. A little attention shows us that these conditions are, that with both eyes we should look at the same object; and again, we find that to look at an object with either eye, is to direct the eye so that the image falls

* The explanation of our seeing objects erect when the image is inverted has been put very simply, by saying, "We *call* that the *lower* end of an object which is next the ground." The observer cannot look into his own eye; he knows *by experience* what *kind* of image corresponds to a man in an upright position. The anatomist tells him that this image is *inverted:* but this does not disturb the process of judging by experience. It does not appear why any one should be perplexed at the notion of seeing objects erect by means of inverted images, rather than at the notion of seeing objects large by means of small images; or cubical and pyramidal, by means of images on a spherical surface; or green and red, by means of images on a black surface. Indeed some persons have contrived to perplex themselves with these latter questions, as well as the first.

The above explanation is not at all affected, as to its substance, if we adopt Sir David Brewster's expression, and say that the *line of visible direction* is a line passing through the center of the spherical surface of the retina, and therefore of course perpendicular to the surface. In speaking of "the inverted image," it has always been supposed to be determined by such lines; and though the point where they intersect may not have been ascertained with exactness by previous physiologists, the philosophical view of the matter was not in any degree vitiated by this imperfection.

on or near a particular point about the middle of the retina. Thus these middle points in the two retinas correspond, and we see an image single when the two images fall on the corresponding points.

Again, as each eye judges of position, and as the two eyes judge similarly, an object will be seen in the same place by one eye and by the other, when the two images which it produces are *similarly situated* with regard to the *corresponding points* of the retina*.

This is the Law of Single Vision, at least so far as regards small objects; namely, objects so small that in contemplating them we consider their position only, and not their solid dimensions. Single vision in such cases is a result of the law of vision simply: and it is a mistake to call in, as some have done, the influence of

* The explanation of single vision with two eyes may be put in another form. Each eye judges immediately of the relative position of all objects within the field of its direct vision. Therefore when we look with both eyes at a *distant* prospect (so distant that the distance between the eyes is small in comparison) the two prospects, being similar collections of forms, will coincide altogether, if a corresponding point in one and in the other coincide. If this be the case, the two images of every object will fall upon corresponding points of the retina, and will appear single.

If the two prospects seen by the two eyes do not exactly coincide, in consequence of nearness of the objects, or distortion of the eyes, but if they nearly coincide, the stronger image of an object absorbs the weaker, and the object is seen single; yet modified by the combination, as will be seen when we speak of the single vision of near objects. When the two images of an object are considerably apart, we see it double.

This explanation is not different in substance from the one given in the text; but perhaps it is better to avoid the assertion that the law of corresponding points is "a distinct and original principle of our constitution," as I had stated in the first edition. The simpler mode of stating the law of our constitution appears to be to say, that each eye determines similarly the position of objects; and that when the positions of an object, as seen by the two eyes, coincide (or nearly coincide) the object is seen single.

habit and of acquired judgments, in order to determine the result in such cases.

To ascribe the apparent singleness of objects to the impressions of vision corrected by the experience of touch*, would be to assert that a person who had not been in the habit of handling what he saw, would see all objects double; and also, to assert that a person beginning with the double world which vision thus offers to him, would, by the continued habit of handling objects, gradually and at last learn to see them single. But all the facts of the case show such suppositions to be utterly fantastical. No one can, in this case, go back from the habitual judgment of the singleness of objects, to the original and direct perception of their doubleness, as the draughtsman goes back from judgments to perception, in representing solid distances and forms by means of perspective pictures. No one can point out any case in which the habit is imperfectly formed; even children of the most tender age look at an object with both eyes, and see it as one.

In cases when the eyes are distorted (in squinting), one eye only is used, or if both are employed, there is double vision; and thus any derangement of the correspondence of motion in the two eyes will produce double-sightedness.

Brown is one of those† who assert that two images suggest a single object because we have *always found* two images to belong to a single object. He urges as an illustration, that the *two* words "he conquered," by custom excite exactly the same notion as the *one* Latin word "vicit;" and thus that two visual images, by the effect of habit, produce the same belief of a single object as one tactual impression. But in order to make this pretended illustration of any value, it ought

* See Brown, Vol. II. p. 81.

† *Lectures*, Vol. II. p. 81.

to be true that when a person has thoroughly learnt the Latin language, he can no longer distinguish any separate meaning in "he" and in "conquered." We can by no effort perceive the double sensation, when we look *at* the object with the two eyes. Those who squint, learn by habit to see objects single: but the habit which they acquire is that of attending to the impressions of one eye only at once, not of combining the two impressions. It is obvious, that if each eye spreads before us the same visible scene, with the same objects and the same relations of place, then, if one object in each scene coincide, the whole of the two visible impressions will be coincident. And here the remarkable circumstance is, that not only each eye judges for itself of the relations of position which come within its field of view; but that there is a superior and more comprehensive faculty which combines and compares the two fields of view; which asserts or denies their coincidence; which contemplates, as in a relative position to one another, these two visible worlds, in which all other relative position is given. This power of confronting two sets of visible images and figured spaces before a purely intellectual tribunal, is one of the most remarkable circumstances in the sense of vision.

9. (2.) *Near Objects.*—We have hitherto spoken of the singleness of objects whose images occupy corresponding positions on the retina of the two eyes. But here occurs a difficulty. If an object of moderate size, a small thick book for example, be held at a little distance from the eyes, it produces an image on the retina of each eye; and these two images are perspective representations of the book from different points of view, (the positions of the two eyes,) and are therefore of different forms. Hence the two images cannot occupy corresponding points of the retina throughout their whole

extent. If the central parts of the two images occupy corresponding points, the boundaries of the two will not correspond. How is it then consistent with the law above stated, that in this case the object appears single?

It may be observed, that the two images in such a case will differ most widely when the object is not a mere surface, but a solid. If a book, for example, be held with one of its upright edges towards the face, the right eye will see one side more directly than the left eye, and the left eye will see another side more directly, and the outline of the two images upon the two retinas will exhibit this difference. And it may be further observed, that this difference in the images received by the two eyes, is a plain and demonstrative evidence of the solidity of the object seen; since nothing but a solid object could (without some special contrivance) produce these different forms of the images in the two eyes.

Hence the absence of exact coincidence in the two images on the retina is the necessary condition of the solidity of the object seen, and must be one of the indications by means of which our vision apprehends an object as solid. And that this is so, Mr. Wheatstone has proved experimentally, by means of some most ingenious and striking contrivances. He has devised* an instrument by which two images (drawn in outline) differing exactly as much as the two images of a solid body seen near the face would differ, are conveyed, one to one eye, and the other to the other. And it is found that when this is effected, the object which the images represent is not only seen single, but is apprehended as solid with a clearness and reality of conviction quite distinct from any impression which a mere perspective representation can give.

* *Phil. Trans.*, 1839.

At the same time it is found that the object is then only apprehended as single when the two images are such as are capable of being excited by one single object placed in solid space, and seen by the two eyes. If the images differ more or otherwise than this condition allows, the result is, that both are seen, their lines crossing and interfering with one another.

It may be observed, too, that if an object be of such large size as not to be taken in by a single glance of the eyes, it is no longer apprehended as single by a direct act of perception; but its parts are looked at separately and successively, and the impressions thus obtained are put together by a succeeding act of the mind. Hence the objects which are directly seen as solid, will be of moderate size; in which case it is not difficult to show that the outlines of the two images will differ from each other only slightly.

Hence we are led to the following, as the Law of Single Vision for *near* objects:—When the two images in the two eyes are situated (part for part) nearly, but not exactly, upon corresponding points, the object is apprehended as single, if the two images are such as are or would be given by a single solid object seen by the two eyes separately: and in this case the object is necessarily apprehended as solid.

This law of vision does not contradict that stated above for distant objects: for when an object is removed to a considerable distance, the images in the two eyes coincide exactly, and the object is seen as single, though without any direct apprehension of its solidity. The first law is a special case of the second. Under the condition of *exactly* corresponding points, we have the perception of singleness, but no evidence of solidity. Under the condition of *nearly* corresponding points, we may have the perception of singleness, and with it, of solidity.

We have before noted it as an important feature in our visual perception, that while we have two distinct impressions upon the sense, which we can contemplate separately and alternately, (the impressions on the two eyes,) we have a higher perceptive faculty which can recognize these two impressions, exactly similar to each other, as only two images of one and the same assemblage of objects. But we now see that the faculty by which we perceive visible objects can do much more than this:—it can not only unite two impressions, and recognize them as belonging to one object in virtue of their coincidence, but it can also unite and identify them, even when they do not exactly coincide. It can correct and adjust their small difference, so that they are both apprehended as representations of the same figure. It can infer from them a real form, not agreeing with either of them; and a solid space, which they are quite incapable of exemplifying. The visual faculty decides whether or not the two ocular images can be pictures of the same solid object, and if they can, it undoubtingly and necessarily accepts them as being so. This faculty operates as if it had the power of calling before it all possible solid figures, and of ascertaining by trial whether any of those will, at the same time, fit both the outlines which are given by the sense. It assumes the reality of solid space, and, if it be possible, reconciles the appearances with that reality. And thus an activity of the mind of a very remarkable and peculiar kind is exercised in the most common act of seeing.

10. It may be said that this doctrine, of such a visual faculty as has been described, is very vague and obscure, since we are not told what are its limits. It adjusts and corrects figures which *nearly* coincide, so as to identify them. But *how* nearly, it may be asked, must the figures approach each other, in order that this adjust-

ment may be possible? What discrepance renders impossible the reconcilement of which we speak? Is it not impossible to give a definite answer to these questions, and therefore impossible to lay down definitely such laws of vision as we have stated? To this I reply, that the indefiniteness thus objected to us, is no new difficulty, but one with which philosophers are familiar, and to which they are already reconciled. It is, in fact, no other than the indefiniteness of the limits of distinct vision. How near to the face must an object be brought, so that we shall cease to see it distinctly? The distance, it will be answered, is indefinite: it is different for different persons; and for the same person, it varies with the degree of effort, attention, and habit. But this indefiniteness is only the indefiniteness, in another form, of the deviation of the two ocular images from one another: and in reply to the question concerning them we must still say, as before, that in doubtful cases, the power of apprehending an object as single, when this *can* be done, will vary with effort, attention, and habit. The assumption that the apparent object exists as a real figure, in real space, is to be verified, if possible; but, in extreme cases, from the unfitness of the point of view, or from any other cause of visual confusion or deception, the existence of a real object corresponding to the appearance may be doubtful; as in any other kind of perception it may be doubtful whether our senses, under disadvantageous circumstances, give us true information. The vagueness of the limits, then, within which this visual faculty can be successfully exercised, is no valid argument against the existence of the faculty, or the truth of the law which we have stated concerning its action.

SECT. IV.—*The Perception of Visible Figure.*

11. *Visible Figure.*—There is one tenet on the subject of vision which appears to me so extravagant and unphilosophical, that I should not have thought it necessary to notice it, if it had not been recently promulgated by a writer of great acuteness in a book which has obtained, for a metaphysical work, considerable circulation. I speak of Brown's opinion* that we have no immediate perception of visible figure. I confess myself unable to comprehend fully the doctrine which he would substitute in the place of the one commonly received. He states it thus†: "When the simple affection of sight is blended with the ideas of suggestion [those arising from touch, &c.] in what are termed the acquired perceptions of vision, as, for example, in the perception of a sphere, it is colour only which is blended with the large convexity, and not a small coloured plane." The doctrine which Brown asserts in this and similar passages, appears to be, that we do not by vision perceive *both* colour and *figure*; but that the colour which we see is blended with the figure which we learn the existence of by other means, as by touch. But if this were possible when we can call in other perceptions, how is it possible when we cannot or do not touch the object? Why does the moon appear round, gibbous, or horned? What sense besides vision suggests to us the idea of *her* figure? And even in objects which we can reach, what is that circumstance in the sense of vision which suggests to us that the colour belongs to the sphere, except that we see the colour *where* we see the sphere? If we do not see figure, we do not see position; for figure is the relative position of the parts of a boundary. If we do not see position, why do we ascribe the yellow colour to

* *Lectures*, Vol. II. p. 82. † *Ib.* Vol. II. p. 90.

the sphere on our left, rather than to the cube on our right? We *associate* the colour with the object, says Dr. Brown; but if his opinion were true, we could not associate two colours with two objects, for we could not apprehend the colours as occupying two different places.

The whole of Brown's reasoning on this subject is so irreconcileable with the first facts of vision, that it is difficult to conceive how it could proceed from a person who has reasoned with great acuteness concerning touch. In order to prove his assertion, he undertakes to examine the only reasons which, he says*, he can imagine for believing the immediate perception of visible figure: (1) That it is absolutely impossible, in our present sensations of sight, to separate colour from extension; and (2) That there are, in fact, figures on the retina corresponding to the apparent figures of objects.

On the subject of the first reason, he says, that the figure which we perceive as associated with colour, is the real, and not the apparent figure. "Is there," he asks, "the slightest consciousness of a perception of visible figure, corresponding to the affected portion of the retina?" To which, though he seems to think an affirmative answer impossible, we cannot hesitate to reply, that there is undoubtedly such a consciousness; that though obscured by being made the ground of habitual inference as to the real figure, this consciousness is constantly referred to by the draughtsman, and easily recalled by any one. We may separate colour, he says again†, from the figures on the retina, as we may separate it from length, breadth, and thickness, which we do not see. But this is altogether false: we cannot separate colour from length, breadth, and thickness, in *any other way*, than by transferring it to the visible figure which

* *Lectures*, Vol. II. p. 83. † *Ib.* p. 84.

we do see. He cannot, he allows, separate the colour from the visible form of the trunk of a large oak; but just as little, he thinks, can he separate it from the convex mass of the trunk, which (it is allowed on all hands) he does not immediately see. But in this he is mistaken: for if he were to make a *picture* of the oak, he would separate the colour from the convex shape, which he does not imitate, but he could not separate it from the visible figure, which he does imitate; and he would then perceive that the fact that he *has not* an immediate perception of the convex form, is necessarily connected with the fact that he *has* an immediate perception of the apparent figure; so far is the rejection of immediate perception in the former case from being a reason for rejecting it in the latter.

Again, with regard to the second argument. It does not, he says, follow, that because a certain figured portion of the retina is affected by light, we should see such a figure; for if a certain figured portion of the olfactory organ were affected by odours, we should not acquire by smell any perception of such figure *. This is merely to say, that because we do not perceive position and figure by one sense, we cannot do so by another. But this again is altogether erroneous. It is an office of our sight to inform us of position, and consequently of figure; for this purpose, the organ is so constructed that the position of the object determines the position of the point of the retina affected. There is nothing of this kind in the organ of smell; objects in different positions and of different forms do not affect different parts of the olfactory nerve, or portions of different shape. Different objects, remote from each other, if perceived by smell, affect the same part of the olfactory organs. This is all quite intelligible; for it is not the office of

* *Lectures,* Vol. II. p. 87.

smell to inform us of position. Of what use or meaning would be the curious and complex structure of the eye, if it gave us only such vague and wandering notions of the colours and forms of the flowers in a garden, as we receive from their odours when we walk among them blindfold? It is, as we have said, the *prerogative* of vision to apprehend position: the places of objects on the retina give this information. We do not suppose that the affection of a certain shape of nervous expanse will necessarily and in all cases give us the impression of figure; but we know that in vision it does; and it is clear that if we did not acquire our acquaintance with visible figure in this way, we could not acquire it in any way*.

The whole of this strange mistake of Brown's appears to arise from the fault already noticed;—that of considering the image on the retina as the *object* instead of the *means* of vision. This indeed is what he says: "the true object of vision is not the distant body itself, but the light that has reached the expansive termination of the optic nerve†." Even if this were so, we do not see why we should not perceive the position of the impression on this expanded nerve. But as we have already said, the impression on the nerve is the means of vision, and enables us to assign a place, or at least a direction, to the object from which the light proceeds, and thus makes vision possible. Brown, indeed, pursues his own peculiar view till he involves the subject in utter confusion. Thus he says‡, "According to the common theory

* When Brown says further (p. 87,) that we can indeed show the image in the dissected eye; but that "it is not in the dissected eye that vision takes place;" it is difficult to see what his drift is. Does he doubt that there is an image formed in the living as completely as in the dissected eye?

* *Lectures*, Vol. II. p. 57. † *Ib.*, Vol. II. p. 89.

[that figure can be perceived by the eye,] a visible sphere is at once to my perception convex and plane; and if the sphere be a large one, it is perceived at once to be a sphere of many feet in diameter, and a plane circular surface of the diameter of a quarter of an inch." It is easy to deduce these and greater absurdities, if we proceed on his strange and baseless supposition that the object and the image on the retina are *both* perceived. But who is conscious of the image on the retina in any other way than as he sees the object by means of it?

Brown seems to have imagined that he was analyzing the perception of figure in the same manner in which Berkeley had analyzed the perception of distance. He ought to have recollected that such an undertaking, to be successful, required him to show *what* elements he analyzed it *into*. Berkeley analyzed the perception of real figure into the interpretation of visible figure according to certain rules which he distinctly stated. Brown analyzes the perception of visible figure into no elements. Berkeley says, that we do not directly perceive distance, but that we perceive something else, from which we infer distance, namely, visible figure and colour, and our own efforts in seeing; Brown says, that we do not see figure, but infer it; what then do we see, which we infer it from? To this he offers no answer. He asserts the seeming perception of visible figure to be a result of "association;"—of "suggestion." But what meaning can we attach to this? Suggestion requires something which suggests; and not a hint is given what it is which suggests position. Association implies two things associated; what is the sensation which we associate with form? What is that visual perception which is not figure, and which we mistake for figure? What perception is it that suggests a square to the eye? What impressions are those which have been associated with a visible triangle, so that the revival of the impressions revives the notion of the triangle? Brown has nowhere pointed out such perceptions and impressions; nor indeed was it possible for him to do so; for the only visual perceptions which he allows to remain, those of colour, most assuredly do not suggest visible figures by their differences; red is not associated with square rather than with round, or with round rather than square. On the contrary, the eye, constructed in a very complex and wonderful manner in order that it may give to us directly the perception of position as well as of colour, has it for one of its prerogatives to give us this information; and the perception of the relative position of each part of the visible boundary of an object constitutes the perception of its apparent figure; which faculty we cannot deny to the eye without rejecting the plain and constant evidence of our senses, making the mechanism of the eye unmeaning, confounding the object with the means of vision, and rendering the mental process of vision utterly unintelligible.

Having sufficiently discussed the processes of perception, I now return to the consideration of the Ideas which these processes assume.

CHAPTER III.

SUCCESSIVE ATTEMPTS AT THE SCIENTIFIC APPLICATION OF THE IDEA OF A MEDIUM.

1. IN what precedes, we have shown by various considerations that we necessarily and universally assume the perception of secondary qualities to take place by means of a medium interjacent between the object and the person perceiving. Perception is affected by various

peculiarities, according to the nature of the quality perceived: but in all cases a medium is equally essential to the process.

This principle, which, as we have seen, is accepted as evident by the common understanding of mankind, is confirmed by all additional reflection and discipline of the mind, and is the foundation of all the theories which have been proposed concerning the processes by which the perception takes place, and concerning the modifications of the qualities thus perceived. The medium, and the mode in which the impression is conveyed through the medium, seem to be different for different qualities; but the existence of the medium leads to certain necessary conditions or alternatives, which have successively made their appearance in science, in the course of the attempts of men to theorize concerning the principal secondary qualities, sound, light, and heat. We must now point out some of the ways, at first imperfect and erroneous, in which the consequences of the fundamental assumption were traced.

2. *Sound.*—In all cases the medium of sensation, whatever it is, is supposed to produce the effect of conveying secondary qualities to our perception by means of its primary qualities. It was conceived to operate by the size, form, and motion of its parts. This is a fundamental principle of the class of sciences of which we have at present to speak.

It was assumed from the first, as we have seen in the passage lately quoted from Aristotle*, that in the conveyance of *sound*, the medium of communication was the air. But although the first theorists were right so far, that circumstance did not prevent their going entirely wrong when they had further to determine the nature of the process. It was conceived by Aristotle

* *Supr.*, p. 282.

that the air acted after the manner of a rigid body;—like a staff, which, receiving an impulse at one end, transmits it to the other. Now this is altogether an erroneous view of the manner in which the air conveys the impulse by which sound is perceived. An approach was made to the true view of this process, by assimilating it to the diffusion of the little circular waves which are produced on the surface of still water when a stone is dropt into it. These little waves begin from the point thus disturbed, and run outwards, expanding on every side, in concentric circles, till they are lost. The propagation of sound through the air from the point where it is produced, was compared by Vitruvius to this diffusion of circular waves in water; and thus the notion of a propagation of impulse by the *waves* of a fluid was introduced, in the place of the former notion of the impulse of an unyielding body.

But though, taking an enlarged view of the nature of the progress of a wave, this is a just representation of the motion of air in conveying sound, we cannot suppose that the process was, at the period of which we speak, rightly understood. For the waves of water were contemplated only as affecting the surface of the water; and as the air has no surface, the communication must take place by means of an internal motion, which can bear only a remote and obscure resemblance to the waves which we see. And even with regard to the waves of water, the mechanism by which they are produced and transferred was not at all understood; so that the comparison employed by Vitruvius must be considered rather as a loose analogy than as an exact scientific explanation.

No correct account of such motions was given, till the formation of the science of Mechanics in modern times had enabled philosophers to understand more distinctly the mode in which motion is propagated through a fluid, and to discern the forces which the process calls into play, so as to continue the motion once begun. Newton introduced into this subject the exact and rigorous conception of an *undulation*, which is the true key to the explanation of impulses conveyed through a fluid.

Even at the present day, the right apprehension of the nature of an undulation transmitted through a fluid is found to be very difficult for all persons except those whose minds have been duly disciplined by mathematical studies. When we see a wave run along the surface of water, we are apt to imagine at first that a portion of the fluid is transferred bodily from one place to another. But with a little consideration we may easily satisfy ourselves that this is not so: for if we look at a field of standing corn, when a breeze blows over it, we see waves like those of water run along its surface. Yet it is clear that in this case the separate stalks of corn only bend backwards and forwards, and no portion of the grain is really conveyed from one part of the field to the other. This is obvious even to popular apprehension. The poet speaks of

> . . . . The rye,
> That stoops its head when whirlwinds rave
> And springs again in eddying wave
> As each wild gust sweeps by.

Each particle of the mass in succession has a small motion backwards and forwards; and by this means a large ridge made by many such particles runs along the mass to any distance. This is the true conception of an undulation in general.

Thus, when an undulation is propagated in a fluid, it is not *matter*, but *form*, which is transmitted from one place to another. The particles along the line of each wave assume a certain arrangement, and this arrangement passes from one part to another, the particles changing their places only within narrow limits, so as to lend themselves successively to the arrangements by which the successive waves, and the intervals between the waves, are formed.

When such an undulation is propagated through air, the wave is composed, not, as in water, of particles which are higher than the rest, but of particles which are closer to each other than the rest. The wave is not a ridge of elevation, but a line of condensation; and as in water we have alternately elevated and depressed lines, we have in air lines alternately condensed and rarefied. And the motion of the particles is not, as in water, up and down, in a direction transverse to that of the wave which runs forwards; in the motion of an undulation through air the motion of each particle is alternately forwards and backwards, while the motion of the undulation is constantly forwards.

This precise and detailed account of the undulatory motion of air by which sound is transmitted was first given by Newton. He further attempted to determine the motions of the separate particles, and to point out the force by which each particle affects the next, so as to continue the progress of the undulation once begun. The motions of each particle must be oscillatory; he assumed the oscillations to be governed by the simplest law of oscillation which had come under the notice of mathematicians, (that of small vibrations of a pendulum;) and he proved that in this manner the forces which are called into play by the contraction and expansion of the parts of the elastic fluid are such as the continuance of the motion requires.

Newton's proof of the exact law of oscillatory motion of the aërial particles was not considered satisfactory by succeeding mathematicians; for it was found that the same result, the development of forces adequate to continue the motion, would follow if any other law of the motion were assumed. Cramer proved this by a sort of *parody* of Newton's proof, in which, by the alteration of a few phrases in this formula of demonstration, it was made to establish an entirely different conclusion.

But the general conception of an undulation as presented by Newton was, as from its manifest mechanical truth it could not fail to be, accepted by all mathematicians; and in proportion as the methods of calculating the motions of fluids were further improved, the necessary consequences of this conception, in the communication of sound through air, were traced by unexceptionable reasoning. This was especially done by Euler and Lagrange, whose memoirs on such motions of fluids are some of the most admirable examples which exist, of refined mathematical methods applied to the solution of difficult mechanical problems.

But the great step in the formation of the theory of sound was undoubtedly that which we have noticed, the introduction of the Conception of an Undulation such as we have attempted to describe it:—a state, condition, or arrangement of the particles of a fluid, which is transferred from one part of space to another by means of small motions of the particles, altogether distinct from the movement of the undulation itself. This is a conception which is not obvious to common apprehension. It appears paradoxical at first sight to speak of a large *wave* (as the tide-wave) running up a river at the rate of twenty miles an hour, while the *stream* of the river is all the while flowing downwards. Yet this is a very common fact. And the conception of such a motion must be fully mastered by all who would reason rightly concerning the transmission of impressions through a medium.

We have described the motion of sound as produced by small motions of the particle forwards and backwards, while the waves, or condensed and rarefied lines, move constantly forwards. It may be asked what right we have to suppose the motion to be of this kind, since when sound is heard, no such motions of the particles of air can be observed, even by refined methods of observation. Thus Bacon declares himself against the hypothesis of such a vibration, since, as he remarks, it cannot be perceived in any visible impression upon the flame of a candle. And to this we reply, that the supposition of this vibration is made in virtue of a principle which is involved in the original assumption of a medium; namely, That *a medium, in conveying secondary qualities, operates by means of its primary qualities*, the bulk, figure, motion, and other mechanical properties of its parts. This is an Axiom belonging to the Idea of a Medium. In virtue of this axiom it is demonstrable that the motion of the air, when any how disturbed, must be such as is supposed in our acoustical reasonings. For the elasticity of the parts of the air, called into play by its expansion and contraction, lead, by a mechanical necessity, to such a motion as we have described. We may add that, by proper contrivances, this motion may be made perceptible in its visible effects. Thus the theory of sound, as an impression conveyed through air, is established upon evident general principles, although the mathematical calculations which are requisite to investigate its consequences are, some of them, of a very recondite kind.

3. *Light.*—The early attempts to explain vision represented it as performed by means of material rays proceeding *from* the eye, by the help of which the eye felt out the form and other visible qualities of an object, as a blind man might do with his staff. But this opinion could not keep its ground long: for it did not even

explain the fact that light is necessary to vision. Light as a peculiar medium was next assumed as the machinery of vision; but the mode in which the impression was conveyed through the medium was left undetermined, and no advance was made towards sound theory, on that subject, by the ancients.

In modern times, when the prevalent philosophy began to assume a mechanical turn (as in the theories of Descartes), light was conceived to be a material substance which is emitted from luminous bodies, and which is also conveyed from all bodies to the eye, so as to render them visible. The various changes of direction by which the rays of light are affected, (reflexion, refraction, &c.,) Descartes explained, by considering the particles of light as small globules, which change their direction when they impinge upon other bodies, according to the laws of mechanics. Newton, with a much more profound knowledge of mechanics than Descartes possessed, adopted, in the most mature of his speculations, nearly the same view of the nature of light; and endeavoured to show that reflexion, refraction, and other properties of light, might be explained as the effects which certain forces, emanating from the particles of bodies, produce upon the luminiferous globules.

But though some of the properties of light could thus be accounted for by the assumption of particles emitted from luminous bodies, and reflected or refracted by forces, other properties came into view which would not admit of the same explanation. The phenomena of *diffraction* (the fringes which accompany shadows) could never be truly represented by such an hypothesis, in spite of many attempts which were made. And the *colours of thin plates*, which show the rays of light to be affected by an alternation of two different conditions at small intervals along their length, led Newton himself to incline, often and strongly, to some hypothesis of undulation. The *double refraction* of Iceland spar, a phenomenon in itself very complex, could, it was found by Huyghens, be expressed with great simplicity by a certain hypothesis of undulations.

Two hypotheses of the nature of the luminiferous medium were thus brought under consideration; the one representing Light as Matter emitted from the luminous object, the other, as Undulations propagated through a fluid. These two hypotheses remained in presence of each other during the whole of the last century, neither of them gaining any material advantage over the other, though the greater part of mathematicians, following Newton, embraced the emission theory. But at the beginning of the present century, an additional class of phenomena, those of the *interference* of two rays of light, were brought under consideration by Dr. Young; and these phenomena were strongly in favour of the undulatory theory, while they were irreconcilable with the hypothesis of emission. If it had not been for the original bias of Newton and his school to the other side, there can be little doubt that from this period light as well as sound would have been supposed to be propagated by undulations; although in this case it was necessary to assume as the vehicle of such undulations a special medium or *ether*. Several points of the phenomena of vision no doubt remained unexplained by the undulatory theory, as absorption, and the natural colours of bodies; but such facts, though they did not confirm, did not evidently contradict the theory of a luminiferous ether; and the facts which such a theory did explain, it explained with singular happiness and accuracy.

But before this undulatory theory could be generally accepted, it was presented in an entirely new point of view by being combined with the facts of *polarization*.

The general idea of polarization must be illustrated hereafter; but we may here remark that Young and Fresnel, who had adopted the undulatory theory, after being embarrassed for some time by the new facts which were thus presented to their notice, at last saw that these facts might be explained by conceiving the vibrations to be transverse to the ray, the motions of the particles being not backwards and forwards in the line in which the impulse travels, but to the right and left of that line. This conception of *transverse vibrations*, though quite unforeseen, had nothing in it which was at all difficult to reconcile with the general notion of an undulation. We have described an undulation, or wave, as a certain condition or arrangement of the particles of the fluid successively transferred from one part of space to another: and it is easily conceivable that this arrangement or wave may be produced by a lateral transfer of the particles from their quiescent positions. This conception of transverse vibrations being accepted, it was found that the explanation of the phenomena of polarization and of those of interference led to the same theory with a correspondence truly wonderful; and this coincidence in the views, collected from two quite distinct classes of phenomena, was justly considered as an almost demonstrative evidence of the truth of this undulatory theory.

It remained to be considered whether the doctrine of transverse vibrations in a fluid could be reconciled with the principles of mechanics. And it was found that by making certain suppositions, in which no inherent improbability existed, the hypothesis of transverse vibrations would explain the laws, both of interference and of polarization of light, in air and in crystals of all kinds, with a surprizing fertility and fidelity.

Thus the undulatory theory of light, like the undulatory theory of sound, is recommended by its conformity to the fundamental principle of the Secondary Mechanical Sciences, that the medium must be supposed to transmit its peculiar impulses according to the laws of mechanics. Although no one had previously dreamt of qualities being conveyed through a medium by such a process, yet when it is once suggested as the only mode of explaining some of the phenomena, there is nothing to prevent our accepting it entirely, as a satisfactory theory for all the known laws of light.

4. *Heat*.—With regard to heat as with regard to light, a fluid medium was necessarily assumed as the vehicle of the property. During the last century, this medium was supposed to be an emitted fluid. And many of the ascertained Laws of Heat, those which prevail with regard to its radiation more especially, were well explained by this hypothesis*. Other effects of heat, however, as for instance *latent heat*†, and the change of *consistence* of bodies‡, were not satisfactorily brought into connexion with the hypothesis; while *conduction*§, which at first did not appear to result from the fundamental assumption, was to a certain extent explained as internal radiation.

But it was by no means clear that an undulatory theory of heat might not be made to explain these phenomena equally well. Several philosophers inclined to such a theory; and finally, Ampère showed that the doctrine that the heat of a body consists in the undulations of its particles propagated by means of the undulations of a medium, might be so adjusted as to explain all which the theory of emission could explain, and moreover to account for facts and laws which were out of

* See the Account of the Theory of Exchanges, *Hist. Ind. Sci.*, B. x. c. i. sect. 2.

† *Ib.*, c. ii. sect. 3.

‡ *Ib.*, c. ii. sect. 2.

§ *Ib.*, c. i. sect. 7.

the reach of that theory. About the same time it was discovered by Prof. Forbes and M. Nobili that radiant heat is, under certain circumstances, polarized. Now polarization had been most satisfactorily explained by means of transverse undulations in the case of light; while all attempts to modify the emission theory so as to include polarization in it, had been found ineffectual. Hence this discovery was justly considered as lending great countenance to the opinion that heat consists in the vibrations of its proper medium.

But what is this medium? Is it the same by which the impressions of light are conveyed? This is a difficult question; or rather it is one which we cannot at present hope to answer with certainty. No doubt the connexion between light and heat is so intimate and constant, that we can hardly refrain from considering them as affections of the same medium. But instead of attempting to erect our systems on such loose and general views of connexion, it is rather the business of the philosophers of the present day to determine the laws of the operation of heat, and its real relation to light, in order that we may afterwards be able to connect the theories of the two qualities. Perhaps in a more advanced state of our knowledge we may be able to state it as an axiom, that two secondary qualities, which are intimately connected in their causes and effects, must be affections of the same medium. But at present it does not appear safe to proceed upon such a principle, although many writers, in their speculations both concerning light and heat, and concerning other properties, have not hesitated to do so.

Some other consequences follow from the Idea of a Medium which must be the subject of another chapter.

Chapter IV.

OF THE MEASURE OF SECONDARY QUALITIES.

Sect. I.—*Scales of Qualities in general.*

The ultimate object of our investigation in each of the Secondary Mechanical Sciences, is the nature of the processes by which the special impressions of sound, light, and heat, are conveyed, and the modifications of which these processes are susceptible. And of this investigation, as we have seen, the necessary basis is the principle, that these impressions are transmitted by means of a medium. But before we arrive at this ultimate object, we may find it necessary to occupy ourselves with several intermediate objects: before we discover the *cause*, it may be necessary to determine the *laws* of the phenomena. Even if we cannot immediately ascertain the mechanism of light or heat, it may still be interesting and important to arrange and measure the effects which we observe.

The idea of a medium affects our proceeding in this research also. We cannot measure secondary qualities in the same manner in which we measure primary qualities, by a mere addition of parts. There is this leading and remarkable difference, that while both classes of qualities are susceptible of changes of magnitude, primary qualities increase by addition of *extension*, secondary, by augmentation of *intensity*. A space is doubled when another equal space is placed by its side; one weight joined to another makes up the sum of the two. But when one degree of warmth is combined with another, or one shade of red colour with another, we cannot in like manner talk of the *sum*. The component parts do not evidently retain their separate existence; we cannot separate a strong green colour into two weaker ones, as we can separate a large force into two smaller. The increase is absorbed into the previous amount, and is no longer in evidence as a part of the whole. And this is the difference which has given birth to the two words *extended*, and *intense*. That is extended which has "partes extra partes," parts outside of parts: that is intense which becomes stronger by some indirect and unapparent increase of agency, like the stretching of the internal springs of a machine, as the term *intense* implies. Extended magnitudes can at will be resolved into the parts of which they were originally composed, or any other which the nature of their extension admits; their proportion is apparent; they are directly and at once subject to the relations of number. Intensive magnitudes cannot be resolved into smaller magnitudes; we can see that they differ, but we cannot tell in what proportion; we have no direct measure of their quantity. How many times hotter than blood is boiling water? The answer cannot be given by the aid of our feelings of heat alone.

The difference, as we have said, is connected with the fundamental principle that we do not perceive secondary qualities directly, but through a medium. We have no natural apprehension of light, or sound, or heat, as they exist in the bodies from which they proceed, but only as they affect our organs. We can only measure them, therefore, by some *Scale* supplied by their effects. And thus while extended magnitudes, as space, time, are measurable directly and of themselves; intensive magnitudes, as brightness, loudness, heat, are measurable only by artificial means and conventional scales. Space, time, measure themselves: the repetition of a smaller space, or time, while it composes a larger one, measures it. But for light and heat we must have Photometers and Thermometers, which measure something which is assumed to be an indication of the quality in question. In one case, the mode of applying the measure, and the meaning of the number resulting, are seen by intuition; in the other, they are consequences of assumption and reasoning. In the one case, they are *Units*, of which the extension is made up; in the other, they are *Degrees* by which the intensity ascends.

2. When we discover any property in a sensible quality, which at once refers us to number or space, we readily take this property as a measure; and thus we make a transition from quality to quantity. Thus Ptolemy in the third chapter of the First Book of his *Harmonics* begins thus: "As to the differences which exist in sounds both in *quality* and in *quantity*, if we consider that difference which refers to the acuteness and graveness, we cannot at once tell to which of the above two classes it belongs, till we have considered the causes of such symptoms." But at the end of the chapter, having satisfied himself that grave sounds result from the magnitude of the string or pipe, other things being equal, he infers, "Thus the difference of acute and grave appears to be a difference of *quantity*."

In the same manner, in order to form Secondary Mechanical Sciences respecting any of the other properties of bodies, we must reduce these properties to a dependence upon quantity, and thus make them subject to measurement. We cannot obtain any sciential truths respecting the comparison of sensible qualities, till we have discovered measures and scales of the qualities which we have to consider; and accordingly, some of the most important steps in such sciences have been the establishment of such measures and scales, and the invention of the requisite instruments.

The formation of the mathematical sciences which rest upon the measures of the intensity of sensible qualities took place mainly in the course of the last century. Perhaps we may consider Lambert, a mathematician who resided in Switzerland, and published about 1750, as the person who first clearly felt the importance of establishing such sciences. His Photometry, Pyrometry, Hygrometry, are examples of the systematic reduction of sensible qualities (light, heat, moisture) to modes of numerical measurement.

We now proceed to speak of such modes of measurement with regard to the most obvious properties of bodies.

Sect. II.—*The Musical Scale.*

3. The establishment of the *Harmonic Canon*, that is, of a Scale and Measure of the musical place of notes, in the relation of *high* and *low*, was the first step in the science of Harmonics. The perception of the differences and relations of musical sounds is the office of the sense of hearing; but these relations are fixed, and rendered accurately recognizable by artificial means. "Indeed, in all the senses," as Ptolemy truly says in the opening of his Harmonics, "the sense discovers what is approximately true, and receives accuracy from another quarter: the reason receives the approximately-true from another quarter, and discovers the accurate truth." We can have no measures of sensible qualities which do not ultimately refer to the sense;—whether they do this immediately, as when we refer Colours to an assumed Standard; or mediately, as when we measure Heat by Expansion, having previously found by an appeal to sense that the expansion increases with the heat. Such relations of sensible qualities cannot be described in words, and can only be apprehended by their appropriate faculty. The faculty by which the relations of sounds

are apprehended is a *musical ear* in the largest acceptation of the term. In this signification the faculty is nearly universal among men; for all persons have musical ears sufficiently delicate to understand and to imitate the modulations corresponding to various emotions in speaking; which modulations depend upon the succession of acuter and graver tones. These are the relations now spoken of, and these are plainly perceived by persons who have very imperfect musical ears, according to the common use of the phrase. But the relations of tones which occur in speaking are somewhat indefinite; and in forming that musical scale which is the basis of our science upon the subject, we take the most definite and marked of such relations of notes; such as occur, not in speaking but in singing. Those musical relations of two sounds which we call the *octave*, the *fifth*, the *fourth*, the *third*, are recognized after a short familiarity with them. These *chords* or *intervals* are perceived to have each a peculiar character, which separates them from the relations of two sounds taken at random, and makes it easy to know them when sung or played on an instrument; and for most persons, not difficult to sing the sounds in succession exactly, or nearly correct. These musical relations, or *concords*, then, are the groundwork of our musical standard. But how are we to name these indescribable sensible characters? how to refer, with unerring accuracy, to a type which exists only in our own perceptions? We must have for this purpose a *Scale* and a *Standard*.

The Musical Scale is a series of eight notes, ascending by certain steps from the first or key-note to the octave above it, each of the notes being fixed by such distinguishable musical relations as we have spoken of above. We may call these notes C, D, E, F, G, A, B, *c*; and we may then say that G is determined by its being a fifth above C; D by its being a fourth below G; E by its being a third above C; and similarly of the rest. It will be recollected that the terms a *fifth*, a *fourth*, a *third*, have hitherto been introduced as expressing certain simple and indescribable musical relations among sounds, which might have been indicated by any other names. Thus we might call the fifth the *dominant*, and the fourth the *subdominant*, as is done in one part of musical science. But the names we have used, which are the common ones, are in fact derived from the number of notes which these intervals include in the scale obtained in the above manner. The notes C, D, E, F, G, being five, the interval from C to G is a fifth, and so of the rest. The fixation of this scale gave the means of describing exactly any note which occurs in the scale, and the method is easily applicable to notes above and below this range; for in a series of sounds higher or lower by an octave than this standard series, the ear discovers a recurrence of the same relations so exact, that a person may sometimes imagine he is producing the same notes as another when he is singing the same air an octave higher. Hence the next eight notes may be conveniently denoted by a repetition of the same letters, as the first; thus, C, D, E, F, G, A, B, *c*, *d*, *e*, *f*, *g*, *a*, *b*; and it is easy to devise a continuation of such cycles. And other admissible notes are designated by a further modification of the standard ones, as by making each note *flat* or *sharp*; which modification it is not necessary here to consider, since our object is only to show how a standard is attainable, and how it serves the ends of science.

We may observe, however, that the above is not an exact account of the first, or early Greek scale; for this scale was founded on a primary division of the interval of two octaves (the extreme range which it admitted) into five *tetrachords*, each tetrachord including the interval of a fourth. All the notes of this series had different names borrowed from this division*; thus *mese* was the middle or key-note; the note below it was *lichanos mesôn*, the next below was *parypate mesôn*, the next lower, *hypate mesôn*. The fifth above *mese* was *nete diazeugmenôn*, the octave was *nete hyperbolæôn*.

4. But supposing a complete system of such denominations established, how could it be with certainty and rigour applied? The human ear is fallible, the organs of voice imperfectly obedient; if this were not so, there would be no such thing as a *good* ear or a *good* voice. What means can be devised of finding at will a *perfect* concord, a fifth or a fourth? Or supposing such concords fixed by an acknowledged authority, how can they be referred to, and the authority adduced? How can we enact a Standard of sounds?

A Standard was discovered in the *Monochord*. A musical string properly stretched, may be made to produce different notes, in proportion as we intercept a longer or shorter portion, and make this portion vibrate. The relation of the length of the strings which thus sound the two notes G and C is fixed and constant, and the same is true of all other notes. Hence the musical interval of any notes of which we know the places in the musical scale, may be reproduced by measuring the lengths of string which are known to give them. If C be of the length 180, D is 169, E is 144, F is 135, G is 120; and thus the musical relations are reduced to numerical relations, and the monochord is a complete and perfect *Tonometer*.

We have here taken the length of the string as the measure of the tone: but we may observe that there is in us a necessary tendency to assume that the ground

* Burney's *History of Music*, Vol. I. p. 28.

of this measure is to be sought in some ulterior cause; and when we consider the matter further, we find this cause in the frequency of these vibrations of the string. The truth that the same note must result from the same frequency of vibration is readily assented to on a slight suggestion of experience. Thus Mersenne*, when he undertakes to determine the frequency of vibrations of a given sound, says "Supponendum est quoscunque nervos et quaslibet chordas unisonum facientes eundem efficere numerum recursuum eodem vel equali tempore, quod perpetuâ constat experientiâ." And he proceeds to apply it to cases where experience could not verify this assertion, or at least had not verified it, as to that of pipes.

The pursuit of these numerical relations of tones forms the science of Harmonics; of which here we do not pretend to give an account, but only to show, how the invention of a Scale and Nomenclature, a Standard and Measure of the tone of sounds, is its necessary basis. We will therefore now proceed to speak of another subject; *colour.*

SECT. III.—*Scales of Colour.*

5. *The Prismatic Scale of Colour.*—A SCALE of Colour must depend originally upon differences discernible by the eye, as a scale of notes depends on differences perceived by the ear. In one respect the difficulty is greater in the case of the visible qualities, for there are no relations of colour which the eye peculiarly singles out and distinguishes, as the ear selects and distinguishes an octave or a fifth. Hence we are compelled to take an arbitrary scale; and we have to find one which is fixed, and which includes a proper collection of colours. The prismatic spectrum, or coloured image produced

* *Harmonia*, Lib. II. Prop. 19.

when a small beam of light passes obliquely through any transparent surface (as the surface of a prism of glass,) offers an obvious Standard as far as it is applicable. Accordingly colours have, for various purposes, been designated by their place in the spectrum ever since the time of Newton; and we have thus a means of referring to such colours as are included in the series *red, orange, yellow, green, blue, violet, indigo,* and the intermediate tints.

But this scale is not capable of numerical precision. If the spectrum could be exactly defined as to its extremities, and if these colours occupied always the same proportional part of it, we might describe any colour in the above series by the measure of its position. But the fact is otherwise. The spectrum is too indefinite in its boundaries to afford any distinct point from which we may commence our measures; and moreover the spectra produced by different transparent bodies differ from each other. Newton had supposed that the spectrum and its parts were the same, so long as the refraction was the same; but his successors discovered that, with the same amount of refraction in different kinds of glass, there are different magnitudes of the spectrum; and what is still worse with reference to our present purpose, that the spectra from different glasses have the colours distributed in different proportions. In order, therefore, to make the spectrum the scale of colour, we must assume some fixed substance; for instance, we may take water, and thus a series approaching to the colours of the rainbow will be our standard. But we should still have an extreme difficulty in applying such a rule. The distinctions of colour which the terms of common language express, are not used with perfect unanimity or with rigorous precision. What one person calls *bluish green* another calls *greenish blue.* Nobody can say what is the precise boundary between red and orange. Thus the prismatic scale of colour was incapable of mathematical exactness, and this inconvenience was felt up to our own times.

But this difficulty was removed by a curious discovery of Wollaston and Fraunhofer; who found that there are, in the solar spectrum, certain fine black Lines which occupy a definite place in the series of colours, and can be observed with perfect precision. We have now no uncertainty as to what coloured light we are speaking of, when we describe it as that part of the spectrum in which Fraunhofer's Line C or D occurs. And thus, by this discovery, the prismatic spectrum of sunlight became, for certain purposes, an exact *Chromatometer*.

6. *Newton's Scale of Colours.*—Still, such a standard, though definite, is arbitrary and seemingly anomalous. The lines A, B, C, D, &c., of Fraunhofer's spectrum are distributed without any apparent order or law; and we do not, in this way, obtain numerical measures, which is what, in all cases, we desire to have. Another discovery of Newton, however, gives us a spectrum containing the same colours as the prismatic spectrum, but produced in another way, so that the colours have a numerical relation. I speak of the laws of the *colours of thin plates*. The little rainbows which we sometimes see in the cracks of broken glass are governed by fixed and simple laws. The kind of colour produced at any point depends on the thickness of the thin plate of air included in the fissure. If the thickness be eight-millionths of an inch, the colour is orange, if fifteen-millionths of an inch, we have green, and so on; and thus these numbers which succeed each other in a regular order from red to indigo, give a numerical measure of each colour; which measure, when we pursue the subject, we find is one of the

bases of all optical theory. The series of colours obtained from plates of air of gradually increasing thickness is called *Newton's Scale of Colours;* but we may observe that this is not precisely what we are here speaking of, a scale of *simple* colours; it is a series produced by certain combinations, resulting from the repetition of the first spectrum, and is mainly useful as a standard for similar phenomena, and not for colour in general. The real scale of colour is to be found, as we have said, in the numbers which express the thickness of the producing film;—in the length of a *fit* in Newton's phraseology, or the length of an undulation in the modern theory.

7. *Scales of Impure Colours.*—The standards just spoken of include (mainly at least) only pure and simple colours; and however complete they may be for certain objects of the science of optics, they are insufficient for other purposes. They do not enable us to put in their place mixed and impure colours. And there is, in the case of colour, a difficulty already noticed, which does not occur in the case of sound; two notes, when sounded together, are not necessarily heard as one; they are recognized as still two, and as forming a concord or a discord. But two colours form a single colour; and the eye cannot, in any way, distinguish between a green compounded of blue and yellow, and the simple, undecomposable green of the spectrum. By composition of three or more colours, innumerable new colours may be generated which form no part of the prismatic series; and by such compositions is woven the infinitely varied web of colour which forms the clothing of nature. How are we to classify and arrange all the possible colours of objects, so that each shall have a place and name? How shall we find a *chromatometer* for impure as well as for pure colour?

Though no optical investigations have depended on a scale of impure colours, such a scale has been wanted and invented for other purposes; for instance, in order to identify and describe objects of natural history. Not to speak of earlier essays, we may notice Werner's Nomenclature of Colours, devised for the purpose of describing minerals. This scale of colour was far superior to any which had previously been promulgated. It was, indeed, arbitrary in the selection of its degrees, and in a great measure in their arrangement; and the colours were described by the usual terms, though generally with some added distinction; as *blackish green*, *bluish green*, *apple-green*, *emerald-green*. But the great merit of the scale was its giving a *fixed* conventional meaning to these terms, so that they lost much of their usual vagueness. Thus *apple-green* did not mean the colour of any green apple casually taken; but a certain definite colour which the student was to bear in mind, whether or not he had ever seen an apple of that exact hue. The words were not a description, but a *record* of the colour: the memory was to retain a *sensation*, not a name.

The imperfection of the system (arising from its arbitrary form) was its incompleteness: however well it served for the reference of the colours which it did contain, it was applicable to no others; and thus, though Werner's enumeration extended to more than a hundred colours, there occur in nature a still greater number which cannot be exactly described by means of it.

In such cases the unclassed colour is, by the Wernerians, defined by stating it as intermediate between two others: thus we have an object described as *between emerald-green and grass-green.* The eye is capable of perceiving a gradation from one colour to another; such as may be produced by a gradual mixture in various ways. And if we image to ourselves such a mixture, we can compare with it a given colour. But in employing this method we have nothing to tell us in what part of the scale we must seek for an approximation to our unclassed colour. We have no rule for discovering where we are to look for the boundaries of the definition of a colour which the Wernerian series does not supply. For it is not always between contiguous members of the series that the undescribed colour is found. If we place emerald-green between apple-green and grass-green, we may yet have a colour intermediate between emerald-green and leek-green; and, in fact, the Wernerian series of colours is destitute of a principle of self-arrangement and gradation; and is thus necessarily and incurably imperfect.

8. We should have a complete Scale of Colours, if we could form a series including all colours, and arranged so that each colour was intermediate in its tint between the adjacent terms of the series; for then, whether we took many or few of the steps of the series for our standard terms, the rest could be supplied by the law of continuity; and any given colour would either correspond to one of the steps of our scale or fall between two intermediate ones. The invention of a Chromatometer for Impure Colours, therefore, requires that we should be able to form all possible colours by such intermediation in a systematic manner; that is, by the mixture or combination of certain elementary colours according to a simple rule: and we are led to ask whether such a process has been shown to be possible.

The colours of the prismatic spectrum obviously do form a continuous series; green is intermediate between its neighbours yellow and blue, orange between red and yellow; and if we suppose the two ends of the spectrum bent round to meet each other, so that the arrangement of the colours may be circular, the violet and indigo will find their appropriate place between the blue and red. And all the interjacent tints of the spectrum, as well as the ones thus named, will result from such an arrangement. Thus all the pure colours are produced by combinations two and two of three primary colours, red, yellow, and blue; and the question suggests itself whether these three are not really the only primary colours, and whether all the impure colours do not arise from mixtures of the three in various proportions. There are various modes in which this suggestion may be applied to the construction of a scale of colours; but the simplest, and the one which appears really to verify the conjecture that all possible colours may be so exhibited, is the following. A certain combination of red, yellow, and blue, will produce black, or pure grey, and when diluted, will give all the shades of grey which intervene between black and white. By adding various shades of grey, then, to pure colours, we may obtain all the possible ternary combinations of red, yellow, and blue; and in this way it is found that we exhaust the range of colours. Thus the circle of pure colours of which we have spoken may be accompanied by several other circles, in which these colours are tinged with a less or greater shade of grey; and in this manner it is found that we have a perfect chromatometer; every possible colour being exhibited either exactly or by means of approximate and contiguous limits. The arrangement of colours has been brought into this final and complete form by M. Merimée, whose Chromatic Scale is published by M. Mirbel in his *Elements of Botany*. We may observe that such a standard affords us a numerical exponent for every colour by means of the proportions of the three primary colours which compose it; or, expressing the same result otherwise, by means of the pure colour which is involved, and the proportion of grey by which it is rendered impure. In such a scale the fundamental elements would be the precise tints of red, yellow, and blue which are found or assumed to be primary; the numerical exponents of each colour would depend upon the arbitrary number of degrees which we interpose between each two primary colours; and between each pure colour and absolute blackness. No such numerical scale has, however, as yet, obtained general acceptation*.

SECT. IV.—*Scales of Light.*

9. *Photometer.*—ANOTHER instrument much needed in optical researches is a *Photometer*, a measure of the intensity of light. In this case, also, the organ of sense, the eye, is the ultimate judge; nor has any effect of light, as light, yet been discovered which we can substitute for such a judgment. All instruments, such as that of Leslie, which employ the heating effect of light, or at least all that have hitherto been proposed, are inadmissible as photometers. But though the eye can

* The reference to *Fraunhofer's Lines*, as a means of determining the place of a colour in the prismatic series, has been objected to, because, as is asserted, the colours which are in the neighbourhood of each line vary with the position of the sun, state of the atmosphere and the like. It is very evident that coloured light refracted by the prism will not give the same spectrum as white light. The spectrum given by white light is of course the one here meant. It is an usual practice of optical experimenters to refer to the colours of such a spectrum, defining them by Fraunhófer's Lines.

I do not know whether it needs explanation that the "first spectrum" in Newton's rings is a ring of the prismatic colours.

I have not had an opportunity of consulting Lambert's *Photometria, sive de mensura et gradibus luminis, colorum, et umbræ*, published in 1760, nor Mayer's *Commentatio de Affinitate Colorum*, (1758,) in which, I believe, he describes a chromatometer. The present work is not intended to be complete as a history; and I hope I have given sufficient historical detail to answer its philosophical purpose.

judge of two surfaces illuminated by light of the same colour, and can determine when they are equally bright, or which is the brighter, the eye can by no means decide at sight the proportion of illumination. How much in such judgments we are affected by contrast, is easily seen when we consider how different is the apparent brightness of the moon at mid-day and at midnight, though the light which we receive from her is, in fact, the same at both periods. In order to apply a scale in this case, we must take advantage of the known numerical relations of light. We are certain that if all other illumination be excluded, two equal luminaries, under the same circumstances, will produce an illumination twice as great as one does; and we can easily prove, from mathematical considerations, that if light be not enfeebled by the medium through which it passes, the illumination on a given surface will diminish as the square of the distance of the luminary increases. If, therefore, we can by taking a fraction thus known of the illuminating effect of one luminary, make it equal to the total effect of another, of which equality the eye is a competent judge, we compare the effects of the two luminaries. In order to make this comparison we may, with Rumford, look at the shadows of the same object made by the two lights, or with Ritchie, we may view the brightness produced on two contiguous surfaces, framing an apparatus so that the equality may be brought about by proper adjustment; and thus a measure will become practicable. Or we may employ other methods as was done by Wollaston*, who reduced the light of the sun by observing it as reflected from a bright globule, and thus found the light of the sun to be 10,000,000,000 times that of Sirius, the brightest fixed star. All these methods are inaccurate, even as methods of comparison; and do not

* *Phil. Trans.*, 1829, p. 19.

offer any fixed or convenient numerical standard; but none better have yet been devised*.

10. *Cyanometer.*—As we thus measure the brightness of a colourless light, we may measure the intensity of any particular colour in the same way; that is, by applying a standard exhibiting the gradations of the colour in question till we find a shade which is seen to agree with the proposed object. Such an instrument we have in the *Cyanometer*, which was invented by Saussure for the purpose of measuring the intensity of the blue colour of the sky. We may introduce into such an instrument a numerical scale, but the numbers in such a scale will be altogether arbitrary.

SECT. V.—*Scales of Heat.*

11. *Thermometers.*—WHEN we proceed to the sensation of heat, and seek a measure of that quality, we find, at first sight, new difficulties. Our sensations of this kind are more fluctuating than those of vision; for we know that the same object may feel warm to one hand and cold to another at the same instant, if the hands have been previously cooled and warmed respectively. Nor can we obtain here, as in the case of light, self-evident numerical relations of the heat communicated in given circumstances; for we know that the effect so produced will depend on the warmth of the body to be heated, as well as on that of the source of heat; the summer sun, which warms our bodies, will not augment the heat of a red-hot iron. The cause of the difference of these cases is, that bodies do not receive the whole of their heat, as they receive the whole of their light, from the immediate influence of obvious external

* Improved Photometers have been devised by Professor Wheatstone, Professor Potter, and Professor Steinheil; but they depend upon principles similar to those mentioned in the text.

agents. There is no readily-discovered absolute cold, corresponding to the absolute darkness which we can easily produce or imagine. Hence we should be greatly at a loss to devise a *Thermometer*, if we did not find an indirect effect of heat sufficiently constant and measurable to answer this purpose. We discover, however, such an effect in the *expansion* of bodies by the effect of heat.

12. Many obvious phenomena show that air, under given circumstances, expands by the effect of heat; the same is seen to be true of liquids, as of water, and spirit of wine; and the property is found to belong also to the metallic fluid, quicksilver. A more careful examination showed that the increase of bulk in some of these bodies by increase of heat was a fact of a nature sufficiently constant and regular to afford a means of measuring that previously intangible quality; and the Thermometer was invented. There were, however, many difficulties to overcome, and many points to settle, before this instrument was fit for the purposes of science.

An explanation of the way in which this was done necessarily includes an important chapter of the history of Thermotics. We must now, therefore, briefly notice historically the progress of the Thermometer. The leading steps of this progress, after the first invention of the instrument, were—The establishment of *fixed points* in the thermometric scale—The *comparison of the scales* of different substances—And the reconcilement of these differences by some method of interpreting them as indications of the absolute *quantity of heat.*

13. It would occupy too much space to give in detail the history of the successive attempts by which these steps were effected. A thermometer is described by Bacon under the title *Vitrum Calendare;* this was an air thermometer. Newton used a thermometer of linseed oil, and he perceived that the first step requisite to give

value to such an instrument was to fix its scale; accordingly he proposed his *Scala Graduum Caloris**. But when thermometers of different liquids were compared, it appeared, from their discrepancies, that this fixation of the scale of heat was more difficult than had been supposed. It was, however, effected. Newton had taken freezing water, or rather thawing snow, as the zero of his scale, which is really a fixed point; Halley and Amontons discovered (in 1693 and 1702) that the heat of boiling water is another fixed point; and Daniel Gabriel Fahrenheit, of Dantzig, by carefully applying these two standard points, produced, about 1714, thermometers, which were constantly consistent with each other. This result was much admired at the time, and was, in fact, the solution of the problem just stated, the *fixation of the scale of heat*.

14. But the scale thus obtained is a conventional not a natural scale. It depends upon the fluid employed for the thermometer. The progress of expansion from the heat of freezing to that of boiling water is different for mercury, oil, water, spirit of wine, air. A degree of heat which is half-way between these two standard points according to a mercurial thermometer, will be below the half-way point in a spirit thermometer, and above it in an air thermometer. Each liquid has its own *march* in the course of its expansion. Deluc and others compared the marches of various liquids, and thus made what we may call a *concordance* of thermometers of various kinds.

15. Here the question further occurs: Is there not some *natural measure* of the degrees of heat? It appears certain that there must be such a measure, and that by means of it all the scales of different liquids must be reconciled. Yet this does not seem to have

* *Phil. Trans.*, 1701.

occurred at once to men's minds. Deluc, in speaking of the researches which we have just mentioned, says*, "When I undertook these experiments, it never once came into my thoughts that they could conduct me with any probability to a table of real degrees of heat. But hope grows with success, and desire with hope." Accordingly he pursued this inquiry for a long course of years.

What are the principles by which we are to be guided to the true measure of heat? Here, as in all the sciences of this class, we have the general principle, that the secondary quality, heat, must be supposed to be perceived in some way by a material medium or fluid. If we take that which is, perhaps, the simplest form of this hypothesis, that the heat depends upon the *quantity* of this fluid, or *caloric*, which is present, we shall find that we are led to propositions which may serve as a foundation for a natural measure of heat. The *Method of Mixtures* is one example of such a result. If we mix together two pints of water, one hot and one cold, is it not manifest that the temperature of the mixture must be midway between the two? Each of the two portions brings with it its own heat. The whole heat, or caloric, of the mixture is the sum of the two; and the heat of each half must be the half of this sum, and therefore its temperature must be intermediate between the temperatures of the equal portions which were mixed. Deluc made experiments founded upon this principle, and was led by them to conclude that "the dilatations of mercury follow an accelerated march for successive equal augmentations of heat."

But there are various circumstances which prevent this method of mixtures from being so satisfactory as at first sight it seems to promise to be. The different *capacities for heat* of different substances, and even of

* *Modif. de l'Atmosph.*, 1782, p. 303.

the same substance at different temperatures, introduce much difficulty into the experiments; and this path of inquiry has not yet led to a satisfactory result.

16. Another mode of inquiring into the natural measure of heat is to seek it by researches on the *law of cooling* of hot bodies. If we assume that the process of cooling of hot bodies consists in a certain material heat flying off, we may, by means of certain probable hypotheses, determine mathematically the law according to which the temperature decreases as time goes on; and we may assume that to be the true measure of temperature which gives to the experimental law of cooling the most simple and probable form.

It appears evident from the most obvious conceptions which we can form of the manner in which a body parts with its superabundant heat, that the hotter a body is, the faster it cools; though it is not clear without experiment, by what law the rate of cooling will depend upon the heat of the body. Newton took for granted the most simple and seemingly natural law of this dependence: he supposed the rate of cooling to be *proportional* to the temperature, and from this supposition he could deduce the temperature of a hot iron, calculating from the original temperature and the time during which it had been cooling. By calculation founded on such a basis, he graduated his thermometer.

17. But a little further consideration showed that the rate of cooling of hot bodies depended upon the temperature of the surrounding bodies, as well as upon its own temperature. Prevost's *Theory of Exchanges** was propounded with a view of explaining this dependence, and was generally accepted. According to this theory, all bodies radiate heat to one another, and are thus constantly giving and receiving heat; and a body

* *Recherches sur la Chaleur*, 1791. *Hist. Ind. Sci.*, B. x. c. i. sect. 2.

which is hotter than surrounding bodies, cools itself, and warms the surrounding bodies, by an exchange of heat for heat, in which they are the gainers. Hence if θ be the temperature of the bodies, or of the space, by which the hot body is surrounded, and $\theta + t$ the temperature of the hot body, the rate of cooling will depend upon the excess of the radiation for a temperature $\theta + t$, above the radiation for a temperature θ.

Accordingly, in the admirable researches of MM. Dulong and Petit upon the cooling of bodies, it was assumed that the rate of cooling of the hot body was represented by the excess of $F(\theta + t)$ above $F(\theta)$; where F represented some mathematical *function*, that is, some expression obtained by arithmetical operations from the temperatures $\theta + t$ and θ; although what these operations are to be, was left undecided, and was in fact determined by the experiments. And the result of their investigations was, that the function is of this kind:—when the temperature increases by equal intervals, the function increases in a continued geometric proportion*. This was, in fact, the same law which had been assumed by Newton and others, with this difference, that they had neglected the term which depends upon the temperature of the surrounding space.

18. This law falls in so well with the best conceptions we can form of the mechanism of cooling upon the supposition of a radiant fluid caloric, that it gives great probability to the scale of temperature on which the simplicity of the result depends. Now the temperatures in the formulæ just referred to were expressed by means of the *air thermometer*. Hence MM. Dulong and Petit justly state that while all different substances employed

* The formula for the rate of cooling is $ma^{\theta+t} - ma^{\theta}$, where the quantity m depends upon the nature of the body, the state of its surface, and other circumstances.—*Ann. Chim.* VII. 150.

as thermometers give different laws of thermotical phenomena, their own success in obtaining simple and general laws by means of the air thermometer, is a strong recommendation of that as the *natural scale of heat.* They add*, "The well-known uniformity of the principal physical properties of all gases, and especially the perfect identity of their laws of dilatation by heat, [a very important discovery of Dalton and Gay Lussac†,] make it very probable that in this class of bodies the disturbing causes have not the same influence as in solids and liquids; and consequently that the changes of bulk produced by the action of heat are here in a more immediate dependence on the force which produces them."

19. Still we cannot consider this point as settled till we obtain a more complete theoretical insight into the nature of heat itself. If it be true that heat consists in the vibrations of a fluid, then, although, as Ampère has shown‡, the laws of radiation will, on mathematical grounds, be the same as they are on the hypothesis of emission, we cannot consider the natural scale of heat as determined, till we have discovered some means of measuring the caloriferous vibrations as we measure luminiferous vibrations. We shall only know what the quantity of heat is when we know what heat itself is;—when we have obtained a theory which satisfactorily explains the manner in which the substance or medium of heat produces it effects. When we see how radiation and conduction, dilatation and liquefaction, are all produced by mechanical changes of the same fluid, we shall then see what the nature of that change is which dilatation really measures, and what relation it bears to any more proper standard of heat.

We may add, that while our thermotical theory is

* *Annales de Chimie*, VII. 153. † *Hist. Ind. Sci.*, B. x. c. ii. sect. 1.
‡ *Ib.*, c. iv.

still so imperfect as it is, all attempts to divine the true nature of the relation between light and heat are premature, and must be in the highest degree insecure and visionary. Speculations in which, from the general assumption of a caloriferous and luminiferous medium, and from a few facts arbitrarily selected and loosely analyzed, a general theory of light and heat is asserted, are entirely foreign to the course of inductive science, and cannot lead to any stable and substantial truth.

20. *Other Instruments for measuring Heat.*—It does not belong to our present purpose to speak of instruments of which the object is to measure, not sensible qualities, but some effect or modification of the cause by which such qualities are produced: such, for instance, are the *Calorimeter*, employed by Lavoisier and Laplace, in order to compare the *specific heat* of different substances; and the *Actinometer*, invented by Sir John Herschel, in order to determine the *effect of the sun's rays* by means of the heat which they communicate in a given time; which effect is, as may readily be supposed, very different under different circumstances of atmosphere and position. The laws of such effects may be valuable contributions to our knowledge of heat, but the interpretation of them must depend on a previous knowledge of the relations which temperature bears to heat, according to the views just explained.

Sect. VI.—*Scales of other Qualities.*

21. Before quitting the subject of the measures of sensible qualities, we may observe that there are several other such qualities for which it would be necessary to have scales and means of measuring, in order to make any approach to science on such subjects. This is true, for instance, of tastes and smells. Indeed some attempts have been made towards a classification of the tastes of

sapid substances, but these have not yet assumed any satisfactory or systematic character; and I am not aware that any instruments has been suggested for *measuring* either the flavour or the odour of bodies which possess such qualities.

22. *Quality of Sounds.*—The same is true of that kind of difference in sounds which is peculiarly termed their *quality;* that character by which, for instance, the sound of a flute differs from that of a hautbois, when the note is the same; or a woman's voice from a boy's.

23. *Articulate Sounds.*—There is also in sounds another difference, of which the nature is still obscure, but in reducing which to rule, and consequently to measure, some progress has nevertheless been made. I speak of the differences of sound considered as *articulate.* Classifications of the sounds of the usual alphabets have been frequently proposed; for instance, that which arranges the *consonants* in the following groups:

Sharp.	Flat.	Sharp Aspirate.	Flat Aspirate.	Nasal.
p	b	ph (*f*)	bh (*v*)	m
k	g (hard)	kh	gh	ng
t	d	th (sharp)	th (flat)	n
s	z	sh	zh	

It is easily perceived that the relations of the sounds in each of these horizontal lines are analogous; and accordingly the rules of derivation and modification of words in several languages proceed upon such analogies. In the same manner the *vowels* may be arranged in an order depending on their sound. But to make such arrangements fixed and indisputable, we ought to know the mechanism by which such modifications are caused. Instruments have been invented by which some of these sounds can be imitated; and if such instruments could be made to produce the above series of articulate sounds, by connected and regular processes, we should find, in the process, a *measure* of the sound produced. This has been in a great degree effected for the Vowels by Professor Willis's artificial mode of imitating them. For he finds that if a musical reed be made to sound through a cylindrical pipe, we obtain by gradually lengthening the cylindrical pipe, the series of vowels I, E, A, O, U, with intermediate sounds*. In this instrument, then, the length of the pipe would determine the vowel, and might be used numerically to express it. Such an instrument so employed would be a measure of vowel quality, and might be called a *Phthongometer*.

Our business at present, however, is not with instruments which might be devised for measuring sensible qualities, but with those which have been so used, and have thus been the basis of the sciences in which such qualities are treated of; and this we have now done sufficiently for our present purpose.

24. There is another Idea which, though hitherto very vaguely entertained, has had considerable influence in the formation, both of the sciences spoken of in the present Book, and on others which will hereafter come under our notice: namely, the Idea of Polarity. This Idea will be the subject of the ensuing Book. And although this Idea forms a part of the basis of various other extensive portions of science, as Optics and Chemistry, it occupies so peculiarly conspicuous a place in speculations belonging to what I have termed the Mechanico-Chemical Sciences, (Magnetism and Electricity,) that I shall designate the discussion of the Idea of Polarity as the Philosophy of those Sciences.

* *Camb. Trans.*, Vol. III. p. 239.

BOOK V.

OF THE PHILOSOPHY OF THE MECHANICO-CHEMICAL SCIENCES.

CHAPTER I.

ATTEMPTS AT THE SCIENTIFIC APPLICATION OF THE IDEA OF POLARITY.

1. IN some of the mechanical sciences, as Magnetism and Optics, the phenomena are found to depend upon position (the position of the magnet, or of the ray of light,) in a peculiar alternate manner. This dependence, as it was first apprehended, was represented by means of certain conceptions of space and force, as for instance by considering the two *poles* of a magnet. But in all such modes of representing these alternations by the conceptions borrowed from other ideas, a closer examination detected something superfluous and something defective; and in proportion as the view which philosophers took of this relation was gradually purified from these incongruous elements, and was rendered more general and abstract by the discovery of analogous properties in new cases, it was perceived that the relation could not be adequately apprehended without considering it as involving a peculiar and independent Idea, which we may designate by the term *Polarity*.

We shall trace some of the forms in which this Idea has manifested itself in the history of science. In doing so we shall not begin, as in other Books of this work we have done, by speaking of the notion as it is employed in common use: for the relation of polarity is of so abstract and technical a nature, that it is not employed, at least in any distinct and obvious manner, on any ordinary or practical occasions. The idea belongs peculiarly to the region of speculation: in persons of common habits of thought it is probably almost or quite undeveloped; and even most of those whose minds have been long occupied by science, find a difficulty in apprehending it in its full generality and abstraction, and stript of all irrelevant hypothesis.

2. *Magnetism.*—The name and the notion of *Poles* were first adopted in the case of a magnet. If we have two magnets, their extremities attract and repel each other alternatively. If the first end of the one attract the first end of the other, it repels the second end, and conversely. In order to express this rule conveniently, the two ends of each magnet are called the *north pole* and the *south pole* respectively, the denominations being borrowed from the poles of the earth and heavens. "These poles," as Gilbert says*, "regulate the motions of the celestial spheres and of the earth. In like manner the magnet has its poles, a northern and a southern one; certain and determined points constituted by nature in the stone, the primary terms of its motions and effects, the limits and governors of many actions and virtues."

The nature of the opposition of properties of which we speak may be stated thus.

The North pole of one magnet attracts the South pole of another magnet.

The North pole of one magnet *repels* the North pole of another magnet.

The South pole of one magnet repels the South pole of another magnet.

* *De Magn.*, Lib. I. c. iii.

The South pole of one magnet *attracts* the North pole of another magnet.

It will be observed that the contrariety of position which is indicated by putting the South pole for the North pole in either magnet, is accompanied by the opposition of mechanical effect which is expressed by changing attraction into repulsion and repulsion into attraction: and thus we have the general feature of polarity:—A contrast of properties corresponding to a contrast of positions.

3. *Electricity.*—When the phenomena of electricity came to be studied, it appeared that they involved relations in some respects analogous to those of magnetism.

Two kinds of electricity were distinguished, the positive and the negative; and it appeared that two bodies electrized positively or two electrized negatively, repelled each other, like two north or two south magnetic poles; while a positively and a negatively electrized body attracted each other, like the north and south poles of two magnets. In conductors of an oblong form, the electricity could easily be made to distribute itself so that one end should be positively and one end negatively electrized; and then such conductors acted on each other exactly as magnets would do.

But in conductors, however electrized, there is no peculiar point which can permanently be considered as the *pole.* The distribution of electricity in the conductor depends upon external circumstances: and thus, although the phenomena offer the general character of *polarity*—alternative results corresponding to alternative positions,—they cannot be referred to poles. Some other mode of representing the forces must be adopted than that which makes them emanate from permanent points as in a magnet.

The phenomena of attraction and repulsion in electrized bodies were conveniently represented by means of the hypothesis of *two* electric *fluids*, a positive and a negative one, which were supposed to be distributed in the bodies. Of these fluids, it was supposed that each repelled its own parts and attracted those of the opposite fluid: and it was found that this hypothesis explained all the obvious laws of electric action. Here then we have the phenomena of polarization explained by a new kind of machinery:—two opposite fluids distributed in bodies, and supplying them, so to speak, with their polar forces. This hypothesis not only explains electrical attraction, but also the electrical spark: when two bodies, of which the neighbouring surfaces are charged with the two opposite fluids, approach near to each other, the mutual attraction of the fluids becomes more and more intense, till at last the excess of fluid on the one body breaks through the air and rushes to the other body, in a form accompanied by light and noise. When this transfer has taken place, the attraction ceases, the positive and the negative fluid having neutralized each other. Their effort was to unite; and this union being effected, there is no longer any force in action. Bodies in their natural unexcited condition may be considered as occupied by a combination of the two fluids: and hence we see how the production of either kind of electricity is necessarily accompanied with the production of an equivalent amount of the opposite kind.

4. *Voltaic Electricity.*—Such is the case in Franklinic electricity,—that which is excited by the common electrical machine. In studying Voltaic electricity, we are led to the conviction that the fluid which is in a condition of momentary equilibrium in electrized conductors, exists in the state of *current* in the voltaic circuit. And here we find polar relations of a new kind existing among the forces. Two voltaic currents attract each other when

they are moving in the same, and repel each other when they are moving in opposite, directions.

But we find, in addition to these, other polar relations of a more abstruse kind, and which the supposition of two fluids does not so readily explain. For instance, if such fluids existed, distinct from each other, it might be expected that it would be possible to exhibit one of them separate from the other. Yet in all the phenomena of electromotive currents, we attempt in vain to obtain one kind of electricity separately. "I have not," says Mr. Faraday*, "been able to find a single fact which could be adduced to prove the theory of two electricities rather than one, in electric currents; or, admitting the hypothesis of two electricities, have I been able to perceive the slightest grounds that one electricity can be more powerful than the other,—or that it can be present without the other,—or that it can be varied or in the slightest degree affected without a corresponding variation in the other." "Thus," he adds, "the polar character of the powers is rigorous and complete." Thus, we too may remark, all the superfluous and precarious parts gradually drop off from the hypothesis which we devise in order to represent polar phenomena; and the abstract notion of polarity—of equal and opposite powers called into existence by a common condition—remains unincumbered with extraneous machinery.

5. *Light.*—Another very important example of the application of the idea of polarity is that supplied by the discovery of the polarization of light. A ray of light may, by various processes, be modified, so that it has different properties according to its different *sides*, although this difference is not perceptible by any common effects. If, for instance, a ray thus modified, pass perpendicularly

* *Researches*, 516.

through a circular glass, and fall upon the eye, we may turn the glass round and round its frame, and we shall made no difference in the brightness of the spot which we see. But if, instead of a glass, we look through a longitudinal slice of tourmaline, the spot is alternately dark and bright as we turn the crystal through successive quadrants. Here we have a contrast of properties (dark and bright) corresponding to a contrast of positions, (the position of a line east and west being contrasted with the position north and south,) which, as we have said, is the general character of polarity. It was with a view of expressing this character that the term *polarization* was originally introduced. Malus was forced by his discoveries into the use of this expression. "We find," he says, in 1811, "that light acquires properties which are relative only to the sides of the ray,—which are the same for the north and south sides of the ray, (using the points of the compass for description's sake only,) and which are different when we go from the north and south to the east or to the west sides of the ray. I shall give the name of *poles* to these sides of the ray, and shall call *polarization* the modification which gives to light these properties relative to these poles. I have *put off* hitherto the admission of this term into the description of the physical phenomena with which we have to do: I did not *dare* to introduce it into the Memoirs in which I published my last observations: but the variety of forms in which this new phenomenon appears, and the difficulty of describing them, compel me to admit this new expression; which signifies simply the modification which light has undergone in acquiring new properties which are not relative to the direction of the ray, but only to its sides considered at right angles to each other, and in a plane perpendicular to its direction."

The theory which represents light as an emission of particles was in vogue at the time when Malus published his discoveries; and some of his followers in optical research conceived that the phenomena which he thus described rendered it necessary to ascribe poles and an axis to each particle of light. On this hypothesis, light would be polarized when the axes of all the particles were in the same direction: and, making such a supposition, it may easily be conceived capable of transmission through a crystal whose axis is parallel to that of the luminous particles, and intransmissible when the axis of the crystal is in a position transverse to that of the particles.

The hypothesis of particles possessing *poles* is a rude and arbitrary assumption, in this as in other cases; but it serves to convey the general notion of polarity, which is the essential feature of the phenomena. The term "polarization of light" has sometimes been complained of in modern times as hypothetical and obscure. But the real cause of obscurity was, that the Idea of Polarity was, till lately, very imperfectly developed in men's minds. As we have seen, the general notion of polarity,—opposite properties in opposite directions,—exactly describes the character of the optical phenomena to which the term is applied.

It is to be recollected that in optics we never speak of the *poles*, but of the *plane of polarization* of a ray. The word *sides*, which Newton and Malus have used, neither of them appears to have been satisfied with; Newton, in employing it, had recourse to the strange Gallicism of speaking of the *coast* of usual and of unusual refraction of a crystal.

The modern theory of optics represents the plane of polarization of light as depending, not on the position in which the axes of the luminiferous particles lie, but on the direction of those transverse vibrations in which light consists. This theory is, as we have stated in the History, recommended by an extraordinary series of successes in accounting for the phenomena. And this hypothesis of transverse vibrations shows us another mechanical mode, (besides the hypothesis of particles with axes,) by which we may represent the polarity of a ray. But we may remark that the general notion of polarity, as applied to light in such cases, would subsist, even if the undulatory theory were rejected. The idea is, as we have before said, independent of all hypothetical machinery.

I need not here refer to the various ways in which light may be polarized, as, for instance, by being reflected from the surface of water or of glass at certain angles, by being transmitted through crystals, and in other ways. In all cases the modification produced, the polarization, is identically the same property. Nor need I mention the various kinds of phenomena which appear as contrasts in the result; for these are not merely light and dark, or white and black, but red and green, and generally, a colour and its *complementary* colour, exhibited in many complex and varied configurations. These multiplied modes in which polarized light presents itself add nothing to the original conception of polarization: and I shall therefore pass on to another subject.

6. *Crystallization.*—Bodies which are perfectly crystallized exhibit the most complete regularity and symmetry of form; and this regularity not only appears in their outward shape, but pervades their whole texture, and manifests itself in their cleavage, their transparency, and in the uniform and determinate optical properties which exist in every part, even the smallest fragment of the mass. If we conceive crystals as composed of particles, we must suppose these particles to be arranged in the most regular manner; for example, if we suppose

each particle to have an axis, we must suppose all these axes to be parallel; for the direction of the axis of the particles is indicated by the physical and optical properties of the crystal, and therefore this direction must be the same for every portion of the crystal. This parallelism of the axes of the particles may be conceived to result from the circumstance of each particle having poles, the opposite poles attracting each other. In virtue of forces acting as this hypothesis assumes, a collection of small magnetic particles would arrange themselves in parallel positions; and such a collection of magnetic particles offers a sort of image of a crystal. Thus we are led to conceive the particles of crystals as polarized, and as determined in their crystalline positions by polar forces. This mode of apprehending the constitution of crystals has been adopted by some of our most eminent philosophers. Thus Berzelius says*, "It is demonstrated, that the regular forms of bodies presuppose an effort of their atoms to touch each other by preference in certain points; that is, they are founded upon a Polarity;"—he adds, "a polarity which can be no other than an electric or magnetic polarity." In this latter clause we have the identity of different kinds of polarity asserted; a principle which we shall speak of in the next chapter. But we may remark, that even without dwelling upon this connexion, any notion which we can form of the structure of crystals necessarily involves the idea of polarity. Whether this polarity necessarily requires us to believe crystals to be composed of atoms which exert an effort to touch each other in certain points by preference, is another question. And, in agreement with what has been said respecting other kinds of polarity, we shall probably find, on a more profound examination of the subject, that while the idea of polarity is essential,

* *Essay on the Theory of Chemical Properties*, 1820, p. 113.

the machinery by which it is thus expressed is precarious and superfluous.

7. *Chemical Affinity.*—We shall have, in the next Book, to speak of Chemical Affinity at some length; but since the ultimate views to which philosophers have been led, induce them to consider the forces of affinity as polar forces, we must enumerate these among the examples of polarity. In chemical processes, opposites tend to unite, and to neutralize each other by their union. Thus an *acid* or an *alkali* combine with vehemence, and form a compound, a neutral salt, which is neither acid nor alkaline.

This conception of contrariety and mutual neutralization, involves the idea of polarity. In the conception, as entertained by the earlier chemists, the idea enters very obscurely: but in the attempts which have more recently been made to connect this relation (of acid and base,) with other relations, the chemical elements have been conceived as composed of particles which possess poles; *like* poles repelling, and *unlike* attracting each other, as they do in magnetic and electric phenomena. This is, however, a rude and arbitrary way of expressing polarity, and, as may be easily shown, involves many difficulties which do not belong to the idea itself. Mr. Faraday, who has been led by his researches to a conviction of the polar nature of the forces of chemical affinity, has expressed their character in a more general manner, and without any of the machinery of particles indued with poles. According to his view, chemical synthesis and analysis must always be conceived as taking place in virtue of equal and opposite forces, by which the particles are united or separated. These forces, by the very circumstance of their being polar, may be transferred from point to point. For if we conceive a string of particles, and if the positive force of the first particle be liberated and brought into action, its negative force also must be set free: this negative force neutralizes the positive force of the next particle, and therefore the negative force of this particle (before employed in neutralizing its positive force,) is set free: this is in the same way transferred to the next particle, and so on. And thus we have a positive force active at one extremity of a line of particles, corresponding to a negative force at the other extremity, all the intermediate particles reciprocally neutralizing each other's action. This conception of the transfer of chemical action was indeed at an earlier period introduced by Grotthus*, and confirmed by Davy. But in Mr. Faraday's hands we see it divested of all that is superfluous, and spoken of, not as a line of particles, but as "an axis of power, having [at every point,] contrary forces, exactly equal, in opposite directions."

8. *General Remarks.*—Thus, as we see, the notion of polarity is applicable to many large classes of phenomena. Yet the idea in a distinct and general form is only of late growth among philosophers. It has gradually been abstracted and refined from many extraneous hypotheses which were at first supposed to be essential to it. We have noticed some of these hypotheses;—as the poles of a body; the poles of the particles of a fluid; two opposite fluids; a single fluid in excess and defect; transverse vibrations. To these others might be added. Thus Dr. Prout† assumes that the polarity of molecules results from their rotation on their axes, the opposite motions of contiguous molecules being the cause of opposite (positive and negative) polarities.

But none of these hypotheses can be proved by the fact of polarity alone; and they have been in succession rejected when they had been assumed on that ground.

* Dumas, *Leçons sur la Philosophie Chimique*, p. 401.

† *Bridgewater Treatise*, p. 559.

Thus Davy, in 1826, speaking of chemical forces says*, "In assuming the idea of two ethereal, subtile, elastic fluids, attractive of the particles of each other, and repulsive as to their own particles, capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities and rendering them equivalent masses, it would be natural to refer the action of the poles to the repulsions of the substances combined with the excess of one fluid, and the attractions of those united to the excess of the other fluid; and a history of the phenomena, not unsatisfactory to the reason, might in this way be made out. But as it is possible likewise to take an entirely different view of the subject, on the idea of the dependence of the results upon the primary attractive powers of the parts of the combination on a single subtile fluid, I shall not enter into any discussion on this obscure part of the theory." Which of these theories will best represent the case, will depend upon the consideration of other facts, in combination with the polar phenomena, as we see in the history of optical theory. In like manner Mr. Faraday proved by experiment† the errour of all theories which ascribe electro-chemical decomposition to the attraction of the poles of the voltaic battery.

In order that they may distinctly image to themselves the idea of polarity, men clothe it in some of the forms of machinery above spoken of; yet every new attempt shows them the unnecessary difficulties in which they thus involve themselves. But on the other hand it is difficult to apprehend this idea divested of all machinery; and to entertain it in such a form that it shall apply at the same time to magnetism and electricity, galvanism and chemistry, crystalline structure and light. The Idea of *Polarity* becomes most pure and

* *Phil. Tr.*, 1826, p. 415. † *Researches*, p. 495, &c.

genuine, when we entirely reject the conception of *Poles*, as Faraday has taught us to do in considering electro-chemical decomposition; but it is only by degrees and by effort that we can reach this point of abstraction and generality.

9. There is one other remark which we may here make. It was a maxim commonly received in the ancient schools of philosophy, that "like attracts like:" but as we have seen, the universal maxim of polar phenomena is, that like *repels* like, and attracts unlike. The north pole attracts the south pole, the positive fluid attracts the negative fluid; opposite elements rush together; opposite motions reduce each other to rest. The permanent and stable course of things is that which results from the balance and neutralization of contrary tendencies. Nature is constantly labouring after repose by the effect of such tendencies; and so far as polar forces enter into her economy, she seeks harmony by means of discord, and unity by opposition.

Although the Idea of Polarity is as yet somewhat vague and obscure, even in the minds of the cultivators of physical science, it has nevertheless given birth to some general principles which have been accepted as evident, and have had great influence on the progress of science. These we shall now consider.

CHAPTER II.

OF THE CONNEXION OF POLARITIES.

1. IT has appeared in the preceding chapter that in cases in which the phenomena suggest to us the idea of polarity, we are also led to assume some material machinery as the mode in which the polar forces are exerted.

We assume, for instance, globular particles which possess poles, or the vibrations of a fluid, or two fluids attracting each other; in every case, in short, some hypothesis by which the existence and operation of the polarity is embodied in geometrical and mechanical properties of a medium; nor is it possible for us to avoid proceeding upon the conviction that some such hypothesis must be true; although the nature of the connexion between the mechanism and the phenomena must still be indefinite and arbitrary.

But since each class of polar phenomena is thus referred to an ulterior cause, of which we know no more than that it has a polar character, it follows that different polarities may result from the same cause manifesting its polar character under different aspects. Taking, for example, the hypothesis of globular particles, if electricity result from an action dependent upon the *poles* of each globule, magnetism may depend upon an action in the *equator* of each globule; or taking the supposition of transverse vibrations, if polarized light result directly from such vibrations, crystallization may have reference to the axes of the elasticity of the medium by which the vibrations are rendered transverse,—so far as the polar character only of the phenomena is to be accounted for. I say this *may* be so, *in so far* only as the polar character of the phenomena is concerned; for whether the relation of electricity to magnetism, or of crystalline forces to light, can really be explained by such hypotheses, remains to be determined by the facts themselves. But since the first necessary feature of the hypothesis is, that it shall give polarity, and since an hypothesis which does this, may, by its mathematical relations, give polarities of different kinds and in different directions, any two co-existent kinds of polarity may result from the same cause, manifesting itself in various manners.

The conclusion to which we are led by these general considerations is, that two co-existing classes of polar phenomena *may* be effects of the same cause. But those who have studied such phenomena more deeply and attentively have, in most or in all cases, arrived at the conviction that the various kinds of polarity in such cases *must* be connected and fundamentally identical. As this conviction has exercised a great influence, both upon the discoveries of new facts and upon the theoretical speculations of modern philosophers, and has been put forward by some writers as a universal principle of science, I will consider some of the cases in which it has been thus applied.

2. *Connexion of Magnetic and Electric Polarity.*—The polar phenomena of electricity and magnetism are clearly analogous in their laws: and obvious facts showed at an early period that there was some connexion between the two agencies. Attempts were made to establish an evident and definite relation between the two kinds of force, which attempts proceeded upon the principle now under consideration;—namely, that in such cases, the two kinds of polarity must be connected. Professor Œrsted, of Copenhagen, was one of those who made many trials founded upon this conviction: yet all these were long unsuccessful. At length, in 1820, he discovered that a galvanic current, passing at right angles near to a magnetic needle, exercises upon it a powerful deflecting force. The connexion once detected between magnetism and galvanism was soon recognized as constant and universal. It was represented in different hypothetical modes by different persons; some considering the galvanic current as the primitive axis, and the magnet as constituted of galvanic currents passing round it at right angles to the magnetic axis; while others conceived the magnetic axis as the primitive one, and

the electric current as implying a magnetic current round the wire. So far as many of the general relations of these two kinds of force were concerned, either mode of representation served to express them; and thus the assumption that the two polarities, the magnetic and the electric, were fundamentally identical, was verified, so far as the phenomena of magnetic attraction, and the like, were concerned.

I need not here mention how this was further confirmed by the experiments in which, by means of the forces thus brought into view, a galvanic wire was made to revolve round a magnet, and a magnet round a galvanic wire;—in which artificial magnets were constructed of coils of galvanic wire;—and finally, in which the galvanic spark was obtained from the magnet. The identity which sagacious speculators had divined even before it was discovered, and which they had seen to be universal as soon as it was brought to light, was completely manifested in every imaginable form.

The relation of the electric and magnetic polarities was found to be, that they were *transverse* to each other, and this relation exhibited under various conditions of form and position of the apparatus, gave rise to very curious and unexpected perplexities. The degree of complication which this relation may occasion, may be judged of from the number of constructions and modes of conception offered by Œrsted, Wollaston, Faraday, and others, for the purpose of framing a technical memory of the results. The magnetic polarity gives us the north and south poles of the needle; the electric polarity makes the current positive and negative; and these pairs of opposites are connected by relations of situation, as above and below, right and left; and give rise to the resulting motion of the needle one way or the other.

3. Ampère, by framing his hypotheses of the action

of voltaic currents and the constitution of magnets, reduced all these technical rules to rigorous deductions from one general principle. And thus the vague and obscure persuasion that there *must* be *some* connexion between electricity and magnetism, so long an idle and barren conjecture, was unfolded into a complete theory, according to which magnetic and electromotive actions are only two different manifestations of the same forces; and all the above-mentioned complex relations of polarities are reduced to one single polarity, that of the electro-dynamic current.

4. As the idea of polarity was thus firmly established and clearly developed, it became an instrument of reasoning. Thus it led Ampère to maintain that the original or elementary forces in electro-dynamic action could not be as M. Biot thought they were, a statical *couple*, but must be directly opposite to each other. The same idea enabled Mr. Faraday to carry on with confidence such reasonings as the following*: "No other known power has like direction with that exerted between an electric current and a magnetic pole; it is tangential, while all other forces acting at a distance are direct. Hence if a magnetic pole on one side of a revolving plate follow its course by reason of its obedience to the tangential force exerted upon it by the very current of electricity which it has itself caused; a similar pole on the other side of the plate should immediately set it free from this force; for the currents which have to be formed by the two poles are in contrary directions." And in Article 1114 of his *Researches*, the same eminent philosopher infers that if electricity and magnetism are considered as the results of a peculiar agent or condition, exerted in determinate directions perpendicular to each other, one must be by some means convertible into the other;

* *Researches*, 244.

and this he was afterwards able to prove to be the case in fact.

Thus the principle that the co-existent polarities of magnetism and electricity are connected and fundamentally identical, is not only true, but is far from being either vague or barren. It has been a fertile source both of theories which have, at present, a very great probability, and of the discovery of new and striking facts. We proceed to consider other similar cases.

5. *Connexion of Electrical and Chemical Polarities.*—The doctrine that the chemical forces by which the elements of bodies are held together or separated, are identical with the polar forces of electricity, is a great discovery of modern times; so great and so recent, indeed, that probably men of science in general have hardly yet obtained a clear view and firm hold of this truth. This doctrine is now, however, entirely established in the minds of the most profound and philosophical chemists of our time. The complete developement and confirmation of this as of other great truths, was preceded by more vague and confused opinions gradually tending to this point; and the progress of thought and of research was impelled and guided, in this as in similar cases, by the persuasion that these co-existent polarities could not fail to be closely connected with each other. While the ultimate and exact theory to which previous incomplete and transitory theories tended is still so new and so unfamiliar, it must needs be a matter of difficulty and responsibility for a common reader to describe the steps by which truth has advanced from point to point. I shall, therefore, in doing this, guide myself mainly by the historical sketches of the progress of this great theory, which, fortunately for us, have been given us by the two philosophers who have

played by far the most important parts in the discovery, Davy and Faraday.

It will be observed that we are concerned here with the progress of theory, and not of experiment, except so far as it is confirmatory of theory. In Davy's Memoir* of 1826, on the Relations of Electrical and Chemical Changes, he gives the historical details to which I have alluded. Already in 1802 he had conjectured that all chemical decompositions might be polar. In 1806 he attempted to confirm this conjecture, and succeeded, to his own satisfaction, in establishing† that the combinations and decompositions by electricity were referable to the law of electrical attractions and repulsions; and advanced the hypothesis (as he calls it,) that chemical and electrical attractions were produced by the same cause, acting in one case on particles, in the other on masses. This hypothesis was most strikingly confirmed by the author's being able to use electrical agency as a more powerful means of chemical decomposition than any which had yet been applied. "Believing," he adds, "that our philosophical systems are exceedingly imperfect, I never attached much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained by the application of it a number of practical results, and considering myself as much the author of it as I was of the decomposition of the alkalies, and having developed it in an elementary work as far as the present state of chemistry seemed to allow, I have never," he says "criticized or examined the manner in which different authors have adopted or explained it, contented, if in the hands of others, it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery." When the doctrine had found an extensive acceptance among chemists,

* *Phil. Trans.*, 1826, p. 383. † *Ibid.*, p. 389.

attempts were made to show that it had been asserted by earlier writers: and though Davy justly denies all value to these pretended anticipations, they serve to show, however dimly, the working of that conviction of the connexion of co-existent properties which all along presided in men's minds during this course of investigation. "Ritter and Winterl have been quoted," Davy says*, "among other persons, as having imagined or anticipated the relation between electrical powers and chemical affinities before the discovery of the pile of Volta. But whoever will read with attention Ritter's 'Evidence that Galvanic action exists in organized nature,' and Winter's *Prolusiones ad Chemiam sæculi decimi noni*, will find nothing to justify this opinion." He then refers to the Queries of Newton at the end of his Optics. "These," he says, "contain more grand and speculative views that might be brought to bear upon this question than any found in the works of modern electricians; but it is very unjust to the experimentalists who by the laborious application of new instruments, have discovered novel facts and analogies, to refer them to any such suppositions as that all attractions, chemical, electrical, magnetical, and gravitative, may depend upon the same cause." It is perfectly true, that such vague opinions, though arising from that tendency to generalize which is the essence of science, are of no value except so far as they are both rendered intelligible, and confirmed by experimental research.

The phenomena of chemical decomposition by means of the voltaic pile, however, led other persons to views very similar to those of Davy. Thus Grotthus in 1805† published an hypothesis of the same kind. "The pile of Volta," he says, "is an electrical magnet, of which each element, that is, each pair of plates, has a positive and a

* *Phil. Trans.*, 1826, p. 384.

† *Ann. Chim.*, LXVIII. 54.

negative pole. The consideration of this polarity suggested to me the idea that a similar polarity may come into play between the elementary particles of water when acted upon by the same electrical agent; and I avow that this thought was for me a flash of light."

6. The thought, however, though thus brought into being, was very far from being as yet freed from vagueness, superfluities, and errours. I have elsewhere noticed* Faraday's remark on Davy's celebrated Memoir of 1806; that "the mode of action by which the effects take place is stated very generally, so generally, indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given." When Davy and others proceeded to give a little more definiteness and precision to the statement of their views, they soon introduced into the theory features which it was afterwards found necessary to abandon. Thus† both Davy, Grotthus, Riffault, and Chompré, ascribed electrical decomposition to the action of the poles, and some of them even pretended to assign the proportion in which the force of the pole diminishes as the distance from it increases. Faraday, as I have already stated, showed that the polarity must be considered as residing not only in what had till then been called the poles, but at every point of the circuit. He ascribed‡ electro-chemical decomposition to internal forces, residing in the particles of the matter under decomposition, not to external forces, exerted by the poles. Hence he shortly afterwards§ proposed to reject the word poles altogether, and to employ instead, the term *electrode*, meaning the

* *Hist. Ind. Sci.*, B. XIV. c. ix. sect. 1.

† See Faraday's Historical Sketch, *Researches*, 481—492.

‡ Art 524.

§ In 1834. Eleventh Series of *Researches*. Art. 662.

doors or passages (of whatever surface formed,) by which the decomposed elements pass out. What have been called the positive and negative poles he further termed the *anode* and *cathode*; and he introduced some other changes in nomenclature connected with these. He then, as I have related in the History*, invented the Volta-electrometer, which enabled him to measure the quantity of voltaic action, and this he found to be identical with the quantity of chemical affinity; and he was thus led to the clearest view of the truth towards which he and his predecessors had so long been travelling, that electrical and chemical forces are identical†.

7. It will, perhaps, be said that this beautiful train of discovery was entirely due to experiment, and not to any *à priori* conviction that co-existent polarities must be connected. I trust I have sufficiently stated that such an *à priori* principle could not be proved, nor even understood, without a most laborious and enlightened use of experiment; but yet I think that the doctrine when once fully unfolded, exhibited clearly, and established as true, takes possession of the mind with a more entire conviction of its certainty and universality, in virtue of the principle we are now considering. When the theory has assumed so simple a form, it appears to derive immense probability (to say the least) from its simplicity. Like the laws of motion, when stated in its most general form, it appears to carry with it its own evidence. And thus this great theory borrows something of its character from the Ideas which it involves, as well as from the Experiments by which it was established.

8. We may find in many of Mr. Faraday's subsequent reasonings, clear evidence that this idea of the connexion of polarities, as now developed, is not limited in its

* *Hist. Ind. Sci.*, B. XIV. c. ix. sect. 2.   † Arts. 915, 916, 917.

application to facts already known experimentally, but, like other ideas, determines the philosopher's researches into the unknown, and gives us the *form* of knowledge even before we possess the *matter*. Thus, he says, in his Thirteenth Series*, "I have long sought, and still seek, for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede the discharge, should also have their correspondent transverse condition or effect." Other similar passages might be found.

I will now consider another case to which we may apply the principle of connected polarities.

9. *Connexion of Chemical and Crystalline Polarities.*—The close connexion between the chemical affinity and the crystalline attraction of elements cannot be overlooked. Bodies never crystallize but when their elements combine chemically; and solid bodies which combine, when they do it most completely and exactly, also crystallize. The forces which hold together the elements of a crystal of alum are the same forces which make it a crystal. There is no distinguishing between the two sets of forces.

Both chemical and crystalline forces are polar, as we stated in the last chapter; but the polarity in the two cases is of a different kind. The polarity of chemical forces is then put in the most distinct form, when it is identified with electrical polarity; the polarity of the particles of crystals has reference to their geometrical form. And it is clear that these two kinds of polarity must be connected. Accordingly, Berzelius expressly asserts† the necessary identity of these two polarities.

* Art. 1658. † *Essay on Chemical Prop.*, 113.

"The regular forms of bodies suppose a polarity which can be no other than an electric or magnetic polarity." This being so seemingly inevitable, we might expect to find the electric forces manifesting some relation to the definite directions of crystalline forms. Mr. Faraday tried, but in vain, to detect some such relation. He attempted to ascertain* whether a cube of rock crystal transmitted the electrical force of tension with different intensity along and across the axis of the crystal. In the first specimen there seemed to be some difference; but in other experiments, made both with rock crystal and with calc spar, this difference disappeared. Although therefore we may venture to assert that there must be some very close connexion between electrical and crystalline forces, we are, as yet, quite ignorant what the nature of the connexion is, and in what kind of phenomena it will manifest itself.

10. *Connexion of Crystalline and Optical Polarities.* —Crystals present to us optical phenomena which have a manifestly polar character. The double refraction, both of uniaxal and of biaxal crystals, is always accompanied with opposite polarization of the two rays; and in this and in other ways light is polarized in directions dependent upon the axes of the crystalline form, that is, on the directions of the polarities of the crystalline particles. The identity of these two kinds of polarity (crystalline and optical) is too obvious to need insisting on; and it is not necessary for us here to decide by what hypothesis this identity may most properly be represented. We may hereafter perhaps find ourselves justified in considering the crystalline forces as determining the elasticity of the luminiferous ether to be different in different directions within the crystal, and thus as determining the refraction and polarization of the light

* *Researches.* Art. 1689.

which the crystal transmits. But at present we merely note this case as an additional example of the manifest connexion and fundamental identity of two co-existent polarities.

11. *Connexion of Polarities in general.*—Thus we find that the connexion of different kinds of polarities, magnetic, electric, chemical, crystalline, and optical, is certain as a truth of experimental science. We have attempted to show further that in the minds of several of the most eminent discoverers and philosophers, such a conviction is something more than a mere empirical result: it is a principle which has regulated their researches while it was still but obscurely seen and imperfectly unfolded, and has given to their theories a character of generality and self-evidence which experience alone cannot bestow.

It will, perhaps, be said that these doctrines,—that scientific researches may usefully be directed by principles in themselves vague and obscure;—that theories may have an evidence superior to and anterior to experience;—are doctrines in the highest degree dangerous, and utterly at variance with the soundest maxims of modern times respecting the cultivation of science.

To the justice and wisdom of this caution I entirely agree: and although I have shown that this principle of the *connexion of polarities*, rightly interpreted and established in each case by experiment, involves profound and comprehensive truths; I think it no less important to remark that, at least in the present stage of our knowledge, we can make no use of this principle without taking care, at every step, to determine by clear and decisive experiments, its proper meaning and application. All endeavours to proceed otherwise have led, and must lead, to ignorance and confusion. Attempts to deduce from our bare idea of polarity, and our fundamental convictions respecting the connexion of polarities, theories concerning the forces which really exist in nature, can hardly have any other result than to bewilder men's minds, and to misdirect their efforts.

So far, indeed, as this persuasion of a connexion among apparently different kinds of agencies, impels men, engaged in the pursuit of knowledge, to collect observations, to multiply, repeat, and vary experiments, and to contemplate the result of these in all aspects and relations, it may be an occasion of the most important discoveries. Accordingly we find that the great laws of phenomena which govern the motions of the planets about the sun, were first discovered by Kepler, in consequence of his scrutinizing the recorded observations with an intense conviction of the existence of geometrical and arithmetical harmonies in the solar system. Perhaps we may consider the discovery of the connexion of magnetism and electricity by Professor Œrsted in 1820, as an example somewhat of the same kind; for he also was a believer in certain comprehensive but undefined relations among the properties of bodies; and in consequence of such views entertained great admiration for the *Prologue to the Chemistry of the Nineteenth Century*, of Winterl, already mentioned. M. Œrsted, in 1803, published a summary of this work; and in so doing, praised the views of Winterl as far more profound and comprehensive than those of Lavoisier. Soon afterwards a Review of this publication appeared in France *, in which it was spoken of as a work only fit for the dark ages, and as the indication of a sect which had for some time "ravaged Germany," and inundated that country with extravagant and unintelligible mysticism. It was, therefore, a kind of triumph to M. Œrsted to be, after some years' labour, the author of one of the

* *Ann. Chim.*, Tom. L. (1804), p. 191.

most remarkable and fertile physical discoveries of his time.

12. It was not indeed without some reason that certain of the German philosophers were accused of dealing in doctrines vast and profound in their aspect, but, in reality, indefinite, ambiguous, and inapplicable. And the most prominent of such doctrines had reference to the principle now under our consideration; they represented the properties of bodies as consisting in certain polarities, and professed to deduce, from the very nature of things, with little or no reference to experiment, the existence and connexion of these polarities. Thus Schelling, in his *Ideas towards a Philosophy of Nature*, published in 1803, says*, "Magnetism is the universal act of investing Multiplicity with Unity; but the universal form of the reduction of Multiplicity to Unity is the Line, pure Longitudinal Extension: hence Magnetism is determination of pure Longitudinal Extension; and as this manifests itself by absolute Cohesion, Magnetism is the determination of absolute Cohesion." And as Magnetism was, by such reasoning, conceived to be proved as a universal property of matter, Schelling asserted it to be a confirmation of his views when it was discovered that other bodies besides iron are magnetic. In like manner he used such expressions as the following†: "The threefold character of the Universal, the Particular, and the Indifference of the two,—as expressed in their Identity, is Magnetism, as expressed in their Difference, is Electricity, and as expressed in the Totality, is Chemical Process. Thus these forms are only one form; and the Chemical Process is a mere transfer of the three Points of Magnetism into the Triangle of Chemistry."

It was very natural that the chemists should refuse

* P. 223. † P. 486.

to acknowledge, in this fanciful and vague language, (delivered, however, it is to be recollected, in 1803,) an anticipation of Davy's doctrine of the identity of electrical and chemical forces, or of Œrsted's electro-magnetic agency. Yet it was perhaps no less natural that the author of such assertions should look upon every great step in the electro-chemical theory as an illustration of his own doctrines. Accordingly we find Schelling welcoming, with a due sense of their importance, the discoveries of Faraday. When he heard of the experiment in which electricity was produced from common magnetism, he fastened with enthusiasm upon the discovery, even before he knew any of its details, and proclaimed it at a public meeting of a scientific body* as one of the most important advances of modern science. We have (he thus reasoned) three effects of polar forces; —electro-chemical Decomposition, electrical Action, Magnetism. Volta and Davy had confirmed experimentally the identity of the two former agencies: Œrsted showed that a closed voltaic circuit acquired magnetic properties: but in order to exhibit the identity of electric and magnetic action it was requisite that electric forces should be extricated from magnetic. This great step Faraday, he remarked, had made, in producing the electric spark by means of magnets.

13. Although conjectures and assertions of the kind thus put forth by Schelling involve a persuasion of the pervading influence and connexion of polarities, which persuasion has already been confirmed in many instances, they involve this principle in a manner so vague and ambiguous that it can rarely, in such a form, be of any use or value. Such views of polarity can never teach us in what cases we are and in what we are not to expect to find polar relations; and indeed tend rather

* Ueber Faraday's *Neueste Entdeckung*. *München*. 1832.

to diffuse error and confusion, than to promote knowledge. Accordingly we cannot be surprized to find such doctrines put forward by their authors as an evidence of the small value and small necessity of experimental science. This is done by the celebrated metaphysician Hegel, in his *Encyclopædia**. "Since," says he, "the plane of incidence and of reflection in simple reflection is the same plane, when a second reflector is introduced which further distributes the illumination reflected from the first, the position of the first plane with respect to the second plane, containing the direction of the first reflection and of the second, has its influence upon the position, illumination or darkening of the object as it appears by the second reflection. This influence must be the strongest when the two planes are what we must call *negatively* related to each other:—that is, when they are at right angles." "But," he adds, "when men infer (as Malus has done) from the modification which is produced by this situation, in the illumination of the reflection, that the molecules of light in themselves, that is, on their different sides, possess different physical energies; and when on this foundation, along with the phenomena of entoptical colours therewith connected, a wide labyrinth of the most complex theory is erected; we have then one of the most remarkable examples of the *inferences* of physics from experiment." If Hegel's reasoning prove anything, it must prove that polarization always accompanies reflection under such circumstances as he describes: yet all physical philosophers know that in the case of metals, in which the reflection is most complete, light is not completely polarized at any angle; and that in other substances the polarization depends upon various circumstances which show how idle and inapplicable is the account he thus gives of the

* Sec. 278.

property. His self-complacent remark about the inferences of physics from experiment, is intended to recommend by comparison his own method of considering the nature of things in themselves; a mode of obtaining physical truth which had been more than exhausted by Aristotle, and out of which no new attempts have extracted anything of value since his time.

14. Thus the general conclusion to which we are led on this subject is, that the persuasion of the existence and connexion or identity of various polarities in nature, although very naturally admitted, and in many cases interpreted and confirmed by observed facts, is of itself, so far as we at present possess it, a very insecure guide to scientific doctrines. When it is allowed to dictate our theories, instead of animating and extending our experimental researches, it leads only to errour, confusion, obscurity, and mysticism.

This Fifth Book, on the subject of Polarities, is a short one compared with most of the others. This arises in a great measure from the circumstance that the Idea of Polarity has only recently been apprehended and applied, with any great degree of clearness, among physical philosophers; and is even yet probably entertained in an obscure and ambiguous manner by most experimental inquirers. I have been desirous of not attempting to bring forward any doctrines upon the subject, except such as have been fully illustrated and exemplified by the acknowledged progress of the physical sciences. If I had been willing to discuss the various speculations which have been published respecting the universal prevalence of polarities in the universe, and their results in every province of nature, I might easily have presented this subject in a more extended form; but this would not have been consistent with my plan of tracing the influence of scientific ideas only so far as they have really aided in disclosing and developing scientific truths. And as the influence of this idea is clearly distinguishable both from those which precede and those which follow in the character of the sciences to which it gives rise, and appears likely to be hereafter of great extent and consequence, it seemed better to treat of it in a separate Book, although of a brevity disproportioned to the rest.

BOOK VI.

THE PHILOSOPHY OF CHEMISTRY.

CHAPTER I.

ATTEMPTS TO CONCEIVE ELEMENTARY COMPOSITION.

1. WE have now to bring into view, if possible, the ideas and general principles which are involved in Chemistry,—the science of the composition of bodies. For in this as in other parts of human knowledge, we shall find that there are certain ideas, deeply seated in the mind, though shaped and unfolded by external observation, which are necessary conditions of the existence of such a science. These ideas it is, which impel man to such a knowledge of the composition of bodies, which give meaning to facts exhibiting this composition, and universality to special truths discovered by experience. These are the Ideas of *Element* and of *Substance.*

Unlike the idea of polarity, of which we treated in the last Book, these ideas have been current in men's minds from very early times, and formed the subject of some of the first speculations of philosophers. It happened however, as might have been expected, that in the first attempts they were not clearly distinguished from other notions, and were apprehended and applied in an obscure and confused manner. We cannot better exhibit the peculiar character and meaning of these ideas than by tracing the form which they have assumed and the efficacy which they have exerted in these successive essays. This, therefore, I shall endeavour to do, beginning with the Idea of Element.

2. That bodies are composed or made up of certain parts, elements, or principles, is a conception which has existed in men's minds from the beginning of the first attempts at speculative knowledge. The doctrine of the Four Elements, earth, air, fire and water, of which all things in the universe were supposed to be constituted, is one of the earliest forms in which this conception was systematized; and this doctrine is stated by various authors to have existed as early as the times of the ancient Egyptians*. The words usually employed by Greek writers to express these elements are ἀρχή, a *principle* or *beginning*, and στοιχεῖον, which probably meant a *letter* (of a word) before it meant an *element* of a compound. For the resolution of a word into its letters is undoubtedly a remarkable instance of a successful analysis performed at an early stage of man's history; and might very naturally supply a metaphor to denote the analysis of substances into their intimate parts, when men began to contemplate such an analysis as a subject of speculation. The Latin word *elementum* itself, though by its form it appears to be a derivative abstract term, comes from some root now obsolete; probably† from a word signifying *to grow* or *spring up*.

The mode in which elements form the compound bodies and determine their properties was at first, as might be expected, vaguely and variously conceived. It will, I trust, hereafter be made clear to the reader that

* Gilbert's *Phys.*, L. I. c. iii.

† Vossius *in voce.* "Conjecto esse ab antiquâ voco *eleo* pro *oleo*, id est *cresco:* à qua significatione *proles*, *suboles*, *adolescens:* ut ab *juratum*, *juramentum;* ab *adjutum*, *adjumentum:* sic ab *eletum*, *elementum:* quia inde omnia crescunt ac nascuntur."

the relation of the elements to the compound involves a peculiar and appropriate Fundamental Idea, not susceptible of being correctly represented by any comparison or combination of other ideas, and guiding us to clear and definite results only when it is illustrated and nourished by an abundant supply of experimental facts. But at first the peculiar and special notion which is required in a just conception of the constitution of bodies was neither discerned nor suspected; and up to a very late period in the history of chemistry, men went on attempting to apprehend the constitution of bodies more clearly by substituting for this obscure and recondite idea of Elementary Composition, some other idea more obvious, more luminous, and more familiar, such as the ideas of Resemblance, Position, and mechanical Force. We shall briefly speak of some of these attempts, and of the errours which were thus introduced into speculations on the relations of elements and compounds.

3. *Compounds assumed to resemble their Elements.*— The first notion was that compounds derive their qualities from their elements by *resemblance*:—they are hot in virtue of a hot element, heavy in virtue of a heavy element, and so on. In this way the doctrine of the *four elements* was framed; for every body is either hot or cold, moist or dry; and by combining these qualities in all possible ways, men devised four elementary substances, as has been stated in the History*.

This assumption of the derivation of the qualities of bodies from similar qualities in the elements was, as we shall see, altogether baseless and unphilosophical, yet it prevailed long and universally. It was the foundation of medicine for a long period, both in Europe and Asia; disorders being divided into hot, cold, and the like; and remedies being arranged according to similar distinctions.

* *Hist. Ind Sci.*, B. I. c. ii. sect. 2.

Many readers will recollect, perhaps, the story* of the indignation which the Persian physicians felt towards the European, when he undertook to cure the ill effects of cucumber upon the patient, by means of mercurial medicine: for cucumber, which is cold, could not be counteracted, they maintained, by mercury, which in their classification is cold also. Similar views of the operation of medicines might easily be traced in our own country. A moment's reflection may convince us that when drugs of any kind are subjected to the chemistry of the human stomach and thus made to operate on the human frame, it is utterly impossible to form the most remote conjecture what the result will be from any such vague notions of their qualities as the common use of our senses can give. And in like manner the common operations of chemistry give rise in almost every instance to products which bear no resemblance to the materials employed. The results of the furnace, the alembic, the mixture, frequently have no visible likeness to the ingredients operated upon. Iron becomes steel by the addition of a little charcoal; but what visible trace of the charcoal is presented by the metal thus modified? The most beautiful colours are given to glass and earthenware by minute portions of the ores of black or dingy metals, as iron and manganese. The worker in metal, the painter, the dyer, the vintner, the brewer, all the artisans in short who deal with practical chemistry, are able to teach the speculative chemist that it is an utter mistake to expect that the qualities of the elements shall be still discoverable, in an unaltered form, in the compound. This first rude notion of an element, that it determines the properties of bodies by resemblance, must be utterly rejected and abandoned before

* See *Hadji Baba*.

we can make any advance towards a true apprehension of the constitution of bodies.

4. This step accordingly was made, when the hypothesis of the four elements was given up, and the doctrine of the *three Principles*, Salt, Sulphur and Mercury, was substituted in its place. For in making this change, as I have remarked in the History*, the real advance was the acknowledgment of the changes produced by the chemist's operations as results to be accounted for by the union and separation of substantial elements, however great the changes, and however unlike the product might be to the materials. And this step once made, chemists went on constantly advancing towards a truer view of the nature of an element, and consequently, towards a more satisfactory theory of chemical operations.

5. Yet we may, I think, note one instance, even in the works of eminent modern chemists, in which this maxim, that we have no right to expect any resemblance between the elements and the compound, is lost sight of. I speak of certain classifications of mineral substances. Berzelius, in his System of Mineral Arrangement, places *sulphur* next to the *sulphurets*. But surely this is an errour, involving the ancient assumption of the resemblance of elements and compounds; as if we were to expect the sulphurets to bear a resemblance to sulphur. All classifications are intended to bring together things resembling each other: the sulphurets of metals have certain general resemblances to each other which make them a tolerably distinct, well determined, class of bodies. But sulphur has no resemblances with these, and no analogies with them, either in physical or even in chemical properties. It is a simple body;

* *Hist. Ind. Sci.*, B. IV. c. i.

and both its resemblances and its analogies direct us to place it along with other simple bodies, (selenium, and phosphorus,) which, united with metals, produce compounds not very different from the sulphurets. Sulphur cannot be, nor approach to being, a sulphuret; we must not confound what it *is* with what it *makes*. Sulphur has its proper influence in determining the properties of the compound into which it enters; but it does not do this according to resemblance of qualities, or according to any principle which properly leads to propinquity in classification.

6. *Compounds assumed to be determined by the Figure of Elements.*—I pass over the fanciful modes of representing chemical changes which were employed by the Alchemists; for these strange inventions did little in leading men towards a juster view of the relations of elements to compounds. I proceed for an instant to the attempt to substitute another obvious conception for the still obscure notion of elementary composition. It was imagined that all the properties of bodies and their mutual operations might be accounted for by supposing them constituted of *particles* of various *forms*, round or angular, pointed or hooked, straight or spiral. This is a very ancient hypothesis, and a favourite one with many casual speculators in all ages. Thus Lucretius undertakes to explain why wine passes rapidly through a sieve and oil slowly, by telling us that the latter substance has its particles either larger than those of the other, or more hooked and interwoven together. And he accounts for the difference of sweet and bitter by supposing the particles in the former case to be round and smooth, in the latter sharp and jagged*. Similar assumptions prevailed in modern times on the revival of the mechanical philosophy, and constitute a large part of the physical schemes of Descartes

* *De Rerum Natura*, II. 390 sqq.

and Gassendi. They were also adopted to a considerable extent by the chemists. Acids were without hesitation assumed to consist of sharp pointed particles; which, "I hope," Lemery says*, "no one will dispute, seeing every one's experience does demonstrate it: he needs but taste an acid to be satisfied of it, for it pricks the tongue like anything keen and finely cut." Such an assumption is not only altogether gratuitous and useless, but appears to be founded in some degree upon a confusion in the metaphorical and literal use of such words as *keen* and *sharp*. The assumption once made, it was easy to accommodate it, in a manner equally arbitrary, to other facts. "A demonstrative and convincing proof that an acid does consist of pointed parts is, that not only all acid salts do crystallize into edges, but all dissolutions of different things, caused by acid liquors, do assume this figure in their crystallization. These crystals consist of points differing both in length and bigness one from another, and this diversity must be attributed to the keener or blunter edges of the different sorts of acids: and so likewise this difference of the points in subtilty is the cause that one acid can penetrate and dissolve with one sort of *mixt*, that another can't rarify at all: Thus *vinegar* dissolves *lead*, which *aqua fortis* can't: *aqua fortis* dissolves *quicksilver*, which *vinegar* will not touch; *aqua regalis* dissolves *gold*, whenas *aqua fortis* cannot meddle with it; on the contrary, *aqua fortis* dissolves *silver*, but can do nothing with gold, and so of the rest."

The leading fact of the vehement combination and complete union of acid and alkali readily suggested a fit form for the particles of the latter class of substances. "This effect," Lemery adds, "may make us reasonably conjecture that an alkali is a terrestrious and solid matter whose forms are figured after such a manner that the

* *Chemistry*, p. 25.

acid points entering in do strike and divide whatever opposes their motion." And in a like spirit are the speculations in Dr. Mead's *Mechanical Account of Poisons* (1745). Thus he explains the poisonous effect of *corrosive sublimate* of mercury by saying* that the particles of the salt are a kind of lamellæ or blades to which the mercury gives an additional weight. If resublimed with three-fourths the quantity of mercury, it loses its corrosiveness, (becoming *calomel*,) which arises from this, that in sublimation "the crystalline blades are divided every time more and more by the force of the fire;" and "the broken pieces of the crystals uniting into little masses of differing figures from their former make, those cutting points are now so much smaller that they cannot make wounds deep enough to be equally mischievous and deadly: and therefore do only vellicate and twitch the sensible membranes of the stomach."

7. Among all this very fanciful and gratuitous assumption we may notice one true principle clearly introduced, namely, that the suppositions which we make respecting the forms of the elementary particles of bodies and their mode of combination must be such as to explain the facts of crystallization, as well as of mere chemical change. This principle we shall hereafter have occasion to insist upon further.

I now proceed to consider a more refined form of assumption respecting the constitution of bodies, yet still one in which a vain attempt is made to substitute for the peculiar idea of chemical composition a more familiar mechanical conception.

8. *Compounds assumed to be determined by the Mechanical Attraction of the Elements.*—When, in consequence of the investigations and discoveries of Newton and his predecessors, the conception of mechanical force had

* P. 199.

become clear and familiar, so far as the action of external forces upon a body was concerned, it was very natural that the mathematicians who had pursued this train of speculation should attempt to apply the same conception to that mutual action of the internal parts of a body by which they are held together. Newton himself had pointed the way to this attempt. In the Preface to the *Principia*, after speaking of what he has done in calculating the effects of forces upon the planets, satellites, &c., he adds, "Would it were permitted us to deduce the other phenomena of nature from mechanical principles by the same kind of reasoning. For many things move me to suspect that all these phenomena depend upon certain forces, by which the particles of bodies, through causes not yet known, are either urged towards each other, and cohere according to regular figures, or are repelled and recede from each other; which forces being unknown, philosophers have hitherto made their attempts upon nature in vain." The same thought is at a later period followed out further in one of the Queries at the end of the Opticks*. "Have not the small particles of bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the rays of light for reflecting, refracting and inflecting them, but also upon one another for producing a great part of the phenomena of nature?" And a little further on he proceeds to apply this expressly to chemical changes. "When Salt of Tartar runs *per deliquium* [or as we now express it, deliquesces] is not this done by an *attraction* between the particles of the Salt of Tartar and the particles of the water which float in the air in the form of vapours? And why does not common salt, or saltpetre, or vitriol, run *per deliquium*, but for want of such an attraction? or why does not Salt of Tartar draw more water out of the

* Query 31.

air than in a certain proportion to its quantity, but for want of an attractive force after it is saturated with water?" He goes on to put a great number of similar cases, all tending to the same point, that chemical combinations cannot be conceived in any other way than as an attraction of particles.

9. Succeeding speculators in his school attempted to follow out this view. Dr. Frend, of Christ Church, in 1710, published his *Prælectiones Chymicæ, in quibus omnes fere Operationes Chymicæ ad vera Principia ex ipsius Naturæ Legibus rediguntur. Oxonii habitæ.* This book is dedicated to Newton, and in the dedication, the promise of advantage to chemistry from the influence of the Newtonian discoveries is spoken of somewhat largely,—much more largely, indeed, than has yet been justified by the sequel. After declaring in strong terms that the only prospect of improving science consists in following the footsteps of Newton, the author adds, "That force of attraction, of which you first so successfully traced the influence in the heavenly bodies, operates in the most minute corpuscles, as you long ago hinted in your *Principia*, and have lately plainly shown in your *Opticks;* and this force we are only just beginning to perceive and to study. Under these circumstances I have been desirous of trying what is the result of this view in chemistry." The work opens formally enough, with a statement of general mechanical principles, of which the most peculiar are these:—That there exists an attractive force by which particles when at very small distances from each other, are drawn together;—that this force is different, according to the different figure and density of the particles;—that the force may be greater on one side of a particle than on the other;—that the force by which particles cohere together arises from attraction, and is variously modi-

fied according to the quantity of contacts." But these principles are not applied in any definite manner to the explanation of specific phenomena. He attempts, indeed, the question of special solvents*. Why does *aqua fortis* dissolve silver and not gold, while *aqua regia* dissolves gold and not silver? which, he says, is the most difficult question in chemistry, and which is certainly a fundamental question in the formation of chemical theory. He solves it by certain assumptions respecting the forces of attraction of the particles, and also the diameter of the particles of the acids and the pores of the metals, all which suppositions are gratuitous.

10. We may observe further, that by speaking, as I have stated that he does, of the figure of particles, he mixes together the assumption of the last section with the one which we are considering in this. This combination is very unphilosophical, or, to say the least, very insufficient, since it makes a new hypothesis necessary. If a body be composed of cubical particles, held together by their mutual attraction, by what force are the parts of each cube held together? In order to understand their structure, we are obliged again to assume a cohesive force of the second order, binding together the particles of each particle. And therefore Newton himself says†, very justly, "The parts of all homogeneal hard bodies which fully touch each other, stick together very strongly: and for explaning how this is, some have invented hooked atoms, *which is begging the question.*" For (he means to imply,) how do the parts of the hook stick together?

The same remark is applicable to all hypotheses in which particles of a complex structure are assumed as the constituents of bodies: for while we suppose bodies and their known properties to result from the mutual

* P. 54. † *Opticks*, p. 364.

actions of these particles, we are compelled to suppose the parts of each particle to be held together by forces still more difficult to conceive, since they are disclosed only by the properties of these particles, which as yet are unknown. Yet Newton himself has not abstained from such hypotheses: thus he says*, "A particle of a salt may be compared to a chaos, being dense, hard, dry, and earthy in the center, and moist and watery in the circumference."

Since Newton's time the use of the term *attraction*, as expressing the cause of the union of the chemical elements of bodies, has been familiarly continued; and has, no doubt, been accompanied in the minds of many persons with an obscure notion that chemical attraction is, in some way, a kind of mechanical attraction of the particles of bodies. Yet the doctrine that chemical "attraction" and mechanical attraction are forces of the same kind has never, so far as I am aware, been worked out into a system of chemical theory; nor even applied with any distinctness as an explanation of any particular chemical phenomena. Any such attenpt, indeed, could only tend to bring more clearly into view the entire inadequacy of such a mode of explanation. For the leading phenomena of chemistry are all of such a nature that no mechanical combination can serve to express them, without an immense accumulation of additional hypotheses. If we take as our problem the changes of colour, transparency, texture, taste, odour, produced by small changes in the ingredients, how can we expect to give a mechanical account of these, till we can give a mechanical account of colour, transparency, texture, taste, odour, themselves? And if our mechanical hypothesis of the elementary constitution of bodies does not explain *such* phenomena as those changes, what can it

* *Opticks*, p. 362.

explain, or what can be the value of it? I do not here insist upon a remark which will afterwards come before us, that even crystalline form, a phenomenon of a far more obviously mechanical nature than those just alluded to, has never yet been in any degree explained by such assumptions as this, that bodies consist of elementary particles exerting forces of the same nature as the central forces which we contemplate in Mechanics.

When therefore Newton asks, "When some stones, as spar of lead, dissolved in proper menstruums, become salts, do not these things show that salts are dry earth and watery acid united by *attraction?*" we may answer, that this mode of expression appears to be intended to identify chemical combination with mechanical attraction;—that there would be no objection to any such identification, if we could, in that way, explain, or even classify well, a collection of chemical facts; but that this has never yet been done by the help of such expressions. Till some advance of this kind can be pointed out, we must necessarily consider the power which produces chemical combination as a peculiar principle, a special relation of the elements, not rightly expressed in mechanical terms. And we now proceed to consider this relation under the name by which it is most familiarly known.

CHAPTER II.

ESTABLISHMENT AND DEVELOPMENT OF THE IDEA OF CHEMICAL AFFINITY.

1. THE earlier chemists did not commonly involve themselves in the confusion into which the mechanical philosophers ran, of comparing chemical to mechanical forces. Their attention was engaged, and their ideas

were moulded, by their own pursuits. They saw that the connexion of elements and compounds with which they had to deal, was a peculiar relation which must be studied directly; and which must be understood, if understood at all, in itself, and not by comparison with a different class of relations. At different periods of the progress of chemistry, the conception of this relation, still vague and obscure, was expressed in various manners; and at last this conception was clothed in tolerably consistent phraseology, and the principles which it involved were, by the united force of thought and experiment, brought into view.

2. The power by which the elements of bodies combine chemically, being, as we have seen, a peculiar agency, different from mere mechanical connexion or attraction, it is desirable to have it designated by a distinct and peculiar name; and the term *Affinity* has been employed for that purpose by most modern chemists. The word "affinity" in common language means, sometimes resemblance, and sometimes relationship and ties of family. It is from the latter sense that the metaphor is borrowed when we speak of "chemical affinity." By the employment of this term we do not indicate resemblance, but disposition to unite. Using the word in a common unscientific manner, we might say that chlorine, bromine, and iodine, have a great natural affinity with each other, for there are considerable resemblances and analogies among them; but these bodies have very little *chemical* affinity for each other. The use of the word in the former sense, of resemblance, can be traced in earlier chemists; but it does not appear to have acquired its peculiar chemical meaning till after Boerhaave's time. Boerhaave, however, is the writer in whom we first find a due apprehension of the peculiarity and importance of the Idea which it now expresses.

When we make a chemical solution*, he says, not only are the particles of the dissolved body separated from each other, but they are closely united to the particles of the solvent. When *aqua regia* dissolves gold, do you not see, he says to his hearers, that there must be between each particle of the solvent and of the metal, a mutual virtue by which each loves, unites with, and holds the other (*amat, unit, retinet*)? The opinion previously prevalent had been that the solvent merely separates the parts of the body dissolved: and most philosophers had conceived this separation as performed by mechanical operations of the particles, resembling, for instance, the operation of wedges breaking up a block of timber. But Boerhaave forcibly and earnestly points out the insufficiency of the conception. This, he says, does not account for what we see. We have not only a separation, but a new combination. There is a force by which the particles of the solvent associate to themselves the parts dissolved, not a force by which they repel and dissever them. We are here to imagine not mechanical action, not violent impulse, not antipathy, but love, at least if love be the desire of uniting. (Non igitur hic etiam actiones mechanicæ, non propulsiones violentæ, non inimicitiæ cogitandæ, sed amicitiæ, si amor dicendus copulæ cupido.) The novelty of this view is evidenced by the mode in which he apologizes for introducing it. "Fateor, paradoxa hæc assertio." To Boerhaave, therefore, (especially considering his great influence as a teacher of chemistry,) we may assign the merit of first diffusing a proper view of Chemical Affinity as a peculiar force, the origin of almost all chemical changes and operations.

3. To Boerhaave is usually assigned also the credit of introducing the *word* "affinity" among chemists; but

* *Elementa Chemiæ.* Lugd. Bat. 1732, p. 677.

I do not find that the word is often used by him in this sense; perhaps not at all*. But however this may be, the term is, on many accounts well worthy to be preserved, as I shall endeavour to show. Other terms were used in the same sense during the early part of the eighteenth century. Thus when Geoffroy, in 1718, laid before the Academy of Paris his Tables of Affinities, which perhaps did more than any other event to fix the Idea of Affinity, he termed them "Tables of the Relations of Bodies;" "*Tables des Rapports:*" speaking however, also, of their "disposition to unite," and using other phrases of the same import.

The term *attraction*, having been recommended by Newton as a fit word to designate the force which produces chemical combination, continued in great favour in England, where the Newtonian philosophy was looked upon as applicable to every branch of science. In France, on the contrary, where Descartes still reigned triumphant, "attraction," the watch-word of the enemy, was a sound never uttered but with dislike and suspicion. In 1718 (in the notice of Geoffroy's Tables,) the Secretary of the Academy, after pointing out some of the peculiar circumstances of chemical combinations, says, "Sympathies and attractions would suit well here, if

* See Dumas, *Leçons de Phil. Chim.*, p. 364. Rees' *Cyclopædia*, Art. Chemistry. In the passage of Boerhaave to which I refer above, *affinitas* is rather opposed to, than identified with, chemical combination. When, he says, the parts of the body to be dissolved are dissevered by the solvent, why do they remain united to the particles of the solvent, and why do not rather both the particles of the solvent and of the dissolved body collect into homogeneous bodies by their *affinity?* "denuo se affinitate suæ naturæ colligant in corpora homogenea?" And the answer is, because they possess another force which counteracts this affinity of homogeneous particles, and makes compounds of different elements. Affinity, in chemistry, now means the tendency of *different* kinds of matter to unite: but it appears, as I have said, to have acquired this sense since Boerhaave's time.

there were such things." "Les sympathies, les attractions conviendroient bien ici, si elles étaient quelque chose." And at a later period, in 1731, having to write the *éloge* of Geoffroy after his death, he says, "He gave, in 1718, a singular system, and a Table of *Affinities*, or Relations of the different substances in chemistry. These affinities gave uneasiness to some persons, who feared that they were *attractions in disguise*, and all the more dangerous in consequence of the seductive forms which clever people have contrived to give them. It was found in the sequel that this scruple might be got over."

This is the earliest published instance, so far as I am aware, in which the word "affinity" is distinctly used for the cause of chemical composition; and taking into account the circumstances, the word appears to have been adopted in France in order to avoid the word *attraction*, which had the taint of Newtonianism. Accordingly we find the word *affinité* employed in the works of French chemists from this time. Thus, in the *Transactions of the French Academy* for 1746, in a paper of Macquer's upon Arsenic, he says*, "On peut facilement rendre raison de ces phenomènes par le moyen des affinités que les différens substances qui entrent dans ces combinaisons, ont les uns avec les autres:" and he proceeds to explain the facts by reference to Geoffroy's Table. And in Macquer's *Elements of Chemistry*, which appeared a few years later, the "affinity of composition" is treated of as a leading part of the subject, much in the same way as has been practised in such books up to the present time. From this period, the word appears to have become familiar to all European chemists in the sense of which we are now speaking. Thus, in the year 1758, the Academy of Sciences at Rouen offered a prize for the best dissertation on Affinity.

* *A. P.* 1746, p. 201.

The prize was shared between M. Limbourg of Theux, near Liege, and M. Le Sage of Geneva*. About the same time other persons (Manherr†, Nicolai‡, and others) wrote on the same subject, employing the same name.

Nevertheless, in 1775, the Swedish chemist Bergman, pursuing still further this subject of Chemical Affinities, and the expression of them by means of Tables, returned again to the old Newtonian term; and designated the disposition of a body to combine with one rather than another of two others as *elective attraction.* And as his work on *Elective Attractions* had great circulation and great influence, this phrase has obtained a footing by the side of *Affinity*, and both one and the other are now in common use among chemists.

4. I have said above that the term *Affinity* is worthy of being retained as a technical term. If we use the word *attraction* in this case, we identify or compare chemical with mechanical attraction; from which identification and comparison, as I have already remarked, no one has yet been able to extract the means of expressing any single scientific truth. If such an identification or comparison be not intended, the use of the same word in two different senses can only lead to confusion; and the proper course, recommended by all the best analogies of scientific history, is to adopt a peculiar term for that peculiar relation on which chemical composition depends. The word *affinity*, even if it were not rigorously proper according to its common meaning, still, being simple, familiar, and well established in this very usage, is much to be preferred before any other.

But further, there are some analogies drawn from

* Thomson's *Chemistry*, III. 10. Limbourg's Dissertation was published at Liege, in 1761; and Le Sage's at Geneva.

† *Dissertatio de Affinitate Corporum.* Vindob. 1762.

‡ *Progr.* I. II. *de Affinitate Corporum Chimica.* Jen. 1775, 1776.

the common meaning of this word, which appear to recommend it as suitable for the office which it has to discharge. For common mechanical attractions and repulsions, the forces by which one body considered as a *whole* acts upon another external to it, are, as we have said, to be distinguished from those more intimate ties by which the *parts* of each body are held together. Now this difference is implied, if we compare the former relations, the attractions and repulsions, to alliances and wars between states, and the latter, the internal union of particles, to those bonds of affinity which connect the citizens of the same state with one another, and especially to the ties of family. We have seen that Boerhaave compares the union of two elements of a compound to their marriage; "we must allow," says an eminent chemist of our own time*, "that there is some truth in this poetical comparison." It contains this truth,—that the two become one to most intents and purposes, and that the unit thus formed (the family) is not a mere juxtaposition of the component parts. And thus the Idea of Affinity as the peculiar principle of chemical composition, is established among chemists, and designated by a familiar and appropriate name.

5. *Analysis is possible.*—We must, however, endeavour to obtain a further insight into this Idea, thus fixed and named. We must endeavour to extricate, if not from the Idea itself, from the processes by which it has obtained acceptation and currency among chemists, some principles which may define its application, some additional specialities in the relations which it implies. This we shall proceed to do.

The Idea of Affinity, as already explained, implies a disposition to combine. But this combination is to be understood as admitting also of a possibility of separa-

* Dumas, *Leçons de Phil. Chim.*, p.363.

tion. Synthesis implies Analysis as conceivable: or to recur to the image which we have already used, Divorce is possible when the Marriage has taken place.

That there is this possibility, is a conviction implied in all the researches of chemists, ever since the true notion of composition began to predominate in their investigations. One of the first persons who clearly expressed this conviction was Mayow, an English physician, who published his *Medico-Physical Tracts* in 1674. The first of them *De Sale-Nitro et Spiritu Nitro-Aerio*, contains a clear enunciation of this principle. After showing how, in the combinations of opposite elements, as acid and alkali, their properties entirely disappear, and a new substance is formed not at all resembling either of the ingredients, he adds*, "Although these salts thus mixed appear to be destroyed, it is still possible for them to be separated from each other, with their powers still entire." He proceeds to exemplify this, and illustrates it by the same image which I have already alluded to: "Salia acida a salibus volatilibus discedunt, ut cum sale fixo tartari, tanquam *sponso* magis idoneo, *conjugium* strictius ineunt." This idea of a synthesis which left a complete analysis still possible, was opposed to a notion previously current, that when two heterogeneous bodies united together and formed a third body, the two constituents were entirely destroyed, and the result formed out of their ruins†. And this conception of synthesis and analysis, as processes which are possible successively and alternately, and each of which supposes the possibility of the other, has been the fundamental and regulative principle of the operations and speculations of analytical chemistry from the time of Mayow to the present day.

6. *Affinity is elective.*—When the idea of chemical

* Cap. XIV., p. 233. † Thomson's *Chemistry*, III. 8.

affinity, or disposition to unite, was brought into view by the experiments and reasonings of chemists, they found it necessary to consider this disposition as elective;—each element *chose* one rather than another of the elements which were presented to it, and quitted its union with one to unite with another which it preferred. This has already appeared in the passage just quoted from Mayow. He adds in the same strain, "I have no doubt that fixed salts choose one acid rather than another, in order that they may coalesce with it in a more intimate union."—"Nullus dubito salia fixa acidum unum præ aliis *eligere*, ut cum eodem arctiore unione coalescant." The same thought is expressed and exemplified by other chemists: they notice innumerable cases in which, when an ingredient is combined with a liquid, if a new substance be immersed which has a greater affinity for the liquid, the liquid combines with the new substance by election, and the former ingredient is *precipitated*. Thus Stahl says*, "In spirit of nitre dissolve silver; put in copper and the silver is thrown down; put in iron and the copper goes down; put in zinc, the iron precipitates; put in volatile alkali, the zinc is separated; put in fixed alkali, the volatile quits its hold."—As may be seen in this example, we have in such cases, not only a preference, but a long gradation of preferences. The spirit of nitre will combine with silver, but it prefers copper; prefers iron more; zinc still more; volatile alkali yet more; fixed alkali the most.

The same thing was proved to obtain with regard to each element; and when this was ascertained, it became the object of chemists to express these degrees of preference, by lists in which substances were arranged according to their disposition to unite with another substance. In this manner was formed Geoffroy's Table of Affinities

* *Zymotechnia*, 1697, p. 117.

(1718), which we have already mentioned. This Table was further improved by other writers, as Gellert (1751) and Limbourg (1761). Finally Bergman improved these Tables still further, taking into account not only the order of affinities of each element for others, but the *sum* of the tendencies to unite of each two elements, which sum, he held, determined the resulting combination when several elements were in contact with each other.

7. As we have stated in the History*, when the doctrine of elective affinities had assumed this very definite and systematic form, it was assailed by Berthollet, who maintained, in his *Essai de Statique Chimique*, (1803,) that chemical affinities are *not* elective:—that, when various elements are brought together, their combinations do not depend upon the kind of elements alone, but upon the quantity of each which is present, that which is most abundant always entering most largely into the resulting compounds. It may seem strange that it should be possible, at so late a period of the science, to throw doubt upon a doctrine which had presided over and directed its progress so long. Proust answered Berthollet, and again maintained that chemical affinity is elective. I have, in the History, given the judgment of Berzelius upon this controversy. "Berthollet," he says, "defended himself with an acuteness which makes the reader hesitate in his judgment; but the great mass of facts finally decided the point in favour of Proust." I may here add the opinion pronounced upon this subject by Dr. Turner†. "Bergman erred in supposing the result of the chemical action to be in every case owing to elective affinity [for this power is modified in its effects by various circumstances]: but

* *Hist. Ind. Sci.*, B. XIV. c. iii.

† *Chemistry*, p. 199. 6th edition.

Berthollet ran into the opposite extreme in declaring that the effects formerly ascribed to that power are never produced by it. That chemical attraction is exerted between different bodies with different degrees of energy, is, I apprehend, indisputable." And he then proceeds to give many instances of differences in affinity which cannot be accounted for by the operation of any modifying causes. Still more recently, M. Dumas has taken a review of this controversy; and, speaking with enthusiasm of the work of Berthollet, as one which had been of inestimable service to himself in his early study of chemistry, he appears at first disposed to award to him the victory in this dispute. But his final verdict leaves undamaged the general principle now under our consideration, that chemical affinity is elective. "For my own part," he says*, "I willingly admit the notions of Berthollet when we have to do with acids or with bases, of which the energy is nearly equal: but when bodies endued with very energetic affinities are in presence of other bodies of which the affinities are very feeble, I propose to adopt the following rule: In a solution, everything remaining dissolved, the strong affinities satisfy themselves, leaving the weak affinities to arrange matters with one another. The strong acids take the strong bases, and the weak acids can only unite with the weak bases. The known facts are perfectly in accordance with this practical rule." It is obvious that this recognition of a distinction between *strong* and *weak* affinities, which operates to such an extent as to determine entirely the result, is a complete acknowledgement of the elective nature of affinity, as far as any person acquainted with chemical operations could contend for it. For it must be allowed by all, that solubility, and other collateral circumstances, influence the course of

* *Leçons de Philosophie Chimique*, p. 386.

chemical combinations, since they determine whether or not there shall take place that contact of elements without which affinity cannot possibly operate.

8. *Affinity is Definite as to quantity.*—In proportion as chemists obtained a clearer view of the products of the laboratory as results of the composition of elements, they saw more and more clearly that these results were definite; that one element not only preferred to combine with another of a certain kind, but also would combine with it to a certain extent and no further, thus giving to the result not an accidental and variable, but a fixed and constant character. Thus salts being considered as the result of the combination of two opposite principles, acid and alkali, and being termed *neutral* when these principles exactly balanced each other, Rouelle (who was Royal Professor at Paris in 1742,) admits of neutral salts with excess of acid, neutral salts with excess of base, and perfect neutral salts. Beaume maintained* against him that there were no salts except those perfectly neutral, the other classes being the results of mixture and imperfect combination. But this question was not adequately treated till chemists made every experiment with the balance in their hands. When this was done, they soon discovered that, in each neutral salt, the proportional weights of the ingredients which composed it were always the same. This was ascertained by Wenzel, whose *Doctrine of the Affinities of Bodies* appeared in 1777. He not only ascertained that the proportions of elements in neutral chemical compounds are definite, but also that they are reciprocal; that is, that if A, a certain weight of a certain acid, neutralize *m*, a certain weight of a certain base, and B, a certain weight of a certain other acid, neutralize *n*, a certain weight of a certain other base; the compound of a A and *n* will also

* Dumas, *Phil. Chim.*, p. 198.

be neutral; as also that of B and *m*. The same views were again presented by Richter in 1792, in his *Principles of the Measure of Chemical Elements.* And along with these facts, that of the combination of elements in multiple proportions being also taken into account, the foundations of the Atomic Theory were laid; and that Theory was propounded in 1803 by Mr. Dalton. That theory, however, rests upon the Idea of Substance, as well as upon that Idea of Chemical Affinity which we are here considering; and the discussion of its evidence and truth must be for the present deferred.

9. The two principles just explained,—that affinity is definite as to the kind, and as to the quantity of the elements which it unites,—have here been stated as results of experimental investigation. That they could never have been clearly understood, and therefore never firmly established, without laborious and exact experiments, is certain; but yet we may venture to say that being once fully known, they possess an evidence beyond that of mere experiment. For how, in fact, can we conceive combinations, otherwise than as definite in kind and quantity? If we were to suppose each element ready to combine with any other indifferently, and indifferently in any quantity, we should have a world in which all would be confusion and indefiniteness. There would be no fixed kinds of bodies; salts, and stones, and ores, would approach to and graduate into each other by insensible degrees. Instead of this, we know that the world consists of bodies distinguishable from each other by definite differences, capable of being classified and named, and of having general propositions asserted concerning them. And as we cannot conceive a world in which this should not be the case, it would appear that we cannot conceive a state of things in which the laws of the combination of elements should not be of that

definite and measured kind which we have above asserted.

This will, perhaps, appear more clearly by stating our fundamental convictions respecting chemical composition in another form, which I shall, therefore, proceed to do.

10. *Chemical Composition determines Physical Properties.*—However obscure and incomplete may be our conception of the internal powers by which the ultimate particles of bodies are held together, it involves, at least, this conviction:—that these powers are what determine bodies to *be* bodies, and therefore contain the reason of all the properties which, as bodies, they possess. The forces by which the particles of a body are held together, also cause it to be hard or soft, heavy or light, opake or transparent, black or red; for if these forces are not the cause of these peculiarities, what can be the cause? By the very supposition which we make respecting these forces, they include all the relations by which the parts are combined into a whole, and therefore they, and they only, must determine all the attributes of the whole. The foundation of all our speculations respecting the intimate constitution of bodies must be this principle, that their composition determines their properties.

Accordingly we find our chemists reasoning from this principle with great confidence, even in doubtful cases. Thus Davy, in his researches concerning the diamond, says: "That some chemical difference must exist between the hardest and most beautiful of the gems and charcoal, between a non-conductor and a conductor of electricity, it is scarcely possible to doubt: and it seems reasonable to expect that a very refined or perfect chemistry will confirm *the analogies of nature;* and show that bodies cannot be the same in their composition or chemical nature, and yet totally different in their chemical properties." It is obvious that the principle here assumed is so far from being a mere result of experience, that it is here appealed to to prove that all previous results of experience on this subject must be incomplete and inaccurate; and that there must be some chemical difference between charcoal and diamond, though none had hitherto been detected.

11. In what manner, according to what rule, the chemical composition shall determine the kind of the substance, we cannot reasonably expect to determine by mere conjecture or assumption, without a studious examination of natural bodies and artificial compounds. Yet even in the most recent times, and among men of science, we find that an assumption of the most arbitrary character has in one case been mixed up with this indisputable principle, that the elementary composition determines the kind of the substance. In the classification of minerals, one school of mineralogists have rightly taken it as their fundamental principle that the chemical composition shall decide the position of the mineral in the system. But they have appended to this principle, arbitrarily and unjustifiably, the maxim that the element which is *largest in quantity* shall fix the class of the substance. To make such an assumption is to renounce, at once, all hope of framing a system which shall be governed by the resemblances of the things classified; for how can we possibly know beforehand that fifty-five per cent. of iron shall give a substance its predominant properties, and that forty-five per cent. shall not? Accordingly, the systems of mineralogical arrangement which have been attempted in this way, (those of Haüy, Phillips, and others,) have been found inconsistent with themselves, ambiguous, and incapable of leading to any general truths.

12. *Chemical Composition and Crystalline Form cor-*

respond.—Thus the physical properties of bodies depend upon their chemical composition, but in a manner which a general examination of bodies with reference to their properties and their composition can alone determine. We may, however, venture to assert further, that the more definite the properties are, the more distinct may we expect to find this dependence. Now the most definite of the properties of bodies are those constant properties which involve relations of space; that is, their figure. We speak not, however, of that external figure, derived from external circumstances, which, so far from being constant and definite, is altogether casual and arbitrary; but of that figure which arises from their internal texture, and which shows itself not only in the regular forms which they spontaneously assume, but in the disposition of the parts to separate in definite directions, and no others. In short, the most definite of the properties of perfect chemical compounds is their *crystalline structure;* and therefore it is evident that the crystalline structure of each body, and the forms which it affects, must be in a most intimate dependence upon its chemical composition.

Here again we are led to the brink of another theory;—that of crystalline structure, which has excited great interest among philosophers ever since the time of Haüy. But this theory involves, besides that idea of chemical composition with which we are here concerned, other conceptions, which enter into the relations of figure. These conceptions, governed principally by the idea of Symmetry, must be unfolded and examined before we can venture to discuss any theory of crystallization: and we shall proceed to do this as soon as we have first duly considered the Idea of Substance and its consequences.

CHAPTER III.

OF THE IDEA OF SUBSTANCE.

1. *Axiom of the Indestructibility of Substance.*—WE now come to an Idea of which the history is very different from those of which we have lately been speaking. Instead of being gradually and recently brought into a clear light, as has been the case with the Ideas of Polarity and Affinity, the Idea of Substance has been entertained in a distinct form from the first periods of European speculation. That this is so, is proved by our finding a principle depending upon this idea current as an axiom among the early philosophers of Greece:—namely, that *nothing can be produced out of nothing*. Such an axiom, more fully stated, amounts to this: that the substance of which a body consists is incapable of being diminished (and consequently incapable of being augmented) in quantity, whatever apparent changes it may undergo. Its form, its distribution, its qualities, may vary, but the substance itself is identically the same under all these variations.

The axiom just spoken of was the great principle of the physical philosophy of the Epicurean school, as it must be of every merely material philosophy. The reader of Lucretius will recollect the emphasis with which it is repeatedly asserted in his poem:

E nilo nil gigni, in nilum nil posse reverti;
Nought comes of nought, nor ought returns to nought.

Those who engaged in these early attempts at physical speculation were naturally much pleased with the clearness which was given to their notions of change, composition, and decomposition, by keeping steadily hold of the Idea of Substance, as marked by this fundamental axiom. Nor has its authority ever ceased to be acknowledged.

A philosopher was asked*, What is the weight of smoke? He answered, "Subtract the weight of the ashes from the weight of the wood which is burnt, and you have the weight of the smoke." This reply would be assented to by all; and it assumes as incontestable that even under the action of fire, the material, the substance, does not perish, but only changes its form.

This principle of the indestructibility of substance might easily be traced in many reasonings and researches, ancient and modern. For instance, when the chemist works with the *retort*, he places the body on which he operates in one part of an inclosed cavity, which, by its bendings and communications, separates at the same time that it confines, the products which result from the action of fire: and he assumes that this process is an analysis of the body into its ingredients, not a creation of anything which did not exist before, or a destruction of anything which previously existed. And he assumes further, that the total quantity of the substance thus analyzed is the sum of the quantities of its ingredients. This principle is the very basis of chemical speculation, as we shall hereafter explain more fully.

2. *The Idea of Substance.*—The axiom above spoken of depends upon the Idea of Substance, which is involved in all our views of external objects. We unavoidably assume that the qualities and properties which we observe are properties of *things*;—that the adjective implies a substantive;—that there is, besides the external characters of things, something *of which* they are the characters. An apple which is red, and round, and hard, is not merely redness, and roundness, and hardness: these circumstances may all alter while the apple remains the same apple. Behind or under the appearances which we see, we conceive something of which we think; or, to use the

* Kant, *Kritik. der R. V.*, p. 167.

metaphor which obtained currency among the ancient philosophers, the attributes and qualities which we observe are supported by and inherent in something: and this something is hence called a *substratum* or *substance*,—that which stands beneath the apparent qualities and supports them.

That we have such an *Idea*, using the term "Idea" in the sense in which I have employed it throughout these disquisitions, is evident from what has been already said. The axiom of the indestructibility of substance proves the existence of the Idea of Substance, just as the Axioms of Geometry and Arithmetic prove the existence of the Ideas of Space and Number. In the case of substance, as of space or number, the ideas cannot be said to be borrowed from experience, for the axioms have an authority of a far more comprehensive and demonstrative character than any which experience can bestow. The axiom that nothing can be produced from nothing and nothing destroyed, is so far from being a result of experience, that it is apparently contradicted by the most obvious observation. It has, at first, the air of a paradox; and by those who refer to it, it is familiarly employed to show how fallacious common observation is. The assertion is usually made in this form;—that nothing is created and nothing annihilated, *notwithstanding* that the common course of our experience appears to show the contrary. The principle is not an empirical, but a necessary and universal truth;—is collected, not from the evidence of our senses, but from the operation of our ideas. And thus the universal and undisputed authority of the axiom proves the existence of the Idea of Substance.

3. *Locke's Denial of the Idea of Substance.*—I shall not attempt to review the various opinions which have been promulgated respecting this Idea: but it may be

worth our while to notice briefly the part which it played in the great controversy concerning the origin of our ideas which Locke's *Essay* occasioned. Locke's object was to disprove the existence of all ideas not derived from Sensation or Reflection: and since the idea of substance as distinct from external qualities, is manifestly not derived directly from sensation, nor by any very obvious or distinct process from reflection, Locke was disposed to exclude the idea as much as possible. Accordingly, in his argumentation against Innate Ideas*, he says plainly, "the idea of substance, which we neither have nor can have by sensation or reflection." And the inference which he draws is, "that we have no such clear idea at all." What then, it may be asked, do we mean by the word *substance?* This also he answers, though somewhat strangely, "We signify nothing by the word *substance*, but only an uncertain supposition of we know not what, *i. e.*, of something whereof we have no particular distinct positive idea, which we take to be the substratum, or support, of those ideas we know." That while he indulged in this tautological assertion of our ignorance and uncertainty, he should still have been compelled to acknowledge that the word substance had some meaning, and should have been driven to explain it by the identical metaphors of "substratum" and "support," is a curious proof how impossible it is entirely to reject this idea.

But as we have already seen, the supposition of the existence of substance is so far from being uncertain, that it carries with it irresistible conviction, and substance is necessarily conceived as something which cannot be produced or destroyed. It may be easily supposed, therefore, that when the controversy between Locke and his assailants came to this point, he would be in some difficulty.

* *Essay*, B. I. ch. iv. s. 18.

And, indeed, though with his accustomed skill in controversy, he managed to retain a triumphant tone, he was driven from his main points. Thus he repels the charge that he took the being of substance to be doubtful*. He says, "Having everywhere affirmed and built upon it that man is a substance, I cannot be supposed to question or doubt of the being of substance, till I can question or doubt of my own being." He attempts to make a stand by saying that *being* of things does not depend upon our *ideas;* but if he had been asked how, without having an *idea* of substance, he *knew* substance *to be*, it is difficult to conceive what answer he could have made. Again, he had said that our idea of substance arises from our "accustoming ourselves to suppose" a substratum of qualities. Upon this his adversary, Bishop Stillingfleet, very properly asks, Is this custom grounded upon true reason or no? To which Locke replies, that it is grounded upon this: That we cannot conceive how simple ideas of sensible qualities should subsist alone; and therefore we suppose them to exist in, and to be supported by some common subject, which support we denote by the name substance. Thus he allows, not only that we necessarily assume the reality of substance, but that we cannot conceive qualities without substance; which are concessions so ample as almost to include all that any advocate for the Idea of Substance need desire.

Perhaps Locke, and the adherents of Locke, in denying that we have an idea of substance in general, were latently influenced by finding that they could not, by any effort of mind, call up any *image* which could be considered as an image of substance in general. That in this sense we have no idea of substance, is plain enough; but in the same sense we have no idea of space in general, or of time, or number, or cause, or resemblance.

* *Essay*, B. II. ch. ii., and *First Letter to the Bishop of Worcester.*

Yet we certainly have such a power of representing to our minds space, time, number, cause, resemblance, as to arrive at numerous truths by means of such representations. These general representations I have all along called Ideas, nor can I discover any more appropriate word; and in this sense, we have also, as has now been shown, an Idea of Substance.

4. *Is all Material Substance heavy?*—The principle that the quantity of the substance of any body remains unchanged by our operations upon it, is, as we have said, of universal validity. But then the question occurs, how are we to ascertain the quantity of substance, and thus, to apply the principle in particular cases. In the case above mentioned, where smoke was to be weighed, it was manifestly assumed that the quantity of the substance might be known by its weight; and that the total quantity being unchanged, the total weight also would remain the same. Now on what grounds do we make this assumption? Is all material substance heavy? and if we can assert this to be so, on what grounds does the truth of the assertion rest? These are not idle questions of barren curiosity; for in the history of that science (Chemistry) to which the idea of substance is principally applicable, nothing less than the fate of a comprehensive and long established theory (the Phlogiston theory) depended upon the decision of this question. When it was urged that the reduction of a metal from a calcined to a metallic form could not consist in the addition of phlogiston, because the metal was lighter than the calx had been; it was replied by some, that this was not conclusive, for that phlogiston was a principle of levity, diminishing the weight of the body to which it was added. This reply was, however, rejected by all the sounder philosophers, and the force of the argument finally acknowledged. But why was this suggestion of a

substance having no weight, or having absolute levity, repudiated by the most reflective reasoners? It is assumed, it appears, that all matter must be heavy; what is the ground of this assumption?

The ground of such an assumption appears to be the following. Our idea of substance includes in it this: —that substance is a quantity capable of addition; and thus capable of making up, by composition, a sum equal to all its parts. But substance, and the quantity of substance, can be known to us only by its attributes and qualities. And the qualities which are capable constantly and indefinitely of increase and diminution by increase and diminution of the parts, must be conceived inseparable from the substance. For the qualities, if removable from the substance at all, must be removable by some operation performed upon the substance; and by the idea of substance, all such operations are only equivalent to separation, junction, and union of parts. Hence those characters which thus universally increase and diminish by addition and subtraction of the things themselves, belong to the substance of the things. They are measures of its quantity, and are not merely its separable qualities.

The weight of bodies is such a character. However we compound or divide bodies, we compound and divide their weight in the same manner. We may dismember a body into the minutest parts; but the sum of the weights of the parts is always equal to the whole weight of the body. The weight of a body can be in no way increased or diminished, except by adding something to it or taking something from it. If we bake a brick, we do not conceive that the change of colour or of hardness, implies that anything has been created or destroyed. It may easily be that the parts have only assumed a new arrangement; but if the brick have lost weight, we suppose that something (moisture for instance) has been removed elsewhere.

Thus weight is apprehended as essential to matter. In considering the dismemberment or analysis of bodies, we assume that there must be some criterion of the quantity of substance; and this criterion can possess no other properties than their weight possesses. If we assume an element which has no weight, or the weight of which is negative, as some of the defenders of phlogiston attempted to do, we put an end to all speculation on such subjects. For if weight is not the criterion of the quantity of one element, phlogiston for instance, why is weight the criterion of the quantity of any other element? We may, by the same right, assume any other real or imaginary element to have levity instead of gravity; or to have a peculiar intensity of gravity which makes its weight no index of its quantity. In short, if we do this, we deprive of all possibility of application our notions of element, analysis, and composition; and violate the postulates on which the questions are propounded which we thus attempt to decide.

We must, then, take a constant and quantitative property of matter, such as weight is, to be an index of the quantity of matter or of substance to which it belongs. I do not here speak of the question which has sometimes been proposed, whether the *weight* or the *inertia* of bodies be the more proper measure of the quantity of matter. For the measure of inertia is regulated by the same assumption as that of substance:—that the quantity of the whole must be equal to the quantity of all the parts: and inertia is measured by weight, for the same reason that substance is so.

Having thus established the certainty, and ascertained the interpretation of the fundamental principle which the Idea of Substance involves, we are prepared to consider its application in the science upon which it has a peculiar bearing.

CHAPTER IV

APPLICATION OF THE IDEA OF SUBSTANCE IN CHEMISTRY.

1. *A Body is Equal to the Sum of its Elements.*—FROM the earliest periods of chemistry the balance has been familiarly used to determine the proportions of the ingredients and of the compound; and soon after the middle of the last century, this practice was so studiously followed, that Wenzel and Richter were thereby led to the doctrine of Definite Proportions. But yet the full value and significance of the balance, as an indispensable instrument in chemical researches, was not understood till the gaseous, as well as solid and fluid ingredients were taken into the account. When this was done, it was found that the principle, that the whole is equal to the sum of its parts, of which, as we have seen, the necessary truth, in such cases, flows from the idea of substance, could be applied in the most rigorous manner. And conversely, it was found that by the use of the balance, the chemist could decide, in doubtful cases, which was a whole, and which were parts.

For chemistry considers all the changes which belong to her province as compositions and decompositions of elements; but still the question may occur, whether an observed change be the one or the other. How can we distinguish whether the process which we contemplate be composition or decomposition?—whether the new body be formed by addition of a new, or subtraction of an old element? Again; in the case of decomposition, we may inquire, What are the ultimate limits of our analysis? If we decompound bodies into others more and more simple, how far can we carry this succession of processes? How far can we proceed in the road of analysis? And in our actual course, what evidence have we that our progress, as far as it has gone, has carried us from the more complex to the more simple?

To this we reply, that the criterion which enables us to distinguish, decidedly and finally, whether our process have been a mere analysis of the proposed body into its ingredients, or a synthesis of some of them with some new element, is the principle stated above, that the weight of the whole is equal to the weight of all the parts. And no process of chemical analysis or synthesis can be considered complete till it has been verified by this fact;—by finding that the weight of the compound is the weight of its supposed ingredients; or, that if there be an element which we think we have detached from the whole, its loss is betrayed by a corresponding diminution of weight.

I have already noticed what an important part this principle has played in the great chemical controversy which ended in the establishment of the oxygen theory. The calcination of a metal was decided to be the union of oxygen with the metal, and not the separation of phlogiston from it, because it was found that in the process of calcination, the weight of the metal increased, and increased exactly as much as the weight of ambient air diminished. When oxygen and hydrogen were exploded together, and a small quantity of water was produced, it was held that this was really a synthesis of water, because, when very great care was taken with the process, the weight of the water which resulted was equal to the weight of the gases which disappeared.

2. *Lavoisier.*—It was when gases came to be considered as entering largely into the composition of liquid

and solid bodies, that extreme accuracy in weighing was seen to be so necessary to the true understanding of chemical processes. It was in this manner discovered by Lavoisier and his contemporaries that oxygen constitutes a large ingredient of calcined metals, of acids, and of water. A countryman of Lavoisier* has not only given most just praise to that great philosopher for having constantly tested all his processes by a careful and skilful use of the balance, but has also claimed for him the merit of having introduced the maxim, that in chemical operations nothing is created and nothing lost. But I think it is impossible to deny that this maxim is assumed in all the attempts at analysis made by his contemporaries, as well as by him. This maxim is indeed included in any clear notion of analysis: it could not be the result of the researches of any one chemist, but was the governing principle of the reasonings of all. Lavoisier, however, employed this principle with peculiar assiduity and skill. In applying it, he does not confine himself to mere additions and subtractions of the quantities of ingredients; but often obtains his results by more complex processes. In one of his investigations he says, "I may consider the ingredients which are brought together, and the result which is obtained as an algebrical equation; and if I successively suppose each of the quantities of this equation to be unknown, I can obtain its value from the rest: and thus I can rectify the experiment by the calculation, and the calculation by the experiment. I have often taken advantage of this method, in order to correct the first results of my experiments, and to direct me in repeating them with proper precautions."

The maxim, that the whole is equal to the sum of all its parts, is thus capable of most important and varied employment in chemistry. But it may be applied in

* M. Dumas, *Leçons de la Philosophie Chimique*. 1837. p. 157.

another form to the exclusion of a class of speculations which are often put forwards.

3. *Maxim respecting Imponderable Elements.*—Several of the phenomena which belong to bodies, as heat, light, electricity, magnetism, have been explained hypothetically by assuming the existence of certain fluids; but these fluids have never been shown to have weight. Hence such hypothetical fluids have been termed *imponderable elements*. It is however plain, that so long as these fluids appear to be without weight, they are not *elements* of bodies in the same sense as those elements of which we have hitherto been speaking. Indeed we may with good reason doubt whether those phenomena depend upon transferable fluids at all. We have seen strong reason to believe that light is not matter, but only motion; and the same thing appears to be probable with regard to heat. Nor is it at all inconceivable that a similar hypothesis respecting electricity and magnetism should hereafter be found tenable. Now if heat, light, and those other agents, be not matter, they are not *elements* in such a sense as to be included in the principle referred to above, That the body is equal to the sum of its elements. Consequently the maxim just stated, that in chemical operations nothing is created, nothing annihilated, does not apply to light and heat. They are not *things*. And whether heat can be produced where there was no heat before, and light struck out from darkness, the ideas of which we are at present treating do not enable us to say. In reasoning respecting chemical synthesis and analysis therefore, we shall only make confusion by attempting to include in our conception the light and heat which are produced and destroyed. Such phenomena may be very proper subjects of study, as indeed they undoubtedly are; but they cannot be studied to advantage by considering

them as sharing the nature of composition and decomposition.

Again: in all attempts to explain the processes of nature, the proper course is, first to measure the facts with precision, and then to endeavour to understand their cause. Now the facts of chemical composition and decomposition, the weights of the ingredients and of the compounds, are facts measurable with the utmost precision and certainty. But it is far otherwise with the light and heat which accompany chemical processes. When combustion, deflagration, explosion, takes place, how can we measure the light or the heat? Even in cases of more tranquil action, though we can apply the thermometer, what does the thermometer tell us respecting the *quantity* of the heat? Since then we have no measure which is of any value as regards such circumstances in chemical changes, if we attempt to account for these phenomena *on chemical principles*, we introduce, into investigations in themselves perfectly precise and mathematically rigorous, another class of reasonings, vague and insecure, of which the only possible effect is to vitiate the whole reasoning, and to make our conclusions inevitably erroneous.

We are led then to this maxim: that *imponderable fluids are* not *to be admitted as chemical elements of bodies**.

4. It appears, I think, that our best and most philo-

* Since we are thus warned by a sound view of the nature of science, from considering chemical affinity as having any hold upon imponderable elements, we are manifestly still more decisively prohibited from supposing mechanical impulse or pressure to have any effect upon such elements. To make this supposition, is to connect the most subtle and incorporeal objects which we know in nature by the most gross material ties. This remark seems to be applicable to M. Poisson's hypothesis that the electric fluid is retained at the surface of bodies by the pressure of the atmosphere.

sophical chemists have proceeded upon this principle in their investigations. In reasoning concerning the constitution of bodies and the interpretation of chemical changes, the attempts to include in these interpretations the heat or cold produced, by the addition or subtraction of a certain hypothetical "caloric," have become more and more rare among men of science. Such statements, and the explanations often put forwards of the light and heat which appear under various circumstances in the form of fire, must be considered as unessential parts of any sound theory. Accordingly we find Mr. Faraday gradually relinquishing such views. In January, 1834, he speaks generally of an hypothesis of this kind*. "I cannot refrain from recalling here the beautiful idea put forth, I believe by Berzelius, in his developement of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination are the consequence of the electric discharge which is at that moment taking place." But in April of the same year†, he observes, that in the combination of oxygen and hydrogen to produce water, electric powers to a most enormous amount are for the time active, but that the flame which is produced gives but feeble traces of such powers. "Such phenomena," therefore, he adds, "may not, cannot, be taken as evidences of the nature of the action; but are merely incidental results, incomparably small in relation to the forces concerned, and supplying no information of the way in which the particles are active on each other, or in which their forces are finally arranged."

In pursuance of this maxim, we must consider as an unessential part of the oxygen theory that portion of it, much insisted upon by its author at the time, in which when sulphur, for instance, combined with oxygen to

* *Researches*, 870. † *Ib.* 960.

produce sulphuric acid, the combustion was accounted for by means of the *caloric* which was supposed to be *liberated* from its combination with oxygen.

5. *Controversy of the Composition of Water.*—There is another controversy of our times to which we may with great propriety apply the maxim now before us. After the glory of having first given a true view of the composition of water had long rested tranquilly upon the names of Cavendish and Lavoisier, a claim was made in favour of James Watt as the real author of this discovery by his son, (Mr. J. Watt,) and his eulogist, (M. Arago*.) It is not to our purpose here to discuss the various questions which have arisen on this subject respecting priority of publication, and respecting the translation of opinions published at one time into the language of another period. But if we look at Watt's own statement of his views, given soon after those of Cavendish had been published, we shall perceive that it is marked by a violation of this maxim: we shall find that he does admit imponderable fluids as chemical elements; and thus shows a vagueness and confusion in his idea of chemical composition. With such imperfection in his views, it is not surprizing that Watt, not only did not anticipate, but did not apprehend quite precisely the discovery of Cavendish and Lavoisier. Watt's statement of his views is as follows†:—"Are we not authorized to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be

* Eloge de James Watt, *Annuaire du Bur. des Long.*, 1839.

† *Phil. Trans.*, 1784, p. 332.

only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?"

When we compare this doubtful and hypothetical statement, involving so much that is extraneous and heterogeneous, with the conclusion of Cavendish, in which there is nothing hypothetical or superfluous, we may confidently assent to the decision which has been pronounced by one* of our own time in favour of Cavendish. And we may with pleasure recognize, in this enlightened umpire, a due appreciation of the value of the maxim on which we are now insisting. "Cavendish," says Mr. Vernon Harcourt, "pared off from the hypotheses their theories of combustion, and *affinities of imponderable for ponderable matter*, as complicating chemical with physical considerations."

6. *Relation of Heat to Chemistry.*—But while we thus condemn the attempts to explain the thermotical phenomena of chemical processes by means of chemical considerations, it may be asked if we are altogether to renounce the hope of understanding such phenomena? It is plain, it may be said, that heat generated in chemical changes is always a very important

* The Rev. W. Vernon Harcourt, Address to the British Association, 1839.—Since the first edition of this work was published, and also since the second edition of the *History of the Inductive Sciences*, Mr. Watt's correspondence bearing upon the question of the Composition of Water has been published by Mr. Muirhead. I do not find, in this publication, any reason for withdrawing what I have stated in the text above: but with reference to the statement in the *History*, it appears that Mr. Cavendish's claim to the discovery was not uncontested in his own time. Mr. Watt had looked at the composition of water, as a problem to be solved, perhaps more distinctly than Mr. Cavendish had done; and he conceived himself wronged by Mr. Cavendish's putting forwards his experiment as the first solution of this problem.

circumstance, and can sometimes be measured, and perhaps reduced to laws; are we prohibited from speculating concerning the causes of such circumstances and such laws? And to this we reply, that we may properly attempt to connect chemical with thermotical processes, *so far as* we have obtained a clear and probable view of the nature of the thermotical processes. When our theory of Thermotics is tolerably complete and certain, we may with propriety undertake to connect it with our theory of Chemistry. But at present we are not far enough advanced in our knowledge of heat to make this attempt with any hope of success. We can hardly expect to understand the part which heat plays in the union of two bodies, when we cannot as yet comprehend in what manner it produces the liquefaction or vaporization of one body. We cannot look to account for Gay Lussac and Dalton's Law, that all gases expand equally by heat, till we learn how heat causes a gas to expand. We cannot hope to see the grounds of Dulong and Petit's Law, that the specific heat of all atoms is the same, till we know much more, not only about atoms, but about specific heat. We have as yet no thermotical theory which even professes to account for all the prominent facts of the subject*: and the theories which have been proposed are of the most diverse kind. Laplace assumes particles of bodies surrounded by atmospheres of caloric†; Cauchy makes heat consist in longitudinal vibrations of the ether of which transverse vibrations produce light: in Ampère's theory‡, heat consists in the vibrations of the particles of bodies. And so long as we have nothing more certain in our conceptions of heat than the alternative of these and other precarious hypotheses, how can we expect to arrive at any real knowledge, by connecting the results of such

* *Hist. Ind. Sci.*, B. x. c. 4. † *Ib.* ‡ *Ib.*

hypotheses with the speculations of Chemistry, of which science the theory is at least equally obscure?

The largest attempts at chemical theory have been made in the form of the Atomic Theory, to which I have just had occasion to allude. I must, therefore, before quitting the subject, say a few words respecting this theory.

Chapter V.

THE ATOMIC THEORY.

1. *The Atomic Theory considered on Chemical Grounds.*—We have already seen that the combinations which result from chemical affinity are definite, a certain quantity of one ingredient uniting, not with an uncertain, but with a certain quantity of another ingredient. But it was found, in addition to this principle, that one ingredient would often unite with another in different proportions, and that, in such cases, these proportions are multiples one of another. In the three salts formed by potassa with oxalic acid, the quantities of acid which combine with the same quantity of alkali are exactly in the proportion of the numbers 1, 2, 4. And the same rule of the existence of multiple proportions is found to obtain in other cases.

It is obvious that such results will be accounted for, if we suppose the base and the acid to consist each of definite equal particles, and that the formation of the salts above mentioned consists in the combination of one particle of the base with one particle of acid, with two particles of acid, and with four particles of acid, respectively. But further; as we have already stated, chemical affinity is not only definite, but reciprocal. The proportions of potassa and soda which form neutral salts being 590 and 391 in one case, they are so in all cases. These numbers represent the *proportions* of weight in which the two bases, potassa and soda, enter into analogous combinations; 590 of potassa is *equivalent* to 391 of soda. These facts with regard to combination are still expressed by the above supposition of equal particles, assuming that the weights of a particle of potassa and of soda are in the proportion of 590 to 391.

But we pursue our analysis further. We find that potassa is a compound of a metallic base, potassium, and of oxygen, in the proportion of 490 to 100; we suppose, then, that the particle of potassa consists of a particle of potassium and a particle of oxygen, and these latter particles, since we see no present need to suppose them divided, potassium and oxygen being simple bodies, we may call *atoms*, and assume to be indivisible. And by supposing all simple bodies to consist of such atoms, and compounds to be formed by the union of two, or three, or more of such atoms, we explain the occurrence of definite and multiple proportions, and we construct the Atomic Theory.

2. *Hypothesis of Atoms.*—So far as the assumption of such atoms as we have spoken of serves to express those laws of chemical composition which we have referred to, it is a clear and useful generalization. But if the Atomic Theory be put forwards (and its author, Dr. Dalton, appears to have put it forwards with such an intention,) as asserting that chemical elements are really composed of *atoms*, that is, of such particles not further divisible, we cannot avoid remarking, that for such a conclusion, chemical research has not afforded, nor can afford, any satisfactory evidence whatever. The smallest observable quantities of ingredients, as well as the largest, combine according to the laws of proportions

and equivalence which have been cited above. How are we to deduce from such facts any inference with regard to the existence of certain smallest possible particles? The Theory, when dogmatically taught as a physical truth, asserts that all observable quantities of elements *are* composed of proportional numbers of particles which can no further be subdivided; but all which observation teaches us is, that *if* there be such particles, they are smaller than the smallest observable quantities. In chemical experiment, at least, there is not the slightest positive evidence for the existence of such atoms. The assumption of *indivisible* particles, smaller than the smallest observable, which combine, particle with particle, will explain the phenomena; but the assumption of particles bearing this proportion, but *not* possessing the property of indivisibility, will explain the phenomena at least equally well. The decision of the question, therefore, whether the Atomic Hypothesis be the proper way of conceiving the chemical combinations of substances, must depend, not upon chemical facts, but upon our conception of substance. In this sense the question is an ancient and curious controversy, and we shall hereafter have to make some remarks upon it.

3. *Chemical Difficulties of the Hypothesis.*—But before doing this, we may observe that there is no small difficulty in reconciling this hypothesis with the facts of chemistry. According to the theory, all salts, compounded of an acid and a base, are analogous in their atomic constitution; and the number of atoms in one such compound being known or assumed, the number of atoms in other salts may be determined. But when we proceed in this course of reasoning to other bodies, as metals, we find ourselves involved in difficulties. The protoxide of iron is a base which, according to all analogy, must consist of one atom of iron and one of oxygen:

but the peroxide of iron is also a base, and it appears by the analysis of this substance that it must consist of *two-thirds* of an atom of iron and one atom of oxygen. Here, then, our indivisible atoms must be divisible, even upon chemical grounds. And if we attempt to evade this difficulty by making the peroxide of iron consist of two atoms of iron and three of oxygen, we have to make a corresponding alteration in the theoretical constitution of all bodies analogous to the protoxide; and thus we overturn the very foundation of the theory. Chemical facts, therefore, not only do not prove the Atomic Theory as a physical truth, but they are not, according to any modification yet devised of the theory, reconcileable with its scheme.

Nearly the same conclusions result from the attempts to employ the Atomic Hypothesis in expressing another important chemical law;—the law of the combinations of gases according to definite proportions of their volumes, experimentally established by Gay Lussac*. In order to account for this law, it has been very plausibly suggested that all gases, under the same pressure, contain an equal number of atoms in the same space; and that when they combine, they unite atom to atom. Thus one volume of chlorine unites with one volume of hydrogen, and form hydrochloric acid†. But then this hydrochloric acid occupies the space of the two volumes; and therefore the proper number of particles cannot be supplied, and the uniform distribution of atoms in all gases maintained, without dividing into two each of the compound particles, constituted of an atom of chlorine and an atom of hydrogen. And thus in this case, also, the Atomic Theory becomes untenable if it be understood to imply the indivisibility of the atoms.

In all these attempts to obtain a distinct physical

* *Hist. Ind. Sc.*, B. XIV. c. 8. † Dumas, *Phil. Chim.* 263.

conception of chemical union by the aid of the Atomic Hypothesis, the atoms are conceived to be associated by certain forces of the nature of mechanical attractions. But we have already seen* that no such mode of conception can at all explain or express the facts of chemical combination; and therefore it is not wonderful that when the Atomic Theory attempts to give an account of chemical relations by contemplating them under such an aspect, the facts on which it grounds itself should be found not to authorize its positive doctrines; and that when these doctrines are tried upon the general range of chemical observation, they should prove incapable of even expressing, without self-contradiction, the laws of phenomena.

4. *Grounds of the Atomic Doctrine.*—Yet the doctrine of atoms, or of substance as composed of indivisible particles, has in all ages had great hold upon the minds of physical speculators; nor would this doctrine ever have suggested itself so readily, or have been maintained so tenaciously, as the true mode of conceiving chemical combinations, if it had not been already familiar to the minds of those who endeavour to obtain a general view of the constitution of nature. The grounds of the assumption of the atomic structure of substance are to be found rather in the idea of substance itself, than in the experimental laws of chemical affinity. And the question of the existence of atoms, thus depending upon an idea which has been the subject of contemplation from the very infancy of philosophy, has been discussed in all ages with interest and ingenuity. On this very account it is unlikely that the question, so far as it bears upon chemistry, should admit of any clear and final solution. Still it will be instructive to look back at some of the opinions which have been delivered respecting this doctrine.

* See Chapter I. of this Book.

5. *Ancient Prevalence of the Atomic Doctrine.*—The doctrine that matter consists of minute, simple, indivisible, indestructible particles as its ultimate elements, has been current in all ages and countries, whenever the tendency of man to wide and subtle speculations has been active. I need not attempt to trace the history of this opinion in the schools of Greece and Italy. It was the leading feature in the physical tenets of the Epicureans, and was adopted by their Roman disciples, as the poem of Lucretius copiously shows us. The same tenet had been held at still earlier periods, in forms more or less definite, by other philosophers. It is ascribed to Democritus, and is said to have been by him derived from Leucippus. But this doctrine is found also, we are told*, among the speculations of another intellectual and acute race, the Hindoos. According to some of their philosophical writers, the ultimate elements of matter are atoms, of which it is proved by certain reasonings, that they are each one-sixth of one of the motes that float in the sunbeam.

This early prevalence of controversies of the widest and deepest kind, which even in our day remain undecided, has in it nothing which need surprize us; or, at least, it has in it nothing which is not in conformity with the general course of the history of philosophy. As soon as any ideas are clearly possessed by the human mind, its activity and acuteness in reasoning upon them are such, that the fundamental antitheses and ultimate difficulties which belong to them are soon brought into view. The Greek and Indian philosophers had mastered completely the Idea of Space, and possessed the Idea of Substance in tolerable distinctness. They were, therefore, quite ready, with their lively and subtle minds, to discuss the question of the finite and infinite divisibility of matter,

* By Mr. Colebrook. *Asiatic Res.* 1824.

so far as it involved only the ideas of space and of substance, and this accordingly they did with great ingenuity and perseverance.

But the ideas of Space and of Substance are far from being sufficient to enable men to form a complete general view of the constitution of matter. We must add to these ideas, that of mechanical Force with its antagonist Resistance, and that of the Affinity of one kind of matter for another. Now the former of these ideas the ancients possessed in a very obscure and confused manner; and of the latter they had no apprehension whatever. They made vague assumptions respecting the impact and pressure of atoms on each other; but of their mutual attraction and repulsion they never had any conception, except of the most dim and wavering kind; and of an affinity different from mere local union they did not even dream. Their speculations concerning atoms, therefore, can have no value for us, except as a part of the history of science. If their doctrines appear to us to approach near to the conclusions of our modern philosophy, it must be because our modern philosophy is that philosophy which has not fully profited by the additional light which the experiments and meditations of later times have thrown upon the constitution of matter.

6. *Bacon.*—Still, when modern philosophers look upon the Atomic Theory of the ancients in a general point of view merely, without considering the special conditions which such a theory must fulfil, in order to represent the discoveries of modern times, they are disposed to regard it with admiration. Accordingly we find Francis Bacon strongly expressing such a feeling. The Atomic Theory is selected and dwelt upon by him as the chain which connects the best parts of the physical philosophy of the ancient and the modern world. Among his works is a remarkable dissertation *On the Philosophy of Democri-*

tus, Parmenides, and Telesius: the last mentioned of whom was one of the revivers of physical science in modern times. In this work he speaks of the atomic doctrine of Democritus as a favourable example of the exertions of the undisciplined intellect. "Hæc ipsa placita, quamvis paulo emendatiora, talia sunt qualia esse possunt illa quæ ab intellectu sibi permisso, nec continenter et gradatim sublevato, profecta videntur."—"These doctrines, thus [in an ancient fable] presented in a better form, are such glimpses of truth as can be obtained by the intellect left to its own natural impulses, and not ascending by successive and connected steps," [as the Baconian philosophy directs.] "Accordingly," he adds, "the doctrine of Atoms, from its going a step beyond the period in which it was advanced, was ridiculed by the vulgar, and severely handled in the disputations of the learned, notwithstanding the profound acquaintance with physical science by which its author was allowed to be distinguished, and from which he acquired the character of a magician."

"However," he continues, "neither the hostility of Aristotle, with all his skill and vigour in disputation, (though, like the Ottoman sultans, he laboured to destroy all his brother philosophers that he might rest undisputed master of the throne of science,) nor the majestic and lofty authority of Plato, could effect the subversion of the doctrine of Democritus. And while the opinions of Plato and Aristotle were rehearsed with loud declamation and professorial pomp in the schools, this of Democritus was always held in high honour by those of a deeper wisdom, who followed in silence a severer path of contemplation. In the days of Roman speculation it kept its ground and its favour; Cicero everywhere speaks of its author with the greatest praise; and Juvenal, who, like poets in general, probably expressed the prevailing judgment of his time, proclaims his merit as a noble exception to the general stupidity of his countrymen.

. . . . Cujus prudentia monstrat
Magnos posse viros et magna exempla daturos
Vervecum in patriâ crassoque sub aere nasci.

"The destruction of this philosophy was not effected by Aristotle and Plato, but by Genseric and Attila, and their barbarians. For then, when human knowledge had suffered shipwreck, those fragments of the Aristotelian and Platonic philosophy floated on the surface like things of some lighter and emptier sort, and so were preserved; while more solid matters went to the bottom, and were almost lost in oblivion."

7. *Modern Prevalence of the Atomic Doctrine.*—It is our business here to consider the doctrine of Atoms only in its bearing upon existing physical sciences, and I must therefore abstain from tracing the various manifestations of it in the schemes of hypothetical cosmologists;—its place among the *vortices* of Descartes, its exhibition in the *monads* of Leibnitz. I will, however, quote a passage from Newton to show the hold it had upon his mind.

At the close of his *Opticks* he says, "All these things being considered, it seems probable to me that God, in the beginning, formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportions to space, as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear or break in pieces; no ordinary power being able to divide what God had made one in the first creation. While the particles continue entire, they may compose bodies of one and the same nature and texture in all ages: but should they wear away or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn particles and fragments of particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And therefore that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compounded bodies being apt to break, not in the midst of solid particles, but where those particles are laid together and only touch in a few points."

We shall hereafter see how extensively the atomic doctrine has prevailed among still more recent philosophers. Not only have the chemists assumed it as the fittest form for exhibiting the principles of multiple proportions; but the physical mathematicians, as Laplace and Poisson, have made it the basis of their theories of heat, electricity, capillary action; and the crystallographers have been supposed to have established both the existence and the arrangement of such ultimate molecules.

In the way in which it has been employed by such writers, the hypothesis of ultimate particles has been of great use, and is undoubtedly permissible. But when we would assert this theory, not as a convenient hypothesis for the expression or calculation of the laws of nature, but as a philosophical truth respecting the constitution of the universe, we find ourselves checked by difficulties of reasoning which we cannot overcome, as well as by conflicting phenomena which we cannot reconcile. I will attempt to state briefly the opposing arguments on this question.

8. *Arguments for and against Atoms.*—The leading arguments on the two sides of the question, in their most general form, may be stated as follows:—

For the Atomic Doctrine.—The appearances which nature presents are compounded of many parts, but if we go on resolving the larger parts into smaller, and so on successively, we must at last come to something simple. For that which is compound can be so no otherwise than by composition of what is simple; and if we suppose all composition to be removed, which hypothetically we may do, there can remain nothing but a number of simple substances, capable of composition, but themselves not compounded. That is, matter being dissolved, resolves itself into atoms.

Against the Atomic Doctrine.—Space is divisible without limit, as may be proved by geometry; and matter occupies space, therefore matter is divisible without limit, and no portion of matter is *indivisible*, or an *atom*.

And to the argument on the other side just stated, it is replied that we cannot even hypothetically divest a body of composition, if by composition we mean the relation of point to point in space. However small be a particle, it is compounded of parts having relation in space.

The Atomists urge again, that if matter be infinitely divisible, a finite body consists of an infinite number of parts, which is a contradiction. To this it is replied, that the finite body consists of an infinite number of parts in the same sense in which the parts are infinitely small, which is no contradiction.

But the opponents of the Atomists not only rebut, but retort this argument drawn from the notion of infinity. Your atoms, they say, are indivisible by any finite force; therefore they are infinitely hard; and thus your finite particles possess infinite properties. To this the Atomists are wont to reply, that they do not mean the hardness of their particles to be infinite, but only so great as to resist all usual natural forces. But here it is plain that their position becomes untenable; for, in the first place, their assumption of this precise degree of hardness in the particles is altogether gratuitous; and in the next place, if it were granted, such particles are not atoms, since in the next moment the forces of nature may be augmented so as to divide the particle, though hitherto undivided.

Such are the arguments for and against the Atomic Theory in its original form. But when these atoms are conceived, as they have been by Newton, and commonly by his followers, to be solid, hard particles exerting attractive and repulsive forces, a new set of arguments come into play. Of these, the principal one may be thus stated: According to the Atomic Theory thus modified, the properties of bodies depend upon the attractions and repulsions of the particles. Therefore, among other properties of bodies, their hardness depends upon such forces. But if the hardness *of the bodies* depends upon the forces, the repulsion, for instance, of the particles, upon what does the hardness *of the particles* depend? what progress do we make in explaining the properties of bodies, when we assume the same properties in our explanation? and to what purpose do we assume that the particles *are* hard?

9. *Transition to Boscovich's Theory.*—To this difficulty it does not appear easy to offer any reply. But if the hardness and solidity of the particles be given up as an incongruous and untenable appendage to the Newtonian view of the Atomic Theory, we are led to the theory of Boscovich, according to which matter consists not of solid particles, but of mere mathematical centers of force. According to this theory, each body is

composed of a number of geometrical points from which emanate forces, following certain mathematical laws in virtue of which the forces become, at certain small distances attractive, at certain other distances repulsive, and at greater distances attractive again. From these forces of the points arise the cohesion of the parts of the same body, the resistance which it exerts against the pressure of another body, and finally the attraction of gravitation which it exerts upon bodies at a distance.

This theory is at least a homogenous and consistent theory, and it is probable that it may be used as an instrument for investigating and expressing true laws of nature; although, as we have already said, the attempt to identify the forces by which the particles of bodies are bound together with mechanical attraction appears to be a confusion of two separate ideas*.

10. *Use of the Molecular Hypothesis.*—In this form, representing matter as a collection of molecules or centers of force, the Atomic Theory has been abundantly employed in modern times as an hypothesis on which calculations respecting the elementary forces of bodies might be conducted. When thus employed, it is to be considered as expressing the principle that the properties of bodies depend upon forces emanating from

* "Boscovich's Theory," that all bodies may be considered as consisting of a mere collection of centers of forces, may be so conceived as possibly to involve an explanation of all the powers which their parts exert, (such powers, namely, as those which produce optical, thermotical, and chemical phenomena;) but this theory cannot supply an explanation of the mechanical properties of a body as a whole, especially of its *inertia*. A collection of mere centers of force can have no inertia. If two bodies are considered as two collections of centers of force, the one attracting the other, there is in this view nothing to limit or determine the velocity with which the one body will approach the other. A world composed of such bodies is not a *material* world: for matter (as we have already seen in Book III. Chapter v.) implies not only force, but something which resists the action of force.

immovable points of their mass. This view of the way in which the properties of bodies are to be treated by the mechanical philosopher was introduced by Newton, and was a natural sequel to the success which he had obtained by reasoning concerning central forces on a large scale. I have already quoted his Preface to the *Principia*, in which he says, "Many things induce me to believe that the rest of the phenomena of nature, as well as those of astronomy, may depend upon certain forces by which the particles of bodies, in virtue of causes not yet known, are urged towards each other and cohere in regular figures, or are mutually repelled and recede; and philosophers, knowing nothing of these forces, have hitherto failed in their examination of nature." Since the time of Newton, this line of speculation has been followed with great assiduity, and by some mathematicians with great success. In particular Laplace has shown that the hypothesis may, in many instances, be made a much closer representation of nature, if we suppose the forces exerted by the particles to decrease so rapidly with the increasing distance from them, that the force is finite only at distances imperceptible to our senses, and vanishes at all remoter points. He has taught the method of expressing and calculating such forces, and he and other mathematicians of his school have applied this method to many of the most important questions of physics; as capillary action, the elasticity of solids, the conduction and radiation of heat. The explanation of many apparently unconnected and curious observed facts by these mathematical theories gives us a strong assurance that its essential principles are true. But it must be observed that the actual constitution of bodies as composed of distinct and separate particles is by no means proved by these coincidences. The assumption, in the reasoning, of certain centers of force acting at a distance, is to be considered as nothing more than a method of reducing to calculation that view of the constitution of bodies which supposes that they exert force at *every* point. It is a mathematical artifice of the same kind as the hypothetical division of a body into infinitesimal parts, in order to find its center of gravity; and no more implies a physical reality than that hypothesis does.

11. *Poisson's Inference.*—When, therefore, M. Poisson, in his views of Capillary Action, treats this hypothetical distribution of centers of force as if it were a physical fact, and blames Laplace for not taking account of their different distribution at the surface of the fluid and below it*, he appears to push the claims of the molecular hypothesis too far. The only ground for the assumption of separate centers, is that we can thus explain the action of the whole mass. The intervals between the centers nowhere enter into this explanation: and therefore we can have no reason for assuming these intervals different in one part of the fluid and in the other. M. Poisson asserts that the density of the fluid diminishes when we approach very near the surface; but he allows that this diminution is not detected by experiment, and that the formulæ on his supposition, so far as the results go, are identical with those of Laplace. It is clear, then, that his doctrine consists merely in the assertion of the necessary truth of a part of the hypothesis which cannot be put to the test of experiment. It is true, that so long as we have before us the hypothesis of separate centers, the particles very near the surface are not in a condition symmetrical with that of the others: but it is also true that this hypothesis is only a step of calculation. There results, at one period of the process of deduction, a stratum of smaller density at the surface of the fluid; but at a succeeding point of

* Poisson, *Théorie de l'Action Capillaire.*

the reasoning the thickness of this stratum vanishes; it has no physical existence.

Thus the *molecular* hypothesis, as used in such cases, does not differ from the doctrine of forces acting at *every point* of the mass; and this principle, which is common to both the opposite views, is the true part of each.

12. *Wollaston's Argument.*—An attempt has been made in another case, but depending on nearly the same arguments, to bring the doctrine of ultimate atoms to the test of observation. In the case of the air, we know that there *is* a diminution of density in approaching the upper surface of the atmosphere, if it have a surface: but it is held by some that except we allow the doctrine of ultimate molecules, it will not be bounded by any surface, but will extend to an infinite distance. This is the reasoning of Wollaston*. "If air consists of any ultimate particles no longer divisible, then must the expansion of the medium composed of them cease at that distance where the force of gravity downwards is equal to the resistance arising from the repulsive force of the medium." But if there be no such ultimate particles, every stratum will require a stratum beyond it to prevent by its weight a further expansion, and thus the atmosphere must extend to an infinite distance. And Wollaston conceived that he could learn from observation whether the atmosphere was thus diffused through all space; for if so, it must, he argued, be accumulated about the larger bodies of the system, as Jupiter and the Sun, by the law of universal gravitation; and the existence of an atmosphere about these bodies, might, he remarked, be detected by its effects in producing refraction. His result is, that "all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate

* *Phil. Trans.*, 1822, p. 89.

atoms of definite magnitude, no longer divisible by repulsion of their parts."

A very little reflection will show us that such a line of reasoning cannot lead to any result. For we know nothing of the law which connects the density with the compressing force, in air so extremely rare as we must suppose it to be near the boundary of the atmosphere. Now there are possible laws of dependence of the density upon the compressing force such that the atmosphere would terminate in virtue of the law without any assumption of atoms. This may be proved by mathematical reasoning. If we suppose the density of air to be as the square root of the compressing force, it will follow that at the very limits of the atmosphere, the strata of equal thickness may observe in their densities such a law of proportion as is expressed by the numbers 7, 5, 3, 1 *.

If it be asked how, on this hypothesis, the density of the highest stratum can be as 1, since there is nothing to compress it, we answer that the upper part of the highest stratum compresses the lower, and that the density diminishes continually to the surface, so that the need of compression and the compressing weight vanish together.

The fallacy of concluding that because the height of the atmosphere is finite, the weight of the highest stratum must be finite, is just the same as the fallacy of those who conclude that when we project a body ver-

* For the compressing force on each being as the whole weight beyond it, will be for the four highest strata, 16, 9, 4 and 1, of which the square roots are as 4, 3, 2, 1, or, as 8, 6, 4, 2; and though these numbers are not exactly as the densities 7, 5, 3, 1, those who are a little acquainted with mathematical reasoning, will see that the difference arises from taking so small a number of strata. If we were to make the strata indefinitely thin, as to avoid error we ought to do, the coincidence would be exact; and thus, according to this law, the series of strata terminates as we ascend, without any consideration of atoms.

tically upwards, because it occupies only a finite time in ascending to the highest point, the velocity at the last instant of the ascent must be finite. For it might be said, if the last velocity of ascent be not finite, how can the body describe the last particle of space in a finite time? and the answer is, that there is no last finite particle of space, and therefore no last finite velocity.

13. *Permanence of Properties of Bodies.*—We have already seen that, in explaining the properties of matter as we find them in nature, the assumption of solid, hard, indestructible particles is of no use or value. But we may remark, before quitting the subject, that Newton appears to have had another reason for assuming such particles, and one well worthy of notice. He wished to express, by means of this hypothesis, the doctrine that the laws of nature do not alter with the course of time. This we have already seen in the quotation from Newton. "The ultimate particles of matter are indestructible, unalterable, impenetrable; for if they could break or wear, the structure of material bodies now would be different from that which it was when the particles were new." No philosopher will deny the truth which is thus conveyed by the assertion of atoms; but it is obviously equally easy for a person who rejects the atomic view, to state this truth by saying that the forces which matter exerts do not vary with time, but however modified by the new modifications of its form, are always unimpaired in quantity, and capable of being restored to their former mode of action.

We now proceed to speculations in which the fundamental conceptions may, perhaps, be expressed, at least in some cases, by means of the arrangement of atoms; but in which the philosophy of the subject appears to require a reference to a new Fundamental Idea.

BOOK VII.

THE PHILOSOPHY OF MORPHOLOGY, INCLUDING CRYSTALLOGRAPHY.

CHAPTER I.

EXPLICATION OF THE IDEA OF SYMMETRY.

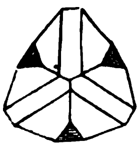
1. WE have seen in the History of the Sciences, that the principle which I have there termed* the principle of developed and metamorphosed Symmetry, has been extensively applied in botany and physiology, and has given rise to a province of science termed Morphology. In order to understand clearly this principle, it is necessary to obtain a clear idea of the Symmetry of which we thus speak. But this Idea of Symmetry is applicable in the inorganic, as well as in the organic kingdoms of nature; it is presented to our eyes in the forms of minerals, as well as of flowers and animals; we must, therefore, take it under our consideration here, in order that we may complete our view of mineralogy, which, as I have repeatedly said, is an essential part of chemical science. I shall accordingly endeavour to unfold the Idea of Symmetry with which we here have to do.

It will of course be understood that by the term *Symmetry* I here intend, not that more indefinite attribute of form which belongs to the domain of the fine arts, as when we speak of the "symmetry" of an edifice

* *Hist. Ind. Sci.*, B. XVII. c. vi.

or of a sculptured figure, but a certain definite relation or property, no less rigorous and precise than other relations of number and position, which is thus one of the sure guides of the scientific faculty, and one of the bases of our exact science.

2. In order to explain what Symmetry is in this sense, let the reader recollect that the bodies of animals consist of *two* equal and similar sets of members, the right and the left side;—that some flowers consist of three or of five equal sets of organs, similarly and regularly disposed, as the iris has *three* straight petals, and three reflexed ones, alternately disposed, the rose has *five* equal and similar sepals of the calyx, and alternate with these, as many petals of the corolla. This orderly and exactly similar distribution of two, or three, or five, or any other number of parts, is Symmetry; and according to its various modifications, the forms thus determined are said to be *symmetrical* with various numbers of members. The classification of these different kinds of symmetry has been most attended to in Crystallography, in which science it is the highest and most general principle by which the classes of forms are governed. Without entering far into the technicalities of the subject, we may point out some of the features of such classes.



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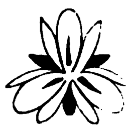

The first of the figures (1) in the margin may represent the summit of a crystal as it appears to an eye looking directly down upon it; the center of the figure represents the summit of a pyramid, and the spaces of various forms which diverge from this point represents sloping sides of the pyramid. Now it will be observed that the figure consists of three portions exactly similar to one another, and that each part or member is repeated in each of these portions. The faces, or pairs of faces, are repeated in *threes*, with exactly similar forms and angles. This figure is said to be *three-membered*, or to have *triangular* symmetry. The same kind of symmetry may exist in a flower, as presented in the accompanying figure, and does, in fact, occur in a large class of flowers, as for example, all the lily tribe. The next pair of figures (2) have four equal and similar portions, and have their members or pairs of members four times repeated. Such figures are termed *four-membered*, and are said to have *square* or *tetragonal* symmetry. The *pentagonal* symmetry, formed by *five* similar *members*, is represented in the next figures (3). It occurs abundantly in the vegetable world, but never among crystals; for the pentagonal figures which crystals sometimes assume, are never exactly regular. But there is still another kind of symmetry (4) in which the opposite ends are exactly similar to each other and also the opposite sides; this is *oblong*, or *two-and-two-membered* symmetry. And finally, we have the case of *simple* symmetry (5) in which the two sides of the object are exactly alike (in opposite positions) without any further repetition.

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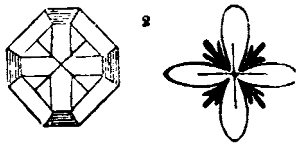

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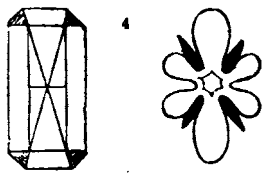

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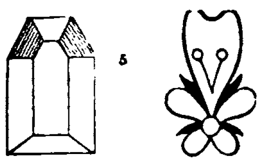

3. These different kinds of symmetry occur in various ways in the animal, vegetable, and mineral kingdom;

thus vertebrate animals have a right and a left side exactly alike, and thus possess *simple* symmetry The same kind of symmetry (simple symmetry) occurs very largely in the forms of vegetables, as in most leaves, in *papilionaceous*, *personate*, and *labiate* flowers. Among minerals, crystals which possess this symmetry are called *oblique-prismatic*, and are of very frequent occurrence. The *oblong*, or *two-and-two membered* symmetry belongs to *right-prismatic* crystals; and may be seen in *cruciferous* flowers, for though these are cross-shaped, the cross has two longer and two shorter arms, or pairs of arms. The *square* or *tetragonal* symmetry occurs in crystals abundantly; to the vegetable world it appears to be less congenial; for though there are flowers with four exactly similar and regularly-disposed petals, as the herb Paris (*Paris quadrifolia*), these flowers appear, from various circumstances, to be deviations from the usual type of vegetable forms. The *trigonal*, or *three-membered* symmetry is found abundantly both in plants and in crystals, while the *pentagonal* symmetry, on the other hand, though by far the most common among flowers, nowhere occurs in minerals, and does not appear to be a possible form of crystals. This pentagonal form further occurs in the animal kingdom, which the oblong, triangular, and square forms do not. Many of Cuvier's *radiate* animals appear in this pentagonal form, as *echini* and *pentacrinites*, which latter have hence their name.

4. The regular, or as they may be called, the *normal* types of the vegetable world appear to be the forms which possess triangular and pentagonal symmetry; from these the others may be conceived to be derived, by transformations resulting from the expansion of one or more parts. Thus it is manifest that if in a three-membered or five-membered flower, one of the petals be

expanded more than the other, it is immediately reduced from pentagonal or trigonal, to simple symmetry. And the oblong or two-and-two membered symmetry of the flowers of cruciferous plants, (in which the stamens are four large and two small ones, arranged in regular opposition,) is held by botanists to result from a normal form with ten stamens; Meinecke explaining this by adhesion, and Sprengel by the metamorphosis of the stamens into petals*.

It is easy to see that these various kinds of symmetry include relations both of form and of number, but more especially of the latter kind; and as this symmetry is often an important character in various classes of natural objects, such classes have often curious numerical properties. One of the most remarkable and extensive of these is the distinction which prevails between monocotyledonous and dicotyledonous plants; the number *three* being the ground of the symmetry of the former, and the number *five*, of the latter. Thus liliaceous and bulbous plants, and the like, have flowers of three or six petals, and the other organs follow the same numbers: while the vast majority of plants are pentandrous, and with their five stamens have also their other parts in fives. This great numerical distinction corresponding to a leading difference of physiological structure cannot but be considered as a highly curious fact in phytology. Such properties of numbers, thus connected in an incomprehensible manner with fundamental and extensive laws of nature, give to numbers an appearance of mysterious importance and efficacy. We learn from history how strongly the study of such properties, as they are exhibited by the phenomena of the heavens, took possession of the mind of Kepler; perhaps it was this which, at an earlier period, contributed in no small degree to

* Sprengel, *Gesch. d. Bot.*, II. 304.

the numerical mysticism of the Pythagoreans in antiquity, and of the Arabians and others in the middle ages. In crystallography, numbers are the primary characters in which the properties of substances are expressed;—they appear, first, in that classification of forms which depends on the degree of symmetry, that is, upon the number of correspondencies; and next, in the laws of derivation, which, for the most part, appear to be common in their occurrence in proportion to the numerical simplicity of their expression. But the manifestation of a governing numerical relation in the organic world strikes us as more unexpected; and the selection of the number *five* as the index of the symmetry of dicotyledonous plants and radiated animals, (a number which is nowhere symmetrically produced in inorganic bodies,) makes this a new and remarkable illustration of the constancy of numerical relations. We may observe, however, that the moment one of these radiate animals has one of its five members expanded, or in any way peculiarly modified, (as happens among the echini) it is reduced to the common type of animals simply symmetrical, with a right and left side.

5. It is not necessary to attempt to enumerate all the kinds of Symmetry, since our object is only to explain what Symmetry is, and for this purpose enough has probably been said already. It will be seen, as soon as the notion of Symmetry in general is well apprehended, that it is or includes a peculiar Fundamental Idea, not capable of being resolved into any of the ideas hitherto examined. It may be said, perhaps, that the Idea of Symmetry is a modification or derivative of our ideas of space and number;—that a symmetrical shape is one which consists of parts exactly similar, repeated a certain number of times, and placed so as to correspond with each other. But on further reflection it will be

seen that this repetition and correspondence of parts in symmetrical figures are something peculiar; for it is not *any* repetition or any correspondence of parts to which we should give the name of symmetry, in the manner in which we are now using the term. Symmetrical arrangements may, no doubt, be concerned with space and position, time and number; but there appears to be implied in them a Fundamental Idea of regularity, of completeness, of complex simplicity, which is not a mere modification of other ideas.

6. It is, however, not necessary, in this and in similar cases to determine whether the idea which we have before us be a peculiar and independent Fundamental Idea or a modification of other ideas, provided we clearly perceive the evidence of those Axioms by means of which the Idea is applied in scientific reasonings. Now in the application of the Idea of Symmetry to crystallography, phytology and zoology, we must have this idea embodied in some principle which asserts more than a mere geometrical or numerical accordance of members. We must have it involved in some vital or productive action, in order that it may connect and explain the facts of the organic world. Nor is it difficult to enunciate such a principle. We may state it in this manner. *All the symmetrical members of a natural product are, under like circumstances, alike affected by the natural formative power.* The parts which we have termed symmetrical, resemble each other, not only in their form and position, but also in the manner in which they are produced and modified by natural causes. And this principle we assume to be necessarily true, however unknown and inconceivable may be the causes which determine the phenomena. Thus it has not yet been found possible to discover or represent to ourselves, in any intelligible manner, the forces by which the various faces of a crystal are consequent

upon its primary form; but the whole of crystallography rests upon this principle, that if one of the primary planes or axes be modified in any manner, all the symmetrical planes and axes must be modified in the same manner. And though accidental mechanical or other causes may interfere with the actual exhibition of such faces, we do not the less assume their crystallographical reality, as inevitably implied in the law of symmetry of the crystal*. And we apply similar considerations to organized beings. We assume that in a regular flower, each of the similar members has the same organization and similar powers of developement; and hence if among these similar parts some are much less developed than others, we consider them as *abortive;* and if we wish to remove doubts as to what are symmetrical members in such a case, we make the inquiry by tracing the anatomy of these members, or by following them in their earlier states of developement, or in cases where their capabilities are magnified by monstrosity or otherwise. The power of developement may be modified by external causes, and thus we may pass from one kind of symmetry to another; as we have already remarked. Thus a regular flower with pentagonal symmetry, growing on a lateral branch, has one petal nearest to the axis of the plant: if this petal be more or less expanded than the others, the pentagonal symmetry is interfered with, and the flower may change to a symmetry of another kind. But it is easy to see that all such conceptions of *expansion*, *abortion*, and any other kind of *metamorphosis*, go upon the supposition of identical faculties and tendencies in each similar member, in so far as such tendencies

* Some crystalline forms, instead of being *holohedral* (provided with their whole number of faces), are *hemihedral* (provided with only half their number of faces). But in these hemihedral forms the half of the faces are still *symmetrically* suppressed.

have any relation to the symmetry. And thus the principle we have stated above is the basis of that which, in the History, we termed the Principle of Developed and Metamorphosed Symmetry.

We shall not at present pursue the other applications of this Idea of Symmetry, but we shall consider some of the results of its introduction into Crystallography.

CHAPTER II.

APPLICATION OF THE IDEA OF SYMMETRY TO CRYSTALS.

1. MINERALS and other bodies of definite chemical composition often exhibit that marked regularity of form and structure which we designate by terming them *Crystals*; and in such crystals, when we duly study them, we perceive the various kinds of symmetry of which we have spoken in the previous chapter. And the different kinds of symmetry which we have there described are now usually distinguished from each other, by writers on crystallography. Indeed it is mainly to such writers that we are indebted for a sound and consistent classification of the kinds and degrees of symmetry of which forms are capable. But this classification was by no means invented as soon as mineralogists applied themselves to the study of crystals. These first attempts to arrange crystalline forms were very imperfect; those, for example, of Linnæus, Werner, Romé de Lisle, and Haüy. The essays of these writers implied a classification at once defective and superfluous. They reduced all crystals to one or other of certain *fundamental forms*; and this procedure might have been a perfectly good method of dividing crystalline forms into classes,

if the fundamental forms had been selected so as to exemplify the different kinds of symmetry. But this was not the case. Haüy's fundamental or "primitive" forms, were, for instance, the following: the *parallelepiped*, the *octahedron*, the *tetrahedron*, the *regular hexagonal prism*, the *rhombic dodecahedron*, and the *double hexagonal pyramid*. Of these, the *octahedron*, the *tetrahedron*, the *rhombic dodecahedron*, all belong to the same kind of symmetry (the TESSULAR systems); also the *hexagonal prism* and the *hexagonal pyramid* both belong to the RHOMBIC system; while the *parallelepiped* is so employed as to include all kinds of symmetry.

It is, however, to be recollected that Haüy, in his selection of primitive forms, not only had an eye to the external form of the crystal and to its degree and kind of regularity, but also made his classification with an especial reference to the cleavage of the mineral, which he considered as a primary element in crystalline analysis. There can be no doubt that the cleavage of a crystal is one of its most important characters: it is a relation of form belonging to the interior, which is to be attended to no less than the form of the exterior. But still, the cleavage is to be regarded only as determining the degree of geometrical symmetry of the body, and not as defining a special geometrical figure to which the body *must* be referred. To have looked upon it in the latter light, was a mistake of the earlier crystallographic speculators, on which we shall shortly have to remark.

2. I have said that the reference of crystals to Primitive Forms might have been well employed as a mode of expressing a just classification of them. This follows as a consequence from the application of the Principle stated in the last chapter, that *all symmetrical members are alike affected*. Thus we may take an upright triangular prism as the representative of the rhombic

system, and if we then suppose one of the upper edges to be cut off, or *truncated*, we must, by the Principle of Symmetry, suppose the other two upper edges to be truncated in precisely the same manner. By this truncation we may obtain the upper part of a rhombohedron; and by truncations of the same kind, symmetrically affecting all the analogous parts of the figure, we may obtain any other form possessing three-membered symmetry. And the same is true of any of the other kinds of symmetry, provided we make a proper selection of a fundamental form. And this was really the method employed by Demeste, Werner, and Romé de Lisle. They assumed a Primitive Form, and then conceived other forms, such as they found in nature, to be derived from the Primitive Form by truncation of the edges, acumination of the corners, and the like processes. This mode of conception was a perfectly just and legitimate expression of the general Idea of Symmetry.

3. The true view of the degrees of symmetry was, as I have already said, impeded by the attempts which Haüy and others made to arrive at primitive forms by the light which cleavage was supposed to throw upon the structure of minerals. At last, however, in Germany, as I have narrated in the History of Mineralogy*, Weiss and Mohs introduced a classification of forms implying a more philosophical principle, dividing the forms into *Systems;* which, employing the terms of the latter writer, we shall call the *tessular*, the *pyramidal* or *square pyramidal*, the *prismatic* or *oblong*, and the *rhombohedral* systems.

Of these forms, the three latter may be at once referred to those kinds of symmetry of which we have spoken in the last chapter. The *rhombohedral* system has *triangular* symmetry, or is three-membered: the *pyramidal* has *square* symmetry, or is four-membered:

* *Hist. Ind. Sci.*, B. xv. c. iv.

the *prismatic* has *oblong* symmetry, and is two-and-two-membered. But the kinds of symmetry which were spoken of in the former chapter, do not exhaust the idea when applied to minerals. For the symmetry which was there explained was such only as can be exhibited on a surface, whereas the forms of crystals are solid. Not only have the right and left parts of the upper surface of a crystal relations to each other; but the upper surface and the lateral faces of the crystal have also their relations; they may be different, or they may be alike. If we take a cube, and hold it so that four of its faces are vertical, not only are all these four sides exactly similar, so as to give square symmetry; but also we may turn the cube, so that any one of these four sides shall become the top, and still the four sides which are thus made vertical, though not the same which were vertical before, are still perfectly symmetrical. Thus this cubical figure possesses more than square symmetry. It possesses square symmetry in a vertical as well as in a horizontal sense. It possesses a symmetry which has the same relation to a *cube* which four-membered symmetry has to a *square*. And this kind of symmetry is termed the *cubical* or *tessular* symmetry. All the other kinds of symmetry have reference to an axis, about which the corresponding parts are disposed; but in tessular symmetry the horizontal and vertical axes are also symmetrical, or interchangeable; and thus the figure may be said to have no axis at all.

4. It has already been repeatedly stated that, by the very idea of symmetry, all the incidents of form must affect alike all the corresponding parts. Now in crystals we have, among these incidents, not only external figure, but *cleavage*, which may be considered as internal figure. Cleavage, then, must conform to the degree of symmetry of the figure. Accordingly cleavage, no less than form, is

to be attended to in determining to what system a mineral belongs. If a crystal were to occur as a square prism or pyramid, it would not on that account necessarily belong to the square pyramidal system. If it were found that it was cleavable parallel to one side of the prism, but not in the transverse direction, it has only oblong symmetry; and the equality of the sides which makes it square is only accidental.

Thus no cleavage is admissible in any system of crystallization which does not agree with the degree of symmetry of the system. On the other hand, *any* cleavage which *is* consistent with the symmetry of the system, is (hypothetically at least) allowable. Thus in the oblong prismatic system we may have a cleavage parallel to one side only of the prism; or parallel to both, but of different distinctness; or parallel to the two diagonals of the prism but of the same distinctness; or we may have both these cleavages together. In the rhombohedral system, the cleavage may be parallel to the sides of the rhombohedron, as in Calc Spar: or, in the same system, the cleavage, instead of being thus oblique to the axis, may be along the axis in those directions which make equal angles with each other: this cleavage easily gives either a triangular or a hexagonal prism. Again, in the tessular system, the cleavage may be parallel to the surface of the cube, which is thus readily separable into other cubes, as in Galena; or the cleavage may be such as to cut off the solid angle of the cube, and since there are eight of these, such cleavage gives us an octahedron, which, however, may be reduced to a tetrahedron, by rejecting all parallel faces, as being mere repetitions of the same cleavage; this is the case with Fluor Spar: or the cube of the tessular system may be cleavable in planes which truncate all the edges of the cube; and as these are twelve, we thus obtain the dodecahedron with rhombic faces: this occurs in Zinc Blende. And thus we see the origin of Haüy's various primitive forms, the tetrahedron, octahedron, and rhombic dodecahedron, all belonging to the tessular system:—they are, in fact, different cleavage forms of that system.

5. I do not dwell upon other incidents of crystals which have reference to form, nor upon the lustre, smoothness, and striation of the surfaces. To all such incidents the general principle applies, that similar parts are similarly affected; and hence, if any parts are found to be constantly and definitely different from other parts of the same sort, they are not similar parts; and the symmetry is to be interpreted with reference to this difference.

We have now to consider the inferences which have been drawn from these incidents of crystallization, with regard to the intimate structure of bodies.

Chapter III.

SPECULATIONS FOUNDED UPON THE SYMMETRY OF CRYSTALS.

1. When a crystal, as, for instance, a crystal of galena, (sulphuret of lead,) is readily divisible into smaller cubes, and these into smaller ones, and so on without limit, it is very natural to represent to ourselves the original cube as really consisting of small cubical elements; and to imagine that it is a philosophical account of the physical structure of such a substance to say that it is made up of cubical molecules. And when the galena crystal has externally the form of a cube, there is no difficulty in such a conception; for the surface of the crystal is also conceived as made up of the surfaces of its cubical molecules. We

conceive the crystal so constituted, as we conceive a wall built of bricks.

But if, as often happens, the galena crystal be an octahedron, a further consideration is requisite in order to understand its structure, pursuing still the same hypothesis. The mineral is still, as in the other case, readily cleavable into small cubes, having their corners turned to the faces of the octahedron. Therefore these faces can no longer be conceived as made up of the faces of cubical elements of which the whole is constituted. If we suppose a pile of such small cubes to be closely built together, but with decreasing width above, so as to form a pyramid, the face of such a pyramid will no longer be plane; it will consist of a great number of the corners or edges of the small elementary cubes. It would appear at first sight, therefore, that such a face cannot represent the smooth polished surface of a crystal.

But when we come to look more closely, this difficulty disappears. For how large are these elementary cubes? We cannot tell, even supposing they really have any size. But we know that they must be, at any rate, very small; so small as to be inappreciable by our senses, for our senses find no limit to the divisibility of minerals by cleavage. Hence the surface of the pyramid above described would not consist of visible corners or edges, but would be roughened by specks of imperceptible size; or rather, by supposing these specks to become still smaller, the roughness becomes smoothness. And thus we may have a crystal with a smooth surface, made up of small cubes in such a manner that their surfaces are all oblique to the surface of the crystal.

Haüy, struck by some instances in which the supposition of such a structure of crystals appeared to account happily for several of their relations and properties, adopted and propounded it as a general theory. The small elements, of which he supposed crystals to be thus built up, he termed *integrant molecules*. The form of these molecules might or might not be the same as the *primitive form* with which his construction was supposed to begin; but there was, at any rate, a close connexion between these forms, since both of them were founded on the cleavage of the mineral. The tenet that crystals are constituted in the manner which I have been describing, I shall call the *Theory of Integrant Molecules*, and I have now to make some remarks on the grounds of this theory.

2. In the case of which I have spoken, the mineral used as the example, galena, readily splits into cubes, and cubes are easily placed together so as to fit eat other, and fill the space which they occupy. The same is the case in the mineral which suggested to Haüy his theory, namely, calc spar. The crystals of this substance are readily divisible into rhombohedrons, a form like a brick with oblique angles; and such bricks can be built together so as to produce crystals of all the immense varieties of form which calc spar presents. This kind of masonry is equally possible in many other minerals; but as we go through the mineral kingdom in our survey, we soon find cases which offer difficulties. Some minerals cleave only in two directions, some in one only; in such cases we cannot by cleavage obtain an integrant molecule of definite form; one of its dimensions, at least, must remain indeterminate and arbitrary. Again, in some instances, we have more than three different planes of cleavage, as in fluor spar, where we have four. The solid, bounded by four planes, is a tetrahedron; or if we take four pairs of parallel faces, an octahedron. But if we attempt to take either of these forms for our integrant molecule, we are met by this difficulty: that a collection of such forms will not fill space. Perhaps this

difficulty will be more readily conceived by the general reader if it be contemplated with reference to plane figures. It will readily be seen that a number of equal squares may be put together so as to fill the space which they occupy; but if we take a number of equal regular octagons, we may easily convince ourselves that no possible arrangement can make them cover a flat space without leaving blank spots between. In like manner octahedrons or tetrahedrons cannot be arranged in solid space so as to fill it. They necessarily leave vacancies. Hence the structure of fluor spar, and similar crystals, was a serious obstacle in the way of the theory of integrant molecules. That theory had been adopted in the first instance because portions of the crystal, obtained by cleavage, could be built up into a solid mass; but this ground of the theory failed altogether in such instances as I have described, and hence the theory, even upon the representations of its adherents, had no longer any claim to assent.

The doctrine of Integral Molecules, however, was by no means given up at once, even in such instances. In this and in other subjects, we may observe that a theory, once constructed and carried into detail, has such a hold upon the minds of those who have been in the habit of applying it, that they will attempt to uphold it by introducing suppositions inconsistent with the original foundations of the theory. Thus those who assert the atomic theory, reconcile it with facts by taking the *halves* of *atoms;* and thus the theory of integrant molecules was maintained for fluor spar, by representing the elementary octahedrons of which crystals are built up, as touching each other only by the edges. The contact of surface with surface amongst integrant molecules had been the first basis of the theory; but this supposition being here inapplicable, was replaced by one which made the theory no longer a representation of the facts (the cleavages), but a mere geometrical construction. Although, however, the inapplicability of the theory to such cases was thus, in some degree, disguised to the disciples of Haüy, it was plain that, in the face of such difficulties, the Theory of Integrant Molecules could not hold its place as a philosophical truth. But it still answered the purpose (a very valuable one, and one to which crystallography is much indebted,) of an instrument for calculating the geometrical relations of the parts of crystals to each other: for the integrant molecules were supposed to be placed layer above layer, each layer as we ascend, *decreasing* by a certain number of molecules and rows of molecules; and the calculation of these *laws of decrement* was, in fact, the best mode then known of determining the positions of the faces. The Theory of Decrements served to express and to determine, in a great number of the most obvious cases, *the laws of phenomena* in crystalline forms, though the Theory of Integrant Morecules could not be maintained as a just view of the structure of crystals.

3. The Theory of Integrant Molecules, however, involved this just and important principle: that a true view of the intimate structure of crystals must include and explain the facts of crystallization, that is, crystalline form and cleavage; and that it must take these into account, according to their degree of symmetry. So far all theories concerning the elements of crystals must agree. And it was soon seen that this was, in reality, all that had been established by the investigations of Haüy and his school. I have already, in the *History*, quoted Weiss's reflections on making this step. "When in 1809," he says*, "I published my Dissertation, I shared the common opinion as to the necessity of the assump-

* *Acad. Berlin.* 1816. p. 307.

tion, and the reality of the existence of a primitive form, at least in a sense not very different from the usual sense of the expression." He then proceeds to relate that he sought a ground for such an opinion, independent of the doctrine of atoms, which he, in common with a great number of philosophers of that time in his own country, was disposed to reject, inclining to believe that the properties of bodies were determined by *forces* which acted in them, and not by *molecules* of which they were composed. He adds, that in pursuing this train of thought, he found, "that out of his primitive forms there was gradually unfolded to his hands that which really governs them, and is not affected by their casual fluctuations; namely, the fundamental relations of their Dimensions," or as we now may call them, *Axes of Symmetry*. With reference to these axes, he found, as he goes on to say, that "a multiplicity of internal oppositions, necessarily and mutually interdependent, are developed in the crystalline mass, each relation having its own polarity; so that the crystalline character is co-extensive with these polarities." The character of these polarities, whether manifested in crystalline faces, cleavage, or any other incidents of crystallization, is necessarily displayed in the degree and kind of symmetry which the crystal possesses: and thus this symmetry, in all our speculations concerning the structure of crystals, necessarily takes the place of that enumeration of primitive forms which were rejected as inconsistent with observed facts, and destitute of sound scientific principle.

I may just notice here what I have stated in the History of Mineralogy*, that the distinction of systems of crystallization, as introduced by Weiss and Mohs, was strikingly confirmed by Sir David Brewster's discoveries respecting the optical properties of minerals. The splen-

* *Hist. Ind. Sci.*, B. xv. c. v.

did phenomena which were produced by passing polarized light through crystals, were found to vary according as the crystals were of the rhombohedral, square pyramidal, oblong prismatic, or tessular system. The optical exactly corresponded with the geometrical symmetry. In the two former systems were crystals *uniaxal* in respect of their optical properties; the oblong prismatic was *biaxal;* while in the tessular, the want of a predominant axis prevented the phenomena here spoken of from occurring at all. The optical experiments must have led to a classification of crystals into the above systems or something nearly equivalent, even had they not been already so arranged by attention to their forms.

4. While in Germany Weiss and Mohs with their disciples, were gradually rejecting what was superfluous in the previous crystallographical hypytheses, philosophers in England were also trying to represent to themselves the constitution of crystals in a manner which should be free from the obviously arbitrary and untenable fictions of the Haüyian school. These attempts, however, were not crowned with much success. One mode of representing the structure of crystals which suggested itself, was to reject the polyhedral forms which Haüy gave to his integrant molecules, and to conceive the elements of crystals as spheres, the properties of the crystal being determined not by the *surfaces*, but by the *position* of the elements. This was done by Wollaston, in the *Philosophical Transactions* for 1813. He applied this view to the tessular system, in which, indeed, the application is not difficult; and he showed that octahedral and tetrahedral figures may be deduced from symmetrical arrangements of equal spherules. But though in doing this, he manifested a perception of the conditions of the problem, he appeared to lose his hold on the real question when he tried to pass on to other systems of

crystallization. For he accounted for the rhombohedral system by supposing the spheres changed into spheroids. Such a procedure involved him in a gratuitous and useless hypothesis: for to what purpose do we introduce the arrangement of atoms (instead of their figure,) as a mode of explaining the symmetry of the crystallization, when at the next step we ascribe to the atom, by an arbitrary fiction, a symmetry of figure of the same kind as that which we have to explain? It is just as easy, and as allowable, to assume an elementary rhombohedron, as to assume elementary spheroids, of which the rhombohedrons are constructed.

5. Many hypotheses of the same kind might be adduced, devised both by mineralogists and chemists. But almost all such speculations have been pursued with a most surprizing neglect of the principle which obviously is the only sound basis on which they can proceed. The principle is this:—that *All hypotheses concerning the arrangement of the elementary atoms of bodies in space must be constructed with reference to the general facts of crystallization.* The truth and importance of this principle can admit of no doubt. For if we make any hypothesis concerning the mode of connexion of the elementary particles of bodies, this must be done with the view of representing to ourselves the forces which connect them, and the results of these forces as manifested in the properties of the bodies. Now the forces which connect the particles of bodies so as to make them crystalline, are manifestly chemical forces. It is only definite chemical compounds which crystallize; and in crystals the force of cohesion by which the particles are held together cannot in any way be distinguished or separated from the chemical force by which their elements are combined. The elements are understood to be combined, precisely because the result is

a definite, apparently homogeneous substance. The properties of the compound bodies depend upon the elements and their mode of combination; for, in fact, these include everything on which they *can* depend. There are no other circumstances than these which can affect the properties of a body. Therefore all those properties which have reference to space, namely, the crystalline properties, cannot depend upon anything else than the arrangement of the elementary molecules in space. These properties are the facts which any hypothesis of the arrangement of molecules must explain, or at least render conceivable; and all such hypotheses, all constructions of bodies by supposed arrangements of molecules, can have no other philosophical object than to account for facts of this kind. If they do not do this, they are mere arbitrary geometrical fictions, which cannot be in any degree confirmed or authorized by an examination of nature, and are therefore not deserving of any regard.

6. Those philosophers who have endeavoured to represent the mode in which bodies are constructed by the combination of their chemical atoms, have often undertaken to show, not only that the atoms are combined, but also in what positions and configurations they are combined. And it is truly remarkable, as I have already said, that they have done this, almost in every instance, without any consideration of the crystalline character of the resulting combinations; from which alone we receive any light as to the relation of their elements in space. Thus Dr. Dalton, in his *Elements of Chemistry*, in which he gave to the world the Atomic Theory as a representation of the doctrine of definite and multiple proportions, also published a large collection of Diagrams, exhibiting what he conceived to be the configuration of the atoms in a great number of the most common combinations of chemical elements. Now these hypothetical diagrams

do not in any way correspond, as to the nature of their symmetry, with the compounds, as we find them displaying their symmetry when they occur crystallized. Carbonate of lime has in reality a triangular symmetry, since it belongs to the rhombohedral system; Dr. Dalton's carbonate of lime would be an oblique rhombic prism or pyramid. Sulphate of baryta is really two-and-two membered; Dr. Dalton's diagram makes it two-and-one membered. Alum is really octahedral or tessular; but according to the diagram it could not be so, since the two ends of the atom are not symmetrical. And the same want of correspondence between the facts and the hypothesis runs through the whole system, It need not surprize us that the theoretical arrangement of atoms does not explain the facts of crystallization; for to produce such an explanation would be a second step in science quite as great as the first, the discovery of the atomic theory in its chemical sense. But we may allow ourselves to be surprized that an utter discrepance between all the facts of crystallization and the figures assumed in the theory, did not suggest any doubt as to the soundness of the mode of philosophizing by which this part of the theory was constructed.

7. Some little accordance between the hypothetical arrangements of chemical atoms and the facts of crystallization, does appear to have been arrived at by some of the theorists to whom we here refer, although by no means enough to show a due conviction of the importanee of the principle stated above. Thus Wollaston, in the Essay above noticed, after showing that a symmetrical arrangement of equal spherules would give rise to octahedral and other tessular figures, remarks, very properly, that the metals, which are simple bodies, crystallize in such forms. M. Ampère* also, in 1814, published a

* *Ann. de Chimie*, tom. xc. p. 43.

brief account of an hypothesis of a somewhat similar nature, and stated himself to have developed this speculation in a Memoir which has not yet, so far as I am aware, been published. In this notice he conceives bodies to be compounded of *molecules*, which, arranged in a polyhedral form, constitute *particles*. These *representative forms* of the particles depend on chemical laws. Thus the particles of oxygen, of hydrogen, and of azote, are composed each of four molecules. Hence it is collected that the particles of nitrous gas are composed of two molecules of oxygen and two of azote; and similar conclusions are drawn respecting other substances. These conclusions, though expressed by means of the polyhedrons thus introduced, are supported by chemical, rather than by crystallographical comparisons. The author does, indeed, appeal to the crystallization of sal ammoniac as an argument*; but as *all* the forms which he introduces appear to belong to the *tessular* system of crystallization, there is, in his reasonings, nothing distinctive; and therefore nothing, crystallographically speaking, of any weight on the side of this theory.

8. Any hypothesis which should introduce any principle of chemical order among the actual forms of minerals, would well deserve attention. At first sight, nothing can appear more anomalous than the forms which occur. We have, indeed, one broad fact, which has an encouraging aspect, the tessular forms in which the pure metals crystallize. The highest degree of chemical and of geometrical simplicity coincide: irregularity disappears precisely where it is excluded by the consideration above stated, that the symmetry of chemical composition must determine the symmetry of crystalline form*.

* *Ann. de Chimie*, tom. xc. p. 83.

† Inasmuch as this law, that the simple metals crystallize in tes-

But if we go on to any other class of crystalline forms, we soon find ourselves lost in our attempts to follow any thread of order. We have indeed many large groups connected by obvious analogies; as the rhombohedral carbonates of lime, magnesia, iron, manganese;—the prismatic carbonates and sulphates of lime, baryta, strontia, lead. But even in these, we cannot form any plausible hypothesis of the arrangement of the elements; and in other cases to which we naturally turn, we can find nothing but confusion. For instance, if we examine the oxides of metals:—those of iron are rhombohedral and tessular; those of copper, tessular; those of tin, of titanium, of manganese, square pyramidal; those of antimony, prismatic; and we have other forms for other substances.

It may be added, that if we take account of the

sular forms, is the most signal example of that connexion between the chemical nature of a body and its crystalline form, I in the former Edition stated it with as much generality as I could find any ground for, and I should have been glad if I could have added confirmation of the law, derived from later observations. But the most recent investigations of crystallographers appear to have afforded exceptions rather than examples of the rule. Arsenic and Tellurium are said to be *rhombohedral*. Antimony, stated by Haüy to be octahedral (and therefore tessular), has been found by more modern observers to be *rhombohedral*. Tin has been obtained by Professor Miller in beautiful crystals belonging to the *pyramidal* system. Professor Nöggerath has observed in Zinc, after cooling from fusion, hexagonal cleavage, rendering it probable that the mineral crystallized in *rhombohedrons* having their axes vertical, like ice. G. Rose conceives it highly probable that Osmium and Iridium are *rhombohedral*. (Poggendorf. Bd. LIV.)

But all the more perfect metals are tessular; namely, Gold, Silver, Mercury, Platinum, Iron, Copper; also Bismuth. Perhaps the observation in which the crystallization of Zinc is affected by its position is, on that very account, no sufficient evidence of its *free* crystallization. We can hardly conceive a collection of perfectly simple, similar particles to crystallize so as to have one pre-eminent axis, without some extraneous action affecting them.

optical properties which, as we have already stated, have constant relations to the crystalline forms, the confusion is still further increased; for the optical dimensions vary in amount, though not in symmetry, where chemistry can trace no difference of composition.

9. We will not quit the subject, however, without noticing the much more promising aspect which it has assumed by the detection of such groups as are referred to in the last article; or in other words, by Mitscherlich's discovery of *Isomorphism*. According to that discovery, there are various elements which may take the place of each other in crystalline bodies, either without any alteration of the crystalline form, or at most with only a slight alteration of its dimensions. Such a group of elements we have in the earths lime and magnesia, the protoxides of iron and manganese: for the carbonates of all these bases occur crystallized in forms of the rhombohedral system, the characteristic angle being nearly the same in all. Now lime and magnesia, by the discoveries of modern chemistry, are really oxides of metals; and therefore all these carbonates have a similar chemical constitution, while they have also a similar crystalline form. Whether or no we can devise any arrangement of molecules by which this connexion of the chemical and the geometrical property can be represented, we cannot help considering the connexion as an extremely important fact in the constitution of bodies; and such facts are more likely than any other to give us some intelligible view of the relations of the ultimate parts of bodies. The same may be said of all the other isomorphous or plesiomorphous groups*. For instance, we have a number of minerals which belong to the same system of crystallization, but in which the chemical composition appears at first sight to be very various:

* See *Hist. Ind. Sci.*, B. XV. c. vi.

namely, spinelle, pleonaste, gahnite, franklinite, chromic iron oxide, magnetic iron oxide: but Abich has shown that all these may be reduced to a common chemical formula;—they are bioxides of one set of bases, combined with trioxides of another set. Perhaps some mathematician may be able to devise some geometrical arrangement of such a group of elements which may possess the properties of the tessular system. Hypothetical arrangements of atoms, thus expressing both the chemical and the crystalline symmetry which we know to belong to the substance, would be valuable steps in analytical science; and when they had been duly verified, the hypotheses might easily be divested of their atomic character.

Thus, as we have already said, mineralogy, understood in its wider sense, as the counterpart of chemistry, has for one of its main objects to discover those relations of the elements of bodies which have reference to space. In this research, the foundation of all sound speculation is the kind and degree of symmetry of form which we find in definite chemical compounds: and the problem at present before the inquirer is, to devise such arrangements of molecules as shall answer the conditions alike of chemistry and of crystallography.

We now proceed to the Classificatory Sciences, of which Mineralogy is one, though hitherto by far the least successful.

BOOK VIII.

THE PHILOSOPHY OF THE CLASSIFICATORY SCIENCES.

Chapter I.

THE IDEA OF LIKENESS AS GOVERNING THE USE OF COMMON NAMES.

1. *Object of the Chapter.*—Not only the Classificatory Sciences, but the application of names to things in the rudest and most unscientific manner, depends upon our apprehending them as *like* each other. We must therefore endeavour to trace the influence and operation of the Idea of Likeness in the common use of language, before we speak of the conditions under which it acquires its utmost exactness and efficacy.

It will be my object to show in this, as in previous cases, that the impressions of sense are apprehended by acts of the mind; and that these mental acts necessarily imply certain relations which may be made the subjects of speculative reasoning. We shall have, if we can, to seize and bring into clear view the principles which the relation of *like* and *unlike* involves, and the mode in which these principles have been developed.

2. *Unity of the Individual.*—But before we can attend to several things as like or unlike, we must be able to apprehend each of these by itself as *one thing*. It may at first sight perhaps appear that this apprehension results immediately from the impressions on our senses, without

any act of our thoughts. A very little attention, however, enables us to see that thus to single out special objects requires a mental operation as well as a sensation. How, for example, without an exertion of mental activity, can we see one tree, in a forest where there are many? We have, spread before us, a collection of colours and forms, green and brown, dark and light, irregular and straight: this is all that sensation gives or can give. But we associate one brown trunk with one portion of the green mass, excluding the rest, although the neighbouring leaves are both nearer in contiguity and more similar in appearance than is the stem. We thus have before us one tree; but this unity is given by the mind itself. We see the green and the brown, but we must *make* the tree before we can see *it*.

That this composition of our sensations so as to form *one thing* implies an act of our own, will perhaps be more readily allowed, if we once more turn our attention to the manner in which we sometimes attempt to imitate and record the objects of sight, by drawing. When we do this, as we have already observed, we mark this unity of each object, by drawing a line to separate the parts which we include from those which we exclude;—an *Outline*. This line corresponds to nothing which we see; the beginner in drawing has great difficulty in discerning it; he has in fact to make it. It is, as has been said by a painter of our own time*, a fiction: but it is a fiction employed to mark a real act of the mind; to designate the singleness of the object in our conception. As we have said elsewhere, we see lines, but especially outlines, by mentally drawing them ourselves.

The same act of conception which the outline thus represents and commemorates in visible objects,—the same combination of sensible impressions into a unit,—is

* Phillips *On Painting*,—Design.

exercised also with regard to the objects of all our senses: and the singleness thus given to each object, is a necessary preliminary to its being named or represented in any other way.

But it may be said, Is it then by an arbitrary act of our own that we put together the branches of the same tree, or the limbs of the same animal? Have we equally the power and the right to make the branch of the fir a part of the neighbouring oak? Can we include in the outline of a man any object with which he happens to be in contact?

Such suppositions are manifestly absurd. And the answer is, that though we give unity to objects by an act of thought, it is not by an *arbitrary* act; but by a process subject to certain conditions;—to conditions which exclude such incongruous combinations as have just been spoken of.

What are these conditions which regulate our apprehension of an object as one?—which determine what portion of our impressions does, and what portion does not belong to the same thing?

3. *Condition of Unity.*—I reply, that the primary and fundamental condition is, that we must be able to make intelligible assertions respecting the object, and to entertain that belief of which assertions are the exposition. A tree *grows*, *sheds* its leaves in autumn, and *buds* again in the spring, *waves* in the wind, or *falls* before the storm. And to the tree belong all those parts which must be included in order that such declarations, and the thoughts which they convey, shall have a coherent and permanent meaning. Those are *its* branches which wave and fall with *its* trunk; those are *its* leaves which grow on *its* branches. The permanent connexions which we observe,—permanent, among unconnected changes which affect the surrounding appearances,—are what we bind together as

belonging to one object. This permanence is the condition of our conceiving the object *as* one. The connected changes may always be described by means of assertions; and the connexion is seen in the identity of the subject of successive predications; in the possibility of applying many verbs to one substantive. We may therefore express the condition of the unity of an object to be this: that *assertions concerning the object shall be possible:* or rather we should say, that the acts of belief which such assertions enunciate shall be possible.

It may seem to be superfluous to put in a form so abstract and remote, the grounds of a process apparently so simple as our conceiving an object to be one. But the same condition to which we have thus been led, as the essential principle of the unity of objects, namely, that propositions shall be possible, will repeatedly occur in the present chapter; and it may serve to illustrate our views, to show that this condition pervades even the simplest cases.

4. *Kinds.*—The mental synthesis of which we have thus spoken, gives us our knowledge of *individual* things; it enables me to apprehend that particular tree or man which I now see, or, by the help of memory, the tree or the man I saw yesterday. But the knowledge with which we have mainly here to do is not a knowledge of individuals but of kinds; of such classes as are indicated by common names. We have to make assertions concerning a tree or a man in general, without regarding what is peculiar to this man or that tree.

Now it is clear that certain individual objects are all called *man*, or all called *tree*, in virtue of some resemblance which they have. If we had not the power of perceiving in the appearances around us, likeness and unlikeness, we could not consider objects as distributed into kinds at all. The impressions of sense would throng upon us, but being uncompared with each other, they would flow away like the waves of the sea, and each vanish from our contemplation when the sensation faded. That we do apprehend surrounding objects as belonging to permanent kinds, as being men and horses, oaks and roses, arises from our having the idea of likeness, and from our applying it habitually, and so far as such a classification requires.

Not only can we employ the idea of likeness in this manner, but we apply it incessantly and universally to the whole mass and train of our sensations. For we have no external sensations to which we cannot apply some language or other, and all language necessarily implies recognition of resemblances. We cannot call an object *green* or *round* without comparing in our thoughts its colour or its shape, with a shape and a colour seen in other objects. All our sensations, therefore, without any exception of kind or time, are subject to this constant process of classification; and the idea of likeness is perpetually operating to distribute them into kinds, at least so far as the use of language requires.

We come then again to the question, Upon what principle, under what conditions, is the idea of likeness thus operative? What are the limits of the classes thus formed? Where does that similarity end, which induces and entitles us to call a thing a *tree?* What universal rule is there for the application of common names, so that we may not apply them wrongly?

5. *Not made by Definitions.*—Perhaps some one might expect in answer to these inquiries a definition or a series of definitions;—might imagine that some description of a tree might be given which might show when the term was applicable and when it was not; and that we might construct a body of rules to which such descriptions must conform. But on consideration it will be clear that the

real solution of our difficulty cannot be obtained in such a manner. For *first*; such descriptions must be given in words, and therefore suppose that we have already satisfied ourselves how words are to be used. If we define a tree to be "a living thing without the power of voluntary motion," we shall be called upon to define "a living thing;" and it is manifest that this renewal of the demand for definition might be repeated indefinitely; and, therefore, we cannot in this way come to a final principle. And in the *next* place, most of those who use language, even with great precision and consistency, would find it difficult or impossible to give good definitions even of a few of the general names which they use; and therefore their practice cannot be regulated by any tacit reference to such definitions. That definitions of terms are of great use and importance in their right place, we shall soon see; but their place is not to regulate the use of common language.

What then, once more, is this regulative principle? What rules do men follow in the use of words, so as commonly to avoid confusion and ambiguity? How do they come to understand each other so well as they ordinarily do, respecting the limits of classes never defined, and which they cannot define? What is the common Convention, or Condition to which they conform?

6. *Condition of the Use of Terms.*—To this we reply, that the Condition which regulates the use of language, is that it shall be capable of being-used;—that is, that general assertions shall be possible. The term *tree* is applicable as far as it is useful in expressing our knowledge concerning trees:—thus we know that trees are fixed in the ground, have a solid stem, branches, leaves, and many other properties. With regard to all the objects which surround us, we have an immense store of

knowledge of such properties, and we employ the names of the objects in such a manner as enables us to express these properties.

But the connexion of such properties is variable and indefinite. Some properties are constantly combined, others occasionally only. The leaves of different oaks resemble each other, the branches resemble far less, and may differ very widely. The term *oak* does not enable us to say that all oaks have straight branches or all crooked. Terms can only express properties as far as they are constant. Not only, therefore, the accumulation of a vast mass of knowledge of the properties and attributes of objects, but also an observation of the habitual *connexion* of such properties is needed, to direct us to the consistent application of terms:—to enable us to apply them so as to express truths. But here again we are largely provided with the requisite knowledge and observation by the common course of our existence. The unintermitting stream of experience supplies us with an incalculable amount of such observed connexions. All men have observed that the associations of the same form of leaves are more constant than of the same form of branches;—that though persons walk in different attitudes none go on all fours; and thus the term *oak* is so applied as to include those cases in which the leaves are alike in form though the branches be unlike; and though we should refuse to apply the term *man* to a class of creatures which habitually and without compulsion used four legs, we make no scruple of affixing it to persons of very different figures. The whole of human experience being composed of such observed connexions, we have thus materials even for the immense multiplicity of names which human language contains; all which names are, as we have said, regulated in their application by the condition of expressing such experience.

Thus amid the countless combinations of properties and divisions of classes which the structure of language implies, scarcely any are arbitrary or capricious. A word which expressed a mere wanton collection of unconnected attributes could hardly be called a *word*; for of such a collection of properties no truth could be asserted, and the word would disappear, for want of some occasion on which it could be used. Though much of the fabric of language appears, not unnaturally, fantastical and purely conventional, it is in fact otherwise. The associations and distinctions of phraseology are not more fanciful than is requisite to make them correspond to the apparent caprices of nature or of thought; and though much in language may be called conventional, the conventions exist for the sake of expressing some truth or opinion, and not for their own sake. The principle, that *the condition of the use of terms is the possibility of general, intelligible, consistent assertions*, is true in the most complete and extensive sense.

7. *Terms may have different Uses.*—The Terms with which we are here most concerned are Names of Classes of natural objects; and when we say that the principle and the limit of such Names are their use in expressing propositions concerning the classes, it is clear that much will depend on the kind of propositions which we mainly have to express: and that the same name may have different limits, according to the purpose we have in view. For example, is the whale properly included in the general term *fish*? When men are concerned in catching marine animals, the main features of the process are the same however the animals may differ; hence whales are classed with fishes, and we speak of the *whale-fishery*. But if we look at the analogies of organization, we find

that, according to these, the whale is clearly not a fish, but a *beast*, (confining this term, for the sake of distinctness, to suckling beasts or *mammals*). In Natural History, therefore, the whale is not included among fish. The indefinite and miscellaneous propositions which language is employed to enunciate in the course of common practical life, are replaced by a more coherent and systematic collection of properties, when we come to aim at scientific knowledge. But we shall hereafter consider the principle of the classifications of Natural History; our present subject is the application of the Idea of Likeness in common practice and common language.

8. *Gradation of Kinds.*—Common names, then, include many individuals associated in virtue of resemblances, and of permanently connected properties; and such names are applicable as far as they serve to express such properties. These collections of individuals are termed *Kinds, Sorts, Classes.*

But this association of particulars is capable of degrees. As individuals by their resemblances form Kinds, so kinds of things, though different, may resemble each other so as to be again associated in a higher Class; and there may be several successive steps of such classification. *Man, horse, tree, stone,* are each a name of a Kind; but *animal* includes the two first and excludes the others; *living thing* is a term which includes *animal* and *tree* but not *stone; body* includes all the four. And such a subordination of kinds may be traced very widely in the arrangements of language.

The condition of the use of the wider is the same as that of the narrower Names of Classes;—they are good as far as they serve to express true propositions. In common language, though such an order of generality may in a variety of instances be easily discerned, it is not systematically and extensively referred to; but this subordination and graduated comprehensiveness is the essence of the methods and nomenclatures of Natural History, as we shall soon have to show.

But such subordination is not without its use, even in common cases, and when it is expressed in the terms of common language. Thus *organized body* is a term which includes plants and animals; *animal* includes beasts, birds, fishes; *beast* includes horses and dogs; *dogs*, again, are greyhounds, spaniels, terriers.

9. *Characters of Kinds.*—Now when we have such a Series of Names and Classes, we find that we take for granted irresistibly that each class has some *character* which distinguishes it from other classes included in the superior division. We ask what kind of beast a dog is; what kind of animal a beast is; and we assume that such questions admit of answer;—that each kind has some mark or marks by which it may be described. And such descriptions may be given: an animal is an organized body *having sensation and volition*; man is a *reasonable* animal. Whether or no we assent to the exactness of these definitions, we allow the propriety of their form. If we maintain these to be wrong, we must believe some others to be right, however difficult it may be to hit upon them. We entertain a conviction that there must be, among things so classed and named, a possibility of defining each.

Now what is the foundation of this postulate? What is the ground of this assumption, that there must exist a definition which we have never seen, and which perhaps no one has seen in a satisfactory form? The knowledge of this definition is by no means necessary to our using the word with propriety; for any one can make true assertions about dogs, but who can define a dog? And yet if the definition be not necessary to enable us to use the word, why is it necessary at all? I allow that we possess an indestructible conviction that there must be such a character of each kind as will supply a definition; but I ask, on what this conviction rests.

I reply, that our persuasion that there must needs be characteristic marks by which things can be defined in words, is founded on the assumption of *the necessary possibility of reasoning.*

The reference of any object or conception to its class without definition, may give us a persuasion that it shares the properties of its class, but such classing does not enable us to reason upon those properties. When we consider man as an animal, we ascribe to him in thought the appetites, desires, affections, which we habitually include in our notion of animal: but except we have expressed these in some definition or acknowledged description of the term animal, we can make no use of the persuasion in ratiocination. But if we have described animals as "beings impelled to action by appetites and passions," we can not only think, but say, "man is an animal, and therefore he is impelled to act by appetites and passions." And if we add a further definition, that "man is a reasonable animal," and if it appear that "reason implies conformity to a rule of action," we can then further infer that man's nature is to conform the results of animal appetite and passion to a rule of action.

The possibility of pursuing any such train of reasoning as this, depends on the definitions, of *animal* and of *man*, which we have introduced; and the possibility of reasoning concerning the objects around us being inevitably assumed by us from the constitution of our nature, we assume consequently the possibility of such definitions as may thus form part of our deduction, and the existence of such defining characters.

10. *Difficulty of Definitions.*—But though men are,

on such grounds, led to make constant and importunate *demands* for definitions of the terms which they employ in their speculations, they are, in fact, far from being able to carry into complete effect the postulate on which they proceed, that they must be able to find definitions which by logical consequence shall lead to the truths they seek. The postulate overlooks the process by which our classes of things are formed and our names applied. This process consisting, as we have already said, in observing permanent connexions of properties, and in fixing them by the attribution of names, is of the nature of the process of induction, of which we shall afterwards have to speak. And the postulate is so far true, that this process of induction being once performed, its result may usually be expressed by means of a few definitions, and may thus lead by a deduction to a train of real truths.

But in the subjects where we principally find such a subordination of classes as we have spoken of, this process of deduction is rarely of much prominence: for example, in the branches of natural history. Yet it is in these subjects that the existence and importance of these characteristic marks, which we have spoken of, principally comes into view. In treating of these marks, however, we enter upon methods which are technical and scientific, not popular and common. And before we make this transition, we have a remark to make on the manner in which writers, without reference to physics or natural history, have spoken of kinds, their subordination, and their marks.

11. "*The Five Words.*"—These things,—the nature and relations of classes,—were, in fact, the subjects of minute and technical treatment by the logicians of the school of Aristotle. Porphyry wrote an Introduction to the *Categories* of that philosopher, which is entitled *On*

the Five Words. The "Five Words" are *Genus, Species, Difference, Property, Accident.* Genus and Species are superior and inferior classes, and are stated* to be capable of repeated subordination. The "most general Genus" is the widest class, the "most special Species" the narrowest. Between these are intermediate classes, which are Genera with regard to those below, and Species with regard to those above them. Thus Being is the most general Genus; under this is Body; under Body is Living Body; under this again Animal; under Animal is Rational Animal, or Man; under Man are Socrates and Plato, and other individual men.

The *Difference* is that which is added to the genus to make the species; thus Rational is the Difference by which the genus Animal is made the species Man; the Difference in this Technical sense is the "Specific," or species-making Difference†. It forms the Definition for the purposes of logic, and corresponds to the "Character" (specific or generic) of the Natural Historians. Indeed several of them, as, for instance, Linnæus, in his *Philosophia Botanica*, always call these Characters the *Difference*, by a traditional application of the Peripatetic terms of art.

Of the other two words, the Property is that which though not employed in defining the class, belongs to every part of it‡: it is, "What happens to all the class, to it alone, and at all times; as to be capable of laughing is a property of a man."

The Accident is that which may be present and absent without the destruction of the subject, as to sleep is an Accident (a thing which happens) to man.

I need not dwell further on this system of technicalities. The most remarkable points in it are those which I have already noticed; the doctrine of the suc-

* Porphyr. *Isagog.* c. 23. † εἰδοποιός. ‡ *Isagog.* c. 4.

cessive subordination of genera, and the fixing attention upon the specific difference. These doctrines, though invented in order to make reasoning more systematic, and at a period anterior to the existence of any classificatory science, have, by a curious contrast with the intentions of their founders, been of scarcely any use in sciences of reasoning, but have been amply applied and developed in the Natural History which arose in later times. We must now treat of the principles on which this science proceeds, and explain what peculiar and technical processes it employs in addition to those of common thought and common language.

Chapter II.

THE METHODS OF NATURAL HISTORY, AS REGULATED BY THE IDEA OF LIKENESS.

Sect. I.—*Natural History in general.*

1. *Idea of Likeness in Natural History.*—The various branches of Natural History, in so far as they are classificatory sciences merely, and do not depend upon physiological views, rest upon the same Idea of Likeness which is the ground of the application of the names, more or less general, of common language. But the nature of science requires that for her purposes this idea should be applied in a more exact and rigorous manner than in its common and popular employment; just as occurs with regard to the other Ideas on which science is founded;—for instance, as the idea of space gives rise, in popular use, to the relations implied in the prepositions and adjectives which refer to position and

form, and in its scientific developement gives rise to the more precise relations of geometry.

The way in which the Idea of Likeness has been applied, so as to lead to the construction of a science, is best seen in Botany: for, in the Classification of Animals, we are inevitably guided by a consideration of the *function* of parts; that is, by an idea of *purpose*, and not of likeness merely: and in Mineralogy, the attempts at classification on the principles of Natural History have been hitherto very imperfectly successful. But in Botany we have an example of a branch of knowledge in which systematic classification has been effected with great beauty and advantage; and in which the peculiarities and principles on which such classification must depend have been carefully studied. Many of the principal botanists, as Linnæus, Adanson, Decandolle, have not only practically applied, but have theoretically enunciated, what they held to be the sound maxims of classificatory science: and have thus enabled us to place before the reader with confidence the philosophy of this kind of science.

2. *Condition of its Use.*—We may begin by remarking that the Idea of Likeness, in its systematic employment, is governed by the same principle which we have already spoken of as regulating the distribution of things into kinds, and the assignment of names in unsystematic thought and speech; namely, the condition that *general propositions shall be possible.* But as in this case the propositions are to be of a scientific form and exactness, the likeness must be treated with a corresponding precision; and its consequences traced by steady and distinct processes. Naturalists must, for their purposes, employ the resemblances of objects in a technical manner. This technical process may be considered as consisting of three steps;—The fixation of the resemblances;

The use of them in making a classification; The means of applying the classification. These three steps may be spoken of as the *Terminology*, the *Plan of the System*, and the *Scheme of the Characters*.

SECT. II.—*Terminology**.

3. *Terminology* signifies the collection of *terms*, or technical words, which belong to the science. But in fixing the meaning of the terms, at least of the descriptive terms, we necessarily fix, at the same time, the perceptions and notions which the terms are to convey; and thus the Terminology of a classificatory science exhibits the elements of its substance as well as of its language. A large but indispensable part of the study of botany (and of mineralogy and zoology also,) consists in the acquisition of the peculiar vocabulary of the science.

The meaning of technical terms can be fixed in the first instance only by convention, and can be made intelligible only by presenting to the senses that which the terms are to signify. The knowledge of a colour by its name can only be taught through the eye. No description can convey to a hearer what we mean by *apple-green* or *French grey*. It might, perhaps, be supposed that, in the first example, the term *apple*, referring to so familiar an object, sufficiently suggests the colour intended. But it may easily be seen that this is not true; for apples are of many different hues of green, and it is only by a conventional selection that we can

* Decandolle and others use the term *Glossology* instead of *Terminology*, to avoid the blemish of a word compounded of two parts taken from different languages. The convenience of treating the termination *ology* (and a few other parts of compounds) as not restricted to Greek combinations, is so great, that I shall venture, in these cases, to disregard this philological scruple.

appropriate the term to one special shade. When this appropriation is once made, the term refers to the sensation, and not to the parts of the term; for these enter into the compound merely as a help to the memory, whether the suggestion be a natural connexion as in "apple-green," or a casual one as in "French grey." In order to derive due advantage from technical terms of this kind, they must be associated *immediately* with the perception to which they belong; and not connected with it through the vague usages of common language. The memory must retain the sensation; and the technical word must be understood as directly as the most familiar word, and more distinctly. When we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to start up in our memory without delay or search.

This, which it is most important to recollect with respect to the simpler properties of bodies, as colour and form, is no less true with respect to more compound notions. In all cases the term is fixed to a peculiar meaning by convention; and the student, in order to use the word, must be completely familiar with the convention, so that he has no need to frame conjectures from the word itself. Such conjectures would always be insecure, and often erroneous. Thus the term *papilionaceous*, applied to a flower, is employed to indicate, not only a resemblance to a butterfly, but a resemblance arising from five petals of a certain peculiar shape and arrangement; and even if the resemblance were much stronger than it is in such cases, yet if it were produced in a different way, as, for example, by one petal, or two only, instead of a "standard," two "wings," and a "keel" consisting of two parts more or less united into one, we should no longer be justified in speaking of it as a "papilionaceous" flower.

The formation of an exact and extensive descriptive language for botany has been executed with a degree of skill and felicity, which, before it was attained, could hardly have been dreamt of as attainable. Every part of a plant has been named; and the form of every part, even the most minute, has had a large assemblage of descriptive terms appropriated to it, by means of which the botanist can convey and receive knowledge of form and structure, as exactly as if each minute part were presented to him vastly magnified. This acquisition was part of the Linnæan reform, of which we have spoken in the *History*. "Tournefort," says Decandolle*, "appears to have been the first who really perceived the utility of fixing the sense of terms in such a way as always to employ the same word in the same sense, and always to express the same idea by the same word; but it was Linnæus who really created and fixed this botanical language, and this is his fairest claim to glory, for by this fixation of language he has shed clearness and precision over all parts of the science."

It is not necessary here to give any detailed account of the terms of botany. The fundamental ones have been gradually introduced, as the parts of plants were more carefully and minutely examined. Thus the flower was successively distinguished into the *calyx*, the *corolla*, the *stamens*, and the *pistils*: the sections of the corolla were termed *petals* by Columna; those of the calyx were called *sepals* by Necker†. Sometimes terms of greater generality were devised; as *perianth* to include the calyx and corolla, whether one or both of these were present‡; *pericarp* for the part inclosing the grain, of whatever kind it be, fruit, nut, pod, &c. And it may easily be imagined that descriptive terms may, by definition and

* *Theor. Elem.*, p. 327. † Dec. 329.

‡ For this Erhart and Decandolle use *Perigone*.

combination, become very numerous and distinct. Thus leaves may be called *pinnatifid**, *pinnatipartite*, *pinnatisect*, *pinnatilobate*, *palmatifid*, *palmatipartite*, &c., and each of these words designates different combinations of the modes and extent of the divisions of the leaf with the divisions of its outline. In some cases arbitrary numerical relations are introduced into the definition: thus a leaf is called *bilobate*† when it is divided into two parts by a notch; but if the notch go to the middle of its length, it is *bifid*; if it go near the base of the leaf, it is *bipartite*; if to the base, it is *bisect*. Thus, too, a pod of a cruciferous plant is a *silica*‡ if it be four times as long as it is broad, but if it be shorter than this it is a *silicula*. Such terms being established, the form of the very complex leaf or frond of a fern is exactly conveyed by the following phrase: "fronds rigid pinnate, pinnæ recurved subunilateral pinnatifid, the segments linear undivided or bifid spinuloso-serrate§."

Other characters, as well as form, are conveyed with the like precision: Colour by means of a classified scale of colours, as we have seen in speaking of the measures of secondary qualities; to which, however, we must add, that the naturalist employs arbitrary names, (such as we have already quoted,) and not mere numerical exponents, to indicate a certain number of selected colours. This was done with most precision by Werner, and his scale of colours is still the most usual standard of naturalists. Werner also introduced a more exact terminology with regard to other characters which are important in mineralogy, as lustre, hardness. But Mohs improved upon this step by giving a numerical scale of hardness, in which *talc* is 1, *gypsum* 2, *calc spar* 3, and so on, as

* Dec. 318. † *Ib.* 493. ‡ *Ib.* 422.

§ Hooker, *Brit. Flo.*, p. 450. *Hymenophyllum Wilsoni*, Scottish filmy-fern, abundant in the Highlands of Scotland and about Killarney.

we have already explained in the History of Mineralogy. Some properties, as specific gravity, by their definition give at once a numerical measure; and others, as crystalline form, require a very considerable array of mathematical calculation and reasoning, to point out their relations and gradations. In all cases the features of likeness in the objects must be rightly apprehended, in order to their being expressed by a distinct terminology. Thus no terms could describe crystals for any purpose of natural history, till it was discovered that in a class of minerals the proportion of the faces might vary, while the angle remained the same. Nor could crystals be described so as to distinguish species, till it was found that the derived and primitive forms are connected by very simple relations of space and number. The discovery of the mode in which characters must be apprehended so that they may be considered as *fixed* for a class, is an important step in the progress of each branch of Natural History; and hence we have had, in the History of Mineralogy and Botany, to distinguish as important and eminent persons those who made such discoveries, Romé de Lisle and Haüy, Cesalpinus and Gesner.

By the continued progress of that knowledge of minerals, plants, and other natural objects, in which such persons made the most distinct and marked steps, but which has been constantly advancing in a more gradual and imperceptible manner, the most important and essential features of similarity and dissimilarity in such objects have been selected, arranged, and fitted with names; and we have thus in such departments, systems of Terminology which fix our attention upon the resemblances which it is proper to consider, and enable us to convey them in words. We have now to speak of the mode in which such resemblances have been employed in the construction of a Systematic Classification.

SECT. III.—*The Plan of the System.*

4. The collection of sound views and maxims by which the resemblances of natural objects are applied so as to form a scientific classification, is a department of the philosophy of natural history which has been termed by some writers (as Decandolle,) *Taxonomy*, as containing the *Laws* of the *Taxis*, (*arrangement*). By some Germans this has been denominated *Systematik*; if we could now form a new substantive after the analogy of the words *Logick, Rhetorick*, and the like, we might call it *Systematick*. But though our English writers commonly use the expression *Systematical Botany* for the Botany of Classification, they appear to prefer the term *Diataxis* for the method of constructing the classification. The rules of such a branch of science are curious and instructive.

In framing a Classification of objects we must attend to their resemblances and differences. But here the question occurs, to *what* resemblances and differences? for a different selection of the points of resemblance would give different results: a plant frequently agrees in leaves with one group of plants, in flowers with another. Which set of characters are we to take as our guide?

The view already given of the regulative principle of all classification, namely, that it must enable us to assert true and general propositions, will obviously occur as applicable here. The object of a scientific Classification is to enable us to enunciate scientific truths: we must therefore classify according to those resemblances of objects (plants or any others,) which bring to light such truths.

But this reply to the inquiry, "On what characters of resemblance we are to found our system," is still too general and vague to be satisfactory. It carries us, however, as far as this;—that since the truths we are to attend to are scientific truths, governed by precise and homogeneous relations, we must not found our scientific Classification on casual, indefinite, and unconnected considerations. We must not, for instance, be satisfied with dividing plants, as Dioscorides does, into *aromatic*, *esculent*, *medicinal*, and *vinous*; or even with the long prevalent distribution into *trees*, *shrubs*, and *herbs*; since in these subdivisions there is no consistent principle.

5. *Latent Reference to Natural Affinity.*—But there may be several kinds of truths, all exact and coherent, which may be discovered concerning plants or any other natural objects; and if this should be the case, our rule leaves us still at a loss in what manner our classification is to be constructed. And, historically speaking, a much more serious inconvenience has been this;—that the task of classification of plants was necessarily performed when the general laws of their form and nature were very little known; or rather, when the existence of such laws was only just beginning to be discerned. Even up to the present day, the general propositions which botanists are able to assert concerning the structure and properties of plants, are extremely imperfect and obscure.

We are thus led to this conclusion:—that the Idea of Likeness could not be applied so as to give rise to a scientific Classification of plants, till considerable progress was made in studying the general relations of vegetable form and life; and that the selection of the resemblances which should be taken into account, must depend upon the nature of the relations which were then brought into view.

But this amounts to saying that, in the consideration of the Classification of vegetables, other Ideas must be called into action as well as the Idea of Likeness. The additional general views to which the more intimate study of plants leads, must depend, like all general truths, upon some regulating Idea which gives unity to scattered facts. No progress could be made in botanical knowledge without the operation of such principles: and such additional Ideas must be employed, besides those of mere likeness and unlikeness, in order to point out that Classification which has a real scientific value.

Accordingly, in the classificatory sciences, Ideas other than Likeness do make their appearance. Such Ideas in botany have influenced the progress of the science, even before they have been clearly brought into view. We have especially the Idea of Affinity, which is the basis of all Natural Systems of Classification, and which we shall consider in a succeeding chapter. The assumption that there *is* a Natural System, an assumption made by all philosophical botanists, implies a belief in the existence of Natural Affinity, and is carried into effect by means of principles which are involved in that Idea. But as the formation of all systems of classification must involve, in a great degree, the Idea of Resemblance and Difference, I shall first consider the effect of that Idea, before I treat specially of Natural Affinity.

6. *Natural Classes.*—Many attempts were made to classify vegetables before the rules which govern a natural system were clearly apprehended. Botanists agree in esteeming some characters as of more value than others, before they had agreed upon any general rules or principles for estimating the relative importance of the characters. They were convinced of the necessity of adding other considerations to that of Resemblance, without seeing clearly what these others ought to be. They aimed at a Natural Classification, without knowing distinctly in what manner it was to be Natural.

The attempts to form *Natural Classes*, therefore, in the first part of their history, belong to the Idea of Likeness, though obscurely modified, even from an early period, by the Ideas of Affinity, and even of Function and of Developement. Hence Natural Classes may, to a certain extent, be treated of in this place.

Natural Classes are opposed to Artificial Classes which are understood to be regulated by an *assumed* character. Yet no classes can be so absolutely Artificial in this sense, as to be framed upon characters *arbitrarily* assumed; for instance, no one would speak of a class of shrubs defined by the circumstance of each having a hundred leaves: for of such a class no assertion could be made, and therefore the class could never come under our notice. In what sense then are Artificial Classes to be understood, as opposed to Natural?

7. *Artificial Classes.*—To this question, the following is the answer. When Natural Classes of a certain small extent have been formed, a system may be devised which shall be regulated by a few selected characters, and which shall not dissever these small Natural Classes, but conform to them as far as they go. If these selected characters be then made absolute and imperative, and if we abandon all attempt to obtain Natural Classes of any higher order and wider extent, we form an Artificial System.

Thus in the Linnæan System of Botanical Classification, it is assumed that certain natural groups, namely, Species and Genera, are established; it is conceived, moreover, that the division of Classes according to the number of stamens and of pistils does not violate the natural connexions of Species and Genera. This arrangement, according to the number of stamens and pistils, (further modified in certain cases by other considerations,) is then made the ground of all the higher divisions of plants, and thus we have an Artificial System.

It has been objected to this view, that the Linnæan Artificial System does not in all cases respect the boundaries of genera, but would, if rigorously applied, distribute the species of the same genus into different artificial classes; it would divide, for instance, the genera *Valeriana, Geranium**, &c. To this we must reply, that so far as the Linnæan System does this, it is an imperfect Artificial System. Its great merit is in its making such a disjunction in comparatively so few cases; and in the artificial characters being, for the most part, obvious and easily applied.

8. *Are Genera Natural?*—It has been objected also that Genera are not Natural groups. Linnæus asserts in the most positive manner that they are†. On which Adanson observes‡, "I know not how any Botanist can maintain such a thesis: that which is certain is, that up to the present time no one has been able to prove it, nor to give an exact definition of a natural genus, but only of an artificial." He then brings several arguments to confirm this view.

But we are to observe, in answer to this, that Adanson improperly confounds the recognition of the existence of a natural group with the invention of a technical mark or definition of it. Genera are groups of species associated in virtue of natural affinity, of general resemblance, of real propinquity: of such groups, certain selected characters, one or few, may usually be discovered, by which the species may be referred to their groups. These Artificial characters do not constitute, but indicate the genus: they are the *Diagnosis*, not the basis of the *Diataxis*: and they are always subject to be

* Decand. *Theor. Elem.*, p. 45. † *Phil. Bot.*, Art. 165.

‡ *Famille de Ph.*, Pref. cv.

rejected, and to have others substituted for them, when they violate the natural connexion of species which a minute and enlarged study discovers.

It is, therefore, no proof that Genera are not Natural, to say that their artificial characters are different in different systems. Such characters are only different attempts to confine the variety of nature within the limits of definition. Nor is it sufficient to say that these groups themselves are different in different writers; that some botanists make genera what others make only species; as *Pedicularis*, *Rhinanthus*, *Euphrasia*, *Antirrhinum**. This discrepancy shows only that the natural arrangement is not yet completely known, even in the smaller groups; a conclusion to which we need not refuse our assent. But in opposition to these negatives, the manner in which Genera have been established proves that they are regulated by the principle of being natural, and by that alone. For they are not formed according to any *à priori* rule. The Botanist does not take any selected or arbitrary part or parts of the plants, and marshal his genera according to the differences of this part. On the contrary, the divisions of genera are sometimes made by means of the flower; sometimes by means of the fruit: the anthers, the stamens, the seeds, the pericarp, and the most varied features of these parts, are used in the most miscellaneous and unsystematic manner. Linnæus has indeed laid down a maxim that the characteristic differences of genera must reside in the fructification†: but Adanson has justly remarked‡, that an arbitrary restriction like this makes the groups artificial: and that in some families other characters are more essential than those of the fructification; as the leaves in the families of *Aparineæ* and *Leguminosæ*, and the disposi-

* Adanson, p. cvi.

† *Phil. Bot.*, Art. 162.

‡ Adanson, Pref., p. cxx.

tion of the flowers in *Labiatæ*. And Naturalists are so far from thinking it sufficient to distribute species into genera by *arbitrary* marks, that we find them in many cases lamenting the absence of good *natural* marks: as in the families of *Umbelliferæ*, where Linnæus declared that any one who could find good characters of genera would deserve great admiration, and where it is only of late that good characters have been discovered and the arrangement settled* by means principally of the ribs of the fruit†.

It is thus clear that Genera are not established on any assumed or preconceived basis. What, then, is the principle which regulates botanists when they try to fix genera? What is the arrangement which they thus wish for, without being able to hit upon it? What is the tendency which thus drives them from the corolla to the anthers, from the flower to the fruit, from the fructification to the leaves? It is plain that they seek something, not of their own devising and creating;—not anything merely conventional and systematic; but something which they conceive to exist in the relations of the plants themselves;—something which is without the mind, not within;—in nature, not in art;—in short, a Natural Order.

Thus the regulative principle of a Genus, or of any other natural group is, that it is, or is supposed to be, natural. And by reference to this principle as our guide, we shall be able to understand the meaning of that indefiniteness and indecision which we frequently find in the descriptions of such groups, and which must appear so strange and inconsistent to any one who does not suppose these descriptions to assume any deeper ground

* Lindley, *Nat. Syst.*, p. 5.

† In like manner we find Cuvier saying of Rondelet that he has "un *sentiment* très vrai des genres." *Hist. Ichth.*, p. 39.

of connexion than an arbitrary choice of the botanist. Thus in the family of the Rose-tree, we are told that the *ovules* are *very rarely* erect*, the *stigmata* are *usually* simple. Of what use, it might be asked, can such loose accounts be? To which the answer is, that they are not inserted in order to distinguish the species, but in order to describe the family, and the total relations of the ovules and of the stigmata of the family are better known by this general statement. A similar observation may be made with regard to the Anomalies of each group, which occur so commonly, that Mr. Lindley, in his *Introduction to the Natural System of Botany*, makes the "Anomalies" an article in each Family. Thus, part of the character of the Rosaceæ is that they have alternate *stipulate* leaves, and that the *albumen* is *obliterated:* but yet in *Lowea*, one of the genera of this family, the stipulæ are *absent;* and the albumen is *present* in another, *Neillia*. This implies, as we have already seen, that the artificial character (or *diagnosis* as Mr. Lindley calls it) is imperfect. It is, though very nearly, yet not exactly, commensurate with the natural group: and hence, in certain cases, this character is made to yield to the general weight of natural affinities.

9. *Difference of Natural History and Mathematics.*— These views,—of classes determined by characters which cannot be expressed in words,—of propositions which state, not what happens in all cases, but only usually,— of particulars which are included in a class though they transgress the definition of it, may very probably surprize the reader. They are so contrary to many of the received opinions respecting the use of definitions and the nature of scientific propositions, that they will probably appear to many persons highly illogical and unphilosophical. But a disposition to such a judgment arises in a great

* Lindley, *Nat. Syst.*, p. 81.

measure from this;—that the mathematical and mathematico-physical sciences have, in a great degree, determined men's views of the general nature and form of scientific truth; while Natural History has not yet had time or opportunity to exert its due influence upon the current habits of philosophizing. The apparent indefiniteness and inconsistency of the classifications and definitions of Natural History belongs, in a far higher degree, to all other except mathematical speculations: and the modes in which approximations to exact distinctions and general truths have been made in Natural History, may be worthy our attention, even for the light they throw upon the best modes of pursuing truth of all kinds.

10. *Natural Groups given by Type not by Definition.*—The further developement of this suggestion must be considered hereafter. But we may here observe, that though in a Natural Group of objects a definition can no longer be of any use as a regulative principle, classes are not, therefore, left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary line without, but by a central point within; not by what it strictly excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of Definition we have a *Type* for our director.

A Type is an example of any class, for instance, a species of a genus, which is considered as eminently possessing the characters of the class. All the species which have a greater affinity with this Type-species than with any others, form the genus, and are ranged about it, deviating from it in various directions and different degrees. Thus a genus may consist of several species which approach very near the type, and of which the claim to a place with it is obvious; while there may be other species which straggle further from this central knot, and which yet are clearly more connected with it than with any other. And even if there should be some species of which the place is dubious, and which appear to be equally bound to two generic types, it is easily seen that this would not destroy the reality of the generic groups, any more than the scattered trees of the intervening plain prevent our speaking intelligibly of the distinct forests of two separate hills.

The Type-species of every genus, the Type-genus of every family, is, then, one which possesses all the characters and properties of the genus in a marked and prominent manner. The Type of the Rose family has alternate stipulate leaves, wants the albumen, has the ovules not erect, has the stigmata simple, and besides these features, which distinguish it from the exceptions or varieties of its class, it has the features which make it prominent in its class. It is one of those which possess clearly several leading attributes; and thus, though we cannot say of any one genus that it *must* be the Type of the family, or of any one species that it *must* be the Type of the genus, we are still not wholly to seek: the Type must be connected by many affinities with most of the others of its group; it must be near the center of the crowd, and not one of the stragglers.

11. It has already been repeatedly stated, as the great rule of all classification, that the classification must serve to assert general propositions. It may be asked *what* propositions we are able to enunciate by means of such classifications as we are now treating of. And the answer is, that the collected knowledge of the characters, habits, properties, organization, and functions of these groups and families, as it is found in the best botanical works, and as it exists in the minds of the best botanists,

exhibits to us the propositions which constitute the science, and to the expression of which the classification is to serve. All that is not strictly definition, that is, all that is not artificial character, in the descriptions of such classes, is a statement of truths, more or less general, more or less precise, but making up, together, the positive knowledge which constitutes the science. As we have said, the consideration of the properties of plants in order to form a system of classification, has been termed Taxonomy, or the Systematick of Botany; all the parts of the descriptions, which, taking the system for granted, convey additional information, are termed the *Physiography* of the science; and the same terms may be applied in the other branches of Natural History.

12. *Artificial and Natural Systems.*—If I have succeeded in making it apparent that an artificial system of characters necessarily implies natural classes which are not severed by the artificial marks, we shall now be able to compare the nature and objects of the Artificial and Natural Systems; points on which much has been written in recent times.

The Artificial System is one which is, or professes to be, entirely founded upon marks selected according to the condition which has been stated, of not violating certain narrow natural groups; namely, in the Linnæan system, the natural genera of plants. The marks which form the basis of the system, being thus selected, are applied rigorously and universally without any further regard to any other characters or indications of affinity. Thus in the Linnæan system, which depends mainly on the number of male organs or stamens, and on the number of female organs or styles, the largest divisions, or the *Classes*, are arranged according to the number of the stamens, and are *monandria*, *diandria*, *triandria*, *tetrandria*, *pentandria*, *hexandria*, and so on: the names being formed of the Greek numerical words, and of the word which implies *male*. And the Orders of each of these Classes are distinguished by the number of styles, and are called *monogynia*, *digynia*, *trigynia*, and so on, the termination of these words meaning *female*. And so far as this numerical division and subdivision go on, the system is a rigorous system, and strictly artificial.

But the condition that the artificial system shall leave certain natural affinities untouched, makes it impossible to go through the vegetable kingdom by a method of mere numeration of stamens and styles. The distinction of flowers with twenty and with thirty stamens is not a fixed distinction: flowers of one and the same kind, as roses, have, some fewer than the former, some more than the latter number. The Artificial System, therefore, must be modified. And there are various relations of connexion and proportion among the stamina which are more permanent and important than their mere number. Thus flowers with two longer and two shorter stamens are not placed in the class *tetrandria*, but are made a separate class *didynamia*; those with four longer and two shorter are in like manner *tetradynamia*, not *hexandria*; those in which the filaments are bound into two bundles are *diadelphia*. All these and other classes are deviations from the plan of the earlier Classes, and are so far defects of the artificial system; but they are deviations requisite in order that the system may leave a basis of natural groups, without which it would not be a System of *Vegetables*. And as the division is still founded on some properties of the stamens, it combines not ill with that part of the system which depends on the number of them. The Classes framed in virtue of these various considerations make up an Artificial System which is tolerably coherent.

"But since the Artificial System thus regards natural groups, in what does it differ from a Natural System?" It differs in this:—That though it allows certain subordinate natural groups, it merely allows *these*, and does not endeavour to ascend to any wider natural groups. It takes all the *higher* divisions of its scheme from its artificial characters, its stamens and pistils, without looking to any natural affinities. It accepts natural *Genera*, but it does not seek natural *Families*, or Orders, or Classes. It *assumes* natural groups, but does not *investigate* any; it forms wider and higher groups, but professes to frame them arbitrarily.

But then, on the other hand, the question occurs, "This being the case, what can be the use of the Artificial System?" If its characters, in the higher stages of classification, be arbitrary, how can it lead us to the natural relations of plants? And the answer is, that it does so in virtue of the original condition, that there shall be certain natural relations which the artificial system shall not transgress; and that its use arises from the facility with which we can follow the artificial arrangement as far as it goes. We can count the stamens and pistils, and thus we know the Class and Order of our plant; and we have then to discover its Genus and Species by means less symmetrical but more natural. The Artificial System, though arbitrary in a certain degree, brings us to a Class in which the whole of each Genus is contained, and there we can find the proper Genus by a suitable method of seeking. No Artificial System can conduct us into the extreme of detail, but it can place us in a situation where the detail is within our reach. We cannot find the house of a foreign friend by its latitude and longitude; but we may be enabled, by a knowledge of the latitude and longitude, to find the city in which he dwells, or at least the island; and we then can reach his abode by following the road or exploring the locality.

The Artificial System is such a method of travelling by latitude and longitude; the Natural System is that which is guided by a knowledge of the country.

The Natural System, then, is that which endeavours to arrange by the natural affinities of objects; and more especially, which attempts to ascend from the lower natural groups to the higher; as for example from genera to natural families, orders, and classes. But as we have already hinted, these expressions of natural affinities, natural groups, and the like, when considered in reference to the idea of resemblance alone, without studying analogy or function, are very vague and obscure. We must notice some of the attempts which were made under the operation of this imperfect view of the subject.

SECT. IV.—*Modes of framing Natural Systems.*

13. Decandolle* distinguishes the attempts at Natural Classifications into three sorts: those of *blind trial*, (*tâtonnement*), those of *general comparison*, and those of *subordination of characters*. The two former do not depend distinctly upon any principle, except resemblance; the third refers us to other views, and must be considered in a future chapter.

Method of Blind Trial.—The notion of the existence of natural classes dependent on the general resemblance of plants,—of an affinity showing itself in different parts and various ways,—though necessarily somewhat vague and obscure, was acted upon at an early period, as we have seen in the formation of genera; and was enunciated in general terms soon after. Thus Magnolius† says that he discerns in plants an affinity, by means of which they may be arranged in families. "Yet it is impossible to

* *Theor. Elem.*, art. 41.

† Dec. *Theor. Elem.*, art. 42. Petri Magnoli, *Prodromus Hist. Gen. Plant.*, 1689.

obtain from the fructification alone the Characters of these families; and I have therefore chosen those parts of plants in which the principal characteristic marks are found, as the root, the stem, the flower, the seed. In some plants there is even a certain resemblance; an affinity which does not consist in the parts considered separately, but in their totality; an affinity which may be felt but not expressed; as we see in the families of agrimonies and cinquefoils, which every botanist will judge to be related, though they differ by their roots, their leaves, their flowers, and their seeds."

This obscure feeling of a resemblance on the whole, a naffinity of an indefinite kind, appears fifty years later in Linnæus's attempts. "In the Natural Classification," he says*, "no *à priori* rule can be admitted, no part of the fructification can be taken exclusively into consideration; but only the simple symmetry of all its parts." Hence though he proposed Natural Families, and even stated the formation of such Families to be the first and last object of all Methods, he never gave the Characters of those groups, or connected them by any method. He even declared it to be impossible to lay down such a system of characters. This persuasion was the result of his having refused to admit into his mind any Idea more profound than that notion of Resemblance of which he had made so much and such successful use; he would not attempt to unravel the Ideas of Symmetry and of Function on which the clear establishment of natural relations must depend. He even despised the study of the inner organization of plants; and reckoned† the *Anatomici*, who studied the anatomy and physiology of plants and the laws of vegetation, among the *Botanophili*, the mere amateurs of his science.

The same notion of general resemblance and affinity,

* Dec., *Theor. Elem.* art. 42. † *Phil. Bot.*, s. 44.

accompanied with the same vagueness, is to be found in the writer who least participated in the general admiration of Linnæus, Buffon. Though it was in a great measure his love of higher views which made him dislike what he considered the pedantry of the Swedish school, he does not seem to have obtained a clearer sight of the principle of the natural method than his rival, except that he did not restrict his Characters to the fructification. Things must be arranged by their resemblances and differences, (he says in 1750*,) "but the resemblances and differences must be taken not from one part but from the whole; and we must attend to the form, the size, the habit, the number and position of the parts, even the substance of the part; and we must make use of these elements in greater or smaller number, as we have need."

14. *Method of General Comparison.*—A countryman of Buffon, who shared with him his depreciating estimate of the Linnæan system, and his wish to found a natural system upon a broader basis, was Adanson; and he invented an ingenious method of apparently avoiding the vagueness of the practice of following the general feeling of resemblance. This method consisted in making many Artificial Systems, in each of which plants were arranged by some one part; and then collecting those plants which came near each other in the greatest number of those Artificial Systems, as plants naturally the most related. Adanson gives an account† of the manner in which this system arose in his mind. He had gone to Senegal, animated by an intense zeal for natural history; and there, amid the luxuriant vegetation of the torrid zone, he found that the methods of Linnæus and Tournefort failed him altogether as means of arranging his

* Adanson, p. CLVI. Buffon, *Hist. Nat.*, t. I. p. 21.

† Pref. p. CLVII.

new botanical treasures. He was driven to seek a new system. "For this purpose," he says, "I examined plants in all their parts, without omitting any, from the roots to the embryo, the folding of the leaves in the bud, their mode of sheathing*, the situation and folding of the embryo and of its radicle in the seed, relatively to the fruit; in short, a number of particulars which few botanists notice. I made in the first place a complete description of each plant, putting each of its parts in separate articles, in all its details; when new species occurred I put down the points in which they differed, omitting those in which they agreed. By means of the aggregate of these comparative descriptions, I perceived that plants arranged themselves into classes or families which could not be artificial or arbitrary, not being founded upon one or two parts, which might change at certain limits, but on all the parts; so that the disproportion of one of these parts was corrected and balanced by the introduction of another." Thus the principle of Resemblance was to suffice for the general arrangement, not by means of a new principle, as Symmetry or Organization, which should regulate its application, but by a numeration of the peculiarities in which the resemblance consisted.

The labour which Adanson underwent in the execution of this thought was immense. By taking each Organ, and considering its situation, figure, number, &c., he framed sixty-five Artificial Systems; and collected his Natural Families by a numerical combination of these. For example, his *sixty-fifth* Artificial System† is that which depends upon the situation of the Ovary with regard to the Flower; according to this system he frames *ten* Artificial Classes, including *ninety-three* Sections: and of these Sections the resulting Natural Arrange-

* "Leur manière de s'engainer."

† Adanson, Pref., p. cccxii.

ment retains *thirty-five*, above one-third: the same estimate is applied in other cases.

But this attempt to make Number supply the defects which the vague notion of Resemblance introduces, however ingenious, must end in failure. For, as Decandolle observes*, it supposes that we know, not only all the Organs of plants, but all the points of view in which it is possible to consider them; and even if this assumption were true, which it is, and long must be, very far from being, the principle is altogether vicious; for it supposes that all these points of view, and all the resulting artificial systems are of equal importance:—a supposition manifestly erroneous. We are thus led back to the consideration of the *Relative Importance* of Organs and their qualities, as a basis for the classification of plants, which no Artificial Method can supersede; and thus we find the necessity of attending to something besides mere external and detached Resemblance. The method of General Comparison cannot, any more than the method of Blind Trial, lead us, with any certainty or clearness, to the Natural Method. Adanson's Families are held by the best botanists to be, for the greater part, Natural; but his hypotheses are unfounded; and his success is probably more due to the dim feeling of Affinity, by which he was unconsciously guided, than to the help he derived from his numerical processes.

In a succeeding chapter I shall treat of that Natural Affinity on which a Natural System must really be founded. But before proceeding to this higher subject, we must say a few words on some of the other parts of the philosophy of Natural History,—the Gradation of Groups, the Nomenclature, the Diagnosis, and the application of the methods to other subjects.

* Dec., *Theor. Elem.*, p. 67.

SECT. V.—*Gradation of Groups.*

15. It has been already noticed (last chapter,) that even that vague application of the idea of resemblance which gives rise to the terms of common language, introduces a subordination of classes, as *man, animal, body, substance.* Such a subordination appears in a more precise form when we employ this idea in a scientific manner as we do in Natural History. We have then a series of divisions, each inclusive of the lower ones, which are expressed by various metaphors in different writers. Thus some have gone as far as eight terms of the series*, and have taken, for the most part, military names for them; as *Hosts, Legions, Phalanxes, Centuries, Cohorts, Sections, Genera, Species.* But the most received series is *Classes, Orders, Genera,* and *Species;* in which, however, we often have other terms interpolated, as *Sub-genera,* or Sections of genera. The expressions *Family* and *Tribe,* are commonly appropriated to natural groups; and we speak of the Vegetable, Animal, Mineral *Kingdom;* but the other metaphors of Provinces, Districts, &c., which this suggests, have not been commonly used†.

It will of course be understood that each ascending step of classification is deduced by the same process from the one below. A Genus is a collection of Species which resemble each other more than they resemble other species; an Order is a collection of Genera having, in like manner, the first degree of resemblance, and so on. How close or how wide the Degrees of Resemblance are, must depend upon the nature of the objects compared, and cannot possibly be prescribed beforehand. Hence the same term, *Class* and *Order* for instance, may imply, in different provinces of nature, very different degrees of

* Adanson, p. cvi.

† *Sub-Kingdom* has recently been employed by some naturalists.

resemblance. The Classes of Animals are Insects, Birds, Fish, Beasts, &c. The Orders of Beasts are *Ruminants*, *Tardigrades*, *Plantigrades*, &c. The two Classes of Plants (according to the Natural Order*,) are *Vascular* and *Cellular*, the latter having neither sexes, flowers, nor spiral vessels. The Vascular Plants are divided into Orders, as *Umbelliferæ*, *Ranunculaceæ*, &c.; but between this Class and its Orders are interposed two other steps:—two Sub-classes, *Dicotyledonous* and *Monocotyledonous*, and two Tribes of each: *Angiospermiæ*, *Gymnospermiæ* of the first; and *Petaloideæ*, *Glumaciæ* of the second. Such interpolations are modifications of the general formula of subordination, for the purpose of accommodating it to the most prominent natural affinities.

16. *Species.*—As we have already seen in tracing the principles of the Natural Method, when by the intimate study of plants we seek to give fixity and definiteness to the notion of resemblance and affinity on which all these divisions depend, we are led to the study of Organization and Analogy. But we make a reference to physiological conditions even from the first, with regard to the lowest step of our arrangement, the *Species;* for we consider it a proof of the impropriety of separating two Species, if it be shown that they can by any course of propagation, culture, and treatment, the one pass into the other. It is in this way, for example, that it has been supposed to be established that the common Primrose, Oxlip, Polyanthus, and Cowslip, are all the same species. Plants which thus, in virtue of external circumstances, as soil, exposure, climate, exhibit differences which may disappear by changing the circumstances, are called *Varieties* of the species. And thus we cannot say that a Species is a collection of individuals which possess the First Degree of Resemblance; for it is clear

* Lindley.

that a primrose resembles another primrose more than it does a cowslip; but this resemblance only constitutes a Variety. And we find that we must necessarily include in our conception of Species, the notion of propagation from the same stock. And thus a Species has been well defined*: "The collection of the individuals descended from one another, or from common parents, and of those which resemble these as much as these resemble each other." And thus the sexual doctrine of plants, or rather the consideration of them as things which propagate their kind, (whether by seed, shoot, or in any other way,) is at the basis of our classifications.

17. The First permanent Degree of Resemblance among organized beings is thus that which depends on this relation of generation, and we might expect that the groups which are connected by this relation would derive their names from the notion of generation. It is curious that both in Greek and Latin languages and in our own, the words which have this origin (γένος, *genus*, *kind*,) do not, in the phraseology of science at least, denote the nearest degree of relationship, but have other terms subordinate to them, which appear etymologically to indicate a mere resemblance of appearance, (εἶδος, *species*, *sort*;) and these latter terms are appropriated to the groups resulting from propagation. Probably the reason of this is, that the former terms (*genus*, &c.) had been applied so widely and loosely before the scientific fixation of terms, that to confine them to what we call *species* would have been to restrict them in a manner too unusual to be convenient.

18. *Varieties. Races.*—The Species, as we have said, is the collection of individuals which resemble each other as much as do the offspring of a common stock. But within the limits of this boundary, there

* Cuv., *Règne Animal*, p. 19.

are often observable differences permanent enough to attract our notice, though capable of being obliterated by mixture in the course of generation. Such different groups are called *Varieties*. Thus the Primrose and Cowslip, as has been stated above, are found to be varieties of the same plant; the Poodle and the Greyhound are well marked varieties of the species *dog*. Such differences are hereditary, and it may be long doubtful whether such hereditary differences are varieties only, or different species. In such cases the term *Race* has been applied.

SECT. VI.—*Nomenclature.*

19. The Nomenclature of any branch of Natural History is the collection of names of all its species; which, when they become extremely numerous, requires some artifice to make it possible to recollect or apply them. The known species of plants, for example, were 10,000 at the time of Linnæus, and are now probably 60,000. It would be useless to endeavour to frame and employ separate names for each of these species.

The division of the objects into a subordinated system of classification enables us to introduce a Nomenclature which does not require this enormous number of names. The artifice employed to avoid this inconvenience is to name a Species by means of two (or it might be more) steps of the successive division. Thus in Botany, each of the genera has its name, and the species are marked by the addition of some epithet to the name of the genus. In this manner about 1,700 generic names, with a moderate number of specific names, were found by Linnæus sufficient to designate with precision all the species of vegetables known at his time. And this *Binary Method* of Nomenclature has been found so convenient that it has been universally adopted in every other department of the Natural History of organized beings.

Many other modes of Nomenclature have been tried, but no other has at all taken root. Linnæus himself appears at first to have intended marking each species by the Generic Name accompanied by a characteristic Descriptive Phrase; and to have proposed the employment of a *trivial* Specific Name, as he termed it, only as a method of occasional convenience. The use of these *trivial names*, has, however, become universal, as we have said, and is by many persons considered the greatest improvement introduced at the Linnæan reform.

Both Linnæus and other writers (as Adanson) have given many maxims with a view of regulating the selection of generic and specific names. The maxims of Linnæus were intended as much as possible to exclude barbarism and confusion, and have, upon the whole, been generally adopted; though many of them were objected to by his contemporaries (Adanson and others*), as capricious or unnecessary innovations. Many of the names, introduced by Linnæus, certainly appear fanciful enough: thus he gives the name of *Bauhinia* to a plant with leaves in pairs, because the Bauhins were a pair of brothers; *Banisteria* is the name of a climbing plant, in honour of Banister, who travelled among mountains. But such names, once established by adequate authority, lose all their inconvenience, and easily become permanent; and hence the reasonableness of the Linnæan rule†, that as such a perpetuation of the names of persons by the names of plants is the only honour botanists have to bestow, it ought to be used with care and caution.

The generic name must, as Linnæus says, be fixed‡

* Pp. cxxix. clxxii.

† *Phil. Bot.*, Sec. 239.

‡ *Ib.*, Sec. 222.

before we attempt to form a specific name; "the latter without the former is like the clapper without the bell." The name of the genus being established, the species may be marked by adding to it "a single word taken at will from any quarter;" that is, not involving a description or any essential property of the plant, but a casual or arbitrary appellation*. Thus the various species of *Hieracium*† are *Hieracium Alpinum, H. Halleri, H. Pilosella, H. dubium, H. murorum*, &c. where we see how different may be the kind of origin of the words.

Attempts have been made at various times to form the names of species from those of genera in some more symmetrical manner. Thus some have numbered the species of genus, 1, 2, 3, &c.; but this method is liable to the inconveniences, first, that it offers nothing for the memory to take hold of; and second, that if a new species intermediate between 1 and 2, 2 and 3, &c., be discovered, it cannot be put in its place. It has also been proposed to mark the species by altering the termination of the genus. Thus Adanson‡, denoting a genus by the name *Fonna* (*Lychnidea*), conceived he might mark five of its species by altering the last vowel, *Fonna, Fonna-e, Fonna-i, Fonna-o, Fonna-u;* then others by *Fonna-ba, Fonna-ka*, and so on. This course would be liable to the same evils which have been noticed as belonging to the numerical method.

The names of plants (and the same is true of animals) have in common practice been binary only, consisting of a generic and a specific name. The Class and Order have not been admitted to form part of the appellation of the species. Indeed it is easy to see that a name which must be identical in so many instances as that of an Order would be, would be felt as superfluous and burdensome. Accordingly, Linnæus makes it a precept§, that

* *Phil. Bot.*, Sec. 260.

† Hooker, *Fl. Scot.*, 228.

‡ Pref. CLXXVI.

§ *Phil. Bot.*, Sec. 215.

the name of the Class and the Order must not be expressed but understood: and hence, he says, Royen, who took *Lilium* for the name of a Class, rightly rejected it as a generic name and substituted *Lirium*, with the Greek termination.

Yet we must not too peremptorily assume such maxims as these to be universal for all classificatory sciences. It is very possible that it may be found advisable to use *three* terms, that of order, genus and species, in designating minerals, as is done in Mohs's nomenclature; for example, *Rhombohedral Calc Haloide*, *Paratomous Hal Baryte*.

It is possible also that it may be found useful in the same science to mark some of the steps of classification by the termination. Thus it has been proposed to confine the termination *ite* to the Order *Silicides* of Naumann, as Apophyll*ite*, Stilb*ite*, Leuc*ite*, &c., and to use names of different form in other orders, as Talc *Spar* for Brennerite, Pyramidal Titanium *Oxide* for Octahedrite. Some such method appears to be the most likely to give us a tolerable mineralogical nomenclature.

Sect. VII.—*Diagnosis*.

20. German Naturalists speak of a part of the general method which they call the *Characteristik* of Natural History, and which is distinguished from the *Systematik* of the science. The *Systematick* arranges the objects by means of all their resemblances, the *Characteristick* enables us to detect their place in the arrangement by means of a few of their characters. What these characters are to be, must be discovered by observation of the groups and divisions of the system when they are formed. To construct a collection of such as shall be clear and fixed, is a useful, and generally a difficult task; for there is usually no apparent connexion between the marks which are used in discriminating the groups, and

the nature of the groups themselves. They are assumed only because the Naturalist, extensively and exactly acquainted with the groups and the properties of the objects which compose them, sees, by a survey of the field, that these marks divide it properly.

The Characteristick has been termed by some English Botanists the *Diagnosis* of plants; a word which we may conveniently adopt. The Diagnosis of any genus or species is different according to the system we follow. Thus in the Linnæan System the Diagnosis of the Rose is in the first place given by its Class and Order: it is Icosandrous, and Polygynous; and then the Generic Distinction is that the calyx is five-cleft, the tube urceolate, including many hairy achenia, the receptacle villous*. In the Natural System the Rose-Tribe are distinguished as being† "Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens, exalbuminous definite seeds, and alternate stipulate leaves." And the true Roses are further distinguished by having "Nuts, numerous, hairy, terminated by the persistent lateral style and inclosed within the fleshy tube of the calyx," &c.

It will be observed that in a rigorous Artificial System the *Systematick* coincides with the *Characteristick;* the *Diataxis* with the *Diagnosis;* the reason why a plant is put in a division is identical with the mode by which it is known to be in the division. The Rose is in the class *icosandria*, because it has many stamens inserted in the calyx; and when we see such a set of stamens we immediately know the class. But this is not the case with the Diagnosis of Natural Families. Thus the genera *Lamium* and *Galeopsis* (Dead Nettle and Hemp Nettle), are each formed into a separate group in virtue of their general resemblances and differences, and not because

* Lindley, *Nat. Syst.*, p. 149. † *Ib.*, p. 81. 3.

the former has one tooth on each side of the lower lip, and the latter a notch in its upper lip, though they are distinguished by these marks.

Thus so far as our Systems are natural, (which, as we have shown, all systems to a certain extent must be), the Characteristick is distinct both from a Natural and an Artificial System; and is, in fact, an Artificial Key to a Natural System. As being Artificial, it takes as few characters as possible; as being Natural, its characters are not selected by any general or prescribed rule, but follow the natural affinities. The Botanists who have made any steps in the formation of a natural method of plants since Linnæus, have all attempted to give a Diagnosis corresponding to the Diataxis of their method.

Chapter III.

APPLICATION OF THE NATURAL HISTORY METHOD TO MINERALOGY.

1. The philosophy of the Sciences of Classification has had great light thrown upon it by discussions concerning the methods which are used in Botany: for that science is one of the most complete examples which can be conceived of the consistent and successful application of the principles and ideas of Classification; and this application has been made in general without giving rise to any very startling paradoxes, or disclosing any insurmountable difficulties. But the discussions concerning methods of Mineralogical Classification have been instructive for quite a different reason: they have brought into view the boundaries and the difficulties of the process of Classification; and have presented examples in which every possible mode of classifying appeared to involve inex-

tricable contradictions. I will notice some of the points of this kind which demand our attention, referring to the works published recently by several mineralogists.

In the History of Mineralogy we noticed the attempt made by Mohs and other Germans to apply to minerals a method of arrangement similar to that which has been so successfully employed for plants. The survey which we have now taken of the grounds of that method will point out some of the reasons of the very imperfect success of this attempt. We have already said that the *Terminology* of Mineralogy was materially reformed by Werner; and including in this branch of the subject (as we must do) the Crystallography of later writers, it may be considered as to a great extent complete. Of the attempts at a Natural arrangement, that of Mohs appears to proceed by the method of *blind trial*, the undefinable perception of relationship, by which the earliest attempts at a Natural Arrangement of plants were made. Breithaupt, however, has made (though I do not know that he has published) an essay in a mode which corresponds very nearly to Adanson's process of *multiplied comparisons*. Having ascertained the specific gravity and hardness of all the species of minerals, he arranged them in a table, representing by two lines at right angles to each other these two numerical quantities. Thus all minerals were distributed according to two co-ordinates representing specific gravity and hardness. He conceived that the groups which were thus brought together were natural groups. On both these methods, and on all similar ones, we might observe, that in minerals as in plants, the mere general notion of Likeness cannot lead us to a real arrangement: this notion requires to have precision and aim given it by some other relation;—by the relation of Chemical Composition in minerals, as by the relation of Organic Function in vegetables. The physical and crystallographical properties of minerals must be studied with reference to their constitution; and they must be arranged into Groups which have some common Chemical Character, before we can consider any advance as made towards a Natural Arrangement.

In reality, it happens in Mineralogy as it happened in Botany, that those speculators are regulated by an obscure perception of this ulterior relation, who do not profess to be regulated by it. Several of the Orders of Mohs have really great unity of chemical character, and thus have good evidence of their being really Natural Orders.

2. Supposing the Diataxis of minerals thus obtained, Mohs attempted the Diagnosis; and his *Characteristick of the Mineral Kingdom*, published at Dresden, in 1820, was the first public indication of his having constructed a system. From the nature of a Characteristick, it is necessarily brief, and without any ostensible principle; but its importance was duly appreciated by the author's countrymen. Since that time, many attempts have been made at improved arrangements of minerals, but none, I think, (except perhaps that of Breithaupt,) professing to proceed rigorously on the principles of Natural History;—to arrange by means of external characters, neglecting altogether, or rather postponing, the consideration of chemical properties. By relaxing from this rigour, however, and by combining physical and chemical considerations, arrangements have been obtained (for example, that of Naumann,) which appear more likely than the one of Mohs to be approximations to an ultimate really natural system. Naumann's Classes are *Hydrolytes, Haloides, Silicides, Metal Oxides, Metals, Sulphurides, Anthracides*, with subdivisions of Orders, as *Anhydrous unmetallic Silicides*. It may be remarked that the designations of these are mostly chemical. As

we have observed already, Chemistry, and Mineralogy in its largest sense, are each the necessary supplement of the other. If Chemistry furnish the Nomenclature, Mineralogy must supply the Physiography: if the Arrangement be founded on External Characters and the Names be independent of Chemistry, the chemical composition of each species is an important scientific Truth respecting it.

3. The inquiry may actually occur, whether any subordination of groups in the mineral kingdom has really been made out. The ancient chemical arrangements, for instance, that of Haüy, though professing to distribute minerals according to Classes, Orders, Genera, and Species, were not only arbitrary, but inapplicable; for the first postulate of any method, that the species should have constant characters of unity and difference, was not satisfied. It was not ascertained that carbonate of lime was really distinguishable in all cases from carbonate of magnesia, or of iron; yet these species were placed in remote parts of the system: and the above carbonates made just so many species; although, if they were distinct from one another at all, they were further distinguishable into additional species. Even now, we may, perhaps, say that the limits of mineralogical species, and their laws of fixity, are not yet clearly seen. For the discoveries of the isomorphous relations and of the optical properties of minerals have rather shown us in what direction the object lies, than led us to the goal. It is clear that, in the mineral kingdom, the Definition of Species, borrowed from the laws of the continuation of the kind, which holds throughout the organic world, fails us altogether, and must be replaced by some other condition: nor is it difficult to see that the definite atomic relations of the chemical constituents, and the definite crystalline angle, must supply the principles of the

Speficic Identity for minerals. Yet the exact limits of definiteness in both these cases (when we admit the effect of mechanical mixtures, &c.) have not yet been completely disentangled. Moreover, any *arbitrary* assumption (as the allowance of a certain per-centage of mixture, or a certain small deviation in the angle,) is altogether contrary to the philosophy of the Natural System, and can lead to no stable views. It is only by laborious, extensive, and minute research, that we can hope to attain to any solid basis of arrangement.

4. Still, though there are many doubts respecting mineralogical species, a large number of such species are so far fixed that they may be supposed capable of being united under the higher divisions of a system with approximate truth. Of these higher divisions, those which have been termed *Orders* appear to tend to something like a fixed chemical character. Thus the *Haloids* of Naumann, and mostly those of Mohs, are combinations of an oxide with an acid, and thus resemble Salts, whence their name. The Silicides contain most of Mohs's *Spaths:* and the Orders *Pyrites*, *Glance*, and *Blende*, are common to Naumann and Mohs; being established by the latter on a difference of external character, which difference is, indeed, very manifest; and being included by the former in one chemical *Class*, *Sulphurides*. The distinctions of *Hydrous* and *Anhydrous*, *Metallic* and *Unmetallic*, are, of course, chemical distinctions, but occur as the differences of Orders in Naumann's mixed system.

We may observe that some French writers, following Haüy's last edition, use, instead of *metallic* and *unmetallic*, *autopside metallic* and *heteropside metallic*; meaning by this phraseology to acknowledge the discovery that earths, &c., *are* metallic, though they do not *appear* to be so, while metals both are and appear metallic. But this seems to be a refinement not only useless but absurd. For what is gained by adding the word *metallic*, which is common to all, and therefore makes no distinction? If certain metals are distinguished by their *appearing* to be metals, this appearance is a reason for giving them the peculiar name, *metals*. Nothing is gained by first bringing earths and metals together, and then immediately separating them again by new and inconvenient names. No proposition can be expressed better by calling *earths heteropside metallic substances*, and therefore such nomenclature is to be rejected.

Granting, then, that the Orders of the best recent mineralogical systems approximate to natural groups, we are led to ask whether the same can be said of the Genera of the Natural History systems, such as those of Mohs and Breithaupt. And here I must confess that I see no principle in these Genera; I have failed to apprehend the conceptions by the application of which they have been constructed: I shall therefore not pass any further judgment upon them. The subordination of Mineralogical Species to Orders is a manifest gain to science: in the interposition of Genera I see nothing but a source of confusion.

5. In Mineralogy, as in other branches of natural history, a reformed arrangement ought to give rise to a reformed Nomenclature; and for this, there is more occasion at present in Mineralogy than there was in Botany at the worst period, at least as far as the extent of the subject allows. The characters of minerals are much more dimly and unfrequently developed than those of plants; hence arbitrary chemical arrangements, which could not lead to any natural groups, and therefore not to any good names, prevailed till recently; and this state of things produced an anarchy in which every man did what seemed right in his own eyes,—proposed species without

any ascertained distinction, and without a thought of subordination, and gave them arbitrary names; and thus with only about two or three hundred known species, we have thousands upon thousands of names, of anomalous form and uncertain application.

Mohs has attempted to reform the Nomenclature of the subject in a mode consistent with his attempt to reform the System. In doing this, he has fatally transgressed a rule always insisted upon by the legislators of Botany, of altering usual names as little as possible; and his names are both so novel and so cumbrous, that they appear to have little chance of permanent currency. They are, perhaps, more unweildy than they need to be, by referring, as we have said, to *three* of the steps of his classification, the Species, Genus, and Order. We may, however, assert confidently, from the whole analogy of natural history, that no good names can be found which do not refer to at least *two* terms of the arrangement. This rule has been practically adopted to a great extent by Naumann, who gives to most of his Haloids the name *Spar*, as Calc spar, Iron spar, &c.; to all his Oxides the terminal word *Erz* (*Ore*); and to the species of the orders *Kies* (*Pyrites*), *Glance*, and *Blende*, these names. It has also been theoretically assented to by Beudant, who proposes that we should say *silicate stilbite, silicate chabasie; carbonate calcaire, carbonate witherite; sulphate couperose*, &c. One great difficulty in this case would arise from the great number of *silicides;* it is not likely that any names would obtain a footing which tacked the term *silicide* to another word for each of these species. The artifice which I have proposed, in order to obviate this difficulty, is that we should make the names of the silicides, and those alone, end in *ite* or *lite*, which a large proportion of them do already.

By this and a few similar contrivances, we might, I conceive, without any inconvenient change, introduce into Mineralogy a systematic nomenclature.

6. I shall now proceed to make a few remarks on a work on Mineralogy more recent than those which I have above noticed, and written with express reference to such difficulties as I have been discussing. I allude to the treatise of M. Necker, *Le Règne Mineral ramené aux Methodes d'Histoire Naturelle**, which also contains various dissertations on the Philosophy of Classification in general, and its application to Mineralogy in particular.

M. Necker remarks very justly, that Mineralogy, as it has hitherto been treated, differs from all other branches of Natural History in this:—that while it is invested with all the forms of the sciences of classification,—Classes, Divisions, Genera, and the like,—the properties of those bodies to which the mineralogical student's attention is directed have no bearing whatever on the classification. A person, he remarks†, might be perfectly well acquainted with all the characters of minerals which Werner or Haüy examined so carefully, and might yet be quite unable to assign to any mineral its place in the divisions of their methods. There is‡ a complete separation between the study of mineralogical characters and the recognition of the name and systematic place of a mineral. Those who know *mineralogy* well, may know *minerals* ill, or hardly at all; the systematist may be in such knowledge vastly inferior to the mineral-dealer or the miner. In this respect there is a complete contrast between this science and other classificatory sciences.

Again, in the best-known systems of Mineralogy, (as those of Werner and Haüy,) the bodies which are grouped together as belonging to the same division, have not, as they have in other classificatory sciences, any resemblance. The different members of the larger

* Paris, 1835. † *Règne Mineral*, p. 3. ‡ *Ib.*, p. 8.

classes are united by the common possession of some abstract property,—as, that they all contain iron. This is a property to which no common circumstance in the bodies themselves corresponds. What is there common to the minerals named oxidulous iron, sulphuret of iron, carbonate of iron, sulphate of iron, except that they all contain iron? And when we have classed these bodies together, what general assertion can we make concerning them, except that which is the ground of our classification, that they contain iron? They have nothing in common with iron or with each other in any other way.

Again, as these classes have no general properties, all the properties are particular to the species; and the descriptions of these necessarily become both tediously long, and inconveniently insulated.

7. These inconveniences arise from making Chemical Composition the basis of Mineralogical Classification without giving Chemical Analysis the first place among Mineral Properties. Shall we, then, correct this omission, so far as it has affected mineralogical systems? Shall we teach the student the chemical analysis of minerals, and then direct him to classify them according to the results of his analysis*?

But why should we do this? To what purpose, or on what ground, do we arrange the results of chemical analysis according to the forms and subordination of natural history? Is not chemistry a science distinct from natural history? Are not the sciences opposed? Is not natural history confined to organic bodies? Can mere chemical elements and their combinations be, with any propriety or consistency, arranged into species, genera, and families? What is the principle on which genera and species depend? Do not species imply individuals? What is an individual in the case of a chemical substance?

* *Règne Mineral*, p. 18.

8. We thus find some of the widest and deepest questions of the philosophy of classification brought under our consideration when we would provide a method for the classification of minerals. The answers to these questions are given by M. Necker; and I shall state some of his opinions; taking the liberty of adding such remarks as are suggested by referring the subject to those principles which have already been established in this work.

M. Necker asserts* that the distinctions of different sciences depend, not on the objects they consider, but on the different and independent points of view on which they proceed. Each science has its logic, that is, its mode of applying the general rules of human reason to its own special case. It has been said by some†, that in minerals, natural history and chemistry contemplate common objects, and thus form a single science. But do chemistry and natural history consider minerals in the same point of view?

The answer is, that they do not. Physics and chemistry consider the properties of bodies in an abstract manner; as, their composition, their elements, their mutual actions, with the laws of these; their forces, as attraction, affinity; all which objects are abstract ideas. In these cases we have nothing to do with bodies themselves, but as the vehicles of the powers and properties which we contemplate.

Natural history, on the other hand, has to do with natural bodies: their properties are not considered abstractedly, but only as characters. If the properties are abstracted, it is but for a moment. Natural history has to describe and class bodies as they are. All which cannot be perceived by the senses, belongs not to its domain, as molecules, atoms, elements.

* *Règne Mineral*, p. 23. † *Ib.*, p. 27.

Natural history* may have recourse to physics or chemistry in order to recognize those properties of bodies which serve as characters; but natural history is not, on that account, physics or chemistry. Classification is the essential business of the natural historian†, to which task chemistry and physics are only instrumental, and the further account of properties only complementary.

It has been said, in support of the doctrine that chemistry and mineralogy are identical, that chemistry does not neglect external characters. "The chemist in describing sulphur, mentions its colour, taste, odour, hardness, transparence, crystalline form, specific gravity; how does he then differ from the mineralogist?" But to this it is replied, that these notices of the external characters of this or any substance are introduced in chemistry merely as convenient marks of recognition; whereas they are essential in mineralogy. If we had taken the account given of several substances instead of one, we should have seen that the chemist and the naturalist consider them in ways altogether different. The chemist will make it his business to discover the mutual action of the substances; he will combine them, form new products, determine the proportions of the elements. The mineralogist will divide the substances into groups according to their properties, and then subdivide these groups, till he refers each substance to its species. Exterior and physical characters are merely accessory and subordinate for the chemist; chemistry is merely instrumental for the mineralogist.

This view agrees with that to which we have been led by our previous reasonings; and may, according to our principles, be expressed briefly by saying, that the Idea which chemistry has to apply is the Idea of Ele-

* *Règne Mineral*, p. 37. † *Ib.*, p. 41.

mentary Composition, while natural history applies the Idea of Graduated Resemblances, and thus performs the task of classification.

9. The question occurs*, whether Natural History can be applied to Inorganic Substances? And the answer to this question is, that it can be applied, if there are such things as inorganic *individuals*, since the resemblances and differences with which natural history has to do are the resemblances and differences of individuals.

What is an Individual? It certainly is not that which is so simple that it cannot be divided. Individual animals are composed of many parts. But if we examine, we shall find that our Idea of an Individual is, that it is a whole composed of parts, which are not similar to the whole, and have not an independent existence, while the whole has an independent existence and a definite form†.

What then is the Mineralogical Individual? At first, while minerals were studied for their use, the most precious of the substances which they contained was looked upon as the characteristic of the mineral. The smallest trace of silver made a mineral an *ore of silver*. Thus forms and properties were disregarded, and *substance* was considered as identical with *mineral*. And hence‡ Daubenton refused to recognize *species* in the mineral kingdom, because he recognized no individuals. He proposed to call *sorts* what we call species. In this way of considering minerals, there are no individuals.

10. But still this is not satisfactory: for if we take a well formed and distinct crystal, this clearly *is* an individual§.

It may be objected, that the crystal is divisible (according to the theory of crystallography) into smaller solids; that these small solids are really the simple objects; and that actual crystals are formed by combinations of these molecules according to certain laws.

* *Règne Minéral*, p. 46. † *Ib.*, p. 52. ‡ *Ib.*, p. 54. § *Ib.*, p. 56.

But, as we have already said, an individual is such, not because it cannot be divided, but because it cannot be divided into parts similar to the whole. As to the division of the form into its component *laws*, this is an abstract proceeding, foreign to natural history *. Therefore there is so far nothing to prevent a crystal from being an individual.

11. We cannot (M. Necker goes on to remark) consider the *Integrant Molecules* as individuals. These are useful abstractions, but abstractions only, which we must not deal with as real objects. Haüy himself warns us† that his doctrine of increments is a purely abstract conception, and that nature, in fact, follows a different process. Accordingly, Weiss and Mohs express laws identical with those of Haüy, without even speaking of molecules; and Wollaston and Davy have deemed it probable that the molecules are not polyhedrons, but spheres or spheroids. Such mere creations of the mind can never be treated as individuals. If the maxim of natural history,—that the Species is a collection of Individuals—be applied so as to make those individuals mere abstractions; or if, instead of Individuals, we take such an abstraction as Substance or Matter, the course of natural history is altogether violated. And yet this errour has hitherto generally prevailed; and mineralogists have classified, not things, but abstract ideas‡.

12. But it may be said §, will not the small solids obtained by Cleavage better answer the idea of individuals? To this it is replied, that these small solids have no independent existence. They are only the result of a mode of division. They are never found separate and independent. The secondary forms which they compose are determined by various circumstances (the nature of the solution, &c.); and the cleavage which pro-

* *Règne Minéral*, p. 58. † *Ib.*, p. 61. ‡ *Ib.*, p. 67.
§ *Ib.*, p. 69.

duces these small solids is only one result among many from the crystalline forces*.

Thus neither Integrant Molecules, nor Solids obtained by Cleavage, can be such mineralogical Individuals as the spirit of natural history requires. Hence it appears that we must take the real Crystals for Individuals†.

13. We must, however, reject crystals (generally large ones) which are obviously formed of several smaller ones of a similar form (as occurs so often in quartz and calc spar.) We must also distinguish cases in which a large regular form is composed of smaller but different regular forms (as octahedrons of fluor spar made up of cubes). Here the small component forms are the individuals. Also we must notice the cases‡ in which we have a natural crystal, similar to the primary form. Here the face will show whether the body is a result obtained by cleavage or a natural individual.

14. It will be objected§, that the crystalline form ought not to be made the dominant character in mineralogy, since it rarely occurs perfect. To this it is replied, that even if the application of the principle be difficult, still it has been shown to be the only true principle, and therefore we have no alternative. But further‖, it is not true that amorphous substances are more numerous than crystals. In Leonhard's *Manual of Oryctognosy*, there are 377 mineral substances. Of these, 281 have a crystalline structure, and 96 only have not been found in a regular form.

Again, the 281 crystalline forms have each its varieties, some of which are crystalline, and some are not so. Now the crystalline varieties amount to 1453, and the uncrystalline to 186 only. Thus mineralogy, according

* *Règne Mineral*, p. 71. † *Ib.*, p. 73. ‡ *Ib.*, p. 75.
§ *Ib.*, p. 79. ‖ *Ib.*, p. 82.

to the view of it here presented, has a sufficiently wide field*.

15. It will be objected†, that according to this mode of proceeding, we must reject from our system all non-crystalline minerals. But we reply, that if the mass be composed of crystals, the size of the crystals makes no difference. Now lamellar and other compact masses are very generally groups of crystals in various positions. Individuals mutilated and mixed together are not the less individuals; and therefore such masses may be treated as objects of natural history.

If we cannot refer all rocks to crystalline species, those which elude our method may appear as an appendix, corresponding to those which botanists call *genera incertæ sedis*‡.

But these genera and species will often be afterwards removed into the cystalline part of the system, by being identified with crystalline species. Thus *pyrope*, &c., have been referred to *garnet*, and *basalt*, *wacke*, &c., to compound rocks. Thus veins of *Dolerite*, visibly composed of two or three elements, pass to an apparently simple state by becoming fine-grained§.

16. Finally‖, we have to ask, are artificial crystals to enter into our classification? M. Necker answers, No; because they are the result of art, like mules, mestizos, hybrids, and the like.

17. Upon these opinions, we may observe, that they appear to be, in the main, consistent with the soundest philosophy. That each natural crystal is an individual, is a doctrine which is the only basis of mineralogy as a Natural Historical science; yet the imperfections and confused unions of crystals make this principle difficult to apply. Perhaps it may be expressed in a more pre-

* *Règne Mineral.*, p. 84. † *Ib*, p. 86. ‡ *Ib.*, p. 91.
§ *Ib.*, p. 93. ‖ *Ib.*, p. 95.

cise manner by referring to the crystalline forces, and to the axes by which their operation is determined, rather than to the external form. *That* portion of a mineral substance is a mineralogical *individual* which is determined by crystalline forces acting to the *same axes*. In this way we avoid the difficulty arising from the absence of faces, and enable ourselves to use either cleavage, or optical properties, or any others, as indications of the identity of the individual. The individual extends so far as the polar forces extend by which crystalline form is determined, whether or not those forces produce their full effect, namely, a perfectly circumscribed polyhedron.

18. There is only one material point on which our principles lead us to differ from M. Necker;—the propriety of including *artificial crystals* in our mineralogical classification. To exclude them, as he does, is a conclusion so entirely at variance with the whole course of his own reasonings, that it is difficult to conceive that he would persist in his conclusion, if his attention were drawn to the question more steadily. For, as he justly says*, each science has its appropriate domain, determined by its peculiar point of view. Now artificial and natural crystals are considered in the same point of view, (namely, with reference to crystalline, physical, and optical properties, as subservient to classification,) and ought, therefore, to belong to the same science. Again, he says†, that Chemistry would reject as useless all notice of the physical properties and external characters of substances, if a *special science* were to take charge of the description and classification of these products. But such a special science must be Mineralogy; for we cannot well make one science of the classification of natural, and another of that of artificial substances: or if we do, the two sciences will be identical in method and

* *Règne Mineral*, p. 23. † *Ib.*, p. 36.

principles, and will extend over each other's boundaries, so that it will be neither useful nor possible to distinguish them. Again, M. Necker's own reasonings on the selection of the individual in mineralogy are supported by well chosen examples*; but these examples are taken from artificial salts; as, for instance, common salt crystallizing in different mixtures. Again, the analogy of mules and mestizos, as products of art, with chemical compounds, is not just. Chemical compounds correspond rather to natural species, propagated by man under the most natural circumstances, in order that he may study the laws of their production†.

19. But the decisive argument against the separation of natural and artificial crystals in our schemes of classification is, that we *cannot* make such a separation. Substances which were long known only as the products of the laboratory, are often discovered, after a time, in natural deposits. Are the crystals which are found in a forgotten retort or solution to be considered as belonging to a different science from those which occur in a deserted mine? And are the crystals which are produced where man has turned a stream of water or air out of its course, to be separated from natural crystals, when the composition, growth, and properties, are exactly the same in both? And again: How many natural crystals can we already produce by synthesis! How many more may we hope to imitate hereafter! M. Necker himself states‡, that Mitscherlich found, in the scoriæ of the mines of Sweden and Germany, artificial minerals having the same composition and the same crystalline

* *Règne Mineral*, p. 71.

† We may remark that M. Necker, in his own arrangement of minerals, inserts among his species iron and lead, which do not occur native.

‡ *Règne Mineral*, p. 151.

form with natural minerals: as silicates of iron, lime, and magnesia, agreeing with peridot; bisilicate of iron, lime, and magnesia, agreeing with pyroxene; red oxide of copper; oxide of zinc; protoxide of iron (*fer oxydulé*); sulphurets of iron, zinc, lead; arseniuret of nickel; black mica. These were accidental results of fusion. But M. Berthier, by bringing together the elements in proper quantities, has succeeded in composing similar minerals, and has thus obtained artificial silicates, with the same forms and the same characters as natural silicates. Other chemists (M. Haldat, M. Becquerel) have, in like manner, obtained, by artificial processes, other crystals, known previously as occurring naturally. How are these crystals, thus identical with natural minerals, to be removed out of the domain of mineralogy, and transferred to a science which shall classify artificial crystals only? If this be done, the mineralogist will not be able to classify any specimen till he has human testimony whether it was found naturally occurring or produced by chemical art. Or is the other alternative to be taken, and are these crystals to be given up to mineralogy because they occur naturally also? But what can be more unphilosophical than to refer to separate sciences the results of chemical processes closely allied, and all but identical? The chemist constructs bisilicates, and these are classified by the mineralogist: but if he constructs a trisilicate, it belongs to another science. All these intolerable incongruities are avoided by acknowledging that artificial, as well as natural, crystals belong to the domain of mineralogy. It is, in fact, the *name* only of *Mineralogy* which appears to discover any inconsistency in this mode of proceeding. Mineralogy is the representative of a science which has a wider office than mineralogists first contemplated; but which must exist, in order that the body of science may be complete. There must, as we have already said, be a Science, the object of which is to classify bodies by their physical characters, in order that we may have some means of asserting chemical truths concerning bodies; some language in which we may express the propositions which chemical analysis discovers. And this Science will have its object prescribed, not by any accidental or arbitrary difference of the story belonging to each specimen;—not by knowing whether the specimen was found in the mine or in the laboratory; produced by attempting to imitate nature, or to do violence to her:—but will have its course determined by its own character. The range and boundaries of this Science will be regulated by the Ideas with which it deals. Like all other sciences, it must extend to everything to which its principles apply. The limits of the province which it includes are fixed by the consideration that it must be a connected whole. No previous definition, no historical accident, no casual phrase, can at all stand in the way of philosophical consistency;—can make this Science exclude what that includes, or oblige it to admit what that rejects. And thus, whatever we call our Science;—whether we term it External Chemistry. Mineralogy, the Natural History of Inorganic Bodies; —since it can be nothing but the Science of the Classification of Inorganic Bodies of definite forms and properties, it must classify all such bodies, whether or not they be minerals, and whether or not they be natural.

20. In the application of the principles of classification to minerals, the question occurs, What are to be considered as mineral *Species?* By Species we are to understand, according to the usage of other parts of natural history, the lowest step of our subordinate divisions;—the most limited of the groups which have definite distinctions. What definite distinctions of groups

of objects of any kind really occur in nature, is to be learnt from an examination of nature: and the result of our inquiries will be some general principle which connects the members of each group, and distinguishes the members of groups which, though contiguous, are different. In the classification of organized bodies, the rule which thus presides over the formation of Species is the principle of *reproduction*. Those animals and those plants are of the same Species which are produced from a common stock, or which resemble each other as much as the progeny of a common stock. Accordingly in practice, if any questions arise whether two varieties of form be of the same or different species, it is settled by reference to the fact of reproduction; and when it is ascertained that the two forms come within the habitual and regular limits of a common circle of reproduction, they are held to be of the same species. Now in crystals, this principle of reproduction disappears altogether, and the basis of the formation of species must be sought elsewhere. We must have some other principle to replace the reproduction which belongs only to organic life. This principle will be, we may expect, one which secures the permanence and regularity of mineral forms, as the reproductive power does of animal and vegetable. Such a principle is the *Power of Crystallization*. The forces of which solidity, cohesion, and crystallization are the result, are those which give to minerals their permanent existence and their physical properties; and ever since the discovery of the distinctions of Crystalline Forms and Crystalline Systems, it is certain that this force distinguishes groups of crystals in the most precise and definite manner. The rhombohedral carbonates of lime and of iron, for instance, are distinguished exactly by the angles of their rhombohedrons. And if, in the case of any proposed crystal, we should doubt to which kind the specimen belongs, the measurement of the angles of cleavage would at once decide the question. The principle of Crystallization therefore appears, from analogy, to be exactly fitted to take the place of the principle of animal Generation. The forces which make the individual permanent and its properties definite, here stand in the place of the forces which preserve the race, while individuals are generated and die.

21. According to this view, the different modifications of the same crystalline form would be *Varieties* only of the same species. All the various solids, for example, which are produced by the different laws of derivation of rhombohedral carbonate of lime, would fall within the same Species. And this appears to be required by the general analogy of Natural History. For these differences of form, produced by the laws of crystalline derivation, are not *definite*. The faces which are added to one form in order to produce another, may be of any size, small or large, and thus the crystal which represents one modification passes by insensible degrees to another. The forms of calc spar, which we call *dog-tooth spar*, *cannon spar*, *nail-head spar*, and the like, appear at first, no doubt, distinct enough; but so do the races of dogs. And we find, in the mineral as in the animal, that the distinction is obliterated by taking such intermediate steps as really occur. And if a fragment of any of these crystals is given us, we can determine that it is rhombohedral carbonate of lime; but it is not possible, in general, to determine to which of the kinds of crystal it has belonged.

22. Notwithstanding these considerations, M. Necker has taken for his basis of mineral species* the *Secondary* Modifications, and not the Primary Forms. Thus

* *Règne Mineral*, p. 396.

cubical galena, *octahedral galena*, and *triform galena*, are, with him, three *species* of crystals.

On this I have to observe, as I have already done, that on this principle we have no *definite* distinction of species; for these forms may and do pass into each other: among cubo-octahedrons of galena occur cubes and octahedrons, as one face or another vanishes, and the transition is insensible. We shall, on this principle, find almost always three or four species in the same tuft of crystals; for almost every individual in such assemblages may exhibit a different combination of secondary faces. Again, in cases where the secondary laws are numerous, it would be impracticable to enumerate all their combinations, and impossible therefore to give a list of species. Accordingly M. Necker* gives seventy-one Species of *spath calcaire*, and then says, "Nous n'avons pas enuméré la dixieme partie des espèces connues de ce genre, qui se montent à plus de huit cents." Again, in many substances, of which few crystals are found, every new specimen would be a new species; if indeed it were perfect enough to be referred to a species at all. But from a specimen without perfect external form, however perfect in crystalline character, although everything else might be known,—angles, optical properties, physical properties, and chemical constitution,—the species could not be determined. Thus M. Necker says† of the micas, "Quant aux espèces propre à chaque genre, la lacune sera presque complète; car jusqu'ici les cristaux entiers de Mica et de Talc n'ont pas été fort communs."

These inconveniences arise from neglecting the leading rule of natural history, that the *predominant principle* of the existence of an object must determine the Species; whether this principle be Reproduction operat-

* *Règne Mineral*, p. 364. † *Ib.*, II. 414.

ing for Developement, or Crystallization operating for Permanence of form. We may add to the above statement of inconveniences this;—that if M. Necker's view of mineralogical species be adopted, the distinction of Species is vague and indefinite, while that of Genera is perfectly precise and rigorous;—an aspect of the system entirely at variance with other parts of Natural History; for in all these the Species is a more definite group than the Genus.

This result follows, as has already been said, from M. Necker's wish to have individuals marked by external form. If, instead of this, we are contented to take for an individual that portion of a mass, of whatever form, which is connected by the continuous influence of the same crystalline forces, by whatever incidents these forces may be manifested, (as cleavage, physical and optical properties, and the like,) our mode of proceeding avoids all the above inconveniences, applies alike to the most perfect and most imperfect specimens, and gives a result agreeable to the general analogy of natural history, and the rules of its methods*.

I now quit the subject of mere Resemblance, and proceed to treat of that natural affinity which Natural Systems of Classification for organic bodies must involve.

* I will not again enter into the subject of Nomenclature; but I may remark that M. Necker has adopted (I. 415) the Nomenclature of Beudant, latinizing the names, and thus converting each into a single word. He has also introduced, besides the names of Genera, names of Families taken from the *typical* Genus. Thus the Family of *Carbonidiens* contains the following genera: *Calcispathum*, *Magnesispathum*, *Dolomispathum*, *Ferrispathum*, &c., *Malachita*, *Azuria*, *Gaylusacia*.

CHAPTER IV.

OF THE IDEA OF NATURAL AFFINITY.

1. IN the Second Chapter of this Book it was shown that although the Classificatory Sciences proceed ostensibly upon the Idea of Resemblance as their main foundation, they necessarily take for granted in the course of their progress a further Idea of Natural Affinity. This appeared* by a general consideration of the nature of Science, by the recognition of natural species and genera, even in Artificial Systems of Classification†, and by the attempts of botanists to form a Natural System. It further appeared that among the processes by which endeavours have been made to frame a Natural System, some, as the method of *Blind Trial* and the method or *General Comparison*, have been altogether unsuccessful being founded only upon a collection of resemblances, casual in the one case and arbitrary in the other. In neither of these processes is there employed any general principle by which we may be definitely directed as to what resemblances we should employ, or by which the result at which we arrive may be verified and confirmed. Our object in the present chapter is to show that the Idea of Natural Affinity supplies us with a principle which may answer such purposes.

I shall first consider the Idea of Affinity as exemplified in organized beings. In doing this, we may appear to take for granted Ideas which have not yet come under our discussion, as the Ideas of Organization, and Vital Function; but it will be found that the principle to which we are led is independent of these additional Ideas.

2. We have already seen that the attempts to discover the divisions which result from this Natural Affinity have led to the consideration of the *Subordina-*

* Art. 5. † Art. 7.

tion of Characters. It is easy to see that some organs are more essential than others to the existence of an organized being; the organs of nutrition, for example, more essential than those of locomotion. But at the same time it is clear that any *arbitrary* assumption of a certain scale of relative values of different kinds of characters will lead only to an Artificial System. This will happen, if, for example, we begin by declaring the nutritive to be superior in importance to the reproductive functions. It is clear that this relation of importance of organs and functions must be collected by the study of the organized beings; and cannot be determined *à priori*, without depriving us of all right to expect a general accordance between our system and the arrangement of nature. We see, therefore, that our notion of Natural Affinity involves in it this consequence;—that it is not to be made out by an *arbitrary* subordination of characters.

3. The functions and actions of living things which we separate from each other in our consideration, cannot be severed in nature. Each function is essential; Life implies a collection of movements, and ceases when any of these movements is stopped. A change in the organization subservient to one set of functions may lead necessarily to a change in the organization belonging to others. We can often see this necessary connexion; and from a comparison of the forms of organized beings,—from the way in which their structure changes in passing from one class to another, we are led to the conviction that there is some general principle which connects and graduates all such changes. When the circulatory system changes, the nervous system changes also: when the mode of locomotion changes, the respiration is also modified.

4. These corresponding changes may be considered

as ways in which the living thing it fitted to its mode of life; as marks of *adaptation to a purpose;* or, as it has been otherwise expressed, as results of the *conditions of existence.* But at the present moment, we put forward these correspondencies in a different light. We adduce them as illustrations of what we mean by Affinity, and what we consider as the tendency of a Natural Classification. It has sometimes been asserted that if we were to classify any of the departments of organized nature by means of one function, and then by means of another, the two classifications, if each strictly consistent with itself, would be consistent with each other. Such an assertion is perhaps more than we are entitled to make with confidence; but it shows very well what is meant by Affinity. The disposition to believe such a general identity of all partial natural classifications, shows how readily we fix upon the notion of Affinity, as a general result of the causes which determine the forms of living things. When these causes or principles, of whatever nature they are conceived to be, vary so as to modify one part of the organization of the being, they also modify another: and thus the groups which exhibit this variation of the fundamental principles of form, are the same, whether the manifestation of the change be sought in one part or in another of the organized structure. The groups thus formed are related by Affinity; and in proportion as we find the evidence of more functions and more organs to the propriety of our groups, we are more and more satisfied that they are Natural Classes. It appears, then, that our Idea of Affinity involves the conviction of the *coincidence of natural arrangements formed on different functions;* and this, rather than the principle of the subordination of some characters to others, is the true ground of the natural method of Classification.

5. For example, Cuvier, after speaking of the Subordination of Characters as the guide which he intends to follow in his arrangement of animals, interprets this principle in such a manner* as to make it agree nearly with the one just stated. "In pursuance of what has been said on methods in general, we now require to know what characters in animals are the most influential, and therefore those which must be made the grounds of the primary divisions." "These," he says, "it is clear must be those which are taken from the animal functions;—sensation and motion:"—But how does he confirm this? Not by showing that the animal functions are independent of, or predominant over, the vegetative, but by observing that they follow the same gradations. "Observation," he continues, "confirms this view, by showing that the degrees of developement and complication of the animal functions agree with those of the vegetative. The heart and the organs of the circulation are a sort of center for the vegetative functions, as the brain and the trunk of the nervous system are for the animal functions. Now we see these two systems descend in the scale, and disappear the one with the other. In the lowest animals, when there are no longer any distinct nerves, there are also no longer distinct fibres, and the organs of digestion are simply hollowed out in the homogeneous mass of the body. The muscular system disappears even before the nervous, in insects; but in general the distribution of the medullary masses corresponds to that of the muscular instruments; a spinal cord, on which knots or ganglions represent so many brains, corresponds to a body divided into numerous rings and supported on pairs of members placed at different points of the length, and so on.

"This *correspondence* of the general forms which

* *Règne Animal*, p. 55.

result from the arrangement of the motive organs, from the distribution of the nervous masses, and from the energy of the circulatory system, must therefore form the ground of the first great sections by which we divide the animal kingdom."

6. Decandolle takes the same view. There must be, he says, *an equilibrium* of the different functions*. And he exemplifies this by the case of the distinction of monocotyledonous and dicotyledonous plants, which being at first established by means of the organs of reproduction, was afterwards found to coincide with the distinction of endogenous and exogenous, which depends on the process of nutrition. "Thus," he adds, "*the natural classes founded on one of the great functions of the vegetable are necessarily the same as those which are founded upon the other function*; and I find here a very useful *criterion* to ascertain whether a class is natural: namely, in order to announce that it is so, it must be arrived at by the two roads which vegetable organization presents. Thus I affirm," he says, "that the division of monocotyledons from dicotyledons, and the distinction of Gramineæ from Cyperaceæ, are real, because in these cases, I arrive at the same result by the reproductive and the nutritive organs; while the distinction of monopetalous and polypetalous, of Rhodoraceæ and Ericineæ appears to me artificial, because I can arrive at it only by the reproductive organs."

Thus the correspondence of the indications of different functions is the criterion of Natural Classes; and this correspondence may be considered as one of the best and most characteristic marks of the fundamental Idea of Affinity. And the Maxim by which all Systems professing to be natural must be tested is this:—that the *arrangement obtained from one set of characters coincides with the arrangement obtained from another set.*

* *Theor. Elem.*, p. 79.

This Idea of Affinity, as a natural connexion among various species, of which connexion all particular resemblances are indications, has principally influenced the attempts at classifying the animal kingdom. The reason why the classification in this branch of Natural History has been more easy and certain than that of the vegetable world is, as Decandolle says*, that besides the functions of nutrition and reproduction, which animals have in common with plants, they have also in addition the function of sensation; and thus have a new means of verification and concordance. But we may add, as a further reason, that the functions of animals are necessarily much more obvious and intelligible to us than those of vegetables, from their clear resemblance to the operations which take place in our own bodies, to which our attention has necessarily been strongly directed.

7. The question here offers itself, whether this Idea of Natural Affinity is applicable to inorganic as well as to organic bodies;—whether there be Natural Affinities among Minerals. And to this we are now enabled to reply by considering whether or not the principle just stated is applicable in such cases. And the conclusion to which our principle leads us is,—that there are such Natural Affinities among Minerals, since there are different sets of characters which may be taken, (and have by different writers been taken,) as the basis of classification. The hardness, specific gravity, colour, lustre, crystallization, and other *external* characters, as they are termed, form one body of properties according to which minerals may be classified; as has in fact been done by Mohs, Breithaupt, and others. The *chemical* constitution of the substances, on the other hand, may be made the principle of their arrangement, as was done by Haüy, and more recently, and on a different scheme, by Berzelius. Which of these is the true and natural

* *Theor. Elem.*, p. 80.

classification? To this we answer, that *each* of these arrangements is true and natural, then, and then only, when it coincides with the other. An arrangement by external characters which gives us classes possessing a common chemical character;—a chemical order which brings together like and separates unlike minerals;—such classifications have the evidence of truth in their agreement with one another. Every classification of minerals which does not aim at and tend to such a result, is so far merely arbitrary; and cannot be subservient to the expression of general chemical and mineralogical truths, which is the proper purpose of such a classification.

8. In the History of Mineralogy I have related the advances which have been made among mineralogists and chemists in modern times towards a System possessing this character of truth. I have there described the mixed systems of Werner and Haüy;—the attempt made by Mohs to form a pure Natural History system;—the first and second attempt of Berzelius to form a pure chemical system; and the failure of both these attempts. But the distinct separation of the two elements of which science requires the coincidence threw a very useful light upon the subject; and the succeeding mixed systems, such as that of Naumann, approached much nearer to the true conditions of the problem than any of the preceding ones had done. Thus, as I have stated, several of Naumann's groups have both a common chemical character and great external resemblances. Such are his *Anhydrous Unmetallic Haloids*—his *Anhydrous Metallic Haloids*—*Hydrous Metallic Haloids*—*Oxides* of metals—*Pyrites*—*Glances*—*Blendes*. The existence of such groups shows that we may hope ultimately to obtain a classification of minerals which shall be both chemically significant and agreeable to the

methods of Natural History: although when we consider how very imperfect as yet our knowledge of the chemical composition of minerals is, we can hardly flatter ourselves that we shall arrive at such a result very soon.

We have thus seen that in Mineralogy, as well as in the sciences which treat of organized bodies, we may apply the Idea of Natural Affinity; of which the fundamental maxim is, that *arrangements obtained from different sets of characters must coincide.*

Since the notion of Affinity is thus applicable to inorganic as well as to organic bodies, it is plain that it is not a mere modification of the Idea of Organization or Function, although it may in some of its aspects appear to approach near to these other Ideas. But these Ideas, or others which are the foundation of them, necessarily enter in a very prominent and fundamental manner into all the other parts of Natural History. To the consideration of these, therefore, we shall now proceed.

BOOK IX.

THE PHILOSOPHY OF BIOLOGY.

Chapter I.

ANALOGY OF BIOLOGY WITH OTHER SCIENCES.

1. In the History of the Sciences, after treating of the Sciences of Classification, we proceeded to what are there termed the Organical Sciences, including in this term Physiology and Comparative Anatomy. A peculiar feature in this group of sciences is that they involve the notion of *living* things. The notion of *Life*, however vague and obscure it may be in men's minds, is apprehended as a peculiar Idea, not resolvable into any other Ideas, such, for instance, as Matter and Motion. The separation between living creatures and inert matter, between organized and unorganized beings, is conceived as a positive and insurmountable barrier. The two classes of objects are considered as of a distinct kind, produced and preserved by different forces. Whether the Idea of Life is really thus original and fundamental, and whether, if so, it be one Idea only, or involve several, it must be the province of true philosophy to determine. What we shall here offer may be considered as an attempt to contribute something to the determination of these questions; but we shall perhaps be able to make it appear that science is at present only in the course of its progress towards a complete solution of such problems.

Since the main feature of those sciences of which we have now to examine the philosophy is, that they involve the Idea of Life, it would be desirable to have them designated by a name expressive of that circumstance. The word *Physiology*, by which they have most commonly been described, means *the Science of Nature;* and though it would be easy to explain, by reference to history, the train of thought by which the word was latterly restricted to *Living Nature*, it is plain that the name is, etymologically speaking, loose and improper. The term *Biology*, which means exactly what we wish to express, *the Science of Life*, has often been used, and has of late become not uncommon among good writers. I shall therefore venture to employ it, in most cases, rather than the word *Physiology*.

2. As I have already intimated, one main inquiry belonging to the Philosophy of Biology, is concerning the Fundamental Idea or Ideas which the science involves. If we look back at the course and the results of our disquisitions respecting other sciences in this work, and assume, as we may philosophically do, that there will be some general analogy between those sciences and this, in their developement and progress, we shall be enabled to anticipate in some measure the nature of the view which we shall now have to take. We have seen that in other subjects the Fundamental Ideas on which science depended, and the Conceptions derived from these, were at first vague, obscure, and confused;—that by gradual steps, by a constant union of thought and observation, these conceptions become more and more clear, more and more definite;—and that when they approached complete distinctness and precision, there were made great positive discoveries into which these conceptions entered, and thus the new precision of thought was fixed and perpetuated in some conspicuous and lasting

truths. Thus we have seen how the first confused mechanical conceptions (Force, and the like,) were, from time to time, growing clearer, down to the epoch of Newton;—how true conceptions of Genera and of wider classes, gradually unfolded themselves among the botanists of the sixteenth and seventeenth centuries;—how the idea of Substance became steady enough to govern the theories of chemists only at the epoch of Lavoisier;—how the Idea of Polarity, although often used by physicists and chemists, is even now somewhat vague and indistinct in the minds of the greater part of speculators. In like manner we may expect to find that the Idea of Life, if indeed *that* be the governing Idea of the Science which treats of Living Things, will be found to have been gradually approaching towards a distinct and definite form among the physiologists of all ages up to the present day. And if this be the case, it may not be considered superfluous, with reference to so interesting a subject, if we employ some space in tracing historically the steps of this progress;—the changes by which the originally loose notion of Life, or of Vital Powers, became more nearly an Idea suited to the purposes of science.

3. But we may safely carry this analogy between Biology and other sciences somewhat further. We have seen, in other sciences, that while men in their speculations were thus tending towards a certain peculiar Idea, but before they as yet saw clearly that it was peculiar and independent, they naturally and inevitably clothed their speculations in conceptions borrowed from some other extraneous idea. And the unsatisfactoriness of all such attempts, and the necessary consequence of this, a constant alteration and succession of such inappropriate hypotheses, were indications and aids of the progress which was going on towards a more genuine form

of the science. For instance, we have seen that in chemistry, so long as men refused to recognize a peculiar and distinct kind of power in the *Affinity* which binds together the elements of bodies, they framed to themselves a series of hypotheses, each constructed according to the prevalent ideas of the time, by which they tried to represent the relation of the compound to the ingredients:—first, supposing that the elements bestowed upon the whole qualities *resembling* their own:—then giving up this supposition, and imagining that the properties of the body depended upon the *shape* of the component particles;—then, as their view expanded, assuming that it was not the shape, but the mechanical *forces* of the particles which gave the body its attributes; —and finally acquiescing in, or rather reluctantly admitting, the idea of *Affinity*, conceived as a peculiar power, different not only from material contact, but from any mechanical or dynamical attraction.

Now we cannot but think it very natural, if we find that the history of Biology offers a series of occurrences of the same nature. The notions of Life in general, or of any Vital Functions or Vital Forces in particular, are obviously very loose and vague as they exist in the minds of most men. The discrepancies and controversies respecting the definitions of all such terms, which are found in all works on physiology, afford us abundant evidence that these notions are not, at least not generally, apprehended with complete clearness and steadiness. We shall therefore find approaches and advances, intermediate steps, gradually leading up to the greatest degree of distinctness which has yet been attained. And in those stages of imperfect apprehension in which the notions of Life and of Vital Powers are still too loose and unformed to be applied independently, we may expect to find them supported and embodied by means of hypotheses borrowed from other subjects, and thus, made so distinct and substantial as to supply at least a temporary possibility of scientific reasoning upon the laws of life.

4. For example, if we suppose that men begin to speculate upon the properties of living things, not acknowledging a peculiar Vital Power, but making use successively of the knowledge supplied by the study of other subjects, we may easily imagine a series of hypotheses along which they would pass.

They would probably, first, in this as in other sciences, have their thoughts occupied by vague and *mystical* notions in which material and spiritual agency, natural and supernatural events, were mixed together without discrimination, and without any clear notion at all. But as they acquired a more genuine perception of the nature of knowledge, they would naturally try to explain vital motions and processes by means of such forces as they had learnt the existence of from other sciences. They might first have a *mechanical* hypothesis, in which the mechanical forces of the solids and fluids which compose organized bodies should be referred to, as the most important influences in the process of life. They might then attend to the actions which the fluids exercise in virtue of their affinity, and might thus form a *chemical* theory. When they had proved the insufficience of these hypotheses, borrowed from the powers which matter exhibits in other cases, they might think themselves authorized to assume some peculiar power or agency, still material, and thus they would have the hypothesis of a *vital fluid*. And if they were driven to reject this, they might think that there was no resource but to assume an immaterial principle of life, and thus they would arrive at the doctrine of an animal *soul*.

Now, through the cycle of hypotheses which we have thus supposed, physiology has actually passed. The conclusions to which the most philosophical minds have been led by a survey of this progress is, that by the failure of all these theories, men have exhausted this path of inquiry, and shown that scientific truth is to be sought in some other manner. But before I proceed further to illustrate this result, it will be proper, as I have already stated, to exhibit historically the various hypotheses which I have described. In doing this I shall principally follow the *History of Medicine* of Sprengel. It is only by taking for my guide a physiologist of acknowledged science and judgment, that I can hope, on such a subject, to avoid errours of detail. I proceed now to give in succession an account of the Mystical, the Iatrochemical, the Iatromathematical, and the Vital-Fluid Schools; and finally of the Psychical School, who hold the Vital Powers to be derived from the Soul (*Psychè*).

Chapter II.

SUCCESSIVE BIOLOGICAL HYPOTHESES.

Sect. I.—*The Mystical School.*

In order to abbreviate as much as can conveniently be done the historical view which I have now to take, I shall altogether pass over the physiological speculations of the ancients, and begin my survey with the general revival of science in modern times.

We need not dwell long on the fantastical and unsubstantial doctrines concerning physiology which prevailed in the sixteenth century, and which flowed in a great measure from the fertile but ill-regulated imaginations of the cultivators of Alchemy and Magic. One of the prominent doctors of this school is the celebrated Paracelsus, whose doctrines contained a combination of biblical interpretations, visionary religious notions, fanciful analogies, and bold experiments in practical medicine. The opinion of a close but mystical resemblance of parts between the universe and the human body,—the *Macrocosm* and the *Microcosm*,—as these two things, thus compared, were termed, had probably come down from the Neoplatonists; it was adopted by the Paracelsists*, and connected with various astrological dreams and cabbalistic riddles. A succession of later Paracelsists†, Rosicrucians, and other fanatics of the same kind, continued into the seventeenth century. Upon their notions was founded the pretension of curing wounds by a sympathetic powder, which Sir Kenelm Digby, among others, asserted; while animal magnetism, and the transfer of diseases from one person to another‡, were maintained by others of this school. They held, too, the doctrines of *astral bodies* corresponding to each terrestrial body; and of the *signatures* of plants, that is, certain features in their external form by which their virtues might be known. How little advantage or progress real physiology could derive from speculations of this kind may be seen from this, that their tendency was to obliterate the distinction between living and lifeless things: according to Paracelsus, all things are alive, eat, drink, and excrete; even minerals and fluids§. According to him and his school, besides material and immaterial beings, there are *elementary Spirits* which hold an intermediate place, *Sylvans*, *Nymphs*, *Gnomes*, *Salamanders*, &c. by whose agency various processes of enchantment may be achieved, and things apparently supernatural explained. Thus this spiritualist scheme dealt with a world of its own by

* Spr., III. 456. † *Ib.*, IV. 270. ‡ *Ib.*, IV. 276.

§ *Ib.*, III. 458. Parac. *De Vita Rerum Naturalium*, p. 889.

means of fanciful inventions and mystical visions, instead of making any step in the study of nature.

Perhaps, however, one of the most fantastical of the inventions of Paracelsus may be considered as indicating a perception of a peculiar character in the vital powers. According to him, the business of digestion is performed by a certain demon whom he calls *Archæus*, who has his abode in the stomach, and who, by means of his alchemical processes, separates the nutritive from the harmful part of our food, and makes it capable of assimilation*. This fanciful notion was afterwards adopted and expanded by Van Helmont†. According to him the stomach and spleen are both under the direction of this Master-spirit, and these two organs form a sort of *Duumvirate* in the body.

But though we may see in such writers occasional gleams of physiological thought, the absence of definite physical relations in the speculations thus promulgated was necessarily intolerable to men of sound understanding and scientific tendencies. Such men naturally took hold of that part of the phenomena of life which could be most distinctly conceived, and which could be apparently explained by means of the sciences then cultivated; and this was the part which appeared to be reducible to chemical conceptions and doctrines. It will readily be supposed that the processes of chemistry have a considerable bearing upon physiological processes, and might, till their range was limited by a sound investigation, be supposed to have still more than they really had; and thus a Physiology was formed which depended mainly upon Chemistry, and the school which held this doctrine has been called the *Iatrochemical* School.

* Spr., III. 468. † *Ib.*, IV. 302.

SECT. II.—*The Iatrochemical School.*

That all physical properties, and therefore chemical relations, have a material influence on physiological results, was already recognized, though dimly, in the Galenic doctrine of the "four elementary qualities." But at the time of Paracelsus, chemical action was more distinctly than before separated from other kinds of physical action; and therefore a physiological doctrine, founded upon chemistry, and freed from the extravagance and mysticism of the Paracelsists, was a very promising path of speculation. Andrew Libavius* of Halle, in Saxony, Physician and Teacher in the Gymnasium at Koberg, is pointed out by Sprengel as the person who began to cultivate chemistry, as distinct from the theosophic fantasies of his predecessors; and Angelus Sala of Vienna†, as his successor. The latter has the laudable distinction of having rejected the prevalent conceits about potable gold, a universal medicine, and the like‡. In Germany already at the beginning of the seventeenth century a peculiar chair of *Chymiatria* was already created at Marpurg: and many in various places pursued the same studies, till, in the middle of the seventeenth century, we come to Lemery§, the principal reformer of pharmaceutical chemistry. But we are not here so much concerned with the practical as with the theoretical parts of Iatrochemistry; and hence we pass on to Sylvius‖ and his system.

The opinion that chemistry had an important bearing upon physiology did not, however, begin with Sylvius. Paracelsus, among his extravagant absurdities, did some service to medicine by drawing attention to this important truth. He used¶ chemical principles for the explanation

* Spr., III. 550. † *Ib.*, IV. 281. ‡ *Ib.*, IV. 283.
§ *Ib.*, IV. 291. ‖ *Ib.*, IV. 336. ¶ *Ib.*, III. 472.

of particular diseases; most or all diseases according to him, arise from the effervescence of salts, from the combustion of sulphur, or from the coagulation of mercury. His medicines were chemical preparations; and it was* an undeniable advantage of the Paracelsian doctrine that chemistry thus became indispensable to the physician. We still retain a remnant of the chemical nomenclature of Paracelsus in the term *tartar*, denoting the stony concretion which forms on the teeth†. According to him there is a certain substance, the basis of all diseases which arise from a thickening of the juices and a collection of earthy matter; and this substance he calls *Tartarus*, because it "burns like the fire of hell." Helmont, the successor of Paracelsus in many absurdities, also followed him in the attempt to give a chemical account, however loose and wild, of the functions of the human body; and is by Sprengel considered, with all his extravagancies, as a meritorious and important discoverer. The notion of the fermentation of fluids‡, and of the aërial product thence resulting, to which he gave the name of *Gas*, forms an important part of his doctrines; and of the six digestions which he assumes, the *first* prepares an acid, which is neutralized by the gall when it reaches the duodenum, and this constitutes the *second* digestion.

I have already, in the History of Chemistry§, stated, that the doctrine of the opposition of acid and alkali, the great step which theoretical chemistry owes to Sylvius, was first brought into view as a physiological tenet, although we had then to trace its consequences in another science. The explanation of all the functions of the animal system, both healthy and morbid, by means of this and other chemical doctrines, and the prescription of methods of cure founded upon such explanations,

* Spr., III. 482.
† *Ib.*, III. 475.
‡ Vol. v., 315.
§ *Hist. Ind. Sci.*, B. XIII. c. 2.

form the scheme of the *iatrochemical* school; a school which almost engrossed the favour of European physicians during the greater part of the seventeenth century.

Sylvius taught medicine at Leyden, from the year 1658, with so much success, that Boerhaave alone surpassed him *. His notions, although he piqued himself on their originality, were manifestly suggested in no small degree (as all such supposed novelties are) by the speculations of his predecessors, and the spirit of the times. Like Helmont †, he considers digestion as consisting in a fermentation; but he states it more definitely as the effervescence of an acid, supplied by the saliva and the pancreatic juice, with the alkali of the gall. By various other hypothetical processes, all of a chemical nature, the blood becomes a collection of various juices, which are the subjects of the speculations of the iatrochemists, to the entire neglect of the solid parts of the body. Diseases were accounted for by a supposed prevalence of one or the other of the acrid principles, the acid or the alkaline: and Sylvius ‡ was bold enough to found upon these hypotheses practical methods of cure, which were in the highest degree mischievous.

The Sylvian doctrine was often combined with some of the notions of the Cartesian system of philosophy; but this mixture I shall not notice, since my present object is to trace the history of a mere chemical physiology as one of the unsuccessful attempts at a philosophy of life. With various modifications, this doctrine was diffused over Europe. It gave rise to several controversies, which turned upon the questions of the novelty of the doctrine, and the use of chemical remedies to which it pointed, as well as upon its theoretical truth. We need not dwell long upon these controversies, al-

* Spr., IV. 336. † *Ib.*, 338. ‡ *Ib.*, IV. 345.

though they were carried on with no small vehemence in their time. Thus the school of Paris opposed all innovation, remained true to the Galenic dogmatism, and declared itself earnestly against all combination of chemistry with medicine; and even against the chemical preparation of medicaments. Guy Patin, a celebrated and learned professor of that day, declares* that the chemists are no better than forgers, and ought to be punished as such. The use of antimonial medicines was a main point of dispute between the iatrochemists and their opponents; Patin maintained that more men had been destroyed by antimony than by the thirty years' war of Germany; and endeavoured to substantiate this assertion by collecting all such cases in his *Martyrologium Antimonii*. It must have been a severe blow to Patin when†, in 1666, the Doctors of the Faculty of Paris, assembled by command of the parliament, declared, by a majority of ninety-two voices, that the use of antimonial medicines was allowable and laudable, and when all attempts to set aside this decision failed.

Florentius Schuyl of Leyden sought to recommend the iatrochemical doctrines, by maintaining that they were to be found in the Hippocratic writings; nor was it difficult to give a chemical interpretation of the humoral pathology of the ancients. The Italian ‡ physicians also, for the most part, took this line, and attempted to show the agreement of the principles of the ancient school of medicine with the new chemical notions. This, indeed, is the usual manner in which the diffusion of new theoretical ideas becomes universal.

The progress of the chemical school of medicine in England § requires our more especial notice. Willis was the most celebrated champion of this sect. He assumed, but with modifications of his own, the three Paracelsian

* Spr., 349. † *Ib.*, IV. 350. ‡ *Ib.*, 368. § *Ib.*, 353.

principles, Salt, Sulphur, and Mercury; considered digestion as the effect of an acid, and explained other parts of the animal economy by distillation, fermentation, and the like. All diseases arise from the want of the requisite *ferment;* and the physician, he says*, may be compared to a vintner, since both the one and the other have to take care that the necessary fermentations go on, that no foreign matter mixes itself with the wine of life, to interrupt or derange those operations. In the middle of the seventeenth century, medicine had reached a point in which the life of the animal body was considered as merely a chemical process; the wish to explain everything on known principles left no recognized difference between organized and unorganized bodies, and diseases were treated according to this delusive notion. The condition of chemistry itself during this period, though not one of brilliant progress, was sufficiently stable and flourishing to give a plausibility to any speculation which was founded on chemical principles; and the real influence of these principles in the animal frame could not be denied.

The iatrochemists were at first resisted, as we have seen, by the adherents of the ancient schools; they were attacked on various grounds, and finally deposed from their ascendency by another sect, which we have to speak of, as the iatromathematical, or mechanical school. This sect was no less unsatisfactory and erroneous in its positive doctrines than the chemists had been; for the animal frame is no more a mere machine than a mere laboratory: but it promoted the cause of truth, by detecting and exposing the insufficient explanations and unproved assertions of the reigning theory.

Boyle was one of the persons who first raised doubts against the current chemical doctrines of his time, as we

* Spr., 354.

have elsewhere noted; but his objections had no peculiar physiological import. Hermann Conring*, the most learned physician of his time, a contemporary with Sylvius, took a view more pertinent to our present object; for he not only rejected the alchemical and hermetical medicines, but taught expressly that chemistry, in its then existing condition, was better fitted to be of use in the practice of pharmacy, than in the theories of physiology and pathology. He made the important assertion, also, that chemical principles do not pre-exist *as such* in the animal body; and that there are higher powers which operate in the organic world, and which do not depend on the form and mixture of matter.

Attempts were made to prove the acid and alkaline nature of the fluids of the human body by means of experiments, as by John Viridet of Geneva†, and by Raimond Vieussens‡, the latter of whom maintained that he had extracted an acid from the blood, and detected a ferment in the stomach. In opposition to him, Hecquet, a disciple of the iatromathematical school, endeavoured to prove that digestion was performed, not by means of fermentation, but by trituration. Hecquet's own opinions cannot be defended; but his objections to the chemical doctrines, and his assertion of the difference of chemical and organical processes, are evidences of just thought§.

The most important opponents of the iatrochemical school were Pitcairn in England, Bohn and Hoffman in Germany, and Boerhaave in Holland. These eminent physicians, about the end of the seventeenth century, argued on the same grounds of observation, that digestion is not fermentation, and that the Sylvian accounts

* Spr., IV. 361. † *Ib.*, IV. 329.
‡ *Ib.*, 350, (1715.) § *Ib.*, 401.

of the origin of diseases by means of acid and alkali are false. The arguments and authority of these and other persons finally gained an ascendancy in the medical world, and soon after this period we may consider the reign of the chemical school of physiology as past. In fact, the attempts to prove its assertions experimentally were of the feeblest kind, and it had no solid basis on which it could rest, so as to resist the shock of the next hypothesis which the progress of the physical sciences might impel against it. We may, therefore, now consider the opinion of the mere chemical nature of the vital processes as disproved, and we proceed next to notice the history of another unsuccessful essay to reduce vital actions to known actions of another kind.

SECT. III.—*The Iatromathematical School.*

In the first Section of this chapter, we enumerated the biological hypotheses which at first present themselves, as the mystical, the mechanical, the chemical. We might have expected that they should occur to men's minds in the order thus stated: and in fact they did so; for the physiology of the ancient materialists, as Democritus and Lucretius, is mechanical so far as it is at all distinct in its views, and thus the mechanical preceded the chemical doctrine. But in modern times, the fluid or chemical physiology was developed before the solid or mechanical: of which the reason appears to have been this;—that Mechanics and Chemistry began to assume a scientific character about the same time; and that of the two, Chemistry not only appeared at first sight more applicable to the functions of the body, because all the more rapid changes appear to be connected with modifications of the fluids of the animal system, but also, by its wider range of facts and more indefinite principles, afforded a better temporary refuge for the mind when perplexed by the difficulties and mysteries which spring out of the speculations concerning life. But if Chemistry was thus at first a more inviting field for the physiologist, Mechanics soon became more attractive in virtue of the splendid results obtained by the schools of Galileo and Newton. And when the insufficiency of chemical physiology was discovered by trial, as we have seen it was, the hope naturally arose, that the mechanical principles which had explained so many of the phenomena of the external universe might also be found applicable to the smaller world of material life;—that the *microcosm* as well as the *macrocosm* might have its mechanical principles. From this hope sprung the Iatromathematical School, or school of Mechanical Physiologists.

We may, however, divide this school into two parts, the Italian, and the Cartesio-Newtonian sect. The former employed themselves in calculating and analyzing a number of the properties of the animal frame which are undoubtedly mechanical; the latter, somewhat intoxicated by the supposed triumphs of the corpuscular philosophy, endeavoured to extend these to physiology, and for this purpose introduced into the subject many arbitrary and baseless hypotheses. I will very briefly mention some of the writers of both these sects.

The main points to which the Italian or genuine Mechanical Physiologists attended, were the application of mechanical calculations to the force of the muscles, and of hydraulical reasonings to the motion of the fluids of the animal system. The success with which Galileo and his disciples had pursued these branches of mechanical philosophy, and the ascendency which they had obtained, first in Italy, and then in other lands, made such speculations highly interesting. Borelli may be considered as the first great name in his line, and his book, *De Motu Animalium*, (*Opus Posthumum*, Romæ, 1680,) is even now a very instructive treatise on the force and action of the bones and muscles. This, certainly one of the most valuable portions of mechanical physiology, has not even yet been so fully developed as it deserves, although John Bernoulli* and his son Daniel† applied to it the resources of analysis, and Pemberton‡, in England, pursued the same subject. Other of these mechanico-physiological problems consisted in referring the pressure of the blood and of the breath to hydrostatical principles. In this manner Borelli was led to assert that the muscles of the heart exert a force of 180,000 pounds§. But a little later, Keill reduced this force to a few ounces‖. Keill and others attempted to determine, on similar principles, the velocity of the blood; we need not notice the controversies which thus arose, since there is not involved in them any peculiar physiological principle.

The peculiar character of the iatromathematical school, as an attempt at physiological theory, is more manifest in its other section, which we have called the Cartesio-Newtonian. The Cartesian system pretended to account for the appearances and changes of bodies by means of the size, figure, and motion of their minute particles. And though this system in its progress towards the intellectual empire of Europe was suddenly overturned by the rise of the Newtonian philosophy, these corpuscular doctrines rather gained than lost by the revolution; for the Newtonian philosophy enlarged the powers of the corpuscular hypothesis, by adding the effects of the attractive and repulsive forces of particles to those of their form and motion. By this means, although Newton's discoveries did not in fact augment

* *De Motu Musculorum.* † *Act. Acad. Petrop.*, I. 170.
‡ *Course of Physiology*, 1773. § Spr., IV. 110. ‖ *Ib.*, 443.

the probability of the corpuscular hypothesis, they so far increased its plausibility, that this hypothesis found favour both with Newton himself and his contemporaries, no less than it had done with the Cartesians.

The attempt to apply this corpuscular hypothesis to physiology was made by Des Cartes himself. The general character of such speculations may easily be guessed*. The secretions are effected by the organs operating after the manner of sieves. Round particles pass through cylindrical tubes, pyramidal ones through triangular pores, cubical particles through square apertures, and thus different kinds of matter are separated. Similar speculations were pursued by other mathematicians: the various diameter of the vessels†, their curvatures, folds, and angles, were made subjects of calculation. Bellini, Donzellini, Gulielmini, in Italy; Perrault, Dodart, in France; Cole, Keill, Jurin, in England, were the principal cultivators of such studies. In the earlier part of the eighteenth century, physiological theorists considered it as almost self-evident that their science required them to reason concerning the size and shape of the particles of the fluids, the diameter and form of the invisible vessels. Such was, for instance, the opinion of Cheyne‡, who held that acute fevers arise from the obstruction of the glands, which occasions a more vehement motion of the blood. Mead, the physician of the King, and the friend of Newton, in like manner explained the effects of poisons by hypotheses concerning the form of their particles§, as we have already seen in speaking of chemistry.

It is not necessary for us to dwell longer on this subject, or to point out the total insufficiency of the mere mechanical physiology. The iatrochemists had neglected

* Spr., IV. 329. † *Ib.*, 432. ‡ *Ib.*, 223.

§ *Mechanical Account of Poisons.* 1702.

the effect of the solids of the living frame; the iatro-mathematicians attended only to these*. And even these were considered only as canals, as cords, as levers, as lifeless machines. These reasoners never looked for any powers of a higher order than the cohesion, the resistance, the gravity, the attraction, which operate in inert matter. If the chemical school assimilated the physician to a vintner or brewer, the mechanical physiologists made him an hydraulic engineer; and, in fact, several of the iatromathematicians were at the same time teachers of engineering and of medicine.

Several of the reasoners of this school combined chemical with their mechanical principles; but it would throw no additional light upon the subject to give any account of these, and I shall therefore go on to speak of the next form of the attempt to explain the processes of life.

SECT. IV.—*The Vital-Fluid School.*

I speak here, not of that opinion which assumes some kind of fluid or ether as the means of communication along the *nerves* in particular, but of the hypothesis that *all* the peculiar functions of *life* depend upon some subtile ethereal substance diffused through the frame;—not of a *Nervous* Fluid, but of a *Vital* Fluid. Again, I distinguish this opinion from the doctrine of an *immaterial* vital power or principle, an Animal Soul, which will be the subject of the next Section: nor is this distinction insignificant; for a material element, however subtile, however much spiritualized, must still act everywhere according to the same laws; whereas we do not conceive an immaterial spirit or soul to be subject to this necessity.

The iatromathematical school could explain to their

* Spr., IV. 419.

own satisfaction how motions, once begun, were transferred and modified; but in many organs of the living frame there seemed to be a power of beginning motion, which is beyond all mere mechanical action. This led to the assumption of a Principle of a higher kind, though still material. Such a Principle was asserted by Frederick Hoffmann, who was born at Halle, in 1660*, and became Professor of Medicine at the newly-established University there in 1694. According to him†, the reason of the greater activity of organized bodies lies in the influence of a material substance of extreme subtilty, volatility, and energy. This is, he holds, no other than the Ether, which, diffused through all nature, produces in plants the bud, the secretion and motion of the juices, and is separated from the blood and lodged in the brain of animals‡. From this, acting through the nerves, must be derived all the actions of the organs in the animal frame; for when the influence of the nerve upon the muscle ceases, muscular motion ceases also.

The mode of operation of this vital fluid was, however, by no means steadily apprehended by Hoffmann and his followers. Its operations are so far mechanical§ that all effects are reduced to motion, yet they cannot be explained according to known mechanical laws. At one time the effects are said to take place according to laws of a Higher Mechanics which are still to be discovered||. At another time, in complete contradiction of the general spirit of the system, metaphysical conceptions are introduced: each particle of the vital fluid is said to have a determined *idea* of the whole mechanism and organism¶, and according to this,

* Spr., v. 254.

† *Ib.*, v. 257.

‡ *De Differentiâ Organismi et Mechanismi*, pp. 48, 67.

§ Spr., v. 262, 3.

|| Hoffmann, *Opp.*, Vol. v. p. 123.

¶ *De Diff. Organ. et Mechan.*, p. 81.

it forms the body and preserves it by its motion. By means of this fluid the soul operates upon the body, and the instincts and the passions have their source in this material sensitive soul. This attribution of ideas to the particles of the fluid is less unaccountable when we recollect that something of the same kind is admitted into Leibnitz's system, whose Monads have also ideas.

Notwithstanding its inconsistencies, Hoffmann's system was received with very general favour both in Germany and in the rest of Europe; the more so, inasmuch as it fell in very well with the philosophy both of Leibnitz and of Newton. The Newtonians were generally inclined to identify the Vital Fluid with the Ether, of which their master was so strongly disposed to assume the existence: and indeed he himself suggested this identification.

When the discoveries made respecting Electricity in the course of the eighteenth century had familiarized men with the notion of a pervading subtile agent, invisible, intangible, yet producing very powerful effects in every part of nature, physiologists also caught at the suggestion of such an agent, and tried, by borrowing or imitating it, to aid the imperfection of their notions of the vital powers. The Vital Principle* was imagined to be a substance of the same kind, by some to be the same substance, with the Electric Fluid. By its agency all these processes in organized bodies were accounted for which cannot be explained by mechanical or chemical laws, as the secretion of various matters (tears, milk, bile, &c.) from an homogeneous fluid, the blood; the production of animal heat, digestion, and the like. According to John Hunter, this attenuated substance pervaded the blood itself, as well as the solid organic frame; and the changes which take place in the blood

* Prichard, *On the Doctrine of a Vital Principle*, p. 12.

which has flowed out of the veins into a basin are explained by saying that it is, for a time, till this vital fluid evaporates, truly alive.

The notion of a Vital Fluid appears also to be favourably looked upon by Cuvier; although with him this doctrine is mainly put forwards in the form of a Nervous Fluid. Yet in the following passage he extends the operation of such an agent to all the vital functions*. "We have only to suppose that all the medullary and nervous parts produce the Nervous Agent, and that they alone conduct it; that is, that it can only be transmitted by them, and that it is changed or consumed by their actions. Then everything appears simple. A detached portion of muscle preserves for some time its irritability, on account of the portion of nerve which always adheres to it. The sensibility and the irritability reciprocally exhaust each other by their exercise, because they change or consume the same agent. All the interior motions of digestion, secretion, excretion, participate in this exhaustion, or may produce it. All local excitation of the nerves brings thither more blood by augmenting the irritability of the arteries, and the afflux of blood augments the real sensibility by augmenting the production of the nervous agent. Hence the pleasures of titillations, the pains of inflammation. The particular sensations increase in the same manner and by the same causes; and the imagination exercises, (still by means of the nerves,) upon the internal fibres of the arteries or other parts, and through them on the sensations, an action analogous to that of the will upon the voluntary motions. As each exterior sense is exclusively disposed to admit the substances which it is to perceive, so each interior organ, secretory or other, is also more excitable by some one agent than by another: and

* *Hist. Sci. Nat. depuis* 1789, I. 214.

hence arises what has been called the *proper sensibility* or *proper life of the organs;* and the influence of specifics which, introduced into the general circulation, affect only certain parts. In fine, if the nervous agent cannot become sensible to us, the reason is that all sensation requires that this agent should be altered in some way or other; and it cannot alter itself.

"Such is the summary idea which we may at present form of the mutual and general working of the vital powers in animals."

Against the doctrine of a Vital Fluid as one uniform material agent pervading the organic frame, an argument has been stated which points out extremely well the philosophical objection to such an hypothesis*. If the Vital Principle be the *same* in all parts of the body, how does it happen, it is asked, that the secretions are so *different?* How do the particles in the blood, separated from their old compounds and united into new ones, under the same influence, give origin to all the different fluids which are produced by the glands? The liver secretes bile, the lacrymal gland, tears, and so on. Is the Vital Principle different in all these organs? To assert this, is to multiply nominal principles without limit, and without any advance in the explanation of facts. Is the Vital Principle the same, but its operation modified by the structure of the organ? We have then two unknown causes, the Vital Principle and the Organic Structure, to account for the effect. By such a multiplication of hypotheses nothing is gained. We may as well say at once, that the structure of the organ, acting by laws yet unknown, is the cause of the peculiar secretion. It is as easy to imagine this structure acting to produce the whole effect, as it is to imagine it modifying the activity of another agent. Thus the hypothesis of the Vital Fluid

* Prichard, *On a Vital Principle*, p. 98.

in this form explains nothing, and does not in any way help onwards the progress of real biological knowledge.

The hypothesis of an *immaterial* vital principle must now be considered.

SECT. V.—*The Psychical School.*

The doctrine of an Animal Soul as the principle which makes the operations of organic different from those of inorganic matter, is quite distinct from, and we may say independent of, the doctrine of the soul as the intelligent, moral, responsible part of man's nature. It is the former doctrine alone of which we have here to speak, and those who thus hold the existence of an immaterial agent as the cause of the phenomena of life, I term the *Psychical School.*

Such a view of the constitution of living things is very ancient. For instance, Aristotle's Treatise "*on the Soul*," goes entirely upon the supposition that the Soul is the cause of motion, and he arrives at the conclusion that there are different *parts* in the Soul; the *nutritive* or *vegetative*, the *sensitive*, and the *rational* *.

But this doctrine is more instructive to us, when it appears as the antagonist of other opinions concerning the nature of life. In this form it comes before us as promulgated by Stahl, whom we have already noticed as one of the great discoverers in chemistry. Born in the same year as Hoffmann, and appointed at his suggestion professor at the same time in the same new university of Halle, he soon published a rival physiological theory. In a Letter to Lucas Schröck, the president of the Academy of Naturalists, he describes the manner in which he was led to form a system for himself †. Educated in the tenets of Sylvius and Willis, according to which all diseases are derived from the acidity of the fluids, Stahl,

* Arist. Περὶ Ψυχῆς, II. 2. † Spr., v. 303.

when a young student, often wondered how these fluids, so liable to be polluted and corrupted, are so wonderfully preserved through innumerable external influences, and seem to be far less affected by these than by age, constitution, passion. No material cause could, he thought, produce such effects. No attention to mechanism or chemistry alone could teach us the true nature and laws of organization.

So far as Stahl recognized the influence, in living bodies, of something beyond the range of mechanics and chemistry, there can be no doubt of the sound philosophy of his views; but when he proceeds to found a positive system of physiology, his tenets become more precarious. The basis of his theory is this*: the body has, as body, no power to move itself, and must always be put in motion by immaterial substances. All motion is a spiritual act†. The source of all activity in the organic body, from which its preservation, the permanency of its composition, and all its other functions proceed, is an immaterial being, which Stahl calls the *Soul;* because, as he says, when the effects are so similar, he will not multiply powers without necessity. Of this principle, he says, as the Hippocratians said of Nature, that "it does without teaching what it ought to do‡," and does it "without consideration§." These ancient tenets Stahl interprets in such a manner that even the involuntary motions proceed from the soul, though without reflection or clear consciousness. It is indeed evident, that there are many customary motions and sensations which are perfectly rational, yet not the objects of distinct consciousness: and thus instinctive motions, and those of which we are quite unconscious, may still be connected with reason. The questions which in this view offer them-

* Spr., v. 308.

† *Ib.*, v. 314.

‡ Stahl, περὶ φύσεως ἀπαίδευτον.

§ οὐκ ἐκ διανοίης.

selves, as, how the soul passes from the mother to the child, he dismisses as unprofitable*. He considers nutrition and secretion as the work of the soul. The corpuscular theory and the doctrine of animal spirits are, he rightly observes, mere hypotheses, which are arbitrary in their character, and only shift the difficulty. For, if the animal spirits are not matter, how can they explain the action of an immaterial substance on the body; and if they are matter, how are they themselves acted on?

This doctrine of the action of the soul on the body, was accepted by many persons, especially by the iatro-mathematicians, who could not but feel the insufficiency of their system without some such supplement: such were Cheyne and Mead. In Germany, Stahl's disciples in physiology were for the most part inconsiderable persons†. Several Englishmen who speculated concerning the metaphysics as well as the physiology of Sensation and Motion, inclined to this psychical view, as Porterfield and Whytt. Among the French, Boissier de Sauvages was the most zealous defender of the Stahlian system. Actions, he says‡, which belong to the preservation of life are determined by a moral not a mechanical necessity. They proceed from the soul, but cannot be controlled by it, as the starting from fear, or the trembling at danger. Unzer, a physician at Altona§, was also a philosophical Stahlian‖.

We need not dwell on the opposition which was offered to this theory, first by Hoffmann, and afterwards by Haller. The former of these had promulgated, as we have seen, the rival theory of a Nervous Fluid, the latter

* This was of course an obvious problem. Harvey, *On Generation*, Exercise 27, p. 148, teaches, "That the egg is not the production of the womb, but of the soul."

† Spr., v. 339, &c.

‡ *Ib.*, 358.

§ A.D. 1799.

‖ Spr., v. 360.

was the principal assertor of the doctrine of Irritability, an important theory on which we may afterwards have to touch. Haller's animosity against the Stahlian hypothesis is a remarkable feature in one who is in general so tolerant in his judgment of opinions. His arguments are taken from the absence of the control of the will over the vital actions, from the want of consciousness accompanying these actions, from the uniformity of them in different conditions of the mind, and from the small sensibility of the heart which is the source of the vital actions. These objections, and the too decided distinction which Haller made between voluntary and involuntary muscles, were very satisfactorily answered by Whytt and Platner. In particular, it was urged that the instinctive actions of brutes are inexplicable by means of mechanism, and may be compared with the necessary vital actions of the human body. Neither kind are accidental, neither kind are voluntary, both are performed without reflection.

Without tracing further the progress of the Psychical Doctrine, I shall borrow a few reflections upon it from Sprengel*:—

"When the opponents of the Stahlian system repeat incessantly that the assumption of a psychical cause in corporeal effects is a metaphysical speculation which does not belong to medicine, they talk to no purpose. The states of the soul are objects of our internal experience, and interest the physician too nearly to allow him to neglect them. The innumerable unconscious efforts of the soul, the powerful and daily effects of the passions upon the body, too often put to confusion those who would expel into the region of metaphysics the dispositions of the mind. The connexion of our knowledge of the soul, as gathered from experience, with our know-

* Spr., v. 383.

ledge of the human body, is far closer than the mechanical and chemical physiologists suspect.

"The strongest objection against the psychical system, and one which has never been sufficiently answered by any of its advocates, is the universality of organic effects in the vegetable kingdom. The comparison of the physiology of plants with the physiology of animals puts the latter in its true light. Without absolutely trifling with the word *soul*, we cannot possibly derive from a soul the organic operations of vegetables. But just as little can we, as some Stahlians have done, draw a sharp line between plants and animals, and ascribe the processes of the former to mere mechanism, while we derive the operations of the latter from an intellectual principle. Not to mention that such a line is not possible, the rise of the sap and the alteration of the fluids of plants cannot be derived entirely from material causes as their highest origin."

Thus, I may add, this psychical theory, however difficult to defend in its detail, does in its generalities express some important truths respecting the vital powers. It not only, like the last theory, gives unity to the living body, but it marks, more clearly than any other theory, the wide interval which separates mechanical and chemical from vital action, and fixes our attention upon the new powers which the consideration of life compels us to assume. It not only reminds us that these powers are elevated above the known laws of the material world, but also that they are closely connected with the world of thought and feeling, of will and reason; and thus it carries us, in a manner in which none of the preceding theories have done, to a true conception of a living, conscious, sentient, active individual.

At the same time we cannot but allow that the life of plants and of the lower orders of animals shows us very clearly that, in order to arrive at any sound and consistent knowledge respecting life, we must form some conception of it from which all the higher attributes which the term "soul" involves, are utterly and carefully excluded; and therefore we cannot but come to the conclusion that the psychical school are right mainly in this; that in ascribing the functions of life to a *soul*, they mark strongly and justly the impossibility of ascribing them to any known attributes of *body*.

Chapter III.

ATTEMPTS TO ANALYZE THE IDEA OF LIFE.

1. *Definitions of Life.*—We have seen in the preceding chapter that all attempts to obtain a distinct conception of the nature of Life in general have ended in failure, and produced nothing beyond a negative result. And the conjecture may now naturally occur, that the cause of this failure resides in an erroneous mode of propounding to ourselves the problem. Instead of contemplating Life as a single Idea, it may perhaps be proper to separate it into several component notions: instead of seeking for one cause of all vital operations, it may be well to look at the separate vital functions, and to seek their causes. When the view of this possibility opens upon us, how shall we endeavour to verify it, and to take advantage of it?

Let us, as one obvious course, take some of the attempts which have been made to *define* Life, and let us see whether they appear to offer to us any analysis of the idea into component parts. Such definitions, when they proceed from men of philosophical minds are the ultimate result of a long course of thought and observation; and by no means deserve to be slighted as arbitrary selections of conditions, or empty forms of words.

2. Life has been defined by Stahl*, "The condition by which a body resists a natural tendency to chemical changes, such as putrefaction." In like manner, M. von Humboldt† defines living bodies to be "those which, notwithstanding the constant operation of causes tending to change their form, are hindered by a certain inward power from undergoing such change." The first of these definitions amounts only to the assertion, that vital processes are not chemical; a negative result, which we may accept as true, but which is, as we have seen, a barren truth. The second appears to be, in its import, identical with the first. An *inward* principle can only be understood as distinguished from known external powers, such as mechanical and chemical agencies. Or if, by an internal principle, we mean such a principle as that of which we are *conscious* within ourselves, we ascribe a soul to all living things: an hypothesis which we have seen is not more effective than the former in promoting the progress of biological science. Nearly the same criticism applies to such definitions as that of Kant: that "Life is an internal faculty producing change, motion, and action."

Other definitions refer us, not to some property residing in the whole of an organized mass, but to the connexion and relation of its parts. Thus M. von Humboldt‡ has given another definition of a living body: that "it is a whole whose parts, arbitrarily separated, no longer resist chemical changes." But this additional assertion concerning the parts, adds nothing of any

* Treviranus, *Biologie*, p. 19. Stahlii, *Theor. Med.*, p. 254.

† *Aphorismen aus d. Chem. Physiol. der Pflanzen*, s. 1.

‡ *Versuche über die gereitzte Muskel und Nervenfäser*, Book II., p. 433.

value to the definition of the whole. And in some of the lower kinds of plants and animals it is hardly true as a fact.

3. Another definition* places the character of Life in "motions serviceable to the body moved." To this it has been objected†, that, on this definition, the earth and the planets are living bodies. Perhaps it would be more philosophical to object to the introduction of so loose a notion as that of a property being *serviceable* to a body. We might also add, that if we speak of all vital functions as *motions*, we make an assumption quite unauthorized, and probably false.

Other definitions refer the idea of Life to the idea of Organization. "Life is the activity of matter according to laws of organization‡." We are then naturally led to ask what is Organization. In reply to this is given us the Kantian definition of Organization, which I have already quoted elsewhere§, "An organized product of nature is that in which all the parts are mutually ends and means||." That this definition involves exact fundamental ideas, and is capable of being made the basis of sound knowledge, I shall hereafter endeavour to show. But I may observe that such a definition leads us somewhat further. If the parts of organized bodies are known to be means to certain ends, this must be known because they fulfil these ends, and produce certain effects by the operation of a certain cause or causes. The question then recurs, what is *the cause* which produces such effects as take place in organized or living bodies? and this is identical with the problem of which in the last chapter we

* Erhard, Röschlaub's *Magazin der Heilkunde*, B. I., st. 1, p. 69.
† Treviranus, *Biologie*, p. 41.
‡ Schmid, *Physiologie*, B. II., p. 274.
§ *Hist. Ind. Sc.*, B. XVII. c. viii. sect. 2.
|| Kant, *Urtheilskraft*, p. 296.

traced the history, and related the failure of physiologists in all attempts at its solution.

4. But what has been just said suggests to us that it may be an improvement to put our problem in another shape:—not to take for granted that the cause of all vital processes is one, but to suppose that there may be several separate causes at work in a living body. If this be so, life is no longer one kind of activity, but several. We have a number of operations which are somehow bound together, and life is the totality of all these: in short, life is not one Function, but a System of Functions.

5. We are thus brought very near to the celebrated definition of life given by Bichat*: "Life is the sum of the functions by which death is resisted." But upon the definition thus stated, we may venture to observe;—first, that the introduction of the notion of *death* in order to define the notion of *life* appears to be unphilosophical. We may more naturally define death with reference to life, as the cessation of life; or at least we may consider life and death as correlative and interdependent notions. Again, the word "sum," used in the way in which it here occurs, appears to be likely to convey an erroneous conception, as if the functions here spoken of were simply added to each other, and connected by co-existence. It is plain that our idea of life involves more than this: the functions are all clearly connected, and mutually depend on each other; nutrition, circulation, locomotion, reproduction,—each has its influence upon all the others. These functions not merely co-exist, but exist with many mutual relations and connexions; they are continued so as to form, not merely a *sum*, but a *system*. And thus we are led to modify Bichat's definition, and to say that *Life is the system of vital functions.*

* *Physiological Researches on Life and Death.*

6. But it will be objected that by such a definition we explain nothing: the notion of *vital functions*, it may be said, involves the idea of *life*, and thus brings us round again to our starting point. Or if not, at least it is as necessary to define Vital Functions as to define Life itself, so that we have made little progress in our task.

To this we reply, that if any one seeks, upon such subjects, some ultimate and independent definition from which he can, by mere reasoning, deduce a series of conclusions, he seeks that which cannot be found. In the Inductive Sciences, a Definition does not form the basis of reasoning, but *points out the course of investigation*. The definition must include words; and the meaning of these words must be sought in the progress and results of observations, as I have elsewhere said*. "The meaning of words is to be sought in the progress of thought; the history of science is our dictionary; the steps of scientific induction are our definitions." It will appear, I think, that it is more easy for us to form an idea of a separate Function of the animal frame, as Nutrition or Reproduction, than to comprehend Life in general under any single idea. And when we say that Life is a system of Vital Functions, we are of course directed to study these functions separately, and (as in all other subjects of scientific research) to endeavour to form of them such clear and definite ideas as may enable us to discover their laws.

7. The view to which we are thus led, of the most promising mode of conducting the researches of Biology, is one which the greatest and most philosophical physiologists of modern times have adopted. Thus Cuvier considers this as the true office of physiology at present. "It belongs to modern times," he says, "to form a just

* *Hist. Ind. Sci.*, B. XIII. c. IX.

classification of the vital phenomena; the task of the present time is to analyze the forces which belong to each organic element, and upon the zeal and activity which are given to this task, depends, according to my judgment, the fortune of physiology*." This classification of the phenomena of life involves, of course, a distinction and arrangement of the vital functions; and the investigation of the powers by which these functions are carried on, is a natural sequel to such a classification.

8. *Classifications of Functions.*—Attempts to classify the Vital Functions of man were made at an early period, and have been repeated in great number up to modern times. The task of classification is exposed to the same difficulties, and governed by the same conditions, in this as in other subjects. Here, as in the case of other things, there may be many classifications which are moderately good and natural, but there is only one which is the best and the true natural system. Here, as in other cases, one classification brings into view one set of relations; another, another; and each may be valuable for its special purpose. Here, as in other cases, the classes may be well constituted, though the boundary lines which divide them be somewhat indistinct, and the order doubtful. Here, as in other cases, we may have approached to the natural classification without having attained it; and here, as in other cases, to *define* our classes is the last and hardest of our problems.

The most ancient classification of the Functions of living things†, is the division of them into *Vital*, *Natural*, and *Animal*. The *Vital* Functions are those which cannot be interrupted without loss of life, as *Circulation*, *Respiration*, and *Nervous Communication*. The *Natural* Functions are those which without the intervention of

* *Hist. Sc. Nat. dep.* 1789, I. 218.

† *Dict. des Sciences Nat.*, art. *Fonctions.*

the will operate on their proper occasions to preserve the bodies of animals; they are *Digestion*, *Absorption*, *Nutrition;* to which was added *Generation*. The *Animal* Functions are those which involve perception and will, by which the animal is distinguished from the vegetable; they are *Sensibility*, *Locomotion*, and *Voice*.

The two great grounds of this division, the distinction of functions which operate continually, and those which operate occasionally; and again, the distinction of functions which involve sensation and voluntary motion from those which do not, are turly of fundamental importance, and gave a real value to this classification. It was, however, liable to obvious objections: namely, *First*, that the names of the classes were ill chosen; for all the functions are natural, all are vital: *Second*, that the lines of demarcation between the classes are indefinite and ambiguous; Respiration is a *vital* function, as being continually necessary to life; but it is also a *natural* function, since it concurs in the formation of the nutritive fluid, and an *animal* function, since it depends in part on the will. But these objections were not fatal, for a classification may be really sound and philosophical, though its boundary lines are vague, and its nomenclature ill selected. The division of the functions we have mentioned kept its ground long; or was employed with a subdivision of one class, so as to make them four; the *vital*, *natural*, *animal* and *sexual* functions.

10. I pass over many intermediate attempts to classify the functions, and proceed to that of Bichat as that which is, I believe, the one most generally assented to in modern times. The leading principle in the scheme of this celebrated physiologist is the distinction between *organic* and *animal* life. This separation is nearly identical with the one just noticed between the vital and animal functions; but Bichat, by the contrasts which he

pointed out between these classes of functions, gave a decided prominence and permanence to the distinction. The Organic Life, which in animals is analogous to the life of vegetables, and the Animal Life, which implies sensation and voluntary motion, have each its system of organs. The center of the animal life is the brain, of the organic life, the heart. The former is carried on by a symmetrical, the latter, by an unsymmetrical system of organs: the former produces intermitting, the latter continuous actions: and, in addition to these, other differences are pointed out. This distinction of the two *lives*, being thus established, each is subdivided into two orders of Functions. The Animal Functions are passive, as *Sensation:* or active, as *Locomotion* and *Voice;* again, the Organic Functions are those of composition, which are concerned in taking matter into the system; *Digestion, Absorption, Respiration, Circulation, Assimilation;* and those of decomposition, which reject the materials when they have discharged their office in the system; and these are again, *Absorption, Circulation,* and *Secretion.* To these are added *Calorification,* or the production of animal heat. It appears, from what has been said, that *Absorption* and *Circulation,* (and we may add *Assimilation* and *Secretion,* which are difficult to separate,) belong alike to the processes of composition and decomposition; nor in truth, can we, with any rigour, separate the centripetal and centrifugal movements in that vortex which, as we shall see, is an apt image of organic life.

Several objections have been made to this classification; and in particular, to the terms thus employed. It has been asserted to be a perversion of language to ascribe to animals *two lives,* and to call the higher faculties in man, perception and volition, the *animal* functions. But, as we have already said, when a classification is really good, such objections, which bear only upon the mode in which it is presented, are by no means fatal: and it is generally acknowledged, by all the most philosophical cultivators of biology, that this arrangement of the functions is better suited to the purposes of the science than those which preceded it.

11. But according to the principles which we have already laid down, the solidity of such a classification is to be verified by its serving as a useful guide in biological researches. If the arrangement which we have explained be really founded in natural relations, it will be found that in proportion as physiologists have studied the separate functions above enumerated, their ideas of these functions, and of the powers by which they are carried on, have become more and more clear;—have tended more and more to the character of exact and rigorous science.

To examine how far this has been the case with regard to all the separate functions, would be to attempt to estimate the value of all the principal physiological speculations of modern times;—a task far too vast and too arduous for any one to undertake who has not devoted his life to such studies. But it may properly come within the compass of our present plan to shew how, with regard to the broader lines of the above classification, there has been such a progress as we have above described, from more loose and inaccurate notions of some of the vital functions to more definite and precise ideas. This I shall attempt to point out in one or two instances.

Chapter IV.

ATTEMPTS TO FORM IDEAS OF SEPARATE VITAL FORCES, AND FIRST OF ASSIMILATION AND SECRETION.

Sect. I.—*Course of Biological Research.*

1. It is to be observed that at present I do not speak of the progress of our knowledge with regard to the detail of the processes which take place in the human body, but of the approach made to some distinct Idea of the specially vital part of each process. In the History of Physiology, it has been seen* that all the great discoveries made respecting the organs and motions of the animal frame have been followed by speculations and hypotheses connected with such discoveries. The discovery of the circulation of the blood led to theories of animal heat; the discovery of the motion of the chyle led to theories of digestion; the close examination of the process of reproduction in plants and animals led to theories of generation. In all these cases, the discovery brought to light some portion of the process which was mechanical or chemical, but it also, in each instance, served to show that the process was something more than mechanical or chemical. The theory attempted to explain the process by the application of known causes; but there always remained some part of it which must unavoidably be referred to an unknown cause. But though unknown, such a cause was not a hopeless object of study. As the vital functions became better and better understood, it was seen more and more clearly at what precise points of the process it was necessary to assume a peculiar vital energy, and what sort of pro-

* *Hist. Ind. Sci.*, B. xvii.

perties this energy must be conceived to possess. It was perceived where, in what manner, in what degree, mechanical and chemical agencies were modified, overruled, or counteracted, by agencies which must be hyper-mechanical and hyper-chemical. And thus the discoveries made in anatomy by a laborious examination of facts, pointed out the necessity of introducing new ideas, in order that the facts might be intelligible. Observation taught much; and among other things, she taught that there was something which could not be observed, but which must, if possible, be conceived. I shall notice a few instances of this.

SECT. II.—*Attempts to form a distinct Conception of Assimilation and Secretion.*

2. *The Ancients.*—That plants and animals grow by taking into their substance matter previously extraneous, is obvious to all; but as soon as we attempt to conceive this process distinctly in detail, we find that it involves no inconsiderable mystery. How does the same food become blood and flesh, bone and hair? Perhaps the earliest attempt to explain this mystery, is that recorded by Lucretius* as the opinion of Anaxagoras, that food contains some bony, some fleshy particles, some of blood, and so on. We might, on this supposition, conceive that the mechanism of the body appropriates each kind of particle to its suitable place.

But it is easy to refute this essay at philosophizing (as Lucretius refutes it) by remarking that we do not find milk in grass, or blood in fruit, though such food gives such products in cattle and in men. In opposition to this "Homoiomereia," the opinion that is forced upon us by the facts is, that the process of nutrition is not a selection merely, but an *assimilation;* the organized

* Lucr., I. 855. Nunc et Anaxagoræ scrutemur ὁμοιομέρειαν.

system does not *find*, but *make*, the additions to its structure.

3. *Buffon*.—This notion of *assimilation* may be variously expressed and illustrated; and all that we can do here, in order to show the progress of thought, is to adduce the speculations of those writers who have been most successful in seizing and marking its peculiar character. Buffon may be taken as an example of the philosophy of his time on this subject. "The body of the animal," says he*, "is a kind of *interior mould*, in which the matter subservient to its increase is modelled and assimilated to the whole, in such a way that, without occasioning any change in the order and proportion of the parts, there results an augmentation in each part taken separately. This increase, this developement, if we would have a *clear idea* of it, how can we obtain it, except by considering the body of the animal, and each of the parts which is to be developed, as so many interior moulds which only receive the accessory matter in the order which results from the position of all their parts? This developement cannot take place, as persons sometimes persuade themselves, by an addition to the outside; on the contrary, it goes on by an intimate susception which penetrates the mass; for, in the part thus developed, the size increases in all parts proportionally, so that the new matter must penetrate it in all its dimensions: and it is quite necessary that this penetration of substance must take place in a certain order, and according to a certain measure; for if this were not so, some parts would develope themselves more than others. Now what can there be which shall prescribe such a rule to the accessory matter except the *interior mould*."

To speak of a *mould* simply, would convey a coarse mechanical notion, which could not be received as any

* *Hist. Nat.*, B. I. c. iii.

useful contribution to physiological speculation. But this *interior* mould is, of course, to be understood figuratively, not as an assemblage of cavities, but as a collection of laws, shaping, directing, and modifying the new matter; giving it not only form, but motion and activity, such as belong to the parts of an organic being.

4. It must be allowed, however, that even with this explanation, the comparison is very loose and insufficient. A *mould* may be permitted to mean a collection of laws, but still it can convey no conception except that of laws regulated by relations of space; and such a conception is very plainly quite inadequate to the purpose. What can we conceive of the interior mould by which chyle is separated from the aliments at the pores of the lacteals, or tears secreted in the lacrymatory gland?

An additional objection to this mode of expression of Buffon is, that it suggests to us only a single marked change in the assimilated matter, not a continuous series of changes. Yet the animal fluids and other substances are, in fact, undergoing a constant series of changes. Food becomes chyme, and chyme becomes chyle; chyle is poured into the blood; from the blood secretions take place, as the bile; the bile is poured into the digestive canal, and a portion of the matter previously introduced is rejected out of the system. Here we must have a series of "interior moulds;" and these must impress matter at its ejection from the organic system as well as at its reception. But, moreover, it is probable that none of the above transformations are quite abrupt. Change is going on between the beginning and the end of each stage of the nutritive circulation. To express the laws of this continuous change, the image of an interior mould is quite unsuited. We must seek a better mode of conception.

5. Vegetable and animal nutrition is, as we have said, a constant circulation. The matter so assumed is not all retained: a perpetual subtraction accompanies a perpetual addition. There is an excretion as well as an intussusception. The matter which is assumed by the living creature is retained only for a while, and is then parted with. The individual is the same, but its parts are in a perpetual flux: they come and go. For a time the matter which belongs to the organic body is bound to it by certain laws: but before it is thus bound, and after it is loose, this matter may circulate about the universe in any other form. Life consists in a permanent influence over a perpetually changing set of particles.

Cuvier.—This condition also has been happily expressed, by means of a comparison, by another great naturalist. "If," says Cuvier *, "if, in order to obtain a just idea of the essence of life, we consider it in the beings where its effects are most simple, we shall soon perceive that it consists in the faculty which belongs to certain bodily combinations to continue during a determinate time under a determinate form; constantly attracting into their composition a part of the surrounding substances, and giving up in return some part of their own substance.

"Life is thus a *vortex*, more or less rapid, more or less complex, which has a constant direction, and which always carries along its stream particles of the same kinds; but in which the individual particles are constantly entering in and departing out; so that the *form* of the living body is more essential to it than its matter.

"So long as this motion subsists, the body in which it takes place is *alive;* it *lives*. When the motion stops finally, the body *dies*. After death, the elements which compose the body, given up to the ordinary chemical

* *Règne Animal*, I. 11.

affinities, soon separate, and the body which was alive is dissolved."

This notion of a vortex* which is permanent while the matter which composes it constantly changes,—of peculiar forces which act in this vortex so long as it exists, and which give place to chemical forces when the circulatory motion ceases,—appears to express some of the leading conditions of the assimilative power of living things in a simple and general manner, and thus tends to give distinctness to the notion of this vital function.

6. But we may observe that this notion of a vortex is still insufficient. Particles are not only taken into the system and circulated through it for a time, but, as we have seen, they are altered in character in a manner to us unintelligible, both at their first admission into the system and at every period of their progress through it. In the vortex each particle is constantly transformed while it whirls.

It may be said, perhaps, that this transformation of the kinds of matter may be conceived to be merely a new arrangement of their particles, and that thus all the changes which take place in the circulating substances are merely so many additional windings in the course of the whirling current. But to say this, is to take for granted the atomic hypothesis in its rudest form. What right have we to assume that blood and tears, bile and milk, consist of like particles of matter differently arranged? What can arrangement, a mere relation of

* The definition of life given by M. de Blainville appears to me not to differ essentially from that of Cuvier. "Un corps vivant est une sorte de foyer chimique où il-y-a à tous momens apport de nouvelles molecules et depart de molecules anciennes; où la composition n'est jamais fixe (si ce n'est d'un certain nombre de parties veritablement mortes ou en depôt), mais toujours pour ainsi dire *in nisu*, d'où mouvement plus ou moins lent et quelquefois chaleur."—*Principes d'Anat. Comp.*, 1822, t. I. p. 16.

space, do towards explaining such differences? Is not the insufficiency, the absurdity of such an assumption proved by the whole course of science? Are not even chemical changes, according to the best views hitherto obtained, something more than a mere new arrangement of particles? And are not vital as much beyond chemical, as chemical are beyond geometrical modifications? It is not enough, then, to conceive life as a vortex. The particles which are taken into the organic frame do more than circulate there. They are, at every point of their circulation, acted upon by laws of an unknown kind, changing the nature of the substance which they compose. Life is a vortex in which vital forces act at every point of the stream: it is not only a current of whirling matter, but a cycle of recurring powers.

7. *Matter and Form.*—This image of a vortex is closely connected with the representation of life offered us by writers of a very different school. In Schelling's *Lectures on Academic Study*, he takes a survey of the various branches of human knowledge, determining according to his own principles the shape which each science must necessarily assume. The peculiar character of organization, according to him *, is that the matter is only an accident of the thing itself, and the organization consists in Form alone. But this Form, by its very opposition to Matter, ceases to be independent of it, and is only ideally separable. In organization, therefore, substance and accident, matter and form, are completely identical †. This notion, that in organization the form is essential and the matter accidental, or, in other words, that the form is permanent and the matter fluctuating and transitory, agrees, if taken in the grossest sense of

* Lect. XIII. p. 288.

† I have not translated Schelling's words, but given their import as far as I could.

matter and form, with Cuvier's image of a vortex. In a whirlpool, or in a waterfall, the form remains, the matter constantly passes away and is renewed. But we have already seen* that in metaphysical speculations in which *matter* and *form* are opposed, the word *form* is used in a far more extensive sense than that which denotes a relation of space. It may indeed designate any change which matter can undergo; and we may very allowably say that food and blood are the same matter under different *forms*. Hence if we assert that *Life is a constant Form of a circulating Matter*, we express Cuvier's notion in a mode free from the false suggestion which "vortex" conveys.

8. We may, however, still add something to this account of life. The circulating parts of the system not only circulate, but they form the non-circulating parts. Or rather, there are no non-circulating parts: all portions of the frame circulate more or less rapidly. The food which we take circulates rapidly in the fluids, more slowly in the flesh, still more slowly in the bones; but in all these parts it is taken into the system, retained there for some time, and finally replaced by other matter. But while it remains in the body, it exercises upon the other circulating parts the powers by which their motion is produced. Nutriment forms and supports the organs, and the organs carry fresh nutriment to its destination. The peculiar forces of the living body, and its peculiar structure, are thus connected in an indescribable manner. The forces produce the structure; the structure, again, is requisite for the exertion of the forces. The Idea of an Organic or Living Being includes this peculiar condition—that its construction and powers are such, that it constantly appropriates to itself new portions of substance which, so appropriated, become

* Book I.

indistinguishable parts of the whole, and serve to carry on subsequently the same functions by which they were assimilated. And thus *Organic Life is a constant Form of a circulating Matter, in which the Matter and the Form determine each other by peculiar laws (that is, by Vital Forces).*

SECT. III.—*Attempts to conceive the forces of Assimilation and Secretion.*

9. I have already stated that in our attempts to obtain clear and scientific Ideas of Vital Forces, we have, in the first place, to seek to understand the course of change and motion in each function, so as to see at what points of the process peculiar causes come into play; and next, to endeavour to obtain some insight into the peculiar character and attributes of these causes. Having spoken of the first part of this mode of investigation in regard to the general nutrition of organic bodies, I must now say a few words on the second part.

The Forces here spoken of are *Vital* Forces. From what has been said, we may see in some measure the distinction between forces of this kind and mechanical or chemical forces; the latter tend constantly to produce a final condition, after which there is no further cause of change: mechanical forces tend to produce equilibrium; chemical forces tend to produce composition or decomposition; and this point once reached, the matter in which these forces reside is altogether inert. But an organic body tends to a constant motion, and the highest activity of organic forces shows itself in continuous change. Again, in mechanical and chemical forces, the force of any aggregate is the sum of the forces of all the parts: the sum of the forces corresponds to the sum of the matter. But in organic bodies, the amount of effect does not depend on the matter, but on the form: the particles lose their separate energy, in order to share in that of the system; they are not added, they are *assimilated.*

10. It is difficult to say whether anything has been gained to science by the various attempts to assign a fixed *name* to the vital force which is thus the immediate cause of Assimilation. It has been called *Organic Attraction* or *Vital Attraction*, *Organic Affinity* or *Vital Affinity*, being thus compared with mechanical Attraction or chemical Affinity. But, perhaps, as the process is certainly neither mechanical nor chemical, it is desirable to appropriate to it a peculiar name; and the name *Assimilation*, or *Organic Assimilation*, by the usage of good biological writers, is generally employed for this purpose, and may be taken as the standard name of this Vital Force. To illustrate this, I will quote a passage from the excellent *Elements of Physiology* of Professor Müller. "In the process of nutrition is exemplified the fundamental principle of *organic assimilation.* Each elementary particle of an organ attracts similar particles from the blood, and by the changes it produces in them, causes them to participate in the vital principle of the organ itself. Nerves take up nervous substance, muscles, muscular substance: even morbid structures have the assimilating power; warts in the skin grow with their own peculiar structure; in an ulcer, the base and border are nourished in a way conformable to the mode of action and secretion determined by the disease."

11. The Force of Organic Assimilation spoken of in the last paragraph denotes peculiarly the force by which each organ appropriates to itself a part of the nutriment received into the system, and thus is maintained and augmented with the growth of the whole. But the growth of the solid parts is only one portion of the function of nutrition; besides this, we must consider the motion and changes of the fluids, and must ask what kind of forces

may be conceived to produce these. What are the powers by which chyle is *absorbed* from the food, by which bile is *secreted* from the blood, by which the circulating *motion* of these and all other fluids of the body are constantly maintained? To the questions,—What are the forces by which *absorption*, *secretion*, and the *vital motions*, of fluids are produced?—no satisfactory answer has been returned. Yet still some steps have been made, which it may be instructive to point out.

12. In *Absorption* it would appear that a part of the agency is inorganic; for not only dead membranes, but inorganic substances, absorb fluids, and even absorb them with elective forces, according to the ingredients of the fluid. A force which is of this kind, and which has been termed *Endosmose*, has been found to produce very curious effects. When a membrane separates two fluids, holding in solution different ingredients, the fluids pass through the membrane in an imperceptible manner, and mix or exchange their elements. The force which produces these effects is capable of balancing a very considerable pressure. It appears, moreover, to depend, at least among other causes, upon attractions operating between the elements of the solids and the fluids, as well as between the different fluids; and this force, though thus apparently of a mechanical and chemical nature, probably has considerable influence in vital phenomena.

13. But still, though Endosmose may account in part for absorption in some cases, it is certain that there is some other vital force at work in this process. There must be, as Müller says*, "an organic attraction of a kind hitherto unknown." "If absorption," he adds†, "is to be explained in a manner analogous to the laws of endosmose, it must be supposed that a chemical affinity.

* *Physiology*, p. 299. † *Ib.*, p. 301.

resulting from the vital process itself, is exerted between the chyme in the intestines and the chyle in the lacteals, by which the chyle is enabled to attract the chyme without being itself attracted by it. But such affinity or attraction would be of a *vital* nature, since it does not exist after death."

14. If the force of absorption be thus mysterious in its nature, the force of *Secretion* is still more so. In this case we have an organ filled with a fine net-work of blood-vessels, and in the cavities of some *gland*, or open part, we have a new fluid formed, of a kind altogether different from the blood itself. It is easily shown that this cannot be explained by any action of pores or capillary tubes. But what conception can we form of the forces by which such a change is produced? Here, again, I shall borrow the expressions of Müller, as presenting the last result of modern physiology. He says*, "The more probable supposition is, that by virtue of imbibition, or the general organic porosity, the fluid portion of the blood becomes diffused through the tissue of the secreting organ; that the external surface of the glandular canals exerts a chemical attraction on the elements of the fluid, infusing into them at the same time a tendency to unite in new combinations; and then repels them in a manner which is certainly quite inexplicable, towards the inner surface of the secreting membrane, or glandular canals." "Although quite unsupported by facts," he adds, "this theory of attraction and repulsion is not without its analogy in physical phenomena; and it would appear that very similar powers effect the elimination of the fluid in secretion, and cause it to be taken up by the lymphatics in absorption." He elsewhere says†, "Absorption seems to depend on an attraction the nature of which is unknown, but of which the very counterpart, as it were,

* *Physiology*, p. 464. † *Ib.*, p. 301.

takes place in secretion; the fluids altered by the secreting action being repelled towards the free side or open surface only of the secreting membranes, and then pressed forwards by the successive portions of the fluids secreted."

15. With regard to the forces which produce the *Motion* of absorbed or secreted fluids along their destined course, it may be seen, from the last quoted sentence, that the same vital force which changes the nature, also produces the movement of the substance. The fluids are pressed forwards by the successive portions absorbed or secreted. That this is the sole cause, or at least a very powerful cause, of the motion of the nutritive fluids in organic bodies, is easily shown by experience. It is found* that the organs which effect the ascent of the sap in trees during the spring are the terminal parts of the roots; that the whole force by which the sap is impelled upwards is the *vis a tergo*, as it has been called, the force pushing from behind, exerted in the roots. And thus the force which produces this motion is exerted exactly at those points where the organic body selects from the contiguous mass those particles which it absorbs and appropriates. And the same may most probably be taken for the cause of the motion of the lymph and chyle; at least, Müller says† that no other motive power has been detected which impels those fluids in their course.

Thus, though we must confess the Vital Force concerned in Assimilation and Secretion to be unknown in its nature, we still obtain a view of some of the attributes which it involves. It has mechanical efficacy, producing motions, often such as would require great mechanical force. But it exerts at the same point both an attraction and a repulsion, attracting matter on one side, and repelling it on the other; and in this circumstance it differs

* Müller, p. 300. † *Ib.*, p. 254.

entirely from mechanical forces. Again, it is not only mechanical but chemical, producing a complete change in the nature of the substance on which it acts; to which we must add that the changes produced by the vital forces are such as, for the most part, our artificial chemistry cannot imitate. But, again, by the action of the vital force at any point of an organ, not only are fluids made to pass, and changed as they pass, but the organ itself is maintained and strengthened, so as to continue or to increase its operation: and thus the vital energy supports its activity by its action, and is augmented by being exerted.

We have thus endeavoured to obtain a view of some of the peculiar characters which belong to the Force of Organic Assimilation;—the Force by which life is kept up, conceived in the most elementary form to which we can reduce it by observation and contemplation. It appears that it is a force which not only produces motion and chemical change, but also *vitalizes* the matter on which it acts, giving to it the power of producing like changes on other matter, and so on indefinitely. It not only circulates the particles of matter, but puts them in a stream of which the flow is developement as well as movement.

The force of Organic Assimilation being thus conceived, it becomes instructive to compare it with the force concerned in Generation, which we shall therefore endeavour to do.

SECT. IV.—*Attempts to conceive the Process of Generation.*

16. At first sight the function of Nutrition appears very different from the function of Generation. In the former case we have merely the existing organs maintained or enlarged, and their action continued; in the

latter, we have a new individual produced and extricated from the parent. The term *Reproduction* has, no doubt, been applied, by different writers, to both these functions;—to the processes by which an organ when mutilated, is restored by the forces of the living body, and to the process by which a new generation of individuals is produced which may be considered as taking the place of the old generation, as these are gradually removed by death. But these are obviously different senses of the word. In the latter case, the term *Reproduction* is figuratively used; for the *same* individuals are not reproduced; but the species is kept up by the propagation of new individuals, as in nutrition the organ is kept up by the assimilation of new matter. To escape ambiguity, I shall avoid using the term *Reproduction* in the sense of *Propagation*.

17. In Nutrition, as we have seen, the matter, which from being at first extraneous, is appropriated by the living system, and directed to the sustentation of the organs, undergoes a series of changes of which the detail eludes our observation and apprehension. The nutriment which we receive contributes to the growth of flesh and bone, viscera and organs of sense. But we cannot trace in its gradual changes a visible preparation for its final office. The portion of matter which is destined to repair the waste of the eye or the skin, is not found assuming a likeness to the parts of the eye or the structure of the skin, as it comes near the place where it is moulded into its ultimate form. The new parts are insinuated among the old ones, in an obscure and imperceptible matter. We can trace their progress only by their effects. The organs *are* nourished, and that is almost all we can learn: we cannot discover *how* this is done. We cannot follow nature through a series of manifest preparations and processes to this result.

18. In Generation the case is quite different. The young being is formed gradually and by a series of distinguishable processes. It is included within the parent before it is extruded, and approaches more or less to the likeness of the parent before it is detached. While it is still an embryo, it shares in the nutriment which circulates through the system of the mother; but its destination is already clear. While the new and the old parts, in every other portion of the mother, are undistinguishably mixed together, this new part, the fœtus, is clearly distinct from the rest of the system, and becomes rapidly more and more so, as the time goes on. And thus there is formed, not a new part, but a new whole; it is not an organ which is kept up, but an offspring which is prepared. The progeny is included in the parent, and is gradually fitted to be separated from it. The young is at first only the developement of a part of the organization of the mother;—of a germ, an ovule. But it is not developed like other organs, retaining its general form. It does not become merely a larger bud, a larger ovule; it is entirely changed; it becomes—from a bud—a blossom, a flower, a fruit, a seed; from an ovule it becomes an egg, a chick, a bird; or it may be, a fœtus, a child. The original rudiment is not merely nourished, but unfolded and transformed through the most marked and remote changes, gradually tending to the form of the new individual.

19. But this is not all. The fœtus is, as we have said, a developement of a portion of the mother's organization. But the fœtus (supposing it female) is a likeness of the mother. The mother, even before conception, contains within herself the germs of her progeny; the female fœtus, therefore, at a certain stage of developement, will contain also the germs of possible progeny; and thus we may have the germs of future generations,

pre-existing and included successively within one another. And this state of things, which thus suggests itself to us as possible, is found to be the case in facts which observation supplies. Anatomists have traced ovules in the unborn fœtus, and thus we have three generations included one within another.

20. Supposing we were to stop here, the process of propagation might appear to be altogether different from that of nutrition. The latter, as we have seen, may be in some measure illustrated by the image of a *vortex*; the former has been represented by the image of a series of germs, *sheathed* one within another successively, and this without any limit. This view of the subject has been termed the doctrine of the *Pre-existence of germs*; and has been designated by German writers by a term "Einschachtelungs-theorie" descriptive of the successive sheathing of which I have spoken. Imitating this term, we may call it *the Theory of successive inclusion*. It has always had many adherents; and has been, perhaps, up to the present time, the most current opinion on the subject of generation. Cuvier inclines to this opinion*. "Fixed forms perpetuating themselves by generation distinguish the species of living things. These forms do not produce themselves, do not change themselves. Life supposes them to exist already; its flame can be lighted only in organization previously prepared; and the most profound meditations and the most delicate researches terminate alike in the mystery of the pre-existence of germs."

21. Yet this doctrine is full of difficulty. It is, as Cuvier says, a mysterious view of the subject;—so mysterious, that it can hardly be accepted by us, who seek distinct conceptions as the basis of our philosophy. Can it be true, not only that the germ of the offspring is

* *Règne Animal*, p. 20.

originally included in the parent, but also the germs of *its* progeny, and so on without limit:—So that each fruitful individual contains in itself an infinite collection of future possible individuals;—a reserve of infinite succeeding generations? This is hard to admit. Have we no alternative? What is the opposite doctrine?

22. The opposite doctrine deserves at least some notice. It extends, to the production of a new individual, the conception of growth by nutrition. According to this view, we suppose propagation to take place, not as in the view just spoken of, by inclusion and extrusion, but by assimilation and developement;—not by the material pre-existence of germs, but by the communication of vital forces to new matter. This opinion appears to be entertained by some of the most eminent physiologists of the present time. Thus, Müller says, "The organic force is also creative. The organic force which resides in the whole, and on which the existence of each part depends, has also the property of generating, from organic matter, the parts necessary to the whole." Life, he adds, is not merely a harmony of the parts. On the contrary, the harmonious action of the parts subsists only by the influence of a force pervading all parts of the body. "This force exists before the harmonizing parts, which are in fact formed by it during the developement of the embryo." And again; "The creative force exists in the germ, and creates in it the essential force of the future animal. The germ is *potentially* the whole animal: during the developement of the germ the parts which constitute the actual whole are produced."

23. In this view, we extend to the reproduction of an individual the same conception of organic assimilation which we have already arrived at, as the best notion we can form of the force by which the reproduction and sustentation of parts takes place. And is not such an

extension really very consistent? If a living thing can appropriate to itself extraneous matter, invest it with its own functions, and thus put it in the stream of constant developement, may we not conceive the developement of a new *whole* to take place in this way as well as of a *part?* If the organized being can infuse into new matter its vital forces, is there any contradiction in supposing this infusion to take place in the full measure which is requisite for the production of a new individual? The force of organic assimilation is transferred to the very matter on which it acts; it may be transferred so that the operation of the forces produces not only an organ, but a system of organs.

24. This identification of the forces which operate in Nutrition and Generation may at first seem forced and obscure, in consequence of the very strong apparent differences of the two processes which we have already noticed. But this defect in the doctrine is remedied by the consideration of what may be considered as intermediate cases. It is not true that, in the nutrition of special organs, the matter is always conveyed to its ultimate destination without being on its way moulded into the form which it is finally to bear, as the embryo is moulded into the form of the future individual. On the contrary, there are cases in which the waste of the organs is supplied by the growth of new ones, which are prepared and formed before they are used, just as the offspring is prepared and formed before it is separated from the parent. This is the case with the teeth of many animals, and especially with the teeth of animals of the crocodile kind. Young teeth grow near the root of the old ones, like buds on the stem of a plant; and as these become fully developed, they take the place of the parent tooth when that dies and is cast away. And these new teeth in their turn are succeeded by others

which germinate from them. Several generations of such teeth, it is said as many as four, have been detected by anatomists, visibly existing at the same time; just as several generations of germs of individuals have been, as we already stated, observed included in one another. But this case of the teeth appears to show very strikingly how insufficient such observations are to establish the doctrine of successive inclusion, or of the pre-existence of germs. Are we to suppose that every crocodile's tooth includes in itself the germs of an infinite number of possible teeth, as in the theory of pre-existing germs, every individual includes an infinite number of individuals? If this be true of teeth, we must suppose that organ to follow laws entirely different from almost every other organ; for no one would apply to the other organs in general such a theory of reproduction. But if such a theory be not maintained respecting the teeth, how can we maintain the theory of the pre-existing germs of individuals, which has no recommendation except that of accounting for exactly the same phenomena?

It would seem, then, that we are, by the closest consideration of the subject, led to conceive the forces by which generation is produced as forces which vitalize certain portions of matter, and thus prepare them for developement according to organic forms; and thus the conception of this Generative Force is identified with the conception of the Force of Organic Assimilation, to which we were led by the consideration of the process of nutrition.

I shall not attempt to give further distinctness and fixity to this conception of one of the vital forces; but I shall proceed to exemplify the same analysis of life by some remarks upon another Vital Process, and the Forces of which it exhibits the operation.

CHAPTER V.

ATTEMPTS TO FORM IDEAS OF SEPARATE VITAL FORCES, *Continued.*— VOLUNTARY MOTION.

1. WE formerly noticed the distinctions of *organic* and *animal* functions, organic and animal forces, as one of the most marked distinctions to which physiologists have been led in their analysis of the vital powers. I have now taken one of the former, the organic class of functions, namely, nutrition; and have endeavoured to point out in some measure the peculiar nature of the vital forces by which this function is carried on. It may serve to show the extent and the difficulty of this subject, if, before quitting it, I offer a few remarks suggested by a function belonging to the other class, the animal functions. This I shall briefly do with respect to *Voluntary Motion.*

2. In the History of Physiology, I have already related the progress of the researches by which the organs employed in voluntary motion became known to anatomists. It was ascertained to the satisfaction of all physiologists, that the immediate agents in such motion are the muscles; that the muscles are in some way contracted, when the nerves convey to them the agency of the will; and that thus the limbs are moved. It was ascertained, also, that the nerves convey sensations from the organs of sense inwards, so as to make these sensations the object of the animal's consciousness. In man and the higher animals, these impressions upon the nerves are all conveyed to one internal organ, the brain; and from this organ all impressions of the will appear to proceed; and thus the brain is the center of animal life,

towards which sensations converge, and from which volitions diverge.

But this being the process, we are led to inquire how far we can obtain any knowledge, or form any conception of the vital forces by means of which the process is carried on. And here I have further stated in the History*, that the transfer of sensations and volitions along the nerves was often represented as consisting in the motion of a Nervous Fluid. I have related that the hypothesis of such a fluid, conveying its impressions either by motions of translation or of vibration, was countenanced by many great names, as Newton, Haller, and even Cuvier. But I have ventured to express my doubt whether this hypothesis can have much value: "for," I have said, "this principle cannot be mechanical, chemical, or physical, and therefore cannot be better understood by embodying it in a fluid. The difficulty we have in conceiving what the force *is*, is not got rid of by explaining the machinery by which it is *transferred*."

3. I may add, that no succeeding biological researches appear to have diminished the force of these considerations. In modern times, attempts have repeatedly been made to identify the nervous fluid with electricity or galvanism. But these attempts have not been satisfactory or conclusive of the truth of such an identity: and Professor Müller probably speaks the judgment of the most judicious physiologists, when he states it as his opinion, after examining the evidence†, "That the vital actions of the nerves are not attended with the developement of any galvanic currents which our instruments can detect; and that the laws of action of the nervous principle are totally different from those of electricity."

That the powers by which the nerves are the instru-

* *Hist. Ind. Sci.* B. XVII. c. V. sect. 2. † *Elem. Phys.*, p. 640.

ments of sensation, and the muscles of motion, are vital endowments, incapable of being expressed or explained by any comparison with mechanical, chemical, and electrical forces, is the result which we should expect to find, judging from the whole analogy of science; and which thus is confirmed by the history of physiology up to the present time. We naturally, then, turn to inquire whether such peculiar vital powers have been brought into view with any distinctness and clearness.

4. The property by which muscles, under proper stimulation, contract and produce motion, has been termed *Irritability* or *Contractility;* the property by which nerves are susceptible of their appropriate impressions has been termed *Sensibility.* A very few words on each of these subjects must suffice.

Irritability.—I have, in the History of Physiology*, noticed that Glisson, a Cambridge professor, distinguished the Irritation of muscles as a peculiar property, different from any merely mechanical or physical action. I have mentioned, also, that he divides Irritation into *natural, vital,* and *animal;* and points out, though briefly, the graduated differences of Irritability in different organs. Although these opinions did not at first attract much notice, about seventy years afterwards attention was powerfully called to this vital force, *Irritability,* by Haller. I shall borrow Sprengel's reflections on this subject.

"Hitherto men had been led to see more and more clearly that the cause of the bodily functions, the fundamental power of the animal frame, is not to be sought in the mechanism, and still less in the mixture of the parts. In this conviction, they had had recourse partly to the quite supersensuous principle of the Soul, partly to the half-material principle of the Animal Spirits, in

* *Hist. Ind. Sci.*, B. XVII. c. V.

order to explain the bodily motions. Glisson alone saw the necessity of assuming an Original Power in the fibres, which, independent of the influence of the animal spirits, should produce contraction in them. And Gorter first held that this Original Power was not to be confined to the muscles, but to be extended to all parts of the living body.

"But as yet the laws of this Power were not known, nor had men come to an understanding whether it were fully distinct from the elasticity of the parts, or by what causes it was put in action. They had neither instituted observations nor experiments which established its relation to other assumed forces of the body. There was still wanting a determination of the peculiar seat of this power, and experiments to trace its gradual differences in different parts of the body. In addition to other causes, the necessity of the assumption of such a power was felt the more, in consequence of the prevalence of Leibnitz's doctrine of the activity of matter; but it was an occult quality, and remained so till Haller, by numerous experiments and solid observations, placed in a clear light the peculiarities of the powers of the animal body."

5. Perhaps, however, Haller did more in the way of determining experimentally the limits and details of the application of this idea of Irritability as a peculiar attribute, than in developing the Idea itself. In this way his merits were great. As early as the year 1739, he published his opinion upon Irritability as the cause of muscular motion, which he promulgated again in 1743. But from the year 1747 he was more attentive to the peculiarities of Irritability, and its difference from the effect of the nerves. In the first edition of his *Physiology*, which appeared in 1747, he distinguished three kinds of Force in muscles,—the Dead Force, the Innate

Force, and the Nervous Power. The first is identical with the elastic force of dead matter, and remains even after death. The *innate force* continues only a short time after death, and discloses itself especially by alternate oscillations; the motions which arise from this are much more lively than those which arise from mere elasticity: they are not excited by tension, nor by pressure, nor by any mechanical alteration, but only by *irritation*. The *nervous force* of the muscle is imparted to it from without by the nerves; it preserves the *irritability*, which cannot long subsist without the influence of the nervous force, but is not identical with it.

In the year 1752, Haller laid before the Society of Göttingen the result of one hundred and ninety experiments; from which it appears to what parts of the animal system Irritability and Nervous Power belong. These I need not enumerate. He also investigated with care its gradations in those parts which do possess it. Thus the heart possesses it in the highest degree, and other organs follow in their order.

6. Haller's doctrine was, that there resides in the muscles a peculiar vital power by which they contract, and that this power is distinct from the attributes of the nerves. And this doctrine has been accepted by the best physiologists of modern times. But this distinction of the *irritability* of the muscles from the *sensibility* of the nerves became somewhat clearer by giving to the former attribute the name of *Contractility*. This accordingly was done; it is, for example, the phraseology used by Bichat. By speaking of *animal sensibility* and *animal contractility*, the passive and the active element of the processes of animal life are clearly separated and opposed to each other. The sensations which we feel, and the muscular action which we exert, may be closely and inseparably connected, yet still they are

clearly distinguishable. We can easily in our apprehension separate the titillation felt in the nose on taking snuff, from the action of the muscles in sneezing; or the perception of an object falling towards the eye, from the exertion which shuts the eye-lid; although in these cases the passive and active part of the process are almost or quite inseparable in fact. And this clear separation of the active from the passive power is something, it would seem, peculiar to the Animal Vital Powers; it is a character by which they differ, not only from mechanical, chemical, and all other merely physical forces, but even from Organic Vital Powers.

7. But this difference between the Animal and the Organic Vital Powers requires to be further insisted upon, for it appears to have been overlooked or denied by very eminent physiologists. For instance, Bichat classifies the Vital Powers as Animal Sensibility, Animal Contractility, Organic Sensibility, Organic Contractility.

Now the view which suggests itself to us, in agreement with what has been said, is this:—that though Animal Sensibility and Animal Contractility are clearly and certainly distinct, Organic Sensibility and Organic Contractility are neither separable in fact nor in our conception, but together make up a single Vital Power. That they are not separable in fact is, indeed, acknowledged by Bichat himself. "The organic contractility," he says*, "can never be separated from the sensibility of the same kind; the reaction of the excreting tubes is immediately connected with the action which the secreted fluids exercise upon them: the contraction of the heart must necessarily succeed the influx of the blood into it." It is not wonderful, therefore, that it should have happened, as he complains, that "authors have by no means

* *Life and Death*, p. 94.

separated these two things, either in their consideration or in language." We cannot avoid asking, Are Organic Sensibility and Organic Contractility really anything more than two different aspects of the same thing, like action and reaction in mechanics, which are only two ways of considering the action which takes place at a point; or like the positive and negative electricities, which, as we have seen, always co-exist and correspond to each other?

8. But we may observe, moreover, that Bichat, by his use of the term Contractility, includes in it powers to which it cannot with any propriety be applied. Why should we suppose that the vital powers of absorption, secretion, assimilation, are of such a nature that the name *contractility* may be employed to describe them? We have seen, in the last chapter, that the most careful study of these powers leads us to conceive them in a manner altogether removed from any notion of contraction. Is it not then an abuse of language which cannot possibly lead to anything but confusion, to write thus*: "The insensible organic contractility is that, by virtue of which the excreting tubes react upon their respective fluids, the secreting organs upon the blood which flows into them, the parts where nutrition is performed upon the nutritive juices, and the lymphatics upon the substances which excite their open extremities." In the same manner he ascribes† to the peculiar sensibility of each organ the peculiarity of its products and operations. An increased absorption is produced by an increased susceptibility of the "absorbent orifices." And thus, in this view, each organic power may be contemplated either as sensibility or as contractility, and may be supposed to be rendered more intense by magnifying either of these its aspects; although, in fact, neither can be

* *Life and Death*, p. 95. † *Ib.*, p. 90.

conceived to be increased without an exactly commensurate increase of the other.

9. This opinion, unfounded as it thus appears to be, that all the different organic vital powers are merely different kinds of Contractility or Excitability, was connected with the doctrines of Brown and his followers, which were so celebrated in the last century, that all diseases arise from increase or from diminution of the Vital Force. The considerations which have already offered themselves would lead us to assent to the judgment which Cuvier has pronounced upon this system. "The theory of excitation," he says, "so celebrated in these later times by its influence upon pathology and therapeutick, is at bottom only a modification of that, in which, including under a common name Sensibility and Irritability," and we may add, applying this name to all the Vital Powers, "the speculator takes refuge in an abstraction so wide, that if, by it, he simplifies medicine, he annihilates all positive physiology*."

10. The separation of the nervous influence and the muscular irritability, although it has led to many highly instructive speculations, is not without its difficulties, when viewed with reference to the Idea of Vital Power. If the irritability of each muscle reside in the muscle itself, how does it differ from a mere mechanical force, as elasticity? But, in point of fact, it is certain that the muscular irritability of the animal body is not an attribute of the muscle itself independent of its connexion with the system. No muscle, or other part, removed from the body, *long* preserves its irritability. This power cannot subsist permanently, except in connexion with an organic whole. This condition peculiarly constitutes irritability a living force: and this condition would be satisfied by considering the force as derived

* *Hist. des Sci. Nat. depuis* 1789, I. 219.

from the nervous system; but it appears that though the nervous system has the most important influence upon all vital actions, the muscular irritability must needs be considered as something distinct. And thus the Irritability or Contractility of the muscle is a peculiar endowment of the texture, but it is at the same time an endowment which can only co-exist with life; it is, in short, a peculiar Vital Power.

11. This necessity of the union of the muscle with the whole nervous system, in order that it may possess irritability, was the meaning of the true part of Stahl's psychical doctrine; and the reason why he and his adherents persisted in asserting the power of the soul even over involuntary motions. This doctrine was the source of much controversy in later times.

"But," says Cuvier*, "this opposition of opinion may be reconciled by the intimate union of the nervous substance with the fibre and the other contractile organic elements, and by their reciprocal action;—doctrines which had been presented with so much probability by physiologists of the Scotch school, but which were elevated above the rank of hypotheses only by the observations of more recent times.

"The fibre does not contract by itself, but by the influence of the nervous filaments, which are always united with it. The change which produces the contraction cannot take place without the concurrence of both these substances; and it is further necessary that it should be occasioned each time by an exterior cause, by a stimulant.

"The will is one of these stimulants; but it only excites the irritability, it does not constitute it; for in the case of persons paralytic from apoplexy, the irritability remains, though the power of the will over it is

* *Hist. des Sci. Nat. depuis* 1789, I. 213.

gone. Thus *irritability* depends in part on the nerve, but not on the *sensibility:* this last is another property, still more admirable and occult than the irritability; but it is only one among several functions of the nervous system. It would be an abuse of words to extend this denomination to functions unaccompanied by perception."

12. Supposing, then, that Contractility is established as a peculiar Vital Power residing in the muscles, we may ask whether we can trace with any further exactness the seat and nature of this power. It would be unsuitable to the nature of the present work to dwell upon the anatomical discussions bearing upon this point. I will only remark that some anatomists maintain* that muscles are contracted by those fibres assuming a zigzag form, which at first were straight. Others (Professor Owen and Dr. A Thompson,) doubt the accuracy of this observation; and conceive that the muscular fibre becomes shorter and thicker, but does not deviate from a right line. We may remark that the latter kind of action appears to be more elementary in its nature. We can conceive a straight line thrown into a zigzag shape by muscular contractions taking place between remote parts of it; but it is difficult to conceive by what *elementary* mode of action a straight fibre could bend itself at certain points, and at certain points only; since the elementary force must act at every point of the fibre, and not at certain selected points.

13. A circumstance which remarkably marks the difference between the vital force of Contractility, inherent in muscles, and any merely dead or mechanical force, is this; that in assuming their contractile state, muscles exert a tension which they could not themselves support or convey if not strengthened by their vital irritability. They are capable of raising weights by their

* Müller, *Elem. Phys.*, p. 887.

exertion, which will tear them asunder when the power of contraction is lost by death. This has induced Cuvier and other physiologists* to believe "that in the moment of action, the particles that compose a fibre, not only approach towards each other longitudinally, but that their cohesive attraction becomes instantaneously much greater than it was before: for without such an increase of cohesive force, the tendency to shorten could not, as it would appear, prevent the fibre from being torn." We see here the difficulty, or rather the impossibility, of conceiving muscular contractility as a mere mechanical force; and perhaps there is little hope of any advantage by calling in the aid of chemical hypothesis to solve the mechanical difficulty. Cuvier conjectures that a sudden change in the chemical composition may thus so quickly and powerfully augment the cohesion. But we may ask, are not a chemical synthesis and analysis, suddenly performed by a mere act of the will, as difficult to conceive as a sudden increase and decrease of mechanical power directly produced by the same cause?

14. *Sensibility.* The nerves are the organs and channels of sensibility. By means of them we receive our sensations, whether of mere pleasure and pain, or of qualities which we ascribe to external objects, as a bitter taste, a sweet odour, a shrill sound, a red colour, a hard or a hot object of touch. Some of these sensations are but obscurely the objects of our consciousness; as for example the feeling which our feet have of the ground, or the sight which our eyes have of neighbouring objects, when we walk in a reverie. In these cases the sensations, though obscure, exist; for they serve to balance and guide us as we walk. In other cases, our sensations are distinctly and directly the objects of our attention.

But our sensations, as we have already said, we

* Prichard, *Vital Prin.*, p. 126.

ascribe as qualities to external objects. By our senses we perceive objects, and thus our *sensations* become *perceptions*. We have not only the sensation of round, purple, and green, repeated and varied, but the perception of a bunch of grapes partly ripe and partly unripe. We have not only sensations of noise and of variously-coloured specks rapidly changing their places, but we have perceptions, by sound and sight, of a stone rolling down the hill and crushing the shrubs in its path. We scarcely ever dwell upon our sensations; our thoughts are employed upon objects. We regard the impressions upon our nerves, not for what they *are*, but for what they *tell* us.

But in what language do the impressions upon the nerves thus speak to us of an external world,—of the forms and qualities and actions of objects? How is it that by the aid of our nervous system we become acquainted not only with impressions but with *things*; that we learn not only the relation of objects to us, but to one another?

15. It has been shown at some length in the previous Books, that the mode in which sensations are connected in our minds so as to convey to us the knowledge of objects and their relations, is by being contemplated with reference to *Ideas*. Our sensations, connected by the Idea of Space, become figures; connected by the Idea of Time, they become causes and effects; connected by the Idea of Resemblance, they become individuals and kinds; connected by the Idea of Organization, they become living things. It has been shown that without these Ideas there can be no connexion among our sensations, and therefore no perception of Figure, Action, Kind, or in short, of bodies under any aspect whatever. Sensations are the rude *Matter* of our perceptions; and are nothing, except so far as they

have *Form* given them by Ideas. But thus moulded by our Ideas, Sensation becomes the source of an endless store of important Knowledge of every possible kind.

16. But one of the most obvious uses of our perceptions and our knowledge is to direct our actions. It is suitable to the condition of our being that when we perceive a bunch of grapes, we should be able to pluck and eat the ripe ones; that when we perceive a stone rushing down the side of a hill, we should be able to move so as to avoid it. And this must be done by moving our limbs; in short, by the use of our muscles. And thus sensation leads, not directly, but through the medium of Ideas, to muscular contraction. I say that sensation and muscular action are in such cases connected through the medium of Ideas. For when we proceed to pluck the grape which we see, the *sensation* does not determine the motion of the hand by any necessary geometrical or mechanical conditions, as an impression made upon a machine determines its motions; but the *perception* leads us to stretch forth the hand to that part of space, wherever it is, where we *know* that the grape is, and this, not in any determinate path, but, it may be, avoiding or removing intervening obstacles, which we also *perceive*. There is in every such case a connexion between the sensation and the resulting action, not of a material but of a mental kind. The cause and the effect are bound together, not by physical but by intellectual ties.

17. And thus in such cases, between the two vital operations, sensation and muscular action, there intervenes, as an intermediate step, perception or knowledge, which is not merely vital but ideal. But this is not all; there is still another mental part of the process which may be readily distinguished from that which we have described. An act of the *Will*, a Volition, is that in the

mind which immediately determines the action of the muscles of the body. And thus Will intervenes between Knowledge and Action; and the cycle of operations which take place when animals act with reference to external objects is this:—Sensation, Perception, Volition, Muscular Contraction.

18. To attempt further to analyze the mental part of this cycle does not belong to the present part of our work. But we may remark here, as we have already remarked in the History*, how irresistibly we are led by physiological researches into the domain of thought and mind. We pass from the body to the soul, from physics to metaphysics; from biology to psychology; from things to persons; from nouns to pronouns. I have there noticed the manner in which Cuvier expresses this transition by the introduction of the pronoun: "The impression of external objects upon the ME, the production of a sensation, of an image, is a mystery impenetrable to our thoughts."

19. But to return to the merely biological part of our speculations. We have arrived, it will be perceived, at this result: that in animal actions there intervenes between the two terms of Sensation and Muscular Contraction, an intermediate process; which may be described as a communication to and from a center. The center is the seat of the sentient and volent faculties, and is of a hyperphysical nature. But the existence of such a center as a necessary element in the functions of the animal life is a truth which is important in biology. This indeed may be taken as the peculiar character of animal, as distinguished from merely organic powers. Accordingly, it is so stated by Bichat. For although he superfluously, as I have tried to show, introduces into his list of vital powers an organic sensibility, he still

* *Hist. Ind. Sci.* B. XVII. c. V. sect. 2.

draws the distinction of which I have spoken; "in the animal life, sensibility is the faculty of receiving an impression *plus* that of referring it to a common center*."

20. But since Sensibility and Contractility are thus connected by reference to a common Center, we may ask, before quitting the subject, what are the different forms which this reference assumes. Is the connexion always attended by the distinct steps of knowledge and will,—by a clear act of consciousness, as in the case which we have taken, of plucking a grape; or may these steps become obscure, or vanish altogether?

We need not further illustrate the former connexion. Such actions as we have described are called *voluntary* actions. In extreme cases, the mental part of the process is obvious enough. But we may gradually pass from these to cases in which the mental operation is more and more obscure.

In walking, in speaking, in eating, in breathing, our muscular exertions are directed by our sensations and perceptions: yet in such processes, how dimly are we conscious of perceptive and directive power! How the mind should be able to exercise such a power, and yet should be scarcely or not at all conscious of its exercise, is a very curious problem. But in all or in most of the above instances, the solution of this problem appears to depend upon psychological rather than biological principles, and therefore does not belong to this place.

21. But in cases at the other extreme, the mental part of the operation vanishes altogether. In many animals, even after decapitation, the limbs shrink when irritated. The motions of the iris are determined by the influence of light on our eyes, without our being aware of the motions. Here sensations produce motions, but with no trace of intervening perception or will.

* *Life and Death*, p. 84.

The sensation appears to be *reflected* back from the central element of animal life, in the form of a muscular contraction; but in this case the sensation is not modified or regulated by any *idea*. These reflected motions have no reference to relations of space or force among surrounding objects. They are blind and involuntary, like the movements of convulsion, depending for direction and amount only on the position and circumstances of the limb itself with its muscles. Here the Center from which the reflection takes place is merely animal, not intellectual.

In this case some physiologists have doubted whether the reflection of the sensation in the form of a muscular contraction does really take place from the Center; and have conceived that sensorial impressions might affect motor nerves without any communication with the nervous Center. But on this subject we may, I conceive, with safety adopt the decision of Professor Müller, deliberately given after a careful examination of the subject. "When impressions made by the action of external stimuli on sensitive nerves give rise to motions in other parts, these motions are never the result of the direct reaction of the sensitive and motor fibres of the nerves on each other; the irritation is conveyed by the sensitive fibres to the brain and spinal cord, and is by these communicated to the motor fibres."

22. Thus we have two extreme cases of the connexion of sensation with muscular action; in one of which the connexion clearly *is*, and in the other it as clearly *is not*, determined by relations of Ideas, in its transit through the nervous Center. There is another highly curious case standing intermediate between these two, and extremely difficult to refer to either. I speak of the case of *Instinct*.

Instinct leads to actions which are *such as if they* *were determined by Ideas.* The lamb follows its mother by instinct; but the motions by which it does this, the special muscular exertions, depend entirely upon the geometrical and mechanical relations of external bodies, as the form of the ground, and the force of the wind. The contractions of the muscles which are requisite in order that the creature may obey its instinct, vary with every variation of these external conditions;—are not determined by any rule or necessity, but by properties of space and force. Thus the action is not governed by sensations directly, but by sensations moulded by ideas. And the same is the case with other cases of instinct. The dog hunts by instinct; but he hunts certain kinds of animals merely, thus showing that his instinct acts according to resemblances and differences; he crosses the field repeatedly to find the track of his prey by scent; thus recognizing the relations of space with reference to the track; he leaps, adjusting his force to the distance and height of the leap with mechanical precision; and thus he practically recognizes the Ideas of Resemblance, Space, and Force.

But have animals such Ideas? In any proper sense in which we can speak of possessing Ideas, it appears plain that they have not. Animals cannot, at any time, be said properly to possess ideas, for ideas imply the possibility of *speculative* knowledge.

23. But even if we allow to animals only the *practical* possession of ideas, we have still a great difficulty remaining. In the case of man, his ideas are unfolded gradually by his intercourse with the external world. The child learns to distinguish forms and positions by a repeated and incessant use of his hands and eyes; he learns to walk, to run, to leap, by slow and laborious degrees; he distinguishes one man from another, and one animal from another, only after repeated mistakes.

Nor can we conceive this to be otherwise. How should the child know at once what muscles he is to exert in order to touch with his hand a certain visible object? How should he know what muscles to exert that he may stand and not fall, till he has tried often? How should he learn to direct his attention to the differences of different faces and persons, till he is roused by some memory, or hope which implies memory? It seems to us as if the sensations could not, without considerable practice, be rightly referred to Ideas of Space, Force, Resemblance, and the like.

Yet that which thus appears impossible, is in fact done by animals. The lamb almost immediately after its birth follows its mother, accommodating the actions of its muscles to the form of the ground. The chick, just escaped from the shell, picks up a minute insect, directing its beak with the greatest accuracy. Even the human infant seeks the breast and exerts its muscles in sucking, almost as soon as it is born. Hence, then, we see that Instinct produces at once actions regulated by Ideas, or, at least, which take place *as if* they were regulated by Ideas; although the Ideas cannot have been developed by exercise, and only appear to exist so far as such actions are concerned.

24. The term *Instinct* may properly be opposed to *Insight*. The former implies an inward principle of action, implanted within a creature and practically impelling it, but not capable of being developed into a subject of contemplation. While the instinctive actions of animals are directed by such a principle, the deliberate actions of man are governed by insight: he can contemplate the ideal relations on which the result of his action depends. He can in his mind map the path he will follow, and estimate the force he will exert, and class the objects he has to deal with, and determine his actions by the rela-

tions wich he thus has present to his mind. He thus possesses Ideas not only practically, but speculatively. And knowing that the Ideas by which he commonly directs his actions, Space, Cause, Resemblance, and the like, have been developed to that degree of clearness in which he possesses them by the assiduous exercise of the senses and the mind from the earliest stage of infancy, and that these Ideas are capable of being still further unfolded into long trains of speculative truth, he is unable to conceive the manner in which animals possess such Ideas as their instinctive actions disclose:—Ideas which neither require to be unfolded nor admit of unfolding; which are adequate for practical purposes without any previous exercise, and inadequate for speculative purposes with whatever labour cultivated.

I have ventured to make these few remarks on Instinct since it may, perhaps, justly be considered as the last province of Biology, where we reach the boundary line of Psychology. I have now, before quitting this subject, only one other principle to speak of.

Chapter VI.

OF THE IDEA OF FINAL CAUSES.

1. By an examination of those notions which enter into all our reasonings and judgments on living things, it appeared that we conceive animal life as a vortex or cycle of moving matter in which the form of the vortex determines the motions, and these motions again support the form of the vortex: the stationary parts circulate the fluids, and the fluids nourish the permanent parts. Each portion ministers to the others, each depends upon the other. The parts make up the whole, but the existence

of the whole is essential to the preservation of the parts. But parts existing under such conditions are *organs*, and the whole is *organized*. This is the fundamental conception of organization. "Organized beings," says the physiologist*, "are composed of a number of essential and mutually dependent parts." "An organized product of nature," says the great metaphysician†, "is that in which all the parts are mutually ends and means."

2. It will be observed that we do not content ourselves with saying that in such a whole, all the parts are *mutually dependent*. This might be true even of a mechanical structure; it would be easy to imagine a framework in which each part should be necessary to the support of each of the others; for example, an arch of several stones. But in such a structure, the parts have no properties which they derive from the whole. They are beams or stones when separate; they are no more when joined. But the same is not the case in an organized whole. The limb of an animal separated from the body, loses the properties of a limb, and soon ceases to retain even its form.

3. Nor do we content ourselves with saying that the parts are *mutually causes and effects*. This is the case in machinery. In a clock, the pendulum by means of the escapement causes the descent of the weight, the weight by the same escapement keeps up the motion of the pendulum. But things of this kind may happen by accident. Stones slide from a rock down the side of a hill and cause it to be smooth; the smoothness of the slope causes stones still to slide. Yet no one would call such a slide an organized system. The system is organized, when the effects which take place among the parts are *essential to our conception of the whole*; when the whole would not *be* a whole, nor the parts, parts, except

* Müller, *Elem.*, p. 18. † Kant, *Urtheilskraft*, p. 296.

these effects were produced; when the effects not only happen in fact, but are included in the idea of the object; when they are not only seen, but foreseen; not only expected, but intended: in short when, instead of being causes and effects, they are *ends* and *means*, as they are termed in the above definition.

Thus we necessarily include, in our Idea of Organization, the notion of an End, a Purpose, a Design; or, to use another phrase which has been peculiarly appropriated in this case, a *Final Cause*. This idea of a Final Cause is an essential condition in order to the pursuing our researches respecting organized bodies.

4. This Idea of Final Cause is not *deduced* from the phenomena by reasoning, but is *assumed* as the only condition under which we can reason on such subjects at all. We do not deduce the Idea of Space, or Time, or efficient Cause from the phenomena about us, but necessarily look at phenomena as subordinate to these Ideas from the beginning of our reasoning. It is true, our ideas of relations of Space, and Time, and Force, may become much more clear by our familiarizing ourselves with particular phenomena: but still, the Fundamental Ideas are not generated but unfolded; not extracted from the external world, but evolved from the world within. In like manner, in the contemplation of organic structures, we consider each part as subservient to some use, and we cannot study the structure as organic without such a conception. This notion of adaptation,—this Idea of an End,—may become much more clear and impressive by seeing it exemplified in particular cases. But still, though suggested and evoked by special cases, it is not furnished by them. If it be not supplied by the mind itself, it can never be logically deduced from the phenomena. It is not a portion of the facts which we study, but it is a principle which connects, includes, and renders

them intelligible; as our other Fundamental Ideas do the classes of facts to which they respectively apply.

5. This has already been confirmed by reference to fact; in the History of Physiology, I have shown that those who studied the structure of animals were irresistibly led to the conviction that the parts of this structure have each its end or purpose;—that each member and organ not merely produces a certain effect or answers a certain use, but is so framed as to impress us with the persuasion that it was constructed *for* that use:—that it was *intended* to produce the effect. It was there seen that this persuasion was repeatedly expressed in the most emphatic manner by Galen;—that it directed the researches and led to the discoveries of Harvey;—that it has always been dwelt upon as a favourite contemplation, and followed as a certain guide, by the best anatomists;—and that it is inculcated by the physiologists of the profoundest views and most extensive knowledge of our own time. All these persons have deemed it a most certain and important principle of physiology, that in every organized structure, plant or animal, each intelligible part has its allotted office:—each organ is designed for its appropriate function:—that nature, in these cases, produces nothing in vain: that, in short, each portion of the whole arrangement has its *final cause;* an end to which it is adapted, and in this end, the reason that it is where and what it is.

6. This Notion of Design in organized bodies must, I say, be supplied by the student of organization out of his own mind: a truth which will become clearer if we attend to the most conspicuous and acknowledged instances of *design*. The structure of the eye, in which the parts are curiously adjusted so as to produce a distinct image on the retina, as in an optical instrument;—the trochlear muscle of the eye, in which the tendon passes

round a support and turns back, like a rope round a pulley;—the prospective contrivances for the preservation of animals, provided long before they are wanted, as the milk of the mother, the teeth of the child, the eyes and lungs of the fœtus:—these arrangements, and innumerable others, call up in us a persuasion that Design has entered into the plan of animal form and progress. And if we bring in our minds this conception of Design, nothing can more fully square with and fit it, than such instances as these. But if we did not already possess the Idea of Design;—if we had not had our notion of mechanical contrivance awakened by inspection of optical instruments, or pulleys, or in some other way;—if we had never been conscious ourselves of providing for the future;—if this were the case, we could not recognize contrivance and prospectiveness in such instances as we have referred to. The facts are, indeed, admirably in accordance with these conceptions, when the two are brought together: but the facts and the conceptions come together from different quarters—from without and from within.

7. We may further illustrate this point by referring to the relations of travellers who tell us that when consummate examples of human mechanical contrivance have been set before savages, they have appeared incapable of apprehending them as proofs of design. This shows that in such cases the Idea of Design had not been developed in the minds of the people who were thus unintelligent: but it no more proves that such an idea does not naturally and necessarily arise, in the progress of men's minds, than the confused manner in which the same savages apprehend the relations of space, or number, or cause, proves that these ideas do not naturally belong to their intellects. All men have these ideas; and it is because they cannot help referring their

sensations to such ideas, that they apprehend the world as existing in time and space, and as a series of causes and effects. It would be very erroneous to say that the belief of such truths is obtained by logical reasoning from facts. And in like manner we cannot logically deduce design from the contemplation of organic structures; although it is impossible for us, when the facts are clearly before us, not to find a reference to design operating in our minds.

8. Again; the evidence of the doctrine of Final Causes as a fundamental principle of Biology may be obscured and weakened in some minds by the constant habit of viewing this doctrine with suspicion as unphilosophical and at variance with morphology. By cherishing such views, it is probable that many persons, physiologists and others, have gradually brought themselves to suppose that many or most of the arrangements which are familiarly adduced as instances of design may be accounted for, or explained away;—that there is a certain degree of prejudice and narrowness of comprehension in that lively admiration of the adaptation of means to ends which common minds derive from the spectacle of organic arrangements. And yet, even in persons accustomed to these views, the strong and natural influence of the Idea of a Final Cause, the spontaneous recognition of the relation of means to an end as the assumption which makes organic arrangements intelligible, breaks forth when we bring before them a new case, with regard to which their genuine convictions have not yet been modified by their intellectual habits. I will offer, as an example which may serve to illustrate this, the discoveries recently made with regard to the process of suckling in the kangaroo. In the case of this, as of other pouched animals, the young animal is removed, while very small and imperfectly formed, from

the womb to the pouch, in which the teats are, and is there placed with its lips against one of the nipples. But the young animal taken altogether is not so large as the nipple, and is therefore incapable of sucking after the manner of common mammals. Here is a difficulty: how is it overcome?—By an appropriate *contrivance:* the nipple, which in common mammals is not furnished with any muscle, is in the kangaroo provided with a powerful extrusory muscle by which the mother can inject the milk into the mouth of her offspring. And again; in order to give attachment to this muscle there is a bone which is not found in animals of other kinds. But this mode of solving the problem of suckling so small a creature introduces another difficulty. If the milk is injected into the mouth of the young one, without any action of its own muscles, what is to prevent the fluid entering the windpipe and producing suffocation? How is this danger avoided?—By another appropriate *contrivance:* there is a funnel in the back of the throat by which the air passage is completely separated from the passage for nutriment, and the injected milk passes in a divided stream on each side of the larynx to the œsophagus*. And as if to show that this apparatus is really formed with a view to the wants of the young one, the structure alters in the course of the animal's growth; and the funnel, no longer needed, is modified and disappears.

With regard to this and similar examples, the remark which I would urge is this:—that no one, however prejudiced or unphilosophical he may in general deem the reference to Final Causes, can, at the first impression, help regarding this curious system of arrangement as the means to an end. So contemplated, it becomes significant, intelligible, admirable: without such a prin-

* Mr. Owen, in *Phil. Trans.*, 1834, p. 348.

ciple, it is an unmeaning complexity, a collection of contradictions, producing an almost impossible result by a portentous conflict of chances. The parts of this apparatus cannot have produced one another; one part is in the mother; another part in the young one: without their harmony they could not be effective; but nothing except design can operate to make them harmonious. They are *intended* to work together; and we cannot resist the conviction of this intention when the facts first come before us. Perhaps there may hereafter be physiologists who, tracing the gradual developement of the parts of which we have spoken, and the analogies which connect them with the structures of other animals, may think that this developement, these analogies, account for the conformation we have described; and may hence think lightly of the explanation derived from the reference to Final Causes. Yet surely it is clear, on a calm consideration of the subject, that the latter explanation is not disturbed by the former; and that the observer's first impression, that this is "an irrefragable evidence of creative foresight*," can never be obliterated; however much it may be obscured in the minds of those who confuse this view by mixing it with others which are utterly heterogeneous to it, and therefore cannot be contradictory.

9. I have elsewhere† remarked how physiologists, who thus look with suspicion and dislike upon the introduction of Final Causes into physiology, have still been unable to exclude from their speculations causes of this kind. Thus Cabanis says‡, "I regard with the great Bacon, the philosophy of Final Causes as sterile; but I have elsewhere acknowledged that it was

* Mr. Owen, in *Phil. Trans.*, 1834, p. 349.

† *Bridgewater Treatise,* p. 352.

‡ *Rapports du Physique et du Moral,* I. 299.

very difficult for the most cautious man never to have recourse to them in his explanations." Accordingly, he says, "The partisans of Final Causes nowhere find arguments so strong in favour of their way of looking at nature as in the laws which preside and the circumstances of all kinds which concur in the reproduction of living races. In no case do the means employed appear so clearly relative to the end." And it would be easy to find similar acknowledgments, express or virtual, in other writers of the same kind. Thus Bichat, after noting the difference between the organic sensibility by which the organs are made to perform their offices, and the animal sensibility of which the nervous center is the seat, says*, "No doubt it will be asked, *why*"—that is, as we shall see, for what *end*—"the organs of internal life have received from nature an inferior degree of sensibility only, and why they do not transmit to the brain the impressions which they receive, while all the acts of the animal life imply this transmission? The reason is simply this, that all the phenomena which establish our connexions with surrounding objects *ought to be*, and are in fact, under the influence of the will; while all those which serve for the purpose of assimilation only, escape, and *ought* indeed to escape, such influence." The *reason* here assigned is the Final Cause; which, as Bichat justly says, we cannot help asking for.

10. Again; I may quote from the writer last mentioned another remark, which shows that in the organical sciences, and in them alone, the Idea of forces as Means acting to an End, is inevitably assumed and acknowledged as of supreme authority. In Biology alone, observes Bichat†, have we to contemplate the state of *disease*. "Physiology is to the movements of living

* *Life and Death*, (trans.) p. 32.

† *Anatomie Générale*, I. liii.

bodies, what astronomy, dynamics, hydraulics, &c., are to those of inert matter: but these latter sciences have no branches which correspond to them as pathology corresponds to physiology. For the same reason all notion of a medicament is repugnant to the physical sciences. A medicament has for its object to bring the properties of the system back to their natural type; but the physical properties never depart from this type, and have no need to be brought back to it: and thus there is nothing in the physical sciences which holds the place of therapeutick in physiology." Or, as we might express it otherwise, of inert forces we have no conception of what they *ought to do*, except what they *do*. The forces of gravity, elasticity, affinity, never act in a *diseased* manner; we never conceive them as failing in their purpose; for we do not conceive them as having any purpose which is answered by one mode of their action rather than another. But with organical forces the case is different; they are necessarily conceived as acting for the preservation and developement of the system in which they reside. If they do not do this, they fail, they are deranged, diseased. They have for their object to conform the living being to a certain type; and if they cause or allow it to deviate from this type, their action is distorted, morbid, contrary to the ends of nature. And thus this conception of organized beings as susceptible of disease, implies the recognition of a state of health, and of the organs and the vital forces as means for preserving this normal condition. The state of health and of perpetual developement is necessarily contemplated as the Final Cause of the processes and powers with which the different parts of plants and animals are endowed.

11. This Idea of a Final Cause is applicable as a fundamental and regulative idea to our speculations

concerning organized creatures only. That there is a purpose in many other parts of the creation, we find abundant reason to believe, from the arrangements and laws which prevail around us. But this persuasion is not to be allowed to regulate and direct our reasonings with regard to inorganic matter, of which conception the relation of means and end forms no essential part. In mere Physics, Final Causes, as Bacon has observed, are not to be admitted as a principle of reasoning. But in the organical sciences, the assumption of design and purpose in every part of every whole, that is, the pervading idea of Final Cause, is the basis of sound reasoning and the source of true doctrine.

12. The Idea of Final Cause, of end, purpose, design, intention, is altogether different from the Idea of Cause, as Efficient Cause, which we formerly had to consider; and on this account the use of the word *Cause* in this phrase has been objected to. If the idea be clearly entertained and steadily applied, the word is a question of subordinate importance. The term *Final Cause* has been long familiarly used, and appears not likely to lead to confusion.

13. The consideration of Final Causes, both in physiology and in other subjects, has at all times attracted much attention, in consequence of its bearing upon the belief of an Intelligent Author of the Universe. I do not intend, in this place, to pursue the subject far in this view: but there is one antithesis of opinion, already noticed in the History of Physiology, on which I will again make a few remarks*.

It has appeared to some persons that the mere aspect of order and symmetry in the works of nature—the contemplation of comprehensive and consistent law—is

* *Hist. Ind. Sci.* B. XVII. chap. viii. On the Doctrine of Final Causes in Physiology.

sufficient to lead us to the conception of a design and intelligence producing the order and carrying into effect the law. Without here attempting to decide whether this is true, we may discern, after what has been said, that the conception of Design, arrived at in this manner, is altogether different from that Idea of Design which is suggested to us by organized bodies, and which we describe as the doctrine of Final Causes. The regular form of a crystal, whatever beautiful symmetry it may exhibit, whatever general laws it may exemplify, does not prove design in the same manner in which design is proved by the provisions for the preservation and growth of the seeds of plants, and of the young of animals. The law of universal gravitation, however wide and simple, does not impress us with the belief of a purpose, as does that propensity by which the two sexes of each animal are brought together. If it could be shown that the symmetrical structure of a flower results from laws of the same kind as those which determine the regular forms of crystals, or the motions of the planets, the discovery might be very striking and important, but it would not at all come under our idea of Final Cause.

14. Accordingly, there have been, in modern times, two different schools of physiologists, the one proceeding upon the idea of Final Causes, the other school seeking in the realm of organized bodies wide laws and analogies from which that idea is excluded. All the great biologists of preceding times, and some of the greatest of modern times, have belonged to the former school; and especially Cuvier, who may be considered as the head of it. It was solely by the assiduous application of this principle of Final Cause, as he himself constantly declared, that he was enabled to make the discoveries which have rendered his name so illustrious, and which contain a far larger portion of important anatomical

and biological truth than it ever before fell to the lot of one man to contribute to the science.

The opinions which have been put in opposition to the principle of Final Causes have, for the most part, been stated vaguely and ambiguously. Among the most definite of such principles, is that which, in the History of the subject, I have termed the Principle of metamorphosed and developed Symmetry, upon which has been founded the science of Morphology.

The reality and importance of this principle are not to be denied by us: we have shown how they are proved by its application in various sciences, and especially in botany. But those advocates of this principle who have placed it in antithesis to the doctrine of Final Causes, have, by this means, done far more injustice to their own favourite doctrine than damage to the one which they opposed. The adaptation of the bones of the skeleton to the muscles, the provision of fulcrums, projecting processes, channels, so that the motions and forces shall be such as the needs of life require, cannot possibly become less striking and convincing, from any discovery of general analogies of one animal frame with another, or of laws connecting the developement of different parts. Whenever such laws are discovered, we can only consider them as the means of producing that adaptation which we so much admire. Our conviction that the Artist works intelligently, is not destroyed, though it may be modified and transferred, when we obtain a sight of his tools. Our discovery of laws cannot contradict our persuasion of ends; our Morphology cannot prejudice our Teleology.

15. The irresistible and constant apprehension of a purpose in the forms and functions of animals has introduced into the writings of speculators on these subjects various forms of expression, more or less precise, more

or less figurative; as, that "animals are framed with a view to the part which they have to play;"—that "nature does nothing in vain;" that "she employs the best means for her ends;" and the like. However metaphorical or inexact any of these phrases may be in particular, yet taken altogether, they convey, clearly and definitely enough to preclude any serious errour, a principle of the most profound reality and of the highest importance in the organical sciences. But some adherents of the morphological school of which I have spoken reject, and even ridicule, all such modes of expression. "I know nothing," says M. Geoffroy Saint Hilaire, "of animals which have to play a part in nature. I cannot make of nature an intelligent being who does nothing in vain; who acts by the shortest mode; who does all for the best." The philosophers of this school, therefore, do not, it would seem, feel any of the admiration which is irresistibly excited in all the rest of mankind at the contemplation of the various and wonderful adaptations for the preservation, the enjoyment, the continuation of the creatures which people the globe;—at the survey of the mechanical contrivances, the chemical agencies, the prospective arrangements, the compensations, the minute adaptations, the comprehensive interdependencies, which zoology and physiology have brought into view, more and more, the further their researches have been carried. Yet the clear and deep-seated conviction of the reality of these provisions, which the study of anatomy produces in its most profound and accurate cultivators, cannot be shaken by any objections to the metaphors or terms in which this conviction is clothed. In regard to the Idea of a Purpose in organization, as in regard to any other idea, we cannot fully express our meaning by phrases borrowed from any extraneous source; but that impossibility arises precisely from the circumstance of its being a Fundamental Idea which is inevitably assumed in our representation of each special fact. The same objection has been made to the idea of mechanical *force*, on account of its being often expressed in metaphorical language; for writers have spoken of an *energy*, *effort*, or *solicitation* to motion; and bodies have been said to be *animated* by a force. Such language, it has been urged, implies volition, and the act of animated beings. But the idea of Force as distinct from mere motion,—as the Cause of motion, or of tendency to motion,—is not on that account less real. We endeavour in vain to conduct our mechanical reasonings without the aid of this idea, and must express it as we can. Just as little can we reason concerning organized beings without assuming that each part has its function, each function its purpose; and so far as our phrases imply this, they will not mislead us, however inexact, or however figurative they be.

16. The doctrine of a purpose in Organization has been sometimes called the doctrine of *the Conditions of Existence*; and has been stated as teaching that each animal must be so framed as to contain in its structure the Conditions which its existence requires. When expressed in this manner, it has given rise to the objection, that it merely offers an identical proposition; since no animal *can* exist without such conditions. But in reality, such expressions as those just quoted give an inadequate statement of the Principle of a Final Cause. For we discover in innumerable cases, arrangements in an animal, of which we see, indeed, that they are subservient to its well being; but the nature of which we never should have been able at all to conjecture, from considering what was necessary to its existence, and which strike us, no less by their unexpectedness than by their adaptation: so far are they from being presented

by any perceptible necessity. Who would venture to say that the trochlear muscle, or the power of articulate speech, must occur in man, because they are the necessary conditions of his existence? When, indeed, the general scheme and mode of being of an animal are known, the expert and profound anatomist can reason concerning the proportions and form of its various parts and organs, and prove in some measure what their relations must be. We can assert, with Cuvier, that certain forms of the viscera require certain forms of the teeth, certain forms of the limbs, certain powers of the senses. But in all this, the functions of self-nutrition and digestion are supposed already existing as ends: and it being taken for granted, as the only conceivable basis of reasoning, that the organs are means to these ends, we may discover what modifications of these organs are necessarily related to and connected with each other. Instead of terming this rule of speculation merely "the Principle of the Conditions of Existence," we might term it "the Principle of the conditions of organs as *Means* adapted to animal existence as their *End*." And how far this principle is from being a mere barren truism, the extraordinary discoveries made by the great assertor of the principle, and universally assented to by naturalists, abundantly prove. The vast extinct creation which is recalled to life in Cuvier's great work, the *Ossemens Fossiles*, cannot be the consequence of a mere identical proposition.

17. It has been objected, also, that the doctrine of Final Causes supposes us to be acquainted with the intentions of the Creator; which, it is insinuated, is a most presumptuous and irrational basis for our reasonings. But there can be nothing presumptuous or irrational in reasoning on that basis, which if we reject, we cannot reason at all. If men really can discern, and

cannot help discerning, a design in certain portions of the works of creation, this perception is the soundest and most satisfactory ground for the convictions to which it leads. The Ideas which we necessarily employ in the contemplation of the world around us, afford us the only natural means of forming any conception of the Creator and Governor of the Universe; and if we are by such means enabled to elevate our thoughts, however inadequately, towards Him, where is the presumption of doing so? or rather, where is the wisdom of refusing to open our minds to contemplations so animating and elevating, and yet so entirely convincing? We possess the ideas of Time and Space, under which all the objects of the universe present themselves to us; and in virtue of these ideas thus possessed, we believe the Creator to be eternal and omnipotent. When we find that we, in like manner, possess the idea of a Design in Creation, and that with regard to ourselves, and creatures more or less resembling ourselves, we cannot but contemplate their constitution under this idea, we cannot abstain from ascribing to the Creator the infinite profundity and extent of design to which all these special instances belong as parts of a whole.

18. I have here considered Design as manifest in organization only: for in that field of speculation it is forced upon us as contained in all the phenomena, and as the only mode of our understanding them. The existence of Final Causes has often been pointed out in other portions of the creation;—for instance, in the apparent adaptations of the various parts of the earth and of the solar system to each other and to organized beings. In these provinces of speculation, however, the principle of Final Causes is no longer the basis and guide, but the sequel and result of our physical reasonings. If in looking at the universe, we follow the widest analogies of

which we obtain a view, we see, however dimly, reason to believe that all its laws are adapted to each other, and intended to work together for the benefit of its organic population, and for the general welfare of its rational tenants. On this subject, however, not immediately included in the principle of Final Causes as here stated, I shall not dwell. I will only make this remark; that the assertion appears to be quite unfounded, that as science advances from point to point, final causes recede before it, and disappear one after the other. The principle of design changes its mode of application indeed, but it loses none of its force. We no longer consider particular facts as produced by special interpositions, but we consider design as exhibited in the establishment and adjustment of the laws by which particular facts are produced. We do not look upon each particular cloud as brought near us that it may drop fatness on our fields; but the general adaptation of the laws of heat, and air, and moisture, to the promotion of vegetation, does not become doubtful. We do not consider the sun as less intended to warm and vivify the tribes of plants and animals, because we find that, instead of revolving round the earth as an attendant, the earth along with other planets revolves round him. We are rather, by the discovery of the general laws of nature, led into a scene of wider design, of deeper contrivance, of more comprehensive adjustments. Final causes, if they appear driven further from us by such an extension of our views, embrace us only with a vaster and more majestic circuit: instead of a few threads connecting some detached objects, they become a stupendous net-work, which is wound round and round the universal frame of things.

19. I now quit the subject of Biology, and with it the circle of sciences depending upon separate original Ideas

and permanent relations. If from the general relations which permanently prevail and constantly recur among the objects around us, we turn to the inquiry of what has actually happened,—if from Science we turn to History,—we find ourselves in a new field. In this region of speculation we can rarely obtain a complete and scientific view of the connexion between objects and events. The past History of Man, of the Arts, of Languages, of the Earth, of the Solar System, offers a vast series of problems, of which perhaps not one has been rigorously solved. Still man, as his speculative powers unfold themselves, cannot but feel prompted and invited to employ his thoughts even on these problems. He cannot but wish and endeavour to understand the connexion between the successive links of such chains of events. He attempts to form a Science which shall be applicable to each of these Histories; and thus he begins to construct the class of sciences to which I now, in the last place, proceed.

BOOK X.

THE PHILOSOPHY OF PALÆTIOLOGY.

CHAPTER I.

OF PALÆTIOLOGICAL SCIENCES IN GENERAL.

1. I HAVE already stated in the *History of the Sciences**, that the class of Sciences which I designate as *Palætiological* are those in which the object is to ascend from the present state of things to a more ancient condition, from which the present is derived by intelligible causes. As conspicuous examples of this class we may take Geology, Glossology or Comparative Philology, and Comparative Archæology. These provinces of knowledge might perhaps be intelligibly described as *Histories;* the History of the Earth,—the History of Languages,—the History of Arts. But these phrases would not fully describe the sciences we have in view; for the object to which we now suppose their investigations to be directed is, not merely to ascertain what the series of events has been, as in the common forms of History, but also how it has been brought about. These sciences are to treat of causes as well as of effects. Such researches might be termed *Philosophical History;* or, in order to mark more distinctly that the *causes* of events are the leading object of attention, *Ætiological History*. But since it will be more convenient to describe this class of sciences by a single appellation, I

* B. XVIII. Introd.

have taken the liberty of proposing to call them* the *Palætiological* Sciences.

While *Palæontology* describes the beings which have lived in former ages without investigating their causes, and *Ætiology* treats of causes without distinguishing historical from mechanical causation; *Palætiology* is a combination of the two sciences; exploring, by means of the second, the phenomena presented by the first. The portions of knowledge which I include in this term are palæontological ætiological sciences.

2. All these sciences are connected by this bond;—that they all endeavour to ascend to a past state, by considering what is the present state of things, and what are the causes of change. Geology examines the existing appearance of the materials which form the earth, infers from them previous conditions, and speculates concerning the forces by which one condition has been made to succeed another. Another science, cultivated with great zeal and success in modern times, compares the languages of different countries and nations, and by an examination of their materials and structure, endeavours to determine their descent from one another: this science has been termed *Comparative Philology*, or *Ethnography;* and by the French, *Linguistique*, a word which we might imitate in order to have a single name for the science, but the Greek derivative *Glossology* appears to be more convenient in its form. The progress of the Arts (Architecture and the like);—how one stage of the culture produced another; and how far we can

* A philological writer, in a very interesting work, (Mr. Donaldson, in his *New Cratylus*, p. 12) expresses his dislike of this word, and suggests that I must mean *palæ-ætiological.* I think the word is more likely to obtain currency in the more compact and euphonious form in which I have used it. It has been adopted by Mr. Winning, in his *Manual of Comparative Philology*, and more recently, by other writers.

trace their maturest and most complete condition to their earliest form in various nations;—are problems of great interest belonging to another subject, which we may for the present term *Comparative Archæology*. I have already noticed, in the *History**, how the researches into the origin of natural objects, and those relating to works of art, pass by slight gradations into each other; how the examination of the changes which have affected an ancient temple or fortress, harbour or river, may concern alike the geologist and the antiquary. Cuvier's assertion that the geologist is an antiquary of a new order, is perfectly correct, for both are palætiologists.

3. We are very far from having exhausted, by this enumeration, the class of sciences which are thus connected. We may easily point out many other subjects of speculation of the same kind. As we may look back towards the first condition of our planet, we may in like manner turn our thoughts towards the first condition of the solar system, and try whether we can discern any traces of an order of things antecedent to that which is now established; and if we find, as some great mathematicians have conceived, indications of an earlier state in which the planets were not yet gathered into their present forms, we have, in the pursuit of this train of research, a palætiological portion of Astronomy. Again, as we may inquire how languages, and how man, have been diffused over the earth's surface from place to place, we may make the like inquiry with regard to the races of plants and animals, founding our inferences upon the existing geographical distribution of the animal and vegetable kingdoms: and thus the Geography of Plants and of Animals also becomes a portion of Palætiology. Again, as we can in some measure trace the progress of Arts from nation to nation and from age

* B. XVIII. Introd.

to age, we can also pursue a similar investigation with respect to the progress of Mythology, of Poetry, of Government, of Law. Thus the philosophical history of the human race, viewed with reference to these subjects, if it can give rise to knowledge so exact as to be properly called *Science*, will supply sciences belonging to the class I am now to consider.

4. It is not an arbitrary and useless proceeding to construct such a Class of sciences. For wide and various as their subjects are, it will be found that they have all certain principles, maxims, and rules of procedure in common; and thus may reflect light upon each other by being treated of together. Indeed it will, I trust, appear, that we may by such a juxtaposition of different speculations, obtain most salutary lessons. And questions, which, when viewed as they first present themselves under the aspect of a special science, disturb and alarm men's minds, may perhaps be contemplated more calmly, as well as more clearly, when they are considered as general problems of palætiology.

5. It will at once occur to the reader that, if we include in the circuit of our classification such subjects as have been mentioned,—politics and law, mythology and poetry,—we are travelling very far beyond the material sciences within whose limits we at the outset proposed to confine our discussion of principles. But we shall remain faithful to our original plan; and for that purpose shall confine ourselves, in this work, to those palætiological sciences which deal with material things. It is true, that the general principles and maxims which regulate these sciences apply also to investigations of a parallel kind respecting the products which result from man's imaginative and social endowments. But although there may be a similarity in the general form of such portions of knowledge, their materials are so different

from those with which we have been hitherto dealing, that we cannot hope to take them into our present account with any profit. Language, Government, Law, Poetry, Art, embrace a number of peculiar Fundamental Ideas, hitherto not touched upon in the disquisitions in which we have been engaged; and most of them involved in far greater perplexity and ambiguity, the subject of controversies far more vehement, than the Ideas we have hitherto been examining. We must therefore avoid resting any part of our philosophy upon sciences, or supposed sciences, which treat of such subjects. To attend to this caution, is the only way in which we can secure the advantage we proposed to ourselves at the outset, of taking, as the basis of our speculations, none but systems of undisputed truths, clearly understood and expressed*. We have already said that we must, knowingly and voluntarily, resign that livelier and warmer interest which doctrines on subjects of Polity or Art possess, and content ourselves with the cold truths of the material sciences, in order that we may avoid having the very foundations of our philosophy involved in controversy, doubt, and obscurity.

6. We may remark, however, that the necessity of rejecting from our survey a large portion of the researches which the general notion of Palætiology includes, suggests one consideration which adds to the interest of our task. We began our inquiry with the trust that any sound views which we should be able to obtain respecting the nature of Truth in the physical sciences, and the mode of discovering it, must also tend to throw light upon the nature and prospects of knowledge of all other kinds;—must be useful to us in moral, political, and philological researches. We stated this as a confident anticipation; and the evidence of the justice

* See Vol. I. p. 8.

of our belief already begins to appear. We have seen, in the last Book, that biology leads us to psychology, if we choose to follow the path; and thus the passage from the material to the immaterial has already unfolded itself at one point; and we now perceive that there are several large provinces of speculation which concern subjects belonging to man's immaterial nature, and which are governed by the same laws as sciences altogether physical. It is not our business here to dwell on the prospects which our philosophy thus opens to our contemplation; but we may allow ourselves, in this last stage of our pilgrimage among the foundations of the physical sciences, to be cheered and animated by the ray that thus beams upon us, however dimly, from a higher and brighter region.

But in our reasonings and examples we shall mainly confine ourselves to the physical sciences; and for the most part to Geology, which in the *History* I have put forwards as the best representative of the Palætiological Sciences.

Chapter II.

OF THE THREE MEMBERS OF A PALÆTIOLOGICAL SCIENCE.

1. *Divisions of such Sciences.*—In each of the Sciences of this class we consider some particular order of phenomena now existing:—from our knowledge of the causes of change among such phenomena, we endeavour to infer the causes which have made this order of things what it is:—we ascend in this manner to some previous stage of such phenomena;—and from that, by a similar course of inference, to a still earlier stage, and to *its* causes.

Hence it will be seen that each such science will consist of two parts,—the knowledge of the Phenomena, and the knowledge of their Causes. And such a division is, in fact, generally recognized in such sciences: thus we have History, and the Philosophy of History; we have Comparison of Languages, and the Theories of the Origin and Progress of Language; we have Descriptive Geology, and Theoretical or Physical Geology. In all these cases, the relation between the two parts in these several provinces of knowledge is nearly the same; and it may, on some occasions at least, be useful to express the distinction in a uniform or general manner. The investigation of causes has been termed *Ætiology* by philosophical writers, and this term we may use, in contradistinction to the mere *Phenomenology* of each such department of knowledge. And thus we should have *Phenomenal Geology* and *Ætiological Geology*, for the two divisions of the science which we have above termed *Descriptive* and *Theoretical Geology*.

2. *The Study of Causes.*—But our knowledge respecting the causes which actually *have* produced any order of phenomena must be arrived at by ascertaining what the causes of change in such matters *can* do. In order to learn, for example, what share earthquakes, and volcanoes, and the beating of the ocean against its shores, ought to have in our Theory of Geology, we must make out what effects these agents of change are able to produce. And this must be done, not hastily, or unsystematically, but in a careful and connected manner; in short, this study of the causes of change in each order of phenomena must become a distinct body of Science, which must include a large amount of knowledge, both comprehensive and precise, before it can be applied to the construction of a theory. We must have an Ætiology corresponding to each order of phenomena.

3. *Ætiology.*—In the History of Geology, I have spoken of the necessity for such an Ætiology with regard to geological phenomena: this necessity I have compared with that which, at the time of Kepler, required the formation of a separate science of Dynamics, (the doctrine of the causes of motion,) before Physical Astronomy could grow out of Phenomenal Astronomy. In pursuance of this analogy, I have there given the name of *Geological Dynamics* to the science which treats of the causes of geological change in general. But, as I have there intimated, in a large portion of the subject the changes are so utterly different in their nature from any modification of motion, that the term *Dynamics*, so applied, sounds harsh and strange. For in this science we have to treat, not only of the subterraneous forces by which parts of the earth's crust are shaken, elevated, or ruptured, but also of the causes which may change the climate of a portion of the earth's surface, making a country hotter or colder than in former ages; again, we have to treat of the causes which modify the forms and habits of animals and vegetables, and of the extent to which the effects of such causes can proceed; whether, for instance, they can extinguish old species and produce new. These and other similar investigations would not be naturally included in the notion of *Dynamics;* and therefore it might perhaps be better to use the term *Ætiology* when we wish to group together all those researches which have it for their object to determine the laws of such changes. In the same manner the Comparison and History of Languages, if it is to lead to any stable and exact knowledge, must have appended to it an Ætiology, which aims at determining the nature and the amount of the causes which really do produce changes in language; as colonization, conquest, the mixture of races, civilization, literature, and the like. And

the same rule applies to all sciences of this class. We shall now make a few remarks on the characteristics of such branches of science as those to which we are led by the above considerations.

4. *Phenomenology requires Classification. Phenomenal Geology.*—The Phenomenal portions of each science imply Classification, for no description of a large and varied mass of phenomena can be useful or intelligible without classification. A representation of phenomena, in order to answer the purposes of science, must be systematic. Accordingly, in giving the History of Descriptive or Phenomenal Geology, I have called it *Systematic Geology,* just as Classificatory Botany is termed *Systematic Botany.* Moreover, as we have already seen, Classification can never be an arbitrary process, but always implies some natural connexion among the objects of the same class; for if this connexion did not exist, the classes could not be made the subjects of any true assertion. Yet though the classes of phenomena which our system acknowledges must be such as already exist in nature, the discovery of these classes is, for the most part, very far from obvious or easy. To detect the true principles of natural classes, and to select marks by which these may be recognized, are steps which require genius and good fortune, and which fall to the lot only of the most eminent persons in each science. In the History, I have pointed out Werner, William Smith, and Cuvier, as the three great authors of Systematic Geology of Europe. The mode of classifying the materials of the earth's surface which was found, by these philosophers, fitted to enunciate such general facts as came under their notice, was to consider the rocks and other materials as divided into successive layers or strata, superimposed one on another, and variously inclined and broken. The German geologist distinguished his strata

for the most part by their mineralogical character; the other two, by the remains of animals and plants which the rocks contained. After a beginning had thus been made in giving a genuine scientific form to phenomenal geology, other steps followed in rapid succession, as has already been related in the History*. The Classification of the Strata was fixed by a suitable Nomenclature. Attempts were made to apply to other countries the order of strata which had been found to prevail in that first studied: and in this manner it was ascertained what rocks in distant regions are the synonyms, or *Equivalents*†, of each other. The knowledge thus collected and systematized was exhibited in the form of Geological Maps.

Moreover, among the phenomena of geology we have Laws of nature as well as Classes. The general form of mountain chains; the relations of the direction and inclination of different chains to each other; the general features of mineral veins, faults, and fissures; the prevalent characters of slaty cleavage;—were the subjects of laws established, or supposed to be established, by extensive observation of facts. In like manner the organic fossils discovered in the strata were found to follow certain laws with reference to the climate which they appeared to have lived in; and the evidence which they gave of a regular zoological developement. And thus, by the assiduous labours of many accomplished and active philosophers, Descriptive or Phenomenal Geology was carried towards a state of completeness.

5. *Phenomenal Uranography.*—In like manner in other palætiological researches, as soon as they approach to an exact and scientific form, we find the necessity of constructing in the first place a science of classification and exact description, by means of which the pheno-

* *Hist. Ind. Sci.*, B. XVIII. c. iii. † *Ib.*, sect. 4.

mena may be correctly represented and compared; and of obtaining by this step a solid basis for an inquiry into the causes which have produced them. Thus the Palætiology of the solar system has, in recent times, drawn the attention of speculators; and a hypothesis has been started, that our sun and his attendant planets have been produced by the condensation of a mass of diffused matter, such as that which constitutes the nebulous patches which we observe in the starry heavens. But the sagest and most enlightened astronomers have not failed to acknowledge, that to verify or to disprove this conjecture, must be the work of many ages of observation and thought. They have perceived also that the first step of the labour requisite for the advancement of this portion of science must be to obtain and to record the most exact knowledge at present within our reach, respecting the phenomena of these nebulæ, with which we thus compare our own system; and, as a necessary element of such knowledge, they have seen the importance of a classification of these objects, and of others, such as Double Stars, of the same kind. Sir William Herschel, who first perceived the bearing of the phenomena of nebulæ upon the history of the solar system, made the observation of such objects his business, with truly admirable zeal and skill; and in the account of the results of his labours, gave a classification of Nebulæ; separating them into, first, *Clusters of Stars;* second, *Resolvable Nebulæ;* third, *Proper Nebulæ;* fourth, *Planetary Nebulæ;* fifth, *Stellar Nebulæ*; sixth, *Nebulous Stars**. And since, in order to obtain from these remote appearances, any probable knowledge respecting our own system, we must discover whether they undergo any changes in the course of ages, he devoted himself to the

* *Phil. Trans.*, 1786 and 1789, and Sir J. Herschel's *Astronomy*, Art. 616.

task of forming a record of their number and appearance in his own time, that thus the astronomers of succeeding generations might have a definite and exact standard with which to compare their observations. Still, this task would have been executed only for that part of the heavens which is visible in this country, if this Hipparchus of the Nebulæ and Double Stars had not left behind him a son who inherited all his father's zeal and more than his father's knowledge. Sir John Herschel in 1833 went to the Cape of Good Hope to complete what Sir William Herschel left wanting; and in the course of five years observed with care all the nebulæ and double stars of the Southern hemisphere. This great *Herschelian Survey of the Heavens*, the completion of which is the noblest monument ever erected by a son to a father, must necessarily be, to all ages, the basis of all speculations concerning the history and origin of the solar system; and has completed, so far as at present it can be completed, the phenomenal portion of Astronomical Palætiology.

6. *Phenomenal Geography of Plants and Animals.*—Again, there is another Palætiological Science, closely connected with the speculations forced upon the geologist by the organic fossils which he discovers imbedded in the strata of the earth;—namely, the Science which has for its object the Causes of the Diffusion and Distribution of the various kinds of Plants and Animals. And the science also has for its first portion and indispensable foundation a description and classification of the existing phenomena. Such portions of science have recently been cultivated with great zeal and success, under the titles of the *Geography of Plants*, and the *Geography of Animals*. And the results of the inquiries thus undertaken have assumed a definite and scientific form by leading to a division of the earth's surface into a certain number of

botanical and zoological *Provinces*, each province occupied by its own peculiar vegetable and animal population. We find, too, in the course of these investigations, various general laws of the phenomena offered to our notice; such, for instance, as this:—that the difference of the animals originally occupying each province, which is clear and entire for the higher orders of animals and plants, becomes more doubtful and indistinct when we descend to the lower kinds of organizations; as Infusoria and Zoophytes* in the animal kingdom, Grasses and Mosses among vegetables. Again, other laws discovered by those who have studied the geography of plants are these:—that countries separated from each other by wide tracts of sea, as the opposite shores of the Mediterranean, the islands of the Indian and Pacific Oceans, have usually much that is common in their vegetation: —and again, that in parallel climates, analogous tribes replace each other. It would be easy to adduce other laws, but those already stated may serve to show the great extent of the portions of knowledge which have just been mentioned, even considered as merely Sciences of Phenomena.

7. *Phenomenal Glossology.*—It is not my purpose in the present work to borrow my leading illutsrations from any portions of knowledge but those which are concerned with the study of material nature; and I shall, therefore, not dwell upon a branch of research, singularly interesting, and closely connected with the one just mentioned, but dealing with relations of thought rather than of things;—I mean the Palætiology of Language;—the theory, so far as the facts enable us to form a theory, of the causes which have led to the resemblances and differences of human speech in various regions and various

* Prichard, *Researches into the Physical History of Mankind*, I. 55, 28.

ages. This, indeed, would be only a portion of the study of the history and origin of the diffusion of animals, if we were to include *man* among the animals whose dispersion we thus investigate; for language is one of the most clear and imperishable records of the early events in the career of the human race. But the peculiar nature of the faculty of speech, and the ideas which the use of it involves, make it proper to treat *Glossology* as a distinct science. And of this science, the first part must necessarily be, as in the other sciences of this order, a classification and comparison of languages governed in many respects by the same rules, and presenting the same difficulties, as other sciences of classification. Such, accordingly, has been the procedure of the most philosophical glossologists. They have been led to throw the languages of the earth into certain large classes or *Families*, according to various kinds of resemblance; as the *Semitic* Family, to which belong Hebrew, Arabic, Chaldean, Syrian, Phœnician, Ethiopian, and the like; the *Indo-European*, which includes Sanskrit, Persian, Greek, Latin, and German; the *Monosyllabic* languages, Chinese, Tibetan, Birman, Siamese; the *Polysynthetic* languages, a class including most of the North-American Indian dialects; and others. And this work of classification has been the result of the labour and study of many very profound linguists, and has advanced gradually from step to step. Thus the Indo-European Family was first formed on an observation of the coincidences between Sanskrit, Greek, and Latin; but it was soon found to include the Teutonic languages, and more recently Dr. Prichard* has shown beyond doubt that the Celtic must be included in the same Family. Other general resemblances and differences of languages have been marked by appro-

* Dr. Prichard, *On the Eastern Origin of the Celtic Nations.* 1831.

priate terms: thus August von Schlegel has denominated them *synthetical* and *analytical*, according as they form their conjugations and declensions by auxiliary verbs and prepositions, or by changes in the word itself: and the *polysynthetic* languages are so named by M. Duponceau, in consequence of their still more complex mode of inflexion. Nor are there wanting, in this science also, general laws of phenomena; such, for instance, is the curious rule of the interchange of consonants in the cognate words of Greek, Gothic, and German, which has been discovered by James Grimm. All these remarkable portions of knowledge, and the great works which have appeared on Glossology, such, for example, as the *Mithridates* of Adelung and Vater, contain, for their largest, and hitherto probably their most valuable part, the phenomenal portion of the science, the comparison of languages as they now are. And beyond all doubt, until we have brought this comparative philology to a considerable degree of completeness, all our speculations respecting the causes which have operated to produce the languages of the earth must be idle and unsubstantial dreams.

Thus in all Palætiological Sciences, in all attempts to trace back the history and discover the origin of the present state of things, the portion of the science which must first be formed is that which classifies the phenomena, and discovers general laws prevailing among them. When this work is performed, and not till then, we may begin to speculate successfully concerning causes, and to make some progress in our attempts to go back to an origin. We must have a *Phenomenal* science preparatory to each *Ætiological* one.

8. *The Study of Phenomena leads to Theory.*—As we have just said, we cannot, in any subject, speculate successfully concerning the causes of the present state of

things, till we have obtained a tolerably complete and systematic view of the phenomena. Yet in reality men have not in any instance waited for this completeness and system in their knowledge of facts before they have begun to form theories. Nor was it natural, considering the speculative propensities of the human mind, and how incessantly it is endeavouring to apply the Idea of Cause, that it should thus restrain itself. I have already noticed this in the History of Geology. "While we have been giving an account," it is there said, "of the objects with which Descriptive Geology is occupied, it must have been felt how difficult it is, in contemplating such facts, to confine ourselves to description and classification. Conjectures and reasonings respecting the causes of the phenomena force themselves upon us at every step; and even influence our classification and nomenclature. Our Descriptive Geology impels us to construct a Physical Geology." And the same is the case with regard to the other subjects which I have mentioned. The mere consideration of the different degrees of condensation of different nebulæ led Herschel and Laplace to contemplate the hypothesis that our solar system is a condensed nebula. Immediately upon the division of the earth's surface into botanical and zoological provinces, and even at an earlier period, the opposite hypotheses of the origin of all the animals of each kind from a single pair, and of their original diffusion all over the earth, were under discussion. And the consideration of the families of languages irresistibly led to speculations concerning the families of the earliest human inhabitants of the earth. In all cases the contemplation of a very few phenomena, the discovery of a very few steps in the history, made men wish for and attempt to form a theory of the history from the very beginning of things.

9. *No sound Theory without Ætiology.*—But though

man is thus impelled by the natural propensities of his intellect to trace each order of things to its causes, he does not at first discern the only sure way of obtaining such knowledge: he does not suspect how much labour and how much method are requisite for success in this undertaking: he is not aware that for each order of phenomena he must construct, by the accumulated results of multiplied observation and distinct thought, a separate Ætiology. Thus, as I have elsewhere remarked*, when men had for the first time become acquainted with some of the leading phenomena of Geology, and had proceeded to speculate concerning the past changes and revolutions by which such results had been produced, they forthwith supposed themselves able to judge what would be the effects of any of the obvious agents of change, as water or volcanic fire. It did not at first occur to them to suspect that their common and extemporaneous judgment on such points was by no means sufficient for sound knowledge. They did not foresee that, before they could determine what share these or any other causes had had in producing the present condition of the earth, they must create a special science whose object should be to estimate the general laws and effects of such assumed causes;—that before they could obtain any sound Geological Theory, they must carefull cultivate Geological Ætiology.

The same disposition to proceed immediately from the facts to the theory, without constructing, as an intermediate step, a science of Causes, might be pointed out in the other sciences of this order. But in all of them this errour has been corrected by the failures to which it led. It soon appeared, for instance, that a more careful inquiry into the effects which climate, food, habit and circumstances can produce in animals, was requisite

* *Hist. Ind. Sci.*, B. XVIII. c. V. sect. 1.

in order to determine how the diversities of animals in different countries have originated. The Ætiology of Animal Life (if we may be allowed to give this name to that study of such causes of change which is at present so zealously cultivated, and which yet has no distinctive designation, except so far as it coincides with the *Organic Geological Dynamics* of our *History*), is now perceived to be a necessary portion of all attempts to construct a history of the earth and its inhabitants.

10. *Cause, in Palætiology.*—We are thus led to contemplate a class of sciences which are commenced with the study of Causes. We have already considered sciences which depended mainly upon the Idea of Cause, namely, the Mechanical Sciences. But it is obvious that the Idea of Cause in the researches now under our consideration must be employed in a very different way from that in which we applied it formerly. Force is the *cause* of motion, because force at all times and under all circumstances, if not counteracted, produces motion; but the cause of the present condition and elevation of the Alps, whatever it was, was manifested in a series of events of which each happened but once, and occupied its proper place in the series of time. The former is *mechanical*, the latter *historical, cause*. In our present investigations, we consider the events which we contemplate, of whatever order they be, as forming a chain which is extended from the beginning of things down to the present time; and the causes of which we now speak are those which connect the successive links of this chain. Every occurrence which has taken place in the history of the solar system, or the earth, or its vegetable and animal creation, or man, has been at the same time effect and cause;—the effect of what preceded, the cause of what succeeded. By being effect and cause, it has occupied some certain portion of time; and the times

which have thus been occupied by effects and causes, summed up and taken altogether, make up the total of Past Time. The Past has been a series of events connected by this historical causation, and the Present is the last term of this series. The problem in the Palætiological Sciences, with which we are here concerned, is, to determine the manner in which each term is derived from the preceding, and thus, if possible, to calculate backwards to the origin of the series.

11. *Various kinds of Cause.*—Those modes by which one term in the natural series of events is derived from another,—the forms of historical causation,—the kinds of connexion between the links of the infinite chain of time,—are very various; nor need we attempt to enumerate them. But these kinds of causation being distinguished from each other, and separately studied, each becomes the subject of a separate Ætiology. Thus the causes of change in the earth's surface, residing in the elements, fire and water, form the main subject of Geological Ætiology. The Ætiology of the vegetable and animal kingdoms investigates the causes by which the forms and distribution of species of plants and animals are affected. The study of causes in Glossology leads to an Ætiology of Language, which shall distinguish, analyze, and estimate the causes by which certain changes are produced in the languages of nations; in like manner we may expect to have an Ætiology of Art, which shall scrutinize the influences by which the various forms of art have each given birth to its successor: by which, for example, there have been brought into being those various forms of architecture which we term Egyptian, Doric, Ionic, Roman, Byzantine, Romanesque, Gothic, Italian, Elizabethan. It is easily seen by this slight survey how manifold and diverse are the kinds of cause which the Palætiological Sciences bring under our consideration.

But in each of those sciences we shall obtain solid and complete systems of knowledge, only so far as we study, with steady thought and careful observation, that peculiar kind of cause which is appropriate to the phenomena under our consideration.

12. *Hypothetical Order of Palætiological Causes.*—The various kinds of historical cause are not only connected with each other by their common bearing upon the historical sciences, but they form a kind of progression which we may represent to ourselves as having acted *in succession* in the hypothetical history of the earth and its inhabitants. Thus assuming, merely as a momentary hypothesis, the origin of the solar system by the condensation of a nebula, we have to contemplate, first, the causes by which the luminous incandescent diffused mass of which a nebula is supposed to be constituted, is gradually condensed, cooled, collected into definite masses, solidified, and each portion made to revolve about its axis, and the whole to travel about another body. We have no difficulty in ascribing the globular form of each mass to the mutual attraction of its particles: but when this form was once assumed, and covered with a solid crust, are there, we may ask, in the constitution of such a body, any causes at work by which the crust might be again broken up and portions of it displaced, and covered with other matter? Again, if we can thus explain the origin of the earth, can we with like success account for the presence of the atmosphere and the waters of earth and ocean? Supposing this done, we have then to consider by what causes such a body could become stocked with vegetable and animal life; for there have not been wanting persons, extravagant speculators, no doubt, who have conceived that even this event in the history of the world might be the work of natural causes. Supposing an origin given to

life upon our earth, we have then, brought before us by geological observations, a series of different forms of vegetable and animal existence; occurring in different strata, and, as the phenomena appear irresistibly to prove, existing at successive periods: and we are compelled to inquire what can have been the causes by which the forms of each period have passed into those of the next. We find, too, that strata, which must have been at first horizontal and continuous, have undergone enormous dislocations and ruptures, and we have to consider the possible effect of aqueous and volcanic causes to produce such changes in the earth's crust. We are thus led to the causes which have produced the present state of things on the earth; and these are causes to which we may hypothetically ascribe, not only the form and position of the inert materials of the earth, but also the nature and distribution of its animal and vegetable population. Man too, no less than other animals, is affected by the operation of such causes as we have referred to, and must, therefore, be included in such speculations. But man's history only begins, where that of other animals ends, with his mere existence. They are stationary, he is progressive. Other species of animals, once brought into being, continue the same through all ages; man is changing, from age to age, his language, his thoughts, his works. Yet even these changes are bound together by laws of causation; and these causes too may become objects of scientific study. And such causes, though not to be dwelt upon now, since we permit ourselves to found our philosophy upon the material sciences only, must still, when treated scientifically, fall within the principles of our philosophy, and must be governed by the same general rules to which all science is subject. And thus we are led by a close and natural connexion, through a series of causes, extending from those which regulate the imperceptible changes of the remotest nebulæ in the heavens, to those which determine the diversities of language, the mutations of art, and even the progress of civilization, polity, and literature.

While I have been speaking of this supposed series of events, including in its course the formation of the earth, the introduction of animal and vegetable life, and the revolutions by which one collection of species has succeeded another, it must not be forgotten, that though I have thus hypothetically spoken of these events as occurring by force of natural causes, this has been done only that the true efficacy of such causes might be brought under our consideration and made the subject of scientific examination. It may be found, that such occurrences as these are quite inexplicable by the aid of any natural causes with which we are acquainted; and thus, the result of our investigations, conducted with strict regard to scientific principles, may be, that we must either contemplate supernatural influences as part of the past series of events, or declare ourselves altogether unable to form this series into a connected chain.

13. *Mode of Cultivating Ætiology:—In Geology.*— In what manner, it may be asked, is Ætiology, with regard to each subject such as we have enumerated, to be cultivated? In order to answer this question, we must, according to our method of proceeding, take the most successful and complete examples which we possess of such portions of science. But in truth, we can as yet refer to few examples of this kind. In Geology, it is only very recently, and principally through the example and influence of Mr. Lyell, that the Ætiology has been detached from the descriptive portion of the science; and cultivated with direct attention: in other sciences the separation has hardly yet been made. But if we

examine what has already been done in Geological Ætiology, or as in the *History* it is termed, Geological Dynamics, we shall find a number of different kinds of investigation which, by the aid of our general principles respecting the formation of sciences, may suffice to supply very useful suggestions for Ætiology in general.

In Geological Ætiology, causes have been studied, in many instances, by attending to their action in the phenomena of the present state of things, and by inferring from this the nature and extent of the action which they may have exercised in former times. This has been done, for example, by Von Hoff, Mr. Lyell, and others, with regard to the operations of rivers, seas, springs, glaciers, and other aqueous causes of change. Again, the same course has been followed by the same philosophers with respect to volcanoes, earthquakes, and other violent agents. Mr. Lyell has attempted to show, too, that there take place, in our own time, not only violent agitations, but slow motions of parts of the earth's crust, of the same kind and order with those which have assisted in producing all anterior changes.

But while we thus seek instruction in the phenomena of the present state of things, we are led to the question, What are the limits of this "present" period? For instance, among the currents of lava which we trace as part of the shores of Italy and Sicily, *which* shall we select as belonging to the existing order of things? In going backwards in time, where shall we draw the line? and why at such particular point? These questions are important, for our estimate of the efficacy of known causes will vary with the extent of the effects which we ascribe to them. Hence the mode in which we group together rocks is not only a step in geological classification, but is also important to Ætiology. Thus when the vast masses of trap rocks in the Western Isles of Scotland and in other countries, which had been maintained by the Wernerians to be of aqueous origin, were, principally by the sagacity and industry of Macculloch, identified as to their nature with the products of recent volcanoes, the amount of effect which might justifiably be ascribed to volcanic agency was materially extended.

In other cases, instead of observing the current effects of our geological causes, we have to estimate the results from what we know of the causes themselves; as when, with Herschel, we calculate the alterations in the temperature of the earth which astronomical changes may possibly produce; or when, with Fourier, we try to calculate the rate of cooling of the earth's surface, on the hypothesis of an incandescent central mass. In other cases, again, we are not able to calculate the effects of our causes rigorously, but estimate them as well as we can, partly by physical reasonings, and partly by comparison with such analogous cases as we can find in the present state of things. Thus Mr. Lyell infers the change of climate which would result if land were transferred from the neighbourhood of the poles to that of the equator, by reasonings on the power of land and water to contain and communicate heat, supported by a reference to the different actual climates of places, lying under the same latitude, but under different conditions as to the distribution of land and water.

Thus our Ætiology is constructed partly from calculation and reasoning, partly from phenomena. But we may observe that when we reason from phenomena to causes, we usually do so by various steps; often ascending from phenomena to mere laws of phenomena, before we can venture to connect the phenomenon confidently with its cause. Thus the law of subterranean heat, that it increases in descending below the surface, is now well established, although the doctrine which ascribes this effect to a central heat is not universally assented to.

14.—*In the Geography of Plants and Animals.*—We may find in other subjects also, considerable contributions towards Ætiology, though not as yet a complete system of science. The Ætiology of vegetables and animals, indeed, has been studied with great zeal in modern times, as an essential preparative to geological theory; for how can we decide whether any assumed causes have produced the succession of species which we find in the earth's strata, except we know what effect of this kind given causes can produce? Accordingly, we find in Mr. Lyell's *Treatise on Geology* the most complete discussion of such questions as belong to these subjects:—for example, the question whether species can be transmuted into other species by the long continued influence of external causes, as climate, food, domestication, combined with internal causes, as habits, appetencies, progressive tendencies. We may observe, too, that as we have brought before us the inquiry what change difference of climate can produce in any species, we have also the inverse problem, how far a different developement of the species, or a different collection of species, proves a difference of climate. In the same way, the geologist of the present day considers the question, whether, in virtue of causes now in action, species are from time to time extinguished; and in like manner, the geologists of an earlier period discussed the question, now long completely decided, whether fossil species in general are really extinct species.

15. *In Languages.*—Even with reference to the Ætiology of language, although this branch of science has hardly been considered separately from the glossological investigations in which it is employed or assumed to be employed, it might perhaps be possible to point

out causes or conditions of change which, being general in their nature, must operate upon all languages alike. Changes made for the sake of euphony when words are modified and combined, occur in all dialects. Who can doubt that such changes of consonants as those by which the Greek roots become Gothic, and the Gothic, German, have for their cause some general principle in the pronunciation of each language? Again, we might attempt to decide other questions of no small interest. Have the terminations of verbs arisen from the accretion of pronouns; or, on the other hand, does the modification of a verb imply a simpler mental process than the insulation of a pronoun, as Adam Smith has maintained? Again, when the language of a nation is changed by the invasion and permanent mixture of an enemy of different speech, is it generally true that it is changed from a synthetic to an analytical structure? I will mention only one more of these wide and general glossological inquiries. Is it true, as Dr. Prichard has suggested*, that languages have become more permanent as we come down towards later times? May we justifiably suppose, with him, that in the very earliest times, nations, when they had separated from one stock, might lose all traces of this common origin out of their languages, though retaining strong evidences of it in their mythology, social forms, and arts, as appears to be the case with the ancient Egyptians and the Indians†?

Large questions of this nature cannot be treated profitably in any other way than by an assiduous study of the most varied forms of living and dead languages. But on the other hand, the study of languages should be prosecuted not only by a direct comparison of one with another, but also with a view to the formation of a science of causes and general principles, embracing such

* *Researches*, II. 221. † *Ib.*, II. 192.

discussions as I have pointed out. It is only when such a science has been formed, that we can hope to obtain any solid and certain results in the Palætiology of language; —to determine, with any degree of substantial proof, what is the real evidence which the wonderful faculty of speech, under its present developements and forms, bears to the events which have taken place in its own history, and in the history of man since his first origin.

16. *Construction of Theories.*—When we have thus obtained, with reference to any such subject as those we have here spoken of, these two portions of science, a Systematic Description of the Facts, and a rigorous Analysis of the Causes,—the *Phenomenology* and the *Ætiology* of the subject,—we are prepared for the third member which completes the science, the *Theory* of the actual facts. We can then take a view of the events which really have happened, discerning their connexion, interpreting their evidence, supplying from the context the parts which are unapparent. We can account for known facts by intelligible causes, we can infer latent facts from manifest effects, so as to obtain a distinct insight into the whole history of events up to the present time, and to see the last result of the whole in the present condition of things. The term *Theory*, when rigorously employed in such sciences as those which we here consider, bears nearly the sense which I have adopted: it implies a consistent and systematic view of the actual facts, combined with a true apprehension of their connexion and causes. Thus if we speak of "a Theory of Mount Etna," or "a Theory of the Paris Basin," we mean a connected and intelligible view of the events by which the rocks in these localities have come into their present condition. Undoubtedly the term *Theory* has often been used in a looser sense; and men have put forth "*Theories of the Earth*," which, instead of includ-

ing the whole mass of actual geological facts and their causes, only assigned, in a vague manner, some causes by which some few phenomena might, it was conceived, be accounted for. Perhaps the portion of our Palætiological Sciences which we now wish to designate, would be more generally understood if we were to describe it as *Theoretical* or *Philosophical History;* as when we talk of "the Theoretical History of Architecture," or "the Philosophical History of Language." And in the same manner we might speak of the Theoretical History of the Animal and Vegetable Kingdoms; meaning, a distinct account of the events which have produced the present distribution of species and families. But by whatever phrase we describe this portion of science, it is plain that such a Theory, such a Theoretical History, must result from the application of causes well understood to facts well ascertained. And if the term *Theory* be here employed, we must recollect that it is to be understood, not in its narrower sense as opposed to facts, but in its wider signification, as including all known facts and differing from them only in introducing among them principles of intelligible connexion. The Theories of which we now speak are true *Theories*, precisely because they are identical with the total system of the *Facts*.

17. *No sound Palætiological Theory yet extant.*—It is not to disparage the present state of science, to say that as yet no such theory exists on any subject. "Theories of the Earth" have been repeatedly published; but when we consider that even the facts of geology have been observed only on a small portion of the earth's surface, and even within those narrow bounds very imperfectly studied, we shall be able to judge how impossible it is that geologists should have yet obtained a well-established Theoretical History of the changes which have taken place in the crust of the terrestrial

globe from its first origin. Accordingly, I have ventured in my History to designate the most prominent of the Theories which have hitherto prevailed as *premature* geological theories*: and we shall soon see that geological theory has not advanced beyond a few conjectures, and that its cultivators are at present mainly occupied with a controversy in which the two extreme hypotheses which first offer themselves to men's minds are opposed to each other. And if we have no theoretical history of the earth which merits any confidence, still less have we any theoretical History of Language, or of the Arts, which we can consider as satisfactory. The Theoretical History of the Vegetable and Animal Kingdoms is closely connected with that of the earth on which they subsist, and must follow the fortunes of geology. And thus we may venture to say that no Palætiological Science, as yet, possesses all its three members. Indeed most of them are very far from having completed and systematized their Phenomenology: in all, the cultivation of Ætiology is but just begun, or is not begun; in all, the Theory must reward the exertions of future, probably of distant, generations.

But in the mean time we may derive some instruction from the comparison of the two antagonist hypotheses of which I have spoken.

CHAPTER III.

OF THE DOCTRINE OF CATASTROPHES AND THE DOCTRINE OF UNIFORMITY.

1. *Doctrine of Catastrophes.*—I HAVE already shown, in the History of Geology, that the attempts to frame a theory of the earth have brought into view two com-

* *Hist. Ind. Sci.*, B. XVIII. c. vii. sect. 3.

pletely opposite opinions:—one, which represents the course of nature as *uniform* through all ages, the causes which produce change having had the same intensity in former times which they have at the present day;—the other opinion, which sees, in the present condition of things, evidences of *catastrophes;*—changes of a more sweeping kind, and produced by more powerful agencies than those which occur in recent times. Geologists who held the latter opinion, maintained that the forces which have elevated the Alps or the Andes to their present height could not have been any forces which are now in action: they pointed to vast masses of strata hundreds of miles long, thousands of feet thick, thrown into highly-inclined positions, fractured, dislocated, crushed: they remarked that upon the shattered edges of such strata they found enormous accumulations of fragments and rubbish, rounded by the action of water, so as to denote ages of violent aqueous action: they conceived that they saw instances in which whole mountains of rock in a state of igneous fusion, must have burst the earth's crust from below: they found that in the course of the revolutions by which one stratum of rock was placed upon another, the whole collection of animal species which tenanted the earth and the seas had been removed, and a new set of living things introduced in its place: finally, they found, above all the strata, vast masses of sand and gravel containing bones of animals, and apparently the work of a mighty deluge. With all these proofs before their eyes, they thought it impossible not to judge that the agents of change by which the world was urged from one condition to another till it reached its present state must have been more violent, more powerful, than any which we see at work around us. They conceived that the evidence of "catastrophes" was irresistible.

2. *Doctrine of Uniformity.*—I need not here repeat the narrative (given in the History*) of the process by which this formidable array of proofs was, in the minds of some eminent geologists, weakened, and at last overcome. This was done by showing that the sudden breaks in the succession of strata were apparent only, the discontinuity of the series which occurred in one country being removed by terms interposed in another locality:—by urging that the total effect produced by existing causes, taking into account the accumulated result of long periods, is far greater than a casual speculator would think possible:—by making it appear that there are in many parts of the world evidences of a slow and imperceptible rising of the land since it was the habitation of now existing species:—by proving that it is not universally true that the strata separated in time by supposed catastrophes contain distinct species of animals:—by pointing out the limited fields of the supposed diluvial action:—and finally, by remarking that though the *creation* of species is a mystery, the *extinction* of species is going on in our own day. Hypotheses were suggested, too, by which it was conceived that the change of climate might be explained, which, as the consideration of the fossil remains seemed to show, must have taken place between the ancient and the modern times. In this manner the whole evidence of catastrophes was explained away: the notion of a series of paroxysms of violence in the causes of change was represented as a delusion arising from our contemplating short periods only, in the action of present causes: length of time was called in to take the place of intensity of force: and it was declared that Geology need not despair of accounting for the revolutions of the earth, as Astronomy accounts for the revolutions of the heavens, by the universal

* *Hist. Ind Sci.*, B. XVIII. c. viii. sect. 2.

action of causes which are close at hand to us, operating through time and space without variation or decay.

An antagonism of opinions, somewhat of the same kind as this, will be found to manifest itself in the other Palætiological Sciences as well as in Geology; and it will be instructive to endeavour to balance these opposite doctrines. I will mention some of the considerations which bear upon the subject in its general form.

3. *Is Uniformity probable à priori?*—The doctrine of Uniformity in the course of nature has sometimes been represented by its adherents as possessing a great degree of *à priori* probability. It is highly unphilosophical, it has been urged, to assume that the causes of the geological events of former times were of a different kind from causes now in action, if causes of this latter kind can in any way be made to explain the facts. The analogy of all other sciences compels us, it was said, to explain phenomena by known, not by unknown, causes. And on these grounds the geological teacher recommended* "an earnest and patient endeavour to reconcile the indications of former change with the evidence of gradual mutations now in progress."

But on this we may remark, that if by *known* causes we mean causes acting with the same intensity which they have had during historical times, the restriction is altogether arbitrary and groundless. Let it be granted, for instance, that many parts of the earth's surface are now undergoing an imperceptible rise. It is not pretended that the rate of this elevation is rigorously uniform; what, then, are the limits of its velocity? Why may it not increase so as to assume that character of violence which we may term a *catastrophe* with reference to all changes hitherto recorded? Why may not the rate of elevation be such that we may conceive the strata to

* Lyell, 4th Ed. B. IV. c. 1, p. 328.

assume *suddenly* a position nearly vertical? and is it, in fact, easy to conceive a position of strata nearly vertical, a position which occurs so frequently, to be *gradually* assumed? In cases where the strata are nearly vertical, as in the Isle of Wight, and hundreds of other places, or where they are actually inverted, as sometimes occurs, are not the causes which have produced the effect as truly *known* causes, as those which have raised the coasts where we trace the former beach in an elevated terrace? If the latter case proves *slow* elevation, does not the former case prove *rapid* elevation? In neither case have we any measure of the time employed in the change; but does not the very nature of the results enable us to discern, that if one was gradual, the other was comparatively sudden?

The causes which are now elevating a portion of Scandinavia can be called known *causes*, only because we know the *effect*. Are not the causes which have elevated the Alps and the Andes known causes in the same sense? We know nothing in either case which confines the intensity of the force within any limit, or prescribes to it any law of uniformity. Why, then, should we make a merit of cramping our speculations by such assumptions? Whether the causes of change do act uniformly;—whether they oscillate only within narrow limits;—whether their intensity in former times was nearly the same as it now is;—these are precisely the questions which we wish Science to answer to us impartially and truly: where is then the wisdom of "an earnest and patient endeavour" to secure an *affirmative* reply?

Thus I conceive that the assertion of an *à priori* claim to probability and philosophical spirit in favour of the doctrine of uniformity, is quite untenable. We must learn from an examination of all the facts, and not from any assumption of our own, whether the course of nature be uniform. The limit of intensity being really unknown, catastrophes are just as probable as uniformity. If a volcano may repose for a thousand years, and then break out and destroy a city; why may not another volcano repose for ten thousand years, and then destroy a continent; or if a continent, why not the whole habitable surface of the earth?

4. *Cycle of Uniformity indefinite.*—But this argument may be put in another form. When it is said that the course of nature is uniform, the assertion is not intended to exclude certain smaller variations of violence and rest, such as we have just spoken of;—alternations of activity and repose in volcanoes; or earthquakes, deluges, and storms, interposed in a more tranquil state of things. With regard to such occurrences, terrible as they appear at the time, they may not much affect the average rate of change; there may be a *cycle*, though an irregular one, of rapid and slow change; and if such cycles go on succeeding each other, we may still call the order of nature uniform, notwithstanding the periods of violence which it involves. The maximum and minimum intensities of the forces of mutation alternate with one another; and we may estimate the average course of nature as that which corresponds to something between the two extremes.

But if we thus attempt to maintain the uniformity of nature by representing it as a series of *cycles*, we find that we cannot discover, in this conception, any solid ground for excluding catastrophes. What is the length of that cycle, the repetition of which constitutes uniformity? What interval from the maximum to the minimum does it admit of? We may take for our cycle a hundred or a thousand years, but evidently such a proceeding is altogether arbitrary. We may mark our cycles by the greatest known paroxysms of volcanic and terremotive agency, but this procedure is no less indefinite and inconclusive than the other.

But further; since the cycle in which violence and repose alternate is thus indefinite in its length and in its range of activity, what ground have we for assuming more than *one* such cycle, extending from the origin of things to the present time? Why may we not suppose the maximum force of the causes of change to have taken place at the earliest period, and the tendency towards the minimum to have gone on ever since? Or instead of only one cycle, there may have been several, but of such length that our historical period forms a portion only of the last;—the feeblest portion of the latest cycle. And thus violence and repose may alternate upon a scale of time and intensity so large, that man's experience supplies no evidence enabling him to estimate the amount. The course of things is *uniform*, to an Intelligence which can embrace the succession of several cycles, but it is *catastrophic* to the contemplation of man, whose survey can grasp a part only of one cycle. And thus the hypothesis of uniformity, since it cannot exclude degrees of change, nor limit the range of these degrees, nor define the interval of their recurrence, cannot possess any essential simplicity which, previous to inquiry, gives it a claim upon our assent superior to that of the opposite catastrophic hypothesis.

5. *Uniformitarian Arguments are Negative only.*—There is an opposite tendency in the mode of maintaining the catastrophist and the uniformitarian opinions, which depends upon their fundamental principles, and shows itself in all the controversies between them. The Catastrophist is affirmative, the Uniformitarian is negative in his assertions: the former is constantly attempting to construct a theory; the latter delights in demolishing all theories. The one is constantly bringing fresh evidence

of some great past event, or series of events, of a striking and definite kind; his antagonist is at every step explaining away the evidence, and showing that it proves nothing. One geologist adduces his proofs of a vast universal deluge; but another endeavours to show that the proofs do not establish either the universality or the vastness of such an event. The inclined broken edges of a certain formation, covered with their own fragments, beneath superjacent horizontal deposits, are at one time supposed to prove a catastrophic breaking up of the earlier strata; but this opinion is controverted by showing that the same formations, when pursued into other countries, exhibit a uniform gradation from the lower to the upper, with no trace of violence. Extensive and lofty elevations of the coast, continents of igneous rock, at first appear to indicate operations far more gigantic than those which now occur; but attempts are soon made to show that time only is wanting to enable the present age to rival the past in the production of such changes. Each new fact adduced by the catastrophist is at first striking and apparently convincing; but as it becomes familiar, it strikes the imagination less powerfully; and the uniformitarian, constantly labouring to produce some imitation of it by the machinery which he has so well studied, at last in every case seems to himself to succeed, so far as to destroy the effect of his opponent's evidence.

This is so with regard to more remote, as well as with regard to immediate evidences of change. When it is ascertained that in every part of the earth's crust the temperature increases as we descend below the surface, at first this fact seems to indicate a central heat: and a central heat naturally suggests an earlier state of the mass, in which it was incandescent, and from which it is now cooling. But this original incandescence of the globe of the earth is manifestly an entire violation of the present course of things; it belongs to the catastrophist view, and the advocates of uniformity have to explain it away. Accordingly, one of them holds that this increase of heat in descending below the surface may very possibly not go on all the way to the center. The heat which increases at first as we descend, may, he conceives, afterwards decrease; and he suggests causes which may have produced such a succession of hotter and colder shells within the mass of the earth. I have mentioned this suggestion in the History of Geology; and have given my reasons for believing it altogether untenable*. Other persons also, desirous of reconciling this subterraneous heat with the tenet of uniformity, have offered another suggestion:—that the warmth or incandescence of the interior parts of the earth does not arise out of an originally hot condition from which it is gradually cooling, but results from chemical action constantly going on among the materials of the earth's substance. And thus new attempts are perpetually making, to escape from the cogency of the reasonings which send us towards an original state of things different from the present. Those who theorize concerning an origin go on building up the fabric of their speculations, while those who think such theories unphilosophical, ever and anon dig away the foundation of this structure. As we have already said, the uniformitarian's doctrines are a collection of negatives.

This is so entirely the case, that the uniformitarian would for the most part shrink from maintaining as positive tenets the explanations which he so willingly uses as instruments of controversy. He puts forward his suggestions as difficulties, but he will not stand by them as doctrines. And this is in accordance with his general tendency; for any of his hypotheses, if insisted upon as

* *Hist. Ind. Sci.*, B. XVIII. c. V. sect 5, and note.

positive theories, would be found inconsistent with the assertion of uniformity. For example, the nebular hypothesis appears to give to the history of the heavens an aspect which obliterates all special acts of creation, for, according to that hypothesis, new planetary systems are constantly forming; but when asserted as the origin of our own solar system, it brings with it an original incandescence, and an origin of the organic world. And if, instead of using the chemical theory of subterraneous heat to neutralize the evidence of original incandescence, we assert it as a positive tenet, we can no longer maintain the infinite past duration of the earth; for chemical forces, as well as mechanical, tend to equilibrium; and that condition once attained, their efficacy ceases. Chemical affinities tend to form new compounds; and though, when many and various elements are mingled together, the play of synthesis and analysis may go on for a long time, it must at last end. If, for instance, a large portion of the earth's mass were originally pure potassium, we can imagine violent igneous action to go on so long as any part remained unoxidized; but when the oxidation of the whole has once taken place, this action must be at an end; for there is in the hypothesis no agency which can reproduce the deoxidized metal. Thus a perpetual motion is impossible in chemistry, as it is in mechanics; and a theory of constant change continued through infinite time, is untenable when asserted upon chemical, no less than upon mechanical principles. And thus the Skepticism of the uniformitarian is of force only so long as it is employed against the Dogmatism of the catastrophist. When the Doubts are erected into Dogmas, they are no longer consistent with the tenet of Uniformity. When the Negations become Affirmations, the Negation of an Origin vanishes also.

6. *Uniformity in the Organic World.*—In speaking

of the violent and sudden changes which constitute catastrophes, our thoughts naturally turn at first to great *mechanical* and *physical* effects;—ruptures and displacements of strata; extensive submersions and emersions of land; rapid changes of temperature. But the catastrophes which we have to consider in geology affect the *organic* as well as the inorganic world. The sudden extinction of one collection of species, and the introduction of another in their place, is a catastrophe, even if unaccompanied by mechanical violence. Accordingly, the antagonism of the catastrophist and uniformitarian school has shown itself in this department of the subject, as well as in the other. When geologists had first discovered that the successive strata are each distinguished by appropriate organic fossils, they assumed at once that each of these collections of living things belonged to a separate creation. But this conclusion, as I have already said, Mr. Lyell has attempted to invalidate, by proving that in the existing order of things, some species become extinct; and by suggesting it as possible, that in the same order it may be true that new species are from time to time produced, even in the present course of nature. And in this, as in the other part of the subject, he calls in the aid of vast periods of time, in order that the violence of the changes may be softened down: and he appears disposed to believe that the actual extinction and creation of species may be so slow as to excite no more notice than it has hitherto obtained; and yet may be rapid enough, considering the immensity of geological periods, to produce such a succession of different collections of species as we find in the strata of the earth's surface.

7. *Origin of the present Organic World.*—The last great event in the history of the vegetable and animal kingdoms was that by which their various tribes were placed in their present seats. And we may form various

hypotheses with regard to the sudden or gradual manner in which we may suppose this distribution to have taken place. We may assume that at the beginning of the present order of things, a stock of each species was placed in the vegetable or animal *province* to which it belongs, by some cause out of the common order of nature; or we may take a uniformitarian view of the subject, and suppose that the provinces of the organic world derived their population from some anterior state of things by the operation of natural causes.

Nothing has been pointed out in the existing order of things which has any analogy or resemblance, of any valid kind, to that creative energy which must be exerted in the production of a new species. And to assume the introduction of new species as "a part of the order of nature," without pointing out any natural fact with which such an event can be classed, would be to reject creation, by an arbitrary act. Hence, even on natural grounds, the most intelligible view of the history of the animal and vegetable kingdoms seems to be, that each period which is marked by a distinct collection of species forms a cycle; and that at the beginning of each such cycle a creative power was exerted, of a kind to which there was nothing at all analogous in the succeeding part of the same cycle. If it be urged that in some cases the same species, or the same genus, runs through two geological formations, which must, on other grounds, be referred to different cycles of creative energy, we may reply that the creation of many new species does not imply the extinction of all the old ones.

Thus we are led by our reasonings to this view, that the present order of things was commenced by an act of creative power entirely different to any agency which has been exerted since. None of the influences which have modified the present races of animals and plants since they were placed in their habitations on the earth's surface can have had any efficacy in producing them at first. We are necessarily driven to assume, as the beginning of the present cycle of organic nature, an event not included in the course of nature. And we may remark that this necessity is the more cogent, precisely because other cycles have preceded the present.

8. *Nebular Origin of the Solar System.*—If we attempt to apply the same antithesis of opinion (the doctrines of Catastrophe and Uniformity,) to the other subjects of palætiological sciences, we shall be led to similar conclusions. Thus, if we turn our attention to astronomical palætiology, we perceive that the nebular hypothesis has a uniformitarian tendency. According to this hypothesis the formation of this our system of sun, planets, and satellites, was a process of the same kind as those which are still going on in the heavens. One after another, nebulæ condense into separate masses, which begin to revolve about each other by mechanical necessity, and form systems of which our solar system is a finished example. But we may remark, that the uniformitarian doctrine on this subject rests on most unstable foundations. We have as yet only very vague and imperfect reasonings to show that by such condensation a *material* system such as ours could result; and the introduction of *organized* beings into such a material system is utterly out of the reach of our philosophy. Here again, therefore, we are led to regard the present order of the world as pointing towards an origin altogether of a different kind from anything which our material science can grasp.

9. *Origin of Languages.*—We may venture to say that we should be led to the same conclusion once more, if we were to take into our consideration those palætiological sciences which are beyond the domain of matter;

for instance, the history of languages. We may explain many of the differences and changes which we become acquainted with, by referring to the action of causes of change which still operate. But what glossologist will venture to declare that the efficacy of such causes has been uniform;—that the influences which mould a language, or make one language differ from others of the same stock, operated formerly with no more efficacy than they exercise now. "Where," as has elsewhere been asked, "do we now find a language in the process of formation, unfolding itself in inflexions, terminations, changes of vowels by grammatical relations, such as characterize the oldest known languages?" Again, as another proof how little the history of languages suggests to the philosophical glossologist the persuasion of a uniform action of the causes of change, I may refer to the conjecture of Dr. Prichard, that the varieties of language produced by the separation of one stock into several, have been greater and greater as we go backwards in history:—that* the formation of sister dialects from a common language, (as the Scandinavian, German, and Saxon dialects from the Teutonic, or the Gaelic, Erse and Welsh from the Celtic,) belongs to the first millennium before the Christian era; while the formation of cognate languages of the same family, as the Sanskrit, Latin, Greek and Gothic, must be placed at least two thousand years before that era; and at a still earlier period took place the separation of the great families themselves, the Indo-European, Semitic, and others, in which it is now difficult to trace the features of a common origin. No hypothesis except one of this kind will explain the existence of the families, groups, and dialects of languages, which we find in existence. Yet this is an entirely different view from that which

* *Researches*, II. 224.

the hypothesis of the uniform progress of change would give. And thus, in the earliest stages of man's career, the revolutions of language must have been, even by the evidence of the theoretical history of language itself, of an order altogether different from any which have taken place within the recent history of man. And we may add, that as the early stages of the progress of language must have widely different from those later ones of which we can in some measure trace the natural causes, we cannot place the origin of language in any point of view in which it comes under the jurisdiction of natural causation at all.

10. *No Natural Origin discoverable.*—We are thus led by a survey of several of the palætiological sciences to a confirmation of the principle formerly asserted*, That in no palætiological science has man been able to arrive at a beginning which is homogeneous with the known course of events. We can in such sciences often go very far back;—determine many of the remote circumstances of the past series of events;—ascend to a point which seems to be near the origin;—and limit the hypotheses respecting the origin itself: but philosophers never have demonstrated, and, so far as we can judge, probably never will be able to demonstrate, what was that primitive state of things from which the progressive course of the world took its first departure. In all these paths of research, when we travel far backwards, the aspect of the earlier portions becomes very different from that of the advanced part on which we now stand; but in all cases the path is lost in obscurity as it is traced backwards towards its starting point: it becomes not only invisible, but unimaginable; it is not only an interruption, but an abyss, which interposes itself between us and any intelligible beginning of things.

* *Hist. Ind. Sci.*, B. XVIII. c. vi. sect. 5.

CHAPTER IV.

OF THE RELATION OF TRADITION TO PALÆTIOLOGY.

1. *Importance of Tradition.*—SINCE the Palætiological Sciences have it for their business to study the train of past events produced by natural causes down to the present time, the knowledge concerning such events which is supplied by the remembrance and records of man, in whatever form, must have an important bearing upon these sciences. All changes in the condition and extent of land and sea, which have taken place within man's observation, all effects of deluges, sea-waves, rivers, springs, volcanoes, earthquakes, and the like, which come within the reach of human history, have a strong interest for the palætiologist. Nor is he less concerned in all recorded instances of the modification of the forms and habits of plants and animals, by the operations of man, or by transfer from one land to another. And when we come to the Palætiology of Language, of Art, of Civilization, we find our subject still more closely connected with history; for in truth these are historical, no less than palætiological investigations. But, confining ourselves at present to the material sciences, we may observe that though the importance of the information which tradition gives us, in the sciences now under our consideration, as, for instance, geology, has long been tacitly recognized; yet it is only recently that geologists have employed themselves in collecting their historical facts upon such a scale and with such comprehensive views as are required by the interest and use of collections of this kind. The Essay of Von Hoff*, *On the Natural Alterations in the Surface of the Earth*

* Vol. I., 1822; Vol. II., 1824.

which are proved by Tradition, was the work which first opened the eyes of geologists to the extent and importance of this kind of investigation. Since that time the same path of research has been pursued with great perseverance by others, especially by Mr. Lyell; and is now justly considered as an essential portion of Geology.

2. *Connexion of Tradition and Science.*—Events which we might naturally expect to have some bearing on geology, are narrated in the historical writings which, even on mere human grounds, have the strongest claim to our respect as records of the early history of the world, and are confirmed by the traditions of various nations all over the globe; namely, the formation of the earth and of its population, and a subsequent deluge. It has been made a matter of controversy how the narrative of these events is to be understood, so as to make it agree with the facts which an examination of the earth's surface and of its vegetable and animal population discloses to us. Such controversies, when they are considered as merely archæological, may occur in any of the palætiological sciences. We may have to compare and to reconcile the evidence of existing phenomena with that of historical tradition. But under some circumstances this process of conciliation may assume an interest of another kind, on which we will make a few remarks.

3. *Natural and Providential History of the World.*—We may contemplate the existence of man upon the earth, his origin and his progress, in the same manner as we contemplate the existence of any other race of animals; namely, in a purely palætiological view. We may consider how far our knowledge of laws of causation enables us to explain his diffusion and migration, his differences and resemblances, his actions and works.

And this is the view of man as a member of the *Natural* Course of Things.

But man, at the same time the contemplator and the subject of his own contemplation, endowed with faculties and powers which make him a being of a different nature from other animals, cannot help regarding his own actions and enjoyments, his recollections and his hopes, under an aspect quite different from any that we have yet had presented to us. We have been endeavouring to place in a clear light the Fundamental Ideas, such as that of Cause, on which depends our knowledge of the natural course of things. But there are other Ideas to which man necessarily refers his actions; he is led by his nature, not only to consider his own actions, and those of his fellow-men, as springing out of this or that cause, leading to this or that material result; but also as good or bad, as what they ought or ought not to be. He has Ideas of *moral* relations as well as those Ideas of material relations with which we have hitherto been occupied. He is a moral as well as a natural agent.

Contemplating himself and the world around him by the light of his Moral Ideas, man is led to the conviction that his moral faculties were bestowed upon him by design and for a purpose; that he is the subject of a Moral Government; that the course of the world is directed by the Power which governs it, to the unfolding and perfecting of man's moral nature; that this guidance may be traced in the career of individuals and of the world; that there is a *Providential* as well as a Natural Course of Things.

Yet this view is beset by no small difficulties. The full developement of man's moral faculties;—the perfection of his nature up to the measure of his own ideas;—the adaptation of his moral being to an ultimate destination, by its transit through a world full of moral evil, in which evil each person has his share;—are effects for which the economy of the world appears to contain no adequate provision. Man, though aware of his moral nature, and ready to believe in an ultimate destination of purity and blessedness, is too feeble to resist the temptation of evil, and too helpless to restore his purity when once lost. He cannot but look for some confirmation of that providential order which he has begun to believe; some provision for those deficiencies in his moral condition which he has begun to feel.

He looks at the history of the world, and he finds that at a certain period it offers to him the promise of what he seeks. When the natural powers of man had been developed to their full extent, and were beginning to exhibit symptoms of decay;—when the intellectual progress of the world appeared to have reached its limit, without supplying man's moral needs;—we find the great Epoch in the Providential History of the world. We find the announcement of a Dispensation by which man's deficiencies shall be supplied and his aspirations fulfilled: we find a provision for the purification, the support, and the ultimate beatification of those who use the provided means. And thus the providential course of the world becomes consistent and intelligible.

4. *The Sacred Narrative.*—But with the new Dispensation, we receive, not only an account of its own scheme and history, but also a written narrative of the providential course of the world from the earliest times, and even from its first creation. This narrative is recognized and authorized by the new dispensation, and accredited by some of the same evidences as the dispensation itself. That the existence of such a sacred narrative should be a part of the providential order of things, cannot but

appear natural; but, naturally also, the study of it leads to some difficulties.

The Sacred Narrative in some of its earliest portions speaks of natural objects and occurrences respecting them. In the very beginning of the course of the world, we may readily believe (indeed, as we have seen in the last chapter, our scientific researches lead us to believe) that such occurrences were very different from anything which now takes place;—different to an extent and in a manner which we cannot estimate. Now the narrative must speak of objects and occurrences in the words and phrases which have derived their meaning from their application to the existing natural state of things. When applied to an initial supernatural state therefore, these words and phrases cannot help being to us obscure and mysterious, perhaps ambiguous and seemingly contradictory.

5. *Difficulties in interpreting the Sacred Narrative.*—The moral and providential relations of man's condition are so much more important to him than mere natural relations, that at first we may well suppose he will accept the Sacred Narrative, as not only unquestionable in its true import, but also as a guide in his views even of mere natural relations. He will try to modify the conceptions which he entertains of objects and their properties, so that the Sacred Narrative of the supernatural condition shall retain the first meaning which he had put upon it in virtue of his own habits in the usage of language.

But man is so constituted that he cannot persist in this procedure. The powers and tendencies of his intellect are such that he cannot help trying to attain true conceptions of objects and their properties by the study of things themselves. For instance, when he at first read of a firmament dividing the waters above from the waters below, he perhaps conceived a transparent floor in the skies, on which the superior waters rested, which descend in rain; but as his observations and his reasonings satisfied him that such a floor could not exist, he became willing to allow (as St. Augustine allowed) that the waters above the firmament are in a state of vapour. And in like manner in other subjects, men, as their views of nature became more distinct and precise, modified, so far as it was necessary for consistency's sake, their first rude interpretations of the Sacred Narrative; so that, without in any degree losing its import as a view of the providential course of the world, it should be so conceived as not to contradict what they knew of the natural order of things.

But this accommodation was not always made without painful struggles and angry controversies. When men had conceived the occurrences of the Sacred Narrative in a particular manner, they could not readily and willingly adopt a new mode of conception; and all attempts to recommend to them such novelties, they resisted as attacks upon the sacredness of the Narrative. They had clothed their belief of the workings of Providence in certain images; and they clung to those images with the persuasion that, without them, their belief could not subsist. Thus they imagined to themselves that the earth was a flat floor, solidly and broadly laid for the convenience of man; and they felt as if the kindness of Providence was disparaged, when it was maintained that the earth was a globe held together only by the mutual attraction of its parts.

The most memorable instance of a struggle of this kind is to be found in the circumstances which attended the introduction of the Heliocentric Theory of Copernicus to general acceptance. On this controversy I have already made some remarks in the *History of Science**, and have attempted to draw from it some lessons which may be useful to us when any similar conflict of opinions may occur. I will here add a few reflections with a similar view.

6. *Such difficulties inevitable.*—In the first place, I remark that such modifications of the current interpretation of the words of Scripture appear to be an inevitable consequence of the progressive character of Natural Science. Science is constantly teaching us to describe known facts in new language; but the language of Scripture is always the same. And not only so, but the language of Scripture is necessarily adapted to the common state of man's intellectual developement, in which he is supposed not to be possessed of science. Hence the phrases used by Scripture are precisely those which science soon teaches man to consider as inaccurate. Yet they are not, on that account, the less fitted for their proper purpose: for if any terms had been used, adapted to a more advanced state of knowledge, they must have been unintelligible among those to whom the Scripture was first addressed. If the Jews had been told that water existed in the clouds in small drops, they would have marvelled that it did not constantly descend; and to have explained the reason of this, would have been to teach Atmology in the sacred writings. If they had read in their Scripture that the earth was a sphere, when it appeared to be a plain, they would only have been disturbed in their thoughts or driven to some wild and baseless imaginations, by a declaration to them so strange. If the Divine Speaker, instead of saying that he would set his bow in the clouds, had been made to declare that he would give to water the property of refracting different colours at different angles, how

* B. v. c. iii. sect. 4.

utterly unmeaning to the hearers would the words have been! And in these cases, the expressions, being unintelligible, startling, and bewildering, would have been such as tended to unfit the Sacred Narrative for its place in the providential dispensation of the world.

Accordingly, in the great controversy which took place in Galileo's time between the defenders of the then customary interpretations of Scripture, and the assertors of the Copernican system of the universe, when the innovators were upbraided with maintaining opinions contrary to Scripture, they replied that Scripture was not intended to teach men astronomy, and that it expressed the acts of divine power in images which were suited to the ideas of unscientific men. To speak of the rising and setting and travelling of the sun, of the fixity and of the foundations of the earth, was to use the only language which would have made the Sacred Narrative intelligible. To extract from these and the like expressions doctrines of science, was, they declared, in the highest degree unjustifiable; and such a course could lead, they held, to no result but a weakening of the authority of Scripture in proportion as its credit was identified with that of these modes of applying it. And this judgment has since been generally assented to by those who most reverence and value the study of the designs of Providence as well as that of the works of nature.

7. *Science tells us nothing concerning Creation.*—Other apparent difficulties arise from the accounts given in the Scripture of the first origin of the world in which we live: for example, Light is represented as created before the Sun. With regard to difficulties of this kind, it appears that we may derive some instruction from the result to which we were led in the last chapter;—namely, that in the sciences which trace the progress of natural occurrences, we can in no case go back to an origin, but

in every instance appear to find ourselves separated from it by a state of things, and an order of events, of a kind altogether different from those which come under our experience. The thread of induction respecting the natural course of the world snaps in our fingers, when we try to ascertain where its beginning is. Since, then, science can teach us nothing positive respecting the beginning of things, she can neither contradict nor confirm what is taught by Scripture on that subject; and thus, as it is unworthy timidity in the lover of Scripture to fear contradiction, so is it ungrounded presumption to look for confirmation, in such cases. The providential history of the world has its own beginning, and its own evidence; and we can only render the system insecure, by making it lean on our material sciences. If any one were to suggest that the nebular hypothesis countenances the Scripture history of the formation of this system, by showing how the luminous matter of the sun might exist previous to the sun itself, we should act wisely in rejecting such an attempt to weave together these two heterogeneous threads;—the one a part of a providential scheme, the other a fragment of a physical speculation.

We shall best learn those lessons of the true philosophy of science which it is our object to collect, by attending to portions of science which have gone through such crises as we are now considering; nor is it requisite, for this purpose, to bring forwards any subjects which are still under discussion. It may, however, be mentioned that such maxims as we are now endeavouring to establish, and the one before us in particular, bear with a peculiar force upon those Palætiological Sciences of which we have been treating in the present Book.

8. *Scientific views, when familiar, do not disturb the authority of Scripture.*—There is another reflection

which may serve to console and encourage us in the painful struggles which thus take place, between those who maintain interpretations of Scripture already prevalent and those who contend for such new ones as the new discoveries of science require. It is this;—that though the new opinion is resisted by one party as something destructive of the credit of Scripture and the reverence which is its due, yet, in fact, when the new interpretation has been generally established and incorporated with men's current thoughts, it ceases to disturb their views of the authority of the Scripture or of the truth of its teaching. When the language of Scripture, invested with its new meaning, has become familiar to men, it is found that the ideas which it calls up are quite as reconcileable as the former ones were, with the most entire acceptance of the providential dispensation. And when this has been found to be the case, all cultivated persons look back with surprise at the mistake of those who thought that the essence of the revelation was involved in their own arbitrary version of some collateral circumstance in the revealed narrative. At the present day, we can hardly conceive how reasonable men could ever have imagined that religious reflections on the stability of the earth, and the beauty and use of the luminaries which revolve round it, would be interfered with by an acknowledgment that this rest and motion are apparent only*. And thus the authority of revelation is not shaken by any changes introduced by the progress of science in the mode of interpreting expressions which describe physical objects and occurrences; provided the new interpretation is admitted at a proper season, and in a proper spirit; so as to soften, as much as possible, both the public controversies and the private scruples which almost inevitably accompany such an alteration.

* I have here borrowed a sentence or two from my own *History*.

9. *When should old Interpretations be given up?*—But the question then occurs, What is the proper season for a religious and enlightened commentator to make such a change in the current interpretation of sacred Scripture? At what period ought the established exposition of a passage to be given up, and a new mode of understanding the passage, such as is, or seems to be, required by new discoveries respecting the laws of nature, accepted in its place? It is plain, that to introduce such an alteration lightly and hastily would be a procedure fraught with inconvenience; for if the change were made in such a manner, it might be afterwards discovered that it had been adopted without sufficient reason, and that it was necessary to reinstate the old exposition. And the minds of the readers of Scripture, always to a certain extent and for a time disturbed by the subversion of their long-established notions, would be distressed without any need, and might be seriously unsettled. While, on the other hand, a too protracted and obstinate resistance to the innovation, on the part of the scriptural expositors, would tend to identify, at least in the minds of many, the authority of the Scripture with the truth of the exposition; and therefore would bring discredit upon the revealed word, when the established interpretation was finally proved to be untenable.

A rule on this subject, propounded by some of the most enlightened dignitaries of the Roman Catholic church, on the occasion of the great Copernican controversy begun by Galileo, seems well worthy of our attention. The following was the opinion given by Cardinal Bellarmine at the time:—"When a *demonstration* shall be found to establish the earth's motion, it will be proper to interpret the sacred Scriptures otherwise than they have hitherto been interpreted in those passages where mention is made of the stability of the earth and movement of the heavens." This appears to be a judicious and reasonable maxim for such cases in general. So long as the supposed scientific discovery is doubtful, the exposition of the meaning of Scripture given by commentators of established credit is not wantonly to be disturbed: but when a scientific theory, irreconcileable with this ancient interpretation, is clearly proved, we must give up the interpretation, and seek some new mode of understanding the passage in question, by means of which it may be consistent with what we know; for if it be not, our conception of the things so described is no longer consistent with itself.

It may be said that this rule is indefinite, for who shall decide when a new theory is completely demonstrated, and the old interpretation become untenable? But to this we may reply, that if the rule be assented to, its application will not be very difficult. For when men have admitted as a general rule, that the current interpretations of scriptural expressions respecting natural objects and events may possibly require, and in some cases certainly will require, to be abandoned, and new ones admitted, they will hardly allow themselves to contend for such interpretations as if they were essential parts of revelation; and will look upon the change of exposition, whether it come sooner or later, without alarm or anger. And when men lend themselves to the progress of truth, in this spirit, it is not of any material importance at what period a new and satisfactory interpretation of the scriptural difficulty is found; since a scientific exactness in our apprehension of the meaning of such passages as are now referred to is very far from being essential to our full acceptance of revelation.

10. *In what Spirit should the Change be accepted?* —Still these revolutions in scriptural interpretation must always have in them something which distresses

and disturbs religious communities. And such uneasy feelings will take a different shape, according as the community acknowledges or rejects a paramount interpretative authority in its religious leaders. In the case in which the interpretation of the Church is binding upon all its members, the more placid minds rest in peace upon the ancient exposition, till the spiritual authorities announce that the time for the adoption of a new view has arrived; but in these circumstances, the more stirring and inquisitive minds, which cannot refrain from the pursuit of new truths and exact conceptions, are led to opinions which, being contrary to those of the Church, are held to be sinful. On the other hand, if the religious constitution of the community allow and encourage each man to study and interpret for himself the Sacred Writings, we are met by evils of another kind. In this case, although, by the unforced influence of admired commentators, there may prevail a general agreement in the usual interpretation of difficult passages, yet as each reader of the Scripture looks upon the sense which he has adopted as being his own interpretation, he maintains it, not with the tranquil acquiescence of one who has deposited his judgment in the hands of his Church, but with the keenness and strenuousness of self-love. In such a state of things, though no judicial severities can be employed against the innovators, there may arise more angry controversies than in the other case.

It is impossible to overlook the lesson which here offers itself, that it is in the highest degree unwise in the friends of religion, whether individuals or communities, unnecessarily to embark their credit in expositions of Scripture on matters which appertain to natural science. By delivering physical doctrines as the teaching of revelation, religion may lose much, but cannot gain anything. This maxim of practical wisdom has often been urged by Christian writers. Thus St. Augustin says*: "In obscure matters and things far removed from our senses, if we read anything, even in the divine Scripture, which may produce diverse opinions without damaging the faith which we cherish, let us not rush headlong by positive assertion to either the one opinion or the other; lest, when a more thorough discussion has shown the opinion which we had adopted to be false, our faith may fall with it: and we should be found contending, not for the doctrine of the sacred Scriptures, but for our own; endeavouring to make our doctrine to be that of the Scriptures, instead of taking the doctrine of the Scriptures to be ours." And in nearly the same spirit, at the time of the Copernican controversy, it was thought proper to append to the work of Copernicus a postil, to say that the work was written to account for the phenomena, and that people must not run on blindly and condemn either of the opposite opinions. Even when the Inquisition, in 1616, thought itself compelled to pronounce a decision upon this subject, the verdict was delivered in very moderate language;— that "the doctrine of the earth's motion appeared to be contrary to Scripture:" and yet, moderate as this expression is, it has been blamed by judicious members of the Roman church as deciding a point such as religious authorities ought not to pretend to decide; and has brought upon that church no ordinary weight of general condemnation. Kepler pointed out, in his lively manner, the imprudence of employing the force of religious authorities on such subjects: *Acies dolabræ in ferrum illisa, postea nec in lignum valet amplius. Capiat hoc cujus interest.* "If you *will* try to chop iron, the axe becomes unable to cut even wood. I warn those whom it concerns."

* Lib. I. *de Genesi*, cap. xviii.

11. *In what Spirit should the Change be urged?*—But while we thus endeavour to show in what manner the interpreters of Scripture may most safely and most properly accept the discoveries of science, we must not forget that there may be errours committed on the other side also; and that men of science, in bringing forward views which may for a time disturb the minds of lovers of Scripture, should consider themselves as bound by strict rules of candour, moderation, and prudence. Intentionally to make their supposed discoveries a means of discrediting, contradicting, or slighting the sacred Scriptures, or the authority of religion, is in them unpardonable. As men who make the science of Truth the business of their lives, and are persuaded of her genuine superiority, and certain of her ultimate triumph, they are peculiarly bound to urge her claims in a calm and temperate spirit; not forgetting that there are other kinds of truth besides that which they peculiarly study. They may properly reject authority in matters of science; but they are to leave it its proper office in matters of religion. I may here again quote Kepler's expressions: "In Theology we balance authorities, in Philosophy we weigh reasons. A holy man was Lactantius who denied that the earth was round; a holy man was Augustin, who granted the rotundity, but denied the antipodes; a holy thing to me is the Inquisition, which allows the smallness of the earth, but denies its motion; but more holy to me is Truth; and hence I prove, from philosophy, that the earth is round, and inhabited on every side, of small size, and in motion among the stars,—and this I do with no disrespect to the Doctors." I the more willingly quote such a passage from Kepler, because the entire ingenuousness and sincere piety of his character does not allow us to suspect in him anything of hypocrisy or latent irony. That similar professions

of respect may be made ironically, we have a noted example in the celebrated Introduction to *Galileo's Dialogue on the Copernican System;* probably the part which was most offensive to the authorities. "Some years ago," he begins, "a wholesome edict was promulgated at Rome, which, in order to check the perilous scandals of the present age, imposed silence upon the Pythagorean opinion of the mobility of the earth. There were not wanting," he proceeds, "persons who rashly asserted that this decree was the result, not of a judicious inquiry, but of passion ill-informed; and complaints were heard that counsellors, utterly unacquainted with astronomical observation, ought not to be allowed, with their sudden prohibitions, to clip the wings of speculative intellects. *At the hearing of rash lamentations like these, my zeal could not keep silence.*" And he then goes on to say, that he wishes, in his *Dialogue*, to show that the subject had been fully examined at Rome. Here the irony is quite transparent, and the sarcasm glaringly obvious. I think we may venture to say that this is not the temper in which scientific questions should be treated; although by some, perhaps, the prohibition of public discussion may be considered as justifying any evasion which is likely to pass unpunished.

12. *Duty of Mutual Forbearance.*—We may add, as a further reason for mutual forbearance in such cases, that the true interests of both parties are the same. The man of science is concerned, no less than any other person, in the truth and import of the divine dispensation; the religious man, no less than the man of science, is, by the nature of his intellect, incapable of believing two contradictory declarations. Hence they have both alike a need for understanding the Scripture in some way in which it shall be consistent with their understanding of nature. It is for their common advantage

to conciliate, as Kepler says, the finger and the tongue of God, his works and his word. And they may find abundant reason to bear with each other, even if they should adopt for this purpose different interpretations, each finding one satisfactory to himself; or if any one should decline employing his thoughts on such subjects at all. I have elsewhere* quoted a passage from Kepler† which appears to me written in a most suitable spirit: "I beseech my reader that, not unmindful of the Divine goodness bestowed upon man, he do with me praise and celebrate the wisdom of the Creator, which I open to him from a more inward explication of the form of the world, from a searching of causes, from a detection of the errours of vision; and that thus not only in the firmness and stability of the earth may we perceive with gratitude the preservation of all living things in nature as the gift of God: but also that in its motion, so recondite, so admirable, we may acknowledge the wisdom of the Creator. But whoever is too dull to receive this science, or too weak to believe the Copernican system without harm to his piety, him, I say, I advise that, leaving the school of astronomy, and condemning, if so he please, any doctrines of the philosophers, he follow his own path, and desist from this wandering through the universe; and that, lifting up his natural eyes, with which alone he can see, he pour himself out from his own heart in worship of God the Creator, being certain that he gives no less worship to God than the astronomer, to whom God has given to see more clearly with his inward eyes, and who, from what he has himself discovered, both can and will glorify God."

13. *Case of Galileo.*—I may perhaps venture here to make a remark or two upon this subject with reference

* *Bridgewater Tr.*, p. 314. † *Com. Stell. Mart.*, Introd.

to a charge brought against a certain portion of the *History of the Inductive Sciences.* Complaint has been made* that the character of the Roman church, as shown in its behaviour towards Galileo, is misrepresented in the account given of it in the History of Astronomy. It is asserted that Galileo provoked the condemnation he incurred; first, by pertinaciously demanding the assent of the ecclesiastical authorities to his opinion of the consistency of the Copernican doctrine with Scripture; and afterwards by contumaciously, and, as we have seen, contumeliously violating the silence which the Church had enjoined upon him. It is further declared that the statement which represents it as the habit of the Roman church to dogmatize on points of natural science is unfounded; as well as the opinion that in consequence of this habit, new scientific truths were promulgated less boldly in Italy than in other countries. I shall reply very briefly on these subjects; for the decision of them is by no means requisite in order to establish the doctrines to which I have been led in the present chapter, nor, I hope, to satisfy my reader that my views have been collected from an impartial consideration of scientific history.

With regard to Galileo, I do not think it can be denied that he obtruded his opinions upon the ecclesiastical authorities in an unnecessary and imprudent manner. He was of an ardent character, strongly convinced himself, and urged on still more by the conviction which he produced among his disciples, and thus he became impatient for the triumph of truth. This judgment of him has recently been delivered by various independent authorities, and has undoubtedly considerable foundation†. As to the question whether authority in matters

* *Dublin Review,* No. IX., July, 1838, p. 72.

† Besides the *Dublin Review*, I may quote the *Edinburgh Review*, which

of natural science were habitually claimed by the authorities of the Church of Rome, I have to allow that I cannot produce instances which establish such a habit. We who have been accustomed to have daily before our eyes the Monition which the Romish editors of Newton thought it necessary to prefix—*Cæterum latis a summo Pontifice contra telluris motum Decretis, nos obsequi profitemur*—were not likely to conjecture that this was a solitary instance of the interposition of the Papal authority on such subjects. But although it would be easy to find declarations of heresy delivered by Romish Universities, and writers of great authority, against tenets belonging to the natural sciences, I am not aware that any other case can be adduced in which the Church or the Pope can be shown to have pronounced such a sentence. I am well contented to acknowledge this; for I should be far more gratified by finding myself compelled to hold up the seventeenth century as a model for the nineteenth in this respect, than by having to sow enmity between the admirers of the past and the present through any disparaging contrast*.

With respect to the attempt made in my History to characterize the intellectual habits of Italy as produced

which I suppose will not be thought likely to have a bias in favour of the exercise of ecclesiastical authority in matters of science; though certainly there is a puerility in the critic's phraseology which does not add to the weight of his judgment. "Galileo contrived to surround the truth with every variety of obstruction. The tide of knowledge, which had hitherto advanced in peace, he crested with angry breakers, and he involved in its surf both his friends and his foes."—*Ed. Rev.*, No. cxxiii. p. 126.

* I may add that the most candid of the adherents of the Church of Rome condemn the assumption of authority in matters of science, made, in this one instance at least, by the ecclesiastical tribunals. The author of the *Ages of Faith* (Book VIII. p. 248), says, "A Congregation, it is to be lamented, declared the new system to be opposed to Scripture, and therefore heretical."

by her religious condition,—certainly it would ill become any student of the history of science to speak slightingly of that country, always the mother of sciences, always ready to catch the dawn and hail the rising of any new light of knowledge. But I think our admiration of this activity and acuteness of mind is by no means inconsistent with the opinion, that new truths were promulgated more boldly beyond the Alps, and that the subtilty of the Italian intellect loved to insinuate what the rough German bluntly asserted. Of the decent duplicity with which forbidden opinions were handled, the reviewer himself gives us instances, when he boasts of the liberality with which Copernican professors were placed in important stations by the ecclesiastical authorities, soon after the doctrine of the motion of the earth had been declared by the same authorities to be contrary to Scripture. And in the same spirit is the process of demanding from Galileo a public and official recantation of opinions which he had repeatedly been told by his ecclesiastical superiors he might hold as much as he pleased. I think it is easy to believe that among persons so little careful to reconcile public profession with private conviction, official decorum was all that was demanded. When Galileo had made his renunciation of the earth's motion on his knees, he rose and said, as we are told, *E pur si muove*—"and yet it *does* move." This is sometimes represented as the heroic soliloquy of a mind cherishing its conviction of the truth, in spite of persecution; I think we may more naturally conceive it uttered as a playful epigram in the ear of a cardinal's secretary, with a full knowledge that it would be immediately repeated to his master*.

Besides the Ideas involved in the material sciences,

* I have somewhat further discussed the case of Galileo in the second edition of the *History*, Vol. I. p. 418, and Notes (Q) and (R).

of which we have already examined the principal ones, there is one Idea or Conception which our Sciences do not indeed include, but to which they not obscurely point; and the importance of this Idea will make it proper to speak of it, though this must be done very briefly.

CHAPTER V.

OF THE CONCEPTION OF A FIRST CAUSE.

1. AT the end of the last chapter but one, we were led to this result,—that we cannot, in any of the Palætiological Sciences, ascend to a beginning which is of the same nature as the existing cause of events, and which depends upon causes that are still in operation. Philosophers never have demonstrated, and probably never will be able to demonstrate, what was the original condition of the solar system, of the earth, of the vegetable and animal worlds, of languages, of arts. On all these subjects the course of investigation, followed backwards as far as our materials allow us to pursue it, ends at last in an impenetrable gloom. We strain our eyes in vain when we try, by our natural faculties, to discern an Origin.

2. Yet speculative men have been constantly employed in attempts to arrive at that which thus seems to be placed out of their reach. The Origin of Languages, the Origin of the present Distribution of Plants and Animals, the Origin of the Earth, have been common subjects of diligent and persevering inquiry. Indeed inquiries respecting such subjects have been, at least till lately, the usual form which Palætiological researches have assumed. *Cosmogony*, the origin of the world, of which, in such speculations, the earth was considered as a principal part, has been a favourite study both of ancient and of modern

times: and most of the attempts at Geology previous to the present period have been *Cosmogonies* or *Geogonies*, rather than that more genuine science which we have endeavoured to delineate. Again: Glossology, though now an extensive body of solid knowledge, was mainly brought into being by inquiries concerning the Original Language spoken by men; and the nature of the first separation and diffusion of languages, the first peopling of the earth by man and by animals, were long sought after with ardent curiosity, although of course with reference to the authority of the Scriptures, as well as the evidence of natural phenomena. Indeed the interest of such inquiries even yet is far from being extinguished. The disposition to explore the past in the hope of finding, by the light of natural reasoning as well as by the aid of revelation, the origin of the present course of things, appears to be unconquerable. "What was the beginning?" is a question which the human race cannot desist from perpetually asking. And no failure in obtaining a satisfactory answer can prevent inquisitive spirits from again and again repeating the inquiry, although the blank abyss into which it is uttered does not even return an echo.

3. What, then, is the reason of an attempt so pertinacious yet so fruitless? By what motive are we impelled thus constantly to seek what we can never find? Why are the errour of our conjectures, the futility of our reasonings, the precariousness of our interpretations, over and over again proved to us in vain? Why is it impossible for us to acquiesce in our ignorance and to relinquish the inquiry? Why cannot we content ourselves with examining those links of the chain of causes which are nearest to us;—those in which the connexion is intelligible and clear; instead of fixing our attention upon those remote portions where we can no longer

estimate its coherence? In short, why did not men from the first take for the subject of their speculations the Course of Nature rather than the Origin of Things?

To this we reply, that in doing what they have thus done, in seeking what they have sought, men are impelled by an intellectual necessity. They cannot conceive a Series of connected occurrences without a Commencement; they cannot help supposing a cause for the Whole, as well as a cause for each part; they cannot be satisfied with a succession of causes without assuming a First Cause. Such an assumption is necessarily impressed upon our minds by our contemplation of a series of causes and effects; that *there must be a First Cause,* is accepted by all intelligent reasoners as an Axiom: and like other Axioms, its truth is necessarily implied in the Idea which it involves.

4. The evidence of this axiom may be illustrated in several ways. In the first place, the axiom is assumed in the argument usually offered to prove the existence of the Deity. Since, it is said, the world now exists, and since nothing cannot produce something, something must have existed from eternity. This Something is the First Cause: it is God.

Now what I have to remark here is this:—the conclusiveness of this argument, as a proof of the existence of one independent, immutable Deity, depends entirely upon the assumption of the axiom above stated. The World, a series of causes and effects, exists: therefore there must be, not only this series of causes and effects, but also a First Cause. It will be easily seen, that without the axiom, that in every series of causes and effects there must be a First Cause, the reasoning is altogether inconclusive.

5. Or to put the matter otherwise: The argument

for the existence of the Deity was stated thus: Something exists, therefore something must have existed from eternity. "Granted," the opponent might say; "but this *something* which has existed from eternity, why may it not be this very series of causes and effects which is now going on, and which appears to contain in itself no indication of beginning or end?" And thus, without the assumption of the necessity of a First Cause, the force of the argument may be resisted.

6. But, it may be asked, how do those who have written to prove the existence of the Deity reply to such an objection as the one just stated? It is natural to suppose that, on a subject so interesting and so long discussed, all the obvious arguments with their replies, have been fully brought into view. What is the result in this case?

The principal modes of replying to the above objection, that the series of causes and effects which now exists, may have existed from eternity, appear to be these.

In the first place, our minds cannot be satisfied with a series of successive, dependent, causes and effects, without something first and independent. We pass from effect to cause, and from that to a higher cause, in search of something on which the mind can rest; but if we can do nothing but repeat this process, there is no use in it. We move our limbs, but make no advance. Our question is not answered, but evaded. The mind cannot acquiesce in the destiny thus presented to it, of being referred from event to event, from object to object, along an interminable vista of causation and time. Now this mode of stating the reply,—to say that the mind *cannot thus be satisfied*, appears to be equivalent to saying that the mind is conscious of a Principle, in virtue of which such a view as this must be rejected;—the mind takes refuge in the assumption of a First Cause, from an employment inconsistent with its own nature.

7. Or again, we may avoid the objection, by putting the argument for the existence of a Deity in this form: The series of causes and effects which we call the *world*, or the *course of nature*, may be considered as a *whole*, and this whole must have a cause of its existence. The whole collection of objects and events may be comprehended as a single effect, and of this effect there must be a cause. This Cause of the Universe must be superior to, and independent of the special events, which, happening in time, make up the universe of which He is the cause. He must exist and exercise causation, before these events can begin: He must be the First Cause.

Although the argument is here somewhat modified in form, the substance is the same as before. For the assumption that we may consider the whole series of causes and effects as a *single effect*, is equivalent to the assumption that besides partial causes we must have a First Cause. And thus the Idea of a First Cause, and the axiom which asserts its necessity, are recognized in the usual argumentation on this subject.

8. This Idea of a First Cause, and the principle involved in the Idea, have been the subject of discussion in another manner. As we have already said, we assume as an axiom that a First Cause must exist; and we assert that God, the First Cause, exists eternal and immutable, by the necessity which the axiom implies. Hence God is said to exist necessarily;—to be a necessarily existing being. And when this *necessary existence* of God had been spoken of, it soon began to be contemplated as a sufficient reason, and as an absolute demonstration of His existence; without any need of referring to the world as an effect, in order to arrive at God as the cause. And thus men conceived that they had obtained a proof of

the existence of the Deity, *à priori*, from Ideas, as well as *à posteriori*, from Effects.

9. Thus, Thomas Aquinas employs this reasoning to prove the *eternity* of God*. "Oportet ponere aliquod primum necessarium quod est per se ipsum necessarium; et hoc est Deus, cum sit prima causa ut dictum est: igitur Deus æternus est, cum omne necessarium per se sit æternum." It is true that the schoolmen never professed to be able to prove the *existence* of the Deity *à priori:* but they made use of this conception of necessary existence in a manner which approached very near to such an attempt. Thus Suarez† discusses the question, "Utrum aliquo modo possit *à priori* demonstrari Deum esse." And resolves the question in this manner: "Ad hunc ergo modum dicendum est: Demonstrato *à posteriori* Deum esse ens necessarium et a se, ex hoc attributo posse *à priori* demonstrari præter illud non posse esse aliud ens necessarium et a se, et consequenter demonstrari Deum esse."

But in modern times attempts were made by Descartes and Samuel Clarke, to prove the Divine existence at once *à priori*, from the conception of necessary existence; which, it was argued, could not subsist without actual existence. This argumentation was acutely and severely criticized by Dr. Waterland.

10. Without dwelling upon a subject, the discussion of which does not enter into the design of the present work, I may remark that the question whether an *à priori* proof of the existence of a First Cause be possible, is a question concerning the nature of our Ideas, and the evidence of the axioms which they involve, of the same kind as many questions which we have already had to discuss. Is our Conception or Idea of a First Cause gathered from

* Aquin. *Contr. Gentil.* Lib. I. c. xiv. p. 21.

† *Metaphys.* Tom. II. Disp. xxix. sect. 3, p. 28.

the effects we see around us? It is plain that we must answer, here as in other cases, that the Idea is not extracted from the phenomena, but assumed in order that the phenomena may become intelligible to the mind;—that the Idea is a necessary one, inasmuch as it does not depend upon observation for its evidence; but that it depends upon observation for its developement, since without some observation, we cannot conceive the mind to be cognizant of the relation of causation at all. In this respect, however, the Idea of a First Cause is no less necessary than the ideas of Space, or Time, or Cause in general. And whether we call the reasoning derived from such a necessity an argument *à priori* or *à posteriori*, in either case it possesses the genuine character of demonstration, being founded upon axioms which command universal assent.

11. I have, however, spoken of our *Conception* rather than of our *Idea* of a First Cause; for the notion of a First Cause appears to be rather a modification of the Fundamental Idea of Cause, which was formerly discussed, than a separate and peculiar Idea. And the Axiom, *that there must be a First Cause*, is recognized by most persons as an application of the general Axiom of Causation, *that every effect must have a cause;* this latter Axiom being applied to the world, considered in its totality, as a single effect. This distinction, however, between an Idea and a Conception, is of no material consequence to our argument; provided we allow the maxim, that there must be a First Cause, to be necessarily and evidently true; whether it be thought better to speak of it as an independent Axiom, or to consider it as derived from the general Axiom of Causation.

12. Thus we necessarily infer a First Cause, although the Palætiological Sciences only point *towards* it, and do not lead us *to* it. But I must observe further; that in

each of the series of events which form the subject of Palætiological research, the First Cause is the *same.* Without here resting upon reasoning founded upon our Conception of a First Cause, I may remark that this identity is proved by the close connexion of all the branches of natural science, and the way in which the causes and the events of each are interwoven with those which belong to the others. We must needs believe that the First Cause which produced the earth and its atmosphere is also the Cause of the plants which clothe its surface; that the First Cause of the vegetable and of the animal world are the same; that the First Cause which produced light produced also eyes; that the First Cause which produced air and organs of articulation produced also language and the faculties by which language is rendered possible: and if *those* faculties, then also all man's other faculties;—the powers by which, as we have said, he discerns right and wrong, and recognizes a providential as well as a natural course of things. Nor can we think otherwise than that the Being who gave these faculties, bestowed them for some purpose;—bestowed them for that purpose which alone is compatible with their nature:—the purpose, namely, of guiding and elevating man in his present career, and of preparing him for another state of being to which they irresistibly direct his hopes. And thus, although, as we have said, no one of the Palætiological Sciences can be traced continuously to an Origin, yet they not only each point to an Origin, but all to the same Origin. Their lines are broken indeed, as they run backwards into the early periods of the world, but yet they all appear to converge to the same invisible point. And this point, thus indicated by the natural course of things, can be no other than that which is disclosed to us as the starting point of the providential course of the world; for we are persuaded by such reasons as have just been hinted, that the Creator of the natural world can be no other than the Author and Governor and Judge of the moral and spiritual world.

13. Thus we are led, by our material sciences, and especially by the Palætiological class of them, to the borders of a higher region, and to a point of view from which we have a prospect of other provinces of knowledge, in which other faculties of man are concerned besides his intellectual, other interests involved besides those of speculation. On these it does not belong to our present plan to dwell: but even such a brief glance as we have taken of the connexion of material with moral speculations may not be useless, since it may serve to show that the principles of truth which we are now laboriously collecting among the results of the physical sciences, may possibly find some application in those parts of knowledge towards which men most naturally look with deeper interest and more serious reverence.

We have been employed up to the present stage of this work in examining the materials of knowledge, namely, Facts and Ideas; and we have dwelt particularly upon the latter element; inasmuch as the consideration of it is, on various accounts, and especially at the present time, by far the most important. We have now to proceed to the remainder of our task;—to determine the processes by which those materials may actually be made to constitute knowledge. We have surveyed the stones of our building: we have found them exactly squared, and often curiously covered with significant imagery and important inscriptions. We have now to discover how they may best be fitted into their places, and cemented together, so that rising stage above stage, they may grow at last into that fair and lofty temple of Truth for which we cannot doubt that they were intended by the Great Architect.

END OF VOLUME I.

Alexander Ziwet

THE

PHILOSOPHY

OF THE

INDUCTIVE SCIENCES,

FOUNDED UPON THEIR HISTORY.

BY WILLIAM WHEWELL, D.D.,

MASTER OF TRINITY COLLEGE, CAMBRIDGE.

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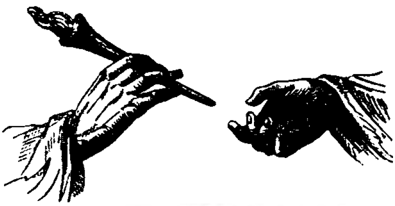
WITH CORRECTIONS AND ADDITIONS, AND
AN APPENDIX, CONTAINING
PHILOSOPHICAL ESSAYS PREVIOUSLY PUBLISHED.

IN TWO VOLUMES.



Λαμπάδια ἔχοντες διαδώσουσιν ἀλλήλοις.

VOLUME THE SECOND.

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PART II.

OF KNOWLEDGE.

De Scientiis tum demum bene sperandum est, quando per Scalam veram et per gradus continuos, et non intermissos aut hiulcos, a particularibus ascendetur ad Axiomatur minora, et deinde ad media, alia aliis superiora, et postremò demum ad generalissima.

In constituendo autem Axiomate, Forma Inductionis alia quam adhuc in usu fuit, excogitanda est; et quæ non ad Principia tantum (quæ vocant) probanda et invenienda, sed etiam ad Axiomata minora, et media, denique omnia.

Bacon, *Nov. Org.*, Aph. civ. cv.

BOOK XI.

OF THE CONSTRUCTION OF SCIENCE.

Chapter I.

OF TWO PRINCIPAL PROCESSES BY WHICH SCIENCE IS CONSTRUCTED.

To the subject of the present Book all that has preceded is subordinate and preparatory. The First Part of this work treated of Ideas: we now enter upon the Second Part, in which we have to consider the Knowledge which arises from them. It has already been stated that Knowledge requires us to possess both Facts and Ideas;—that every step in our knowledge consists in applying the ideas and conceptions furnished by our minds to the facts which observation and experiment offer to us. When our conceptions are clear and distinct, when our facts are certain and sufficiently numerous, and when the conceptions, being suited to the nature of the facts, are applied to them so as to produce an exact and universal accordance, we attain knowledge of a precise and comprehensive kind, which we may term *Science*. And we apply this term to our knowledge still more decidedly when, facts being thus included in exact and general propositions, such propositions are, in the same manner, included with equal rigour in propositions of a higher degree of generality; and these again in others of a still wider nature, so as to form a large and systematic whole.

But after thus stating, in a general way, the nature of science, and the elements of which it consists, we have been examining with a more close and extensive scrutiny, some of those elements; and we must now return to our main subject, and apply to it the results of our long investigation. We have been exploring the realm of Ideas; we have been passing in review the difficulties in which the workings of our own minds involve us when we would make our conceptions consistent with themselves: and we have endeavoured to get a sight of the true solutions of these difficulties. We have now to inquire how the results of these long and laborious efforts of thought find their due place in the formation of our knowledge. What do we gain by these attempts to make our notions distinct and consistent; and in what manner is the gain of which we thus become possessed, carried to the general treasure-house of our permanent and indestructible knowledge? After all this battling in the world of ideas, all this struggling with the shadowy and changing forms of intellectual perplexity, how do we secure to ourselves the fruits of our warfare, and assure ourselves that we have really pushed forwards the frontier of the empire of Science? It is by such an appropriation that the task which we have had in our hands during the last nine Books of this work, must acquire its real value and true place in our design.

In order to do this, we must reconsider, in a more definite and precise shape, the doctrine which has already been laid down;—that our knowledge consists in applying Ideas to Facts; and that the conditions of real knowledge are that the ideas be distinct and appropriate, and exactly applied to clear and certain facts. The steps by which our knowledge is advanced are those by which one or the other of these two processes is rendered more complete;—by which *conceptions* are *made* *more clear* in themselves, or by which the conceptions more strictly *bind together the facts*. These two processes may be considered as together constituting the whole formation of our knowledge; and the principles which have been established in the preceding Books, bear principally upon the former of these two operations; —upon the business of elevating our conceptions to the highest possible point of precision and generality. But these two portions of the progress of knowledge are so clearly connected with each other, that we shall consider them in immediate succession. And having now to consider these operations in a more exact and formal manner than it was before possible to do, we shall designate them by certain constant and technical phrases. We shall speak of the two processes by which we arrive at science, as *the Explication of Conceptions* and *the Colligation of Facts:* we shall show how the discussions in which we have been engaged have been necessary in order to promote the former of these offices; and we shall endeavour to point out modes, maxims, and principles by which the second of the two tasks may also be furthered.

CHAPTER II.

OF THE EXPLICATION OF CONCEPTIONS.

SECT. I.—*Its historical Progress.*

1. WE have given the appellation of *Ideas* to certain comprehensive forms of thought,—as *space, number, cause, composition, resemblance,*—which we apply to the phenomena which we contemplate. But the special modifications of these ideas which are exemplified in

particular facts, we have termed *Conceptions;* as *a circle, a square number, an accelerating force, a neutral combination* of elements, a *genus.* Such Conceptions involve in themselves certain necessary and universal relations derived from the Ideas just enumerated; and these relations are an indispensable portion of the texture of our knowledge. But to determine the contents and limits of this portion of our knowledge, requires an examination of the Ideas and Conceptions from which it proceeds. The Conceptions must be, as it were, carefully *unfolded,* so as to bring into clear view the elements of truth with which they are marked from their ideal origin. This is one of the processes by which our knowledge is extended and made more exact; and this I shall describe as the *Explication of Conceptions.*

In the preceding Books we have discussed a great many of the Fundamental Ideas of the most important existing sciences. We have, in those Books, abundant exemplifications of the process now under our consideration. We shall here add a few general remarks, suggested by the survey which we have thus made.

2. Such discussions as those in which we have been engaged concerning our fundamental Ideas, have been the course by which, historically speaking, those Conceptions which the existing sciences involve have been rendered so clear as to be fit elements of exact knowledge. The disputes concerning the various kinds and measures of *Force* were an important part of the progress of the science of Mechanics. The struggles by which philosophers attained a right general conception of *plane,* of *circular,* of *elliptical Polarization,* were some of the most difficult steps in the modern discoveries of Optics. A Conception of the *Atomic Constitution* of bodies, such as shall include what we know, and assume nothing more, is even now a matter of conflict

among Chemists. The debates by which, in recent times, the Conceptions of *Species* and *Genera* have been rendered more exact, have improved the science of Botany: the imperfection of the science of Mineralogy arises in a great measure from the circumstance, that in that subject, the Conception of a *Species* is not yet fixed. In physiology, what a vast advance would that philosopher make, who should establish a precise, tenable, and consistent Conception of *Life!*

Thus discussions and speculations concerning the import of very abstract and general terms and notions, may be, and in reality have been, far from useless and barren. Such discussions arose from the desire of men to impress their opinions on others, but they had the effect of making the opinions much more clear and distinct. In trying to make others understand them, they learnt to understand themselves. Their speculations were begun in twilight, and ended in the full brilliance of day. It was not easily and at once, without expenditure of labour or time, that men arrived at those notions which now form the elements of our knowledge; on the contrary, we have, in the history of science, seen how hard discoverers, and the forerunners of discoverers, have had to struggle with the indistinctness and obscurity of the intellect, before they could advance to the critical point at which truth became clearly visible. And so long as, in this advance, some speculators were more forward than others, there was a natural and inevitable ground of difference of opinion, of argumentation, of wrangling. But the tendency of all such controversy is to diffuse truth and to dispel errour. Truth is consistent, and can bear the tug of war; Errour is incoherent, and falls to pieces in the struggle. True Conceptions can endure the sun, and become clearer as a fuller light is obtained; confused and inconsistent notions vanish like visionary spectres at the break of a brighter day. And thus all the controversies concerning such Conceptions as science involves have ever ended in the establishment of the side on which the truth was found.

3. Indeed, so complete has been the victory of truth in most of these instances, that at present we can hardly imagine the struggle to have been necessary. The very essence of these triumphs is that they lead us to regard the views we reject as not only false, but inconceivable. And hence we are led rather to look back upon the vanquished with contempt than upon the victors with gratitude. We now despise those who in the Copernican controversy could not conceive the apparent motion of the sun on the heliocentric hypothesis;—or those who, in opposition to Galileo, thought that a uniform force might be that which generated a velocity proportional to the space;—or those who held there was something absurd in Newton's doctrine of the different refrangibility of differently coloured rays;—or those who imagined that when elements combine, their sensible qualities must be manifest in the compound;—or those who were reluctant to give up the distinction of vegetables into herbs, shrubs, and trees. We cannot help thinking that men must have been singularly dull of comprehension, to find a difficulty in admitting what is to us so plain and simple. We have a latent persuasion that we in their place should have been wiser and more clear-sighted;—that we should have taken the right side, and given our assent at once to the truth.

4. Yet in reality, such a persuasion is a mere delusion. The persons who, in such instances as the above, were on the losing side, were very far, in most cases, from being persons more prejudiced, or stupid, or narrow-minded, than the greater part of mankind now are;

and the cause for which they fought was far from being a manifestly bad one, till it had been so decided by the result of the war. It is the peculiar character of scientific contests, that what is only an epigram with regard to other warfare is a truth in this;—They who are defeated are really in the wrong. But they may, nevertheless, be men of great subtilty, sagacity, and genius; and we nourish a very foolish self-complacency when we suppose that we are their superiors. That this is so, is proved by recollecting that many of those who have made very great discoveries have laboured under the imperfection of thought which was the obstacle to the next step in knowledge. Though Kepler detected with great acuteness the Numerical Laws of the solar system, he laboured in vain to conceive the very simplest of the Laws of Motion by which the paths of the planets are governed. Though Priestley made some important steps in chemistry, he could not bring his mind to admit the doctrine of a general Principle of Oxidation. How many ingenious men in the last century rejected the Newtonian Attraction as an impossible chimera! How many more, equally intelligent, have, in the same manner, in our own time, rejected, I do not now mean as false, but as inconceivable, the doctrine of Luminiferous Undulations! To err in this way is the lot, not only of men in general, but of men of great endowments, and very sincere love of truth.

5. And those who liberate themselves from such perplexities, and who thus go on in advance of their age in such matters, owe their superiority in no small degree to such discussions and controversies as those to which we now refer. In such controversies, the conceptions in question are turned in all directions, examined on all sides; the strength and the weakness of the maxims which men apply to them are fully tested; the light of

the brightest minds is diffused to others. Inconsistency is unfolded into self-contradiction; axioms are built up into a system of necessary truths; and ready exemplifications are accumulated of that which is to be proved or disproved concerning the ideas which are the basis of the controversy.

The History of Mechanics from the time of Kepler to that of Lagrange, is perhaps the best exemplification of the mode in which the progress of a science depends upon such disputes and speculations as give clearness and generality to its elementary conceptions. This, it is to be recollected, is the kind of progress of which we are now speaking; and this is the principal feature in the portion of scientific history which we have mentioned. For almost all that was to be done by reference to observation, was executed by Galileo and his disciples. What remained was the task of generalization and simplification. And this was promoted in no small degree by the various controversies which took place within that period concerning mechanical conceptions:—as, for example, the question concerning the measure of the Force of Percussion;—the war of the *Vis Viva*;—the controversy of the Center of Oscillation;—of the independence of Statics and Dynamics;—of the principle of Least Action;—of the evidence of the Laws of Motion;—and of the number of Laws really distinct. None of these discussions was without its influence in giving generality and clearness to the mechanical ideas of mathematicians: and therefore, though remote from general apprehension, and dealing with very abstract notions, they were of eminent use in the perfecting the science of mechanics. Similar controversies concerning fundamental notions, those, for example, which Galileo himself had to maintain, were no less useful in the formation of the science of hydrostatics. And the like struggles and conflicts, whether they take the form of controversies between several persons, or only operate in the efforts and fluctuations of the discoverer's mind, are always requisite before the conceptions acquire that clearness which makes them fit to appear in the enunciation of scientific truth.

This, then, is one object of the preceding Books;—to bring under the reader's notice the main elements of the controversies which have thus had so important a share in the formation of the existing body of science, and the decisions on the controverted points to which the mature examination of the subject has led; and thus to give an abundant exhibition of that step which we term the Explication of Conceptions.

SECT. II.—*Use of Definitions.*

6. The result of such controversies as we have been speaking of, often appears to be summed up in a *Definition*; and the controversy itself has often assumed the form of a battle of definitions. For example, the inquiry concerning the Laws of Falling Bodies led to the question whether the proper Definition of a *uniform force* is, that it generates a velocity proportional to the *space* from rest, or to the *time*. The controversy of the *Vis Viva* was, what was the proper Definition of the *measure of force*. A principal question in the classification of minerals is, what is the Definition of a *mineral species*. Physiologists have endeavoured to throw light on their subject, by Defining *organization*, or some similar term.

7. It is very important for us to observe, that these controversies have never been questions of insulated and *arbitrary* Definitions, as men seem often tempted to suppose them to have been. In all cases there is a tacit assumption of some Proposition which is to be expressed

by means of the Definition, and which gives it its importance. The dispute concerning the Definition thus acquires a real value, and becomes a question concerning true and false. Thus in the discussion of the question, What is a Uniform Force? it was taken for granted that "gravity is a uniform force:"—in the debate of the *Vis Viva*, it was assumed that "in the mutual action of bodies the whole effect of the force is unchanged:"—in the zoological definition of Species, (that it consists of individuals which have, or may have, sprung from the same parents,) it is presumed that "individuals so related resemble each other more than those which are excluded by such a definition;" or perhaps, that "species so defined have permanent and definite differences." A definition of Organization, or of any other term, which was not employed to express some principle, would be of no value.

The establishment, therefore, of a right Definition of a Term may be a useful step in the explication of our conceptions; but this will be the case then only when we have under our consideration some Proposition in which the Term is employed. For then the question really is, how the Conception shall be understood and defined in order that the Proposition may be true.

8. The establishment of a Proposition requires an attention to observed Facts, and can never be rightly derived from our Conceptions alone. We must hereafter consider the necessity which exists that the Facts should be rightly bound together, as well as that our Conceptions should be clearly employed, in order to lead us to real knowledge. But we may observe here that, in such cases at least as we are now considering, the two processes are co-ordinate. To unfold our Conceptions by the means of Definitions, has never been serviceable to science, except when it has been associated with an

immediate *use* of the Definitions. The endeavour to define a Uniform Force was combined with the assertion that "gravity is a uniform force:" the attempt to define Accelerating Force was immediately followed by the doctrine that "accelerating forces may be compounded:" the process of defining Momentum was connected with the principle that "momenta gained and lost are equal:" naturalists would have given in vain the Definition of Species which we have quoted, if they had not also given the "characters" of species so separated. Definition and Proposition are the two handles of the instrument by which we apprehend truth; the former is of no use without the latter. Definition may be the best mode of explaining our Conception, but that which alone makes it worth while to explain it in any mode, is the opportunity of using it in the expression of Truth. When a Definition is propounded to us as a useful step in knowledge, we are always entitled to ask what Principle it serves to enunciate. If there be no answer to this inquiry, we define and give clearness to our conceptions in vain. While we labour at such a task, we do but light up a vacant room;—we sharpen a knife with which we have nothing to cut;—we take exact aim, while we load our artillery with blank cartridge;—we apply strict rules of grammar to sentences which have no meaning.

If, on the other hand, we have under our consideration a proposition probably established, every step which we can make in giving distinctness and exactness to the Terms which this proposition involves, is an important step towards scientific truth. In such cases, any improvement in our Definition is a real advance in the explication of our Conception. The clearness of our Expressions casts a light upon the Ideas which we contemplate and convey to others.

9. But though *Definition* may be subservient to a

right explication of our conceptions, it is *not essential* to that process. It is absolutely necessary to every advance in our knowledge, that those by whom such advances are made should possess clearly the conceptions which they employ: but it is by no means necessary that they should unfold these conceptions in the words of a formal Definition. It is easily seen, by examining the course of Galileo's discoveries, that he had a distinct conception of the *Moving Force* which urges bodies downwards upon an inclined plane, while he still hesitated whether to call it *Momentum*, *Energy*, *Impetus*, or *Force*, and did not venture to offer a Definition of the thing which was the subject of his thoughts. The Conception of *Polarization* was clear in the minds of many optical speculators, from the time of Huyghens and Newton to that of Young and Fresnel. This Conception we have defined to be "Opposite properties depending upon opposite positions;" but this notion was, by the discoverers, though constantly assumed and expressed by means of superfluous hypotheses, never clothed in definite language. And in the mean time, it was the custom, among subordinate writers on the same subjects, to say, that the term *Polarization* had no definite meaning, and was merely an expression of our ignorance. The Definition which was offered by Haüy and others of a *Mineralogical Species*;—"The same elements combined in the same proportions, with the same fundamental form;"—was false, inasmuch as it was incapable of being rigorously applied to any one case; but this defect did not prevent the philosophers who propounded such a Definition from making many valuable additions to mineralogical knowledge, in the way of identifying some species and distinguishing others. The right Conception which they possessed in their minds prevented their being misled by their own very erroneous Definition. The want of any precise Definitions of *Strata*, and *Formations*, and *Epochs*, among geologists, has not prevented the discussions which they have carried on upon such subjects from being highly serviceable in the promotion of geological knowledge. For however much the apparent vagueness of these terms might leave their arguments open to cavil, there was a general understanding prevalent among the most intelligent cultivators of the science, as to what was meant in such expressions; and this common understanding sufficed to determine what evidence should be considered conclusive and what inconclusive, in these inquiries. And thus the distinctness of Conception, which is a real requisite of scientific progress, existed in the minds of the inquirers, although Definitions, which are a partial and accidental evidence of this distinctness, had not yet been hit upon. The idea had been developed in men's minds, although a clothing of words had not been contrived for it, nor, perhaps, the necessity of such a vehicle felt: and thus that essential condition of the progress of knowledge of which we are here speaking existed; while it was left to the succeeding speculators to put this unwritten Rule in the form of a verbal Statute.

10. Men are often prone to consider it as a thoughtless *omission* of an essential circumstance, and as a *neglect* which involves some blame, when knowledge thus assumes a form in which Definitions, or rather Conceptions, are implied but are not expressed. But in such a judgment, they assume *that* to be a matter of choice requiring attention only, which is in fact as difficult and precarious as any other portion of the task of discovery. To define, so that our Definition shall have any scientific value, requires no small portion of that sagacity by which truth is detected. As we have already said, Definitions and Propositions are co-ordinate in their use and in their

origin. In many cases, perhaps in most, the Proposition which contains a scientific truth, is apprehended with confidence, but with some vagueness and vacillation, before it is put in a positive, distinct, and definite form. It is thus known to be true, before it can be enunciated in terms each of which is rigorously defined. The business of Definition is part of the business of discovery. When it has been clearly seen what ought to be our Definition, it must be pretty well known what truth we have to state. The Definition, as well as the discovery, supposes a decided step in our knowledge to have been made. The writers on Logic in the middle ages, made *Definition* the last stage in the progress of knowledge; and in this arrangement at least, the history of science, and the philosophy derived from the history, confirm their speculative views. If the Explication of our Conceptions ever assume the form of a Definition, this will come to pass, not as an arbitrary process, or as a matter of course, but as the mark of one of those happy efforts of sagacity to which all the successive advances of our knowledge are owing.

SECT. III.—*Use of Axioms.*

11. Our Conceptions, then, even when they become so clear as the progress of knowledge requires, are not adequately expressed, or necessarily expressed at all, by means of Definitions. We may ask, then, whether there is any *other mode* of expression in which we may look for the evidence and exposition of that peculiar exactness of thought which the formation of science demands. And in answer to this inquiry, we may refer to the previous discussions respecting many of the Fundamental Ideas of the sciences. It has there been seen that these Ideas involve many elementary truths which enter into the texture of our knowledge, introducing into it connex-

ions and relations of the most important kind, although these elementary truths cannot be deduced from any verbal definition of the idea. It has been seen that these elementary truths may often be enunciated by means of *Axioms*, stated in addition to, or in preference to, Definitions. For example, the Idea of Cause, which forms the basis of the science of Mechanics, makes its appearance in our elementary mechanical reasonings, not as a Definition, but by means of the Axioms that "Causes are measured by their effects," and that "Reaction is equal and opposite to action." Such Axioms, tacitly assumed or occasionally stated, as maxims of acknowledged validity, belong to all the Ideas which form the foundations of the sciences, and are constantly employed in the reasoning and speculations of those who think clearly on such subjects. It may often be a task of some difficulty to detect and enunciate in words the Principles which are thus, perhaps silently and unconsciously, taken for granted by those who have a share in the establishment of scientific truth: but inasmuch as these Principles are an essential element in our knowledge, it is very important to our present purpose to separate them from the associated materials, and to trace them to their origin. This accordingly I have attempted to do, with regard to a considerable number of the most prominent of such Ideas, in the preceding Books. The reader will there find many of these Ideas resolved into Axioms and Principles by means of which their effect upon the elementary reasonings of the various sciences may be expressed. That part of the Work is intended to form, in some measure, a representation of the Ideal Side of our physical knowledge;—a Table of those contents of our Conceptions which are not received directly from facts;—an exhibition of Rules to which we know that truth must conform.

Sect. IV.—*Clear and appropriate Ideas.*

12. In order, however, that we may see the necessary cogency of these rules, we must possess, clearly and steadily, the Ideas from which the rules flow. In order to perceive the necessary relations of the Circles of the Sphere, we must possess clearly the Idea of Solid Space:—in order that we may see the demonstration of the composition of forces, we must have the Idea of Cause moulded into a distinct Conception of Statical Force. This is that *Clearness of Ideas* which we stipulate for in any one's mind, as the first essential condition of his making any new step in the discovery of truth. And we now see what answer we are able to give, if we are asked for a Criterion of this Clearness of Idea. The Criterion is, that the person shall *see* the necessity of the Axioms belonging to each Idea;—shall accept them in such a manner as to perceive the cogency of the reasonings founded upon them. Thus a person has a clear Idea of Space who follows the reasonings of geometry and fully apprehends their conclusiveness. The Explication of Conceptions, which we are speaking of as an essential part of real knowledge, is the process by which we bring the Clearness of our Ideas to bear upon the Formation of our Knowledge. And this is done, as we have now seen, not always, nor generally, nor principally, by laying down a Definition of the Conception; but by acquiring such a possession of it in our minds as enables, indeed compels us, to admit, along with the Conception, all the Axioms and Principles which it necessarily implies, and by which it produces its effect upon our reasonings.

13. But in order that we may make any real advance in the discovery of truth, our Ideas must not only be clear, they must also be *appropriate*. Each science has for its basis a different class of Ideas; and the

steps which constitute the progress of one science can never be made by employing the Ideas of another kind of science. No genuine advance could ever be obtained in Mechanics by applying to the subject the Ideas of Space and Time merely:—no advance in Chemistry, by the use of mere Mechanical Conceptions:—no discovery in Physiology, by referring facts to mere Chemical and Mechanical Principles. Mechanics must involve the Conception of *Force;*—Chemistry, the Conception of *Elementary Composition;*—Physiology, the Conception of *Vital Powers.* Each science must advance by means of its *appropriate* Conceptions. Each has its own field, which extends as far as its principles can be applied. I have already noted the separation of several of these fields by the divisions of the preceding Books. The Mechanical, the Secondary Mechanical, the Chemical, the Classificatory, the Biological Sciences form so many great Provinces in the Kingdom of knowledge, each in a great measure possessing its own peculiar fundamental principles. Every attempt to build up a new science by the application of principles which belong to an old one, will lead to frivolous and barren speculations.

This truth has been exemplified in all the instances in which subtle speculative men have failed in their attempts to frame new sciences, and especially in the essays of the ancient schools of philosophy in Greece, as has already been stated in the History of Science. Aristotle and his followers endeavoured in vain to account for the mechanical relation of forces in the lever by applying the *inappropriate* geometrical conceptions of the properties of the circle:—they speculated to no purpose about the elementary composition of bodies, because they assumed the *inappropriate* conception of *likeness* between the elements and the compound, instead of the genuine notion of elements merely *deter-*

mining the qualities of the compound. And in like manner, in modern times, we have seen, in the history of the fundamental ideas of the physiological sciences, how all the *inappropriate* mechanical and chemical and other ideas which were applied in succession to the subject failed in bringing into view any genuine physiological truth.

14. That the real cause of the failure in the instances above mentioned lay in the *Conceptions*, is plain. It was not ignorance of the facts which in these cases prevented the discovery of the truth. Aristotle was as well acquainted with the fact of the proportion of the weights which balance on a lever as Archimedes was, although Archimedes alone gave the true mechanical reason for the proportion.

With regard to the doctrine of the four elements indeed, the inapplicability of the conception of composition of qualities, required, perhaps, to be proved by some reference to facts. But this conception was devised at first, and accepted by succeeding times, in a blind and gratuitous manner, which could hardly have happened if men had been awake to the necessary condition of our knowledge;—that the conceptions which we introduce into our doctrines are not arbitrary or accidental notions, but certain peculiar modes of apprehension strictly determined by the subject of our speculations.

15. It may, however, be said that this injunction that we are to employ *appropriate* Conceptions only in the formation of our knowledge, cannot be of practical use, because we can only determine what Ideas *are* appropriate, by finding that they truly combine the facts. And this is to a certain extent true. Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin;—some fortunate cast of intellect, rising above all rules. No maxims can be given which

inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius: nor will an inquirer of truly inventive mind need to come to the teacher of inductive philosophy to learn how to exercise the faculties which nature has given him. Such persons as Kepler or Fresnel, or Brewster, will have their powers of discovering truth little augmented by any injunctions respecting Distinct and Appropriate Ideas; and such men may very naturally question the utility of rules altogether.

16. But yet the opinions which such persons may entertain, will not lead us to doubt concerning the value of the attempts to analyze and methodize the process of discovery. Who would attend to Kepler if he had maintained that the speculations of Francis Bacon were worthless? Notwithstanding what has been said, we may venture to assert that the maxim which points out the necessity of Ideas appropriate as well as clear, for the purpose of discovering truth, is not without its use. It may, at least, have a value as a caution or prohibition, and may thus turn us away from labours certain to be fruitless. We have already seen that this maxim, if duly attended to, would have at once condemned, as wrongly directed, the speculations of physiologists of the mathematical, mechanical, chemical, and vital-fluid schools; since the Ideas which the teachers of these schools introduce, cannot suffice for the purposes of physiology, which seeks truths respecting the vital powers. Again, it is clear from similar considerations that no definition of a mineralogical species by chemical characters alone can answer the end of science, since we seek to make mineralogy, not an analytical but a classificatory science*. Even

* This agrees with what M. Necker has well observed in his "*Règne Mineral*," that those who have treated mineralogy as a merely chemical science, have substituted the analysis of substances for the classification of individuals. See above, B. VIII. chap. iii.

before the appropriate conception is matured in men's minds so that they see clearly what it is, they may still have light enough to see what it is not.

17. Another result of this view of the necessity of appropriate Ideas, combined with a survey of the history of science is, that though for the most part, as we shall see, the progress of science consists in accumulating and combining Facts rather than in debating concerning Definitions; there are still certain periods when the *discussion* of Definitions may be the most useful mode of cultivating some special branch of science. This discussion is of course always to be conducted by the light of facts; and, as has already been said, along with the settlement of every good Definition will occur the corresponding establishment of some Proposition. But still at particular periods, the want of a Definition, or of the clear conceptions which Definition supposes, may be peculiarly felt. A good and tenable Definition of *Species* in Mineralogy would at present be perhaps the most important step which the science could make. A just conception of the nature of *Life*, (and if expressed by means of a Definition, so much the better,) can hardly fail to give its possessor an immense advantage in the speculations which now come under the consideration of physiologists. And controversies respecting Definitions, in these cases, and such as these, may be very far from idle and unprofitable.

Thus the knowledge that Clear and Appropriate Ideas are requisite for discovery, although it does not lead to any very precise precepts, or supersede the value of natural sagacity and inventiveness, may still be of use to us in our pursuit after truth. It may show us what course of research is, in each stage of science, recommended by the general analogy of the history of knowledge; and it may both save us from hopeless and barren paths of speculation, and make us advance with more courage and confidence, to know that we are looking for discoveries in the manner in which they have always hitherto been made.

SECT. V.—*Accidental Discoveries.*

18. Another consequence follows from the views presented in this Chapter, and it is the last I shall at present mention. *No scientific discovery* can, with any justice, be considered *due to accident.* In whatever manner facts may be presented to the notice of a discoverer, they can never become the materials of exact knowledge, except they find his mind already provided with precise and suitable conceptions by which they may be analyzed and connected. Indeed, as we have already seen, facts cannot be observed as Facts, except in virtue of the Conceptions which the observer* himself unconsciously supplies; and they are not Facts of Observation for any purpose of Discovery, except these familiar and unconscious acts of thought be themselves of a just and precise kind. But supposing the Facts to be adequately observed, they can never be combined into any new Truth, except by means of some new Conceptions, clear and appropriate, such as I have endeavoured to characterize. When the observer's mind is prepared with such instruments, a very few facts, or it may be a single one, may bring the process of discovery into action. But in such cases, this previous condition of the intellect, and not the single fact, is really the main and peculiar cause of the success. The fact is merely the occasion by which the engine of discovery is brought into play sooner or later. It is, as I have elsewhere said, only the spark which discharges a gun already loaded and pointed; and there is little propriety in speaking of such an accident as the cause why the bullet hits the mark. If it were true that

* Book I. c. ii.

the fall of an apple was the occasion of Newton's pursuing the train of thought which led to the doctrine of universal gravitation, the habits and constitution of Newton's intellect, and not the apple, were the real source of this great event in the progress of knowledge. The common love of the marvellous, and the vulgar desire to bring down the greatest achievements of genius to our own level, may lead men to ascribe such results to any casual circumstances which accompany them; but no one who fairly considers the real nature of great discoveries, and the intellectual processes which they involve, can seriously hold the opinion of their being the effect of accident.

19. Such accidents never happen to common men. Thousands of men, even of the most inquiring and speculative men, had seen bodies fall; but who, except Newton, ever followed the accident to such consequences? And in fact, how little of his train of thought was contained in, or even directly suggested by, the fall of the apple! If the apple fall, said the discoverer, why should not the moon, the planets, the satellites, fall?" But how much previous thought,—what a steady conception of the universality of the laws of motion gathered from other sources,—were requisite, that the inquirer should see any connexion in these cases! Was it by accident that he saw in the apple an image of the moon, and of every body in the solar system?

20. The same observations may be made with regard to the other cases which are sometimes adduced as examples of accidental discovery. It has been said, "By the accidental placing of a rhomb of calcareous spar upon a book or line Bartholinus discovered the property of the *Double Refraction* of light." But Bartholinus could have seen no such consequence in the accident if he had not previously had a clear conception of *single*

refraction. A lady, in describing an optical experiment which had been shown her, said of her teacher, "He told me to *increase and diminish the angle of refraction*, and at last I found that he only meant me to move my head up and down." At any rate, till the lady had acquired the notions which the technical terms convey, she could not have made Bartholinus's discovery by means of his accident. "By accidentally combining two rhombs in different positions," it is added, "Huyghens discovered the *Polarization* of Light." Supposing that this experiment had been made without design, what Huyghens really observed, was that the images appeared and disappeared alternately as he turned one of the rhombs round. But was it an easy or an obvious business to analyze this curious alternation into the circumstances of the rays of light having *sides*, as Newton expressed it, and into the additional hypotheses which are implied in the term "polarization?" Those will be able to answer this question, who have found how far from easy it is to understand clearly what is meant by "polarization" in this case, now that the property is fully established. Huyghens's success depended on his clearness of thought, for this enabled him to perform the intellectual analysis, which never would have occurred to most men, however often they had "accidentally combined two rhombs in different positions. "By accidentally looking through a prism of the same substance, and turning it round, Malus discovered the polarization of light by reflection." Malus saw that, in some positions of the prism, the light reflected from the windows of the Louvre thus seen through the prism, became dim. A common man would have supposed this dimness the result of accident; but Malus's mind was differently constituted and disciplined. He considered the position of the window, and of the prism; repeated the experiment over and over; and in virtue of the eminently distinct conceptions of space which he possessed, resolved the phenomena into its geometrical conditions. A believer in accident would not have sought them; a person of less clear ideas would not have found them. A person must have a strange confidence in the virtue of chance, and the worthlessness of intellect, who can say that "in all these fundamental discoveries appropriate ideas had no share," and that the discoveries "might have been made by the most ordinary observers."

21. I have now, I trust, shown in various ways, how the *Explication of Conceptions*, including in this term their clear developement from Fundamental Ideas in the discoverer's mind, as well as their precise expression in the form of Definitions or Axioms, when that can be done, is an essential part in the establishment of all exact and general physical truths. In doing this, I have endeavoured to explain in what sense the possession of clear and appropriate ideas is a main requisite for every step in scientific discovery. That it is far from being the only step, I shall soon have to show; and if any obscurity remain on the subject treated of in the present chapter, it will, I hope, be removed when we have examined the other elements which enter into the constitution of our knowledge.

Chapter III.

OF FACTS AS THE MATERIALS OF SCIENCE.

1. We have now to examine how Science is built up by the combination of Facts. In doing this, we suppose that we have already obtained a supply of definite and certain Facts, free from obscurity and doubt. We

must, therefore, first consider under what conditions Facts can assume this character.

When we inquire what Facts are to be made the materials of Science, perhaps the answer which we should most commonly receive would be, that they must be *True Facts*, as distinguished from any mere inferences or opinions of our own. We should probably be told that we must be careful in such a case to consider as Facts, only what we really observe;—that we must assert only what we see; and believe nothing except upon the testimony of our senses.

But such maxims are far from being easy to apply, as a little examination will convince us.

2. It has been explained, in the preceding part of this work, that all perception of external objects and occurrences involves an active as well as a passive process of the mind;—includes not only Sensations, but also Ideas by which Sensations are bound together, and have a unity given to them. From this it follows, that there is a difficulty in separating in our perceptions what we receive from without, and what we ourselves contribute from within;—what we perceive, and what we infer. In many cases, this difficulty is obvious to all: as, for example, when we witness the performances of a juggler or a ventriloquist. In these instances, we imagine ourselves to see and to hear what certainly we do not see and hear. The performer takes advantage of the habits by which our minds supply interruptions and infer connexions; and by giving us fallacious indications, he leads us to perceive as an actual fact, what does not happen at all. In these cases, it is evident that we ourselves assist in making the fact; for we make one which does not really exist. In other cases, though the fact which we perceive be true, we can easily see that a large portion of the perception is our own act; as when, from

the sight of a bird of prey we infer a carcase, or when we read a half-obliterated inscription. In the latter case, the mind supplies the meaning, and perhaps half the letters; yet we do not hesitate to say that we actually *read* the inscription. Thus, in many cases, our own inferences and interpretations enter into our facts. But this happens in many instances in which it is at first sight less obvious. When any one has seen an oak-tree blown down by a strong gust of wind, he does not think of the occurrence any otherwise than as a *Fact* of which he is assured by his senses. Yet by what sense does he perceive the Force which he thus supposes the wind to exert? By what sense does he distinguish an Oak-tree from all other trees? It is clear upon reflection, that in such a case, his own mind supplies the conception of extraneous impulse and pressure, by which he thus interprets the motions observed, and the distinction of different kinds of trees, according to which he thus names the one under his notice. The Idea of Force, and the idea of definite Resemblances and Differences, are thus combined with the impressions on our senses, and form an undistinguished portion of that which we consider as the Fact. And it is evident that we can in no other way perceive Force, than by seeing motion; and cannot give a Name to any object, without not only seeing a difference of single objects, but supposing a difference of classes of objects. When we speak as if we saw impulse and attraction, things and classes, we really see only objects of various forms and colours, more or less numerous, variously combined. But do we really perceive so much as this? When we see the form, the size, the number, the motion of objects, are these really mere impressions on our senses, unmodified by any contribution or operation of the mind itself? A very little attention will suffice to convince us that this is not the case. When we see a windmill turning, it may happen, as we have elsewhere noticed*, that we mistake the direction in which the sails turn: when we look at certain diagrams, they may appear either convex or concave: when we see the moon first in the horizon and afterwards high up in the sky, we judge her to be much larger in the former than in the latter position, although to the eye she subtends the same angle. And in these cases and the like, it has been seen that the errour and confusion which we thus incur arise from the mixture of acts of the mind itself with impressions on the senses. But such acts are, as we have also seen, *inseparable* portions of the process of perception. A certain activity of the mind is involved, not only in seeing objects erroneously, but in seeing them at all. With regard to solid objects, this is generally acknowledged. When we seem to see an edifice occupying space in all dimensions, we really see only a representation of it as it appears referred by perspective to a surface. The inference of the solid form is an operation of our own, alike when we look at a reality and when we look at a picture. But we may go further. Is plane Figure really a mere Sensation? If we look at a decagon, do we see at once that it has ten sides, or is it not necessary for us to count them: and is not counting an act of the mind? All objects are seen in space; all objects are seen as one or many: but are not the Idea of Space and the Idea of Number requisite in order that we may thus apprehend what we see? That these Ideas of Space and Number involve a connexion derived from the mind, and not from the senses, appears, as we have already seen, from this, that those Ideas afford us the materials of universally and necessary truths:—such truths as the senses cannot possibly supply. And thus,

* Book II. c. vi. sect. 6.

even the perception of such facts as the size, shape, and number of objects, cannot be said to be impressions of sense, distinct from all acts of mind, and cannot be expected to be free from errour on the ground of their being mere observed Facts.

Thus the difficulty which we have been illustrating, of distinguishing Facts from inferences and from interpretations of facts, is not only great, but amounts to an impossibility. The separation at which we aimed in the outset of this discussion, and which was supposed to be necessary in order to obtain a firm groundwork for science, is found to be unattainable. We cannot obtain a sure basis of Facts, by rejecting all inferences and judgments of our own, for such inferences and judgments form an unavoidable element in all Facts. We cannot exclude our Ideas from our Perceptions, for our Perceptions involve our Ideas.

3. But still, it cannot be doubted that in selecting the Facts which are to form the foundation of Science, we must reduce them to their most simple and certain form; and must reject everything from which doubt or errour may arise. Now since this, it appears, cannot be done, by rejecting the Ideas which all Facts involve, in what manner are we to conform to the obvious maxim, that the Facts which form the basis of Science must be perfectly definite and certain?

The analysis of facts into Ideas and Sensations, which we have so often referred to, suggests the answer to this inquiry. We are not able, nor need we endeavour, to exclude Ideas from our Facts; but we may be able to discern, with perfect distinctness, the Ideas which we include. We cannot observe any phenomena without applying to them such Ideas as Space and Number, Cause and Resemblance, and usually, several others; but we may avoid applying these Ideas in a wavering or obscure manner, and confounding Ideas with one another. We cannot read any of the inscriptions which nature presents to us, without interpreting them by means of some language which we ourselves are accustomed to speak; but we may make it our business to acquaint ourselves perfectly with the language which we thus employ, and to interpret it according to the rigorous rules of grammar and analogy.

This maxim, that when Facts are employed as the basis of Science, we must distinguish clearly the Ideas which they involve, and must apply these in a distinct and rigorous manner, will be found to be a more precise guide than we might perhaps at first expect. We may notice one or two Rules which flow from it.

4. In the first place, Facts, when used as the materials of physical Science, must be *referred to Conceptions of the Intellect only*, all emotions of fear, admiration, and the like, being rejected or subdued. Thus, the observations of phenomena which are related as portents and prodigies, striking terrour and boding evil, are of no value for purposes of science. The tales of armies seen warring in the sky, the sound of arms heard from the clouds, fiery dragons, chariots, swords seen in the air, may refer to meteorological phenomena; but the records of phenomena observed in the state of mind which these descriptions imply can be of no scientific value. We cannot make the poets our observers.

> Armorum sonitum toto Germania cœlo
> Audiit; insolitis tremuerunt motibus Alpes.
> Vox quoque per lucos vulgo exaudita silentes
> Ingens, et simulacra modis pallentia miris
> Visa sub obscurum noctis: pecudesque locutæ.

The mixture of fancy and emotion with the observation of facts has often disfigured them to an extent which is too familiar to all to need illustration. We have an example of this result, in the manner in which Comets are described in the treatises of the middle ages. In such works, these bodies are regularly distributed into several classes, accordingly as they assume the form of a sword, of a spear, of a cross, and so on. When such resemblances had become matters of interest, the impressions of the senses were governed, not by the rigorous conceptions of form and colour, but by these assumed images; and under these circumstances, we can attach little value to the statement of what was seen.

In all such phenomena, the reference of the objects to the exact Ideas of Space, Number, Position, Motion, and the like, is the first step of Science: and accordingly, this reference was established at an early period in those sciences which made an early progress, as, for instance, astronomy. Yet even in astronomy there appears to have been a period when the predominant conceptions of men in regarding the heavens and the stars pointed to mythical story and supernatural influence, rather than to mere relations of space, time, and motion: and of this primeval condition of those who gazed at the stars, we seem to have remnants in the Constellations, in the mythological Names of the Planets, and in the early prevalence of Astrology. It was only at a later period, when men had begun to measure the places, or at least to count the revolutions of the stars, that astronomy had its birth.

5. And thus we are led to another Rule:—that in collecting Facts which are to be made the basis of Science, the Facts are to be observed, as far as possible, *with reference to place, figure, number, motion*, and the like Conceptions; which, depending upon the Ideas of Space and Time, are the most universal, exact, and simple of our conceptions. It was by early attention to these relations in the case of the heavenly bodies, that

the ancients formed the science of Astronomy: it was by not making precise observations of this kind in the case of terrestrial bodies, that they failed in framing a science of the Mechanics of Motion. They succeeded in Optics as far as they made observations of this nature; but when they ceased to trace the geometrical paths of rays in the actual experiment, they ceased to go forwards in the knowledge of this subject.

6. But we may state a further Rule:—that though these relations of Time and Space are highly important in almost all Facts, we are not to confine ourselves to these: but are to consider the phenomena *with reference to other Conceptions also:* it being always understood that these conceptions are to be made as exact and rigorous as those of geometry and number. Thus the science of Harmonics arose from considering sounds with reference to *Concords* and *Discords;* the science of Mechanics arose from not only observing motions as they take place in Time and Space, but further, referring them to *Force* as their *Cause.* And in like manner, other sciences depend upon other Ideas, which, as I have endeavoured to show, are not less fundamental than those of Time and Space; and like them, capable of leading to rigorous consequences.

7. Thus the Facts which we assume as the basis of Science are to be freed from all the mists which imagination and passion throw round them; and to be separated into those elementary Facts which exhibit simple and evident relations of Time, or Space, or Cause, or some other Ideas equally clear. We resolve the complex appearances which nature offers to us, and the mixed and manifold modes of looking at these appearances which rise in our thoughts, into limited, definite, and clearly-understood portions. This process we may term the *Decomposition of Facts.* It is the beginning

of exact knowledge,—the first step in the formation of all Science. This Decomposition of Facts into Elementary Facts, clearly understood and surely ascertained, must precede all discovery of the laws of nature.

8. But though this step is necessary, it is not infallibly sufficient. It by no means follows that when we have thus decomposed Facts into Elementary Truths of observation, we shall soon be able to combine these, so as to obtain Truths of a higher and more speculative kind. We have examples which show us how far this is from being a necessary consequence of the former step. Observations of the weather, made and recorded for many years, have not led to any general truths, forming a science of Meteorology: and although great numerical precision has been given to such observations by means of barometers, thermometers, and other instruments, still, no general laws regulating the cycles of change of such phenomena have yet been discovered. In like manner the faces of crystals, and the sides of the polygons which these crystals form, were counted, and thus numerical facts were obtained, perfectly true and definite, but still of no value for purposes of science. And when it was discovered what Element of the form of crystals it was important to observe and measure, namely, the Angle made by two faces with each other, this discovery was a step of a higher order, and did not belong to that department, of mere exact observation of manifest Facts, with which we are here concerned.

9. When the Complex Facts which nature offers to us are thus decomposed into Simple Facts, the decomposition, in general, leads to the introduction of *Terms* and Phrases, more or less technical, by which these Simple Facts are described. When Astronomy was thus made a science of measurement, the things measured were soon described as *Hours*, and *Days*, and

Cycles, *Altitude* and *Declination*, *Phases* and *Aspects*. In the same manner, in Music, the concords had names assigned them, as *Diapente*, *Diatessaron*, *Diapason*; in studying Optics, the *Rays* of light were spoken of as having their course altered by *Reflexion* and *Refraction*; and when useful observations began to be made in Mechanics, the observers spoke of *Force*, *Pressure*, *Momentum*, *Inertia*, and the like.

10. When we take phenomena in which the leading Idea is Resemblance, and resolve them into precise component Facts, we obtain some kind of Classification; as, for instance, when we lay down certain Rules by which particular trees, or particular animals are to be known. This is the earliest form of Natural History; and the Classification which it involves is that which corresponds, nearly or exactly, with the usual Names of the objects thus classified.

11. Thus the first attempts to render observation certain and exact, lead to a decomposition of the obvious facts into Elementary Facts, connected by the Ideas of Space, Time, Number, Cause, Likeness, and others: and into a Classification of the Simple Facts, more or less just, and marked by Names either common or technical. Elementary Facts, and Individual Objects, thus observed and classified, form the materials of Science; and any improvement in Classification or Nomenclature, or any discovery of a Connexion among the materials thus accumulated, leads us fairly within the precincts of Science. We must now, therefore, consider the manner in which Science is built up of such materials;—the process by which they are brought into their places, and the texture of the bond which unites and cements them.

CHAPTER IV.

OF THE COLLIGATION OF FACTS.

1. FACTS such as the last Chapter speaks of are, by means of such Conceptions as are described in the preceding Chapter, bound together so as to give rise to those general Propositions of which Science consists. Thus the Facts that the planets revolve about the sun in certain periodic times and at certain distances, are included and connected in Kepler's Law, by means of such Conceptions as the *squares of numbers*, the *cubes of distances*, and the *proportionality* of these quantities. Again the existence of this proportion in the motions of any two planets, forms a set of Facts which may all be combined by means of the Conception of a certain *central accelerating force*, as was proved by Newton. The whole of our physical knowledge consists in the establishment of such propositions; and in all such cases, Facts are bound together by the aid of suitable Conceptions. This part of the formation of our knowledge I have called the *Colligation of Facts*: and we may apply this term to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science. On the steps by which science is thus collected from phenomena we shall proceed now to make a few remarks.

2. Science begins with *Common* Observation of facts, in which we are not conscious of any peculiar discipline or habit of thought exercised in observing. Thus the common perceptions of the appearances and recurrences of the celestial luminaries, were the first steps of Astro-

nomy: the obvious cases in which bodies fall or are supported, were the beginning of Mechanics; the familiar aspects of visible things, were the origin of Optics; the usual distinctions of well-known plants, first gave rise to Botany. Facts belonging to such parts of our knowledge are noticed by us, and accumulated in our memories, in the common course of our habits, almost without our being aware that we are observing and collecting facts. Yet such facts may lead to many scientific truths; for instance, in the first stages of Astronomy (as we have shown in the *History*) such facts lead to Methods of Intercalation and Rules of the Recurrence of Eclipses. In succeeding stages of science, more especial attention and preparation on the part of the observer, and a selection of certain *kinds* of facts, becomes necessary; but there is an early period in the progress of knowledge at which man is a physical philosopher, without seeking to be so, or being aware that he is so.

3. But in all stages of the progress, even in that early one of which we have just spoken, it is necessary, in order that the facts may be fit materials of any knowledge, that they should be decomposed into Elementary Facts, and that these should be observed with precision. Thus, in the first infancy of astronomy, the recurrence of phases of the moon, of places of the sun's rising and setting, of planets, of eclipses, was observed to take place at intervals of certain definite numbers of days, and in a certain exact order; and thus it was, that the observations became portions of astronomical science. In other cases, although the facts were equally numerous, and their general aspect equally familiar, they led to no science, because their exact circumstances were not apprehended. A vague and loose mode of looking at facts very easily observable, left men for a long time under the belief that a body, ten times as heavy as another,

falls ten times as fast;—that objects immersed in water are always magnified, without regard to the form of the surface;—that the magnet exerts an irresistible force;—that crystal is always found associated with ice;—and the like. These and many others are examples how blind and careless man can be, even in observation of the plainest and commonest appearances; and they show us that the mere faculties of perception, although constantly exercised upon innumerable objects, may long fail in leading to any exact knowledge.

4. If we further inquire what was the favourable condition through which some special classes of facts were, from the first, fitted to become portions of science, we shall find it to have been principally this;—that these facts were considered with reference to the Ideas of Time, Number, and Space, which are Ideas possessing peculiar definiteness and precision; so that with regard to them, confusion and indistinctness are hardly possible. The interval from new moon to new moon was always a particular number of days: the sun in his yearly course rose and set near to a known succession of distant objects: the moon's path passed among the stars in a certain order:—these are observations in which mistake and obscurity are not likely to occur, if the smallest degree of attention is bestowed upon the task. To count a number is, from the first opening of man's mental faculties, an operation which no science can render more precise. The relations of space are nearest to those of number in obvious and universal evidence. Sciences depending upon these Ideas arise with the first dawn of intellectual civilization. But few of the other Ideas which man employs in the acquisition of knowledge possess this clearness in their common use. The Idea of *Resemblance* may be noticed, as coming next to those of Space and Number in original precision; and the

Idea of *Cause*, in a certain vague and general mode of application, sufficient for the purposes of common life, but not for the ends of science, exercises a very extensive influence over men's thoughts. But the other Ideas on which science depends, with the Conceptions which arise out of them, are not unfolded till a much later period of intellectual progress; and therefore, except in such limited cases as I have noticed, the observations of common spectators and uncultivated nations, however numerous or varied, are of little or no effect in giving rise to Science.

5. Let us now suppose that, besides common every-day perception of facts, we turn our attention to some other occurrences and appearances, with a design of obtaining from them speculative knowledge. This process is more peculiarly called *Observation*, or, when we ourselves occasion the facts, *Experiment*. But the same remark which we have already made, still holds good here. These facts can be of no value, except they are resolved into those exact Conceptions which contain the essential circumstances of the case. They must be determined, not indeed necessarily, as has sometimes been said, "according to Number, Weight, and Measure;" for, as we have endeavoured to show in the preceding Books*, there are many other Conceptions to which phenomena may be subordinated, quite different from these, and yet not at all less definite and precise. But in order that the facts obtained by observation and experiment may be capable of being used in furtherance of our exact and solid knowledge, they must be apprehended and analyzed according to some Conceptions which, applied for this purpose, give distinct and definite results, such as can be steadily taken hold of and reasoned from; that is, the facts must be referred to Clear and Appro-

* Books V., VI., VII., VIII., IX., X.

priate Ideas, according to the manner in which we have already explained this condition of the derivation of our knowledge. The phenomena of light, when they are such as to indicate sides in the ray, must be referred to the Conception of *polarization*; the phenomena of mixture, when there is an alteration of qualities as well as quantities, must be combined by a Conception of *elementary composition*. And thus, when mere position, and number, and resemblance, will no longer answer the purpose of enabling us to connect the facts, we call in other Ideas, in such cases more efficacious, though less obvious.

6. But how are we, in these cases, to discover such Ideas, and to judge which will be efficacious, in leading to a scientific combination of our experimental data? To this question, we must in the first place answer, that the first and great instrument by which facts, so observed with a view to the formation of exact knowledge, are combined into important and permanent truths, is that peculiar Sagacity which belongs to the genius of a Discoverer; and which, while it supplies those distinct and appropriate Conceptions which lead to its success, cannot be limited by rules, or expressed in definitions. It would be difficult or impossible to describe in words the habits of thought which led Archimedes to refer the conditions of equilibrium on the lever to the Conception of *pressure*, while Aristotle could not see in them anything more than the results of the strangeness of the properties of the circle;—or which impelled Pascal to explain by means of the Conception of the *weight of air*, the facts which his predecessors had connected by the notion of nature's horrour of a vacuum;—or which caused Vitello and Roger Bacon to refer the magnifying power of a convex lens to the bending of the rays of light towards the perpendicular by *refraction*, while

others conceived the effect to result from the matter of medium, with no consideration of its form. These are what are commonly spoken of as felicitous and inexplicable strokes of inventive talent; and such, no doubt, they are. No rules can ensure to us similar success in new cases; or can enable men who do not possess similar endowments, to make like advances in knowledge.

7. Yet still, we may do something in tracing the process by which such discoveries are made; and this it is here our business to do. We may observe that these, and the like discoveries, are not improperly described as happy *Guesses;* and that Guesses, in these as in other instances, imply various suppositions made, of which some one turns out to be the right one. We may, in such cases, conceive the discoverer as inventing and trying many conjectures, till he finds one which answers the purpose of combining the scattered facts into a single rule. The discovery of general truths from special facts is performed, commonly at least, and more commonly than at first appears, by the use of a series of Suppositions, or *Hypotheses,* which are looked at in quick succession, and of which the one which really leads to truth is rapidly detected, and when caught sight of, firmly held, verified, and followed to its consequences. In the minds of most discoverers, this process of invention, trial, and acceptance or rejection of the hypothesis, goes on so rapidly that we cannot trace it in its successive steps. But in some instances, we can do so; and we can also see that the other examples of discovery do not differ essentially from these. The same intellectual operations take place in other cases, although this often happens so instantaneously that we lose the trace of the progression. In the discoveries made by Kepler, we have a curious and memorable exhibition of this process in its details. Thanks to his communicative disposi-

tion, we know that he made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each, before he established the true doctrine, that the planet's path is an ellipse. We know, in like manner, that Galileo made wrong suppositions respecting the laws of falling bodies, and Mariotte, concerning the motion of water in a siphon, before they hit upon the correct view of these cases.

8. But it has very often happened in the history of science, that the erroneous hypotheses which preceded the discovery of the truth have been made, not by the discoverer himself, but by his precursors; to whom he thus owed the service, often an important one in such cases, of exhausting the most tempting forms of errour. Thus the various fruitless suppositions by which Kepler endeavoured to discover the law of refraction, led the way to its real detection by Snell; Kepler's numerous imaginations concerning the forces by which the celestial motions are produced,—his "physical reasonings" as he termed them,—were a natural prelude to the truer physical reasonings of Newton. The various hypotheses by which the suspension of vapour in air had been explained, and their failure, left the field open for Dalton with his doctrine of the mechanical mixture of gases. In most cases, if we could truly analyze the operation of the thoughts of those who make, or who endeavour to make discoveries in science, we should find that many more suppositions pass through their minds than those which are expressed in words; many a possible combination of conceptions is formed and soon rejected. There is a constant invention and activity, a perpetual creating and selecting power at work, of which the last results only are exhibited to us. Trains of hypotheses are called up and pass rapidly in review; and the judgment makes its choice from the varied group.

9. It would, however, be a great mistake to suppose that the hypotheses, among which our choice thus lies, are constructed by an enumeration of obvious cases, or by a wanton alteration of relations which occur in some first hypothesis. It may, indeed, sometimes happen that the proposition which is finally established is such as may be formed, by some slight alteration, from those which are justly rejected. Thus Kepler's elliptical theory of Mars's motions, involved relations of lines and angles much of the same nature as his previous false suppositions: and the true law of refraction so much resembles those erroneous ones which Kepler tried, that we cannot help wondering how he chanced to miss it. But it more frequently happens that new truths are brought into view by the application of new Ideas, not by new modifications of old ones. The cause of the properties of the Lever was learnt, not by introducing any new *geometrical* combination of lines and circles, but by referring the properties to genuine *mechanical* Conceptions. When the Motions of the Planets were to be explained, this was done, not by merely improving the previous notions, of cycles of time, but by introducing the new conception of *epicycles* in space. The doctrine of the Four Simple Elements was expelled, not by forming any new scheme of elements which should impart, according to new rules, their sensible qualities to their compounds, but by considering the elements of bodies as *neutralizing* each other. The Fringes of Shadows could not be explained by ascribing new properties to the single rays of light, but were reduced to law by referring them to the *interference* of several rays.

Since the true supposition is thus very frequently something altogether diverse from all the obvious conjectures and combinations, we see here how far we are from being able to reduce discovery to rule, or to give any precepts by which the want of real invention and sagacity shall be supplied. We may warn and encourage these faculties when they exist, but we cannot create them, or make great discoveries when they are absent.

10. The Conceptions which a true theory requires are very often clothed in a *Hypothesis* which connects with them several superfluous and irrelevant circumstances. Thus the Conception of the Polarization of Light was originally represented under the image of *particles* of light having their poles all turned in the same direction. The Laws of Heat may be made out perhaps most conveniently by conceiving Heat to be a *Fluid*. The Attraction of Gravitation might have been successfully applied to the explanation of facts, if Newton had throughout treated Attraction as the result of an *Ether* diffused through space; a supposition which he has noticed as a possibility. The doctrine of Definite and Multiple Proportions may be conveniently expressed by the hypothesis of *Atoms*. In such cases, the Hypothesis may serve at first to facilitate the introduction of a new Conception. Thus a pervading Ether might for a time remove a difficulty, which some persons find considerable, of imagining a body to exert force at a distance. A Particle with Poles is more easily conceived than Polarization in the abstract. And if hypotheses thus employed will really explain the facts by means of a few simple assumptions, the laws so obtained may afterwards be reduced to a simpler form than that in which they were first suggested. The general laws of Heat, of Attraction, of Polarization, of Multiple Proportions, are now certain, whatever image we may form to ourselves of their ultimate causes.

11. In order, then, to discover scientific truths, suppositions consisting either of new Conceptions, or of new Combinations of old ones, are to be made, till we

find one which succeeds in binding together the Facts. But how are we to find this? How is the trial to be made? What is meant by "success" in these cases? To this we reply, that our inquiry must be, whether the Facts have the same relation in the Hypothesis which they have in reality;—whether the results of our suppositions agree with the phenomena which nature presents to us. For this purpose, we must both carefully observe the phenomena, and steadily trace the consequences of our assumptions, till we can bring the two into comparison. The Conceptions which our hypotheses involve, being derived from certain Fundamental Ideas, afford a basis of rigorous reasoning, as we have shown in the Books respecting those Ideas. And the results to which this reasoning leads, will be susceptible of being verified or contradicted by observation of the facts. Thus the Epicyclical Theory of the Moon, once assumed, determined what the moon's place among the stars ought to be at any given time, and could therefore be tested by actually observing the moon's places. The doctrine that musical strings of the same length, stretched with weights of 1, 4, 9, 16, would give the musical intervals of an octave, a fifth, a fourth, in succession, could be put to the trial by any one whose ear was capable of appreciating those intervals: and the inference which follows from this doctrine by numerical reasoning,—that there must be certain imperfections in the concords of every musical scale,—could in like manner be confirmed by trying various modes of *Temperament*. In like manner all received theories in science, up to the present time, have been established by taking up some supposition, and comparing it, directly or by means of its remoter consequences, with the facts it was intended to embrace. Its agreement, under certain cautions and conditions, of which we may hereafter speak, is held to be the evidence of its truth. It answers its genuine purpose, the Colligation of Facts.

12. When we have, in any subject, succeeded in one attempt of this kind, and obtained some true Bond of Unity by which the phenomena are held together, the subject is open to further prosecution; which ulterior process may, for the most part, be conducted in a more formal and technical manner. The first great outline of the subject is drawn; and the finishing of the resemblance of nature demands a more minute pencilling, but perhaps requires less of genius in the master. In the pursuance of this task, rules and precepts may be given, and features and leading circumstances pointed out, of which it may often be useful to the inquirer to be aware.

Before proceeding further, I shall speak of some characteristic marks which belong to such scientific processes as are now the subject of our consideration, and which may sometimes aid us in determining when the task has been rightly executed.

Chapter V.

OF CERTAIN CHARACTERISTICS OF SCIENTIFIC INDUCTION.

Sect. I.—*Invention a part of Induction.*

1. The two operations spoken of in the preceding chapters,—the Explication of the Conceptions of our own minds, and the Colligation of observed Facts by the aid of such Conceptions,—are, as we have just said, inseparably connected with each other. When united, and employed in collecting knowledge from the phenomena which the world presents to us, they constitute the mental

process of *Induction;* which is usually and justly spoken of as the genuine source of all our *real general knowledge* respecting the external world. And we see, from the preceding analysis of this process into its two constituents, from what origin it derives each of its characters. It is *real*, because it arises from the combination of Real Facts, but it is *general*, because it implies the possession of General Ideas. Without the former, it would not be knowledge of the External World; without the latter, it would not be Knowledge at all. When Ideas and Facts are separated from each other, the neglect of Facts gives rise to empty speculations, idle subtleties, visionary inventions, false opinions concerning the laws of phenomena, disregard of the true aspect of nature: while the want of Ideas leaves the mind overwhelmed, bewildered, and stupified by particular sensations, with no means of connecting the past with the future, the absent with the present, the example with the rule; open to the impression of all appearances, but capable of appropriating none. Ideas are the *Form*, facts the *Material*, of our structure. Knowledge does not consist in the empty mould, or in the brute mass of matter, but in the rightly-moulded substance. Induction gathers general truths from particular facts;—and in her harvest, the corn and the reaper, the solid ears and the binding band, are alike requisite. All our knowledge of nature is obtained by Induction; the term being understood according to the explanation we have now given. And our knowledge is then most complete, then most truly deserves the name of Science, when both its elements are most perfect;—when the Ideas which have been concerned in its formation have, at every step, been clear and consistent;—and when they have, at every step also, been employed in binding together real and certain Facts. Of such Induction, I have already given so many examples and illustrations in the two preceding chapters, that I need not now dwell further upon the subject.

2. Induction is familiarly spoken of as the process by which we collect a *General Proposition* from a number of *Particular Cases:* and it appears to be frequently imagined that the general proposition results from a mere juxta-position of the cases, or at most, from merely conjoining and extending them. But if we consider the process more closely, as exhibited in the cases lately spoken of, we shall perceive that this is an inadequate account of the matter. The particular facts are not merely brought together, but there is a New Element added to the combination by the very act of thought by which they are combined. There is a Conception of the mind introduced in the general proposition, which did not exist in any of the observed facts. When the Greeks, after long observing the motions of the planets, saw that these motions might be rightly considered as produced by the motion of one wheel revolving in the inside of another wheel, these Wheels were Creations of their minds, added to the Facts which they perceived by sense. And even if the wheels were no longer supposed to be material, but were reduced to mere geometrical spheres or circles, they were not the less products of the mind alone,—something additional to the facts observed. The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected, till the discoverer supplies from his own stores a Principle of Connexion. The pearls are there, but they will not hang together till some one provides the String. The distances and periods of the planets were all so many separate facts; by Kepler's Third Law they are connected into a single truth: but the Conceptions which this law involves were supplied by Kepler's mind, and without these, the facts were of no avail. The planets described ellipses round the sun, in

the contemplation of others as well as of Newton; but Newton conceived the deflection from the tangent in these elliptical motions in a new light,—as the effect of a Central Force following a certain law; and then it was, that such a force was discovered truly to exist.

Thus* in each inference made by Induction, there is introduced some General Conception, which is given, not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a New Generality. In order to obtain our inference, we travel beyond the cases which we have before us; we consider them as mere exemplifications of some Ideal Case in which the relations are complete and intelligible. We take a Standard, and measure the facts by it; and this Standard is constructed by us, not offered by Nature. We assert, for example, that a body left to itself will move on with unaltered velocity; not because our senses ever disclosed to us a body doing this, but because (taking this as our Ideal Case) we find that all actual cases are intelligible and explicable by means of the Conception of *Forces*, causing change and motion, and exerted by surrounding bodies. In like manner, we see bodies striking each other, and thus moving and stopping, accelerating and retarding each other: but in all this, we do not perceive by our senses that abstract quantity, *Momentum*, which is always lost by one body as it is gained by another. This Momentum is a creation of the mind, brought in among the facts, in order to convert their apparent confusion into order, their seeming chance into certainty, their perplexing variety into simplicity. This the Conception of *Momentum gained and lost* does: and in like manner, in any other case in which a truth is established by Induction, some

* I repeat here remarks made at the end of the *Mechanical Euclid*, p. 178.

Conception is introduced, some Idea is applied, as the means of binding together the facts, and thus producing the truth.

3. Hence in every inference by Induction, there is some Conception *superinduced* upon the Facts: and we may henceforth conceive this to be the peculiar import of the term *Induction*. I am not to be understood as asserting that the term was originally or anciently employed with this notion of its meaning; for the peculiar feature just pointed out in Induction has generally been over-looked. This appears by the accounts generally given of Induction. "Induction," says Aristotle*, "is when by means of one extreme term† we infer the other extreme term to be true of the middle term." Thus, (to take such exemplifications as belong to our subject,) from knowing that Mercury, Venus, Mars, describe ellipses about the Sun, we infer that all Planets describe ellipses about the Sun. In making this inference syllogistically, we assume that the evident proposition, "Mercury, Venus, Mars, do what all Planets do," may be taken *conversely*, "All Planets do what Mercury, Venus, Mars, do." But we may remark that, in this passage, Aristotle (as was natural in his line of discussion) turns his attention entirely to the *evidence* of the inference; and overlooks a step which is of far more importance to our knowledge, namely, the *invention* of the second extreme term. In the above instance, the particular luminaries, Mercury, Venus, Mars, are one logical *Extreme;* the general designation Planets is the *Middle Term;* but having these before us, how do we come to

* *Analyt. Prior.*, Lib. II. c. 23. Περὶ της επαγωγῆς.

† The syllogism here alluded to would be this:—
Mercury, Venus, Mars, describe ellipses about the Sun;
All Planets do what Mercury, Venus, Mars, do;
Therefore all Planets describe ellipses about the Sun.

think of *description of ellipses*, which is the other Extreme of the syllogism? When we have once invented this "second Extreme Term," we may, or may not, be satisfied with the evidence of the syllogism; we may, or may not, be convinced that, so far as this property goes, the extremes are co-extensive with the middle term*; but the *statement* of the syllogism is the important step in science. We know how long Kepler laboured, how hard he fought, how many devices he tried, before he hit upon this *Term*, the Elliptical Motion. He rejected, as we know, many other "second Extreme Terms," for example, various combinations of epicyclical constructions, because they did not represent with sufficient accuracy the special facts of observation. When he had established his premiss, that "Mars does describe an Ellipse about the Sun," he does not hesitate to *guess* at least that, in this respect, he might *convert* the other premiss, and assert that "All the Planets do what Mars does." But the main business was, the inventing and verifying the proposition respecting the Ellipse. The Invention of the Conception was the great step in the *discovery*; the Verification of the Proposition was the great step in the *proof* of the discovery. If Logic consists in pointing out the conditions of proof, the Logic of Induction must consist in showing what are the conditions of proof, in such inferences as this: but this subject must be pursued in the next chapter; I now speak principally of the act of *Invention*, which is requisite in every inductive inference.

4. Although in every inductive inference, an act of invention is requisite, the act soon slips out of notice. Although we bind together facts by superinducing upon them a new Conception, this Conception, once introduced

* Eἰ οὖν ἀντιστρέφει τὸ Γ τῷ B καὶ μὴ ὑπερτείνει τὸ μέσον.—Aristot. *Ibid.*

and applied, is looked upon as inseparably connected with the facts, and necessarily implied in them. Having once had the phenomena bound together in their minds in virtue of the Conception, men can no longer easily restore them back to the detached and incoherent condition in which they were before they were thus combined. The pearls once strung, they seem to form a chain by their nature. Induction has given them a unity which it is so far from costing us an effort to preserve, that it requires an effort to imagine it dissolved. For instance, we usually represent to ourselves the Earth as *round*, the Earth and the Planets as *revolving* about the Sun, and as *drawn* to the Sun by a Central Force; we can hardly understand how it could cost the Greeks, and Copernicus, and Newton, so much pains and trouble to arrive at a view which is to us so familiar. These are no longer to us Conceptions caught hold of and kept hold of by a severe struggle; they are the simplest modes of conceiving the facts: they are really Facts. We are willing to *own* our obligation to those discoverers, but we hardly *feel* it: for in what other manner (we ask in our thoughts,) could we represent the facts to ourselves?

Thus we see why it is that this step of which we now speak, the Invention of a new Conception in every inductive inference, is so generally overlooked that it has hardly been noticed by preceding philosophers. When once performed by the discoverer, it takes a fixed and permanent place in the understanding of every one. It is a thought which, once breathed forth, permeates all men's minds. All fancy they nearly or quite knew it before. It oft was thought, or almost thought, though never till now expressed. Men accept it and retain it, and know it cannot be taken from them, and look upon it as their own. They will not and cannot part with it, even though they may deem it trivial and obvious. It is a secret, which once uttered, cannot be recalled, even though it be despised by those to whom it is imparted. As soon as the leading term of a new theory has been pronounced and understood, all the phenomena change their aspect. There is a standard to which we cannot help referring them. We cannot fall back into the helpless and bewildered state in which we gazed at them when we possessed no principle which gave them unity. Eclipses arrive in mysterious confusion: the notion of a *Cycle* dispels the mystery. The Planets perform a tangled and mazy dance; but *Epicycles* reduce the maze to order. The Epicycles themselves run into confusion; the conception of an *Ellipse* makes all clear and simple. And thus from stage to stage, new elements of intelligible order are introduced. But this intelligible order is so completely adopted by the human understanding, as to seem part of its texture. Men ask Whether Eclipses follow a Cycle; Whether the Planets describe Ellipses; and they imagine that so long as they do not *answer* such questions rashly, they take nothing for granted. They do not recollect how much they assume in *asking* the question:—how far the conceptions of Cycles and of Ellipses are beyond the visible surface of the celestial phenomena:—how many ages elapsed, how much thought, how much observation, were needed, before men's thoughts were fashioned into the words which they now so familiarly use. And thus they treat the subject, as we have seen Aristotle treating it; as if it were a question, not of invention, but of proof; not of substance, but of form: as if the main thing were not *what* we assert, but *how* we assert it. But for our purpose, it is requisite to bear in mind the feature which we have thus attempted to mark; and to recollect that, in every inference by induction, there is a Conception supplied by the mind and superinduced upon the Facts.

5. In collecting scientific truths by Induction, we often find (as has already been observed), a Definition and a Proposition established at the same time,—introduced together, and mutually dependent on each other. The combination of the two constitutes the Inductive act; and we may consider the Definition as representing the superinduced Conception, and the Proposition as exhibiting the Colligation of Facts.

Sect. II.—*Use of Hypotheses.*

6. To discover a Conception of the mind which will justly represent a train of observed facts is, in some measure, a process of conjecture, as I have stated already; and as I then observed, the business of conjecture is commonly conducted by calling up before our minds several suppositions, and selecting that one which most agrees with what we know of the observed facts. Hence he who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and among the endowments which lead to his success, we must reckon that fertility of invention which ministers to him such imaginary schemes, till at last he finds the one which conforms to the true order of nature. A facility in devising hypotheses, therefore, is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensable to his task. It is, for his purposes, much better that he should be too ready in contriving, too eager in pursuing systems which promise to introduce law and order among a mass of unarranged facts, than that he should be barren of such inventions and hopeless of such success. Accordingly, as we have already noticed, great discoverers have often invented hypotheses which would not answer to all the facts, as well as those which would; and have fancied themselves to have discovered

laws, which a more careful examination of the facts overturned.

The tendencies of our speculative nature*, carrying us onwards in pursuit of symmetry and rule, and thus producing all true theories, perpetually show their vigour by overshooting the mark. They obtain something, by aiming at much more. They detect the order and connexion which exist, by conceiving imaginary relations of order and connexion which have no existence. Real discoveries are thus mixed with baseless assumptions; profound sagacity is combined with fanciful conjecture; not rarely, or in peculiar instances, but commonly, and in most cases; probably in all, if we could read the thoughts of discoverers as we read the books of Kepler. To try wrong guesses is, with most persons, the only way to hit upon right ones. The character of the true philosopher is, not that he never conjectures hazardously, but that his conjectures are clearly conceived, and brought into rigid contact with facts. He sees and compares distinctly the Ideas and the Things;—the relations of his notions to each other and to phenomena. Under these conditions, it is not only excusable, but necessary for him, to snatch at every semblance of general rule,—to try all promising forms of simplicity and symmetry.

Hence advances in knowledge† are not commonly made without the previous exercise of some boldness and license in guessing. The discovery of new truths requires, undoubtedly, minds careful and scrupulous in

* I here take the liberty of characterizing inventive minds in general in the same phraseology which, in the History of Science, I have employed in reference to particular examples. These expressions are what I have used in speaking of the discoveries of Copernicus.—*Hist. Ind. Sci.*, B. v. c. ii.

† These observations are made on occasion of Kepler's speculations, and are illustrated by reference to his discoveries.—*Hist. Ind. Sci.*, B. v. c. iv. sect. 1.

examining what is suggested; but it requires, no less, such as are quick and fertile in suggesting. What is Invention, except the talent of rapidly calling before us the many possibilities, and selecting the appropriate one? It is true, that when we have rejected all the inadmissible suppositions, they are often quickly forgotten; and few think it necessary to dwell on these discarded hypotheses, and on the process by which they were condemned. But all who discover truths, must have reasoned upon many errours to obtain each truth; every accepted doctrine must have been one chosen out of many candidates. If many of the guesses of philosophers of bygone times now appear fanciful and absurd, because time and observation have refuted them, others, which were at the time equally gratuitous, have been confirmed in a manner which makes them appear marvellously sagacious. To form hypotheses, and then to employ much labour and skill in refuting, if they do not succeed in establishing them, is a part of the usual process of inventive minds. Such a proceeding belongs to the *rule* of the genius of discovery, rather than (as has often been taught in modern times) to the *exception*.

7. But if it be an advantage for the discoverer of truth that he be ingenious and fertile in inventing hypotheses which may connect the phenomena of nature, it is indispensably requisite that he be diligent and careful in comparing his hypotheses with the facts, and ready to abandon his invention as soon as it appears that it does not agree with the course of actual occurrences. This constant comparison of his own conceptions and supposition with observed facts under all aspects, forms the leading employment of the discoverer: this candid and simple love of truth, which makes him willing to suppress the most favourite production of his own ingenuity as soon as it appears to be at variance with realities,

constitutes the first characteristic of his temper. He must have neither the blindness which cannot, nor the obstinacy which will not, perceive the discrepancy of his fancies and his facts. He must allow no indolence, or partial views, or self-complacency, or delight in seeming demonstration, to make him tenacious of the schemes which he devises, any further than they are confirmed by their accordance with nature. The framing of hypotheses is, for the inquirer after truth, not the end, but the beginning of his work. Each of his systems is invented, not that he may admire it and follow it into all its consistent consequences, but that he may make it the occasion of a course of active experiment and observation. And if the results of this process contradict his fundamental assumptions, however ingenious, however symmetrical, however elegant his system may be, he rejects it without hesitation. He allows no natural yearning for the offspring of his own mind to draw him aside from the higher duty of loyalty to his sovereign, Truth: to her he not only gives his affections and his wishes, but strenuous labour and scrupulous minuteness of attention.

We may refer to what we have said of Kepler, Newton, and other eminent philosophers, for illustrations of this character. In Kepler we have remarked* the courage and perseverance with which he undertook and executed the task of computing his own hypotheses: and, as a still more admirable characteristic, that he never allowed the labour he had spent upon any conjecture to produce any reluctance in abandoning the hypothesis, as soon as he had evidence of its inaccuracy. And in the history of Newton's discovery that the moon is retained in her orbit by the force of gravity, we have noticed the same moderation in maintaining the hypothesis, after it had once occurred to the author's mind.

* *Hist. Ind. Sci.*, B. v. c. iv. sect. 1.

The hypothesis required that the moon should fall from the tangent of her orbit every second through a space of sixteen feet; but according to his first calculations it appeared that in fact she only fell through a space of thirteen feet in that time. The difference seems small, the approximation encouraging, the theory plausible; a man in love with his own fancies would readily have discovered or invented some probable cause of the difference. But Newton acquiesced in it as a disproof of his conjecture, and "laid aside at that time any further thoughts of this matter*."

8. It has often happened that those who have undertaken to instruct mankind have not possessed this pure love of truth and comparative indifference to the maintenance of their own inventions. Men have frequently adhered with great tenacity and vehemence to the hypotheses which they have once framed; and in their affection for these, have been prone to overlook, to distort, and to misinterpret facts. In this manner, *Hypotheses* have so often been prejudicial to the genuine pursuit of truth, that they have fallen into a kind of obloquy; and have been considered as dangerous temptations and fallacious guides. Many warnings have been uttered against the fabrication of hypotheses by those who profess to teach philosophy; many disclaimers of such a course by those who cultivate science.

Thus we shall find Bacon frequently discommending this habit, under the name of "anticipation of the mind," and Newton thinks it necessary to say emphatically "hypotheses non fingo." It has been constantly urged that the inductions by which sciences are formed must be *cautious* and *rigorous*; and the various imaginations which passed through Kepler's brain, and to which he has given utterance, have been blamed or pitied as la-

* *Hist. Ind. Sci.*, B. VII. c. ii. sect. 3.

mentable instances of an unphilosophical frame of mind. Yet it has appeared in the preceding remarks that hypotheses rightly used are among the helps, far more than the dangers, of science;—that scientific induction is not a "cautious" or a "rigorous" process in the sense of *abstaining from* such suppositions, but in *not adhering to* them till they are confirmed by fact, and in carefully seeking from facts confirmation or refutation. Kepler's character was, not that he was peculiarly given to the construction of hypotheses, but that he narrated with extraordinary copiousness and candour the course of his thoughts, his labours, and his feelings. In the minds of most persons, as we have said, the inadmissible suppositions, when rejected, are soon forgotten: and thus the trace of them vanishes from the thoughts, and the successful hypothesis alone holds its place in our memory. But in reality, many other transient suppositions must have been made by all discoverers;—hypotheses which are not afterwards asserted as true systems, but entertained for an instant;—"tentative hypotheses," as they have been called. Each of these hypotheses is followed by its corresponding train of observations, from which it derives its power of leading to truth. The hypothesis is like the captain, and the observations like the soldiers of an army: while he appears to command them, and in this way to work his own will, he does in fact derive all his power of conquest from their obedience, and becomes helpless and useless if they mutiny.

Since the discoverer has thus constantly to work his way onwards by means of hypotheses, false and true, it is highly important for him to possess talents and means for rapidly *testing* each supposition as it offers itself. In this as in other parts of the work of discovery, success has in general been mainly owing to the native ingenuity and sagacity of the discoverer's mind. Yet some Rules tending to further this object have been delivered by eminent philosophers, and some others may perhaps be suggested. Of these we shall here notice only some of the most general, leaving for a future chapter the consideration of some more limited and detailed processes by which, in certain cases, the discovery of the laws of nature may be materially assisted.

SECT. III.—*Tests of Hypotheses.*

9. A Maxim which it may be useful to recollect is this;—that *hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of errour.* The object of such inventions is to bind together facts which without them are loose and detached; and if they do this, they may lead the way to a perception of the true rule by which the phenomena are associated together, even if they themselves somewhat misstate the matter. The imagined arrangement enables us to contemplate, as a whole, a collection of special cases which perplex and overload our minds when they are considered in succession; and if our scheme has so much of truth in it as to conjoin what is really connected, we may afterwards duly correct or limit the mechanism of this connexion. If our hypothesis renders a reason for the agreement of cases really similar, we may afterwards find this reason to be false, but we shall be able to translate it into the language of truth.

A conspicuous example of such an hypothesis, one which was of the highest value to science, though very incomplete, and as a representation of nature altogether false, is seen in the *Doctrine of epicycles* by which the ancient astronomers explained the motions of the sun, moon, and planets. This doctrine connected the places and velocities of these bodies at particular times in a manner which was, in its general features, agreeable to nature. Yet this doctrine was erroneous in its assertion of the circular nature of all the celestial motions, and in making the heavenly bodies revolve round the earth. It was, however, of immense value to the progress of astronomical science; for it enabled men to express and reason upon many important truths which they discovered respecting the motion of the stars, up to the time of Kepler. Indeed we can hardly imagine that astronomy could, in its outset, have made so great a progress under any other form, as it did in consequence of being cultivated in this shape of the incomplete and false epicyclical hypothesis.

We may notice another instance of an exploded hypothesis, which is generally mentioned only to be ridiculed, and which undoubtedly is both false in the extent of its assertion, and unphilosophical in its expression; but which still, in its day, was not without merit. I mean the doctrine of *Nature's horrour of a vacuum* (*fuga vacui*), by which the action of siphons and pumps and many other phenomena were explained, till Mersenne and Pascal taught a truer doctrine. This hypothesis was of real service; for it brought together many facts which really belong to the same class, although they are very different in their first aspect. A scientific writer of modern times* appears to wonder that men did not at once divine the weight of the air from which the phenomena formerly ascribed to the *fuga vacui* really result. "Loaded, compressed by the atmosphere," he says, "they did not recognize its action. In vain all nature testified that air was elastic and heavy; they shut their eyes to her testimony. The water rose in pumps and flowed in siphons at that time, as it does at this day. They could not separate the boards of a pair of bellows of which the holes were

* Deluc, *Modifications de l'Atmosphere*, Partie I.

stopped; and they could not bring together the same boards without difficulty, if they were at first separated. Infants sucked the milk of their mothers; air entered rapidly into the lungs of animals at every inspiration; cupping-glasses produced tumours on the skin; and in spite of all the striking proofs of the weight and elasticity of the air, the ancient philosophers maintained resolutely that air was light, and explained all these phenomena by the horrour which they said nature had for a vacuum." It is curious that it should not have occurred to the author while writing this, that if these facts, so numerous and various, can all be accounted for by *one* principle, there is a strong presumption that the principle is not altogether baseless. And in reality is it not true that nature *does* abhor a vacuum, and do all she can to avoid it? No doubt this power is not unlimited; and we can trace it to a mechanical cause, the pressure of the circumambient air. But the tendency, arising from this pressure, which the bodies surrounding a space void of air have to rush into it, may be expressed, in no extravagant or unintelligible manner, by saying that nature has a repugnance to a vacuum.

That imperfect and false hypotheses, though they may thus explain *some* phenomena, and may be useful in the progress of science, cannot explain *all* phenomena; —and that we are never to rest in our labours or acquiesce in our results, till we have found some view of the subject which *is* consistent with *all* the observed facts:— will of course be understood. We shall afterwards have to speak of the other steps of such a progress.

10. The hypotheses which we accept ought to explain phenomena which we have observed. But they ought to do more than this: our hypotheses ought to *foretel* phenomena which have not yet been observed;—at least all of the same kind as those which the hypothesis was

invented to explain. For our assent to the hypothesis implies that it is held to be true of all particular instances. That these cases belong to past or to future times, that they have or have not already occurred, makes no difference in the applicability of the rule to them. Because the rule prevails, it includes all cases; and will determine them all, if we can only calculate its real consequences. Hence it will predict the results of new combinations, as well as explain the appearances which have occurred in old ones. And that it does this with certainty and correctness, is one mode in which the hypothesis is to be verified as right and useful.

The scientific doctrines which have at various periods been established have been verified in this manner. For example, the *Epicyclical Theory* of the heavens was confirmed by its *predicting* truly eclipses of the sun and moon, configurations of the planets, and other celestial phenomena; and by its leading to the construction of Tables by which the places of the heavenly bodies were given at every moment of time. The truth and accuracy of these predictions were a proof that the hypothesis was valuable and, at least to a great extent, true; although, as was afterwards found, it involved a false representation of the structure of the heavens. In like manner, the discovery of the *Laws of Refraction* enabled mathematicians to *predict*, by calculation, what would be the effect of any new form or combination of transparent lenses. Newton's hypothesis of *Fits of Easy Transmission and Easy Reflection* in the particles of light, although not confirmed by other kinds of facts, involved a true statement of the law of the phenomena which it was framed to include, and served to *predict* the forms and colours of thin plates for a wide range of given cases. The hypothesis that Light operates by *Undulations* and

Interferences, afforded the means of *predicting* results under a still larger extent of conditions. In like manner in the progress of chemical knowledge, the doctrine of *Phlogiston* supplied the means of *foreseeing* the consequence of many combinations of elements, even before they were tried; but the *Oxygen Theory*, besides affording predictions, at least equally exact, with regard to the general results of chemical operations, included all the facts concerning the relations of weight of the elements and their compounds, and enabled chemists to *foresee* such facts in untried cases. And the Theory of *Electro-magnetic Forces*, as soon as it was rightly understood, enabled those who had mastered it to *predict* motions such as had not been before observed, which were accordingly found to take place.

Men cannot help believing that the laws laid down by discoverers must be in a great measure identical with the real laws of nature, when the discoverers thus determine effects beforehand in the same manner in which nature herself determines them when the occasion occurs. Those who can do this, must, to a considerable extent, have detected nature's secret;—must have fixed upon the conditions to which she attends, and must have seized the rules by which she applies them. Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded. To trace order and law in that which has been observed, may be considered as interpreting what nature has written down for us, and will commonly prove that we understand her alphabet. But to predict what has not been observed, is to attempt ourselves to use the legislative phrases of nature; and when she responds plainly and precisely to that which we thus utter, we cannot but suppose that we have in a great measure made ourselves masters of the meaning and structure of her language. The prediction of results, even of the same kind as those which have been observed, in new cases, is a proof of real success in our inductive processes.

11. We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, when the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides.

Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions*.

It is exemplified principally in some of the greatest discoveries. Thus it was found by Newton that the doctrine of the Attraction of the Sun varying according to the Inverse Square of this distance, which explained Kepler's *Third Law* of the proportionality of the cubes of the distances to the squares of the periodic times of the planets, explained also his *First* and *Second Laws* of the elliptical motion of each planet; although no connexion of these laws had been visible before. Again, it appeared that the force of Universal Gravitation, which had been inferred from the *Perturbations* of the moon and planets by the sun and by each other, also accounted for the fact, apparently altogether dissimilar and remote, of the *Precession of the equinoxes*. Here was a most striking and surprizing coincidence, which gave to the theory a stamp of truth beyond the power of ingenuity to counterfeit. In like manner in Optics; the hypothesis of alternate Fits of easy Transmission and Reflection would explain the colours of thin plates, and indeed was devised and adjusted for that very purpose; but it could give no account of the phenomena of the fringes of shadows. But the doctrine of Interferences, constructed at first with reference to phenomena of the nature of the *Fringes*, explained also the *Colours of thin plates* better than the supposition of the fits invented for that very purpose. And we have in Physical Optics another example of the same kind, which is quite as striking as the explanation of precession by inferences from the facts of perturbation. The doctrine of Undulations propagated in a Spheroidal Form was contrived at first by Huyghens, with a view to explain the laws of *Double Refraction* in calc-spar; and was pursued with the same view by Fresnel. But in the course of the investigation it appeared, in a most unexpected and wonderful manner, that this same doctrine of spheroidal undulations, when it was so modified as to account for the directions of the two refracted rays, accounted also for the positions of their *Planes of Polarization**; a phenomenon which, taken by itself, it had perplexed previous mathematicians, even to represent.

The Theory of Universal Gravitation, and of the Undulatory Theory of Light, are, indeed, full of examples of this Consilience of Inductions. With regard to the

* *Hist. Ind. Sci.*, B. IX. c. xi. sect. 4.

latter, it has been justly asserted by Herschel, that the history of the undulatory theory was a succession of *felicities* *. And it is precisely the unexpected coincidences of results drawn from distant parts of the subject which are properly thus described. Thus the Laws of the *Modification of polarization* to which Fresnel was led by his general views, accounted for the Rule respecting the *Angle at which light is polarized*, discovered by Sir D. Brewster †. The conceptions of the theory pointed out peculiar *Modifications* of the phenomena when *Newton's rings* were produced by polarized light, which modifications were ascertained to take place in fact, by Arago and Airy ‡. When the beautiful phenomena of *Dipolarized light* were discovered by Arago and Biot, Young was able to declare that they were reducible to the general laws of *Interference* which he had already established §. And what was no less striking a confirmation of the truth of the theory, *Measures* of the same element deduced from various classes of facts were found to coincide. Thus the *Length* of a luminiferous undulation, calculated by Young from the measurement of *Fringes* of shadows, was found to agree very nearly with the previous calculation from the colours of *Thin plates* ‖.

No example can be pointed out, in the whole history of science, so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false. If we take one class of facts only, knowing the law which they follow, we may construct an hypothesis, or perhaps several, which may represent them: and as new circumstances are discovered, we may often adjust the hypothesis so as to correspond to these also. But when the hypothesis, of itself and without adjustment for the pur-

* See *Hist. Ind. Sci.*, B. IX. c. xii. † *Ib.*, c. xi. sect. 4.
‡ *Ib.*, c. xiii. sect. 6. § *Ib.*, c. xi. sect. 5. ‖ *Ib.*, c. xi. sect. 2.

pose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet been produced in favour of falsehood.

12. In the preceding Article I have spoken of the hypothesis with which we compare our facts as being framed *all at once*, each of its parts being included in the original scheme. In reality, however, it often happens that the various suppositions which our system contains are *added* upon occasion of different researches. Thus in the Ptolemaic doctrine of the heavens, new epicycles and eccentrics were added as new inequalities of the motions of the heavenly bodies were discovered; and in the Newtonian doctrine of material rays of light, the supposition that these rays had "fits," was added to explain the colours of thin plates; and the supposition that they had "sides" was introduced on occasion of the phenomena of polarization. In like manner other theories have been built up of parts devised at different times.

This being the mode in which theories are often framed, we have to notice a distinction which is found to prevail in the progress of true and of false theories. In the former class all the additional suppositions *tend to simplicity* and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity. In false theories, the contrary is the case. The new suppositions are something altogether additional;—not suggested by the original scheme; perhaps difficult to reconcile with it. Every such addition adds to the complexity of the hypothetical system, which at last becomes unmanageable, and is compelled to surrender its place to some simpler explanation.

Such a false theory, for example, was the ancient doctrine of eccentrics and epicycles. It explained the general succession of the Places of the Sun, Moon, and Planets; it would not have explained the proportion of their Magnitudes at different times, if these could have been accurately observed; but this the ancient astronomers were unable to do. When, however, Tycho and other astronomers came to be able to observe the planets accurately in all positions, it was found that *no* combination of *equable* circular motions would exactly represent all the observations. We may see, in Kepler's works, the many new modifications of the epicyclical hypothesis which offered themselves to him; some of which would have agreed with the phenomena with a certain degree of accuracy, but not so great a degree as Kepler, fortunately for the progress of science, insisted upon obtaining. After these epicycles had been thus accumulated, they all disappeared and gave way to the simpler conception of an *elliptical* motion. In like manner, the discovery of new inequalities in the Moon's motions encumbered her system more and more with new machinery, which was at last rejected all at once in favour of the *elliptical* theory. Astronomers could not but suppose themselves in a wrong path, when the prospect grew darker and more entangled at every step.

Again; the Cartesian system of Vortices might be said to explain the primary phenomena of the revolutions of planets about the sun, and satellites about planets. But the elliptical form of the orbits required new suppositions. Bernoulli ascribed this curve to the shape of the planet, operating on the stream of the vortex in a manner similar to the rudder of a boat. But then the motions of the aphelia, and of the nodes,—the perturbations,—even the action of gravity towards the earth,—could not be accounted for without new and independent suppositions. Here was none of the simplicity of truth. The theory of Gravitation, on the other hand, became more simple as the facts to be explained became more numerous. The attraction of the sun accounted for the motions of the planets; the attraction of the planets was the cause of the motion of the satellites. But this being assumed, the perturbations, the motions of the nodes and aphelia, only made it requisite to extend the attraction of the sun to the satellites, and that of the planets to each other:—the tides, the spheroidal form of the earth, the precession, still required nothing more than that the moon and sun should attract the parts of the earth, and that these should attract each other;—so that all the suppositions resolved themselves into the single one, of the universal gravitation of all matter. It is difficult to imagine a more convincing manifestation of simplicity and unity.

Again, to take an example from another science;—the doctrine of Phlogiston brought together many facts in a very plausible manner,—combustion, acidification, and others,—and very naturally prevailed for a while. But the balance came to be used in chemical operations, and the facts of weight as well as of combination were to be accounted for. On the phlogistic theory, it appeared that this could not be done without a new supposition, and *that*, a very strange one;—that phlogiston was an element not only not heavy, but absolutely light, so that it diminished the weight of the compounds into which it entered. Some chemists for a time adopted this extravagant view; but the wiser of them saw, in the necessity of such a supposition to the defence of the theory, an evidence that the hypothesis of an element *phlogiston* was erroneous. And the opposite hypothesis, which taught that oxygen was subtracted, and not phlogiston added, was accepted because it required no such novel and inadmissible assumption.

Again, we find the same evidence of truth in the progress of the Undulatory Theory of light, in the course of its application from one class of facts to another. Thus we explain Reflection and Refraction by undulations; when we come to Thin Plates, the requisite "fits" are already involved in our fundamental hypothesis, for they are the length of an undulation: the phenomena of Diffraction also require such intervals; and the intervals thus required agree exactly with the others in magnitude, so that no new property is needed. Polarization for a moment appears to require some new hypothesis; yet this is hardly the case; for the direction of our vibrations is hitherto arbitrary:—we allow polarization to decide it, and we suppose the undulations to be transverse. Having done this for the sake of Polarization, we turn to the phenomena of Double Refraction, and inquire what new hypothesis they require. But the answer is, that they require none: the supposition of transverse vibrations, which we have made in order to explain Polarization, gives us also the law of Double Refraction. Truth may give rise to such a coincidence; falsehood cannot. Again, the facts of Dipolarization come into view. But they hardly require any new assumption; for the difference of optical elasticity of crystals in different directions, which is already assumed in uniaxal crystals*, is extended to biaxal exactly according to the law of symmetry; and this being done, the laws of the phenomena, curious and complex as they are, are fully explained. The phenomena of Circular Polarization by internal reflection, instead of requiring a new hypothesis,

* *Hist. Ind. Sci.*, B. IX. c. xi. sect. 5.

are found to be given by an interpretation of an apparently inexplicable result of an old hypothesis. The Circular Polarization of Quartz and its Double Refraction does indeed appear to require a new assumption, but still not one which at all disturbs the form of the theory; and in short, the whole history of this theory is a progress, constant and steady, often striking and startling, from one degree of evidence and consistence to another of higher order.

In the Emission Theory, on the other hand, as in the theory of solid epicycles, we see what we may consider as the natural course of things in the career of a false theory. Such a theory may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition —an addition to the machinery: and as observation goes on, these incoherent appendages accumulate, till they overwhelm and upset the original frame-work. Such has been the hypothesis of the Material Emission of light. In its original form, it explained Reflection and Refraction: but the colours of Thin Plates added to it the Fits of easy Transmission and Reflection; the phenomena of Diffraction further invested the emitted particles with complex laws of Attraction and Repulsion; Polarization gave them Sides: Double Refraction subjected them to peculiar Forces emanating from the axes of the crystal: finally, Dipolarization loaded them with the complex and unconnected contrivance of Moveable Polarization: and even when all this had been done, additional mechanism was wanting. There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters. The philosopher builds the machine, but its parts do not fit. They hold together only while he presses them. This is not the character of truth.

As another example of the application of the Maxim now under consideration, I may perhaps be allowed to refer to the judgment which, in the History of Thermotics, I have ventured to give respecting Laplace's Theory of Gases. I have stated*, that we cannot help forming an unfavourable judgment of this theory, by looking for that great characteristic of true theory; namely, that the hypotheses which were assumed to account for *one class* of facts are found to explain *another class* of a different nature. Thus Laplace's firs suppositions explain the connexion of Compression with Density, (the law of Boyle and Mariotte,) and the connexion of Elasticity with Heat, (the law of Dalton and Gay Lussac.) But the theory requires other assumptions when we come to Latent Heat; and yet these new assumptions produce no effect upon the calculations in any application of the theory. When the hypothesis, constructed with reference to the Elasticity and Temperature, is applied to another class of facts, those of Latent Heat, we have no Simplification of the Hypothesis, and therefore no evidence of the truth of the theory.

13. The two last sections of this chapter direct our attention to two circumstances, which tend to prove, in a manner which we may term irresistible, the truth of the theories which they characterize:—the *Consilience of Inductions* from different and separate classes of facts;—and the progressive *Simplification of the Theory* as it is extended to new cases. These two Characters are, in fact, hardly different; they are exemplified by the same cases. For if these Inductions, collected from one class of facts, supply an unexpected explanation of a new class, which is the case first spoken of, there will be no need for new machinery in the hypothesis to apply it to the newly-contemplated facts; and thus, we have a case in

* *Hist. Ind. Sci.*, B. x. c. iv.

which the system does not become more complex when its application is extended to a wider field, which was the character of true theory in its second aspect. The Consiliences of our Inductions give rise to a constant Convergence of our Theory towards Simplicity and Unity.

But, moreover, both these cases of the extension of the theory, without difficulty or new suppositions, to a wider range and to new classes of phenomena, may be conveniently considered in yet another point of view; namely, as successive steps by which we gradually ascend in our speculative views to a higher and higher point of generality. For when the theory, either by the concurrence of two indications, or by an extension without complication, has included a new range of phenomena, we have, in fact, a new induction of a more general kind, to which the inductions formerly obtained are subordinate, as particular cases to a general proposition. We have in such examples, in short, an instance of *successive generalization*. This is a subject of great importance, and deserving of being well illustrated; it will come under our notice in the next chapter.

Chapter VI.

OF THE LOGIC OF INDUCTION.

1. The subject to which the present chapter refers is described by phrases which are at the present day familiarly used in speaking of the progress of knowledge. We hear very frequent mention of *ascending from particular to general* propositions, and from these to propositions still more general;—of truths *included* in other truths of a higher degree of generality;—of different *stages of generalization*;—and of the *highest step* of the

process of discovery, to which all others are subordinate and preparatory. As these expressions, so familiar to our ears, especially since the time of Francis Bacon, denote, very significantly, processes and relations which are of great importance in the formation of science, it is necessary for us to give a clear account of them, illustrated with general exemplifications; and this we shall endeavour to do.

We have, indeed, already explained that science consists of propositions which include the facts from which they were collected; and other wider propositions, collected in like manner from the former, and including them. Thus, that the stars, the moon, the sun, rise, culminate, and set, are facts *included* in the proposition that the heavens, carrying with them all the celestial bodies, have a diurnal revolution about the axis of the earth. Again, the observed monthly motions of the moon, and the annual motions of the sun, are *included* in certain propositions concerning the movements of those luminaries with respect to the stars. But all these propositions are really *included* in the doctrine that the earth, revolving on its axis, moves round the sun, and the moon round the earth. These movements, again, considered as facts, are explained and *included* in the statement of the forces which the earth exerts upon the moon, and the sun upon the earth. Again, this doctrine of the forces of these two bodies is *included* in the assertion, that all the bodies of the solar system, and all parts of matter, exert forces, each upon each. And we might easily show that all the leading facts in astronomy are comprehended in the same generalization. In like manner with regard to any other science, so far as its truths have been well established and fully developed, we might show that it consists of a gradation of propositions, proceeding from the most special facts to the most general theoretical assertions. We shall exhibit this gradation in some of the principal branches of science.

2. This gradation of truths, successively included in other truths, may be conveniently represented by *Tables* resembling the genealogical tables by which the derivation of descendants from a common ancestor is exhibited; except that it is proper in this case to invert the form of the Table, and to make it converge to unity downwards instead of upwards, since it has for its purpose to express, not the derivation of many from one, but the collection of one truth from many things. Two or more co-ordinate facts or propositions may be ranged side by side, and joined by some mark of connexion, (a bracket, as ⏟ or ⎵,) beneath which may be placed the more general proposition which is collected by induction from the former. Again, propositions co-ordinate with this more general one may be placed on a level with it; and the combination of these, and the result of the combination, may be indicated by brackets in the same manner; and so on, through any number of gradations. By this means the streams of knowledge from various classes of facts will constantly run together into a smaller and smaller number of channels; like the confluent rivulets of a great river, coming together from many sources, uniting their ramifications so as to form larger branches, these again uniting in a single trunk. The *genealogical tree* of each great portion of science, thus formed, will contain all the leading truths of the science arranged in their due co-ordination and subordination. Such Tables, constructed for the sciences of Astronomy and of Optics, will be given at the end of this chapter.

3. The union of co-ordinate propositions into a proposition of a higher order, which occurs in this Tree of Science wherever two twigs unite in one branch, is, in each case, an example of *Induction*. The single propo-

sition is collected by the process of induction from its several members. But here we may observe, that the image of a mere *union* of the parts at each of these points, which the figure of a tree or a river presents, is very inadequate to convey the true state of the case; for in Induction, as we have seen, besides mere collection of particulars, there is always a *new conception*, a principle of connexion and unity, supplied by the mind, and superinduced upon the particulars. There is not merely a juxta-position of materials, by which the new proposition contains all that its component parts contained; but also a formative act exerted by the understanding, so that these materials are contained in a new shape. We must remember, therefore, that our Inductive Tables, although they represent the elements and the order of these inductive steps, do not fully represent the whole signification of the process in each case.

4. The principal features of the progress of science spoken of in the last chapter are clearly exhibited in these Tables; namely, the *Consilience of Inductions*, and the constant Tendency to Simplicity observable in true theories. Indeed in all cases in which from propositions of considerable generality, propositions of a still higher degree are obtained, there is a convergence of inductions; and if in one of the lines which thus converge, the steps be rapidly and suddenly made in order to meet the other line, we may consider that we have an example of Consilience. Thus when Newton had collected from Kepler's Laws the Central Force of the sun, and from these, combined with other facts, the Universal Force of all the heavenly bodies, he suddenly turned round to include in his generalization the Precession of the Equinoxes, which he declared to arise from the attraction of the sun and moon upon the protuberant part of the terrestrial spheroid. The apparent remoteness of this fact, in its nature, from the others with which he thus asso-

ciated it, causes this part of his reasoning to strike us as a remarkable example of *Consilience*. Accordingly, in the Table of Astronomy we find that the columns which contain the facts and theories relative to the *sun* and *planets*, after exhibiting several stages of induction within themselves, are at length suddenly connected with a column till then quite distinct, containing the *precession of the equinoxes*. In like manner, in the Table of Optics, the columns which contain the facts and theories relative to *double refraction*, and those which include *polarization by crystals*, each go separately through several stages of induction; and then these two sets of columns are suddenly connected by Fresnel's mathematical induction that double refraction and polarization arise from the same cause: thus exhibiting a remarkable *Consilience*.

5. The constant *Tendency to Simplicity* in the sciences of which the progress is thus represented, appears from the form of the Table itself; for the single trunk into which all the branches converge, contains in itself the substance of all the propositions by means of which this last generalization was arrived at. It is true, that this ultimate result is sometimes not so simple as in the Table it appears: for instance, the ultimate generalization of the Table exhibiting the progress of Physical Optics,—namely, that Light consists in Undulations,—must be understood as including some other hypotheses; as, that the undulations are transverse, that the ether through which they are propagated has its elasticity in crystals and other transparent bodies regulated by certain laws; and the like. Yet still, even acknowledging all the complication thus implied, the Table in question evidences clearly enough the constant advance towards unity, consistency, and simplicity, which have marked the progress of this Theory. The same is the case in the Inductive Table of Astronomy in a still greater degree.

6. These Tables naturally afford the opportunity of

assigning to each of the distinct steps of which the progress of science consists, the name of the *Discoverer* to whom it is due. Every one of the inductive processes which the brackets of our Tables mark, directs our attention to some person by whom the induction was first distinctly made. These names I have endeavoured to put in their due places in the Tables; and the Inductive Tree of our knowledge in each science becomes, in this way, an exhibition of the claims of each discoverer to distinction, and, as it were, a Genealogical Tree of scientific nobility. It is by no means pretended that such a tree includes the names of all the meritorious labourers in each department of science. Many persons are most usefully employed in collecting and verifying truths, who do not advance to any new truths. The labours of a number of such are included in each stage of our ascent. But such Tables as we have now before us will present to us the names of all the most eminent discoverers: for the main steps of which the progress of science consists, are transitions from more particular to more general truths, and must therefore be rightly given by these Tables; and those must be the greatest names in science to whom the principal events of its advance are thus due.

7. The Tables, as we have presented them, exhibit the course by which we pass from particular to general through various gradations, and so to the most general. They display the order of *discovery*. But by reading them in an inverted manner, beginning at the single comprehensive truths with which the Tables end, and tracing these back into the more partial truths, and these again into special facts, they answer another purpose;—they exhibit the process of *verification* of discoveries once made. For each of our general propositions is true in virtue of the truth of the narrower propositions which it

involves; and we cannot satisfy ourselves of its truth in any other way than by ascertaining that these its constituent elements are true. To assure ourselves that the sun attracts the planets with forces varying inversely as the square of the distance, we must analyze by geometry the motion in an ellipse about the focus, so as to see that it does imply such a force. We must also verify those calculations by which the observed places of each planet are stated to be included in an ellipse. These calculations involve assumptions respecting the path which the earth describes about the sun, which assumptions must again be verified by reference to observation. And thus, proceeding from step to step, we resolve the most general truths into their constituent parts; and these again into their parts; and by testing, at each step, both the reality of the asserted ingredients and the propriety of the conjunction, we establish the whole system of truths, however wide and various it may be.

8. It is a very great advantage, in such a mode of exhibiting scientific truths, that it resolves the verification of the most complex and comprehensive theories, into a number of small steps, of which almost any one falls within the reach of common talents and industry. That *if* the particulars of any one step be true, the generalization also is true, any person with a mind properly disciplined may satisfy himself by a little study. That each of these particular propositions *is* true, may be ascertained, by the same kind of attention, when this proposition is resolved into *its* constituent and more special propositions. And thus we may proceed, till the most general truth is broken up into small and manageable portions. Of these portions, each may appear by itself narrow and easy; and yet they are so woven together, by hypothesis and conjunction, that the truth of the parts necessarily assures us of the truth of the whole.

The verification is of the same nature as the verification of a large and complex statement of great sums received by a mercantile office on various accounts from many quarters. The statement is separated into certain comprehensive heads, and these into others less extensive; and these again into smaller collections of separate articles, each of which can be inquired into and reported on by separate persons. And thus at last, the mere addition of numbers performed by these various persons, and the summation of the results which they obtain, executed by other accountants, is a complete and entire security that there is no error in the whole of the process.

9. This comparison of the process by which we verify scientific truth to the process of Book-keeping in a large commercial establishment, may appear to some persons not sufficiently dignified for the subject. But, in fact, the possibility of giving this formal and business-like aspect to the evidence of science, as involved in the process of successive generalization, is an inestimable advantage. For if no one could pronounce concerning a wide and profound theory except he who could at once embrace in his mind the whole range of inference, extending from the special facts up to the most general principles, none but the greatest geniuses would be entitled to judge concerning the truth or errour of scientific discoveries. But, in reality, we seldom need to verify more than one or two steps of such discoveries at one time; and this may commonly be done (when the discoveries have been fully established and developed,) by any one who brings to the task clear conceptions and steady attention. The progress of science is gradual: the discoveries which are successively made, are also verified successively. We have never any very large collections of them on our hands at once. The doubts

and uncertainties of any one who has studied science with care and perseverance are generally confined to a few points. If he can satisfy himself upon these, he has no misgivings respecting the rest of the structure; which has indeed been repeatedly verified by other persons in like manner. The fact that science is capable of being resolved into separate processes of verification, is that which renders it possible to form a great body of scientific truth, by adding together a vast number of truths, of which many men, at various times and by multiplied efforts, have satisfied themselves. The treasury of Science is constantly rich and abundant, because it accumulates the wealth which is thus gathered by so many, and reckoned over by so many more: and the dignity of Knowledge is no more lowered by the multiplicity of the tasks on which her servants are employed, and the narrow field of labour to which some confine themselves, than the rich merchant is degraded by the number of offices which it is necessary for him to maintain, and the minute articles of which he requires an exact statement from his accountants.

10. The analysis of doctrines inductively obtained, into their constituent facts, and the arrangement of them in such a form that the conclusiveness of the induction may be distinctly seen, may be termed *the Logic of Induction.* By *Logic* has generally been meant a system which teaches us so to arrange our reasonings that their truth or falsehood shall be evident in their form. In *deductive* reasonings, in which the general principles are assumed, and the question is concerning their application and combination in particular cases, the device which thus enables us to judge whether our reasonings are conclusive, is the *Syllogism;* and this *form,* along with the rules which belong to it, does in fact supply us with a criterion of deductive or demonstrative reasoning.

The *Inductive Table*, such as it is presented in the present chapter, in like manner supplies the means of ascertaining the truth of our *inductive* inferences, so far as the *form* in which our reasoning may be stated can afford such a criterion. Of course some care is requisite in order to reduce a train of demonstration into the form of a series of syllogisms; and certainly not less thought and attention are required for resolving all the main doctrines of any great department of science into a graduated table of co-ordinate and subordinate inductions. But in each case, when this task is once executed, the evidence or want of evidence of our conclusions appears immediately in a most luminous manner. In each step of induction, our Table enumerates the particular facts, and states the general theoretical truth which includes these and which these constitute. The special act of attention by which we satisfy ourselves that the facts *are* so included,—that the general truth *is* so constituted,—then affords little room for errour, with moderate attention and clearness of thought.

11. We may find an example of this *act of attention* thus required, at any one of the steps of induction in our Tables; for instance, at the step in the early progress of astronomy at which it was inferred, that the earth is a globe, and that the sphere of the heavens performs a diurnal revolution round this globe of the earth. How was this established in the belief of the Greeks, and how is it fixed in our conviction? As to the globular form, we find that as we travel to the north, the apparent pole of the heavenly motions, and the constellations which are near it, seem to mount higher, and as we proceed southwards they descend. Again, if we proceed from two different points considerably to the east and west of each other, and travel directly northwards from each, as from the south of Spain to the north of Scotland, and

from Greece to Scandinavia, these two north and south lines will be much nearer to each other in their northern than in their southern parts. These and similar facts, as soon as they are clearly estimated and connected in the mind, are *seen to be consistent* with a convex surface of the earth, and with no other: and this notion is further confirmed by observing that the boundary of the earth's shadow upon the moon is always circular; it being supposed to be already established that the moon receives her light from the sun, and that lunar eclipses are caused by the interposition of the earth. As for the assertion of the diurnal revolution of the starry sphere, it is merely putting the visible phenomena in an exact geometrical form: and thus we establish and verify the doctrine of the revolution of the sphere of the heavens about the globe of the earth, by contemplating it so as to *see* that it does really and exactly include the particular facts from which it is collected.

We may, in like manner, illustrate this mode of verification by any of the other steps of the same Table. Thus if we take the great Induction of Copernicus, the heliocentric scheme of the solar system, we find it in the Table exhibited as including and explaining, *first*, the diurnal revolution just spoken of; *second*, the motions of the moon among the fixed stars; *third*, the motions of the planets with reference to the fixed stars and the sun; *fourth*, the motion of the sun in the ecliptic. And the scheme being clearly conceived, we *see* that all the particular facts *are* faithfully represented by it; and this agreement, along with the simplicity of the scheme, in which respect it is so far superior to any other conception of the solar system, persuade us that it is really the plan of nature.

In exactly the same way, if we attend to any of the several remarkable discoveries of Newton, which form the principal steps in the latter part of the Table, as for instance, the proposition that the sun attracts all the planets with a force which varies inversely as the square of the distance, we find it proved by its including three other propositions previously established;—*first*, that the sun's mean force on different planets follows the specified variation (which is proved from Kepler's third law); *second*, that the force by which each planet is acted upon in different parts of its orbit tends to the sun (which is proved by the equable description of areas); *third*, that this force in different parts of the same orbit is also inversely as the square of the distance (which is proved from the elliptical form of the orbit). And the Newtonian generalization, when its consequences are mathematically traced, is *seen* to agree with each of these particular propositions, and thus is fully established.

12. But when we say that the more general proposition *includes* the several more particular ones, we must recollect what has before been said, that these particulars form the general truth, not by being merely enumerated and added together, but by being seen *in a new light*. No mere verbal recitation of the particulars can decide whether the general proposition is true; a special act of thought is requisite in order to determine how truly each is included in the supposed induction. In this respect the Inductive Table is not like a mere schedule of accounts, where the rightness of each part of the reckoning is tested by mere addition of the particulars. On the contrary, the Inductive truth is never the mere *sum* of the facts. It is made into something more by the introduction of a new mental element; and the mind, in order to be able to supply this element, must have peculiar endowments and discipline. Thus looking back at the instances noticed in the last article, how are we to see that a convex surface of the earth is

necessarily implied by the convergence of meridians towards the north, or by the visible descent of the north pole of the heavens as we travel south? Manifestly the student, in order to see this, must have clear conceptions of the relations of space, either naturally inherent in his mind, or established there by geometrical cultivation,—by studying the properties of circles and spheres. When he is so prepared, he will feel the force of the expressions we have used, that the facts just mentioned are *seen to be consistent* with a globular form of the earth; but without such aptitude he will not see this consistency: and if this be so, the mere assertion of it in words will not avail him in satisfying himself of the truth of the proposition.

In like manner, in order to perceive the force of the Copernican induction, the student must have his mind so disciplined by geometrical studies, or otherwise, that he sees clearly how absolute motion and relative motion would alike produce apparent motion. He must have learnt to cast away all prejudices arising from the seeming fixity of the earth; and then he will see that there is nothing which stands in the way of the induction, while there is much which is on its side. And in the same manner the Newtonian induction of the law of the sun's force from the elliptical form of the orbit, will be evidently satisfactory to him only who has such an insight into Mechanics as to see that a curvilinear path must arise from a constantly deflecting force; and who is able to follow the steps of geometrical reasoning by which, from the properties of the ellipse, Newton proves this deflection to be in the proportion in which he asserts the force to be. And thus in all cases the inductive truth must indeed be verified by comparing it with the particular facts; but then this comparison is possible for him only whose mind is properly disciplined and prepared in the use of those conceptions, which, in addition to the facts, the act of induction requires.

13. In the Tables some indication is given, at several of the steps, of the act which the mind must thus perform, besides the mere conjunction of facts, in order to attain to the inductive truth. Thus in the cases of the Newtonian inductions just spoken of, the inferences are stated to be made "By Mechanics;" and in the case of the Copernican induction, it is said that, "By the nature of motion, the apparent motion is the same, whether the heavens or the earth have a diurnal motion; and the latter is more simple." But these verbal statements are to be understood as mere hints*: they cannot supersede the necessity of the student's contemplating for himself the mechanical principles and the nature of motion thus referred to.

14. In the Common or Syllogistic Logic, a certain *Formula* of language is used in stating the reasoning, and is useful in enabling us more readily to apply the Criterion of Form to alleged demonstrations. This formula is the usual Syllogism; with its members, Major Premiss, Minor Premiss, and Conclusion. It may naturally be asked whether in Inductive Logic there is any such Formula? whether there is any standard form of words in which we may most properly express the inference of a general truth from particular facts?

At first it might be supposed that the formula of Inductive Logic need only be of this kind: "These particulars, and all known particulars of the same kind, are exactly included in the following general proposition." But a moment's reflection on what has just been said will show us that this is not sufficient: for the particulars are not merely *included* in the general proposition. It

* In the Inductive Tables they are marked by an asterisk

is not enough that they appertain to it by enumeration. It is, for instance, no adequate example of Induction to say, "Mercury describes an elliptical path, so does Venus, so do the Earth, Mars, Jupiter, Saturn, Uranus; therefore all the Planets describe elliptical paths." This is, as we have seen, the mode of stating the *evidence* when the proposition is once suggested; but the Inductive step consists in the *suggestion* of a conception not before apparent. When Kepler, after trying to connect the observed places of the planet Mars in many other ways, found at last that the conception of an ellipse would include them all, he obtained a truth by induction: for this conclusion was not obviously included in the phenomena, and had not been applied to these facts previously. Thus in our Formula, besides stating that the particulars are included in the general proposition, we must also imply that the generality is constituted by a new Conception,—new at least in its application.

Hence our Inductive Formula might be something like the following: "These particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition." It is of course requisite that the Conceptions should be perfectly clear, and should precisely embrace the facts, according to the explanation we have already given of those conditions.

15. It may happen, as we have already stated, that the Explication of a Conception, by which it acquires its due distinctness, leads to a Definition, which Definition may be taken as the summary and total result of the intellectual efforts to which this distinctness is due. In such cases, the Formula of Induction may be modified according to this condition; and we may state the inference by saying, after an enumeration and analysis of the appropriate facts, "These facts are completely and dis-

tinctly expressed by adopting the following Definition and Proposition."

This Formula has been adopted in stating the Inductive Propositions which constitute the basis of the science of Mechanics, in a work intitled *The Mechanical Euclid.* The fundamental truths of the subject are expressed in *Inductive Pairs* of Assertions, consisting each of a Definition and a Proposition, such as the following:

DEF.—A *Uniform Force* is that which acting in the direction of the body's motion, adds or subtracts equal velocities in equal times.

PROP.—Gravity is a Uniform Force.

Again,

DEF.—Two *Motions* are *compounded* when each produces its separate effect in a direction parallel to itself.

PROP.—When any Force acts upon a body in motion, the motion which the Force would produce in the body at rest is compounded with the previous motion of the body.

And in like manner in other cases.

In these cases the proposition is, of course, established, and the definition realized, by an enumeration of the facts. And in the case of inferences made in such a form, the Definition of the Conception and the Assertion of the Truth are both requisite and are correlative to one another. Each of the two steps contains the verification and justification of the other. The Proposition derives its meaning from the Definition; the Definition derives its reality from the Proposition. If they are separated, the Definition is arbitrary or empty, the Proposition vague or ambiguous.

16. But it must be observed that neither of the preceding Formulæ expresses the full cogency of the inductive proof. They declare only that the results *can* be

clearly explained and rigorously deduced by the employment of a certain Definition and a certain Proposition. But in order to make the conclusion demonstrative, which in perfect examples of Induction it is, we ought to be able to declare that the results can be clearly explained and rigorously declared *only* by the Definition and Proposition which we adopt. And in reality, the conviction of the sound inductive reasoner does reach to this point. The Mathematician asserts the Laws of Motion, seeing clearly that they (or laws equivalent to them) afford the only means of clearly expressing and deducing the actual facts. But this conviction, that the inductive inference is not only consistent with the facts, but necessary, finds its place in the mind gradually, as the contemplation of the consequences of the proposition, and the various relations of the facts, becomes steady and familiar. It is scarcely possible for the student at once to satisfy himself that the inference is thus inevitable. And when he arrives at this conviction, he sees also, in many cases at least, that there may be other ways of expressing the substance of the truth established, besides that special Proposition which he has under his notice.

We may, therefore, without impropriety, renounce the undertaking of conveying in our formula this final conviction of the necessary truth of our inference. We may leave it to be thought, without insisting upon saying it, that in such cases what *can* be true, *is* true. But if we wish to express the ultimate significance of the Inductive Act of thought, we may take as our Formula for the Colligation of Facts by Induction, this:—"The several Facts are exactly expressed as one Fact if, *and only if*, we adopt the Conception and the Assertion" of the inductive inference.

17. I have said that the mind must be properly dis-

ciplined in order that it may see the necessary connexion between the facts and the general proposition in which they are included. And the perception of this connexion, though treated as *one step* in our inductive inference, may imply *many steps* of demonstrative proof. The connexion is this, that the particular case is included in the general one, that is, may be *deduced* from it: but this deduction may often require many links of reasoning. Thus in the case of the inference of the law of the force from the elliptical form of the orbit by Newton, the proof that in the ellipse the deflection from the tangent is inversely as the square of the distance from the focus of the ellipse, is a ratiocination consisting of several steps, and involving several properties of Conic Sections; these properties being supposed to be previously established by a geometrical system of demonstration on the special subject of the Conic Sections. In this and similar cases the Induction involves many steps of Deduction. And in such cases, although the Inductive Step, the Invention of the Conception, is really the most important, yet since, when once made, it occupies a familiar place in men's minds; and since the Deductive Demonstration is of considerable length and requires intellectual effort to follow it at every step; men often admire the deductive part of the proposition, the geometrical or algebraical demonstration, far more than that part in which the philosophical merit really resides.

18. Deductive reasoning is virtually a collection of syllogisms, as has already been stated; and in such reasoning, the general principles, the Definitions and Axioms, necessarily stand at the *beginning* of the demonstration. In an inductive inference, the Definitions and Principles are the *final result* of the reasoning, the ultimate effect of the proof. Hence when an Inductive Proposition is to be established by a proof involving several steps of

demonstrative reasoning, the enunciation of the Proposition will contain, explicitly or implicitly, principles which the demonstration proceeds upon as axioms, but which are really inductive inferences. Thus in order to prove that the force which retains a planet in an ellipse varies inversely as the square of the distance, it is taken for granted that the Laws of Motion are true, and that they apply to the planets. Yet the doctrine that this is so, as well as the law of the force, were established only by this and the like demonstrations. The doctrine which is the *hypothesis* of the deductive reasoning, is the *inference* of the inductive process. The special facts which are the basis of the inductive inference, are the conclusion of the train of deduction. And in this manner the deduction establishes the induction. The principle which we gather from the facts is true, because the facts can be derived from it by rigorous demonstration. Induction moves upwards, and deduction downwards, on the same stair.

But still there is a great difference in the character of their movements. Deduction descends steadily and methodically, step by step: Induction mounts by a leap which is out of the reach of method. She bounds to the top of the stair at once; and then it is the business of Deduction, by trying each step in order, to establish the solidity of her companion's footing. Yet these must be processes of the same mind. The Inductive Intellect makes an assertion which is subsequently justified by demonstration; and it shows its sagacity, its peculiar character, by enunciating the proposition when as yet the demonstration does not exist: but then it shows that it *is* sagacity, by also producing the demonstration.

It has been said that inductive and deductive reasoning are contrary in their scheme; that in Deduction we infer particular from general truths; while in Induction we infer general from particular: that Deduction consists of many steps, in each of which we apply known general propositions in particular cases; while in Induction we have a single step, in which we pass from many particular truths to one general proposition. And this is truly said; but though contrary in their motions, the two are the operation of the same mind travelling over the same ground. Deduction is a necessary part of Induction. Deduction justifies by calculation what Induction had happily guessed. Induction recognizes the ore of truth by its weight; Deduction confirms the recognition by chemical analysis. Every step of Induction must be confirmed by rigorous deductive reasoning, followed into such detail as the nature and complexity of the relations (whether of quantity or any other) render requisite. If not so justified by the supposed discoverer, it is *not* Induction.

19. Such Tabular arrangements of propositions as we have constructed may be considered as the *Criterion of Truth* for the doctrines which they include. They are the Criterion of Inductive Truth, in the same sense in which Syllogistic Demonstration is the Criterion of Necessary Truth,—of the certainty of conclusions, depending upon evident First Principles. And that such Tables are really a Criterion of the truth of the propositions which they contain, will be plain by examining their structure. For if the connexion which the inductive process assumes be ascertained to be in each case real and true, the assertion of the general proposition merely collects together ascertained truths; and in like manner each of those more particular propositions is true, because it merely expresses collectively more special facts: so that the most general theory is only the assertion of a great body of facts, duly classified and

subordinated. When we assert the truth of the Copernican theory of the motions of the solar system, or of the Newtonian theory of the forces by which they are caused, we merely assert the groups of propositions which, in the Table of Astronomical Induction, are included in these doctrines; and ultimately, we may consider ourselves as merely asserting at once so many Facts, and therefore, of course, expressing an indisputable truth.

20. At any one of these steps of Induction in the Table, the inductive proposition is a *Theory* with regard to the Facts which it includes, while it is to be looked upon as a *Fact* with respect to the higher generalizations in which it is included. In any other sense, as was formerly shown, the opposition of Fact and Theory is untenable, and leads to endless perplexity and debate. Is it a Fact or a Theory that the planet Mars revolves in an Ellipse about the Sun? To Kepler, employed in endeavouring to combine the separate observations by the Conception of an Ellipse, it is a Theory; to Newton, engaged in inferring the law of force from a knowledge of the elliptical motion, it is a Fact. There are, as we have already seen, no special attributes of Theory and Fact which distinguish them from one another. Facts are phenomena apprehended by the aid of conceptions and mental acts, as Theories also are. We commonly call our observations *Facts,* when we apply, without effort or consciousness, conceptions perfectly familiar to us: while we speak of Theories, when we have previously contemplated the Facts and the connecting Conception separately, and have made the connexion by a conscious mental act. The real difference is a difference of relation; as the same proposition in a demonstration is the *premiss* of one syllogism and the *conclusion* in another;

—as the same person is a father and a son. Propositions are Facts and Theories, according as they stand above or below the Inductive Brackets of our Tables.

21. To obviate mistakes I may remark that the terms *higher* and *lower*, when used of generalizations, are unavoidably represented by their opposites in our Inductive Tables. The highest generalization is that which includes all others; and this stands the lowest on our page, because, reading downwards, that is the place which we last reach.

There is a distinction of the knowledge acquired by Scientific Induction into two kinds, which is so important that we shall consider it in the succeeding chapter.

Chapter VII.

OF LAWS OF PHENOMENA AND OF CAUSES.

1. In the first attempts at acquiring an exact and connected knowledge of the appearances and operations which nature presents, men went no further than to learn *what* takes place, not *why* it occurs. They discovered an Order which the phenomena follow, Rules which they obey; but they did not come in sight of the Powers by which these rules are determined, the Causes of which this order is the effect. Thus, for example, they found that many of the celestial motions took place as if the sun and stars were carried round by the revolutions of certain celestial spheres; but what causes kept these spheres in constant motion, they were never able to explain. In like manner in modern times, Kepler discovered that the planets describe ellipses, before Newton explained why they select this particular curve, and describe it in a particular manner. The laws of reflec-

tion, refraction, dispersion, and other properties of light have long been known; the causes of these laws are at present under discussion. And the same might be said of many other sciences. The discovery of *the Laws of Phenomena* is, in all cases, the first step in exact knowledge; these Laws may often for a long period constitute the whole of our science; and it is always a matter requiring great talents and great efforts, to advance to a knowledge of the *Causes* of the phenomena.

Hence the larger part of our knowledge of nature, at least of the certain portion of it, consists of the knowledge of the Laws of Phenomena. In Astronomy indeed, besides knowing the rules which guide the appearances, and resolving them into the real motions from which they arise, we can refer these motions to the forces which produce them. In Optics, we have become acquainted with a vast number of laws by which varied and beautiful phenomena are governed; and perhaps we may assume, since the evidence of the undulatory theory has been so fully developed, that we know also the Causes of the Phenomena. But in a large class of sciences, while we have learnt many Laws of Phenomena, the causes by which these are produced are still unknown or disputed. Are we to ascribe to the operation of a fluid or fluids, and if so, in what manner, the facts of heat, magnetism, electricity, galvanism? What are the forces by which the elements of chemical compounds are held together? What are the forces, of a higher order, as we cannot help believing, by which the course of vital action in organized bodies is kept up? In these and other cases, we have extensive departments of science; but we are as yet unable to trace the effects to their causes; and our science, so far as it is positive and certain, consists entirely of the laws of phenomena.

2. In those cases in which we have a division of the

science which teaches us the doctrine of the causes, as well as one which states the rules which the effects follow, I have distinguished the two portions of the science by certain terms. I have thus spoken of *Formal* Astronomy and *Physical* Astronomy. The latter phrase has long been commonly employed to describe that department of Astronomy which deals with those forces by which the heavenly bodies are guided in their motions; the former adjective appears well suited to describe a collection of rules depending on those ideas of space, time, position, number, which are, as we have already said, the *forms* of our apprehension of phenomena. The laws of phenomena may be considered as *formulæ*, expressing results in terms of those ideas. In like manner, I have spoken of Formal Optics and Physical Optics; the latter division including all speculations concerning the machinery by which the effects are produced. Formal Acoustics and Physical Acoustics may be distinguished in like manner, although these two portions of science have been a good deal mixed together by most of those who have treated of them. Formal Thermotics, the knowledge of the laws of the phenomena of heat, ought in like manner to lead to Physical Thermotics, or the Theory of Heat with reference to the mode in which its effects are produced;—a branch of science which as yet can hardly be said to exist.

3. What *kinds of cause* are we to admit in science? This is an important, and by no means an easy question. In order to answer it, we must consider in what manner our progress in the knowledge of causes has hitherto been made. By far the most conspicuous instance of success in such researches, is the discovery of the causes of the motions of the heavenly bodies. In this case, after the formal laws of the motions,—their conditions as to space and time,—had become known, men were

enabled to go a step further; to reduce them to the familiar and general cause of motion—mechanical force; and to determine the laws which this force follows. That this was a step in addition to the knowledge previously possessed, and that it was a real and peculiar truth, will not be contested. And a step in any other subject which should be analogous to this in astronomy;—a discovery of causes and forces as certain and clear as the discovery of universal gravitation;—would undoubtedly be a vast advance upon a body of science consisting only of the laws of phenomena.

4. But although physical astronomy may well be taken as a standard in estimating the value and magnitude of the advance from the knowledge of phenomena to the knowledge of causes; the peculiar features of the transition from formal to physical science in that subject must not be allowed to limit too narrowly our views of the nature of this transition in other cases. We are not, for example, to consider that the step which leads us to the knowledge of causes in any province of nature must necessarily consist in the discovery of centers of forces, and collections of such centers, by which the effects are produced. The discovery of the causes of phenomena may imply the detection of a fluid by whose undulations, or other operations, the results are occasioned. The phenomena of acoustics are, we know, produced in this manner by the air; and in the cases of light, heat, magnetism, and others, even if we reject all the theories of such fluids which have hitherto been proposed, we still cannot deny that such theories are intelligible and possible, as the discussions concerning them have shown. Nor can it be doubted that if the assumption of such a fluid, in any case, were as well evidenced as the doctrine of universal gravitation is, it must be considered as a highly valuable theory.

5. But again; not only must we, in aiming at the formation of a Causal Section in each Science of Phenomena, consider fluids and their various modes of operation admissible, as well as centers of mechanical force; but we must be prepared, if it be necessary, to consider the forces, or powers to which we refer the phenomena, under still more general aspects, and invested with characters different from mere mechanical force. For example; the forces by which the chemical elements of bodies are bound together, and from which arise, both their sensible texture, their crystalline form, and their chemical composition, are certainly forces of a very different nature from the mere attraction of matter according to its mass. The powers of assimilation and reproduction in plants and animals are obviously still more removed from mere mechanism; yet these powers are not on that account less real, nor a less fit and worthy subject of scientific inquiry.

6. In fact, these forces—mechanical, chemical and vital,—as we advance from one to the other, each bring into our consideration new characters; and what these characters are, has appeared in the survey which we have made of the Fundamental Ideas of the various sciences. It was then shown that the forces by which chemical effects are produced necessarily involve the Idea of Polarity,—they are polar forces; the particles tend together in virtue of opposite properties which in the combination neutralize each other. Hence, in attempting to advance to a theory of Causes in chemistry, our task is by no means to invent laws of *mechanical* force, and collections of forces, by which the effects may be produced. We know beforehand that no such attempt can succeed. Our aim must be to conceive such new kinds of force, including polarity among their characters, as may best render the results intelligible.

7. Thus in advancing to a Science of Cause in any subject, the labour and the struggle is, not to analyze the phenomena according to any preconceived and already familiar ideas, but to form distinctly new conceptions, such as do really carry us to a more intimate view of the processes of nature. Thus in the case of astronomy, the obstacle which deferred the discovery of the true causes from the time of Kepler to that of Newton, was the difficulty of taking hold of mechanical conceptions and axioms with sufficient clearness and steadiness; which, during the whole of that interval, mathematicians were learning to do. In the question of causation which now lies most immediately in the path of science, that of the causes of electrical and chemical phenomena, the business of rightly fixing and limiting the conception of polarity, is the proper object of the efforts of discoverers. Accordingly a large portion of Mr. Faraday's recent labours* is directed, not to the attempt at discovering new laws of phenomena, but to the task of throwing light upon the conception of polarity, and of showing how it must be understood, so that it shall include electrical induction and other phenomena, which have commonly been ascribed to forces acting mechanically at a distance. He is by no means content, nor would it answer the ends of science that he should be, with stating the results of his experiments; he is constantly, in every page, pointing out the interpretation of his experiments, and showing how the conception of polar forces enters into this interpretation. "I shall," he says†, "use every opportunity which presents itself of returning to that strong test of truth, experiment; but," he adds, "I shall necessarily have occasion to speak

* Eleventh, Twelfth, and Thirteenth Series of Researches, *Phil. Trans.* 1837 and 8.

† Art. 1318.

theoretically, and even hypothetically." His hypothesis that electrical inductive action always takes place by means of a continuous line of polarized particles, and not by attraction and repulsion at a distance, if established, cannot fail to be a great step on our way towards a knowledge of causes, as well as phenomena, in the subjects under his consideration.

8. The process of obtaining new conceptions is, to most minds, far more unwelcome than any labour in employing old ideas. The effort is indeed painful and oppressive; it is feeling in the dark for an object which we cannot find. Hence it is not surprizing that we should far more willingly proceed to seek for new causes by applying conceptions borrowed from old ones. Men were familiar with solid frames, and with whirlpools of fluid, when they had not learnt to form any clear conception of attraction at a distance. Hence they at first imagined the heavenly motions to be caused by crystalline spheres, and vortices. At length they were taught to conceive central forces, and then they reduced the solar system to these. But having done this, they fancied that all the rest of the machinery of nature must be central forces. We find Newton expressing this conviction*, and the mathematicians of the last century acted upon it very extensively. We may especially remark Laplace's labours in this field. Having explained, by such forces, the phenomena of capillary attraction, he attempted to apply the same kind of explanation to the reflection, refraction, and double refraction of light;—to the constitution of gases;—the operation of heat. It was soon seen that the explanation of refraction was arbitrary, and that of double refraction illusory; while polarization entirely eluded the grasp of this machinery. Centers of force would no longer represent the modes of causation

* Multa me movent, &c., Pref. to the *Principia*, already quoted.

which belonged to the phenomena. Polarization required some other contrivance, such as the undulatory theory supplied. No theory of light can be of any avail in which the fundamental idea of polarity is not clearly exhibited.

9. The sciences of magnetism and electricity have given rise to theories in which this relation of polarity is exhibited by means of two opposite fluids† ;—a positive and a negative fluid, or a vitreous and a resinous, for electricity, and a boreal and an austral fluid for magnetism. The hypothesis of such fluids gives results agreeing in a remarkable manner with the facts and their measures, as Coulomb and others have shown. It may be asked how far we may, in such a case, suppose that we have discovered the true cause of the phenomena, and whether it is sufficiently proved that these fluids really exist. The right answer seems to be, that the hypothesis certainly represents the truth so far as regards the polar relation of the two energies, and the laws of the attractive and repulsive forces of the particles in which these energies reside; but that we are not entitled to assume that the vehicles of these energies possess other attributes of material fluids, or that the forces thus ascribed to the particles are the primary elementary forces from which the action originates. We are the more bound to place this cautious limit to our acceptance of the Coulombian theory, since in electricity Faraday has in vain endeavoured to bring into view one of the polar fluids without the other: whereas such a result ought to be possible if there were two separable fluids. The impossibility of this separate exhibition of one fluid appears to show that the fluids are real only so far as they are polar. And Faraday's view above mentioned, according to which the attractions at a distance are resolved into the action of lines of polarized

* *Hist. Ind. Sci.*, B. XI. c. ii.

particles of air, appears still further to show that the conceptions hitherto entertained of electrical forces, according to the Coulombian theory, do not penetrate to the real and intimate nature of the causation belonging to this case.

10. Since it is thus difficult to know when we have seized the true cause of the phenomena in any department of science, it may appear to some persons that physical inquirers are imprudent and unphilosophical in undertaking this research of causes; and that it would be safer and wiser to confine ourselves to the investigation of the laws of phenomena, in which field the knowledge which we obtain is definite and certain. Hence there have not been wanting those who have laid it down as a maxim that "science must study only the laws of phenomena, and never the mode of production*." But it is easy to see that such a maxim would confine the breadth and depth of scientific inquiries to a most scanty and miserable limit. Indeed, such a rule would defeat its own object; for the laws of phenomena, in many cases, cannot be even expressed or understood without some hypothesis respecting their mode of production. How could the phenomena of polarization have been conceived or reasoned upon, except by imagining a polar arrangement of particles, or transverse vibrations, or some equivalent hypothesis? The doctrines of fits of easy transmission, the doctrine of moveable polarization, and the like, even when erroneous as representing the whole of the phenomena, were still useful in combining some of them into laws; and without some such hypotheses the facts could not have been followed out. The doctrine of a fluid caloric may be false; but without imagining such a fluid, how could the movement of heat from one part of a body to another be conceived? It may

* Comte, *Philosophie Positive*.

be replied that Fourier, Laplace, Poisson, who have principally cultivated the Theory of Heat, have not conceived it as a fluid, but have referred conduction to the radiation of the molecules of bodies, which they suppose to be separate points. But this molecular constitution of bodies is itself an assumption of the mode in which the phenomena are produced; and the radiation of heat suggests inquiries concerning a fluid emanation, no less than its conduction does. In like manner, the attempts to connect the laws of phenomena of heat and of gases, have led to hypotheses respecting the constitution of gases, and the combination of their particles with those of caloric, which hypotheses may be false, but are probably the best means of discovering the truth.

To debar science from inquiries like these, on the ground that it is her business to inquire into facts, and not to speculate about causes, is a curious example of that barren caution which hopes for truth without daring to venture upon the quest of it. This temper would have stopped with Kepler's discoveries, and would have refused to go on with Newton to inquire into the mode in which the phenomena are produced. It would have stopped with Newton's optical facts, and would have refused to go on with him and his successors to inquire into the mode in which these phenomena are produced. And, as we have abundantly shown, it would, on that very account, have failed in seeing what the phenomena really are.

In many subjects the attempt to study the laws of phenomena, independently of any speculations respecting the causes which have produced them, is neither possible for human intelligence nor for human temper. Men cannot contemplate the phenomena without clothing them in terms of some hypothesis, and will not be schooled to suppress the questionings which at every moment rise up within them concerning the causes of the phenomena. Who can attend to the appearances which come under the notice of the geologist;—strata regularly bedded, full of the remains of animals such as now live in the depths of the ocean, raised to the tops of mountains, broken, contorted, mixed with rocks such as still flow from the mouths of volcanos;—who can see phenomena like these, and imagine that he best promotes the progress of our knowledge of the earth's history, by noting down the facts, and abstaining from all inquiry whether these are really proofs of past states of the earth and of subterraneous forces, or merely an accidental imitation of the effects of such causes? In this and similar cases, to proscribe the inquiry into causes would be to annihilate the science.

Finally, this caution does not even gain its own single end, the escape from hypotheses. For, as we have said, those who will not seek for new and appropriate causes of newly-studied phenomena, are almost inevitably led to ascribe the facts to modifications of causes already familiar. They may declare that they will not hear of such causes as vital powers, elective affinities, electric, or calorific, or luminiferous ethers or fluids; but they will not the less on that account assume hypotheses equally unauthorized; for instance—universal mechanical forces; a molecular constitution of bodies; solid, hard, inert matter;—and will apply these hypotheses in a manner which is arbitrary in itself as well as quite insufficient for its purpose.

11. It appears, then, to be required, both by the analogy of the most successful efforts of science in past times and by the irrepressible speculative powers of the human mind, that we should attempt to discover both the *laws of phenomena*, and their *causes*. In every de-

partment of science, when prosecuted far enough, these two great steps of investigation must succeed each other. The laws of phenomena must be known before we can speculate concerning causes; the causes must be inquired into when the phenomena have been reduced to rule. In both these speculations the suppositions and conceptions which occur must be constantly tested by reference to observation and experiment. In both we must, as far as possible, devise hypotheses which, when we thus test them, display those characters of truth of which we have already spoken;—an agreement with facts such as will stand the most patient and rigid inquiry; a provision for predicting truly the results of untried cases; a consilience of inductions from various classes of facts; and a progressive tendency of the scheme to simplicity and unity.

We shall attempt hereafter to give several rules of a more precise and detailed kind for the discovery of the causes, and still more, of the laws of phenomena. But it will be useful in the first place to point out the Classification of the Sciences which results from the principles already established in this word. And for this purpose we must previously decide the question, whether the practical Arts, as Medicine and Engineering, must be included in our list of Sciences.

Chapter VIII.

OF ART AND SCIENCE.

1. The distinction of Arts and Sciences very materially affects all classifications of the departments of Human Knowledge. It is often maintained, expressly or tacitly, that the Arts are a part of our knowledge, in the

same sense in which the Sciences are so; and that Art is the application of Science to the purposes of practical life. It will be found that these views require some correction, when we understand *Science* in the exact sense in which we have throughout endeavoured to contemplate it, and in which alone our examination of its nature can instruct us in the true foundations of our knowledge.

When we cast our eyes upon the early stages of the histories of nations, we cannot fail to be struck with the consideration, that in many countries the Arts of life already appear, at least in some rude form or other, when, as yet, nothing of science exists. A practical knowledge of astronomy, such as enables them to reckon months and years, is found among all nations except the mere savages. A practical knowledge of mechanics must have existed in those nations which have left us the gigantic monuments of early architecture. The pyramids and temples of Egypt and Nubia, the Cyclopean walls of Italy and Greece, the temples of Magna Græcia and Sicily, the obelisks and edifices of India, the cromlechs and Druidical circles of countries formerly Celtic,—must have demanded no small practical mechanical skill and power. Yet those modes of reckoning time must have preceded the rise of speculative astronomy; these structures must have been erected before the theory of mechanics was known. To suppose, as some have done, a great body of science, now lost, to have existed in the remote ages to which these remains belong, is not only quite gratuitous and contrary to all analogy, but is a supposition which cannot be extended so far as to explain all such cases. For it is impossible to imagine that *every* art has been preceded by the science which renders a reason for its processes. Certainly men formed wine from the grape, before they possessed a science of fermentation;

the first instructor of every artificer in brass and iron can hardly be supposed to have taught the chemistry of metals as a science; the inventor of the square and the compasses had probably no more knowledge of demonstrated geometry than have the artisans who now use those implements; and finally, the use of speech, the employment of the inflections and combinations of words, must needs be assumed as having been prior to any general view of the nature and analogy of language. Even at this moment, the greater part of the arts which exist in the world are not accompanied by the sciences on which they theoretically depend. Who shall state to us the general chemical truths to which the manufactures of glass, and porcelain, and iron, and brass, owe their existence? Do not almost all artisans practise many successful artifices long before science explains the ground of the process? Do not arts at this day exist, in a high state of perfection, in countries in which there is no science, as China and India? These countries and many others have no theories of mechanics, of optics, of chemistry, of physiology; yet they construct and use mechanical and optical instruments, make chemical combinations, take advantage of physiological laws. It is too evident to need further illustration that art may exist without science;—that it has usually been anterior to it, and even now commonly advances independently, leaving science to follow as it can.

2. We here mean by *Science*, that exact, general, speculative knowledge, of which we have, throughout this work, been endeavouring to exhibit the nature and rules. Between such science and the *practical Arts* of life, the points of difference are sufficiently manifest. The object of Science is *Knowledge;* the object of Art are *Works.* The latter is satisfied with producing its

material results; to the former, the operations of matter, whether natural or artificial, are interesting only so far as they can be embraced by intelligible principles. The end of art is the beginning of science; for when it is seen *what* is done, then comes the question *why* it is done. Art may have fixed general rules, stated in words; but she has these merely as means to an end: to Science, the propositions which she obtains are each, in itself, a sufficient end of the effort by which it is acquired. When Art has brought forth her product, her task is finished; Science is constantly led by one step of her path to another. Each proposition which she obtains impels her to go onwards to other propositions more general, more profound, more simple. Art puts elements together, without caring to know what they are, or why they coalesce. Science analyzes the compound, and at every such step strives not only to perform, but to understand the analysis. Art advances in proportion as she becomes able to bring forth products more multiplied, more complex, more various; but Science, straining her eyes to penetrate more and more deeply into the nature of things, reckons her success in proportion as she sees, in all the phenomena, however multiplied, complex, and varied, the results of one or two simple and general laws.

3. There are many acts which man, as well as animals, performs by the guidance of nature, without seeing or seeking the reason why he does so; as the acts by which he balances himself in standing or moving, and those by which he judges of the form and position of the objects around him. These actions have their reason in the principles of geometry and mechanics; but of such reasons he who thus acts is unaware: he works blindly, under the impulse of an unknown principle which we call *Instinct*. When man's speculative nature seeks and finds

the reasons why he should act thus or thus;—why he should stretch out his arm to prevent his falling, or assign a certain position to an object in consequence of the angles under which it is seen;—he may perform the same actions as before, but they are then done by the aid of a different faculty, which, for the sake of distinction, we may call *Insight.* Instinct is a purely active principle; it is seen in deeds alone; it has no power of looking inwards; it asks no questions; it has no tendency to discover reasons or rules; it is the opposite of Insight.

4. Art is not identical with Instinct: on the contrary, there are broad differences. Instinct is stationary; Art is progressive. Instinct is mute; it acts, but gives no rules for acting: Art can speak; she can lay down rules. But though Art is thus separate from Instinct, she is not essentially combined with Insight. She can see what to do, but she needs not to see why it is done. She may lay down rules, but it is not her business to give reasons. When man makes *that* his employment, he enters upon the domain of science. Art takes the phenomena and laws of nature as she finds them: that they are multiplied, complex, capricious, incoherent, disturbs her not. She is content that the rules of nature's operations should be perfectly arbitrary and unintelligible, provided they are constant, so that she can depend upon their effects. But Science is impatient of all appearance of caprice, inconsistency, irregularity, in nature. She will not believe in the existence of such characters. She resolves one apparent anomaly after another; her task is not ended till every thing is so plain and simple, that she is tempted to believe she sees that it could by no possibility have been otherwise than it is.

5. It may be said that, after all, Art does really involve the knowledge which Science delivers;—that the

artisan who raises large weights, practically *knows* the properties of the mechanical powers;—that he who manufactures chemical compounds is virtually acquainted with the laws of chemical combination. To this we reply, that it might on the same grounds be asserted, that he who acts upon the principle that two sides of a triangle are greater than the third is really acquainted with geometry; and that he who balances himself on one foot knows the properties of the center of gravity. But this is an acquaintance with geometry and mechanics which even brute animals possess. It is evident that it is not of such knowledge as this that we have here to treat It is plain that this mode of possessing principles is altogether different from that contemplation of them on which science is founded. We neglect the most essential and manifest differences, if we confound our unconscious assumptions with our demonstrative reasonings.

6. The real state of the case is, that the principles which Art *involves*, Science alone *evolves*. The truths on which the success of Art depends, lurk in the artist's mind in an undeveloped state; guiding his hand, stimulating his invention, balancing his judgment, but not appearing in the form of enunciated propositions. Principles are not to him direct objects of meditation: they are secret Powers of Nature, to which the forms which tenant the world owe their constancy, their movements, their changes, their luxuriant and varied growth, but which he can nowhere directly contemplate. That the creative and directive principles which have their lodgment in the artist's mind, when *unfolded* by our speculative powers into systematic shape, become science, is true; but it is precisely this process of developement which gives to them their character of science. In practical Art, principles are unseen guides, leading us by invisible strings through paths where the end alone is

looked at: it is for Science to direct and purge our vision so that these airy ties, these principles and laws, generalizations and theories, become distinct objects of vision. Many may feel the intellectual monitor, but it is only to her favourite heroes that the Goddess of Wisdom visibly reveals herself.

7. Thus Art, in its earlier stages at least, is widely different from Science, independent of it, and anterior to it. At a later period, no doubt, Art may borrow aid from Science; and the discoveries of the philosopher may be of great value to the manufacturer and the artist. But even then, this application forms no essential part of the science: the interest which belongs to it is not an intellectual interest. The augmentation of human power and convenience may impel or reward the physical philosopher; but the processes by which man's repasts are rendered more delicious, his journeys more rapid, his weapons more terrible, are not, therefore, Science. They may involve principles which are of the highest interest to science; but as the advantage is not practically more precious because it results from a beautiful theory, so the theoretical principle has no more conspicuous place in science because it leads to convenient practical consequences. The nature of science is purely intellectual; knowledge alone,—exact general truth,—is her object; and we cannot mix with such materials, as matters of the same kind, the merely empirical maxims of art, without introducing endless confusion into the subject, and making it impossible to attain any solid footing in our philosophy.

8. I shall therefore not place, in our Classification of the Sciences, the Arts, as has generally been done; nor shall I notice the applications of sciences to art, as forming any separate portion of each science. The sciences, considered as bodies of general speculative

truths, are what we are here concerned with; and applications of such truths, whether useful or useless, are important to us only as illustrations and examples. Whatever place in human knowledge the Practical Arts may hold, they are not Sciences. And it is only by this rigorous separation of the Practical from the Theoretical, that we can arrive at any solid conclusions respecting the nature of truth, and the mode of arriving at it, such as it is our object to attain.

Chapter IX.

OF THE CLASSIFICATION OF SCIENCES.

1. The Classification of Sciences has its chief use in pointing out to us the extent of our powers of arriving at truth, and the analogies which may obtain between those certain and lucid portions of knowledge with which we are here concerned, and those other portions, of a very different interest and evidence, which we here purposely abstain to touch upon. The classification of human knowledge will, therefore, have a more peculiar importance when we can include in it the moral, political, and metaphysical, as well as the physical portions of our knowledge. But such a survey does not belong to our present undertaking: and a general view of the connexion and order of the branches of sciences which our review has hitherto included, will even now possess some interest; and may serve hereafter as an introduction to a more complete scheme of the general body of human knowledge.

2. In this, as in any other case, a sound classification must be the result, not of any assumed principles imperatively applied to the subject, but of an examination of

the objects to be classified;—of an analysis of them into the principles in which they agree and differ. The Classification of Sciences must result from the consideration of their nature and contents. Accordingly, that review of the sciences in which the History of them engaged us, led to a Classification, of which the main features are indicated in that work. The Classification thus obtained, depends neither upon the faculties of the mind to which the separate parts of our knowledge owe their origin, nor upon the objects which each science contemplates; but upon a more natural and fundamental element;—namely, the *Ideas* which each science involves. The Ideas regulate and connect the facts, and are the foundations of the reasoning, in each science: and having in the present work more fully examined these Ideas, we are now prepared to state here the classification to which they lead. If we have rightly traced each science to the Conceptions which are really fundamental *with regard to it*, and which give rise to the first principles on which it depends, it is not necessary for our purpose that we should decide whether these Conceptions are absolutely ultimate principles of thought, or whether, on the contrary, they can be further resolved into other Fundamental Ideas. We need not now suppose it determined whether or not Number is a mere modification of the Idea of Time, and Force a mere modification of the Idea of Cause: for however this may be, our Conception of Number is the foundation of Arithmetic, and our Conception of Force is the foundation of Mechanics. It is to be observed also that in our classification, each Science may involve, not only the Ideas or Conceptions which are placed opposite to it in the list, but also all which *precede* it. Thus Formal Astronomy involves not only the Conception of Motion, but also those which are the foundation of Arithmetic and Geometry. In like manner, Physical Astronomy employs the Sciences of Statics and Dynamics, and thus, rests on their foundations; and they, in turn, depend upon the Ideas of Space and of Time, as well as of Cause.

3. We may further observe, that this arrangement of Sciences according to the Fundamental Ideas which they involve, points out the transition from those parts of human knowledge which have been included in our History and Philosophy, to other regions of speculation into which we have not entered. We have repeatedly found ourselves upon the borders of inquiries of a psychological, or moral, or theological nature. Thus the History of Physiology* led us to the consideration of Life, Sensation, and Volition; and at these Ideas we stopped, that we might not transgress the boundaries of our subject as then predetermined. It is plain that the pursuit of such conceptions and their consequences, would lead us to the sciences (if we are allowed to call them sciences) which contemplate not only animal, but human principles of action, to Anthropology and Psychology. In other ways, too, the Ideas which we have examined, although manifestly the foundations of sciences such as we have here treated of, also plainly pointed to speculations of a different order; thus the Idea of a Final Cause is an indispensable guide in Biology, as we have seen; but the conception of Design as directing the order of nature, once admitted, soon carries us to higher contemplations. Again, the Class of Palætiological Sciences which we were in the *History* led to construct, although we there admitted only one example of the Class, namely Geology, does in reality include many vast lines of research; as the history and causes of the diffusion of plants and animals, the history of languages, arts, and

* *Hist. Ind. Sci.* B. XVII. c. V. sect. 2.

consequently of civilization. Along with these researches, comes the question how far these histories point backwards to a natural or a supernatural origin; and the Idea of a First Cause is thus brought under our consideration. Finally, it is not difficult to see that as the Physical Sciences have their peculiar governing Ideas, which support and shape them, so the Moral and Political Sciences also must similarly have their fundamental and formative Ideas, the source of universal and certain truths, each of their proper kind. But to follow out the traces of this analogy, and to verify the existence of those Fundamental Ideas in Morals and Politics, is a task quite out of the sphere of the work in which we are here engaged.

4. We may now place before the reader our Classification of the Sciences. I have added to the list of Sciences, a few not belonging to our present subject, that the nature of the transition by which we are to extend our philosophy into a wider and higher region may be in some measure perceived.

We may observe that the term *Physics*, when confined to a peculiar class of Sciences, is usually understood to exclude the Mechanical Sciences on the one side, and Chemistry on the other; and thus embraces the Secondary Mechanical and Analytico-Mechanical Sciences. But the adjective *Physical* applied to any science and opposed to *Formal*, as in Astronomy and Optics, implies those speculations in which we consider not only the Laws of Phenomena but their Causes; and generally, as in those cases, their Mechanical Causes.

Fundamental Ideas or Conceptions.	Sciences.	Classification.
Space	Geometry	Pure Mathematical Sciences.
Time	. . . .	
Number	Arithmetic	
Sign	Algebra	
Limit	Differentials	
Motion	Pure Mechanism	Pure Motional Sciences
	Formal Astronomy	
Cause		
Force	Statics	Mechanical Sciences.
Matter	Dynamics	
Inertia	Hydrostatics	
Fluid Pressure	Hydrodynamics	
	Physical Astronomy	
Outness		
Medium *of Sensation*	Acoustics	Secondary Mechanical Sciences. (Physics.)
Intensity *of Qualities*	Formal Optics	
Scales of Qualities	Physical Optics	
	Thermotics	
	Atmology	
Polarity	Electricity	Analytico-Mechanical Sciences. (Physics.)
	Magnetism	
	Galvanism	
Element (*Composition*)		
Chemical Affinity		
Substance (*Atoms*)	Chemistry	Analytical Science.
Symmetry	Crystallography	Analytico-Classificatory Sciences.
Likeness	Systematic Mineralogy	
Degrees of Likeness	Systematic Botany	Classificatory Sciences.
	Systematic Zoology	
Natural Affinity	Comparative Anatomy	
(*Vital Powers*)		
Assimilation		
Irritability		
(*Organization*)	Biology	Organical Sciences.
Final Cause		
Instinct		
Emotion	Psychology	
Thought		
Historical Causation	Geology	Palætiological Sciences.
	Distribution of Plants and Animals	
	Glossology	
	Ethnography	
First Cause	Natural Theology.	

In the next Book, we shall trace the opinions of some of the most eminent writers, respecting the sources of our knowledge of nature and the rules which may aid us in seeking it. For the knowledge of a true Scientific Method is a science resembling other sciences; and the ideas and views which it involves have been in some measure gradually developed into clearness and certainty by successive attempts. We may, therefore, acquire a more confident persuasion of the right direction of our path, by seeing how far it coincides with that which has been pointed out, with more or less distinctness, by many of the most sagacious and vigorous intellects who have bestowed their attention upon this inquiry.

BOOK XII.

REVIEW OF OPINIONS ON THE NATURE OF KNOWLEDGE AND THE METHODS OF SEEKING IT.

CHAPTER I.

INTRODUCTION.

BY the examination of the elements of human thought in which we have been engaged, and by a consideration of the history of the most clear and certain parts of our knowledge, we have been led to certain doctrines respecting the progress of that exact and systematic knowledge which we call Science; and these doctrines we have endeavoured to lay before the reader in the preceding Book. The questions on which we have thus ventured to pronounce have had a strong interest for man, from the earliest period of his intellectual progress, and have been the subjects of lively discussion and bold speculation in every age. We conceive that in the doctrines to which our researches have conducted us, we have a far better hope that we possess a body of permanent truths, than the earlier essays on the same subjects could furnish. For we have not taken our examples of knowledge at hazard, as earlier speculators did, and were almost compelled to do; but have drawn our materials from the vast store of unquestioned truths which modern science offers to us: and we have formed our judgment concerning the nature and progress of knowledge by considering what such science is, and how it has reached its present condition. But though we have thus pursued our speculations concerning knowledge with advantages which earlier writers did not possess, it is still both interesting and instructive for us to regard the opinions upon this subject which have been delivered by the philosophers of past times. It is especially interesting to see some of the truths which we have endeavoured to expound, gradually dawning in men's minds, and assuming the clear and permanent form in which we can now contemplate them. I shall therefore, in this Book, pass in review many of the opinions of the writers of various ages concerning the mode by which man best acquires the truest knowledge; and I shall endeavour, as we proceed, to appreciate the real value of such judgments, and their place in the progress of sound philosophy.

In this estimate of the opinions of others, I shall be guided by those general doctrines which I have, as I trust, established in the preceding part of this work. And without attempting here to give any summary of these doctrines, I may remark that there are two main principles by which speculations on such subjects in all ages are connected and related to each other; namely, the opposition of *Ideas* and *Sensations*, and the distinction of *practical* and *speculative* knowledge. The opposition of Ideas and Sensations is exhibited to us in the antithesis of Theory and Fact, which are necessarily considered as distinct and of opposite natures, and yet necessarily identical, and constituting Science by their identity. In like manner, although practical knowledge is in substance identical with speculative, (for all knowledge is speculation,) there is a distinction between the two in their history, and in the subjects by which they are exemplified, which distinction is quite essential in judging of the philosophical views of the ancients. The alternatives of identity and diversity, in these two antitheses,—the successive separation, opposition, and reunion of principles which thus arise,—have produced, (as they may easily be imagined capable of doing,) a long and varied series of systems concerning the nature of knowledge; among which we shall have to guide our course by the aid of the views already presented.

I am far from undertaking, or wishing, to review the whole series of opinions which thus comes under our view; and I do not even attempt to examine all the principal authors who have written on such subjects. I merely wish to select some of the most considerable forms which such opinions have assumed, and to point out in some measure the progress of truth from age to age. In doing this, I can only endeavour to seize some of the most prominent features of each time and of each step; and I must pass rapidly from classical antiquity to those which we have called the dark ages, and from them to modern times. At each of these periods the modifications of opinion, and the speculations with which they were connected, formed a vast and tangled maze, into the byways of which our plan does not allow us to enter. We shall esteem ourselves but too fortunate, if we can discover the single track by which ancient led to modern philosophy.

I must also repeat that my survey of philosophical writers is here confined to this one point,—their opinions on the nature of knowledge and the method of science. I with some effort avoid entering upon other parts of the philosophy of those of whom I speak; I knowingly pass by those portions of their speculations which are in many cases the most interesting and celebrated;—their opinions concerning the human soul, the Divine governor of the world, the foundations or leading doctrines of politics, religion, and general philosophy. I am desirous that my reader should bear this in mind, since he must otherwise be offended with the scanty and partial view which I give in this place of the philosophers whom I enumerate.

Chapter II.

PLATO.

There would be small advantage in beginning our examination earlier than the period of the Socratic School at Athens; for although the spirit of inquiry on such subjects had awoke in Greece at an earlier period, and although the peculiar aptitude of the Grecian mind for such researches had shown itself repeatedly in subtle distinctions and acute reasonings, all the positive results of these early efforts were contained in a more definite form in the reasonings of the Platonic age. Anterior to that time, the Greeks did not possess plain and familiar examples of exact knowledge, such as the truths of Arithmetic, Geometry, Astronomy, and Optics, became in the school of Plato; nor were the antitheses of which we spoke above, so distinctly and fully unfolded as we find them in Plato's works.

The question which hinges upon one of these antitheses, occupies a prominent place in several of the Platonic dialogues;—namely, whether our knowledge be obtained by means of Sensation or of Ideas. One of the doctrines which Plato most earnestly inculcated upon his countrymen was, that we do not *know* concerning sensible objects, but concerning ideas. The first attempts of the Greeks at metaphysical analysis had given rise to a school which maintained that material objects are the only realities. In opposition to this, arose another school, which taught that material objects have no permanent reality, but are ever waxing and waning, constantly changing their substance. "And hence," as Aristotle says*, "arose the doctrine of ideas which the Platonists held. For they assented to the opinion of Heraclitus, that all sensible objects are in a constant state of flux. So that if there is to be any knowledge and science, it must be concerning some permanent natures, different from the sensible natures of objects; for there can be no permanent science respecting that which is perpetually changing. It happened that Socrates turned his speculations to the moral virtues, and was the first philosopher who endeavoured to give universal definitions of such matters. He wished to reason systematically, and therefore he tried to establish definitions, for definitions are the basis of systematic reasoning. There are two things which may justly be looked upon as steps in philosophy due to Socrates; inductive reasonings, and universal definitions;—both of them steps which belong to the foundations of science. Socrates, however, did not make universals, or definitions separable from the objects; but his followers separated them, and these essences they termed *Ideas*." And the same account is given by other writers†. "Some existences are sensible, some intelligible: and according to Plato, they who wish to understand the principles of things, must first separate the ideas from the things, such as the ideas of Similarity, Unity, Number, Magnitude, Position, Motion: second, that we must assume an absolute Fair, Good, Just, and the like: third, that we must consider the ideas of relation, as Knowledge, Power: recollecting that the things which we perceive have this or that appellation applied to them because

* *Metaph.*, XII. 4. † Diog. Laert. *Vit. Plat.*

they partake of this or that Idea; those things being *just* which participate in the idea of The Just, those being *beautiful*, which contain the idea of The Beautiful." And many of the arguments by which this doctrine was maintained are to be found in the Platonic dialogues. Thus the opinion that true knowledge consists in sensation, which had been asserted by Protagoras and others, is refuted in the *Theætetus:* and we may add, so victoriously refuted, that the arguments there put forth have ever since exercised a strong influence upon the speculative world. It may be remarked that in the minds of Plato and of those who have since pursued the same paths of speculation, the interest of such discussions as those we are now referring to, was by no means limited to their bearing upon mere theory; but was closely connected with those great questions of morals which have always a practical import. Those who asserted that the only foundation of knowledge was sensation, asserted also that the only foundation of virtue was the desire of pleasure. And in Plato, the metaphysical part of the disquisitions concerning knowledge in general, though independent in its principles, always seems to be subordinate in its purpose to the questions concerning the knowledge of our duty.

Since Plato thus looked upon the Ideas which were involved in each department of knowledge as forming its only essential part, it was natural that he should look upon the study of Ideas as the true mode of pursuing knowledge. This he himself describes in the *Philebus**. "The best way of arriving at truth is not very difficult to point out, but most hard to pursue. All the arts which have ever been discovered, were revealed in this manner. It is a gift of the gods to man, which, as I conceive, they sent down by some Prometheus, in a blaze of light; and

* T. II. p. 16, c, d. ed. Bekker, t. v. p. 437.

the ancients, more clear-sighted than we, and less removed from the gods, handed down this traditionary doctrine: that whatever is said to be, comes of One and of Many, and comprehends in itself the Finite and the Infinite in coalition (being One Kind, and consisting of Infinite Individuals). And this being the state of things, we must, in each case, endeavour to seize the One Idea (the idea of the Kind) as the chief point; for we shall find that it is there. And when we have seized this one thing, we may then consider how it comprehends in itself two, or three, or any other number; and, again, examine each of these ramifications separately; till at last we perceive, not only that One is at the same time One and Many, but also how many. And when we have thus filled up the interval between the Infinite and the One, we may consider that we have done with each one. The gods then, as I have said, taught us by tradition thus to contemplate, and to learn, and to teach one another. But the philosophers of the present day seize upon the One, at hazard, too soon or too late, and then immediately snatch at the Infinite; but the intermediate steps escape them, by which the subject is subdivided, so that it can be the subject of logical exposition and discussion."

It would seem that what the author here describes as the most perfect form of exposition, is that which refers each object to its place in a classification containing a complete series of subordinations, and which gives a definition of each class. We have repeatedly remarked that, in sciences of classification, each new definition which gives a tenable and distinct separation of classes is an important advance in our knowledge; but that such definitions are rather the last than the first step in each advance. In the progress of real knowledge, these definitions are always the results of a laborious study of individual cases, and are never arrived at by a pure effort of thought, which is what Plato appears to have imagined as the true mode of philosophizing. And still less do the advances of other sciences consist in seizing at once upon the highest generality, and filling in afterwards all the intermediate steps between that and the special instances. On the contrary, as we have seen, the ascents from particular to general are all successive; and each step of this ascent requires time, and labour, and a patient examination of actual facts and objects.

It would, of course, be absurd to blame Plato for having inadequate views of the nature of progressive knowledge, at the time when knowledge could hardly be said to have begun its progress. But we already find in his speculations, as appears in the passages just quoted from his writings, several points brought into view which will require our continued attention as we proceed. In overlooking the necessity of a gradual and successive advance from the less general to the more general truths, Plato shared in a dimness of vision which prevailed among philosophers to the time of Francis Bacon. In thinking too slightly of the study of actual nature, he manifested a bias from which the human intellect freed itself in the vigorous struggles which terminated the dark ages. In pointing out that all knowledge implies a unity of what we observe as manifold, which unity is given by the mind, Plato taught a lesson which has of late been too obscurely acknowledged, the recoil by which men repaired their long neglect of facts having carried them for a while so far as to think that facts were the whole of our knowledge. And in analyzing this principle of Unity, by which we thus connect sensible things, into various Ideas, such as Number, Magnitude, Position, Motion, he made a highly important step, which it has been the business of philosophers in succeeding times to complete and to follow out.

But the efficacy of Plato's speculations in their bearing upon physical science, and upon theory in general, was much weakened by the confusion of practical with theoretical knowledge, which arose from the ethical propensities of the Socratic school. In the Platonic Dialogues, Art and Science are constantly spoken of indiscriminately. The skill possessed by the Painter, the Architect, the Shoemaker, is considered as a just example of human science, no less than the knowledge which the geometer or the astronomer possesses of the theoretical truths with which he is conversant. Not only so; but traditionary and mythological tales, mystical imaginations and fantastical etymologies, are mixed up, as no less choice ingredients, with the most acute logical analyses, and the most exact conduct of metaphysical controversies. There is no distinction made between the knowledge possessed by the theoretical psychologist and the physician, the philosophical teacher of morals and the legislator or the administrator of law. This, indeed, is the less to be wondered at, since even in our own time the same confusion is very commonly made by persons not otherwise ignorant or uncultured.

On the other hand, we may remark finally, that Plato's admiration of Ideas was not a barren imagination, even so far as regarded physical science. For, as we have seen*, he had a very important share in the introduction of the theory of epicycles, having been the first to propose to astronomers in a distinct form, the problem of which that theory was the solution; namely, "to explain the celestial phenomena by the combination of equable circular motions." This demand of an ideal hypothesis which should exactly express the phenomena

* *Hist. Ind. Sci.*, B. III. c. ii.

(as well as they could then be observed), and from which, by the interposition of suitable steps, all special cases might be deduced, falls in well with those views respecting the proper mode of seeking knowledge which we have quoted from the *Philebus*. And the Idea which could thus represent and replace all the particular Facts, being not only sought but found, we may readily suppose that the philosopher was, by this event, strongly confirmed in his persuasion that such an Idea was indeed what the inquirer ought to seek. In this conviction all his genuine followers up to modern times have participated; and thus, though they have avoided the errour of those who hold that facts alone are valuable as the elements of our knowledge, they have frequently run into the opposite errour of too much despising and neglecting facts, and of thinking that the business of the inquirer after truth was only a profound and constant contemplation of the conceptions of his own mind. But of this hereafter.

Chapter III.

ARISTOTLE.

The views of Aristotle with regard to the foundations of human knowledge are very different from those of his tutor Plato, and are even by himself put in opposition to them. He dissents altogether from the Platonic doctrine that Ideas are the true materials of our knowledge; and after giving, respecting the origin of this doctrine, the account which we quoted in the last chapter, he goes on to reason against it. "Thus," he says*, "they devised Ideas of all things which are spoken of as universals:

* *Metaph.* XII. 4.

much as if any one having to count a number of objects, should think that he could not do it while they were few, and should expect to count them by making them more numerous. For the kinds of things are almost more numerous than the special sensible objects, by seeking the causes of which they were led to their Ideas." He then goes on to urge several other reasons against the assumption of Ideas and the use of them in philosophical researches.

Aristotle himself establishes his doctrines by trains of reasoning. But reasoning must proceed from certain First Principles; and the question then arises, Whence are these First Principles obtained? To this he replies, that they are the result of *Experience*, and he even employs the same technical expression by which we at this day describe the process of collecting these principles from observed facts;—that they are obtained by *Induction*. I have already quoted passages in which this statement is made*. "The way of reasoning," he says†, "is the same in philosophy, and in any art or science: we must collect the *facts* (τὰ ὑπαρχόντα), and the things to which the facts happen, and must have as large a supply of these as possible, and then we must examine them according to the terms of our syllogisms." . . . "There are peculiar principles in each science; and in each case these principles must be obtained from *experience*. Thus astronomical observation supplies the principles of astronomical science. For the phenomena being rightly taken, the demonstrations of astronomy were discovered; and the same is the case with any other Art or Science. So that if the facts in each case be taken, it is our business to construct the demonstrations. For if *in our natural history* (κατὰ τὴν ἱστορίαν) we have omitted none of the facts and properties which belong to the subject,

* *Hist. Ind. Sci.*, B. I. c. iii. sect. 2. † *Analyt. Prior.*, I. 30.

we shall learn what we can demonstrate and what we cannot." And, again*, "It is manifest that if any sensation be wanting, there must be some knowledge wanting, which we are thus prevented from having. For we acquire knowledge either *by Induction* (ἐπαγωγὴ) or by Demonstration: and Demonstration is from universals, but Induction from particulars. It is impossible to have universal theoretical propositions except by Induction: and we cannot make inductions without having sensation; for sensation has to do with particulars."

It is easy to show that Aristotle uses the term *Induction*, as we use it, to express the process of collecting a general proposition from particular cases in which it is exemplified. Thus in a passage which we have already quoted†, he says, "Induction, and Syllogism from Induction, is when we attribute one extreme term to the middle by means of the other." The import of this technical phraseology will further appear by the example which he gives: "We find that several animals which are deficient in bile are longlived, as man, the horse, the mule; hence we infer that *all* animals which are deficient in bile are longlived."

We may observe, however, that both Aristotle's notion of induction, and many other parts of his philosophy, are obscure and imperfect, in consequence of his refusing to contemplate ideas as something distinct from sensation. It thus happens that he always assumes the ideas which enter into his proposition as *given*; and considers it as the philosopher's business to determine whether such propositions are true or not: whereas the most important feature in induction is, as we have said, the *introduction* of a new idea, and not its employment when once introduced. That the mind in this manner

* *Analyt. Post.*, I. 18.

† *Anal. Pri.*, II. 23, περὶ τῆς ἐπαγωγῆς.

gives unity to that which is manifold,—that we are thus led to speculative principles which have an evidence higher than any others,—and that a peculiar sagacity in some men seizes upon the conceptions by which the facts may be bound into true propositions,—are doctrines which form no essential part of the philosophy of the Stagirite, although such views are sometimes recognized, more or less clearly, in his expressions. Thus he says*, "There can be no knowledge when the sensation does not continue in the mind. For this purpose, it is necessary both to perceive, and to have some *unity* in the mind; (αἰσθανομένοις ἔχειν ἝΝ ΤΙ ἐν τῇ ψυχῇ) and many such perceptions having taken place, some difference is then perceived: and from the remembrance of these arises Reason. Thus from Sensation comes Memory, and from Memory of the same thing often repeated comes Experience: for many acts of Memory make up one Experience. And from Experience, or from any Universal Notion which takes a permanent place in the mind,—from the *unity in the manifold*, the same some one thing being found in many facts,—springs the first principle of Art and of Science; of Art, if it be employed about production; of Science, if about existence."

I will add to this, Aristotle's notice of *Sagacity*; since, although little or no further reference is made to this quality in his philosophy, the passage fixes our attention upon an important step in the formation of knowledge. "Sagacity," (ἀγχίνοια) he says†, "is a hitting by guess (εὐστοχία τις) upon the middle term (the conception common to two cases) in an inappreciable time. As for example, if any one seeing that the bright side of the moon is always towards the sun, suddenly perceives why this is; namely, because the moon shines by the light of the sun:—or if he sees a person talking with a

* *Anal. Post.*, II. 19. † *Ib.*, I. 34.

rich man, he guesses that he is borrowing money;—or conjectures that two persons are friends, because they are enemies of the same person."—To consider only the first of these examples;—the conception here introduced, that of a body shining by the light which another casts upon it, is not contained in the observed facts, but introduced by the mind. It is, in short, that conception which, in the act of induction, the mind superadds to the phenomena as they are presented by the senses: and to invent such appropriate conceptions, such "eustochies," is, indeed, the precise office of inductive sagacity.

At the end of this work (the *Later Analytics*) Aristotle ascribes our knowledge of principles to Intellect, (νοῦς) or, as it appears necessary to translate the word, *Intuition**. "Since, of our intellectual habits by which we aim at truth, some are always true, but some admit of being false, as Opinion and Reasoning, but Science and Intuition are always true; and since there is nothing which is more certain than Science except Intuition; and since Principles are better known to us than the Deductions from them; and since all Science is connected by reasoning, we cannot have Science respecting Principles. Considering this then, and that the beginning of Demonstration cannot be Demonstration, nor the beginning of Science, Science; and since, as we have said, there is no other kind of truth, Intuition must be the beginning of Science."

What is here said, is, no doubt, in accordance with the doctrines which we have endeavoured to establish respecting the nature of Science, if by this *Intuition* we understand that contemplation of certain Fundamental Ideas, which is the basis of all rigorous knowledge. But notwithstanding this apparent approximation, Aristotle was far from having an habitual and practical possession

* *Anal. Post.*, II. 19.

of the principles which he thus touches upon. He did not, in reality, construct his philosophy by giving Unity to that which was manifold, or by seeking in Intuition principles which might be the basis of Demonstration; nor did he collect, in each subject, fundamental propositions by an induction of particulars. He rather endeavoured to divide than to unite; he employed himself, not in combining facts, but in analyzing notions; and the criterion to which he referred his analysis was, not the facts of our experience, but our habits of language. Thus his opinions rested, not upon sound inductions, gathered in each case from the phenomena by means of appropriate Ideas; but upon the loose and vague generalizations which are implied in the common use of speech.

Yet Aristotle was so far consistent with his own doctrine of the derivation of knowledge from experience, that he made in almost every province of human knowledge, a vast collection of such special facts as the experience of his time supplied. These collections are almost unrivalled, even to the present day, especially in Natural History; in other departments, when to the facts we must add the right Inductive Idea, in order to obtain truth, we find little of value in the Aristotelic works. But in those parts which refer to Natural History, we find not only an immense and varied collection of facts and observations, but a sagacity and acuteness in classification which it is impossible not to admire. This indeed appears to have been the most eminent faculty in Aristotle's mind.

The influence of Aristotle in succeeding ages will come under our notice shortly.

Chapter IV.

THE LATER GREEKS.

Thus while Plato was disposed to seek the essence of our knowledge in Ideas alone, Aristotle, slighting this source of truth, looked to Experience as the beginning of Science; and he attempted to obtain, by division and deduction, all that Experience did not immediately supply. And thus, with these two great names, began that struggle of opposite opinions which has ever since that time agitated the speculative world, as men have urged the claims of Ideas or of Experience to our respect, and as alternately each of these elements of knowledge has been elevated above its due place, while the other has been unduly depressed. We shall see the successive turns of this balanced struggle in the remaining portions of this review.

But we may observe that practically the influence of Plato predominated rather than that of Aristotle, in the remaining part of the history of ancient philosophy. It was, indeed, an habitual subject of dispute among men of letters, whether the sources of true knowledge are to be found in the Senses or in the Mind; the Epicureans taking one side of this alternative, and the Academics another, while the Stoics in a certain manner included both elements in their view. But none of these sects showed their persuasion that the materials of knowledge were to be found in the domain of Sense, by seeking them there. No one appears to have thought of following the example of Aristotle, and gathering together a store of observed facts. We may except, perhaps, assertions belonging to some provinces of Natural History, which were collected by various writers: but in these, the mixed character of the statements, the want of discrimination in the estimate of evidence, the credulity and love of the marvellous which the authors for the most part displayed, showed that instead of improving upon the example of Aristotle, they were wandering further and further from the path of real knowledge. And while they thus collected, with so little judgment, such statements as offered themselves, it hardly appears to have occurred to any one to enlarge the stores of observation by the aid of experiment; and to learn what the laws of nature were, by trying what were their results in particular cases. They used no instruments for obtaining an insight into the constitution of the universe, except logical distinctions and discussions; and proceeded as if the phenomena familiar to their predecessors must contain all that was needed as a basis for natural philosophy. By thus contenting themselves with the facts which the earlier philosophers had contemplated, they were led also to confine themselves to the ideas which those philosophers had put forth. For all the most remarkable alternatives of hypothesis, so far as they could be constructed with a slight and common knowledge of phenomena, had been promulgated by the acute and profound thinkers who gave the first impulse to philosophy: and it was not given to man to add much to the original inventions of *their* minds till he had undergone anew a long discipline of observation, and of thought employed upon observation. Thus the later authors of the Greek Schools became little better than commentators on the earlier; and the common places with which the different schools carried on their debates, —the constantly recurring argument, with its known attendant answer,—the distinctions drawn finer and finer and leading to nothing,—render the speculations of those times a *scholastic* philosophy, in the same sense in which we employ the term when we speak of the labours of the middle ages. It will be understood that I now refer to that which is here my subject, the opinions concerning our knowledge of nature, and the methods in use for the purpose of obtaining such knowledge. Whether the moral speculations of the ancient world were of the same stationary kind, going their round in a limited circle, like their metaphysics and physics, must be considered on some other occasion.

As a specimen of the later Greek reasonings on physical philosophy, I may take a passage from Galen's Commentary on the Treatise of Hippocrates, *On the Elements*. "What, then," he asks*, "is the method of discovering these Elements? To me it seems there can be no other than that which was introduced by Hippocrates. For we must reason first, considering if an Element be a thing which is one, according to its idea; (ἕν τι τὴν ἰδέαν;) and next, if many and various and dissimilar, how many, and of what kind they are, and how related by their association. Now that the First Element is not one only, comprizing both our bodies and other things, Hippocrates shows. For if man were one Element only, he could not fall sick; for there would be nothing which could derange his health, if he were of one Element only." We have seen, in the *History of Science*, that Galen is one of the greatest names in ancient Physiology: but when he makes the attempt to pass at one step from the most familiar facts to the ultimate constitution of the universe, it is not wonderful that his reasonings are of no real value or import.

Before we quit the ancients we may observe some peculiarities in the Roman disciples of the Greek philosophy, which may be worthy our notice.

* Lib. I. c. ii.

Chapter V.

THE ROMANS.

The Romans had no philosophy but that which they borrowed from the Greeks; and what they thus received, they hardly made entirely their own. The vast and profound question of which we have been speaking, the relation between Existence and our Knowledge of what exists, they never appear to have fathomed, even so far as to discern how wide and deep it is. In the developement of the ideas by which nature is to be understood, they went no further than their Greek masters had gone, nor indeed was more to be looked for. And in the practical habit of accumulating observed facts as materials for knowledge, they were much less discriminating and more credulous than their Greek predecessors. The descent from Aristotle to Pliny, in the judiciousness of the authors and the value of their collections of facts, is immense.

Since the Romans were thus servile followers of their Greek teachers, and little acquainted with any example of new truths collected from the world around them, it was not to be expected that they could have any just conception of that long and magnificent ascent from one set of truths to others of higher order and wider compass, which the history of science began to exhibit when the human mind recovered its progressive habits. Yet some dim presentiment of the splendid career thus destined for the intellect of man appears from time to time to have arisen in their minds. Perhaps the circumstance which most powerfully contributed to suggest this vision, was the vast intellectual progress which they were themselves conscious of having made, through the introduction of the Greek philosophy; and to this may be added, perhaps, some other features of national character. Their temper was too stubborn to acquiesce in the absolute authority of the Greek philosophy, although their minds were not inventive enough to establish a rival by its side. And the wonderful progress of their political power had given them a hope in the progress of man which the Greeks never possessed. The Roman, as he believed the fortune of his State to be destined for eternity, believed also in the immortal destiny and endless advance of that Intellectual Republic of which he had been admitted a denizen.

It is easy to find examples of such feelings as I have endeavoured to describe. The enthusiasm with which Lucretius and Virgil speak of physical knowledge, manifestly arises in a great measure from the delight which they had felt in becoming acquainted with the Greek theories.

> Me vero primum dulces ante omnia musæ
> Quarum sacra fero ingenti perculsus amore,
> Accipiant, cœlique vias et sidera monstrent,
> Defectus solis varios, Lunæque labores!
> Felix qui potuit rerum cognoscere causas!

Ovid* expresses a similar feeling.

> Felices animos quibus hæc cognoscere primis
> Inque domos superas scandere cura fuit! . . .
> Admovere oculis distantia sidera nostris
> Ætheraque ingenio supposuere suo.
> Sic petitur cœlum: non ut ferat Ossam Olympus
> Summaque Peliacus sidera tanget apex.

And from the whole tenour of these and similar passages, it is evident that the intellectual pleasure which arises from our first introduction to a beautiful physical theory had a main share in producing this enthusiasm at the contemplation of the victories of science; although undoubtedly the moral philosophy, which was never sepa-

* L. I., *Fast.*

rated from the natural philosophy, and the triumph over superstitious fears which a knowledge of nature was supposed to furnish, added warmth to the feeling of exultation.

We may trace a similar impression in the ardent expressions which Pliny* makes use of in speaking of the early astronomers, and which we have quoted in the *History*. "Great men! elevated above the common standard of human nature, by discovering the laws which celestial occurrences obey, and by freeing the wretched mind of man from the fears which eclipses inspired."

This exulting contemplation of what science had done, naturally led the mind to an anticipation of further achievements still to be performed. Expressions of this feeling occur in Seneca, and are of the most remarkable kind, as the following example will show†.

"Why do we wonder that comets, so rare a phenomena, have not yet had their laws assigned?—that we should know so little of their beginning and their end, when their recurrence is at wide intervals? It is not yet fifteen hundred years since Greece,

Stellis numeros et nomina fecit,

reckoned the stars, and gave them names. There are still many nations which are acquainted with the heavens by sight only; which do not yet know why the moon disappears, why she is eclipsed. It is but lately that among us philosophy has reduced these matters to a certainty. The day shall come when the course of time and the labour of a maturer age shall bring to light what is yet concealed. One generation, even if it devoted itself to the skies, is not enough for researches so extensive. How then can it be so, when we divide this scanty allowance of years into no equal shares between our studies and our vices? These things then must be explained by a

* *Hist. Nat.* I. 75. † *Quæst. Nat.*, VII. 25.

long succession of inquiries. We have but just begun to know how arise the morning and evening appearances, the stations, the progressions, and the retrogradations of the fixed stars which put themselves in our way;—which appearing perpetually in another and another place compel us to be curious. Some one will hereafter demonstrate in what region the comets wander; why they move so far asunder from the rest; of what size and nature they are. Let us be content with what we have discovered: let posterity contribute its share to truth." Again he adds* in the same strain. "Let us not wonder that what lies so deep is brought out so slowly. How many animals have become known for the first time in this age! And the members of future generations shall know many of which we are ignorant. Many things are reserved for ages to come, when our memory shall have passed away. The world would be a small thing indeed, if it did not contain matter of inquiry *for* all the world. Eleusis reserves something for the second visit of the worshipper. *So too Nature does not at once disclose all* HER *mysteries.* We think ourselves initiated; we are but in the vestibule. The arcana are not thrown open without distinction and without reserve. This age will see some things; that which comes after us, others."

While we admire the happy coincidence of these conjectures with the soundest views which the history of science teaches us, we must not forget that they are merely conjectures, suggested by very vague impressions, and associated with very scanty conceptions of the laws of nature. Seneca's *Natural Questions*, from which the above extract is taken, contains a series of dissertations on various subjects of Natural Philosophy; as Meteors, Rainbows, Lightning, Springs, Rivers, Snow, Hail, Rain, Wind, Earthquakes and Comets. In the whole of these

* *Quæst. Nat.*, VII. 30, 31.

dissertations, the statements are loose, and the explanations of little or no value. Perhaps it may be worth our while to notice a case in which he refers to an observation of his own, although his conclusion from it be erroneous. He is arguing* against the opinion that Springs arise from the water which falls in rain. "In the first place," he says, "I, a very diligent digger in my vineyard, affirm that no rain is so heavy as to moisten the earth to the depth of more than ten feet. All the moisture is consumed in this outer crust, and descends not to the lower part." We have here something of the nature of an experiment; and indeed, as we may readily conceive, the instinct which impels man to seek truth by experiment can never be altogether extinguished. Seneca's experiment was deprived of its value by the indistinctness of his ideas, which led him to rest in the crude conception of the water being "consumed" in the superficial crust of the earth.

It is unnecessary to pursue further the reasonings of the Romans on such subjects, and we now proceed to the ages which succeeded the fall of their empire.

Chapter VI.

THE SCHOOLMEN OF THE MIDDLE AGES.

In the *History of the Sciences* I have devoted a Book to the state of Science in the middle ages, and have endeavoured to analyze the intellectual defects of that period. Among the characteristic features of the human mind during those times, I have noticed Indistinctness of Ideas, a Commentatorial Spirit, Mysticism, and Dogmatism. The account there given of this portion of the

* *Quæst. Nat.*, III. 7.

history of man belongs, in reality, rather to the present work than to the History of Progressive Science. For, as we have there remarked, theoretical Science was, during the period of which we speak, almost entirely stationary; and the investigation of the causes of such a state of things may be considered as a part of that review, in which we are now engaged, of the vicissitudes of man's acquaintance with the methods of discovery. But when we offered to the world a history of science, to leave so large a chasm unexplained, would have made the series of events seem defective and broken; and the survey of the Middle Ages was therefore inserted. I would beg to refer to that portion of the former work the reader who wishes for information in addition to what is here given.

The Indistinctness of Ideas and the Commentatorial Disposition of those ages have already been here brought under our notice. Viewed with reference to the opposition between Experience and Ideas, on which point, as we have said, the succession of opinions in a great measure turns, it is clear that the commentatorial method belongs to the ideal side of the question: for the commentator seeks for such knowledge as he values, by analyzing and illustrating what his author has said; and, content with this material of speculation, does not desire to add to it new stores of experience and observation. And with regard to the two other features in the character which we gave to those ages, we may observe that Dogmatism demands for philosophical theories the submission of mind, due to those revealed religious doctrines which are to guide our conduct and direct our hopes: while Mysticism elevates ideas into realities, and offers them to us as the objects of our religious regard. Thus the Mysticism of the middle ages and their Dogmatism alike arose from not discriminating the offices of theoretical and practical philosophy. Mysticism claimed for ideas the dignity and reality of principles of moral action and religious hope: Dogmatism imposed theoretical opinions respecting speculative points with the imperative tone of rules of conduct and faith.

If, however, the opposite claims of theory and practice interfered with the progress of science by the confusion they thus occasioned, they did so far more by drawing men away altogether from mere physical speculations. The Christian religion, with its precepts, its hopes, and its promises, became the leading subject of men's thoughts; and the great active truths thus revealed, and the duties thus enjoined, made all inquiries of mere curiosity appear frivolous and unworthy of man. The Fathers of the Church sometimes philosophized ill; but far more commonly they were too intent upon the great lessons which they had to teach, respecting man's situation in the eyes of his Heavenly Master, to philosophize at all respecting things remote from the business of life and of no importance in man's spiritual concerns.

Yet man has his intellectual as well as his spiritual wants. He has faculties which demand systems and reasons, as well as precepts and promises. The Christian doctor, who knew so much more than the heathen philosopher respecting the Creator and Governor of the universe, was not long content to know or to teach less, respecting the universe itself. While it was still maintained that Theology was the only really important study, Theology was so extended and so fashioned as to include all other knowledge: and after no long time, the Fathers of the Church themselves became the authors of systems of universal knowledge.

But when this happened, the commentatorial spirit was still in its full vigour. The learned Christians could not, any more than the later Greeks or the Romans, devise, by the mere force of their own invention, new systems, full, comprehensive, and connected, like those of the heroic age of philosophy. The same mental tendencies which led men to look for speculative coherence and completeness in the view of the universe, led them also to admire and dwell upon the splendid and acute speculations of the Greeks. They were content to find, in these immortal works, the answers to the questions which their curiosity prompted; and to seek what further satisfaction they might require, in analyzing and unfolding the doctrines promulgated by those great masters of knowledge. Thus the Christian doctors became, as to general philosophy, commentators upon the ancient Greek teachers.

Among these, they selected Aristotle as their peculiar object of admiration and study. The vast store, both of opinions and facts, which his works contain, his acute distinctions, his cogent reasons in some portions of his speculations, his symmetrical systems in almost all, naturally commended him to the minds of subtle and curious men. We may add that Plato, who taught men to contemplate Ideas separate from Things, was not so well fitted for general acceptance as Aristotle, who rejected this separation. For although the due apprehension of this opposition of ideas and sensations is a necessary step in the progress of true philosophy, it requires a clearer view and a more balanced mind than the common herd of students possess; and Aristotle, who evaded the necessary perplexities in which this antithesis involves us, appeared, to the temper of those times, the easier and the plainer guide of the two.

The Doctors of the middle ages having thus adopted Aristotle as their master in philosophy, we shall not be surprized to find them declaring, after him, that experience is the source of our knowledge of the visible world. But though, like the Greeks, they thus talked of experiment, like the Greeks, they showed little disposition to discover the laws of nature by observation of facts. This barren and formal recognition of experience or sensation as one source of knowledge, not being illustrated by a practical study of nature, and by real theoretical truths obtained by such a study, remained ever vague, wavering, and empty. Such a mere acknowledgement cannot, in any times, ancient or modern, be considered as indicating a just apprehension of the true basis and nature of science.

In imperfectly perceiving how, and how far, experience is the source of our knowledge of the external world, the teachers of the middle ages were in the dark; but so, on this subject, have been almost all the writers of all ages, with the exception of those who in recent times have had their minds enlightened by contemplating philosophically the modern progress of science. The opinions of the doctors of the middle ages on such subjects generally had those of Aristotle for their basis; but the subject was often still further analyzed and systematized, with an acute and methodical skill hardly inferior to that of Aristotle himself.

The Stagirite, in the beginning of his *Physics*, had made the following remarks. "In all bodies of doctrine which involve principles, causes, or elements, Science and Knowledge arise from the knowledge of these; (for we then consider ourselves to *know* respecting any subject, when we know its first cause, its first principles, its ultimate elements.) It is evident, therefore, that in seeking a knowledge of nature, we must first know what are its principles. But the course of our knowledge is, from the things which are better known and more manifest to us, to the things which are more certain and evident in nature. For those things which are most evident in truth, are not most evident to us.

[And consequently we must advance from things obscure in nature, but manifest to us, towards the things which are really in nature more clear and certain.] The things which are first obvious and apparent to us are complex; and from these we obtain, by analysis, principles and elements. We must proceed from universals to particulars. For the whole is better known to our senses than the parts, and for the same reason, the universal better known than the particular. And thus words signify things in a large and indiscriminate way, which is afterwards analyzed by definition; as we see that the children at first call all men *father*, and all women *mother*, but afterwards learn to distinguish."

There are various assertions contained in this extract which came to be considered as standard maxims, and which occur constantly in the writers of the middle ages. Such are, for instance, the maxim, "Verè scire est per causas scire;" the remark, that compounds are known to us before their parts, and the illustration from the expressions used by children. Of the mode in which this subject was treated by the schoolmen, we may judge by looking at passages of Thomas Aquinas which treat of the subject of the human understanding. In the *Summa Theologiæ*, the eighty-fifth Question is *On the manner and order of understanding*, which subject he considers in eight Articles; and these must, even now, be looked upon as exhibiting many of the most important and interesting points of the subject. They are, *First*, Whether our understanding understands by abstracting ideas (*species*) from appearances; *Second*, Whether intelligible species abstracted from appearances are related to our understanding as that *which* we understand, or that *by which* we understand; *Third*, Whether our understanding does naturally understand universals first; *Fourth*, Whether our understanding can understand many things at once;

Fifth, Whether our understanding understands by compounding and dividing; *Sixth*, Whether the understanding can err; *Seventh*, Whether one person can understand the same thing better than another; *Eighth*, Whether our understanding understands the indivisible sooner than the divisible. And in the discussion of the last point, for example, reference is made to the passage of Aristotle which we have already quoted. "It may seem," he says, "that we understand the indivisible before the divisible; for *the Philosopher* says that we understand and know by knowing principles and elements; but indivisibles are the principles and elements of divisible things. But to this we may reply, that in our receiving of science, principles and elements are not always first; for sometimes from the sensible effects we go on to the knowledge of intelligible principles and causes." We see that both the objection and the answer are drawn from Aristotle.

We find the same close imitation of Aristotle in Albertus Magnus, who, like Aquinas, flourished in the thirteenth century. Albertus, indeed, wrote treatises corresponding to almost all those of the Stagirite, and was called the *Ape of Aristotle*. In the beginning of his *Physics*, he says, "Knowledge does not always begin from that which is first according to the nature of things, but from that of which the knowledge is easiest. For the human intellect, on account of its relation to the senses (*propter reflexionam quam habet ad sensum*), collects science from the senses; and thus it is easier for our knowledge to begin from that which we can apprehend by sense, imagination, and intellect, than from that which we apprehend by intellect alone." We see that he has somewhat systematized what he has borrowed.

This disposition to dwell upon and systematize the leading doctrines of metaphysics assumed a more definite and permanent shape in the opposition of the

Realists and Nominalists. The opposition involved in this controversy is, in fact, that fundamental antithesis of Sense and Ideas about which philosophy has always been engaged; and of which we have marked the manifestation in Plato and Aristotle. The question, What is the object of our thoughts when we reason concerning the external world? must occur to all speculative minds: and the difficulties of the answer are manifest. We must reply, either that our own Ideas, or that Sensible Things, are the elements of our knowledge of nature. And then the scruples again occur,—how we have any *general* knowledge if our thoughts are fixed on particular objects; and, on the other hand,—how we can attain to any *true* knowledge of nature by contemplating ideas which are not identical with objects in nature. The two opposite opinions maintained on this subject were, on the one side,—that our general propositions refer to objects which are *real*, though divested of the peculiarities of individuals; and, on the other side,—that in such propositions, individuals are not represented by any reality, but bound together by a *name*. These two views were held by the Realists and Nominalists respectively: and thus the Realist manifested the adherence to Ideas, and the Nominalist the adherence to the impressions of Sense, which have always existed as opposite yet correlative tendencies in man.

The Realists were the prevailing sect in the Scholastic times: for example, both Thomas Aquinas and Duns Scotus, the *Angelical* and the *Subtle* Doctor, held this opinion, although opposed to each other in many of their leading doctrines on other subjects. And as the Nominalist, fixing his attention upon sensible objects, is obliged to consider what is the *principle of generalization*, in order that the possibility of any general proposition may be conceivable; so on the other hand, the Realist, beginning with the contemplation of universal ideas, is compelled to ask what is the *principle of individuation*, in order that he may comprehend the application of general propositions in each particular instance. This inquiry concerning the principle of individuation was accordingly a problem which occupied all the leading minds among the Schoolmen*. It will be apparent from what has been said, that it is only one of the many forms of the fundamental antithesis of the Ideas and the Senses, which we have constantly before us in this review.

The recognition of the derivation of our knowledge, in part at least, from Experience, though always loose and incomplete, appears often to be independent of the Peripatetic traditions. Thus Richard of St. Victor, a writer of contemplative theology in the twelfth century, says†, that "there are three sources of knowledge, experience, reason, faith. Some things we prove by experiment, others we collect by reasoning, the certainty of others we hold by believing. And with regard to temporal matters, we obtain our knowledge by actual experience; the other guides belong to divine knowledge." Richard also propounds a division of human knowledge which is clearly not derived directly from the ancients, and which shows that considerable attention must have been paid to such speculations. He begins by laying down clearly and broadly the distinction, which, as we have seen, is of primary importance, between *practice* and *theory*. *Practice*, he says, includes seven mechanical arts; those of the clothier, the armourer, the navigator, the hunter, the physician, and the player. *Theory* is threefold, divine, natural, doctrinal; and is thus divided into Theology, Physics, and Mathematics. *Mathematics*, he adds, treats

* See the opinion of Aquinas, in Degerando, *Hist. Com. des Syst.* IV. 499; of Duns Scotus, *ib.*, IV. 523.

† *Liber Excerptionum*, Lib. I. c. i.

of the invisible *forms* of visible things. We have seen that by many profound thinkers this word *forms* has been selected as best fitted to describe those relations of things which are the subject of mathematics. Again, Physics discovers causes from their effects and effects from their causes. It would not be easy at the present day to give a better account of the object of physical science. But Richard of St. Victor makes this account still more remarkably judicious, by the examples to which he alludes; which are earthquakes, the tides, the virtues of plants, the instincts of animals, the classification of minerals, plants and reptiles.

> Unde tremor terris, quâ vi maria alta tumescant,
> Herbarum vires, animos irasque ferarum,
> Omne genus fruticum, lapidum quoque, reptiliumque.

He further adds*, "Physical science ascends from effects to causes, and descends again from causes to effects." This declaration Francis Bacon himself might have adopted. It is true, that Richard would probably have been little able to produce any clear and definite instances of knowledge, in which this ascent and descent were exemplified; but still the statement, even considered as a mere conjectural thought, contains a portion of that sagacity and comprehensive power which we admire so much in Bacon.

Richard of St. Victor, who lived in the twelfth century, thus exhibits more vigour and independence of speculative power than Thomas Aquinas, Albertus Magnus, and Duns Scotus, in the thirteenth. In the interval, about the end of the twelfth century, the writings of Aristotle had become generally known in the West; and had been elevated into the standard of philosophical doctrine, by the divines mentioned above, who felt a reverent sympathy with the systematizing and subtle

* *Tr. Ex.* Lib. I. c. vii.

spirit of the Stagirite as soon as it was made manifest to them. These doctors, following the example of their great forerunner, reduced every part of human knowledge to a systematic form; the systems which they thus framed were presented to men's minds as the only true philosophy, and dissent from them was no longer considered to be blameless. It was an offence against religion as well as reason to reject the truth, and the truth could be but one. In this manner arose that claim which the Doctors of the Church put forth to control men's opinions upon all subjects, and which we have spoken of in the *History of Science* as the Dogmatism of the Middle Ages. There is no difficulty in giving examples of this characteristic. We may take for instance a Statute of the University of Paris, occasioned by a Bull of Pope John XXI., in which it is enacted, "that no Master or Bachelor of any faculty, shall presume to read lectures upon any author in a private room, on account of the many perils which may arise therefrom; but shall read in public places, where all may resort, and may faithfully report what is there taught; excepting only books of Grammar and Logic, in which there can be no presumption." And certain errors of Brescain are condemned in a Rescript* of the papal Legate Odo, with the following expressions: "Whereas, as we have been informed, certain Logical professors treating of Theology in their disputations, and Theologians treating of Logic, contrary to the command of the law are not afraid to mix and confound the lots of the Lord's heritage; we exhort and admonish your University, all and singular, that they be content with the landmarks of the Sciences and Faculties which our Fathers have fixed; and that having due fear of the curse pronounced in the law against him who removeth his neighbour's landmark,

* Tenneman, VIII. 461.

you hold such sober wisdom according to the Apostles, that ye may by no means incur the blame of innovation or presumption."

The account which, in the *History of Science*, I gave of Dogmatism as a characteristic of the middle ages, has been indignantly rejected by a very pleasing modern writer, who has, with great feeling and great diligence, brought into view the merits and beauties of those times, termed by him *Ages of Faith*. He urges * that religious authority was never claimed for physical science: and he quotes from Thomas Aquinas, a passage in which the author protests against the practice of confounding opinions of philosophy with doctrines of faith. We might quote in return the Rescript † of Stephen, bishop of Paris, in which he declares that there can be but one truth, and rejects the distinction of things being true according to philosophy and not according to the Catholic faith; and it might be added, that among the errours condemned in this document are some of Thomas Aquinas himself. We might further observe, that if no physical doctrines were condemned in the times of which we now speak, this was because, on such subjects, no new opinions were promulgated, and not because opinion was free. As soon as new opinions, even on physical subjects, attracted general notice, they were prohibited by authority, as we see in the case of Galileo ‡.

* *Mores Catholici, or Ages of Faith*, VIII p. 247.

† Tenneman, VIII. 460.

‡ If there were any doubt on this subject, we might refer to the writers who afterwards questioned the supremacy of Aristotle, and who with one voice assert that an infallible authority had been claimed for him. Thus Laurentius Valla: "Quo minus ferendi sunt recentes Peripatetici, qui nullius sectæ hominibus interdicunt libertate ab Aristotle dissentiendi, quasi sophos hic, non philosophus." *Pref. in Dial.* (Tenneman, IX. 29.) So Ludovicus Vives: "Sunt ex philosophis et ex theologis qui non solem quo Aristoteles pervenit extremum esse aiunt na-

But this disinclination to recognize philosophy as independent of religion, and this disposition to find in new theories, even in physical ones, something contrary to religion or scripture, are, it would seem, very natural tendencies of theologians; and it would be unjust to assert that these propensities were confined to the periods when the authority of papal Rome was highest; or that the spirit which has in a great degree controlled and removed such habits was introduced by the Reformation of religion in the sixteenth century. We must trace to other causes, the clear and general recognition of Philosophy, as distinct from Theology, and independent of her authority. In the earlier ages of the Church, indeed, this separation had been acknowledged. St. Augustin says, "A Christian should beware how he speaks on questions of natural philosophy, as if they were doctrines of Holy Scripture; for an infidel who should hear him deliver absurdities could not avoid laughing. Thus the Christian would be confused, and the infidel but little edified; for the infidel would conclude that our authors really entertained these extravagant opinions, and therefore they would despise them, to their own eternal ruin. Therefore the opinions of philosophers should never be proposed as dogmas of faith, or rejected as contrary to faith, when it is not certain that they are so." These words are quoted with

turæ, sed quâ pervenit eam rectissimam esse omnium et certissimam in natura viam." (Tenneman, IX. 43.) We might urge too, the evasions practised by philosophical Reformers, through fear of the dogmatism to which they had to submit; for example, the protestation of Telesius at the end of the Proem to his work, *De Rerum Natura:* "Nec tamen, si quid eorum quæ nobis posita sunt, sacris literis, Catholicæve ecclesiæ decretis non cohæreat, tenendum id, quin penitus rejiciendum asseveramus contendimusque. Neque enim *humana* modo *ratio* quævis, sed *ipse* etiam *sensus* illis *posthabendus*, et si illis non congruat, abnegandus omnino et ipse etiam est sensus."

approbation by Thomas Aquinas, and it is said*, are cited in the same manner in every encyclopedical work of the middle ages. This warning of genuine wisdom was afterwards rejected, as we have seen; and it is only in modern times that its value has again been fully recognized. And this improvement we must ascribe, mainly, to the progress of physical science. For a great body of undeniable truths on physical subjects being accumulated, such as had no reference to nor connexion with the truths of religion, and yet such as possessed a strong interest for most men's minds, it was impossible longer to deny that there were wide provinces of knowledge which were not included in the dominions of Theology, and over which she had no authority. In the fifteenth and sixteenth centuries, the fundamental doctrines of mechanics, hydrostatics, optics, magnetics, chemistry, were established and promulgated; and along with them, a vast train of consequences, attractive to the mind by the ideal relations which they exhibited, and striking to the senses by the power which they gave man over nature. Here was a region in which philosophy felt herself entitled and impelled to assert her independence. From this region, there is a gradation of subjects in which philosophy advances more and more towards the peculiar domain of religion; and at some intermediate points there have been, and probably will always be, conflicts respecting the boundary line of the two fields of speculation. For the limit is vague and obscure, and appears to fluctuate and shift with the progress of time and knowledge.

Our business at present is not with the whole extent and limits of philosophy, but with the progress of physical science more particularly, and the methods by

* *Ages of Faith*, VIII. 247: to the author of which I am obliged for this quotation.

which it may be attained: and we are endeavouring to trace historically the views which have prevailed respecting such methods, at various periods of man's intellectual progress. Among the most conspicuous of the revolutions which opinions on this subject have undergone, is the transition from an implicit trust in the internal powers of man's mind to a professed dependence upon external observation; and from an unbounded reverence for the wisdom of the past, to a fervid expectation of change and improvement. The origin and progress of this disposition of mind;—the introduction of a state of things in which men not only obtained a body of indestructible truths from experience, and increased it from generation to generation, but professedly, and we may say, ostentatiously, declared such to be the source of their knowledge, and such their hopes of its destined career;—the rise, in short, of Experimental Philosophy, not only as a habit, but as a Philosophy of Experience, is what we must now endeavour to exhibit.

Chapter VII.

THE INNOVATORS OF THE MIDDLE AGES.

1. *General Remarks.*—In the rise of Experimental Philosophy, understanding the term in the way just now stated, two features have already been alluded to: the disposition to cast off the prevalent reverence for the opinions and methods of preceding teachers with an eager expectation of some vast advantage to be derived from a change; and the belief that this improvement must be sought by drawing our knowledge from external observation rather than from mere intellectual efforts; —*the Insurrection against Authority*, and *the Appeal*

to Experience. These two movements were closely connected; but they may easily be distinguished, and in fact, persons were very prominent in the former part of the task, who had no comprehension of the latter principle, from which alone the change derives its value. There were many Malcontents who had not the temper, talent or knowledge, which fitted them to be Reformers.

The authority which was questioned, in the struggles of which we speak, was that of the Scholastic System, the combination of Philosophy with Theology; of which Aristotle, presented in the form and manner which the Doctors of the Church had imposed upon him, is to be considered the representative. When there was demanded of men a submission of the mind, such as this system claimed, the natural love of freedom in man's bosom, and the speculative tendencies of his intellect, rose in rebellion, from time to time, against the ruling oppression. We find in all periods of the scholastic ages examples of this disposition of man to resist overstrained authority; the tendency being mostly, however, combined with a want of solid thought, and showing itself in extravagant pretensions and fantastical systems put forwards by the insurgents. We have pointed out one such opponent* of the established systems, even among the Arabian schoolmen, a more servile race than ever the Europeans were. We may here notice more especially an extraordinary character who appeared in the thirteenth century, and who may be considered as belonging to the Prelude of the Reform in Philosophy, although he had no share in the Reform itself.

2. *Raymond Lully.*—Raymond Lully is perhaps traditionally best known as an Alchemist, of which art he appears to have been a cultivator. But this was only one of the many impulses of a spirit ardently thirsty

* Algazel. See *Hist. Ind. Sci.*, B. IV. c. i.

of knowledge and novelty. He had*, in his youth, been a man of pleasure, but was driven by a sudden shock of feeling to resolve on a complete change of life. He plunged into solitude, endeavoured to still the remorse of his conscience by prayer and penance, and soon had his soul possessed by visions which he conceived were vouchsafed him. In the feeling of religious enthusiasm thus excited, he resolved to devote his life to the diffusion of Christian truth among Heathens and Mahomedans. For this purpose, at the age of thirty he betook himself to the study of Grammar, and of the Arabic language. He breathed earnest supplications for an illumination from above; and these were answered by his receiving from heaven, as his admirers declare, his *Ars Magna*, by which he was able without labour or effort to learn and apply all knowledge. The real state of the case is, that he put himself in opposition to the established systems, and propounded a New Art, from which he promised the most wonderful results; but that his Art really is merely a mode of combining ideal conceptions without any reference to real sources of knowledge, or any possibility of real advantage. In a Treatise addressed, in A. D. 1310, to King Philip of France, entitled *Liber Lamentationis Duodecim Principiorum Philosophiæ contra Averroistas*, Lully introduces Philosophy, accompanied by her twelve Principles, (Matter, Form, Generation, &c.) uttering loud complaints against the prevailing system of doctrine; and represents her as presenting to the king a petition that she may be upheld and restored by her favourite, the Author. His *Tabula Generalis ad omnes Scientias applicabilis* was begun the 15th September, 1292, in the Harbour of Tunis, and finished in 1293, at Naples. In order to frame an Art of thus tabulating all existing sciences,

* Tenneman, VIII. 830.

and indeed all possible knowledge, he divides into various classes the conceptions with which he has to deal. The first class contains nine *Absolute Conceptions:* Goodness, Greatness, Duration, Power, Wisdom, Will, Virtue, Truth, Majesty. The second class has nine *Relative Conceptions:* Difference, Identity, Contrariety, Beginning, Middle, End, Majority, Equality, Minority. The third class contains nine *Questions:* Whether? What? Whence? Why? How great? How circumstanced? When? Where? and How? The fourth class contains the nine *Most General Subjects:* God, Angel, Heaven, Man, *Imaginativum, Sensitivum, Vegetativum, Elementativum, Instrumentativum.* Then come nine *Prædicaments,* nine *Moral Qualities,* and so on. These conceptions are arranged in the compartments of certain concentric moveable circles, and give various combinations by means of triangles and other figures, and thus propositions are constructed.

It must be clear at once, that real knowledge, which is the union of facts and ideas, can never result from this machinery for shifting about, joining and disjoining, empty conceptions. This, and all similar schemes, go upon the supposition that the logical combinations of notions do of themselves compose knowledge; and that really existing things may be arrived at by a successive system of derivation from our most general ideas. It is imagined that by distributing the nomenclature of abstract ideas according to the place which they can hold in our propositions, and by combining them according to certain conditions, we may obtain formulæ including all possible truths, and thus fabricate a science in which all sciences are contained. We thus obtain the means of talking and writing upon all subjects, without the trouble of thinking: the revolutions of the emblematical figures are substituted for the operations of the mind. Both exertion of thought, and knowledge of facts, become superfluous. And this reflection, adds an intelligent author*, explains the enormous number of books which Lully is said to have written; for he might have written those even during his sleep, by the aid of a moving power which should keep his machine in motion. Having once devised this invention for manufacturing science, Lully varied it in a thousand ways, and followed it into a variety of developements. Besides Synoptical Tables, he employs Genealogical Trees, each of which he dignifies with the name of the Tree of Science. The only requisite for the application of his System was a certain agreement in the numbers of the classes into which different subjects were distributed; and as this symmetry does not really exist in the operations of our thoughts, some violence was done to the natural distinction and subordination of conceptions, in order to fit them for the use of the System.

Thus Lully, while he professed to teach an Art which was to shed new light upon every part of science, was in fact employed in a pedantic and trifling repetition of known truths or truisms; and while he complained of the errours of existing methods, he proposed in their place one which was far more empty, barren, and worthless, than the customary processes of human thought. Yet his method is spoken of† with some praise by Leibnitz, who indeed rather delighted in the region of ideas and words, than in the world of realities. But Francis Bacon speaks far otherwise and more justly on this subject‡. "It is not to be omitted that some men, swollen with emptiness rather than knowledge, have laboured to produce a certain Method, not deserving the name of a legitimate Method, since it is rather

* Degerando, IV. 535. † *Opera*, v. 16.
‡ *Works*, VII. 296.

a method of imposture: which yet is doubtless highly grateful to certain would-be philosophers. This method scatters about certain little drops of science in such a manner that a smatterer may make a perverse and ostentatious use of them with a certain show of learning. Such was the Art of Lully, which consisted of nothing but a mass and heap of the words of each science; with the intention that he who can readily produce the words of any science shall be supposed to know the science itself. Such collections are like a rag shop, where you find a patch of everything, but nothing which is of any value."

3. *Roger Bacon.*—We now come to a philosopher of a very different character, who was impelled to declare his dissent from the reigning philosophy by the abundance of his knowledge, and by his clear apprehension of the mode in which real knowledge had been acquired and must be increased.

Roger Bacon was born in 1214, near Ilchester, in Somersetshire, of an old family. In his youth he was a student at Oxford, and made extraordinary progress in all branches of learning. He then went to the University of Paris, as was at that time the custom of learned Englishmen, and there received the degree of Doctor of Theology. At the persuasion of Robert Grostête, bishop of Lincoln, he entered the brotherhood of Franciscans in Oxford, and gave himself up to study with extraordinary fervour. He was termed by his brother monks *Doctor Mirabilis.* We know from his own works, as well as from the traditions concerning him, that he possessed an intimate acquaintance with all the science of his time which could be acquired from books; and that he had made many remarkable advances by means of his own experimental labours. He was acquainted with Arabic, as well as with the other languages common in his time. In the title of his works, we find the whole range of science and philosophy, Mathematics and Mechanics, Optics, Astronomy, Geography, Chronology, Chemistry, Magic, Music, Medicine, Grammar, Logic, Metaphysics, Ethics, and Theology; and judging from those which are published, these works are full of sound and exact knowledge. He is, with good reason, supposed to have discovered, or to have had some knowledge of, several of the most remarkable inventions which were made generally known soon afterwards; as gunpowder, lenses, burning specula, telescopes, clocks, the correction of the calendar, and the explanation of the rainbow.

Thus possessing, in the acquirements and habits of his own mind, abundant examples of the nature of knowledge and of the process of invention, Roger Bacon felt also a deep interest in the growth and progress of science, a spirit of inquiry respecting the causes which produced or prevented its advance, and a fervent hope and trust in its future destinies; and these feelings impelled him to speculate worthily and wisely respecting a Reform of the Method of Philosophizing. The manuscripts of his works have existed for nearly six hundred years in many of the libraries of Europe, and especially in those of England; and for a long period the very imperfect portions of them which were generally known, left the character and attainments of the author shrouded in a kind of mysterious obscurity. About a century ago, however, his *Opus Majus* was published* by Dr. S. Jebb, principally from a manuscript in the library of Trinity College, Dublin; and this contained most or all of the

* *Fratris Rogeri Bacon Ordinis Minorum* Opus Majus *ad Clementem Quartum, Pontificem Romanum, ex MS. Codice Dubliniensi cum aliis quibusdam collato nunc primum edidit* S. Jebb, M.D. Londini, 1733.

separate works which were previously known to the public, along with others still more peculiar and characteristic. We are thus able to judge of Roger Bacon's knowledge and of his views, and they are in every way well worthy our attention.

The *Opus Majus* is addressed to Pope Clement the Fourth, whom Bacon had known when he was legate in England as Cardinal-bishop of Sabina, and who admired the talents of the monk, and pitied him for the persecutions to which he was exposed. On his elevation to the papal chair, this account of Bacon's labours and views was sent, at the earnest request of the pontiff. Besides the *Opus Majus*, he wrote two others, the *Opus Minus* and *Opus Tertium;* which were also sent to the pope, as the author says*, "on account of the danger of roads, and the possible loss of the work." These works still exist unpublished, in the Cottonian and other libraries.

The *Opus Majus* is a work equally wonderful with regard to its general scheme, and to the special treatises with which the outlines of the plan are filled up. The professed object of the work is to urge the necessity of a reform in the mode of philosophizing, to set forth the reasons why knowledge had not made a greater progress, to draw back attention to the sources of knowledge which had been unwisely neglected, to discover other sources which were yet almost untouched, and to animate men in the undertaking, by a prospect of the vast advantages which it offered. In the developement of this plan, all the leading portions of science are expounded in the most complete shape which they had at that time assumed; and improvements of a very wide and striking kind are proposed in some of the principal of these departments. Even if the work had had no

* *Opus Majus*, Præf.

leading purpose, it would have been highly valuable as a treasure of the most solid knowledge and soundest speculations of the time; even if it had contained no such details, it would have been a work most remarkable for its general views and scope. It may be considered as, at the same time, the *Encyclopedia* and the *Novum Organon* of the thirteenth century.

Since this work is thus so important in the history of Inductive Philosophy I shall give, in a note, a view* of its divisions and contents. But I must now endeavour to point out more especially the way in which the various principles, which the reform of scientific method involved, are here brought into view.

* Contents of Roger Bacon's *Opus Majus*.

Part I. On the four causes of human ignorance:—Authority, Custom, Popular Opinion, and the Pride of supposed Knowledge.

Part II. On the source of perfect wisdom in the Sacred Scripture.

Part III. On the Usefulness of Grammar.

Part IV. On the Usefulness of Mathematics.

(1.) The necessity of Mathematics in Human Things (published separately as the *Specula Mathematica*).

(2.) The necessity of Mathematics in Divine Things.—1°. This study has occupied holy men: 2°. Geography: 3°. Chronology: 4°. Cycles; the Golden Number, &c.: 5°. Natural Phenomena, as the Rainbow: 6°. Arithmetic: 7°. Music.

(3.) The Necessity of Mathematics in Ecclesiastical Things. 1°. The Certification of Faith: 2°. The Correction of the Calendar.

(4.) The Necessity of Mathematics in the State.—1°. Of Climates: 2°. Hydrography: 3°. Geography: 4°. Astrology.

Part V. On Perspective (published separately as *Perspectiva*).

(1.) The organs of vision.

(2.) Vision in straight lines.

(3.) Vision reflected and refracted.

(4.) De multiplicatione specierum (on the propagation of the impressions of light, heat, &c.)

Part VI. On Experimental Science.

One of the first points to be noticed for this purpose, is the resistance to authority; and at the stage of philosophical history with which we here have to do, this means resistance to the authority of Aristotle, as adopted and interpreted by the Doctors of the Schools. Bacon's work* is divided into Six Parts; and of these Parts, the First is, Of the four universal Causes of all Human Ignorance. The causes thus enumerated† are:—the force of unworthy authority;—traditionary habit;—the imperfection of the undisciplined senses;—and the disposition to conceal our ignorance and to make an ostentatious show of our knowledge. These influences involve every man, occupy every condition. They prevent our obtaining the most useful and large and fair doctrines of wisdom, the secrets of all sciences and arts. He then proceeds to argue, from the testimony of philosophers themselves, that the authority of antiquity, and especially of Aristotle, is not infallible. "We find‡ their books full of doubts, obscurities, and perplexities. They scarce agree with each other in one empty question or one worthless sophism, or one operation of science, as one man agrees with another in the practical operations of medicine, surgery, and the like arts of Secular men. Indeed," he adds, "not only the philosophers, but the saints have fallen into errours which they have afterwards retracted," and this he instances in Augustin, Jerome, and others. He gives an admirable sketch of the progress of philosophy from the Ionic School to Aristotle; of whom he speaks with great applause. "Yet," he adds§, "those who came after him corrected him in some things, and added many things to his works, and shall go on adding to the end of the world." Aristotle, he adds, is now called peculiarly‖ the Philoso-

* *Op. Maj.*, p. 1. † *Ib.*, p. 2. ‡ *Ib.*, p. 10.
§ *Op. Maj.*, p. 36. ‖ *Autonomaticè.*

pher, "yet there was a time when his philosophy was silent and unregarded, either on account of the rarity of copies of his works, or their difficulty, or from envy; till after the time of Mahomet, when Avicenna and Averroes, and others, recalled this philosophy into the full light of exposition. And although the Logic, and some other works were translated by Boethius from the Greek, yet the philosophy of Aristotle first received a quick increase among the Latins at the time of Michael Scot; who, in the year of our Lord 1230, appeared, bringing with him portions of the books of Aristotle on Natural Philosophy and Mathematics. And yet a small part only of the works of this author is translated, and a still smaller part is in the hands of common students." He adds further* (in the Third Part of the *Opus Majus*, which is a Dissertation on Language) that the translations which are current of these writings, are very bad and imperfect. With these views, he is moved to express himself somewhat impatiently† respecting these works: "If I had," he says, "power over the works of Aristotle, I would have them all burnt; for it is only a loss of time to study in them, and a course of errour, and a multiplication of ignorance beyond expression." "The common herd of students," he says, "with their heads, have no principle by which they can be excited to any worthy employment; and hence they mope and

* *Op. Maj.*, p 46.

† See *Pref.* to Jebb's edition. The passages there quoted, however, are not extracts from the *Opus Majus*, but (apparently) from the *Opus Minus* (*MS. Cott.* Tib. c. 5.) "Si haberem potestatem supra libros Aristotelis, ego facerem omnes cremari; quia non est nisi temporis amissio studere in illis, et causa erroris, et multiplicatio ignorantiæ ultra id quod valeat explicari. . . . Vulgus studentum cum capitibus suis non habet unde excitetur ad aliquid dignum, et ideo languet et *asininat* circa male translata, et tempus et studium amittit in omnibus et expensas."

and make asses of themselves over their bad translations, and lose their time, and trouble, and money."

The remedies which he recommends for these evils, are, in the first place, the study of that only perfect wisdom which is to be found in the sacred Scripture*, in the next place, the study of mathematics and the use of experiment†. By the aid of these methods, Bacon anticipates the most splendid progress for human knowledge. He takes up the strain of hope and confidence which we have noticed as so peculiar in the Roman writers; and quotes some of the passages of Seneca which we adduced in illustration of this:—that the attempts in science were at first rude and imperfect, and were afterwards improved;—that the day will come, when what is still unknown shall be brought to light by the progress of time and the labours of a longer period;—that one age does not suffice for inquiries so wide and various;—that the people of future times shall know many things unknown to us;—and that the time shall arrive when posterity will wonder that we overlooked what was so obvious. Bacon himself adds anticipations more peculiarly in the spirit of his own time. "We have seen," he says, at the end of the work, "how Aristotle, by the ways which wisdom teaches, could give to Alexander the empire of the world. And this the Church ought to take into consideration against the infidels and rebels, that there may be a sparing of Christian blood, and especially on account of the troubles that shall come to pass in the days of Antichrist; which by the grace of God, it would be easy to obviate, if prelates and princes would encourage study, and join in searching out the secrets of nature and art."

It may not be improper to observe here that this belief in the appointed progress of knowledge, is not

* Part II. † Parts IV., V. and VI.

combined with any overweening belief in the unbounded and independent power of the human intellect. On the contrary, one of the lessons which Bacon draws from the state and prospects of knowledge, is the duty of faith and humility. "To him," he says*, "who denies the truth of the faith because he is unable to understand it, I will propose in reply the course of nature, and as we have seen it in examples." And after giving some instances, he adds, "These, and the like, ought to move men and to excite them to the reception of divine truths. For if, in the vilest objects of creation, truths are found, before which the inward pride of man must bow, and believe though it cannot understand, how much more should man humble his mind before the glorious truths of God!" He had before said†: "Man is incapable of perfect wisdom in this life; it is hard for him to ascend towards perfection, easy to glide downwards to falsehoods and vanities: let him then not boast of his wisdom, or extol his knowledge. What he knows is little and worthless, in respect of that which he believes without knowing; and still less, in respect of that which he is ignorant of. He is mad who thinks highly of his wisdom; he most mad, who exhibits it as something to be wondered at." He adds, as another reason for humility, that he has proved by trial, he could teach in one year, to a poor boy, the marrow of all that the most diligent person could acquire in forty years' laborious and expensive study.

To proceed somewhat more in detail with regard to Roger Bacon's views of a Reform in Scientific Inquiry, we may observe that by making Mathematics and Experiment the two great points of his recommendation, he directed his improvement to the two essential parts of all knowledge, Ideas and Facts, and thus took the course

* *Op. Maj.*, p. 476. † *Ib.*, p. 15.

which the most enlightened philosophy would have suggested. He did not urge the prosecution of experiment, to the comparative neglect of the existing mathematical sciences and conceptions; a fault which there is some ground for ascribing to his great namesake and successor Francis Bacon: still less did he content himself with a mere protest against the authority of the schools, and a vague demand for change, which was almost all that was done by those who put themselves forward as reformers in the intermediate time. Roger Bacon holds his way steadily between the two poles of human knowledge; which, as we have seen, it is far from easy to do. "There are two modes of knowing," says he*; "by argument, and by experiment. Argument concludes a question; but it does not make us feel certain, or acquiesce in the contemplation of truth, except the truth be also found to be so by experience." It is not easy to express more decidedly the clearly seen union of exact conceptions with certain facts, which, as we have explained, constitutes real knowledge.

One large division of the *Opus Majus* is "On the Usefulness of Mathematics," which is shown by a copious enumeration of existing branches of knowledge, as Chronology, Geography, the Calendar, and (in a separate Part) Optics. There is a chapter†, "in which it is proved by reason, that all science requires mathematics." And the arguments which are used to establish this doctrine, show a most just appreciation of the office of mathematics in science. They are such as follows:—

* *Op. Maj.* p. 445, see also p. 448. "Scientiæ aliæ sciunt sua principia invenire per experimenta, sed conclusiones per argumenta facta ex principiis inventis. Si vero debeant habere experientiam conclusionum suarum particularem et completam, tunc oportet quod habeant per adjutorium istius scientiæ nobilis, (experimentalis.)"

† *Op. Maj.*, p. 60.

That other sciences use examples taken from mathematics as the most evident:—That mathematical knowledge is, as it were, innate in us, on which point he refers to the well known dialogue of Plato, as quoted by Cicero:—That this science, being the easiest, offers the best introduction to the more difficult:—That in mathematics, things as known to us are identical with things as known to nature:—That we can here entirely avoid doubt and errour, and obtain certainty and truth: —That mathematics is prior to other sciences in nature, because it takes cognizance of quantity, which is apprehended by intuition, (*intuitu intellectus.*) "Moreover," he adds*, "there have been found famous men, as Robert, bishop of Lincoln, and Brother Adam Marshman, (de Marisco) and many others, who by the power of mathematics have been able to explain the causes of things; as may be seen in the writings of these men, for instance, concerning the Rainbow and Comets, and the generation of heat, and climates, and the celestial bodies."

But undoubtedly the most remarkable portion of the *Opus Majus* is the Sixth and last Part, which is entitled "De Scientia experimentali." It is indeed an extraordinary circumstance to find a writer of the thirteenth century, not only recognizing experiment as one source of knowledge, but urging its claims as something far more important than men had yet been aware of, exemplifying its value by striking and just examples, and speaking of its authority with a dignity of diction which sounds like a foremurmur of the Baconian sentences uttered nearly four hundred years later. Yet this is the character of what we here find†. "Experimental science, the sole mistress of

* *Op. Maj.*, p. 64.

† "Veritates magnificas in terminis aliarum scientarium in quas per nullam viam possunt illæ scientia, hæc sola scientiarum domina speculativarum, potest dare." *Op. Maj.*, p. 465.

speculative sciences, has three great Prerogatives among other parts of knowledge: First she tests by experiment the noblest conclusions of all other sciences: Next she discovers respecting the notions which other sciences deal with, magnificent truths to which these sciences of themselves can by no means attain: her Third dignity is, that she by her own power and without respect of other sciences, investigates the secrets of nature."

The examples which Bacon gives of these "Prerogatives" are very curious, exhibiting, among some errour and credulity, sound and clear views. His leading example of the First Prerogative, is the Rainbow, of which the cause, as given by Aristotle, is tested by reference to experiment with a skill which is, even to us now, truly admirable. The examples of the Second Prerogative are three:—*first*, the art of making an artificial sphere which shall move with the heavens by natural influences, which Bacon trusts may be done, though astronomy herself cannot do it—"et tunc," he says, "thesaurum unius regis valeret hoc instrumentum;"—*secondly*, the art of prolonging life, which experiment may teach, though medicine has no means of securing it except by regimen*;—*thirdly*, the art of making gold finer than fine gold, which goes beyond the power of alchemy. The Third Prerogative of experimental science, arts independent of the received sciences, is exemplified in many curious examples, many of them whimsical tra-

* One of the ingredients of a preparation here mentioned, is the flesh of a dragon, which, it appears, is used as food by the Ethiopians. The mode of preparing this food cannot fail to amuse the reader. "Where there are good flying dragons, by the art which they possess, they draw them out of their dens, and have bridles and saddles in readiness, and they ride upon them, and make them bound about in the air in a violent manner, that the hardness and toughness of the flesh may be reduced, as boars are hunted and bulls are baited before they are killed for eating." *Op. Maj.*, p. 470.

ditions. Thus it is said that the character of a people may be altered by altering the air*. Alexander, it seems, applied to Aristotle to know whether he should exterminate certain nations which he had discovered, as being irreclaimably barbarous; to which the philosopher replied, "If you can alter their air, permit them to live, if not, put them to death." In this part, we find the suggestion that the fire-works made by children, of saltpetre, might lead to the invention of a formidable military weapon.

It could not be expected that Roger Bacon, at a time when experimental science hardly existed, could give any *precepts* for the discovery of truth by experiment. But nothing can be a better *example* of the method of such investigation, than his inquiry concerning the cause of the Rainbow. Neither Aristotle, nor Avicenna, nor Seneca, he says, have given us any clear knowledge of this matter, but experimental science can do so. Let the experimenter (*experimentator*) consider the cases in which he finds the same colours, as the hexagonal crystals from Ireland and India; by looking into these he will see colours like these of the rainbow. Many think that this arises from some special virtue of these stones and their hexagonal figure; let therefore the experimenter go on, and he will find the same in other transparent stones, in dark ones as well as in light-coloured. He will find the same effect also in other forms than the hexagon, if they be furrowed in the surface, as the Irish crystals are. Let him consider too, that he sees the same colours in the drops which are dashed from oars in the sunshine;—and in the spray thrown by a mill wheel;—and in the dew drops which lie on the grass in a meadow on a summer morning;—and if a man takes water in his mouth and projects it on one side into a

* *Op. Maj.*, p. 473.

sunbeam;—and if in an oil lamp hanging in the air, the rays fall in certain positions upon the surface of the oil; —and in many other ways, are colours produced. We have here a collection of instances, which are almost all examples of the same kind as the phenomenon under consideration; and by the help of a principle collected by induction from these facts, the colours of the rainbow were afterwards really explained.

With regard to the form and other circumstances of the bow he is still more precise. He bids us measure the height of the bow and of the sun, to show that the center of the bow is exactly opposite to the sun. He explains the circular form of the bow,—its being independent of the form of the cloud, its moving when we move, its flying when we follow,—by its consisting of the reflections from a vast number of minute drops. He does not, indeed, trace the course of the rays through the drop, or account for the precise magnitude which the bow assumes; but he approaches to the verge of this part of the explanation; and must be considered as having given a most happy example of experimental inquiry into nature, at a time when such examples were exceedingly scanty. In this respect, he was more fortunate than Francis Bacon, as we shall hereafter see.

We know but little of the biography of Roger Bacon, but we have every reason to believe that his influence upon his age was not great. He was suspected of magic, and is said to have been put into close confinement in consequence of this charge. In his work he speaks of Astrology, as a science well worth cultivating. "But," says he, "Theologians and Decretists, not being learned in such matters, and seeing that evil as well as good may be done, neglect and abhor such things, and reckon them among Magic Arts." We have already seen, that at the very time when Bacon was thus raising his voice against the habit of blindly following authority, and seeking for all science in Aristotle, Thomas Aquinas was employed in fashioning Aristotle's tenets into that fixed form in which they became the great impediment to the progress of knowledge. It would seem, indeed, that something of a struggle between the progressive and stationary powers of the human mind was going on at this time. Bacon himself says*, "Never was there so great an appearance of wisdom, nor so much exercise of study in so many Faculties, in so many regions, as for this last forty years. Doctors are dispersed everywhere, in every castle, in every burgh, and especially by the students of two Orders, (he means the Franciscans and Dominicans, who were almost the only religious orders that distinguished themselves by an application to study†,) which has not happened except for about forty years. And yet there was never so much ignorance, so much errour." And in the part of his work which refers to Mathematics, he says of that study‡, that it is the door and the key of the sciences; and that the neglect of it for thirty or forty years has entirely ruined the studies of the Latins. According to these statements, some change, disastrous to the fortunes of science, must have taken place about 1230, soon after the foundation of the Dominican and Franciscan Orders§. Nor can we doubt that the adoption of the Aristotelian philosophy by these two Orders, in the form in which the Angelical Doctor had systematized it, was one of the events which most tended to defer, for three centuries, the reform which Roger Bacon urged as a matter of crying necessity in his own time.

* Quoted by Jebb, *Pref.* to *Op. Maj.*

† Mosheim, *Hist.* III. 161.

‡ *Op. Maj.*, p. 57.

§ Mosheim, III. 161.

Chapter VIII.

THE REVIVAL OF PLATONISM.

1. *Causes of Delay in the Advance of Knowledge.*—In the insight possessed by learned men into the method by which truth was to be discovered, the fourteenth and fifteenth centuries went backwards, rather than forwards, from the point which had been reached in the thirteenth. Roger Bacon had urged them to have recourse to experiment; but they returned with additional and exclusive zeal to the more favourite employment of reasoning upon their own conceptions. He had called upon them to look at the world without; but their eyes forthwith turned back upon the world within. In the constant oscillation of the human mind between Ideas and Facts, after having for a moment touched the latter, it seemed to swing back more impetuously to the former. Not only was the philosophy of Aristotle firmly established for a considerable period, but when men began to question its authority, they attempted to set up in its place a philosophy still more purely ideal, that of Plato. It was not till the actual progress of experimental knowledge for some centuries had given it a vast accumulation of force, that it was able to break its way fully into the circle of speculative science. The new Platonist schoolmen had to run their course, the practical discoverers had to prove their merit by their works, the Italian innovators had to utter their aspirations for a change, before the second Bacon could truly declare that the time for a fundamental reform was at length arrived.

It cannot but seem strange, to any one who attempts to trace the general outline of the intellectual progress of man, and who considers him as under the guidance of a Providential sway, that he should thus be permitted to wander so long in a wilderness of intellectual darkness; and even to turn back, by a perverse caprice as it might seem, when on the very border of the brighter and better land which was his destined inheritance. We do not attempt to solve this difficulty: but such a course of things naturally suggests the thought, that a progress in physical science is not the main object of man's career, in the eyes of the Power who directs the fortunes of our race. We can easily conceive that it may have been necessary to man's general welfare that he should continue to turn his eyes inwards upon his own heart and faculties, till Law and Duty, Religion and Government, Faith and Hope, had been fully incorporated with all the past acquisitions of human intellect; rather than that he should have rushed on into a train of discoveries tending to chain him to the objects and operations of the material world. The systematic Law* and philosophical Theology which acquired their ascendancy in men's minds at the time of which we speak, kept them engaged in a region of speculations which perhaps prepared the way for a profounder and wider civilization, for a more elevated and spiritual character, than might have been possible without such a preparation. The great Italian poet of the fourteenth century speaks with strong admiration of the founders of the system which prevailed in his time. Thomas, Albert, Gratian, Peter Lombard, occupy distinguished places in the Paradise. The first, who is the poet's instructor, says,—

Io fui degli agni della santa greggia
Che Domenico mena per cammino
U' ben s'impingua se non si vaneggia.
  Questo che m'è a destra piu vicino

* Gratian published the *Decretals* in the twelfth century; and the Canon and Civil Law became a regular study in the universities soon afterwards.

Frate e maestro fummi; ed esso Alberto
E di Cologna, ed io Tomas d'Aquino. . . . .
Quell' altro fiammeggiar esce del riso
De Grazian, che l'uno et l'altro foro
Ajutò si che piace in Paradiso.

I, then, was of the lambs that Dominic
Leads, for his saintly flock, along the way
Where well they thrive not swoln with vanity.
He nearest on my right-hand brother was
And master to me; Albert of Cologne
Is this; and of Aquinum Thomas, I. . . . .
That next resplendence issues from the smile
Of Gratian who to either forum lent
Such help as favour wins in Paradise.

It appears probable that neither poetry, nor painting, nor the other arts which require for their perfection a lofty and spiritualized imagination, would have appeared in the noble and beautiful forms which they assumed in the fourteenth and fifteenth century, if men of genius had, at the beginning of that period, made it their main business to discover the laws of nature, and to reduce them to a rigorous scientific form. Yet who can doubt that the absence of these touching and impressive works would have left one of the best and purest parts of man's nature without its due nutriment and developement? It may perhaps be a necessary condition in the progress of man, that the Arts which aim at beauty should reach their excellence before the Sciences which seek speculative truth; and if this be so, we inherit, from the middle ages, treasures which may well reconcile us to the delay which took place in their cultivation of experimental science.

However this may be, it is our business at present to trace the circumstances of this very lingering advance. We have already noticed the contest of the Nominalists and Realists, which was one form, though, with regard to scientific methods, an unprofitable one, of the antithesis of Ideas and Things. Though, therefore, this struggle continued, we need not dwell upon it. The Nominalists denied the real existence of Ideas, which doctrine was to a great extent implied in the prevailing systems; but the controversy in which they thus engaged, did not lead them to seek for knowledge in a new field and by new methods. The arguments which Occam the Nominalist opposes to those of Duns Scotus the Realist, are marked with the stamp of the same system, and consist only in permutations and combinations of the same elementary conceptions. It was not till the impulse of external circumstances was added to the discontent, which the more stirring intellects felt towards the barren dogmatism of their age, that the activity of the human mind was again called into full play, and a new career of progression entered upon, till then undreamt of, except by a few prophetic spirits.

2. *Causes of Progress.*—These circumstances were principally the revival of Greek and Roman literature, the invention of Printing, the Protestant Reformation, and a great number of curious discoveries and inventions in the arts, which were soon succeeded by important steps in speculative physical science. Connected with the first of these events, was the rise of a party of learned men who expressed their dissatisfaction with the Aristotelian philosophy, as it was then taught, and manifested a strong preference for the views of Plato. It is by no means suitable to our plan to give a detailed account of this new Platonic school; but we may notice a few of the writers who belong to it, so far at least as to indicate its influence upon the Methods of pursuing science.

In the fourteenth century *, the frequent intercourse of the most cultivated persons of the Eastern and West-

* Tenneman, IX. 14.

ern Empire, the increased study of the Greek language in Italy, the intellectual activity of the Italian States, the discovery of manuscripts of the classical authors, were circumstances which excited or nourished a new and zealous study of the works of Greek and Roman genius. The genuine writings of the ancients, when presented in their native life and beauty, instead of being seen only in those lifeless fragments and dull transformations which the scholastic system had exhibited, excited an intense enthusiasm. Europe, at that period, might be represented by Plato's beautiful allegory, of a man who, after being long kept in a dark cavern, in which his knowledge of the external world is gathered from the images which stream through the chinks of his prison, is at last led forth into the full blaze of day. It was inevitable that such a change should animate men's efforts and enlarge their faculties. Greek literature became more and more known, especially by the influence of learned men who came from Constantinople into Italy: these teachers, though they honoured Aristotle, reverenced Plato no less, and had never been accustomed to follow with servile submission of thought either these or any other leaders. The effect of such influences soon reveals itself in the works of that period. Dante has woven into his *Divina Comedia* some of the ideas of Platonism. Petrarch, who had formed his mind by the study of Cicero, and had thus been inspired with a profound admiration for the literature of Greece, learnt Greek from Barlaam, a monk who came as ambassador from the Emperor of the East to the Pope, in 1339. With this instructor, the poet read the works of Plato; struck by their beauty, he contributed, by his writings and his conversation, to awake in others an admiration and love for that philosopher, which soon became strongly and extensively prevalent among the learned in Italy.

3. *Hermolaus Barbarus, &c.*—Along with this feeling there prevailed also, among those who had learnt to relish the genuine beauties of the Greek and Latin writers, a strong disgust for the barbarisms in which the scholastic philosophy was clothed. Hermolaus Barbarus*, who was born in 1454, at Venice, and had formed his taste by the study of classical literature, translated, among other learned works, Themistius's paraphrastic exposition of the Physics of Aristotle; with the view of trying whether the Aristotelian Natural Philosophy could not be presented in good Latin, which the scholastic teachers denied. In his Preface he expresses great indignation against those philosophers who have written and disputed on philosophical subjects in barbarous Latin, and in an uncultured style, so that all refined minds are repelled from these studies by weariness and disgust. They have, he says, by this barbarism, endeavoured to secure to themselves, in their own province, a supremacy without rivals or opponents. Hence they maintain that mathematics, philosophy, jurisprudence, cannot be expounded in correct Latin;—that between these sciences and the genuine Latin language there is a great gulf, as between things that cannot be brought together: and on this ground they blame those who combine the study of philology and eloquence with that of science. This opinion, adds Hermolaus, perverts and ruins our studies; and is highly prejudicial and unworthy in respect to the state. Hermolaus awoke in others, as for instance, in John Picus of Mirandula, the same dislike to the reigning school philosophy. As an opponent of the same kind, we may add Marius Nizolius of Bersallo, a scholar who carried his admiration of Cicero to an exaggerated extent, and who was led, by a controversy with the defenders of the scholastic philosophy, to pub-

* Tenneman, IX. 25.

lish (1553) a work *On the True Principles and True Method of Philosophizing.* In the title of this work, he professes to give "the true principles of almost all arts and sciences, refuting and rejecting almost all the false principles of the Logicians and Metaphysicians." But although, in the work, he attacks the scholastic philosophy, he does little or nothing to justify the large pretensions of his title; and he excited, it is said, little notice. It is therefore curious that Leibnitz should have thought it worth his while to re-edit this work, which he did in 1670, adding remarks of his own.

4. *Nicolaus Cusanus.*—Without dwelling upon this opposition to the scholastic system on the ground of taste, I shall notice somewhat further those writers who put forwards Platonic views, as fitted to complete or to replace the doctrines of Aristotle. Among these, I may place Nicolaus Cusanus, so called from Cus, a village on the Moselle, where he was born in 1401; who was afterwards raised to the dignity of cardinal. We might, indeed, at first be tempted to include Cusanus among those persons who were led to reject the old philosophy by being themselves agents in the progressive movement of physical science. For he published, before Copernicus, and independently of him, the doctrine that the earth is in motion*. But it should be recollected that in order to see the possibility of this doctrine, and its claims to acceptance, no new reference to observation was requisite. The Heliocentric System was merely a new mode of representing to the mind facts with which all astronomers had long been familiar. The system might very easily have been embraced and inculcated by Plato himself; as indeed it is said to have been actually taught by Pythagoras. The mere adoption of the Heliocentric view,

* "Jam nobis manifestum est terram istam in veritate moveri," &c. —*De Doctâ Ignorantiâ*, Lib. II. cap. 12.

therefore, without attempting to realize the system in detail, as Copernicus did, cannot entitle a writer of the fifteenth century to be looked upon as one of the authors of the discoveries of that period; and we must consider Cusanus as a speculative anti-Aristotelian, rather than as a practical reformer.

The title of Cusanus's book, *De Doctâ Ignorantiâ*, shows how far he was from agreeing with those who conceived that, in the works of Aristotle, they had a full and complete system of all human knowledge. At the outset of this book*, he says, after pointing out some difficulties in the received philosophy, "If, therefore, the case be so, (as even the most profound Aristotle, in his *First Philosophy*, affirms,) that in things most manifest by nature, there is a difficulty, no less than for an owl to look at the sun; since the appetite of knowledge is not implanted in us in vain, we ought to desire to know that we are ignorant. If we can fully attain to this, we shall arrive at *Instructed Ignorance*." How far he was from placing the source of knowledge in experience, as opposed to ideas, we may see in the following passage† from another work of his, *On Conjectures*. "Conjectures must proceed from our mind, as the real world proceeds from the infinite Divine Reason. For since the human mind, the lofty likeness of God, participates, as it may, in the fruitfulness of the creative nature, it doth from itself, as the image of the Omnipotent Form, bring forth reasonable thoughts which have a similitude to real existences. Thus the Human Mind exists as a conjectural form of the world, as the Divine Mind is its real form." We have here the Platonic or ideal side of knowledge put prominently and exclusively forwards.

5. *Marsilius Ficinus, &c.*—A person who had much more influence on the diffusion of Platonism was Marsi-

* *De Doct. Ignor.*, Lib. I. c. 1. † *De Conjecturis*, Lib. I. c. 3, 4.

lius Ficinus, a physician of Florence. In that city there prevailed, at the time of which we speak, the greatest enthusiasm for Plato. George Gemistius Pletho, when in attendance upon the Council of Florence, had imparted to many persons the doctrines of the Greek philosopher; and, among others, had infused a lively interest on this subject into the elder Cosmo, the head of the family of the Medici. Cosmo formed the plan of founding a Platonic academy. Ficinus*, well instructed in the works of Plato, Plotinus, Proclus, and other Platonists, was selected to further this object, and was employed in translating the works of these authors into Latin. It is not to our present purpose to consider the doctrines of this school, except so far as they bear upon the nature and methods of knowledge; and therefore I must pass by, as I have in other instances done, the greater part of their speculations, which related to the nature of God, the immortality of the soul, the principles of Goodness and Beauty, and other points of the same order. The object of these and other Platonists of this school, however, was not to expel the authority of Aristotle by that of Plato. Many of them had come to the conviction that the highest ends of philosophy were to be reached only by bringing into accordance the doctrines of Plato and of Aristotle. Of this opinion was John Picus, Count of Mirandula and Concordia; and under this persuasion he employed the whole of his life in labouring upon a work, *De Concordiâ Platonis et Aristotelis*, which was not completed at the time of his death, in 1494; and has never been published. But about a century later, another writer of the same school, Francis Patricius†, pointing out the discrepancies between the two Greek teachers, urged the propriety of deposing Aristotle from the supremacy he had so long enjoyed. "Now all these

* Born in 1433. † Born 1529, died 1597.

doctrines, and others not a few," he says*, "since they are Platonic doctrines, philosophically most true, and consonant with the Catholic faith, whilst the Aristotelian tenets are contrary to the faith, and philosophically false, who will not, both as a Christian and a philosopher, prefer Plato to Aristotle? And why should not hereafter, in all the colleges and monasteries of Europe, the reading and study of Plato be introduced? Why should not the philosophy of Aristotle be forthwith exiled from such places? Why must men continue to drink the mortal poison of impiety from that source?" with much more in the same strain.

The Platonic school, of which we have spoken, had, however, reached its highest point of prosperity before this time, and was already declining. About 1500, the Platonists appeared to triumph over the Peripatetics†; but the death of their great patron, Cardinal Bessarion, about this time, and we may add, the hollowness of their system in many points, and its want of fitness for the wants and expectations of the age, turned men's thoughts partly back to the established Aristotelian doctrines, and partly forwards to schemes of bolder and fresher promise.

6. *Francis Patricius.*—Patricius, of whom we have just spoken, was one of those who had arrived at the conviction that the formation of a new philosophy, and not merely the restoration of an old one, was needed. In 1593, appeared his *Nova de Universis Philosophia*; and the mode in which it begins‡ can hardly fail to remind us of the expressions which Francis Bacon soon afterwards used in the opening of a work of the same nature. "Francis Patricius, being about to found anew the true

* *Aristoteles Exotericus*, p. 50.

† Tiraboschi, t. VII. part ii. p. 411.

‡ "Franciscus Patricius, novam veram integram de universis conditurus philosophiam, sequentia uti verissima prænuntiare est ausus. Præ-

philosophy of the universe, dared to begin by announcing the following indisputable principles." Here, however, the resemblance between Patricius and true inductive philosophers ends. His principles are barren *à priori* axioms; and his system has one main element, *Light*, (*Lux*, or *Lumen*,) to which all operations of nature are referred. In general cultivation, and practical knowledge of nature, he was distinguished among his contemporaries. In various passages of his works he relates* observations which he had made in the course of his travels, in Cyprus, Corfu, Spain, the mountains of the Modenese, and Dalmatia, which was his own country; his observations relate to light, the saltness of the sea, its flux and reflux, and other points of astronomy, meteorology, and natural history. He speaks of the sex of plants†; rejects judicial astrology; and notices the astronomical systems of Copernicus, Tycho, Fracastoro, and Torre. But the mode in which he speaks of experiments proves, what indeed is evident from the general scheme of his system, that he had no due appreciation of the place which observation must hold in real and natural philosophy.

7. *Picus, Agrippa, &c.*—It had been seen in the later philosophical history of Greece, how readily the ideas of the Platonic school lead on to a system of unfathomable and unbounded mysticism. John Picus, of Mirandula‡, added to the study of Plato and the Neoplatonists, a mass of allegorical interpretations of the Scriptures, and the

Prænunciata ordine persecutus, divinis oraculis, geometricis rationibus, clarissimisque experimentis comprobavit.

Ante primum nihil,
Post primum omnia,
A principio omnia," &c.

His other works are *Panaugia*, *Pancosmia*, *Dissertationes Peripateticæ*.

* Tiraboschi, t. VII. part ii. p. 411.

† *Dissert. Peripatet.*, t. II. lib. v. sub fin.

‡ Tenneman, IX. 148.

dreams of the Cabbala, a Jewish system *, which pretends to explain how all things are an emanation of the Deity. To this his nephew, Francis Picus, added a reference to inward illumination †, by which knowledge is obtained, independently of the progress of reasoning. John Reuchlin, or Capnio, born 1455; John Baptist Helmont, born 1577; Francis Mercurius Helmont, born 1618, and others, succeeded John Picus in his admiration of the Cabbala: while others, as Jacob Bœhmen, rested upon internal revelations like Francis Picus. And thus we have a series of mystical writers, continued into modern times, who may be considered as the successors of the Platonic school; and who all exhibit views altogether erroneous with regard to the nature and origin of knowledge. Among the various dreams of this school are certain wide and loose analogies of terrestrial and spiritual things. Thus in the writings of Cornelius Agrippa (who was born 1487, at Cologne) we have such systems as the following ‡: —"Since there is a threefold world, elemental, celestial, and intellectual, and each lower one is governed by that above it, and receives the influence of its powers: so that the very Archetype and Supreme Author transfuses the virtues of his omnipotence into us through angels, heavens, stars, elements, animals, plants, stones,—into us, I say, for whose service he has framed and created all these things;—the Magi do not think it irrational that we should be able to ascend by the same degrees, the same worlds, to this Archetype of the world, the Author and First Cause of all, of whom all things are, and from whom they proceed; and should not only avail ourselves of those powers which exist in the nobler works of creation, but also should be able to attract other powers, and add them to these."

* Tenneman, IX. 167. † *Ib.*, 158.

‡ Agrippa, *De Occult. Phil.*, Lib. I. c. I.

Agrippa's work, *De Vanitate Scientiarum*, may be said rather to have a skeptical and cynical, than a Platonic, character. It is a declamation*, in a melancholy mood, against the condition of the sciences in his time. His indignation at the worldly success of men whom he considered inferior to himself, had, he says, metamorphosed him into a dog, as the poets relate of Hecuba of Troy, so that his impulse was to snarl and bark. His professed purpose, however, was to expose the dogmatism, the servility, the self-conceit, and the neglect of religious truth which prevailed in the reigning Schools of philosophy. His views of the nature of science, and the modes of improving its cultivation, are too imperfect and vague to allow us to rank him among the reformers of science.

8. *Paracelsus, Fludd, &c.*—The celebrated Paracelsus† put himself forwards as a reformer in philosophy, and obtained no small number of adherents. He was, in most respects, a shallow and impudent pretender; and had small knowledge of the literature or science of his time: but by the tone of his speaking and writing he manifestly belongs to the mystical school of which we are now speaking. Perhaps by the boldness with which he proposed new systems, and by connecting these with the practical doctrines of medicine, he contributed something to the introduction of a new philosophy. We have seen in the History of Chemistry that he was the author of the system of Three Principles, (salt, sulphur, and mercury,) which replaced the ancient doctrine of Four Elements, and prepared the way for a true science of chemistry. But the salt, sulphur, and mercury of Paracelsus were not, he tells his disciples, the visible bodies which

* Written in 1526.

† Philip Aurelius Theophrastus Bombastus von Hohenheim, also called Paracelsus Eremita, born at Einsiedlen in Switzerland, in 1493.

we call by those names, but certain invisible, astral, or sidereal elements. The astral salt is the basis of the solidity and incombustible parts in bodies; the astral sulphur is the source of combustion and vegetation; the astral mercury is the origin of fluidity and volatility. And again, these three elements are analogous to the three elements of man,—Body, Spirit, and Soul.

A writer of our own country, belonging to this mystical school, is Robert Fludd, or De Fluctibus, who was born in 1571, in Kent, and after pursuing his studies at Oxford, travelled for several years. Of all the Theosophists and Mystics, he is by much the most learned; and was engaged in various controversies with Mersenne, Gassendi, Kepler, and others. He thus brings us in contact with the next class of philosophers whom we have to consider, the practical reformers of philosophy;—those who furthered the cause of science by making, promulgating, or defending the great discoveries which now began to occupy men. He adopted the principle, which we have noticed elsewhere*, of the analogy of the Macrocosm and Microcosm, the world of nature and the world of man. His system contains such a mixture and confusion of physical and metaphysical doctrines as might be expected from his ground-plan, and from his school. Indeed his object, the general object of mystical speculators, is to identify physical with spiritual truths. Yet the influence of the practical experimental philosophy which was now gaining ground in the world may be traced in him. Thus he refers to experiments on distillation to prove the existence and relation of the regions of water, air, and fire, and of the spirits which correspond to them; and is conceived, by some persons †, to have anticipated Torricelli in the invention of the Barometer.

* B. IX. c. 2. s. 1. The Mystical School of Biology.

† Tenneman, IX. 221.

We need no further follow the speculations of this school. We see already abundant reason why the reform of the methods of pursuing science could not proceed from the Platonists. Instead of seeking knowledge by experiment, they immersed themselves deeper than even the Aristotelians had done in traditionary lore, or turned their eyes inwards in search of an internal illumination. Some attempts were made to remedy the defects of philosophy by a recourse to the doctrines of other sects of antiquity, when men began to feel more distinctly the need of a more connected and solid knowledge of nature than the established system gave them. Among these attempts were those of Berigard*, Magernus, and especially Gassendi, to bring into repute the philosophy of the Ionian school, of Democritus and of Epicurus. But these endeavours were posterior in time to the new impulse given to knowledge by Copernicus, Kepler, and Galileo, and were influenced by views arising out of the success of these discoveries, and they must, therefore, be considered hereafter. In the mean time, some independent efforts (arising from speculative rather than practical reformers) were made to cast off the yoke of the Aristotelian dogmatism, and to apprehend the true form of that new philosophy which the most active and hopeful minds saw to be needed; and we must give some account of these attempts, before we can commit ourselves to the full stream of progressive philosophy.

* Tenneman, 265.

CHAPTER IX.

THE THEORETICAL REFORMERS OF SCIENCE.

WE have already seen that Patricius, about the middle of the sixteenth century, announced his purpose of founding anew the whole fabric of philosophy; but that, in executing this plan, he ran into wide and baseless hypotheses, suggested by *à priori* conceptions rather than by external observation; and that he was further misled by fanciful analogies resembling those which the Platonic mystics loved to contemplate. The same time, and the period which followed it, produced several other essays which were of the same nature, with the exception of their being free from the peculiar tendencies of the Platonic school: and these insurrections against the authority of the established dogmas, although they did not directly substitute a better positive system in the place of that which they assailed, shook the authority of the Aristotelian system, and led to its overthrow; which took place as soon as these theoretical were aided by other practical reformers.

Bernardinus Telesius.—Italy, always, in modern times, fertile in the beginnings of new systems, was the soil on which these innovators arose. These earliest and most conspicuous of them is Bernardinus Telesius, who was born in 1508, at Cosenza, in the kingdom of Naples. His studies, carried on with great zeal and ability, first at Milan and then at Rome, made him well acquainted with the knowledge of his times; but his own reflections convinced him that the basis of science, as then received, was altogether erroneous; and led him to attempt a reform, with which view, in 1565, he published, at Rome, his work*, "*Bernardinus Telesius, of Cosenza, on the*

* Bernardini Telesii Consentini *De Rerum Natura juxta propria Principia.*

Nature of Things, according to principles of his own." In the preface of this work he gives a short account* of the train of reflection by which he was led to put himself in opposition to the Aristotelian philosophy. This kind of autobiography occurs not unfrequently in the writings of theoretical reformers; and shows how livelily they felt the novelty of their undertaking. After the storm and sack of Rome in 1527, Telesius retired to Padua, as a peaceful seat of the muses; and there studied philosophy and mathematics, with great zeal, under the direction of Jerom Amalthæus and Frederic Delphinus. In these studies he made great progress; and the knowledge which he thus acquired threw a new light upon his view of the Aristotelian philosophy. He undertook a closer examination of the Physical Doctrines of Aristotle; and as the result of this, he was astonished how it could have been possible that so many excellent men, so many nations, and even almost the whole human race, should, for so long a time, have allowed themselves to be carried away by a blind reverence for a teacher, who had committed errours so numerous and grave as he perceived to exist in "the philosopher." Along with this view of the insufficiency of the Aristotelian philosophy, arose, at an early period, the thought of erecting a better system in its place. With this purpose he left Padua, when he had received the degree of Doctor, and went to Rome, where he was encouraged in his design by the approval and friendly exhortations of distinguished men of letters, amongst whom were Ubaldino Bandinelli and Giovanni della Casa. From Rome he went to his native place, when the incidents and occupations of a married life for a while interrupted his phi-

* I take this account from Tenneman: this Proem was omitted in subsequent editions of Telesius, and is not in the one which I have consulted. Tenneman, *Gesch. d. Phil.*, IX. 280.

losophical project. But after his wife was dead, and his eldest son grown to manhood, he resumed with ardour the scheme of his youth; again studied the works of Aristotle and other philosophers, and composed and published the first two books of his treatise. The opening to this work sufficiently exhibits the spirit in which it was conceived. Its object is stated in the title to be to show, that "the construction of the world, the magnitude and nature of the bodies contained in it, are not to be investigated by reasoning, which was done by the ancients, but are to be apprehended by the senses, and collected from the things themselves." And the Proem is in the same strain. "They who before us have inquired concerning the construction of this world and of the things which it contains, seem indeed to have prosecuted their examination with protracted vigils and great labour, but *never to have looked at it.*" And thus, he observes, they found nothing but errour. This he ascribes to their presumption. "For, as it were, attempting to rival God in wisdom, and venturing to seek for the principles and causes of the world by the light of their own reason, and thinking they had found what they had only invented, they made an arbitrary world of their own." "*We* then," he adds, "not relying on ourselves, and of a duller intellect than they, propose to ourselves to turn our regards to the world itself and its parts."

The execution of the work, however, by no means corresponds to the announcement. The doctrines of Aristotle are indeed attacked; and the objections to these, and to other received opinions, form a large part of the work. But these objections are supported by *à priori* reasoning, and not by experiments. And thus, rejecting the Aristotelian physics, he proposes a system at least equally baseless; although, no doubt, grateful to the author from its sweeping and apparently simple character. He assumes three principles, Heat, Cold, and Matter: Heat is the principle of motion, Cold of immobility, and Matter is the corporeal substratum, in which these incorporeal and active principles produce their effects. It is easy to imagine that, by combining and separating these abstractions in various ways, a sort of account of many natural phenomena may be given; but it is impossible to ascribe any real value to such a system. The merit of Telesius must be considered to consist in his rejection of the Aristotelian errours, in his perception of the necessity of a reform in the method of philosophizing, and in his persuasion that this reform must be founded on experiments rather than on reasoning. When he said*, "We propose to ourselves to turn our eyes to the world itself, and its parts, their passions, actions, operations and species," his view of the course to be followed was right; but his purpose remained but ill fulfilled, by the arbitrary edifice of abstract conceptions which his system exhibits.

Francis Bacon, who, about half a century later, treated the subject of a reform of philosophy in a far more penetrating and masterly manner, has given us his judgment of Telesius. In his view, he considers Telesius as the restorer of the Atomic philosophy, which Democritus and Parmenides taught among the ancients; and according to his custom, he presents an image of this philosophy in an adaptation of a portion of ancient mythology†. The Celestial Cupid, who, with Cœlus, was the parent of the Gods and of the Universe, is exhibited as a representation of matter and its properties, according to the

* Proem.

† "De Principiis atque Originibus secundum fabulas Cupidinis et Cœli: sive Parmenidis et Telesii et præcipuè Democriti Philosophia tractata in Fabula de Cupidine."

Democritean philosophy. "Concerning Telesius," says Bacon, "we think well, and acknowledge him as a lover of truth, a useful contributor to science, an amender of some tenets, the first of recent men. But we have to do with him as the restorer of the philosophy of Parmenides, to whom much reverence is due." With regard to this philosophy, he pronounces a judgment which very truly expresses the cause of its rashness and emptiness. "It is," he says, "such a system* as naturally proceeds from the intellect, abandoned to its own impulse, and not rising from experience to theory continuously and successively." Accordingly, he says that, "Telesius, although learned in the Peripatetic philosophy (if that were anything), which indeed, he has turned against the teachers of it, is hindered by his affirmations, and is more successful in destroying than in building."

The work of Telesius excited no small notice, and was placed in the *Index Expurgatorius.* It made many disciples, a consequence probably due to its spirit of system-making, no less than to its promise of reform, or its acuteness of argument; for till trial and reflection have taught man modesty and moderation, he can never be content to receive knowledge in the small successive instalments in which nature gives it forth to him. It is the makers of large systems, arranged with an appearance of completeness and symmetry, who, principally, give rise to Schools of philosophy.

(*Thomas Campanella*).—Accordingly, Telesius may be looked upon as the founder of a School. His most distinguished successor was Thomas Campanella, who was born in 1568, at Stilo, in Calabria. He showed great talents at an early age, prosecuting his studies at Cosenza,

* "Talia sunt qualia possunt esse ea quæ ab intellectu sibi permisso, nec ab experimentis continenter et gradatim sublevato, profecta videntur."

the birth-place of the great opponent of Aristotle and reformer of philosophy. He, too, has given us an account* of the course of thought by which he was led to become an innovator. "Being afraid that not genuine truth, but falsehood in the place of truth, was the tenant of the Peripatetic School, I examined all the Greek, Latin, and Arabic commentators of Aristotle, and hesitated more and more, as I sought to learn whether what they have said were also to be read in the world itself, which I had been taught by learned men was the living book of God. And as my doctors could not satisfy my scruples, I resolved to read all the books of Plato, Pliny, Galen, the Stoics, and the Democriteans, and especially those of Telesius; and to compare them with that *first and original writing, the world;* that thus from the primary autograph, I might learn if the copies contained anything false." Campanella probably refers here to an expression of Plato, who says, "the world is God's epistle to mankind." And this image, of the natural world as an original manuscript, while human systems of philosophy are but copies, and may be false ones, became a favourite thought of the reformers, and appears repeatedly in their writings from this time. "When I held my public disputation at Cosenza," Campanella proceeds, "and still more, when I conversed privately with the brethren of the monastery, I found little satisfaction in their answers; but Telesius delighted me, on account of his freedom in philosophizing, and because he rested upon the nature of things, and not upon the assertions of men."

With these views and feelings, it is not wonderful that Campanella, at the early age of twenty-two (1590,) published a work remarkable for the bold promise of its

* Thom. Campanella *de Libris propriis*, as quoted in Tenneman, IX. 291.

title: "*Thomas Campanella's Philosophy demonstrated to the senses, against those who have philosophized in a arbitrary and dogmatical manner, not taking nature for their guide; in which the errours of Aristotle and his followers are refuted from their own assertions and the laws of nature; and all the imaginations feigned in the place of nature by the Peripatetics are altogether rejected; with a true defence of Bernardin Telesius of Cosenza, the greatest of philosophers; confirmed by the opinions of the ancients, here elucidated and defended, especially those of the Platonists.*"

This work was written in answer to a book published against Telesius by a Neapolitan professor named Marta; and it was the boast of the young author that he had only employed eleven months in the composition of his defence, while his adversary had been engaged eleven years in preparing his attack. Campanella found a favourable reception in the house of the Marchese Lavelli, and there employed himself in the composition of an additional work, entitled *On the Sense of Things and Magic*, and in other literary labours. These, however, are full of the indications of an enthusiastic temper, inclined to mystical devotion, and of opinions bearing the cast of pantheism. For instance, the title of the book last quoted sets forth as demonstrated in the course of the work, that "the world is the living and intelligent statue of God; and that all its parts, and particles of parts, are endowed some with a clearer, some with a more obscure sense, such as suffices for the preservation of each and of the whole." Besides these opinions, which could not fail to make him obnoxious to the religious authorities, Campanella* engaged in schemes of political revolution, which involved him in danger and calamity. He took part in a conspiracy, of which the

* Economisti *Italiani*, Tom. I. p. xxxiii.

object was to cast off the tyranny of Spain, and to make Calabria a republic. This design was discovered; and Campanella, along with others, was thrown into prison and subjected to torture. He was kept in confinement twenty-seven years; and at last obtained his liberation by the interposition of Pope Urban VIII. He was, however, still in danger from the Neapolitan Inquisition; and escaped in disguise to Paris, where he received a pension from the king, and lived in intercourse with the most eminent men of letters. He died there in 1639.

Campanella was a contemporary of Francis Bacon, whom we must consider as belonging to an epoch to which the Calabrian school of innovators was only a prelude. I shall not therefore further follow the connexion of writers of this order. Tobias Adami, a Saxon writer, an admirer of Campanella's works, employed himself, about 1620, in adapting them to the German public, and in recommending them strongly to German philosophers. Descartes, and even Bacon, may be considered as successors of Campanella; for they too were theoretical reformers; but they enjoyed the advantage of the light which had, in the mean time, been thrown upon the philosophy of science, by the great practical advances of Kepler, Galileo, and others. To these practical reformers we must soon turn our attention; but we may first notice one or two additional circumstances belonging to our present subject.

Campanella remarks that both the Peripatetics and the Platonists conducted the learner to knowledge by a long and circuitous path, which he wished to shorten by setting out from the sense. Without speaking of the methods which he proposed, we may notice one maxim* of considerable value which he propounds, and to which we have already been led. "We begin to reason from

* Tenneman, IX. 305.

sensible objects, and definition is the end and epilogue of science. It is not the beginning of our knowing, but only of our teaching."

(*Andrew Cæsalpinus.*)—The same maxim had already been announced by Cæsalpinus, a contemporary of Telesius; (he was born at Arezzo in 1520, and died at Rome in 1603.) Cæsalpinus is a great name in science, though professedly an Aristotelian. It has been seen in the *History of Science**, that he formed the first great epoch of the science of botany by his systematic arrangement of plants, and that in this task he had no successor for nearly a century. He also approached near to the great discovery of the circulation of the blood†. He takes a view of science which includes the remark that we have just quoted from Campanella: "We reach perfect knowledge by three steps: Induction, Division, Definition. By Induction, we collect likeness and agreement from observation; by Division, we collect unlikeness and disagreement; by Definition, we learn the proper substance of each object. Induction makes universals from particulars, and offers to the mind all intelligible matter; Division discovers the difference of universals, and leads to species; Definition resolves species into their principles and elements‡." Without asserting this to be rigorously correct, it is incomparably more true and philosophical than the opposite view, which represents definition as the beginning of our knowledge; and the establishment of such a doctrine is a material step in inductive philosophy§.

(*Giordano Bruno.*)—Among the Italian innovators of this time we must notice the unfortunate Giordano Bruno, who was born at Nola about 1550, and burnt at Rome in 1600. He is, however, a reformer of a different

* *Hist. Ind. Sci.*, B. XVI. c. iii. sect. 2. † *Ib.*, B. XVII. ch. ii. sect. 1.
‡ *Quæst. Peripateticæ*, I. 1. § Tenneman, IX. 108.

school from Campanella; for he derives his philosophy from Ideas and not from Observation. He represents himself as the author of a new doctrine, which he terms the *Nolan Philosophy.* He was a zealous promulgator and defender of the Copernican system of the universe, as we have noticed in the *History of Science**. Campanella also wrote in defence of that system.

It is worthy of remark that a thought which is often quoted from Francis Bacon, occurs in Bruno's *Cena di Cenere*, published in 1584; I mean, the notion that the later times are more aged than the earlier. In the course of the dialogue, the Pedant, who is one of the interlocutors, says, "In antiquity is wisdom;" to which the Philosophical Character replies, "If you knew what you were talking about, you would see that your principle leads to the opposite result of that which you wish to infer;—I mean, that *we* are older, and have lived longer, than our predecessors." He then proceeds to apply this, by tracing the course of astronomy through the earlier astronomers up to Copernicus.

(*Peter Ramus.*)—I will notice one other reformer of this period, who attacked the Aristotelian system on another side, on which it was considered to be most impregnable. This was Peter Ramus, (born in Picardy in 1515,) who ventured to denounce the *Logic* of Aristotle as unphilosophical and useless. After showing an extraordinary aptitude for the acquirement of knowledge in his youth, when he proceeded to the degree of Master of Arts, he astonished his examiners by choosing for the subject of the requisite disputation the thesis†, "that all which Aristotle has said is not true." This position, so startling in 1535, he defended for the whole day, without being defeated. This was, however, only a formal academical exercise, which did not necessarily imply any per-

* *Hist. Ind. Sci.*, B. v. c. iii. sect 2. † Tenneman, IX. 420.

manent conviction of the opinion thus expressed. But his mind was really labouring to detect and remedy the errours which he thus proclaimed. From him, as from the other reformers of this time, we have an account of this mental struggle*. He says, in a work on this subject, "I will candidly and simply explain how I was delivered from the darkness of Aristotle. When, according to the laws of our university, I had spent three years and a half in the Aristotelian philosophy, and was now invested with the philosophical laurel as a Master of Arts, I took an account of the time which I had consumed in this study; and considered on what subjects I should employ this logical art of Aristotle, which I had learnt with so much labour and noise. I found it made me not more versed in history or antiquities, more eloquent in discourse, more ready in verse, more wise in any subject. Alas for me! how was I overpowered, how deeply did I groan, how did I deplore my lot and my nature, how did I deem myself to be by some unhappy and dismal fate and frame of mind abhorrent from the Muses, when I found that I was one who, after all my pains, could reap no benefit from that wisdom of which I heard so much, as being contained in the Logic of Aristotle." He then relates, that he was led to the study of the Dialogues of Plato, and was delighted with the kind of analysis of the subjects discussed which Socrates is there represented as executing. "Well," he adds, "I began thus to reflect within myself—(I should have thought it impious to say it to another)—What, I pray you, prevents me from *socratizing*; and from asking, without regard to Aristotle's authority, whether Aristotle's Logic be true and correct? It may be that that philosopher leads us wrong; and if so, no wonder that I cannot find in his books the treasure which is not

* Rami, *Animadversiones Aristotelicæ*, I. IV.

there. What if his dogmas be mere figments? Do I not tease and torment myself in vain, trying to get a harvest from a barren soil?" He convinced himself that the Aristotelian logic was worthless: and constructed a new system of Logic, founded mainly on the Platonic process of exhausting a subject by analytical classification of its parts. Both works, his *Animadversions on Aristotle*, and his *Logic*, appeared in 1543. The learned world was startled and shocked to find a young man, on his first entrance into life, condemning as faulty, fallacious, and useless, that part of Aristotle's works which had always hitherto been held as a masterpiece of philosophical acuteness, and as the Organon of scientific reasoning. And in truth, it must be granted that Ramus does not appear to have understood the real nature and object of Aristotle's Logic; while his own system could not supply the place of the old one, and was not of much real value. This dissent from the established doctrines was, however, not only condemned but punished. The printing and selling of his books was forbidden through France; and Ramus was stigmatized by a sentence* which declared him rash, arrogant, impudent, and ignorant, and prohibited from teaching logic and philosophy. He was, however, afterwards restored to the office of professor: and though much attacked, persisted in his plan of reforming, not only Logic but Physics and Metaphysics. He made his position still more dangerous by adopting the reformed religion; and during the unhappy civil wars of France, he was deprived of his professorship, driven from Paris, and had his library plundered. He endeavoured, but in vain, to engage a German professor, Schegk, to undertake the reform of the Aristotelian Physics; a portion of knowledge in which he felt himself not to be strong. Unhappily for himself, he

* See *Hist. Ind. Sci.*, B. IV. c. iv. sect. 4.

afterwards returned to Paris, where he perished in the massacre of St. Bartholomew in 1572.

Ramus's main objection to the Aristotelian Logic is, that it is not the image of the natural process of thought; an objection which shows little philosophical insight; for the course by which we obtain knowledge may well differ from the order in which our knowledge, when obtained, is exhibited. We have already seen that Ramus's contemporaries, Cæsalpinus and Campanella, had a wiser view; placing definition as the last step in knowing, but the first in teaching. But the effect which Ramus produced was by no means slight. He aided powerfully in turning the minds of men to question the authority of Aristotle on all points; and had many followers, especially among the Protestants. Among the rest, Milton, our great poet, published "Artis Logicæ plenior Institutio *ad Petri Rami methodum concinnata;*" but this work, appearing in 1672, belongs to a succeeding period.

(*The Reformers in general.*)—It is impossible not to be struck with the series of misfortunes which assailed the reformers of philosophy of the period we have had to review. Roger Bacon was repeatedly condemned and imprisoned; and, not to speak of others who suffered under the imputation of magical arts, Telesius is said* to have been driven from Naples to his native city by calumny and envy; Cæsalpinus was accused of atheism†; Campanella was imprisoned for twenty-seven years and tortured; Giordano Bruno was burnt at Rome as a heretic; Ramus was persecuted during his life, and finally murdered by his personal enemy Jacques Charpentier, in a massacre of which the plea was religion. It is true, that for the most part these misfortunes were not principally due to the attempts at philosophical reform, but were connected rather with politics or religion. But we

* Tenneman, ix. 200. † *Ib.* ix. 108.

cannot doubt that the spirit which led men to assail the received philosophy, might readily incline them to reject some tenets of the established religion; since the boundary line of these subjects is difficult to draw. And as we have seen, there was in most of the persons of whom we have spoken, not only a well-founded persuasion of the defects of existing systems, but an eager spirit of change, and a sanguine anticipation of some wide and lofty philosophy, which was soon to elevate the minds and conditions of men. The most unfortunate were, for the most part, the least temperate and judicious reformers. Patricius, who, as we have seen, declared himself against the Aristotelian philosophy, lived and died at Rome in peace and honour*.

(*Melancthon.*)—It is not easy to point out with precision the connexion between the efforts at a Reform in Philosophy, and the great Reformation of Religion in the sixteenth century. The disposition to assert (practically at least) a freedom of thinking, and to reject the corruptions which tradition had introduced and authority maintained, naturally extended its influence from one subject to another; and especially in subjects so nearly connected as theology and philosophy. The Protestants, however, did not reject the Aristotelian system; they only reformed it, by going back to the original works of the author, and by reducing it to a conformity with Scripture. In this reform, Melancthon was the chief author, and wrote works on Logic, Physics, Morals, and Metaphysics, which were used among Protestants. On the subject of the origin of our knowledge, his views contained a very philosophical improvement of the Aristotelian doctrines. He recognized the importance of Ideas, as well as of Experience. "We could not," he says†,

* Tenneman, IX. 246.

† Melancthon, *De Anima*, p. 207, quoted in Tenneman, IX. 121.

"proceed to reason at all, except there were by nature innate in man certain fixed points, that is, principles of science;—as Number, the recognition of Order and Proportion, logical, geometrical, physical and moral Principles. Physical principles are such as these,—everything which exists proceeds from a cause,—a body cannot be in two places at once,—time is a continued series of things or of motions,—and the like." It is not difficult to see that such Principles partake of the nature of the Fundamental Ideas which we have attempted to arrange and enumerate in a previous part of this work.

Before we proceed to the next chapter, which treats of the Practical Reformers of Scientific Method, let us for an instant look at the strong persuasion that the time of a philosophical revolution was at hand, implied in the titles of the works of this period. Telesius published *De Rerum Natura juxta propria principia;* Francis Helmont, *Philosophia vulgaris refutata;* Patricius, *Nova de Universis Philosophia;* Campanella, *Philosophia sensibus demonstrata, adversus errores Aristotelis:* Bruno professed himself the author of a *Nolan Philosophy;* and Ramus of a *New Logic.* The age announced itself pregnant; and the eyes of all who took an interest in the intellectual fortunes of the race, were looking eagerly for the expected offspring.

CHAPTER X.

THE PRACTICAL REFORMERS OF SCIENCE.

Character of the Practical Reformers.—WE now come to a class of speculators who had perhaps a greater share in bringing about the change from stationary to progressive knowledge, than those writers who so loudly announced the revolution. The mode in which the philosophers of whom we now speak produced their impressions on men's minds, was very different from the procedure of the theoretical reformers. What these talked of, they did; what these promised, they performed. While the theorists concerning knowledge proclaimed that great advances were to be made, the practical discoverers went steadily forwards. While one class spoke of a complete Reform of scientific Methods, the other, boasting little, and often thinking little of Method, proved the novelty of their instrument by obtaining new results. While the metaphysicians were exhorting men to consult experience and the senses, the physicists were examining nature by such means with unparalleled success. And while the former, even when they did for a moment refer to facts, soon rushed back into their own region of ideas, and tried at once to seize the widest generalizations, the latter, fastening their attention upon the phenomena, and trying to reduce them to laws, were carried forwards by steps measured and gradual, such as no conjectural view of scientific method had suggested; but leading to truths as profound and comprehensive as any which conjecture had dared to anticipate. The theoretical reformers were bold, self-confident, hasty, contemptuous of antiquity, ambitious of ruling all future speculations, as they whom they sought to depose had ruled the past. The practical reformers were cautious, modest, slow, despising no knowledge, whether borrowed from tradition or observation, confident in the ultimate triumph of science, but impressed with the conviction that each single person could contribute a little only to its progress. Yet though thus working rather than speculating,—dealing with particulars more than with generals,—employed mainly in adding to knowledge, and not in defining what knowledge is, or how additions are to be made to it,—these men, thoughtful, curious, and of comprehensive minds, were constantly led to important views on the nature and methods of science. And these views, thus suggested by reflections on their own mental activity, were gradually incorporated with the more abstract doctrines of the metaphysicians, and had a most important influence in establishing an improved philosophy of science. The indications of such views we must now endeavour to collect from the writings of the discoverers of the times preceding the seventeenth century.

Some of the earliest of these indications are to be found in those who dealt with Art rather than with Science. I have already endeavoured to show that the advance of the arts which give us a command over the powers of nature, is generally prior to the formation of exact and speculative knowledge concerning those powers. But Art, which is thus the predecessor of Science, is, among nations of acute and active intellects, usually its parent. There operates, in such a case, a speculative spirit, leading men to seek for the reasons of that which they find themselves able to do. How slowly, and with what repeated deviations men follow this leading, when under the influence of a partial and dogmatical philosophy, the late birth and slow growth of sound physical theory shows. But at the period of which we now speak, we find men, at length, proceeding in obedience to the impulse which thus drives them from practice to theory;—from an acquaintance with phenomena to a free and intelligent inquiry concerning their causes.

Leonardo da Vinci.—I have already noted, in the History of Science, that the Indistinctness of Ideas, which was long one main impediment to the progress of science in the middle ages, was first remedied among architects and engineers. These men, so far at least as mechanical ideas were concerned, were compelled by their employments to judge rightly of the relations and properties of the materials with which they had to deal; and would have been chastised by the failure of their works, if they had violated the laws of mechanical truth. It was not wonderful, therefore, that these laws became known to them first. We have seen, in the *History*, that Leonardo da Vinci, the celebrated painter, who was also an engineer, is the first writer in whom we find the true view of the laws of equilibrium of the lever in the most general case. This artist, a man of a lively and discursive mind, is led to make some remarks* on the formation of our knowledge, which may show the opinions on that subject that already offered themselves at the beginning of the sixteenth century†. He expresses himself as follows:—"Theory is the general, Experiments are the soldiers. The interpreter of the artifices of nature is Experience: she is never deceived. Our judgment sometimes is deceived, because it expects effects which Experience refuses to allow." And again, "We must consult Experience, and vary the circumstances till we have drawn from them general rules; for it is she who furnishes true rules. But of what use, you ask, are these rules? I reply, that they direct us in the researches of nature and the operations of art. They prevent our imposing upon ourselves and others, by promising ourselves results which we cannot obtain."

"In the study of the sciences which depend on mathematics, those who do not consult nature but authors, are not the children of nature, they are only her grandchildren. She is the true teacher of men of genius.

* His works have never been published, and exist in manuscript in the library of the Institute at Paris. Some extracts were published by Venturi, *Essai sur les Ouvrages de Leonard da Vinci*. Paris, 1797.

† Leonardo died in 1520, at the age of 78.

But see the absurdity of men! They turn up their noses at a man who prefers to learn from nature herself rather than from authors who are only her clerks."

In another place, in reference to a particular case, he says, "Nature begins from the Reason and ends in Experience; but for all that, we must take the opposite course; begin from the Experiment and try to discover the Reason."

Leonardo was born forty-six years before Telesius; yet we have here an estimate of the value of experience far more just and substantial than the Calabrian school ever reached. The expressions contained in the above extracts, are well worthy our notice;—that experience is never deceived;—that we must vary our experiments, and draw from them general rules;—that nature is the original source of knowledge, and books only a derivative substitute;—with the lively image of the sons and grandsons of nature. Some of these assertions have been deemed, and not without reason, very similar to those made by Bacon a century later. Yet it is probable that the import of such expressions, in Leonardo's mind, was less clear and definite than that which they acquired by the progress of sound philosophy. When he says that theory is the general and experiments the soldiers, he probably meant that theory directs men what experiments to make; and had not in his mind the notion of a theoretical Idea ordering and brigading the Facts. When he says that Experience is the interpreter of Nature, we may recollect, that in a more correct use of this image, Experience and Nature are the writing, and the Intellect of man the interpreter. We may add, that the clear apprehension of the importance of Experience led, in this as in other cases, to an unjust depreciation of the value of what science owed to books. Leonardo would have made little progress, if he had attempted to master a complex science, astronomy for instance, by means of observation alone, without the aid of books.

But in spite of such criticism, Leonardo's maxims show extraordinary sagacity and insight; and they appear to us the more remarkable, when we see how rare such views are for a century after his time.

Copernicus.—For we by no means find, even in those practical discoverers to whom, in reality, the revolution in science, and consequently in the philosophy of science, was due, this prompt and vigorous recognition of the supreme authority of observation as a ground of belief; this bold estimate of the probable worthlessness of traditional knowledge; and this plain assertion of the reality of theory founded upon experience. Among such discoverers, Copernicus must ever hold a most distinguished place. The heliocentric theory of the universe, established by him with vast labour and deep knowledge, was, for the succeeding century, the field of discipline and exertion of all the most active speculative minds. Men, during that time, proved their freedom of thought, their hopeful spirit, and their comprehensive view, by adopting, inculcating, and following out the philosophy which this theory suggested. But in the first promulgation of the theory, in the works of Copernicus himself, we find a far more cautious and reserved temper. He does not, indeed, give up the reality of his theory, but he expresses himself so as to avoid shocking those who might (as some afterwards did) think it safe to speak of it as an *hypothesis* rather than a truth. In his preface addressed to the Pope*, after speaking of the difficulties in the old and received doctrines, by which he was led to his own theory, he says, "Hence I began to think of the mobility of the earth; and although the opinion seemed absurd, yet because I knew that to others before me this liberty had

* Paul III., in 1543.

been conceded, of imagining any kinds of circles in order to explain the phenomena of the stars, I thought it would also be readily granted me, that I might try whether, by supposing the earth to be in motion, I might not arrive at a better explanation than theirs, of the revolutions of the celestial orbs." Nor does he anywhere assert that the seeming absurdity had become a certain truth, or betray any feeling of triumph over the mistaken belief of his predecessors. And, as I have elsewhere shown, his disciples* indignantly and justly defended him from the charge of disrespect towards Ptolemy and other ancient astronomers. Yet Copernicus is far from compromising the value or evidence of the great truths which he introduced to general acceptance; and from sinking in his exposition of his discoveries below the temper which had led to them. His quotation from Ptolemy, that "He who is to follow philosophy must be a freeman in mind," is a grand and noble maxim, which it well became him to utter.

Fabricius.—In another of the great discoverers of this period, though employed on a very different subject, we discern much of the same temper. Fabricius of Acquapendente†, the tutor and forerunner of our Harvey, and one of that illustrious series of Paduan professors who were the fathers of anatomy‡, exhibits something of the same respect for antiquity, in the midst of his original speculations. Thus in a dissertation§ *On the Action of the Joints*, he quotes Aristotle's Mechanical Problems to prove that in all animal motion there must be some quiescent fulcrum; and finds merit even in Aristotle's ignorance. "Aristotle," he says‖, "did not know that motion was produced by the muscle; and after staggering

* *Hist. Ind. Sci.*, B. v. c. ii.

† Born 1537, died 1619.

‡ *Hist. Ind. Sci.*, B. xvii. c. ii. sect. 1.

§ Fabricius, *De Motu Locali*, p. 182.

‖ P. 199.

about from one supposition to another, at last is compelled by the facts themselves to recur to an innate spirit, which, he conceives, is contracted, and which pulls and pushes. And here we cannot help admiring the genius of Aristotle, who, though ignorant of the muscle, invents something which produces nearly the same effect as the muscle, namely, contraction and pulling." He then, with great acuteness, points out the distinction between Aristotle's opinions, thus favourably interpreted, and those of Galen. In all this, we see something of the wish to find all truths in the writings of the ancients, but nothing which materially interferes with freedom of inquiry. The anatomists have in all ages and countries been practically employed in seeking knowledge from observation. Facts have ever been to them a subject of careful and profitable study; while the ideas which enter into the wider truths of the science, are, as we have seen, even still involved in obscurity, doubt, and contest.

Maurolycus.—Francis Maurolycus of Messana, whose mathematical works were published in 1575, was one of the great improvers of the science of optics in his time. In his Preface to his Treatise on the Spheres, he speaks of previous writers on the same subject; and observes that as they have not superseded one another, they have not rendered it unfit for any one to treat the subject afresh. "Yet," he says, "it is impossible to amend the errours of all who have preceded us. This would be a task too hard for Atlas, although he supports the heavens. Even Copernicus is tolerated, who makes the sun to be fixed, and the earth to move round it in a circle; and who is more worthy of a whip or a scourge than of a refutation." The mathematicians and astronomers of that time were not the persons most sensible of the progress of physical knowledge; for the bases of their science, and a great part of its substance, were contained in the writings of the ancients; and till the time of Kepler, Ptolemy's work was, very justly, looked upon as including all that was essential in the science.

Benedetti.—But the writers on Mechanics were naturally led to present themselves as innovators and experimenters; for all that the ancients had taught concerning the doctrine of motion was erroneous; while those who sought their knowledge from experiment, were constantly led to new truths. John Baptist Benedetti, a Venetian nobleman, in 1599, published his *Speculationum Liber*, containing, among other matter, a treatise on Mechanics, in which several of the Aristotelian errours were refuted. In the Preface to this Treatise, he says, "Many authors have written much, and with great ability, on Mechanics; but since nature is constantly bringing to light something either new, or before unnoticed, I too wished to put forth a few things hitherto unattempted, or not sufficiently explained." In the doctrine of motion he distinctly and at some length condemns and argues against all the Aristotelian doctrines concerning motion, weight, and many other fundamental principles of physics. Benedetti is also an adherent of the Copernican doctrine. He states* the enormous velocity which the heavenly bodies must have, if the earth be the centre of their motions; and adds, "which difficulty does not occur according to the beautiful theory of the Samian Aristarchus, expounded in a divine manner by Nicolas Copernicus; against which the reasons alleged by Aristotle are of no weight." Benedetti throughout shows no want of the courage or ability which were needed in order to rise in opposition against the dogmas of the Peripatetics. He does not, however, refer to experiment in a very direct manner; indeed most of the facts on which the elementary truths of mechanics

* *Speculationum Liber*, p. 195.

rest, were known and admitted by the Aristotelians; and therefore could not be adduced as novelties. On the contrary, he begins with *à priori* maxims, which experience would not have confirmed. "Since," he says*, "we have undertaken the task of proving that Aristotle is wrong in his opinions concerning motion, there are certain absolute truths, the objects of the intellect known of themselves, which we must lay down in the first place." And then, as an example of these truths, he states this: "Any two bodies of equal size and figure, but of different materials, will have their natural velocities in the same proportion as their weights;" where by their natural velocities, he means the velocities with which they naturally fall downwards.

Gilbert.—The greatest of these practical reformers of science is our countryman, William Gilbert; if, indeed, in virtue of the clear views of the prospects which were then opening to science, and of the methods by which her future progress was to be secured, while he exemplified those views by physical discoveries, he do not rather deserve the still higher praise of being at the same time a theoretical and a practical reformer. Gilbert's physical researches and speculations were employed principally upon subjects on which the ancients had known little or nothing; and on which therefore it could not be doubtful whether tradition or observation was the source of knowledge. Such was magnetism; for the ancients were barely acquainted with the attractive property of the magnet. Its polarity, including repulsion as well as attraction, its direction towards the north, its limited variation from this direction, its declination from the horizontal position, were all modern discoveries. Gilbert's work† on the magnet and on the magnetism of

* *Speculationum Liber*, p. 169.

† Gulielmi Gilberti, *Colcestriensis*, *Medici Londinensis*, *De Mag-*

the earth, appeared in 1600; and in this, he repeatedly maintains the superiority of experimental knowledge over the physical philosophy of the ancients. His preface opens thus: "Since in making discoveries and searching out the hidden causes of things, stronger reasons are obtained from trustworthy experiments and demonstrable arguments, than from probable conjectures and the dogmas of those who philosophize in the usual manner," he has, he says, "endeavoured to proceed from common magnetical experiments to the inward constitution of the earth." As I have stated in the History of Magnetism*, Gilbert's work contains all the fundamental facts of that science, so fully stated, that we have, at this day, little to add to them. He is not, however, by the advance which he thus made, led to depreciate the ancients, but only to claim for himself the same liberty of philosophizing which they had enjoyed†. "To those ancient and first parents of philosophy, Aristotle, Theophrastus, Ptolemy, Hippocrates, Galen, be all due honour; from them it was that the stream of wisdom has been derived down to posterity. But our age has discovered and brought to light many things which they, if they were yet alive, would gladly embrace. Wherefore we also shall not hesitate to expound, by probable hypotheses, those things which by long experience we have ascertained."

In this work the author not only adopts the Copernican doctrine of the earth's motion, but speaks‡ of the contrary supposition as utterly absurd, founding his argument mainly on the vast velocities which such a supposition requires us to ascribe to the celestial bodies. Dr. Gilbert was physician to Queen Elizabeth and to James

nete, Magneticisque Corporibus, et de Magno Magnete Tellure, Physiologia Nova, plurimis et Argumentis et Experimentis demonstrata.

* *Hist. Ind. Sci.*, B. XII. c. i. † Pref.

‡ *De Magnete*, Lib. VI. c. 3, 4.

the First, and died in 1603. Sometime after his death the executors of his brother published another work of his, *De Mundo nostro Sublunari Philosophia Nova*, in which similar views are still more comprehensively presented. In this he says, "The two lords of philosophy, Aristotle and Galen, are held in worship like gods, and rule the schools;—the former by some destiny obtained a sway and influence among philosophers, like that of his pupil Alexander among the kings of the earth;—Galen, with like success, holds his triumph among the physicians of Europe." This comparison of Aristotle to Alexander was also taken hold of by Bacon. Nor is Gilbert an unworthy precursor of Bacon in the view he gives of the History of Science, which occupies the first three chapters of his Philosophy. He traces this history from "the simplicity and ignorance of the ancients," through "the fabrication of the fable of the four elements," to Aristotle and Galen. He mentions with due disapproval the host of commentators which succeeded, the alchemists, the "shipwreck of science in the deluge of the Goths," and the revival of letters and genius in the time of "our grandfathers." "This later age," he says, "has exploded the Barbarians, and restored the Greeks and Latins to their pristine grace and honour. It remains, that if they have written aught in errour, this should be remedied by better and more productive processes (*frugiferis institutis*,) not to be contemned for their novelty; (for nothing which is true is really new, but is perfect from eternity, though to weak man it may be unknown;) and that thus Philosophy may bear her fruit." The reader of Bacon will not fail to recognize, in these references to "fruit-bearing" knowledge, a similarity of expression with the *Novum Organon*.

Bacon does not appear to me to have done justice to his contemporary. He nowhere recognizes in the labours of Gilbert a community of purpose and spirit with his own. On the other hand, he casts upon him a reflection which he by no means deserves. In the *Advancement of Learning* *, he says, "Another errour is, that men have used to infect their meditations, opinions, and doctrines, with some conceits which they have most admired, or some sciences to which they have most applied; and given all things else a tincture according to them, utterly untrue and unproper. .... So have the alchemists made a philosophy out of a few experiments of the furnace; and Gilbertus, our countryman, hath made a philosophy out of the observations of a loadstone," (in the Latin, philosophiam etiam e magnete elicuit.) And in the same manner he mentions him in the *Novum Organon* †, as affording an example of an empirical kind of philosophy, which appears to those daily conversant with the experiments, probable, but to other persons incredible and empty. But instead of blaming Gilbert for disturbing and narrowing science by a too constant reference to magnetical rules, we might rather censure Bacon, for not seeing how important in all natural philosophy are those laws of attraction and repulsion of which magnetical phenomena are the most obvious illustration. We may find ground for such a judgment in another passage in which Bacon speaks of Gilbert. In the Second Book ‡ of the *Novum Organon*, having classified motions, he gives, as one kind, what he calls, in his figurative language, *motion for gain*, or *motion of need*, by which a body shuns heterogeneous, and seeks cognate bodies. And he adds, "The Electrical operation, concerning which Gilbert and others since him have made up such a wonderful story, is nothing less than the appetite of a body, which, excited by friction, does not well tolerate the air, and prefers another tangible body if it be found

* *Nov. Org.*, Book I. † Book I. Aph. 64. ‡ Vol. IX. 185.

near." Bacon's notion of an appetite in the body is certainly much less philosophical than Gilbert's, who speaks of light bodies as drawn towards amber by certain material radii*; and we might perhaps venture to say that Bacon here manifests a want of clear mechanical ideas. Bacon, too, showed his inferior aptitude for physical research in rejecting the Copernican doctrine which Gilbert adopted. In the *Advancement of Learning*†, suggesting a history of the opinions of philosophers, he says that he would have inserted in it even recent theories, as those of Paracelsus; of Telesius, who restored the philosophy of Parmenides; or Patricius, who resublimed the fumes of Platonism; or Gilbert, who brought back the dogmas of Philolaus. But Bacon quotes‡ with pleasure Gilbert's ridicule of the Peripatetics' definition of heat. They had said, that heat is that which separates heterogeneous and unites homogeneous matter; which, said Gilbert, is as if any one were to define *man* as that which sows wheat and plants vines.

Galileo, another of Gilbert's distinguished contemporaries, had a higher opinion of him. He says§, "I extremely admire and envy this author. I think him worthy of the greatest praise for the many new and true observations which he has made, to the disgrace of so many vain and fabling authors; who write, not from their own knowledge only, but repeat everything they hear from the foolish and vulgar, without attempting to satisfy themselves of the same by experience; perhaps that they may not diminish the size of their books."

Galileo.—Galileo was content with the active and successful practice of experimental inquiry; and did not demand that such researches should be made expressly

* *De Magnete*, p. 60.

† Book III. c. 4.

‡ *Nov. Org.*, Book II. Aph. 48.

§ Drinkwater's *Life of Galileo*, p. 18.

subservient to that wider and more ambitious philosophy, on which the author of the *Novum Organon* employed his powers. But still it now becomes our business to trace those portions of Galileo's views which have reference to the theory, as well as the practice, of scientific investigation. On this subject, Galileo did not think more profoundly, perhaps, than several of his contemporaries; but in the liveliness of expression and illustration with which he recommended his opinions on such topics, he was unrivalled. Writing in the language of the people, in the attractive form of dialogue, with clearness, grace, and wit, he did far more than any of his predecessors had done to render the new methods, results, and prospects of science familiar to a wide circle of readers, first in Italy, and soon, all over Europe. The principal points inculcated by him were already becoming familiar to men of active and inquiring minds; such as,—that knowledge was to be sought from observation, and not from books;—that it was absurd to adhere to, and debate about, the physical tenets of Aristotle and the rest of the ancients. On persons who followed this latter course, Galileo fixed the epithet of Paper Philosophers*; because, as he wrote in a letter to Kepler, this sort of men fancied that philosophy was to be studied like the *Æneid* or *Odyssee*, and that the true reading of nature was to be detected by the collation of texts. Nothing so much shook the authority of the received system of Physics as the experimental discoveries, directly contradicting it, which Galileo made. By experiment, as I have elsewhere stated†, he disproved the Aristotelian doctrine that bodies fall quickly or slowly in proportion to their weight. And when he had invented the telescope, a number of new discoveries of the most striking kind (the inequalities of the moon's

* *Life of Galileo*, p. 9. † *Hist. Ind. Sci.*, B. VI. c. ii. sect. 5.

surface, the spots in the sun, the moon-like phases of Venus, the satellites of Jupiter, the ring of Saturn,) showed, by the evidence of the eyes, how inadequate were the conceptions, and how erroneous the doctrines of the ancients, respecting the constitution of the universe. How severe the blow was to the disciples of the ancient schools, we may judge by the extraordinary forms of defence in which they tried to intrench themselves. They would not look through Galileo's glasses; they maintained that what was seen was an illusion of witchcraft; and they tried, as Galileo says*, with logical arguments, as if with magical incantations, to charm the new planets out of the sky. No one could be better fitted than Galileo for such a warfare. His great knowledge, clear intellect, gaiety, and light irony, (with the advantage of being in the right,) enabled him to play with his adversaries as he pleased. Thus when an Aristotelian† rejected the discovery of the irregularities in the moon's surface, because, according to the ancient doctrine, her form was a perfect sphere, and held that the apparent cavities were filled with an invisible crystal substance; Galileo replied, that he had no objection to assent to this, but that then he should require his adversary in return to believe that there were on the same surface invisible crystal mountains ten times as high as those visible ones which he had actually observed and measured.

We find in Galileo many thoughts which have since become established maxims of modern philosophy. "Philosophy," he says‡, "is written in that great book, I mean the Universe, which is constantly open before our eyes; but it cannot be understood, except we first know the language and learn the characters in which it is written." With this thought he combines some other

* *Life of Galileo*, p. 29. † *Ib.*, p. 33. ‡ *Il Saggiatore*, II. 247.

lively images. One of his interlocutors says concerning another, "Sarsi perhaps thinks that philosophy is a book made up of the fancies of men, like the *Iliad* or *Orlando Furioso*, in which the matter of least importance is, that what is written be true." And again, with regard to the system of authority, he says, "I think I discover in him a firm belief that, in philosophizing, it is necessary to lean upon the opinion of some celebrated author; as if our mind must necessarily remain unfruitful and barren till it be married to another man's reason."—"No," he says, "the case is not so.—When we have the decrees of Nature, authority goes for nothing; reason is absolute*."

In the course of Galileo's controversies, questions of the logic of science came under discussion. Vincenzio di Grazia objected to a proof from induction which Galileo adduced, because *all* the particulars were not enumerated; to which the latter justly replies†, that if induction were required to pass through all the cases, it would be either useless or impossible;—impossible when the cases are innumerable; useless when they have each already been verified, since then the general proposition adds nothing to our knowledge.

One of the most novel of the characters which Science assumes in Galileo's hands is, that she becomes cautious. She not only proceeds leaning upon Experience, but she is content to proceed a little way at a time. She already begins to perceive that she must rise to the heights of knowledge by many small and separate steps. The philosopher is desirous to know much, but resigned to be ignorant for a time of that which cannot yet be known. Thus when Galileo discovered the true law of the motion of a falling body‡, that the velocity increases proportionally to the time from the beginning of the fall, he did not

* *Il Saggiatore*, II. 200. † *Ib.*, I. 501.

‡ *Hist. Ind. Sci.*, B. VI. c. ii. sect. 2.

insist upon immediately assigning the cause of this law. "The cause of the acceleration of the motions of falling bodies is not," he says, "a necessary part of the investigation." Yet the conception of this acceleration, as the result of the continued action of the force of gravity upon the falling body, could hardly fail to suggest itself to one who had formed the idea of force. In like manner, the truth that the velocities, acquired by bodies falling down planes of equal heights, are all equal, was known to Galileo and his disciples, long before he accounted for it*, by the principle, apparently so obvious, that the momentum generated is as the moving force which generates it. He was not tempted to rush at once, from an experimental truth to a universal system. Science had learnt that she must move step by step; and the gravity of her pace already indicated her approaching maturity and her consciousness of the long path which lay before her.

But besides the genuine philosophical prudence which thus withheld Galileo from leaping hastily from one inference to another, he had perhaps a preponderating inclination towards facts; and did not feel, so much as some other persons of his time, the need of reducing them to ideas. He could bear to contemplate laws of motion without being urged by an uncontrollable desire to refer them to conceptions of force.

Kepler.—In this respect his friend Kepler differed from him; for Kepler was restless and unsatisfied till he had reduced facts to laws, and laws to causes; and never acquiesced in ignorance, though he tested with the most rigorous scrutiny that which presented itself in the shape of knowledge to fill the void. It may be seen in the History of Astronomy† with what perseverance, energy, and fertility of invention, Kepler pursued his labours,

* *Hist. Ind. Sci.*, B. VI. c. ii. sect. 4.

† *Ib.*, B. V. c. iv. sect. 1.

(enlivened and relieved by the most curious freaks of fancy,) with a view of discovering the rules which regulate the motions of the planet Mars. He represents this employment under the image of a warfare; and describes* his object to be "to triumph over Mars, and to prepare for him, as for one altogether vanquished, tabular prisons and equated eccentric fetters;" and when "the enemy, left at home a despised captive, had burst all the chains of the equations, and broken forth of the prisons of the tables;"—when "it was buzzed here and there that the victory is vain, and that the war is raging anew as violently as before;"—that is, when the rules which he had proposed did not coincide with the facts;—he by no means desisted from his attempts, but "suddenly sent into the field a reserve of new physical reasonings on the rout and dispersion of the veterans," that is, tried new suppositions suggested by such views as he then entertained of the celestial motions. His efforts to obtain the formal laws of the planetary motions resulted in some of the most important discoveries ever made in astronomy; and if his physical reasonings were for the time fruitless, this arose only from the want of that discipline in mechanical ideas which the minds of mathematicians had still to undergo; for the great discoveries of Newton in the next generation showed that, in reality, the next step of the advance was in this direction. Among all Kepler's fantastical expressions, the fundamental thoughts were sound and true; namely, that it was his business, as a physical investigator, to discover a mathematical rule which governed and included all the special facts; and that the rules of the motions of the planets must conform to some conception of causation.

The same characteristics,—the conviction of rule and cause, perseverance in seeking these, inventiveness in

* *De Stell. Mart.*, p. IV. c. 51. (1609.) Drinkwater's *Kepler*, p. 33.

devising hypotheses, love of truth in trying and rejecting them, and a lively Fancy playing with the Reason without interrupting her,—appear also in his work on Optics; in which he tried to discover the exact law of optical refraction*. In this undertaking he did not succeed entirely; nor does he profess to have done so. He ends his numerous attempts by saying, "Now, reader, you and I have been detained sufficiently long while I have been attempting to *collect into one fagot* the measures of different refractions."

In this and in other expressions, we see how clearly he apprehended that *colligation of facts* which is the main business of the practical discoverer. And by his peculiar endowments and habits, Kepler exhibits an essential portion of this process, which hardly appears at all in Galileo. In order to bind together facts, theory is requisite as well as observation,—the cord as well as the fagots. And the true theory is often, if not always, obtained by trying several and selecting the right. Now of this portion of the discoverer's exertions, Kepler is a most conspicuous example. His fertility in devising suppositions, his undaunted industry in calculating the results of them, his entire honesty and candour in resigning them if these results disagreed with the facts, are a very instructive spectacle; and are fortunately exhibited to us in the most lively manner in his own garrulous narratives. Galileo urged men by precept as well as example to begin their philosophy from observation; Kepler taught them by his practice that they must proceed from observation by means of hypotheses. The one insisted upon facts; the other dealt no less copiously with ideas. In the practical, as in the speculative portion of our history, this antithesis shows itself; although in the practical part we cannot have the two

* Published 1604. *Hist. Ind. Sci.*, B. IX. c. ii.

elements separated, as in the speculative we sometimes have.

In the *History of Science**, I have devoted several pages to the intellectual character of Kepler, inasmuch as his habit of devising so great a multitude of hypotheses, so fancifully expressed, had led some writers to look upon him as an inquirer who transgressed the most fixed rules of philosophical inquiry. This opinion has arisen, I conceive, among those who have forgotten the necessity of Ideas as well as Facts for all theory; or who have overlooked the impossibility of selecting and explicating our ideas without a good deal of spontaneous play of the mind. It must, however, always be recollected that Kepler's genius and fancy derived all their scientific value from his genuine and unmingled love of truth. These qualities appeared, not only in the judgment he passed upon hypotheses, but also in matters which more immediately concerned his reputation. Thus when Galileo's discovery of the telescope disproved several opinions which Kepler had published and strenuously maintained, he did not hesitate a moment to retract his assertions and range himself by the side of Galileo, whom he vigorously supported in his warfare against those who were incapable of thus cheerfully acknowledging the triumph of new facts over their old theories.

Tycho.—There remains one eminent astronomer, the friend and fellow-labourer of Kepler, whom we must not separate from him as one of the practical reformers of science. I speak of Tycho Brahe, who is, I think, not justly appreciated by the literary world in general, in consequence of his having made a retrograde step in that portion of astronomical theory which is most familiar to the popular mind. Though he adopted the

* *Hist. Ind. Sci.*, B. v. c. iv. sect. 1.

Copernican view of the motion of the planets about the sun, he refused to acknowledge the annual and diurnal motion of the earth. But notwithstanding this mistake, into which he was led by his interpretation of Scripture rather than of nature, Tycho must ever be one of the greatest names in astronomy. In the philosophy of science also, the influence of what he did is far from inconsiderable; and especially its value in bringing into notice these two points:—that not only are observations the beginning of science, but that the progress of science may often depend upon the observer's pursuing his task regularly and carefully for a long time, and with well devised instruments; and again, that observed facts offer a *succession* of laws which we discover as our observations become better, and as our theories are better adapted to the observations. With regard to the former point, Tycho's observatory was far superior to all that had preceded it*, not only in the optical, but in the mechanical arrangements; a matter of almost equal consequence. And hence it was that his observations inspired in Kepler that confidence which led him to all his labours and all his discoveries. "Since," he says†, "the divine goodness has given us in Tycho Brahe an exact observer, from whose observations this errour of eight minutes in the calculations of the Ptolemaic hypothesis is detected, let us acknowledge and make use of this gift of God: and since this errour cannot be neglected, these eight minutes alone have prepared the way for an entire reform of Astronomy, and are to be the main subject of this work."

With regard to Tycho's discoveries respecting the moon, it is to be recollected that besides the first inequality of the moon's motion, (the *equation of the center*, arising from the elliptical form of her orbit,) Ptolemy

* *Hist. Ind. Sci.*, B. VII. c. vi. sect. 1. † *De Stell. Mart.*, p. 11, c. 19.

had discovered a second inequality, the *evection*, which, as we have observed in the History of this subject*, might have naturally suggested the suspicion that there were still other inequalities. In the middle ages, however, such suggestions, implying a constant progress in science, were little attended to; and, we have seen, that when an Arabian astronomer† had really discovered another inequality of the moon, it was soon forgotten, because it had no place in the established systems. Tycho not only rediscovered the lunar inequality, (the *variation*,) thus once before won and lost, but also two other inequalities; namely‡, the *change of inclination* of the moon's orbit as the line of nodes moves round, and an inequality in the motion of the line of nodes. Thus, as I have elsewhere said, it appeared that the discovery of a rule is a step to the discovery of deviations from that rule, which require to be expressed in other rules. It became manifest to astronomers, and through them to all philosophers, that in the application of theory to observation, we find, not only the stated phenomena, for which the theory does account, but also *residual phenomena*, which are unaccounted for, and remain over and above the calculation. And it was seen further, that these residual phenomena might be, altogether or in part, exhausted by new theories.

These were valuable lessons; and the more valuable inasmuch as men were now trying to lay down maxims and methods for the conduct of science. A revolution was not only at hand, but had really taken place, in the great body of real cultivators of science. The occasion now required that this revolution should be formally recognized;—that the new intellectual power should be clothed with the forms of government;—that the new

* *Hist. Ind. Sci.*, B. II. c. iv. sect. 6. † *Ib.*, sect. 8.
‡ Montucla, I. 566.

philosophical republic should be acknowledged as a sister state by the ancient dynasties of Aristotle and Plato. There was needed some great Theoretical Reformer, to speak in the name of the Experimental Philosophy; to lay before the world a declaration of its rights and a scheme of its laws. And thus our eyes are turned to Francis Bacon, and others who like him attempted this great office. We quit those august and venerable names of discoverers, whose appearance was the prelude and announcement of the new state of things then opening; and in doing so, we may apply to them the language which Bacon applies to himself*:—

> Χαίρετε Κήρυκες Διὸς ἄγγελοι ἠδὲ καὶ ἀνδρῶν.
> Hail Heralds, Messengers of Gods and Men!

Chapter XI.

FRANCIS BACON.

1. It is a matter of some difficulty to speak of the character and merits of this illustrious man, as regards his place in that philosophical history with which we are here engaged. If we were to content ourselves with estimating him according to the office which, as we have just seen, he claims for himself†, as merely the harbinger and announcer of a sounder method of scientific inquiry than that which was recognized before him, the task would be comparatively easy. For we might select from his writings those passages in which he has delivered opinions and pointed out processes, then novel and strange, but since confirmed by the experience of actual discoverers, and by the judgments of the wisest of suc-

* *De Augm.*, Lib. IV. c. 1.

† And in other passages: thus, "Ego enim buccinator tantum pugnam non ineo." *Nov. Org.*, Lib. IV. c. 1.

ceeding philosophers; and we might pass by, without disrespect, but without notice, maxims and proposals which have not been found available for use;—views so indistinct and vague, that we are even yet unable to pronounce upon their justice;—and boundless anticipations, dictated by the sanguine hopes of a noble and comprehensive intellect. But if we thus reduce the philosophy of Bacon to that portion which the subsequent progress of science has rigorously verified, we shall have to pass over many of those declarations which have excited most notice in his writings, and shall lose sight of many of those striking thoughts which his admirers most love to dwell upon. For he is usually spoken of, at least in this country, as a teacher who not only commenced, but in a great measure completed, the Philosophy of Induction. He is considered, not only as having asserted some general principles, but laid down the special rules of scientific investigation; as not only one of the Founders, but the supreme Legislator of the modern Republic of Science; not only the Hercules who slew the monsters that obstructed the earlier traveller, but the Solon who established a constitution fitted for all future time.

2. Nor is it our purpose to deny that of such praise he deserves a share which, considering the period at which he lived, is truly astonishing. But it is necessary for us in this place to discriminate and select that portion of his system which, bearing upon *physical* science, has since been confirmed by the actual history of science. Many of Bacon's most impressive and captivating passages contemplate the extension of the new methods of discovering truth to intellectual, to moral, to political, as well as to physical science. And how far, and how, the advantages of the inductive method may be secured for those important branches of speculation, it will at

some future time be a highly interesting task to examine. But our plan requires us at present to omit the consideration of these; for our purpose is to learn what the genuine course of the formation of science is, by tracing it in those portions of human knowledge, which, by the confession of all, are most exact, most certain, most complete. Hence we must here deny ourselves the dignity and interest which float about all speculations in which the great moral and political concerns of men are involved. It cannot be doubted that the commanding position which Bacon occupies in men's estimation arises from his proclaiming a reform in philosophy of so comprehensive a nature;—a reform which was to infuse a new spirit into every part of knowledge. Physical Science has tranquilly and noiselessly adopted many of his suggestions; which were, indeed, her own natural impulses, not borrowed from him; and she is too deeply and satisfactorily absorbed in contemplating her results, to talk much about the methods of obtaining them which she has thus instinctively pursued. But the philosophy which deals with mind, with manners, with morals, with polity, is conscious still of much obscurity and perplexity; and would gladly borrow aid from a system in which aid is so confidently promised. The aphorisms and phrases of the *Novum Organon* are far more frequently quoted by metaphysical, ethical, and even theological writers, than they are by the authors of works on physics.

3. Again, even as regards physics, Bacon's fame rests upon something besides the novelty of the maxims which he promulgated. That a revolution in the method of scientific research was going on, all the greatest physical investigators of the sixteenth century were fully aware, as we have shown in the last chapter. But their writings conveyed this conviction to the public at large somewhat slowly. Men of letters, men of the world, men

of rank, did not become familiar with the abstruse works in which these views were published; and above all, they did not, by such occasional glimpses as they took of the state of physical science, become aware of the magnitude and consequences of this change. But Bacon's lofty eloquence, wide learning, comprehensive views, bold pictures of the coming state of things, were fitted to make men turn a far more general and earnest gaze upon the passing change. When a man of his acquirements, of his talents, of his rank and position, of his gravity and caution, poured forth the strongest and loftiest expressions and images which his mind could supply, in order to depict the "Great Instauration" which he announced;—in order to contrast the weakness, the blindness, the ignorance, the wretchedness, under which men had laboured while they followed the long beaten track, with the light, the power, the privileges, which they were to find in the paths to which he pointed;—it was impossible that readers of all classes should not have their attention arrested, their minds stirred, their hopes warmed; and should not listen with wonder and with pleasure to the strains of prophetic eloquence in which so great a subject was presented. And when it was found that the prophecy was verified; when it appeared that an immense change in the methods of scientific research really *had* occurred;—that vast additions to man's knowledge and power had been acquired, in modes like those which had been spoken of; —that further advances might be constantly looked for;—and that a progress, seemingly boundless, was going on in the direction in which the seer had thus pointed;—it was natural that men should hail him as the leader of the revolution; that they should identify him with the event which he was the first to announce; that they should look upon him as the author of that which he had, as they perceived, so soon and so thoroughly comprehended.

4. For we must remark, that although (as we have seen) he was not the only, nor the earliest writer, who declared that the time was come for such a change, he not only proclaimed it more emphatically, but understood it, in its general character, much more exactly, than any of his contemporaries. Among the maxims, suggestions and anticipations which he threw out, there were many of which the wisdom and the novelty were alike striking to his immediate successors;—there are many which even now, from time to time, we find fresh reason to admire, for their acuteness and justice. Bacon stands far above the herd of loose and visionary speculators who, before and about his time, spoke of the establishment of new philosophies. If we must select some one philosopher as the Hero of the revolution in scientific method, beyond all doubt Francis Bacon must occupy the place of honour.

We shall, however, no longer dwell upon these general considerations, but shall proceed to notice some of the more peculiar and characteristic features of Bacon's philosophy; and especially those views, which, occurring for the first time in his writings, have been fully illustrated and confirmed by the subsequent progress of science, and have become a portion of the permanent philosophy of our times.

5. (I.) The first great feature which strikes us in Bacon's philosophical views is that which we have already noticed;—his confident and emphatic announcement of a *New Era* in the progress of science, compared with which the advances of former times were poor and trifling. This was with Bacon no loose and shallow opinion, taken up on light grounds and involving only vague general notions. He had satisfied himself of the justice

of such a view by a laborious course of research and reflection. In 1605, at the age of forty-four, he published his Treatise of the *Advancement of Learning*, in which he takes a comprehensive and spirited survey of the condition of all branches of knowledge which had been cultivated up to that time. This work was composed with a view to that reform of the existing philosophy which Bacon always had before his eyes; and in the Latin edition of his works, forms the First Part of the *Instauratio Magna*. In the Second Part of the Instauratio, the *Novum Organon*, published in 1620, he more explicitly and confidently states his expectations on this subject. He points out how slightly and feebly the examination of nature had been pursued up to his time, and with what scanty fruit. He notes the indications of this in the very limited knowledge of the Greeks who had till then been the teachers of Europe, in the complaints of authors concerning the subtilty and obscurity of the secrets of nature, in the dissensions of sects, in the absence of useful inventions resulting from theory, in the fixed form which the sciences had retained for two thousand years. Nor, he adds*, is this wonderful; for how little of his thought and labour has man bestowed upon science! Out of twenty-five centuries scarce six have been favourable to the progress of knowledge. And even in those favoured times, natural philosophy received the smallest share of man's attention; while the portion so given was marred by controversy and dogmatism; and even those who have bestowed a little thought upon this philosophy, have never made it their main study, but have used it as a passage or drawbridge to serve other objects. And thus, he says, the great Mother of the Sciences is thrust down with indignity to the offices of a handmaid; is made to minister to the labours of

* Lib. I. Aphor. 78 *et seq.*

medicine or mathematics, or to give the first preparatory tinge to the immature minds of youth. For these and similar considerations of the errours of past time, he draws hope for the future, employing the same argument which Demosthenes uses to the Athenians: "That which is worst in the events of the past, is the best as a ground of trust in the future. For if you had done all that became you, and still had been in this condition, your case might be desperate; but since your failure is the result of your own mistakes, there is good hope that, correcting the errour of your course, you may reach a prosperity yet unknown to you."

6. (II.) All Bacon's hope of improvement indeed was placed in an entire *change of the Method* by which science was pursued; and the boldness, and at the same time, (the then existing state of science being considered) the definiteness of his views of the change that was requisite are truly remarkable.

That all knowledge must begin with observation, is one great principle of Bacon's philosophy; but I hardly think it necessary to notice the inculcation of this maxim as one of his main services to the cause of sound knowledge, since it had, as we have seen, been fully insisted upon by others before him, and was growing rapidly into general acceptance without his aid. But if he was not the first to tell men that they must collect their knowledge from observation, he had no rival in his peculiar office of teaching them *how* science must thus be gathered from experience.

It appears to me that by far the most extraordinary parts of Bacon's works are those in which, with extreme earnestness and clearness, he insists upon a *graduated and successive induction*, as opposed to a hasty transit from special facts to the highest generalizations. The nineteenth Axiom of the First Book of the *Novum Organon* contains a view of the nature of true science most exact and profound; and, so far as I am aware, at the time perfectly new. "There are two ways, and can only be two, of seeking and finding truth. The one, from sense and particulars, takes a flight to the most general axioms, and from those principles and their truth, settled once for all, invents and judges of intermediate axioms. The other method collects axioms from sense and particulars, ascending *continuously and by degrees*, so that in the end it arrives at the most general axioms; this latter way is the true one, but hitherto untried."

It is to be remarked, that in this passage Bacon employs the term *axioms* to express any propositions collected from facts by induction, and thus fitted to become the starting-point of deductive reasonings. How far propositions so obtained may approach to the character of axioms in the more rigorous sense of the term, we have already in some measure examined; but that question does not here immediately concern us. The truly remarkable circumstance is to find this recommendation of a continuous advance from observation, by limited steps, through successive gradations of generality, given at a time when speculative men in general had only just begun to perceive that they must begin their course from experience in some way or other. How exactly this description represents the general structure of the soundest and most comprehensive physical theories, all persons who have studied the progress of science up to modern times can bear testimony; but perhaps this structure of science cannot in any other way be made so apparent as by those Tables of successive generalizations in which we have exhibited the history and constitution of some of the principal physical sciences, in the Chapter of the preceding Book which treats of the Logic of Induction. And the view which Bacon thus took of the true progress of science was not only new, but, so far as I am aware, has never been adequately illustrated up to the present day.

7. It is true, as I observed in the last chapter, that Galileo had been led to see the necessity, not only of proceeding from experience in the pursuit of knowledge, but of proceeding cautiously and gradually; and he had exemplified this rule more than once, when, having made one step in discovery, he held back his foot, for a time, from the next step, however tempting. But Galileo had not reached this wide and commanding view of the successive subordination of many steps, all leading up at last to some wide and simple general truth. In catching sight of this principle, and in ascribing to it its due importance, Bacon's sagacity, so far as I am aware, wrought unassisted and unrivalled.

8. Nor is there any wavering or vagueness in Bacon's assertion of this important truth. He repeats it over and over again; illustrates it by a great number of the most lively metaphors and emphatic expressions. Thus he speaks of the successive *floors* (*tabulata*) of induction; and speaks of each science as a *pyramid** which has observation and experience for its basis. No images can better exhibit the relation of general and particular truths, as our own Inductive Tables may serve to show.

9. (III.) Again; not less remarkable is his contrasting this true Method of Science (while it was almost, as he says, yet untried) with the ancient and *vicious Method*, which began, indeed, with facts of observation, but rushed

* *Aug. Sc.*, Lib. III. c. 4. p. 194. So in other places, as *Nov. Org.*, I. Aphorism 104. "De scientiis tum demum bene sperandum est quando per scalam veram et per gradus continuos, et non intermissos aut hiulcos a particularibus ascendetur ad axiomata minora, et deinde ad media, alia aliis superiora, et postremo demum ad generalissima."

at once, and with no gradations, to the most general principles. For this was the course which had been actually followed by all those speculative reformers who had talked so loudly of the necessity of beginning our philosophy from experience. All these men, if they attempted to frame physical doctrines at all, had caught up a few facts of observation, and had erected a universal theory upon the suggestions which these offered. This process of illicit generalization, or, as Bacon terms it, Anticipation of Nature (*anticipatio naturæ*), in opposition to the Interpretation of Nature, he depicts with singular acuteness, in its character and causes. "These two ways," he says* "both begin from sense and particulars; but their discrepancy is immense. The one merely skims over experience and particulars in a cursory transit; the other deals with them in a due and orderly manner. The one, at its very outset, frames certain general abstract principles, but useless; the other gradually rises to those principles which have a real existence in nature."

"The former path," he adds†, "that of illicit and hasty generalization, is one which the intellect follows when abandoned to its own impulse; and this it does from the requisitions of logic. For the mind has a yearning which makes it dart forth to generalities, that it may have something to rest in; and after a little dallying with experience, becomes weary of it; and all these evils are augmented by logic, which requires these generalities to make a show with in its disputations."

"In a sober, patient, grave intellect," he further adds, "the mind, by its own impulse, (and more especially if it be not impeded by the sway of established opinions) attempts in some measure that other and true way, of gradual generalization; but this it does with small profit;

* *Nov. Org.*, I. Aph. 22. † *Ib.*, Aph. 20.

for the intellect, except it be regulated and aided, is a faculty of unequal operation, and altogether unapt to master the obscurity of things."

The profound and searching wisdom of these remarks appears more and more, as we apply them to the various attempts which men have made to obtain knowledge; when they begin with the contemplation of a few facts, and pursue their speculations, as upon most subjects they have hitherto generally done; for almost all such attempts have led immediately to some process of illicit generalization, which introduces an interminable course of controversy. In the physical sciences, however, we have the further inestimable advantage of seeing the other side of the contrast exemplified: for many of them, as our Inductive Tables show us, have gone on according to the most rigorous conditions of gradual and successive generalization; and in consequence of this circumstance in their constitution, possess, in each part of their structure, a solid truth, which is always ready to stand the severest tests of reasoning and experiment.

We see how justly and clearly Bacon judged concerning the mode in which facts are to be employed in the construction of science. This, indeed, has ever been deemed his great merit: insomuch that many persons appear to apprehend the main substance of his doctrine to reside in the maxim that facts of observation, and such facts alone, are the essential elements of all true science.

10. (IV.) Yet we have endeavoured to establish the doctrine that facts are but one of two ingredients of knowledge both equally necessary;—that *Ideas* are no less indispensable than facts themselves; and that except these be duly unfolded and applied, facts are collected in vain. Has Bacon then neglected this great portion of

his subject? Has he been led by some partiality of view, or some peculiarity of circumstances, to leave this curious and essential element of science in its pristine obscurity? Was he unaware of its interest and importance?

We may reply that Bacon's philosophy, in its effect upon his readers in general, does *not* give due weight or due attention to the ideal element of our knowledge. He is considered as peculiarly and eminently the asserter of the value of experiment and observation. He is always understood to belong to the experiential, as opposed to the ideal school. He is held up in contrast to Plato and others who love to dwell upon that part of knowledge which has its origin in the intellect of man.

11. Nor can it be denied that Bacon has, in the finished part of his *Novum Organum*, put prominently forwards the necessary dependence of all our knowledge upon Experience, and said little of its dependence, equally necessary, upon the Conceptions which the intellect itself supplies. It will appear, however, on a close examination, that he was by no means insensible or careless of this internal element of all connected speculation. He held the balance, with no partial or feeble hand, between phenomena and ideas. He urged the Colligation of Facts, but he was not the less aware of the value of the Explication of Conceptions.

12. This appears plainly from some remarkable Aphorisms in the *Novum Organum*. Thus, in noticing the causes of the little progress then made by science, he states this:—"In the current Notions, all is unsound, whether they be logical or physical. *Substance*, *quality*, *action*, *passion*, even *being*, are not good Conceptions; still less are *heavy*, *light*, *dense*, *rare*, *moist*, *dry*, *generation*, *corruption*, *attraction*, *repulsion*, *element*, *matter*, *form*, and others of that kind; all are fantastical and ill-defined."

And in his attempt to exemplify his own system, he hesitates* in accepting or rejecting the notions of *elementary*, *celestial*, *rare*, as belonging to fire, since, as he says, they are vague and ill-defined notions (*notiones vagæ nec bene terminatæ*). In that part of his work which appears to be completed, there is not, so far as I have noticed, any attempt to fix and define any notions thus complained of as loose and obscure. But yet such an undertaking appears to have formed part of his plan; and in the *Abecedarium Naturæ* †, which consists of the heads of various portions of his great scheme, marked by letters of the alphabet, we find the titles of a series of dissertations "On the Conditions of Beings," which must have had for their object the elucidation of divers Notions essential to science, and which would have been contributions to the Explication of Conceptions, such as we have attempted in a former part of this work. Thus some of the subjects of these dissertations are;—Of Much and Little;—Of Durable and Transitory;—Of Natural and Monstrous;—Of Natural and Artificial. When the philosopher of induction came to discuss these, considered as *conditions of existence*, he could not do other than develope, limit, methodize, and define the Ideas involved in these Notions, so as to make them consistent with themselves, and a fit basis of demonstrative reasoning. His task would have been of the same nature as ours has been, in that part of this work which treats of the Fundamental Ideas of the various classes of sciences.

13. Thus Bacon, in his speculative philosophy, took firmly hold of both the handles of science; and if he had completed his scheme, would probably have given due attention to Ideas, no less than to Facts, as an element of our knowledge; while in his view of the general

* *Nov. Org.*, Lib. II. Aph. 19.

† *Inst. Mag.*, Par. III. (Vol. VIII. p. 244.)

method of ascending from facts to principles, he displayed a sagacity truly wonderful. But we cannot be surprized, that in attempting to exemplify the method which he recommended, he should have failed. For the method could be exemplified only by some important discovery is physical science; and great discoveries, even with the most perfect methods, do not come at command. Moreover although the general structure of his scheme was correct, the precise import of some of its details could hardly be understood, till the actual progress of science had made men somewhat familiar with the kind of steps which it included.

14. (V.) Accordingly, Bacon's *Inquisition into the Nature of Heat*, which is given in the Second Book of the *Novum Organon* as an example of the mode of interrogating Nature, cannot be looked upon otherwise than as a complete failure. This will be evident if we consider that, although the exact nature of heat is still an obscure and controverted matter, the science of Heat now consists of many important truths; and that to none of these truths is there any approximation in Bacon's essay. From his process he arrives at this, as the "forma or true definition" of heat;—"that it is an expansive, restrained motion, modified in certain ways, and exerted in the smaller particles of the body." But the steps by which the science of Heat really advanced were, (as may be seen in the history* of the subject,) these;—The discovery of a *measure* of heat or temperature (the thermometer); The establishment of the *laws* of conduction and radiation; of the *laws* of specific heat, latent heat, and the like. Such steps have led to Ampère's *hypothesis*†, that heat consists in the vibrations of an imponderable fluid; and to Laplace's *hypothesis*, that temperature consists in the internal radiation of such a fluid. These hypotheses

* *Hist. Ind. Sci.*, B. x. c. i.

† *Ib.*, c. iv.

cannot yet be said to be even probable; but at least they are so modified as to include some of the preceding laws which are firmly established; whereas Bacon's hypothetical motion includes no laws of phenomena, explains no process, and is indeed itself an example of illicit generalization.

15. One main ground of Bacon's ill fortune in this undertaking appears to be, that he was not aware of an important maxim of inductive science, that we must first obtain the *measure* and ascertain the *laws* of phenomena, before we endeavour to discover their *causes*. The whole history of thermotics up to the present time has been occupied with the *former* step, and the task is not yet completed: it is no wonder, therefore, that Bacon failed entirely, when he so prematurely attempted the *second*. His sagacity had taught him that the progress of science must be gradual; but it had not led him to judge adequately how gradual it must be, nor of what different kinds of inquiries, taken in due order, it must needs consist, in order to obtain success.

Another mistake, which could not fail to render it unlikely that Bacon should really exemplify his precepts by any actual advance in science, was, that he did not justly appreciate the sagacity, the inventive genius, which all discovery requires. He conceived that he could supersede the necessity of such peculiar endowments. "Our method of discovery in science," he says*, "is of such a nature, that there is not much left to acuteness and strength of genius, but all degrees of genius and intellect are brought nearly to the same level." And he illustrates this by comparing his method to a pair of compasses, by means of which a person with no manual skill may draw a perfect circle. In the same spirit he speaks of proceeding by *due rejections*; and appears to

* *Nov. Org.*, Lib. I. Aph. 61.

imagine that when we have obtained a collection of facts, if we go on successively rejecting what is false, we shall at last find that we have, left in our hands, that scientific truth which we seek. I need not observe how far this view is removed from the real state of the case. The necessity of a *conception* which must be furnished by the mind in order to bind together the facts, could hardly have escaped the eye of Bacon, if he had cultivated more carefully the ideal side of his own philosophy. And any attempts which he could have made to construct such conceptions by mere rule and method, must have ended in convincing him that nothing but a peculiar inventive talent could supply that which was thus not contained in the facts, and yet was needed for the discovery.

16. (VI.) Since Bacon, with all his acuteness, had not divined circumstances so important in the formation of science, it is not wonderful that his attempt to reduce this process to a *Technical Form* is of little value. In the first place, he says*, we must prepare a natural and experimental history, good and sufficient; in the next place, the instances thus collected are to be arranged in Tables in some orderly way; and then we must apply a legitimate and true induction. And in his example†, he first collects a great number of cases in which heat appears under various circumstances, which he calls "a Muster of Instances before the intellect," (*comparentia instantiarum ad intellectum*,) or a *Table of the Presence* of the thing sought. He then adds a *Table of its Absence* in proximate cases, containing instances where heat does not appear; then a *Table of Degrees*, in which it appears with greater or less intensity. He then adds‡, that we must try to exclude several obvious suppositions, which he does by reference to some of the instances he

* *Nov. Org.*, Lib. II. Aph. 10. † Aph. 11.

‡ Aph. 15. p. 105.

has collected; and this step he calls the *Exclusive*, or the *Rejection of Natures*. He then observes, (and justly,) that whereas truth emerges more easily from errour than from confusion, we may, after this preparation, *give play to the intellect*, (fiat permissio intellectus,) and make an attempt at induction, liable afterwards to be corrected; and by this step, which he terms his *First Vindemiation*, or *Inchoate Induction*, he is led to the proposition concerning heat, which we have stated above.

17. In all the details of his example he is unfortunate. By proposing to himself to examine at once into the *nature* of heat, instead of the laws of special classes of phenomena, he makes, as we have said, a fundamental mistake; which is the less surprizing since he had before him so few examples of the right course in the previous history of science. But further, his collection of instances is very loosely brought together; for he includes in his list the *hot* taste of aromatic plants, the *caustic* effects of acids, and many other facts which cannot be ascribed to heat without a studious laxity in the use of the word. And when he comes to that point where he permits his intellect its range, the conception of *motion* upon which it at once fastens, appears to be selected with little choice or skill, the suggestion being taken from flame*, boiling liquids, a blown fire, and some other cases. If from such examples we could imagine heat to be motion, we ought at least to have some gradation to cases of heat where no motion is visible, as in a red-hot iron. It would seem that, after a large collection of instances had been looked at, the intellect, even in its first attempts, ought not to have dwelt upon such an hypothesis as this.

18. After these steps, Bacon speaks of several classes of instances which, singling them out of the general and

* Page 110.

indiscriminate collection of facts, he terms *Instances with Prerogative;* and these he points out as peculiar aids and guides to the intellect in its task. These Instances with Prerogative have generally been much dwelt upon by those who have commented on the *Novum Organon.* Yet, in reality, such a classification, as has been observed by one of the ablest writers of the present day*, is of little service in the task of induction. For the instances are, for the most part, classed, not according to the ideas which they involve, or to any obvious circumstance in the facts of which they consist, but according to the extent or manner of their influence upon the inquiry in which they are employed. Thus we have Solitary Instances, Migrating Instances, Ostensive Instances, Clandestine Instances, so termed according to the degree in which they exhibit, or seem to exhibit, the property whose nature we would examine. We have Guide-Post Instances, (*Instantiæ Crucis,*) Instances of the Parted Road, of the Doorway, of the Lamp, according to the guidance they supply to our advance. Such a classification is much of the same nature as if, having to teach the art of building, we were to describe tools with reference to the amount and place of the work which they must do, instead of pointing out their construction and use:—as if we were to inform the pupil that we must have tools for lifting a stone up, tools for moving it sideways, tools for laying it square, tools for cementing it firmly. Such an enumeration of ends would convey little instruction as to the means. Moreover, many of Bacon's classes of instances are vitiated by the assumption that the "form," that is, the general law and cause of the property which is the subject of investigation, is to be looked for directly in the instances;

* Herschel, *On the Study of Nat. Phil.*, Art. 192.

which, as we have seen in his inquiry concerning heat, is a fundamental errour.

19. Yet his phraseology in some cases, as in the *instantia crucis*, serves well to mark the place which certain experiments hold in our reasonings: and many of the special examples which he gives are full of acuteness and sagacity. Thus he suggests swinging a pendulum in a mine, in order to determine whether the attraction of the earth arises from the attraction of its parts; and observing the tide at the same moment in different parts of the world, in order to ascertain whether the motion of the water is expansive or progressive; with other ingenious proposals. These marks of genius may serve to counterbalance the unfavourable judgment of Bacon's aptitude for physical science which we are sometimes tempted to form, in consequence of his false views on other points; as his rejection of the Copernican system, and his undervaluing Gilbert's magnetical speculations. Most of these errours arose from a too ambitious habit of intellect, which would not be contented with any except very wide and general truths; and from an indistinctness of mechanical, and perhaps, in general, of mathematical ideas:—defects which Bacon's own philosophy was directed to remedy, and which, in the progress of time, it has remedied in others.

20. (VII.) Having thus freely given our judgment concerning the most exact and definite portion of Bacon's precepts, it cannot be necessary for us to discuss at any length the value of those more vague and general *Warnings* against prejudice and partiality, against intellectual indolence and presumption, with which his works abound. His advice and exhortations of this kind are always expressed with energy and point, often clothed in the happiest forms of imagery; and hence it has come to pass, that such passages are perhaps more familiar to the

general reader than any other parts of his writings. Nor are Bacon's counsels without their importance, when we have to do with those subjects in which prejudice and partiality exercise their peculiar sway. Questions of politics and morals, of manners, taste, or history, cannot be subjected to a scheme of rigorous induction; and though on such matters we venture to assert general principles, these are commonly obtained with some degree of insecurity, and depend upon special habits of thought, not upon mere logical connexion. Here, therefore, the intellect may be perverted, by mixing, with the pure reason, our gregarious affections, or our individual propensities; the false suggestions involved in language, or the imposing delusions of received theories. In these dim and complex labyrinths of human thought, *the Idol of the Tribe*, or *of the Den*, *of the Forum*, or *of the Theatre*, may occupy men's minds with delusive shapes, and may obscure or pervert their vision of truth. But in that Natural Philosophy with which we are here concerned, there is little opportunity for such influences. As far as a physical theory is completed through all the steps of a just induction, there is a clear daylight diffused over it which leaves no lurking-place for prejudice. Each part can be examined separately and repeatedly; and the theory is not to be deemed perfect till it will bear the scrutiny of all sound minds alike. Although, therefore, Bacon, by warning men against the idols or fallacious images above spoken of, may have guarded them from dangerous errour, his precepts have little to do with Natural Philosophy: and we cannot agree with him when he says*, that the doctrine concerning these idols bears the same relation to the interpretation of nature as the doctrine concerning sophistical paralogisms bears to common logic.

* *Nov. Org.*, Lib. I. Aph. 40.

21. (VIII.) There is one very prominent feature in Bacon's speculations which we must not omit to notice; it is a leading and constant object with him to apply his knowledge to *Use*. The insight which he obtains into nature, he would employ in commanding nature for the service of man. He wishes to have not only principles but works. The phrase which best describes the aim of his philosophy is his own*, "Ascendendo ad *axiomata*, descendendo ad *opera*." This disposition appears in the first aphorism of the *Novum Organon*, and runs through the work. "Man, the *minister* and interpreter of nature, *does* and understands, so far as he has, in fact or in thought, observed the course of nature; and he cannot know or *do* more than this." It is not necessary for us to dwell much upon this turn of mind; for the whole of our present inquiry goes upon the supposition that an acquaintance with the laws of nature is worth our having for its own sake. It may be universally true, that Knowledge is Power; but we have to do with it not as Power, but as Knowledge. It is the formation of Science, not of Art, with which we are here concerned. It may give a peculiar interest to the history of science, to show how it constantly tends to provide better and better for the wants and comforts of the body; but *that* is not the interest which engages us in our present inquiry into the nature and course of philosophy. The consideration of the means which promote man's material well-being often appears to be invested with a kind of dignity, by the discovery of general laws which it involves; and the satisfaction which rises in our minds at the contemplation of such cases, men sometimes ascribe, with a false ingenuity, to the love of mere bodily enjoyment. But it is never difficult to see that this baser and coarser element is not the real source of our admiration. Those

* *Nov. Org.*, Lib. I. Ax. 103.

who hold that it is the main business of science to construct instruments for the uses of life, appear sometimes to be willing to accept the consequence which follows from such a doctrine, that the first shoemaker was a philosopher worthy of the highest admiration*. But those who maintain such paradoxes, often, by a happy inconsistency, make it their own aim, not to devise some improved covering for the feet, but to delight the mind with acute speculations, exhibited in all the graces of wit and fancy.

It has been said† that the key of the Baconian doctrine consists in two words, Utility and Progress. With regard to the latter point, we have already seen that the hope and prospect of a boundless progress in human knowledge had sprung up in men's minds, even in the early times of imperial Rome; and were most emphatically expressed by that very Seneca who disdained to reckon the worth of knowledge by its value in food and clothing. And when we say that Utility was the great business of Bacon's philosophy, we forget one-half of his characteristic phrase. "Ascendendo ad axiomata," no less than "descendendo ad opera," was, he repeatedly declared, the scheme of his path. He constantly spoke, we are told by his secretary‡, of two kinds of experiments, *experimenta fructifera*, and *experimenta lucifera*.

Again; when we are told by modern writers that Bacon merely recommended such induction as all men instinctively practise, we ought to recollect his own earnest and incessant declarations to the contrary. The induction hitherto practised is, he says, of no use for obtaining solid science. There are two ways§, "hæc via in usu est," "altera vera, sed intentata." Men have con-

* *Edinb. Rev.*, No. cxxxii. p. 65. † *Ib.*

‡ Pref. to the *Nat. Hist.*, I. 243.

§ *Nov. Org.*, Lib. I. Aph. 19.

stantly been employed in *anticipation*; in illicit induction. The intellect left to itself rushes on in this road*; the conclusions so obtained are persuasive†; far more persuasive than inductions made with due caution‡. But still this method must be rejected if we would obtain true knowledge. We shall then at length have ground of good hope for science when we proceed in another manner§. We must rise, not by a leap, but by small steps, by successive advances, by a gradation of ascents, trying our facts, and clearing our notions at every interval. The scheme of true philosophy, according to Bacon, is not obvious and simple, but long and technical, requiring constant care and self-denial to follow it. And we have seen that, in this opinion, his judgment is confirmed by the past history and present condition of science.

Again; it is by no means a just view of Bacon's character to place him in contrast to Plato. Plato's philosophy was the philosophy of Ideas; but it was not left for Bacon to set up the philosophy of Facts in opposition to that of Ideas. That had been done fully by the speculative reformers of the sixteenth century. Bacon had the merit of showing that Facts and Ideas must be combined; and not only so, but of divining many of the special rules and forms of this combination, when as yet there were no examples of them, with a sagacity hitherto quite unparalleled.

22. (IX.) With Bacon's unhappy political life we have here nothing to do. But we cannot but notice with pleasure how faithfully, how perseveringly, how energetically he discharged his great philosophical office of a Reformer of Methods. He had conceived the pur-

* *Nov. Org.*, Lib. I. Aph. 20. † Aph. 27. ‡ *Ib.*, 28.

§ Aph. 104. So Aph. 105. "In constituendo axiomate forma *inductionis* alia quam adhuc in usu fuit excogitanda est," &c.

pose of making this his object at an early period. When meditating the continuation of his *Novum Organon*, and speaking of his reasons for trusting that his work will reach some completeness of effect, he says*, "I am by two arguments thus persuaded. First, I think thus from the zeal and constancy of my mind, which has not waxed old in this design, nor, after so many years, grown cold and indifferent; I remember that about forty years ago I composed a juvenile work about these things, which with great contrivance and a pompous title I called *temporis partum maximum*, or the most considerable birth of time; Next, that on account of its usefulness, it may hope the Divine blessing." In stating the grounds of hope for future progress in the sciences, he says†: "Some hope may, we conceive, be ministered to men by our own example: and this we say, not for the sake of boasting, but because it is useful to be said. If any despond, let them look at me, a man among all others of my age most occupied with civil affairs, nor of very sound health, (which brings a great loss of time;) also in this attempt the first explorer, following the footsteps of no man, nor communicating on these subjects with any mortal; yet, having steadily entered upon the true road and made my mind submit to things themselves, one who has, in this undertaking, made, (as we think,) some progress." He then proceeds to speak of what may be done by the combined and more prosperous labours of others, in that strain of noble hope and confidence, which rises again and again, like a chorus, at intervals in every part of his writings. In the *Advancement of Learning* he had said, "I could not be true and constant to the argument I handle, if I were not willing to go beyond others, but yet not more willing than to have others go beyond me again." In the Preface to the

* *Ep. ad P. Fulgentium. Op.*, x. 330. † *Nov. Org.*, I. Aph. 113.

Instauratio Magna, he had placed among his postulates those expressions which have more than once warmed the breast of a philosophical reformer*. "Concerning ourselves we speak not; but as touching the matter which we have in hand, this we ask;—that men be of good hope, neither feign and imagine to themselves this our Reform as something of infinite dimension and beyond the grasp of mortal man, when in truth it is the end and true limit of infinite errour; and is by no means unmindful of the condition of mortality and humanity, not confiding that such a thing can be carried to its perfect close in the space of a single age, but assigning it as a task to a succession of generations." In a later portion of the *Instauratio* he says: "We bear the strongest love to the *human republic*, our common country; and we by no means abandon the hope that there will arise and come forth some man among posterity, who will be able to receive and digest all that is best in what we deliver; and whose care it will be to cultivate and perfect such things. Therefore, by the blessing of the Deity, to tend to this object, to open up the fountains, to discover the useful, to gather guidance for the way, shall be our task; and from this we shall never, while we remain in life, desist."

23. (X.) We may add, that the spirit of piety as well as of hope which is seen in this passage, appears to have been habitual to Bacon at all periods of his life. We find in his works several drafts of portions of his great scheme, and several of them begin with a prayer. One of these entitled, in the edition of his works, "The Student's Prayer," appears to me to belong probably to his early youth. Another, entitled "The Writer's Prayer," is inserted at the end of the Preface of the *Instauratio*, as it was finally published. I will conclude my notice of this wonderful man by inserting here these two prayers.

* See the motto to Kant's *Kritik der Reinen Vernunft*.

"To God the Father, God the Word, God the Spirit, we pour forth most humble and hearty supplications; that he, remembering the calamities of mankind, and the pilgrimage of this our life, in which we wear out days few and evil, would please to open to us new refreshments out of the fountains of his goodness for the alleviating of our miseries. This also we humbly and earnestly beg, that human things may not prejudice such as are divine; neither that, from the unlocking of the gates of sense, and the kindling of a greater natural light, anything of incredulity, or intellectual night, may arise in our minds towards divine mysteries. But rather, that by our mind thoroughly cleansed and purged from fancy and vanities, and yet subject and perfectly given up to the Divine oracles, there may be given unto faith the things that are faith's."

"Thou, O Father, who gavest the visible light as the first-born of thy creatures, and didst pour into man the intellectual light as the top and consummation of thy workmanship, be pleased to protect and govern this work, which coming from thy goodness, returneth to thy glory. Thou, after thou hadst reviewed the works which thy hands had made, beheldest that everything was very good, and thou didst rest with complacency in them. But man, reflecting on the works which he had made, saw that all was vanity and vexation of spirit, and could by no means acquiesce in them. Wherefore, if we labour in thy works with the sweat of our brows, thou wilt make us partakers of thy vision and thy Sabbath. We humbly beg that this mind may be steadfastly in us; and that thou, by our hands, and also by the hands of others on whom thou shalt bestow the same spirit, wilt please to convey a largess of new alms to thy family of mankind. These things we commend to thy everlasting love, by our Jesus, thy Christ, God with us. Amen."

CHAPTER XII.

FROM BACON TO NEWTON.

I. *Harvey.*—WE have already seen that Bacon was by no means the first mover or principal author of the revolution in the method of philosophizing which took place in his time; but only the writer who proclaimed in the most impressive and comprehensive manner, the scheme, the profit, the dignity, and the prospects of the new philosophy. Those, therefore, who after him, took up the same views are not to be considered as his successors, but as his fellow-labourers; and the line of historical succession of opinions must be pursued without special reference to any one leading character, as the principal figure of the epoch. I resume this line, by noticing a contemporary and fellow-countryman of Bacon, Harvey, the discoverer of the circulation of the blood. This discovery was not published and generally accepted till near the end of Bacon's life; but the anatomist's reflections on the method of pursuing science, though strongly marked with the character of the revolution that was taking place, belong to a very different school from the Chancellor's. Harvey was a pupil of Fabricius of Acquapendente, whom we noticed among the practical reformers of the sixteenth century. He entertained, like his master, a strong reverence for the great names which had ruled in philosophy up to that time, Aristotle and Galen; and was disposed rather to recommend his own method by exhibiting it as the true interpretation of ancient wisdom, than to boast of its novelty. It is true, that he assigns, as his reason for publishing some of his researches*, "that by revealing the method I use in searching into things, I might pro-

* *Anatomical Exercitations concerning the Generation of Living Creatures*, 1653. Preface.

pose to studious men, a new and (if I mistake not) a surer path to the attainment of knowledge*;" but he soon proceeds to fortify himself with the authority of Aristotle. In doing this, however, he has the very great merit of giving a living and practical character to truths which exist in the Aristotelian works, but which had hitherto been barren and empty professions. We have seen that Aristotle had asserted the importance of experience as one root of knowledge; and in this had been followed by the schoolmen of the middle ages: but this assertion came with very different force and effect from a man, the whole of whose life had been spent in obtaining, by means of experience, knowledge which no man had possessed before. In Harvey's general reflections, the necessity of both the elements of knowledge, sensations and ideas, experience and reason, is fully brought into view, and rightly connected with the metaphysics of Aristotle. He puts the antithesis of these two elements with great clearness. "Universals are chiefly

* He used similar expressions in conversation. George Ent, who edited his *Generation of Animals*, visited him, "at that time residing not far from the city; and found him very intent upon the perscrutation of nature's works, and with a countenance as cheerful, as mind imperturbed; Democritus like, chiefly searching into the cause of natural things." In the course of conversation the writer said, "It hath always been your choice about the secrets of Nature, to consult Nature herself." "'Tis true," replied he; "and I have constantly been of opinion that from thence we might acquire not only the knowledge of those less considerable secrets of Nature, but even a certain admiration of that Supreme Essence, the Creator. And though I have ever been ready to acknowledge, that many things have been discovered by learned men of former times; yet do I still believe that the number of those which remain yet concealed in the darkness of impervestigable Nature is much greater. Nay, I cannot forbear to wonder, and sometimes smile at those, who persuade themselves, that all things were so consummately and absolutely delivered by Aristotle, Galen, or some other great name, as that nothing was left to the superaddition of any that succeeded."

known to us, for science is begot by reasoning from universals to particulars; yet that very comprehension of universals in the understanding springs from the perception of singulars in our sense." Again, he quotes Aristotle's apparently opposite assertions:—that made in his *Physics**, "that we must advance from things which are first known to us, though confusedly, to things more distinctly intelligible in themselves; from the whole to the part; from the universal to the particular;" and that made in the *Analytics*†; that "Singulars are more known to us and do first exist according to sense: for nothing is in the understanding which was not before in the sense." Both, he says, are true, though at first they seem to clash: for "though in knowledge we begin with sense, sensation itself is a universal thing." This he further illustrates; and quotes Seneca, who says, that "Art itself is nothing but the *reason* of the work, implanted in the Artist's mind:" and adds, "the same way by which we gain an Art, by the very same way we attain any kind of science or knowledge whatever; for as Art is a habit whose object is something to be done, so Science is a habit whose object is something to be known; and as the former proceedeth from the imitation of examples, so this latter, from the knowledge of things natural. The source of both is from sense and experience; since [but] it is impossible that Art should be rightly purchased by the one or Science by the other without a direction from ideas." Without here dwelling on the relation of Art and Science, (very justly stated by Harvey, except that ideas exist in a very different form in the mind of the Artist and the Scientist) it will be seen that this doctrine, of science springing from experience with a direction from ideas, is exactly that which we have repeatedly urged, as the true view of the

* Lib. I. c. 2, 3. † *Anal. Post.*, II.

subject. From this view, Harvey proceeds to infer the importance of a reference to sense in his own subject, not only for first discovering, but for receiving knowledge: "Without experience, not other men's but our own, no man is a proper disciple of any part of natural knowledge; without experimental skill in anatomy, he will no better apprehend what I shall deliver concerning generation, than a man born blind can judge of the nature and difference of colours, or one born deaf, of sounds." "If we do otherwise, we may get a humid and floating opinion, but never a solid and infallible knowledge: as is happenable to those who see foreign countries only in maps, and the bowels of men falsely described in anatomical tables. And hence it comes about, that in this rank age, we have many sophisters and bookwrights, but few wise men and philosophers." He had before declared "how unsafe and degenerate a thing it is, to be tutored by other men's commentaries, without making trial of the things themselves; especially since Nature's book is so open and legible." We are here reminded of Galileo's condemnation of the "paper philosophers." The train of thought thus expressed by the practical discoverers, spread rapidly with the spread of the new knowledge that had suggested it, and soon became general and unquestioned.

II. *Descartes.*—Such opinions are now among the most familiar and popular of those which are current among writers and speakers; but we should err much if we were to imagine that after they were once propounded they were never resisted or contradicted. Indeed, even in our own time, not only are such maxims very frequently practically neglected or forgotten, but the opposite opinions, and views of science quite inconsistent with those we have been explaining, are often promulgated and widely accepted. The philosophy of pure ideas has its commonplaces, as well as the philosophy of experience. And at the time of which we speak, the former philosophy, no less than the latter, had its great asserter and expounder; a man in his own time more admired than Bacon, regarded with more deference by a large body of disciples all over Europe, and more powerful in stirring up men's minds to a new activity of inquiry. I speak of Descartes, whose labours, considered as a philosophical system, were an endeavour to revive the method of obtaining knowledge by reasoning from our own ideas only, and to erect it in opposition to the method of observation and experiment. The Cartesian philosophy contained an attempt at a counter-revolution. Thus in this author's *Principia Philosophiæ* *, he says that "he will give a short account of the principal phenomena of the world, not that he may use them as reasons to prove anything; for," adds he, "we desire to deduce effects from causes, not causes from effects; but only in order that out of the innumerable effects which we learn to be capable of resulting from the same causes, we may determine our mind to consider some rather than others." He had before said, "The principles which we have obtained [by pure *à priori* reasoning] are so vast and so fruitful, that many more consequences follow from them than we see contained in this visible world, and even many more than our mind can ever take a full survey of." And he professes to apply this method in detail. Thus in attempting to state the three fundamental laws of motion, he employs only *à priori* reasonings, and is in fact led into errour in the third law which he thus obtains †. And in his *Dioptrics* ‡ he pretends to deduce the laws of reflection and refraction of light from certain comparisons (which are, in truth, arbitrary,) in which the radiation of light is represented by

* Pars III. p. 45. † See *Hist. Ind. Sci.*, B. VI. c. ii. ‡ Cap. I. II.

the motion of a ball impinging upon the reflecting or refracting body. It might be represented as a curious instance of the caprice of fortune, which appears in scientific as in other history, that Kepler, professing to derive all his knowledge from experience, and exerting himself with the greatest energy and perseverance, failed in detecting the law of refraction; while Descartes, who professed to be able to despise experiment, obtained the true law of sines. But as we have stated in the *History**, Descartes appears to have learnt this law from Snell's papers. And whether this be so or not, it is certain that notwithstanding the profession of independence which his philosophy made, it was in reality constantly guided and instructed by experience. Thus in explaining the Rainbow (in which his portion of the discovery merits great praise) he speaks† of taking a globe of glass, allowing the sun to shine on one side of it, and noting the colours produced by rays after two refractions and one reflection. And in many other instances, indeed in all that relates to physics, the reasonings and explanations of Descartes and his followers were, consciously or unconsciously, directed by the known facts, which they had observed themselves or learnt from others.

But since Descartes thus, speculatively at least, set himself in opposition to the great reform of scientific method which was going on in his time, how, it may be asked, did he acquire so strong an influence over the most active minds of his time? How is it that he became the founder of a large and distinguished school of philosophers? How is it that he not only was mainly instrumental in deposing Aristotle from his intellectual throne, but for a time appeared to have established himself with almost equal powers, and to have rendered the

* *Hist. Ind. Sci.*, B. IX. c. ii. † *Meteorum*, c. viii. p. 187.

Cartesian school as firm a body as the Peripatetic had been?

The causes to be assigned for this remarkable result are, I conceive, the following. In the first place, the physicists of the Cartesian school did, as I have just stated, found their philosophy upon experiment; and did not practically, nor indeed, most of them, theoretically, assent to their master's boast of showing what the phenomena *must be*, instead of looking to see what they *are*. And as Descartes had really incorporated in his philosophy all the chief physical discoveries of his own and preceding times, and had delivered, in a more general and systematic shape than any one before him, the principles which he thus established, the physical philosophy of his school was in reality far the best then current; and was an immense improvement upon the Aristotelian doctrines, which had not yet been displaced as a system. Another circumstance which gained him much favour, was the bold and ostentatious manner in which he professed to begin his philosophy by liberating himself from all preconceived prejudice. The first sentence of his philosophy contains this celebrated declaration: "Since," he says, "we begin life as infants, and have contracted various judgments concerning sensible things before we possess the entire use of our reason, we are turned aside from the knowledge of truth by many prejudices: from which it does not appear that we can be any otherwise delivered, than if once in our life we make it our business to doubt of everything in which we discern the smallest suspicion of uncertainty." In the face of this sweeping rejection or unhesitating scrutiny of all preconceived opinions, the power of the ancient authorities and masters in philosophy must obviously shrink away; and thus Descartes came to be considered as the great hero of the overthrow of the Aristotelian dogmatism. But in addition to these causes, and perhaps more powerful than all, in procuring the assent of men to his doctrines, came the deductive and systematic character of his philosophy. For although all knowledge of the external world is in reality only to be obtained from observation, by inductive steps,—minute, perhaps, and slow, and many, as Galileo and Bacon had already taught;—the human mind conforms to these conditions reluctantly and unsteadily, and is ever ready to rush to general principles, and then to employ itself in deducing conclusions from these by synthetical reasonings; a task grateful, from the distinctness and certainty of the result, and the accompanying feeling of our own sufficiency. Hence men readily overlooked the precarious character of Descartes' fundamental assumptions, in their admiration of the skill with which a varied and complex Universe was evolved out of them. And the complete and systematic character of this philosophy attracted men no less than its logical connexion. I may quote here what a philosopher* of our own time has said of another writer: "He owed his influence to various causes; at the head of which may be placed that genius for system which, though it cramps the growth of knowledge, perhaps finally atones for that mischief by the zeal and activity which it rouses among followers and opponents, who discover truth by accident when in pursuit of weapons for their warfare. A system which attempts a task so hard as that of subjecting vast provinces of human knowledge to one or two principles, if it presents some striking instances of conformity to superficial appearances, is sure to delight the framer; and for a time to subdue and captivate the student too entirely for sober reflection and rigorous examination. In the first instance consistency passes for truth. When principles in

* Mackintosh, *Dissertation on Ethical Science.*

some instances have proved sufficient to give an unexpected explanation of facts, the delighted reader is content to accept as true all other deductions from the principles. Specious premises being assumed to be true, nothing more can be required than logical inference. Mathematical forms pass current as the equivalent of mathematical certainty. The unwary admirer is satisfied with the completeness and symmetry of the plan of his house, unmindful of the need of examining the firmness of the foundation and the soundness of the materials. The system-maker, like the conqueror, long dazzles and overawes the world; but when their sway is past, the vulgar herd, unable to measure their astonishing faculties, take revenge by trampling on fallen greatness." Bacon had showed his wisdom in his reflections on this subject, when he said that "Method, carrying a show of total and perfect knowledge, hath a tendency to generate acquiescence."

The main value of Descartes' physical doctrines consisted in their being arrived at in a way inconsistent with his own professed method, namely, by a reference to observation. But though he did in reality begin from facts, his system was nevertheless a glaring example of that errour which Bacon had called *Anticipation*; that illicit generalization which leaps at once from special facts to principles of the widest and remotest kind; such, for instance, as the Cartesian doctrine, that the world is an absolute *plenum*, every part being full of matter of some kind, and that all natural effects depend on the laws of motion. Against this fault, to which the human mind is so prone, Bacon had lifted his warning voice in vain, so far as the Cartesians were concerned; as indeed, to this day, one theorist after another pursues his course, and turns a deaf ear to the Verulamian injunctions; perhaps even complacently boasts that he founds his theory upon observation; and forgets that there are, as the aphorism of the *Novum Organon* declares, two ways by which this may be done;—the one hitherto in use and suggested by our common tendencies, but barren and worthless; the other almost untried, to be pursued only with effort and self-denial, but alone capable of producing true knowledge.

III. *Gassendi.*—Thus the lessons which Bacon taught were far from being generally accepted and applied at first. The amount of the influence of these two men, Bacon and Descartes, upon their age, has often been a subject of discussion. The fortunes of the Cartesian school have been in some measure traced in the History of Science. But I may mention the notice taken of these two philosophers by Gassendi, a contemporary and countryman of Descartes. Gassendi, as I have elsewhere stated*, was associated with Descartes in public opinion, as an opponent of the Aristotelian dogmatism; but was not in fact a follower or profound admirer of that writer. In a Treatise on Logic, Gassendi gives an account of the Logic of various sects and authors; treating, in order, of the Logic of Zeno (the Eleatic), of Euclid (the Megarean), of Plato, of Aristotle, of the Stoics, of Epicurus, of Lullius, of Ramus; and to these he adds the Logic of Verulam, and the Logic of Cartesius. "We must not," he says, "on account of the celebrity it has obtained, pass over the Organon or Logic of Francis Bacon Lord Verulam, High Chancellor of England, whose noble purpose in our time it has been, to make an Instauration of the Sciences." He then gives a brief account of the *Novum Organon*, noticing the principal features in its rules, and especially the distinction between the vulgar induction which leaps at once from particular experiments to the more general axioms, and the chastised and gradual in-

* *Hist. Ind. Sci.*, B. VII. c. i.

duction, which the author of the *Organon* recommends. In his account of the Cartesian Logic, he justly observes, that "He too imitated Verulam in this, that being about to build up a new philosophy from the foundation, he wished in the first place to lay aside all prejudice: and having then found some solid principle, to make that the ground-work of his whole structure. But he proceeds by a very different path from that which Verulam follows; for while Verulam seeks aid from things, to perfect the cogitation of the intellect, Cartesius conceives, that when we have laid aside all knowledge of things, there is, in our thoughts alone, such a resource, that the intellect may by its own power arrive at a perfect knowledge of all, even the most abstruse things."

The writings of Descartes have been most admired, and his method most commended, by those authors who have employed themselves upon metaphysical rather than physical subjects of inquiry. Perhaps we might say that, in reference to such subjects, this method is not so vicious as at first, when contrasted with the Baconian induction, it seems to be: for it might be urged that the *thoughts* from which Descartes begins his reasonings are, in reality *experiments* of the kind which the subject requires us to consider: each such thought is a fact in the intellectual world; and of such facts, the metaphysician seeks to discover the laws. I shall not here examine the validity of this plea; but shall turn to the consideration of the actual progress of physical science and its effect on men's minds.

IV. *Actual progress in Science.*—The practical discoverers were indeed very active and very successful during the seventeenth century which opened with Bacon's survey and exhortations. The laws of nature, of which men had begun to obtain a glimpse in the preceding century, were investigated with zeal and sagacity, and the consequence was that the foundations of most of the modern physical sciences were laid. That mode of research by experiment and observation, which had, a little time ago, been a strange, and to many, an unwelcome innovation, was now become the habitual course of philosophers. The revolution from the philosophy of tradition to the philosophy of experience was completed. The great discoveries of Kepler belonged to the preceding century. They are not, I believe, noticed, either by Bacon or by Descartes; but they gave a strong impulse to astronomical and mechanical speculators, by showing the necessity of a sound science of motion. Such a science Galileo had already begun to construct. At the time of which I speak, his disciples* were still labouring at this task, and at other problems which rapidly suggested themselves. They had already convinced themselves that air had weight; in 1643 Torricelli proved this practically by the invention of the Barometer; in 1647, Pascal proved it still further by sending the Barometer to the top of a mountain. Pascal and Boyle brought into clear view the fundamental laws of fluid equilibrium; Boyle and Mariotte determined the law of the compression of air as regulated by its elasticity. Otto Guericke invented the air pump, and by his "Magdeburg Experiments" on a vacuum, illustrated still further the effects of the air. Guericke pursued what Gilbert had begun, the observation of electrical phenomena; and these two physicists made an important step, by detecting repulsion as well as attraction in these phenomena. Gilbert had already laid the foundations of the science of Magnetism. The law of refraction, at which Kepler had laboured in vain, was, as we have seen, discovered by Snell (about 1621), and published by Descartes.

* Castelli, Torricelli, Viviani, Baliani, Gassendi, Mersenne, Borelli, Cavalleri.

Mersenne had discovered some of the more important parts of the theory of Harmonics. In sciences of a different kind, the same movement was visible. Chemical doctrines tended to assume a proper degree of generality, when Sylvius in 1679 taught the opposition of acid and alkali, and Stahl, soon after, the phlogistic theory of combustion. Steno had remarked the most important law of crystallography in 1669, that the angles of the same kind of crystals are always equal. In the sciences of classification, about 1680, Ray and Morison in England resumed the attempt to form a systematic botany, which had been interrupted for a hundred years, from the time of the memorable essay of Cæsalpinus. The grand discovery of the circulation of the blood by Harvey about 1619, was followed in 1651 by Pecquet's discovery of the course of the chyle. There could now no longer be any question whether science was progressive, or whether observation could lead to new truths.

Among these cultivators of science, such sentiments as have been already quoted became very familiar;—that knowledge is to be sought from nature herself by observation and experiment;—that in such matters tradition is of no force when opposed to experience, and that mere reasonings without facts cannot lead to solid knowledge. But I do not know that we find in these writers any more special rules of induction and scientific research which have since been confirmed and universally adopted. Perhaps too, as was natural in so great a revolution, the writers of this time, especially the second-rate ones, were somewhat too prone to disparage the labours and talents of Aristotle and the ancients in general, and to overlook the ideal element of our knowledge, in their zealous study of phenomena. They urged, sometimes in an exaggerated manner, the superiority of modern times in all that regards science, and the supreme and sole importance of facts in scientific investigations. There prevailed among them also a lofty and dignified tone of speaking of the condition and prospects of science, such as we are accustomed to admire in the Verulamian writings; for this, in a less degree, is epidemic among those who a little after his time speak of the new philosophy.

V. *Otto Guericke, &c.*—I need not illustrate these characteristics at any great length. I may as an example notice Otto Guericke's Preface to his *Experimenta Magdeburgica* (1670). He quotes a passage from Kircher's Treatise on the Magnetic Art, in which the author says, "Hence it appears how all philosophy, except it be supported by experiments, is empty, fallacious, and useless; what monstrosities philosophers, in other respects of the highest and subtlest genius, may produce in philosophy by neglecting experiment. Thus Experience alone is the Dissolver of Doubts, the Reconciler of Difficulties, the sole Mistress of Truth, who holds a torch before us in obscurity, unties our knots, teaches us the true causes of things." Guericke himself reiterates the same remark, adding that "philosophers, insisting upon their own thoughts and arguments merely, cannot come to any sound conclusion respecting the natural constitution of the world." Nor were the Cartesians slow in taking up the same train of reflection. Thus Gilbert Clark who, in 1660, published* a defence of Descartes' doctrine of a *plenum* in the universe, speaks in a tone which reminds us of Bacon, and indeed was very probably caught from him. "Natural philosophy formerly consisted entirely of loose and most doubtful controversies, carried on in high sounding words, fit rather to delude than to instruct men. But at last (by the favour of the Deity) there shone forth some more divine intellects, who taking as their counsel-

* *De Plenitudine Mundi, in qua defenditur Cartesiana Philosophia contra sententias Francisci Baconi, Th. Hobbii et Sethi Wardi.*

lors reason and experience together, exhibited a new method of philosophizing. Hence has been conceived a strong hope that philosophers may embrace, not a shadow or empty image of Truth, but Truth herself: and that Physiology (Physics) scattering these controversies to the winds, will contract an alliance with Mathematics. Yet this is hardly the work of one age; still less of one man. Yet let not the mind despond, or doubt not that, one party of investigators after another following the same method of philosophizing, at last, under good auguries, the mysteries of nature being daily unlocked as far as human feebleness will allow, Truth may at last appear in full, and these nuptial torches may be lighted."

As another instance of the same kind, I may quote the Preface to the First volume of the Transactions of the Academy of Sciences at Paris. "It is only since the present century," says the writer, "that we can reckon the revival of Mathematics and Physics. M. Descartes and other great men have laboured at this work with so much success, that in this department of literature, the whole face of things has been changed. Men have quitted a sterile system of physics, which for several generations had been always at the same point; the reign of words and terms is passed; men will have things; they establish principles which they understand, they follow those principles; and thus they make progress. Authority has ceased to have more weight than Reason: that which was received without contradiction because it had been long received, is now examined, and often rejected: and philosophers have made it their business to consult, respecting natural things, Nature herself rather than the Ancients." These had now become the commonplaces of those who spoke concerning the course and method of the Sciences.

VI. *Hooke.*—In England, as might be expected, the influence of Francis Bacon was more directly visible. We find many writers, about this time, repeating the truths which Bacon had proclaimed, and in almost every case showing the same imperfections in their views which we have noticed in him. We may take as an example of this Hooke's Essay, entitled "A General Scheme or Idea of the present state of Natural Philosophy, and how its defects may be remedied by a Methodical proceeding in the making Experiments and collecting Observations; whereby to compile a Natural History as a solid basis for the superstructure of true Philosophy." This Essay may be looked upon as an attempt to adapt the *Novum Organon* to the age which succeeded its publication. We have in this imitation, as in the original, an enumeration of various mistakes and impediments which had in preceding times prevented the progress of knowledge; exhortations to experiment and observation as the only solid basis of Science; very ingenious suggestions of trains of inquiry, and modes of pursuing them; and a promise of obtaining scientific truths when facts have been duly accumulated. This last part of his scheme the author calls *a Philosophical Algebra;* and he appears to have imagined that it might answer the purpose of finding unknown causes from known facts, by means of certain regular processes, in the same manner as Common Algebra finds unknown from known quantities. But this part of the plan appears to have remained unexecuted. The suggestion of such a method was a result of the Baconian notion that invention in a discoverer might be dispensed with. We find Hooke adopting the phrases in which this notion is implied: thus he speaks of the understanding as "being very prone to run into the affirmative way of judging, and wanting patience to follow and prosecute the negative way of inquiry, by rejection of disagreeing natures." And he follows Bacon also in the errour of attempting at once to obtain from the facts the discovery of a "nature," instead of investigating first the measures and the laws of phenomena. I return to more general notices of the course of men's thoughts on this subject.

VII. *Royal Society.*—Those who associated themselves together for the prosecution of science quoted Bacon as their leader, and exulted in the progress made by the philosophy which proceeded upon his principles. Thus in Oldenburg's Dedication of the Transactions of the Royal Society of London for 1670, to Robert Boyle, he says; "I am informed by such as well remember the best and worst days of the famous Lord Bacon, that though he wrote his *Advancement of Learning* and his *Instauratio Magna* in the time of his greatest power, yet his greatest reputation rebounded first from the most intelligent foreigners in many parts of Christendom:" and after speaking of his practical talents and his public employments, he adds, "much more justly still may we wonder how, without any great skill in Chemistry, without much pretence to the Mathematics or Mechanics, without optic aids or other engines of late invention, he should so much transcend the philosophers then living, in judicious and clear instructions, in so many useful observations and discoveries, I think I may say beyond the records of many ages." And in the end of the Preface to the same volume, he speaks with great exultation of the advance of science all over Europe, referring undoubtedly to facts then familiar. "And now let envy snarl, it cannot stop the wheels of active philosophy, in no part of the known world;—not in France, either in Paris or in Caen:—not in Italy, either in Rome, Naples, Milan, Florence, Venice, Bononia or Padua;—in none of the Universities either on this or on that side of the seas, Madrid and Lisbon, all the best spirits in Spain and Portugal, and the spacious and remote dominions to them belonging;—the Imperial Court and the Princes of Germany; the Northern Kings and their best luminaries; and even the frozen Muscovite and Russian have all taken the operative ferment: and it works high and prevails every way, to the encouragement of all sincere lovers of knowledge and virtue."

Again, in the Preface for 1672, he pursues the same thought into detail. "We must grant that in the last age, when operative philosophy began to recover ground, and to tread on the heels of triumphant Philology; emergent adventures and great successes were encountered by dangerous oppositions and strong obstructions. Galilæus and others in Italy suffered extremities for their celestial discoveries; and here in England Sir Walter Raleigh, when he was in his greatest lustre, was notoriously slandered to have erected a school of atheism, because he gave countenance to chemistry, to practical arts, and to curious mechanical operations, and designed to form the best of them into a college. And Queen Elizabeth's Gilbert was a long time esteemed extravagant for his magnetisms; and Harvey for his diligent researches in pursuance of the circulation of the blood. But when our renowned Lord Bacon had demonstrated the methods for a perfect restoration of all parts of real knowledge; and the generous and philosophical Peireskius had, soon after, agitated in all parts to redeem the most instructive antiquities, and to excite experimental essays and fresh discoveries; the success became on a sudden stupendous; and effective philosophy began to sparkle, and even to flow into beams of shining light all over the world."

The formation of the Royal Society of London and of the Academy of Sciences of Paris, from which proceeded the declamations just quoted, were among many indications, belonging to this period, of the importance which states as well as individuals had by this time begun to attach to the cultivation of science. The English Society was established almost immediately when the restoration of the monarchy appeared to give a promise of tranquillity to the nation (in 1660), and the French Academy very soon afterwards (in 1666). These measures were very soon followed by the establishment of the Observatories of Paris and Greenwich (in 1667 and 1675); which may be considered to be a kind of public recognition of the astronomy of observation, as an object on which it was the advantage and the duty of nations to bestow their wealth.

VIII. *Bacon's New Atalantis.*—When philosophers had their attention turned to the boundless prospect of increase to the knowledge and powers and pleasures of man which the cultivation of experimental philosophy seemed to promise, it was natural that they should think of devising institutions and associations by which such benefits might be secured. Bacon had drawn a picture of a society organized with a view to such purpose, in his fiction of the "New Atalantis." The imaginary teacher who explains this institution to the inquiring traveller, describes it by the name of *Solomon's House;* and says*, "The end of our foundation is the knowledge of causes and secret motions of things; and the enlarging the bounds of the human empire to effecting of things possible." And, as parts of this House, he describes caves and wells, chambers and towers, baths and gardens, parks and pools, dispensatories and furnaces, and many other contrivances, provided for the purpose of making experiments of many kinds. He describes also the various employments of the Fellows of this College, who take a share in its researches. There are *merchants of light*, who bring books and inventions from foreign countries;

* Bacon's *Works*, Vol. II. 111.

depredators, who gather the experiments which exist in books; *mystery-men*, who collect the experiments of the mechanical arts; *pioneers* or *miners*, who invent new experiments; and *compilers*, "who draw the experiments of the former into titles and tables, to give the better light for the drawing of observations and axioms out of them." There are also *dowry-men or benefactors*, that cast about how to draw out of the experiments of their fellows things of use and practice for man's life; *lamps*, that direct new experiments of a more penetrating light than the former; *inoculators*, that execute the experiments so directed. Finally, there are the *interpreters of nature*, that raise the former discoveries by experiments into greater observations (that is, more general truths) axioms and aphorisms. Upon this scheme we may remark, that fictitious as it undisguisedly is, it still serves to exhibit very clearly some of the main features of the author's philosophy:—namely, his steady view of the necessity of ascending from facts to the most general truths by several stages;—an exaggerated opinion of the aid that could be derived in such a task from technical separation of the phenomena and a distribution of them into tables;—a belief, probably incorrect, that the offices of experimenter and interpreter may be entirely separated, and pursued by different persons with a certainty of obtaining success;—and a strong determination to make knowledge constantly subservient to the uses of life.

IX. *Cowley.*—Another project of the same kind, less ambitious but apparently more directed to practice, was published a little later (1657) by another eminent man of letters in this country. I speak of Cowley's "Proposition for the Advancement of Experimental Philosophy." He suggests that a College should be established at a short distance from London, endowed with a revenue of four thousand pounds, and consisting of twenty professors with other members. The objects of the labours of these professors he describes to be, first, to examine all knowledge of nature delivered to us from former ages and to pronounce it sound or worthless; second, to recover the lost inventions of the ancients; third, to improve all arts that we now have; lastly, to discover others that we yet have not. In this proposal we cannot help marking the visible declension from Bacon's more philosophical view. For we have here only a very vague indication of improving old arts and discovering new, instead of the two clear Verulamian antitheses, Experiments and Axioms deduced from them, on the one hand, and on the other an ascent to general Laws, and a derivation, from these, of Arts for daily use. Moreover the prominent place which Cowley has assigned to the verifying the knowledge of former ages and recovering "the lost inventions and drowned lands of the ancients," implies a disposition to think too highly of traditionary knowledge; a weakness which Bacon's scheme shows *him* to have fully overcome. And thus it has been up to the present day, that with all Bacon's mistakes, in the philosophy of scientific method few have come up to him, and perhaps none have gone beyond him.

Cowley exerted himself to do justice to the new philosophy in verse as well as prose, and his Poem to the Royal Society expresses in a very noble manner those views of the history and prospects of philosophy which prevailed among the men by whom the Royal Society was founded. The fertility and ingenuity of comparison which charaterize Cowley's poetry are well known; and these qualities are in this instance largely employed for the embellishment of his subject. Many of the comparisons which he exhibits are apt and striking. Philosophy is a ward whose estate (human knowledge) is, in his nonage, kept from him by his guardians and tutors; (a case which the ancient rhetoricians were fond of taking as a subject of declamation;) and these wrong-doers retain him in unjust tutelage and constraint for their own purposes; until

> Bacon at last, a mighty man, arose,
> (Whom a wise King, and Nature, chose
> Lord Chancellor of both their laws,)
> And boldly undertook the injured pupil's cause.

Again, Bacon is one who breaks a scarecrow Priapus which stands in the garden of knowledge. Again, Bacon is one who, instead of a picture of painted grapes, gives us real grapes from which we press "the thirsty soul's refreshing wine." Again, Bacon is like Moses, who led the Hebrews forth from the barren wilderness, and ascended Pisgah;—

> Did on the very border stand
> Of the blest promised land,
> And from the mountain's top of his exalted wit
> Saw it himself and showed us it.

The poet however adds, that Bacon discovered, but did not conquer this new world; and that the men whom he addresses must subdue these regions. These "champions" are then ingeniously compared to Gideon's band:

> Their old and empty pitchers first they brake
> And with their hands then lifted up the light.

There were still at this time some who sneered at or condemned the new philosophy; but the tide of popular opinion was soon strongly in its favour. I have elsewhere* noticed a pasquinade of the poet Boileau in 1682, directed against the Aristotelians. At this time, and indeed for long afterwards, the philosophers of France were Cartesians. The English men of science,

* *Hist. Ind. Sci.*, B. VII. c. i.

although partially and for a time they accepted some of Descartes' opinions, for the most part carried on the reform independently, and in pursuance of their own views. And they very soon found a much greater leader than Descartes to place at their head, and to take as their authority, so far as they acknowledged authority, in their speculations. I speak of Newton, whose influence upon the philosophy of science I must now consider.

Chapter XIII.

NEWTON.

1. Bold and extensive as had been the anticipations of those whose minds were excited by the promise of the new philosophy, the discoveries of Newton respecting the mechanics of the universe, brought into view truths more general and profound than those earlier philosophers had hoped or imagined. With these vast accessions to human knowledge, men's thoughts were again set in action; and philosophers made earnest and various attempts to draw, from these extraordinary advances in science, the true moral with regard to the conduct and limits of the human understanding. They not only endeavoured to verify and illustrate, by these new portions of science, what had recently been taught concerning the methods of obtaining sound knowledge; but they were also led to speculate concerning many new and more interesting questions relating to this subject. They saw, for the first time, or at least far more clearly than before, the distinction between the inquiry into the *laws*, and into the *causes* of phenomena. They were tempted to ask, how far the discovery of causes could be carried;

and whether it would soon reach, or clearly point to, the ultimate cause. They were driven to consider whether the properties which they discovered were essential properties of all matter, necessarily and primarily involved in its essence, though revealed to us at a late period by their derivative effects. These questions even now agitate the thoughts of speculative men. Some of them have already, in this work, been discussed, or arranged in the places which our view of the philosophy of these subjects assigns to them. But we must here notice them as they occurred to Newton himself and his immediate followers.

2. The general Baconian notion of the method of philosophizing, that it consists in ascending from phenomena, through various stages of generalization, to truths of the highest order, received, in Newton's discovery of the universal mutual gravitation of every particle of matter, that pointed actual exemplification, for want of which it had hitherto been almost overlooked, or at least very vaguely understood. That great truth, and the steps by which it was established, afford, even now, by far the best example of the successive ascent, from one scientific truth to another,—of the repeated transition from less to more general propositions,—which we can yet produce; as may be seen in the Table which exhibits the relation of these steps in Book XI. Newton himself did not fail to recognize this feature in the truths which he exhibited. Thus, he says*, "By the way of Analysis we proceed from compounds to ingredients, as from motions to the forces producing them; and in general, from effects to their causes, and from particular causes to more general ones, till the argument end in the most general." And in like manner in another Query†: "The main business of natural philo-

* *Opticks*, Qu. 31, near the end. † Qu. 28.

sophy is to argue from phenomena without feigning hypotheses, and to deduce causes from effects, till we come to the First Cause, which is certainly not mechanical."

3. Newton appears to have had a horrour of the term *hypothesis*, which probably arose from his acquaintance with the rash and illicit general assumptions of Descartes. Thus in the passage just quoted, after declaring that gravity must have some other cause than matter, he says, "Later philosophers banish the consideration of such a cause out of Natural Philosophy, feigning hypotheses for explaining all things mechanically, and referring other causes to metaphysics." In the celebrated Scholium at the end of the *Principia*, he says, "Whatever is not deduced from the phenomena, is to be termed *hypothesis*; and hypotheses, whether metaphysical or physical, or occult causes, or mechanical, have no place in experimental philosophy. In this philosophy, propositions are deduced from phenomena, and rendered general by induction." And in another place, he arrests the course of his own suggestions, saying, "Verum hypotheses non fingo." I have already attempted to show that this is, in reality, a superstitious and self-destructive spirit of speculation. Some hypotheses are necessary, in order to connect the facts which are observed; some new principle of unity must be applied to the phenomena, before induction can be attempted. What is requisite is, that the hypothesis should be close to the facts, and not connected with them by other arbitrary and untried facts; and that the philosopher should be ready to resign it as soon as the facts refuse to confirm it. We have seen in the *History*, that it was by such a use of hypotheses, that both Newton himself, and Kepler, on whose discoveries those of Newton were based, made their discoveries. The suppositions of a force tending to the sun and varying inversely as the square of the

distance; of a mutual force between all the bodies of the solar system; of the force of each body arising from the attraction of all its parts; not to mention others, also propounded by Newton,—were all hypotheses before they were verified as theories. It is related that when Newton was asked how it was that he saw into the laws of nature so much further than other men, he replied, that if it were so, it resulted from his keeping his thoughts steadily occupied upon the subject which was to be thus penetrated. But what is this occupation of the thoughts, if it be not the process of keeping the phenomena clearly in view, and trying, one after another, all the plausible hypotheses which seem likely to connect them, till at last the true law is discovered? Hypotheses so used are a necessary element of discovery.

4. With regard to the details of the process of discovery, Newton has given us some of his views, which are well worthy of notice, on account of their coming from him; and which are real additions to the philosophy of this subject. He speaks repeatedly of the *analysis* and *synthesis* of observed facts; and thus marks certain steps in scientific research, very important, and not, I think, clearly pointed out by his predecessors. Thus he says*, "As in Mathematics, so in Natural Philosophy, the investigation of difficult things by the method of analysis ought ever to precede the method of composition. This analysis consists in making experiments and observations, and in drawing general conclusions from them by induction, and admitting of no objections against the conclusions, but such as are taken from experiments or other certain truths. And although the arguing from experiments and observations by induction be no demonstration of general conclusions; yet it is the best way of arguing which the nature of things

* *Opticks*, Qu. 31.

admits of, and may be looked upon as so much the stronger, by how much the induction is more general." And he then observes, as we have quoted above, that by this way of analysis we proceed from compounds to ingredients, from motions to forces, from effects to causes, and from less to more general causes. The *analysis* here spoken of includes the steps which in this work we call the *decomposition* of facts, the exact *observation* and *measurement* of the phenomena, and the *colligation* of facts; the necessary intermediate step, the *selection* and *explication* of the appropriate conception, being passed over, in the fear of seeming to encourage the fabrication of hypotheses. The *synthesis* of which Newton here speaks consists of those steps of *deductive reasoning*, proceeding from the conception once assumed, which are requisite for the comparison of its consequences with the observed facts. This statement of the process of research, is, as far as it goes, perfectly exact.

5. In speaking of Newton's precepts on the subject, we are naturally led to the celebrated "Rules of Philosophizing," inserted in the second edition of the *Principia*. These rules have generally been quoted and commented on with an almost unquestioning reverence. Such Rules, coming from such an authority, cannot fail to be highly interesting to us; but at the same time, we cannot here evade the necessity of scrutinizing their truth and value, according to the principles which our survey of this subject has brought into view. The Rules stand at the beginning of that part of the *Principia* (the Third Book) in which he infers the mutual gravitation of the sun, moon, planets, and all parts of each. They are as follows:

"Rule I. We are not to admit other causes of natural things than such as both are true, and suffice for explaining their phenomena.

"Rule II. Natural effects of the same kind are to be referred to the same causes, as far as can be done.

"Rule III. The qualities of bodies which cannot be increased or diminished in intensity, and which belong to all bodies in which we can institute experiments, are to be held for qualities of all bodies whatever.

"Rule IV. In experimental philosophy, propositions collected from phenomena by induction, are to be held as true either accurately or approximately, notwithstanding contrary hypotheses; till other phenomena occur by which they may be rendered either more accurate or liable to exception."

In considering these Rules, we cannot help remarking, in the first place, that they are constructed with an intentional adaptation to the case with which Newton has to deal,—the induction of Universal Gravitation; and are intended to protect the reasonings before which they stand. Thus the first Rule is designed to strengthen the inference of gravitation from the celestial phenomena, by describing it as a *vera causa*, a true cause; the second countenances the doctrine that the planetary motions are governed by mechanical forces, as terrestrial motions are; the third rule appears intended to justify the assertion of gravitation, as a *universal* quality of bodies; and the fourth contains, along with a general declaration of the authority of induction, the author's usual protest against hypotheses, levelled at the Cartesian hypotheses especially.

6. *Of the First Rule.*—We, however, must consider these Rules in their general application, in which point of view they have often been referred to, and have had very great authority allowed them. One of the points which has been most discussed, is that maxim which requires that the causes of phenomena which we assign should be true causes, *veræ causæ*. Of course this does

not mean that they should be *the* true or right cause; for although it is the philosopher's aim to discover such causes, he would be little aided in his search of truth, by being told that it is truth which he is to seek. The rule has gènerally been understood to prescribe that in attempting to account for any class of phenomena, we must assume such causes only, as *from other considerations*, we know to exist. Thus gravity, which was employed in explaining the motions of the moon and planets, was already known to exist and operate at the earth's surface.

Now the Rule thus interpreted is, I conceive, an injurious limitation of the field of induction. For it forbids us to look for a cause, except among the causes with which we are already familiar. But if we follow this rule, how shall we ever become acquainted with any new cause? Or how do we know that the phenomena which we contemplate do really arise from some cause which we already truly know? If they do not, must we still insist upon making them depend upon some of our known causes; or must we abandon the study of them altogether? Must we, for example, resolve to refer the action of radiant heat to the air, rather than to any peculiar fluid or ether, because the former is known to exist, the latter is merely assumed for the purpose of explanation? But why should we do this? Why should we not endeavour to learn the cause from the effects, even if it be not already known to us? We can infer causes, which are new when we first become acquainted with them. Chemical Forces, Optical Forces, Vital Forces, are known to us only by chemical and optical and vital phenomena; must we, therefore, reject their existence or abandon their study? They do not conform to the double condition, that they shall be sufficient and *also* real: they are true, only so far as they explain the facts, but are they, therefore, unintelligible or useless? Are they not highly important and instructive subjects of speculation? And if the gravitation which rules the motions of the planets had not existed at the earth's surface;—if it had been there masked and concealed by the superior effect of magnetism, or some other extraneous force, might not Newton still have inferred, from Kepler's laws, the tendency of the planets to the sun; and from their perturbations, their tendency to each other? His discoveries would still have been immense, if the cause which he assigned had not been a *vera causa* in the sense now contemplated.

7. But what do we mean by calling gravity a "true cause?" How do we learn its reality? Of course, by its effects, with which we are familiar;—by the weight and fall of bodies about us. These strike even the most careless observer. No one can fail to see that all bodies which we come in contact with are heavy;—that gravity acts in our neighbourhood here upon earth. Hence, it may be said, this cause is at any rate a true cause, whether it explains the celestial phenomena or not.

But if this be what is meant by a *vera causa*, it appears strange to require that in all cases we should find such a one to account for all classes of phenomena. Is it reasonable or prudent to demand that we shall reduce every set of phenomena, however minute, or abstruse, or complicated, to causes so obviously existing as to strike the most incurious, and to be familiar among men? How can we expect to find *such veræ causæ* for the delicate and recondite phenomena which an exact and skilful observer detects in chemical, or optical, or electrical experiments? The facts themselves are too fine for vulgar apprehension; their relations, their symmetries, their measures require a previous discipline to understand them. How then can their causes be found among those agencies with which the common unscientific herd of mankind are familiar? What likelihood is there that causes held for real by such persons, shall explain facts which such persons cannot see or cannot understand?

Again: if we give authority to such a rule, and require that the causes by which science explains the facts which she notes and measures and analyzes, shall be causes which men, without any special study, have already come to believe in, from the effects which they casually see around them, what is this, except to make our first rude and unscientific persuasions the criterion and test of our most laborious and thoughtful inferences? What is it, but to give to ignorance and thoughtlessness the right of pronouncing upon the convictions of intense study and long-disciplined thought? "Electrical atmospheres" surrounding electrized bodies, were at one time held to be a "true cause" of the effects which such bodies produce. These atmospheres, it was said, are obvious to the senses; we feel them like a spider's web on the hands and face. Æpinus had to answer such persons, by proving that there are no atmospheres, no effluvia, but only repulsion. He thus, for a *true cause* in the vulgar sense of the term, substituted an *hypothesis;* yet who doubts that what he did was an advance in the science of electricity?

8. Perhaps some persons may be disposed to say, that Newton's Rule does not enjoin us to take those causes only which we clearly know, or suppose we know, to be really existing and operating, but only causes *of such kinds* as we have already satisfied ourselves do exist in nature. It may be urged that we are entitled to infer that the planets are governed in their motions by an attractive force, because we find, in the bodies immediately subject to observation and experiment, that such

motions are produced by attractive forces, for example by that of the earth. It may be said that we might on similar grounds infer forces which unite particles of chemical compounds, or deflect particles of light, because we see adhesion and deflection produced by forces.

But it is easy to show that the Rule, thus laxly understood, loses all significance. It prohibits no hypothesis; for all hypotheses suppose causes *such as*, in some case or other, we have seen in action. No one would think of explaining phenomena by referring them to forces and agencies altogether different from any which are known; for on this supposition, how could he pretend to reason about the effects of the assumed causes, or undertake to prove that they would explain the facts? Some close similarity with some known kind of cause is requisite, in order that the hypothesis may have the appearance of an explanation. No forces, or virtues, or sympathies, or fluids, or ethers, would be excluded by *this* interpretation of *veræ causæ*. Least of all, would such an interpretation reject the Cartesian hypothesis of vortices; which undoubtedly, as I conceive, Newton intended to condemn by his Rule. For that *such* a case as a whirling fluid, carrying bodies round a center in orbits, does occur, is too obvious to require proof. Every eddying stream, or blast that twirls the dust in the road, exhibits examples of such action, and would justify the assumption of the vortices which carry the planets in their courses; as indeed, without doubt, such facts suggested the Cartesian explanation of the solar system. The vortices, in this mode of considering the subject, are at the least as *real* a cause of motion as gravity itself.

9. Thus the Rule which enjoins "true causes," is nugatory, if we take *veræ causæ* in the extended sense of any causes of a real *kind*, and unphilosophical if we

understand the term of *those very* causes which we familiarly suppose to exist. But it may be said that we are to designate as "true causes," not those which are collected in a loose, confused and precarious manner, by undisciplined minds, from obvious phenomena, but those which are justly and rigorously inferred. Such a cause, it may be added, gravity is; for the facts of the downward pressures and downward motions of bodies at the earth's surface lead us, by the plainest and strictest induction, to the assertion of such a force. Now to this interpretation of the Rule there is no objection; but then, it must be observed, that on this view, terrestrial gravity is inferred by the same process as celestial gravitation; and the cause is no more entitled to be called "true," because it is obtained from the former, than because it is obtained from the latter class of facts. We thus obtain an intelligible and tenable explanation of a *vera causa;* but then, by this explanation, its *verity* ceases to be distinguishable from its other condition, that it "suffices for the explanation of the phenomena." The assumption of universal gravitation accounts for the fall of a stone; it also accounts for the revolutions of the Moon or of Saturn; but since both these explanations are of the same kind, we cannot with justice make the one a criterion or condition of the admissibility of the other.

10. But still, the Rule, so understood, is so far from being unmeaning or frivolous, that it expresses one of the most important tests which can be given of a sound physical theory. It is true, the explanation of one set of facts may be of the same nature as the explanation of the other class: but then, that the cause explains *both* classes, gives it a very different claim upon our attention and assent from that which it would have if it explained one class only. The very circumstance that the two

explanations coincide, is a most weighty presumption in their favour. It is the testimony of two witnesses in behalf of the hypothesis; and in proportion as these two witnesses are separate and independent, the conviction produced by their agreement is more and more complete. When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition. This coincidence of propositions inferred from separate classes of facts, is exactly what we noticed in the last Book, as one of the most decisive characteristics of a true theory, under the name of *Consilience of Inductions*.

That Newton's First Rule of Philosophizing, so understood, authorizes the inferences which he himself made, is really the ground on which they are so firmly believed by philosophers. Thus when the doctrine of a gravity varying inversely as the square of the distance from the body, accounted at the same time for the relations of times and distances in the planetary orbits and for the amount of the moon's deflection from the tangent of her orbit, such a doctrine became most convincing: or again, when the doctrine of the universal gravitation of all parts of matter, which explained so admirably the inequalities of the moon's motions, also gave a satisfactory account of a phenomenon utterly different, the precession of the equinoxes. And of the same kind is the evidence in favour of the undulatory theory of light, when the assumption of the length of an undulation, to which we are led by the colours of thin plates, is found to be identical with that length which explains the phenomena of diffraction; or when the hypothesis of transverse vibrations, suggested by the facts of polarization, explains also the laws of double refraction. When such a convergence of two trains of induction points to the same spot, we can no longer suspect that we are wrong. Such an accumulation of proof really persuades us that we have to do with a *vera causa*. And if this kind of proof be multiplied;—if we again find other facts of a sort uncontemplated in framing our hypothesis, but yet clearly accounted for when we have adopted the supposition;—we are still further confirmed in our belief; and by such accumulation of proof we may be so far satisfied, as to believe without conceiving it possible to doubt. In this case, when the validity of the opinion adopted by us has been repeatedly confirmed by its sufficiency in unforeseen cases, so that all doubt is removed and forgotten, the theoretical cause takes its place among the realities of the world, and becomes *a true cause*.

11. Newton's Rule then, to avoid mistakes, might be thus expressed; That "we may, provisorily, assume such hypothetical cause as will account for any given class of natural phenomena; but that when two different classes of facts lead us to the same hypothesis, we may hold it to be a *true cause*." And this Rule will rarely or never mislead us. There are no instances, in which a doctrine recommended in this manner has afterwards been discovered to be false. There have been hypotheses which have explained many phenomena, and kept their ground long, and have afterwards been rejected. But these have been hypotheses which explained only one class of phenomena; and their fall took place when another kind of facts was examined and brought into conflict with the former. Thus the system of eccentrics and epicycles accounted for all the observed *motions* of the planets, and was the means of expressing and transmitting all astronomical knowledge for two thousand years. But then, how was it overthrown? By considering the *dis-*

tances as well as motions of the heavenly bodies. Here was a second class of facts; and when the system was adjusted so as to agree with the one class, it was at variance with the other. These cycles and epicycles could not be true, because they could not be made a just representation of the facts. But if the measures of distance as well as of position had conspired in pointing out the cycles and epicycles, as the paths of the planets, the paths so determined could not have been otherwise than their real paths; and the epicyclical theory would have been, at least geometrically, true.

12. *Of the Second Rule.*—Newton's Second Rule directs that "natural events of the *same kind* are to be referred to the *same causes*, so far as can be done." Such a precept at first appears to help us but little; for all systems, however little solid, profess to conform to such a rule. When any theorist undertakes to explain a class of facts, he assigns causes which, according to him, will by their natural action, as seen in other cases, produce the effects in question. The events which he accounts for by his hypothetical cause, are, he holds, of the same kind as those which such a cause is known to produce. Kepler, in ascribing the planetary motions to magnetism, Descartes, in explaining them by means of vortices, held that they were referring celestial motions to the causes which give rise to terrestrial motions of the same kind. The question is, *Are* the effects of the same kind? This once settled, there will be no question about the propriety of assigning them to the same cause. But the difficulty is, to determine *when* events are of the same kind. Are the motions of the planets of the same kind with the motion of a body moving freely in a curvilinear path, or do they not rather resemble the motion of a floating body swept round by a whirling current? The Newtonian and the Cartesian answered this question

differently. How then can we apply this Rule with any advantage?

13. To this we reply, that there is no way of escaping this uncertainty and ambiguity, but by obtaining a clear possession of the ideas which our hypothesis involves, and by reasoning rigorously from them. Newton asserts that the planets move in free paths, acted on by certain forces. The most exact calculation gives the closest agreement of the results of this hypothesis with the facts. Descartes asserts that the planets are carried round by a fluid. The more rigorously the conceptions of force and the laws of motion are applied to this hypothesis, the more signal is its failure in reconciling the facts to one another. Without such calculation, we can come to no decision between the two hypotheses. If the Newtonian hold that the motions of the planets are *evidently* of the *same kind* as those of a body describing a curve in free space, and therefore, like that, to be explained by a force acting upon the body; the Cartesian denies that the planets do move in free space. They are, he maintains, immersed in a plenum. It is only when it appears that comets pass through this plenum in all directions with no impediment, and that no possible form and motion of its whirlpools can explain the forces and motions which are observed in the solar system, that he is compelled to allow the Newtonian's classification of events of the *same kind.*

Thus it does not appear that this Rule of Newton can be interpreted in any distinct and positive manner, otherwise than as enjoining that, in the task of induction, we employ clear ideas, rigorous reasoning, and close and fair comparison of the results of the hypothesis with the facts. These are, no doubt, important and fundamental conditions of a just induction; but in this injunction we find no peculiar or technical criterion by which we may satisfy ourselves that we are right, or detect our errours. Still, of such general prudential rules, none can be more wise than one which thus, in the task of connecting facts by means of ideas, recommends that the ideas be clear, the facts, correct, and the chain of reasoning which connects them, without a flaw.

14. *Of the Third Rule.*—The Third Rule, that "qualities which are observed without exception be held to be universal," as I have already said, seems to be intended to authorize the assertion of gravitation as a universal attribute of matter. We formerly stated, in treating of Mechanical Ideas*, that this application of such a Rule appears to be a mode of reasoning far from conclusive. The assertion of the universality of any property of bodies must be grounded upon the reason of the case, and not upon any arbitrary maxim. Is it intended by this Rule to prohibit any further examination how far gravity is an original property of matter, and how far it may be resolved into the result of other agencies? We know perfectly well that this was not Newton's intention; since the cause of gravity was a point which he proposed to himself as a subject of inquiry. It would certainly be very unphilosophical to pretend, by this Rule of Philosophizing, to prejudge the question of such hypotheses as that of Mosotti, That gravity is the excess of the electrical attraction over electrical repulsion: and yet to adopt this hypothesis, would be to suppose electrical forces more truly universal than gravity; for according to the hypothesis, gravity, being the inequality of the attraction and repulsion, is only an accidental and partial relation of these forces. Nor would it be allowable to urge this Rule as a reason of assuming that double stars are attracted to each other by a force varying according to the inverse square of the distance;

* Book III. c. x.

without examining, as Herschel and others have done, the orbits which they really describe. But if the Rule is not available in such cases, what is its real value and authority? and in what cases are they exemplified?

15. In a former part of this work*, it was shown that the fundamental laws of motion, and the properties of matter which these involve, are, after a full consideration of the subject, unavoidably assumed as universally true. It was further shown, that although our knowledge of these laws and properties be gathered from experience, we are strongly impelled, some philosophers think, authorized, to look upon these as not only universally, but necessarily true. It was also stated, that the law of gravitation, though its universality may be deemed probable, does not apparently involve the same necessity as the fundamental laws of motion. But it was pointed out that these are some of the most abstruse and difficult questions of the whole of philosophy; involving the profound, perhaps insoluble, problem of the identity or diversity of Ideas and Things. It cannot, therefore, be deemed philosophical to cut these Gordian knots by peremptory maxims, which encourage us to decide without rendering a reason. Moreover, it appears clear that the reason which is rendered for this Rule by the Newtonians is quite untenable; namely, that we know extension, hardness, and inertia, to be universal qualities of bodies by experience alone, and that we have the same evidence of experience for the universality of gravitation. We have already observed that we cannot, with any propriety, say that we *find* by experience all bodies are extended. This could not be a just assertion, except we could conceive the possibility of our finding the contrary. But who can conceive our finding by experience some bodies which are not extended? It appears, then, that the reason

* Book III. c. ix. x. xi.

given for the Third Rule of Newton involves a mistake respecting the nature and authority of experience. And the Rule itself cannot be applied without attempting to decide, by the casual limits of observation, questions which necessarily depend upon the relations of ideas.

16. *Of the Fourth Rule.*—Newton's Fourth Rule is, that "Propositions collected from phenomena by induction, shall be held to be true, notwithstanding contrary hypotheses; but shall be liable to be rendered more accurate, or to have their exceptions pointed out, by additional study of phenomena." This Rule contains little more than a general assertion of the authority of induction, accompanied by Newton's usual protest against hypotheses.

The really valuable part of the Fourth Rule is that which implies that a constant verification, and, if necessary, rectification, of truths discovered by induction, should go on in the scientific world. Even when the law is, or appears to be, most certainly exact and universal, it should be constantly exhibited to us afresh in the form of experience and observation. This is necessary, in order to discover exceptions and modifications if such exist; and if the law be rigorously true, the contemplation of it, as exemplified in the world of phenomena, will best give us that clear apprehension of its bearings which may lead us to see the ground of its truth.

The concluding clause of this Fourth Rule appears, at first, to imply that all inductive propositions are to be considered as merely provisional and limited, and never secure from exception. But to judge thus would be to underrate the stability and generality of scientific truths; for what man of science can suppose that we shall hereafter discover exceptions to the universal gravitation of all parts of the solar system? And it is plain that the author did not intend the restriction to be applied so rigorously; for in the Third Rule, as we have just seen he authorizes us to infer universal properties of matter from observation, and carries the liberty of inductive inference to its full extent. The Third Rule appears to encourage us to assert a law to be universal, even in cases in which it has not been tried; the Fourth Rule seems to warn us that the law may be inaccurate, even in cases in which it has been tried. Nor is either of these suggestions erroneous; but both the universality and the rigorous accuracy of our laws are proved by reference to Ideas rather than to Experience; a truth which, perhaps, the philosophers of Newton's time were somewhat disposed to overlook.

17. The disposition to ascribe all our knowledge to Experience, appears in Newton and the Newtonians by other indications; for instance, it is seen in their extreme dislike to the ancient expressions by which the principles and causes of phenomena were described, as the *occult causes* of the Schoolmen, and the *forms* of the Aristotelians, which had been adopted by Bacon. Newton says*, that the particles of matter not only possess inertia, but also active principles, as gravity, fermentation, cohesion; he adds, "These principles I consider not as Occult Qualities, supposed to result from the Specific Forms of things, but as General Laws of Nature, by which the things themselves are formed: their truth appearing to us by phenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult. And the Aristotelians gave the name of *occult qualities*, not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to the unknown causes of manifest effects: such as would be the causes of gravity, and of mag-

* *Optics*, Qu. 31.

netick and electrick attractions, and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us, and incapable of being discovered and made manifest. Such occult qualities put a stop to the improvement of Natural Philosophy, and therefore of late years have been rejected. To tell us that every species of things is endowed with an occult specific quality by which it acts and produces manifest effects, is to tell us nothing: but to derive two or three general principles of motion from phenomena, and afterwards to tell us how the properties and actions of all corporeal things follow from these manifest principles, would be a great step in philosophy, though the causes of those principles were not yet discovered: and therefore I scruple not to propose the principles of motion above maintained, they being of very general extent, and leave their causes to be found out."

18. All that is here said is highly philosophical and valuable; but we may observe that the investigation of *specific forms*, in the sense in which some writers had used the phrase, was by no means a frivolous or unmeaning object of inquiry. Bacon and others had used *form* as equivalent to *law**. If we could ascertain that arrangement of the particles of a crystal from which its external crystalline form and other properties arise, this arrangement would be the *internal form* of the crystal. If the undulatory theory be true, the *form* of light is transverse vibrations: if the emission theory be maintained, the *form*

* *Nov. Org.*, Lib. II. Aph. 2. Licet enim in natura nihil existet præter corpora individua, edentia actus puros individuos ex lege; in doctrinis tamen illa ipsa lex, ejusque inquisitio, et inventio, et explicatio, pro fundamento est tam ad sciendum quam ad operandum. Eam autem *legem*, ejusque *paragraphos*, *formarum* nomine intelligimus; præsertim cum hoc vocabulum invaluerit, et familiter occurrat.

Aph. 17. Eadem res est *forma* calidi vel *forma* luminis, et *lex* calidi aut *lex* luminis.

of light is particles moving in straight lines, and deflected by various forces. Both the terms, *form* and *law*, imply an ideal connexion of sensible phenomena; form supposes matter which is moulded to the form; law supposes objects which are governed by the law. The former term refers more precisely to existences, the latter to occurrences. The latter term is now the more familiar, and is, perhaps, the better metaphor: but the former also contains the essential antithesis which belongs to the subject, and might be used in expressing the same conclusions.

But occult causes, employed in the way in which Newton describes, had certainly been very prejudicial to the progress of knowledge, by stopping inquiry with a mere word. The absurdity of such pretended explanations had not escaped ridicule. The pretended physician in the comedy gives an example of an occult cause or virtue.

> Mihi demandatur
> A doctissimo Doctore
> *Quare* Opium facit dormire:
> Et ego respondeo,
> *Quia* est in eo
> *Virtus dormitiva*,
> Cujus natura est sensus assoupire.

19. But the most valuable part of the view presented to us in the quotation just given from Newton is the distinct separation, already noticed as peculiarly brought into prominence by him, of the determination of the *laws* of phenomena, and the investigation of their *causes*. The maxim, that the former inquiry must precede the latter, and that if the general laws of facts be discovered, the result is highly valuable, although the causes remain unknown, is extremely important; and had not, I think, ever been so strongly and clearly stated, till Newton both

repeatedly promulgated the precept, and added to it the weight of the most striking examples.

We have seen that Newton, along with views the most just and important concerning the nature and methods of science, had something of the tendency, prevalent in his time, to suspect or reject, at least speculatively, all elements of knowledge except observation. This tendency was, however, in him so corrected and restrained by his own wonderful sagacity and mathematical habits, that it scarcely led to any opinion which we might not safely adopt. But we must now consider the cases in which this tendency operated in a more unbalanced manner, and led to the assertion of doctrines which, if consistently followed, would destroy the very foundations of all general and certain knowledge.

Chapter XIV.

LOCKE AND HIS FRENCH FOLLOWERS.

1. In the constant opposition and struggle of the schools of philosophy, which consider our Senses and our Ideas, respectively, as the principal sources of our knowledge, we have seen that at the period of which we now treat, the tendency was to exalt the external and disparage the internal element. The disposition to ascribe our knowledge to observation alone, had already, in Bacon's time, led him to dwell to a disproportionate degree upon that half of his subject; and had tinged Newton's expressions, though it had not biassed his practice. But this partiality soon assumed a more prominent shape, becoming extreme in Locke, and extravagant in those who professed to follow him.

Indeed Locke appears to owe his popularity and influence as a popular writer mainly to his being one of the first to express, in a plain and unhesitating manner, opinions which had for some time been ripening in the minds of a large portion of the cultivated public. Hobbes had already promulgated the main doctrines, which Locke afterwards urged, on the subject of the origin and nature of our knowledge: but in him these doctrines were combined with offensive opinions on points of morals, government, and religion, so that their access to general favour was impeded: and it was to Locke that they were indebted for the extensive influence which they soon after obtained. Locke owed this authority mainly to the intellectual circumstances of the time. Although a writer of great merit, he by no means possesses such metaphysical acuteness or such philosophical largeness of view, or such a charm of writing, as must necessarily give him the high place he has held in the literature of Europe. But he came at a period when the reign of Ideas was tottering to its fall. All the most active and ambitious spirits had gone over to the new opinions, and were prepared to follow the fortunes of the Philosophy of Experiment, then in the most prosperous and brilliant condition, and full of still brighter promise. There were, indeed, a few learned and thoughtful men who still remained faithful to the empire of Ideas; partly, it may be, from a too fond attachment to ancient systems; but partly, also, because they knew that there were subjects of vast importance, in which experience did not form the whole foundation of our knowledge. They knew, too, that many of the plausible tenets of the new philosophy were revivals of fallacies which had been discussed and refuted in ancient times. But the advocates of mere experience came on with a vast store of weighty truth among their artillery, and with the energy which the advance usually bestows. The ideal system of philosophy could, for the present, make no effectual resistance; Locke, by putting himself at the head of the assault, became the hero of his day: and his name has been used as the watchword of those who adhere to the philosophy of the senses up to our own times.

2. Locke himself did not assert the exclusive authority of the senses in the extreme unmitigated manner in which some who call themselves his disciples have done. But this is the common lot of the leaders of revolutions, for they are usually bound by some ties of affection and habit to the previous state of things, and would not destroy all traces of that condition: while their followers attend, not to their inconsistent wishes, but to the meaning of the revolution itself; and carry out, to their genuine and complete results, the principles which won the victory, and which have been brought out more sharp from the conflict. Thus Locke himself does not assert that all our ideas are derived from Sensation, but from Sensation *and Reflection*. But it was easily seen that, in this assertion, two very heterogeneous elements were conjoined: that while to pronounce Sensation the origin of ideas, is a clear decided tenet, the acceptance or rejection of which determines the general character of our philosophy; to make the same declaration concerning Reflection, is in the highest degree vague and ambiguous; since reflection may either be resolved into a mere modification of sensation, as was done by one school, or may mean all that the opposite school opposes to sensation, under the name of Ideas. Hence the clear and strong impression which fastened upon men's minds, and which does in fact represent all the systematic and consistent part of Locke's philosophy, was, that in it all our ideas are represented as derived from Sensation.

3. We need not spend much time in pointing out

the inconsistencies into which Locke fell; as all must fall into inconsistencies who recognize no source of knowledge except the senses. Thus he maintains that our Idea of Space is derived from the senses of sight and touch; our Idea of Solidity from the touch alone. Our Notion of Substance is an unknown support of unknown qualities, and is illustrated by the Indian fable of the tortoise which supports the elephant, which supports the world. Our Notion of Power or Cause is in like manner got from the senses. And yet, though these ideas are thus mere fragments of our experience, Locke does not hesitate to ascribe to them necessity and universality when they occur in propositions. Thus he maintains the necessary truth of geometrical properties: he asserts that the resistance arising from solidity is absolutely insurmountable*; he conceives that nothing short of Omnipotence can annihilate a particle of matter†; and he has no misgivings in arguing upon the axiom that Every thing must have a cause. He does not perceive that, upon his own account of the origin of our knowledge, we can have no right to make any of these assertions. If our knowledge of the truths which concern the external world were wholly derived from experience, all that we could venture to say would be,—that geometrical properties of figures are true *as far as we have tried them*;—that we have seen *no example* of a solid body being reduced to occupy less space by pressure, or of a material substance annihilated by natural means;—and that *wherever we have examined*, we have found that every change has had a cause. Experience can never entitle us to declare that what she has not seen is impossible; still less, that things which she can not see are certain. Locke himself intended to throw no doubt upon the certainty of either human or divine knowledge; but his principles, when

* Book XI. c. iv. sect. 3. † *Ib.*, c. xiii. sect. 22.

men discarded the temper in which he applied them, and the checks to their misapplication which he conceived that he had provided, easily led to a very comprehensive scepticism. His doctrines tended to dislodge from their true bases the most indisputable parts of knowledge; as, for example, pure and mixed mathematics. It may well be supposed, therefore, that they shook the foundations of many other parts of knowledge in the minds of common thinkers.

It was not long before these consequences of the overthrow of ideas showed themselves in the speculative world. I have already in a previous part of this work* mentioned Hume's sceptical inferences from Locke's maxim, that we have no ideas except those which we acquire by experience; and the doctrines set up in opposition to this by the metaphysicians of Germany. I might trace the progress of the sensational opinions in Britain till the reaction took place here also: but they were so much more clearly and decidedly followed out in France, that I shall pursue their history in that country.

4. *The French Followers of Locke, Condillac, &c.*—Most of the French writers who adopted Locke's leading doctrines, rejected the "Reflection," which formed an anomalous part of his philosophy, and declared that Sensation alone was the source of ideas. Among these writers, Condillac was the most distinguished. He expressed the leading tenet of their school in a clear and pointed manner by saying that "All ideas are transformed sensations." We have already considered this phrase†, and need not here longer dwell upon it.

Opinions such as these tend to annihilate, as we have seen, one of the two co-ordinate elements of our know-

* Book III. c. iii. Modern Opinions respecting the Idea of Cause.

† B. I. c. iv.

ledge. Yet they were far from being so prejudicial to the progress of science, or even of the philosophy of science, as might have been anticipated. One reason of this was, that they were practically corrected, especially among the cultivators of Natural Philosophy, by the study of mathematics; for that study did really supply all that was requisite on the ideal side of science, so far as the ideas of space, time, and number, were concerned, and partly also with regard to the idea of cause and others. And the methods of discovery, though the philosophy of them made no material advance, were practically employed with so much activity, and in so many various subjects, that a certain kind of prudence and skill in this employment was very widely diffused.

5. *Importance of Language.*—In one respect this school of metaphysicians rendered a very valuable service to the philosophy of science. They brought into prominent notice the great importance of *words* and *terms* in the formation and progress of knowledge, and pointed out that the office of language is not only to convey and preserve our thoughts, but to perform the analysis in which reasoning consists. They were led to this train of speculation, in a great measure, by taking pure mathematical science as their standard example of substantial knowledge. Condillac, rejecting, as we have said, almost all those ideas on which universal and demonstrable truths must be based, was still not at all disposed to question the reality of human knowledge; but was, on the contrary, a zealous admirer of the evidence and connexion which appear in those sciences which have the ideas of space and number for their foundation, especially the latter. He looked for the grounds of the certainty and reality of the knowledge which these sciences contain; and found them, as he conceived, in the

nature of the *language* which they employ. The *Signs* which are used in arithmetic and algebra enable us to keep steadily in view the identity of the same quantity under all the forms which, by composition and decomposition, it may be made to assume; and these Signs also not only express the operations which are performed, but suggest the extension of the operations according to analogy. Algebra, according to him, is only a very perfect language; and language answers its purpose of leading us to truth, by possessing the characteristics of algebra. Words are the symbols of certain groups of impressions or facts; they are so selected and applied as to exhibit the analogies which prevail among these facts; and these analogies are the truths of which our knowledge consists. "Every language is an analytical method; every analytical method is a language*;" these were the truths "alike new and simple," as he held, which he conceived that he had demonstrated. "The art of speaking, the art of writing, the art of reasoning, the art of thinking, are only, at bottom, one and the same art†." Each of these operations consists in a succession of analytical operations; and words are the marks by which we are able to fix our minds upon the steps of this analysis.

6. The analysis of our impressions and notions does in reality lead to truth, not only in virtue of the identity of the whole with its parts, as Condillac held, but also in virtue of certain Ideas which govern the synthesis of our sensations, and which contain the elements of universal truths, as we have all along endeavoured to show. But although Condillac overlooked or rejected this doctrine, the importance of words, as marking the successive steps of this synthesis and analysis, is not less than he represented it to be. Every truth, once established by induc-

* *Langue des Calculs*, p. 1. † *Grammaire*, p. xxxvi.

tion from facts, when it is become familiar under a brief and precise form of expression, becomes itself a fact; and is capable of being employed, along with other facts of a like kind, as the materials of fresh inductions. In this successive process, the term, like the cord of a fagot, both binds together the facts which it includes, and makes it possible to manage the assemblage as a single thing. On occasion of most discoveries in science, the selection of a technical term is an essential part of the proceeding. In the *History of Science*, we have had numerous opportunities of remarking this; and the List of technical terms given as an Index to that work, refers us, by almost every word, to one such occasion. And these terms, which thus have had so large a share in the formation of science, and which constitute its language, do also offer the means of analyzing its truths, each into *its* constituent truths; and these into facts more special, till the original foundations of our most general propositions are clearly exhibited. The relations of general and particular truths are most evidently represented by the Inductive Tables given in Book XI. But each step in each of these Tables has its proper form of expression, familiar among the cultivators of science; and the analysis which our Tables display, is commonly performed in men's minds, when it becomes necessary, by fixing the attention successively upon a series of words, not upon the lines of a Table. Language offers to the mind such a scale or ladder as the Table offers to the eye; and as such Tables present to us, as we have said, the Logic of Induction, that is, the formal conditions of the soundness of our reasoning from facts, we may with propriety say that a just analysis of the meaning of words is an essential portion of Inductive Logic.

In saying this, we must not forget that a decomposition of general truths into ideas, as well as into facts, belongs to our philosophy; but the point we have here to remark, is the essential importance of words to the latter of these processes. And this point had not ever had its due weight assigned to it till the time of Condillac and other followers of Locke, who pursued their speculations in the spirit I have just described. The doctrine of the importance of terms is the most considerable addition to the philosophy of science which has been made since the time of Bacon*.

7. *The French Encyclopædists.*—The French *Encyclopédie,* published in 1751, of which Diderot and Dalembert were the editors, may be considered as representing the leading characters of European philosophy during the greater part of the eighteenth century. The writers in this work belong for the most part to the school of Locke and Condillac; and we may make a few remarks upon them, in order to bring into view one or two points in addition to what we have already said of that school. The *Discours Préliminaire,* written by Dalembert, is celebrated as containing a view of the origin of our knowledge, and the connexion and classification of the sciences.

A tendency of the speculations of the Encyclopedists, as of the School of Locke in general, is to reject all ideal principles of connexion among facts, as something which experience, the only source of true knowledge, does not give. Hence all certain knowledge consists only in the recognition of the same thing under different aspects, or different forms of expression. Axioms are not the result

* Since the selection and construction of terms is thus a matter of so much consequence in the formation of science, it is proper that systematic rules, founded upon sound principles, should be laid down for the performance of this operation. Some such rules are accordingly suggested in a subsequent part of this work.

of an original relation of ideas, but of the use, or it may be the abuse*, of words. In like manner, the propositions of Geometry are a series of modifications,—of distortions, so to speak,—of one original truth; much as if the proposition were stated in the successive forms of expression presented by a language which was constantly growing more and more artificial. Several of the sciences which rest upon physical principles, that is, (says the writer) truths of experience or simple hypotheses, have only an experimental or hypothetical certainty. Impenetrability added to the idea of extent is a mystery in addition: the nature of motion is a riddle for philosophers: the metaphysical principle of the laws of percussion is equally concealed from them. The more profoundly they study the idea of matter and of the properties which represent it, the more obscure this idea becomes; the more completely does it escape them.

8. This is a very common style of reflection, even down to our own times. I have endeavoured to show that concerning the Fundamental Ideas of space, of force and resistance, of substance, external quality, and the like, we know enough to make these Ideas the grounds of certain and universal truths;—enough to supply us with axioms from which we can demonstratively reason. If men wish for any other knowledge of the nature of matter than that which ideas, and facts conformable to ideas, give them, undoubtedly their desire will be frustrated, and they will be left in a mysterious vacancy; for it does not appear how such knowledge as they ask for could be knowledge at all. But in reality, this complaint of our ignorance of the real nature of things proceeds from the rejection of ideas, and the assumption of the senses alone as the ground of knowledge. "Obser-

* *Disc. Prélim.*, p. viii.

vation and calculation are the only sources of truth:"—this is the motto of the school of which we now speak. And its import amounts to this:—that they reject all ideas except the idea of number, and recognize the modifications which parts undergo by addition and subtraction as the only modes in which true propositions are generated. The laws of nature are assemblages of facts: the truths of science are assertions of the identity of things which are the same. "By the avowal of almost all philosophers," says a writer of this school*, "the most sublime truths, when once simplified and reduced to their lowest terms, are converted into facts, and thenceforth present to the mind only this proposition; the white is white, the black is black."

These statements are true in what they positively assert, but they involve errour in the denial which by implication they convey. It is true that observation and demonstration are the only sources of scientific truth; but then, demonstration may be founded on other grounds besides the elementary properties of number. It is true that the theory of gravitation is but the assertion of a general fact; but this is so, not because a sound theory does not involve ideas, but because our apprehension of a fact does.

9. Another characteristic indication of the temper of the Encyclopedists and of the age to which they belong, is the importance by them assigned to those practical *Arts* which minister to man's comfort and convenience. Not only, in the body of the Encyclopedia, are the Mechanical Arts placed side by side with the Sciences, and treated at great length; but in the Preliminary Discourse, the preference assigned to the liberal over the mechanical Arts is treated as a prejudice†, and the value of science is spoken of as measured by its

* Helvetius *Sur l'Homme*, c. xxiii.

† P. xiii.

utility. "The discovery of the Mariner's Compass is not less advantageous to the human race than the explanation of its properties would be to physics.—Why should we not esteem those to whom we owe the fusee and the escapement of watches as much as the inventors of Algebra?" And in the classification of sciences which accompanies the Discourse, the labours of artisans of all kinds have a place.

This classification of the various branches of science contained in the Dissertation is often spoken of. It has for its basis the classification proposed by Bacon, in which the parts of human knowledge are arranged according to the faculties of the mind in which they originate; and these faculties are taken, both by Bacon and by Dalembert, as Memory, Reason, and Imagination. The insufficiency of Bacon's arrangement as a scientific classification is so glaring, that the adoption of it, with only superficial modifications, at the period of the Encyclopedia, is a remarkable proof of the want of original thought and real philosophy at the time of which we speak.

10. We need not trace further the opinion which derives all our knowledge from the senses in its application to the philosophy of Science. Its declared aim is to reduce all knowledge to the knowledge of Facts; and it rejects all inquiries which involve the Idea of Cause, and similar Ideas, describing them as "metaphysical," or in some other damnatory way. It professes, indeed, to discard all Ideas; but, as we have long ago seen, some Ideas or other are inevitably included even in the simplest Facts. Accordingly the speculations of this school are compelled to retain the relations of Position, Succession, Number and Resemblance, which are rigorously ideal relations. The philosophy of Sensation, in order to be consistent, ought to reject these Ideas along with

the rest, and to deny altogether the possibility of general knowledge.

When the opinions of the Sensational School had gone to an extreme length, a Reaction naturally began to take place in men's minds. Such have been the alternations of opinion, from the earliest ages of human speculation. Man may perhaps have existed in an original condition in which he was only aware of the impressions of Sense; but his first attempts to analyze his perceptions brought under his notice Ideas as a separate element, essential to the existence of knowledge. Ideas were thenceforth almost the sole subject of the study of philosophers; of Plato and his disciples, professedly; of Aristotle, and still more of the followers and commentators of Aristotle, practically. And this continued till the time of Galileo, when the authority of the Senses again began to be asserted; for it was shown by the great discoveries which were then made, that the Senses had at least some share in the promotion of knowledge. As discoveries more numerous and more striking were supplied by Observation, the world gradually passed over to the opinion that the share which had been ascribed to Ideas in the formation of real knowledge was altogether a delusion, and that Sensation alone was true. But when this was asserted as a general doctrine, both its manifest falsity and its alarming consequences roused men's minds, and made them recoil from the extreme point to which they were approaching. Philosophy again oscillated back towards Ideas; and over a great part of Europe, in the clearest and most comprehensive minds, this regression from the dogmas of the Sensational School is at present the prevailing movement. We shall conclude our review by noticing a few indications of this state of things.

Chapter XV.

THE REACTION AGAINST THE SENSATIONAL SCHOOL.

1. When Locke's *Essay* appeared, it was easily seen that its tendency was to urge, in a much more rigorous sense than had previously been usual, the ancient maxim of Aristotle, adopted by the schoolmen of the middle ages, that "nothing exists in the intellect but what has entered by the senses." Leibnitz expressed in a pointed manner the limitation with which this doctrine had always been understood. "Nihil est in intellectu quod non prius fuerit in sensu;—*nempe*," he added, "*nisi intellectus ipse.*" To this it has been objected*, that we cannot say that the intellect is *in* the intellect. But this remark is obviously frivolous; for the faculties of the understanding (which are what the argument against the Sensational School requires us to reserve) may be said to be *in* the understanding, with as much justice as we may assert there are *in* it the impressions derived from sense. And when we take account of these faculties, and of the Ideas to which, by their operation, we necessarily subordinate our apprehension of phenomena, we are led to a refutation of the philosophy which makes phenomena, unconnected by Ideas, the source of all knowledge. The succeeding opponents of the Lockian school insisted upon and developed in various ways this remark of Leibnitz, or some equivalent view.

2. It was by inquiries into the foundations of Morals that English philosophers were led to question the truth of Locke's theory. Dr. Price, in his *Review of the Principal Questions in Morals*, first published in 1757, maintained that we cannot with propriety assert all our ideas

* See Mr. Sharpe's *Essays*.

to be derived from sensation and reflection. He pointed out, very steadily, the other source. "The power, I assert, that *understands*, or the faculty within us that discerns *truth*, and that compares all the objects of thought and *judges* of them, is a spring of new ideas*." And he exhibits the antithesis in various forms. "Were not *sense* and *knowledge* entirely different, we should rest satisfied with sensible impressions, such as light, colours and sounds, and inquire no further about them, at least when the impressions are strong and vigorous: whereas, on the contrary, we necessarily desire some further acquaintance with them, and can never be satisfied till we have subjected them to the survey of reason. Sense presents *particular* forms to the mind, but cannot rise to any *general* ideas. It is the intellect that examines and compares the presented forms, that rises above individuals to universal and abstract ideas; and thus looks downward upon objects, takes in at one view an infinity of particulars, and is capable of discovering general truths. Sense sees only the outside of things, reason acquaints itself with their natures. Sensation is only a mode of feeling in the mind; but knowledge implies an active and vital energy in the mind†."

3. The necessity of refuting Hume's inferences from the mere-sensation system led other writers to limit, in various ways, their assent to Locke. Especially was this the case with a number of intelligent metaphysicians in Scotland, as Reid, Beattie, Dugald Stewart, and Thomas Brown. Thus Reid asserts‡, "that the account which Mr. Locke himself gives of the Idea of Power cannot be reconciled to his favourite doctrine, that all our simple ideas have their origin from sensation or reflection." Reid remarks, that our memory and our reasoning power

* P. 16. † P. 18.

‡ *Essays on the Powers of the Human Mind*, III. 31.

come in for a share in the origin of this idea: and in speaking of reasoning, he obviously assumes the axiom that every event must have a cause. By succeeding writers of this school, the assumption of the fundamental principles, to which our nature in such cases irresistibly directs us, is more clearly pointed out. Thus Stewart defends the form of expression used by Price*. "A variety of intuitive judgments might be mentioned, involving simple ideas, which it is impossible to trace to any origin but to the power which enables us to form these judgments. Thus it is surely an intuitive truth that the sensations of which I am conscious, and all those I remember, belong to one and the same being, which I call *myself*. Here is an intuitive judgment involving the simple idea of *Identity*. In like manner, the changes which I perceive in the universe impress me with a conviction that some cause must have operated to produce them. Here is an intuitive judgment involving the simple Idea of *Causation*. When we consider the adjacent angles made by a straight line standing upon another, and perceive that their sum is equal to two right angles, the judgment we form involves a simple idea of *Equality*. To say, therefore, that the Reason or the Understanding is a source of new ideas, is not so exceptionable a mode of speaking as has been sometimes supposed. According to Locke, *Sense* furnishes our ideas, and Reason perceives their agreements and disagreements. But the truth is, that these agreements and disagreements are in many instances, simple ideas, of which no analysis can be given; and of which the origin must therefore be referred to Reason, according to Locke's own doctrine." This view, according to which the Reason or Understanding is the source of certain simple ideas, such as Identity, Causation, Equality, which ideas are necessarily involved in the

* *Outlines of Moral Phil.*, p. 138.

intuitive judgments which we form, when we recognize fundamental truths of science, approaches very near in effect to the doctrine which in this work we have presented, of Fundamental Ideas belonging to each science, and manifesting themselves in the axioms of the science. It may be observed, however, that by attempting to enumerate these ideas and axioms, so as to lay the foundations of the whole body of physical science; and by endeavouring, as far as possible, to simplify and connect each group of such Ideas; we have at least given a more systematic form to this doctrine. We have, moreover, traced it into many consequences to which it necessarily leads, but which do not appear to have been contemplated by the metaphysicians of the Scotch school. But I gladly acknowledge my obligations to the writers of that school; and I trust that in the near agreement of my views on such points with theirs, there is ground for believing the system of philosophy which I have in this work presented, to be that to which the minds of thoughtful men, who have meditated on such subjects, are generally tending.

4. As a further instance that such a tendency is at work, I may make a quotation from an eminent English philosophical writer of another school. "If you will be at the pains," says Archbishop Whately*, "carefully to analyze the simplest description you hear of any transaction or state of things, you will find that the process which almost invariably takes place is, in logical language, this: that each individual *has in his mind* certain major premises or principles relative to the subject in question;—that observation of what actually presents itself to the senses, supplies minor premises; and that the statement given (and which is reported as a thing experienced) consists, in fact, of the *conclusions* drawn from the

* *Polit. Econ.*, p. 76.

combinations of these premises." The major premises here spoken of are the Fundamental Ideas, and the Axioms and Propositions to which they lead; and whatever is regarded as a fact of observation is necessarily a conclusion in which these propositions are assumed; for these contain, as we have said, the conditions of our experience. Our experience conforms to these axioms and their consequences, whether or not the connexion be stated in a logical manner, by means of premises and a conclusion.

5. The same persuasion is also suggested by the course which the study of metaphysics has taken of late years in France. In that country, as we have seen, the Sensational System, which was considered as the necessary consequence of the revolution begun by Locke, obtained a more complete ascendancy than it did in England; and in that country too, the reaction, among metaphysical and moral writers, when its time came, was more decided and rapid than it was among Locke's own countrymen. It would appear that M. Laromiguière was one of the first to give expression to this feeling, of the necessity of a modification of the sensational philosophy. He bgan by professing himself the disciple of Condillac, even while he was almost unconsciously subverting the fundamental principles of that writer. And thus, as M. Cousin justly observes*, his opinions had the more powerful effect from being presented, not as thwarting and contradicting, but as sharing and following out the spirit of his age. M. Laromiguière's work, entitled *Essai sur les Facultés de l'Ame*, consists of lectures given to the Faculty of Letters of the Academy of Paris, in the years 1811, 1812 and 1813. In the views which these lectures present, there is much which the author has in common with Condillac. But he is led by his investigation to assert†, that it is

* *Fragmens Philosophiques*, I. 53. † *Ib.*, I. 67.

not true that sensation is the sole fundamental element of our thoughts and our understanding. *Attention* also is requisite: and here we have an element of quite another kind. For sensation is passive; attention is active. Attention does not spring out of sensation; the passive principle is not the reason of the active principle. Activity and passivity are two facts entirely different. Nor can this activity be defined or derived; being, as the author says, a fundamental idea. The distinction is manifest by its own nature; and we may find evidence of it in the very forms of language. To *look* is more than to *see;* to *hearken* is more than to *hear.* The French language marks this distinction with respect to other senses also. "On *voit,* et l'on *regarde;* on *entend,* et l'on *écoute;* on *sent,* et l'on *flaire;* on *goûte,* et l'on *savoure.*" And thus the mere sensation, or capacity of feeling, is only the occasion on which the attention is exercised; while the attention is the foundation of all the operations of the understanding.

The reader of the former part of this work, will have seen how much we have insisted upon the activity of the mind, as the necessary basis of all knowledge. In all observation and experience, the mind is active, and by its activity apprehends all sensations in subordination to its own ideas; and thus it becomes capable of collecting knowledge from phenomena, since ideas involve general relations and connexions, which sensations of themselves cannot involve. And thus we see that, in this respect also, our philosophy stands at that point to which the speculations of the most reflective men have of late constantly been verging.

6. M. Cousin himself, from whom we have quoted the above account of Laromiguière, shares in this tendency, and has argued very energetically and successfully against the doctrines of the Sensational School. He has

made it his office once more to bring into notice among his countrymen, the doctrine of ideas as the sources of knowledge; and has revived the study of Plato, who may still be considered as one of the great leaders of the ideal school. But the larger portion of M. Cousin's works refers to questions out of the reach of our present review, and it would be unsuitable to dwell longer upon them in this place.

7. We turn to speculations more closely connected with our present subject. M. Ampère, a French man of science, well entitled by his extensive knowledge, and large and profound views, to deal with the philosophy of the sciences, published in 1834, his *Essai sur la Philosophie des Sciences, ou Exposition analytique d'un Classification Naturelle de toutes les Connaissances Humaines.* In this remarkable work we see strong evidence of the progress of the reaction against the system which derives our knowledge from sensation only. The author starts from a maxim, that in classing the sciences, we must not only regard the nature of the objects about which each science is concerned, but also the point of view under which it considers them: that is, the *ideas* which each science involves. M. Ampère also gives briefly his views of the intellectual constitution of man; a subject on which he had long and sedulously employed his thoughts; and these views are far from belonging to the Sensational School. Human thought, he says, is composed of phenomena and of conceptions. Phenomena are external, or *sensitive;* and internal, or *active.* Conceptions are of four kinds; *primitive,* as space and motion, duration and cause; *objective,* as our idea of matter and substance; *onomatic,* or those which we associate with the general terms which language presents to us; and *explicative,* by which we ascend to causes after a comparative study of phenomena. He teaches further, that in deriving ideas from sensation,

the mind is not passive; but exerts an action which, when voluntary, is called *attention*, but when it is, as it often is, involuntary, may be termed *reaction*.

I shall not dwell upon the examination of these opinions*; but I may remark, that both in the recognition of conceptions as an original and essential element of the mind, and in giving a prominent place to the active function of the mind, in the origin of our knowledge, this view approaches to that which I have presented in the preceding part of this work; although undoubtedly with considerable differences.

8. The classification of the sciences which M. Ampère proposes, is founded upon a consideration of the sciences themselves; and is, the author conceives, in accordance with the conditions of natural classifications, as exhibited in Botany and other sciences. It is of a more symmetrical kind, and exhibits more steps of subordination, than that to which I have been led; it includes also practical Art as well as theoretical Science; and it is extended to moral and political as well as physical Sciences. It will not be necessary for me here to examine it in detail: but I may remark, that it is throughout a *dichotomous* division, each higher number being subdivided into two lower ones, and so on. In this way, M. Ampère obtains sciences of the First Order, each of which is divided into two sciences of the Second, and four of the Third Order. Thus Mechanics is divided into *Cinematics*, *Statics*, *Dynamics*, and *Molecular Mechanics;* Physics is divided into *Experimental Physics*, *Chemistry*, *Stereometry*, and *Atomology;* Geology is divided into *Physical Geography*, *Mineralogy*, *Geonomy*, and *Theory of the Earth*. Without here criticizing these divisions or their principle, I may observe that *Cinematics*, the doctrine of motion

* See also the vigorous critique of Locke's *Essay*, by Lemaistre, *Soirées de St. Petersbourg*.

without reference to the force which produces it, is a portion of knowledge which our investigation has led us also to see the necessity of erecting into a separate science; and which we have termed *Pure Mechanism*. Of the divisions of Geology, *Physical Geography*, especially as explained by M. Ampère, is certainly a part of the subject, both important and tolerably distinct from the rest. *Geonomy* contains what we have termed in the History, *Descriptive Geology*;—the exhibition of the facts separate from the inquiry into their causes; while our *Physical Geology* agrees with M. Ampère's *Theory of the Earth*. *Mineralogy* appears to be placed by him in a different place from that which it occupies in our scheme: but in fact, he uses the term for a different science;—he applies it to the classification not of *simple minerals*, but of *rocks*, which is a science auxiliary to geology, and which has sometimes been called *Petralogy*. What we have termed *Mineralogy*, M. Ampère unites with *Chemistry*. "It belongs," he says*, "to Chemistry, and not to Mineralogy, to inquire how many atoms of silicium and of oxygen compose silica; to tell us that its primitive form is a rhombohedron of certain angles, that it is called *quartz*, &c.: leaving, on one hand, to Molecular Geometry the task of explaining the different secondary forms which may result from the primitive form; and on the other hand, leaving to Mineralogy the office of describing the different varieties of quartz, and the rocks in which they occur, according as the quartz is crystallized, transparent, coloured, amorphous, solid, or in sand." But we may remark, that by adopting this arrangement, we separate from Mineralogy almost all the knowledge, and absolutely all the general knowledge, which books professing to treat of that science have usually contained. The consideration of Mineralogical

* P. 210.

Classifications, which, as may be seen in the *History of Science*, is so curious and instructive, is forced into the domain of Chemistry, although many of the persons who figure in it were not at all properly chemists. And we lose, in this way, the advantage of that peculiar office which, in our arrangement, Mineralogy fills; of forming a rigorous transition from the sciences of classification to those which consider the mathematical properties of bodies; and connecting the external characters and the internal constitution of bodies by means of a system of important general truths. I conceive, therefore, that our disposition of this science, and our mode of applying the name, are far more convenient than those of M. Ampère.

9. We have seen the reaction against the pure sensational doctrines operating very powerfully in England and in France. But it was in Germany that these doctrines were most decidedly rejected; and systems in extreme opposition to these put forth with confidence, and received with applause. Of the authors who gave this impulse to opinions in that country, Kant was the first, and by far the most important. I have already endeavoured to explain how he was roused, by the skepticism of Hume, to examine wherein the fallacy lay which appeared to invalidate all reasonings from effect to cause; and how this inquiry terminated in a conviction that the foundations of our reasonings on this and similar points were to be sought in the mind, and not in the phenomena;—in the *subject* and not in the *object*. The revolution in the customary mode of contemplating human knowledge which Kant's opinions involved, was most complete. He himself, with no small justice, compares* it with the change produced by Copernicus's theory of the solar system. "Hitherto," he says, "men

* *Kritik der Reinen Vernunft*, Pref., p. xv.

have assumed that all our knowledge must be regulated by the objects of it; yet all attempts to make out anything concerning objects *à priori* by means of our conceptions," (as for instance their geometrical properties) "must, on this foundation, be unavailing. Let us then try whether we cannot make out something more in the problems of metaphysics, by assuming that objects must be regulated by our knowledge, since this agrees better with that supposition, which we are prompted to make, that we can know something of them *à priori.* This thought is like that of Copernicus, who, when he found that nothing was to be made of the phenomena of the heavens so long as everything was supposed to turn about the spectator, tried whether the matter might not be better explained if he made the spectator turn, and left the stars at rest. We may make the same essay in metaphysics, as to what concerns our intuitive knowledge respecting objects. If our apprehension of objects must be regulated by the properties of the objects, I cannot comprehend how we can possibly know anything about them *à priori.* But if the object, as apprehended by us, be regulated by the constitution of our faculties of apprehension, I can readily conceive this possibility." From this he infers that our experience must be regulated by our conceptions.

10. This view of the nature of knowledge soon superseded entirely the doctrines of the Sensational School among the metaphysicians of Germany. These philosophers did not gradually modify and reject the dogmas of Locke and Condillac, as was done in England and France*; nor did they endeavour to ascertain the extent

* The sensational system never acquired in Germany the ascendancy which it obtained in England and France; but I am compelled here to pass over the history of philosophy in Germany, except so far as it affects ourselves.

of the empire of Ideas by a careful survey of its several provinces, as we have been doing in the previous part of the present work. The German metaphysicians saw at once that Ideas and Things, the Subjective and the Objective elements of our knowledge, were, by Kant's system, brought into opposition and correlation, as equally real and equally indispensable. Seeing this, they rushed at once to the highest and most difficult problem of philosophy,—to determine what this correlation is;—to discover how Ideas and Things are at the same time opposite and identical;—how the world, while it is distinct from and independent of us, is yet, as an object of our knowledge, governed by the conditions of our thoughts. The attempts to solve this problem, taken in the widest sense, including the forms which it assumes in Morals, Politics, the Arts, and Religion, as well as in the Material Sciences, have, since that time, occupied the most profound speculators of Germany; and have given rise to a number of systems, which, rapidly succeeding each other, have, each in its day, been looked upon as a complete solution of the problem. To trace the characters of these various systems, does not belong to the business of the present Book: my task at present is ended when I have shown, as I have now done, how the progress of thought in the philosophical world, followed from the earliest up to the present time, has led to that recognition of the co-existence and joint necessity of the two opposite elements of our knowledge; and when I have pointed out processes adapted to the extension of our knowledge, which a true view of its nature has suggested or may suggest.

In the latter portion of my task something still remains to be done, which will be the subject of the ensuing Book.

Chapter XVI.

FURTHER ADVANCE OF THE SENSATIONAL SCHOOL*.

I shall now take the liberty of noticing the views published by a contemporary writer; not that it forms part of my design to offer any criticism upon the writings of all those who have treated of those subjects on which we are now employed; but because we can more distinctly in this manner point out the contrasts and ultimate tendencies of the several systems of opinion which have come under our survey. And since from among these systems we have endeavoured to extract and secure the portion of truth which remains in each, and to reject the rest, we are led to point out the errours on which our attention is thus fixed, in recent as well as older writers.

M. Auguste Comte published in 1830 the first, and in 1835 the second volume† of his *Cours de Philosophie Positive;* of which the aim is not much different from that of the present work, since as he states, (p. viii.) such a title as the *Philosophy of the Sciences* would describe a part of his object, and would be inappropriate only by excluding that portion (not yet published) which refers to speculations concerning social relations. By employing the term *Philosophie Positive,* he wishes to distinguish the philosophy involved in the present state of our sciences from the previous forms of human knowledge. For according to him, each branch of knowledge passes, in the course of man's history, through three

* This chapter, now first published, is printed as it was written previously to the publication of the former edition.

† I believe I had not then seen the third volume (published in 1838) or the subsequent ones.

different states; it is first *theological*, then *metaphysical*, then *positive*. By the latter term he implies a state which includes nothing but general representations of facts;—phenomena *arranged according* to relations of succession and resemblance. This "positive philosophy" rejects all inquiry after causes, which he holds to be void of sense* and inaccessible. All such conceptions belong to the "metaphysical" state of science which deals with abstract forces, real entities, and the like. Still more completely does he reject, as altogether antiquated and absurd, the "theological" view of phenomena. Indeed he conceives† that any one's own consciousness of what passes within himself is sufficient to convince him of the truth of the law of the three phases through which knowledge must pass. "Does not each of us," he says, "in contemplating his own history, recollect that he has been successively a *theologian* in his infancy, a *metaphysician* in his youth, and a *physicist* in his ripe age? This may easily be verified for all men who are up to the level of their time."

It is plain from such statements, and from the whole course of his work, that M. Comte holds, in their most rigorous form, the doctrines to which the speculations of Locke and his successors led; and which tended, as we have seen, to the exclusion of all ideas except those of number and resemblance. As M. Comte refuses to admit into his philosophy the fundamental idea of Cause, he of course excludes most of the other ideas, which are, as we endeavoured to show, the foundations of science; such as the ideas of Media by which secondary qualities are made known to us; the ideas of Chemical Attraction, of Polar Forces, and the like. He would reduce all science to the mere expression of laws of phenomena, expressed in formulæ of space, time, and number; and would condemn

* I. p. 14. † I. p. 7.

as unmeaning, and as belonging to an obsolete state of science, all endeavours to determine the causes of phenomena, or even to refer them to any of the other ideas just mentioned.

In a previous part of this work (B. XIII. c. vi.) I have shown, I trust decisively, that it is the genuine office of science to inquire into the causes as well as the laws of phenomena;—that such an inquiry cannot be avoided; and that it has been the source of almost all the science we possess. I need not here repeat the arguments there urged; but I may make a remark or two upon M. Comte's hypothesis, that all science is first "metaphysical" and then "positive;" since it is in virtue of this hypothesis that he rejects the investigation of causes, as worthy only of the infancy of science. All discussions concerning ideas, M. Comte would condemn as "metaphysical," and would consider as mere preludes to positive philosophy. Now I venture to assert, on the contrary, that discussions concerning ideas, and real discoveries, have in every science gone hand in hand. There is no science in which the pretended order of things can be pointed out. There is no science in which the discoveries of the laws of phenomena, when once begun, have been carried on independently of discussions concerning ideas. There is no science in which the expression of the laws of phenomena can at this time dispense with ideas which have acquired their place in science in virtue of metaphysical considerations. There is no science in which the most active disquisitions concerning ideas did not come *after*, not *before*, the first discovery of laws of phenomena. In Astronomy, the discovery of the phenomenal laws of the epicyclical motions of the heavens led to assumptions of the metaphysical principle of equable circular motions: Kepler's discoveries would never have been made but for his metaphysical notions. These discoveries of the laws of phenomena did not lead immediately to Newton's theory, *because* a century of metaphysical discussions was requisite as a preparation. Newton then discovered, not merely a law of phenomena, but a *cause;* and *therefore* he was the greatest of discoverers. The same is the case in Optics; the ancients possessed some share of our knowledge of facts; but meddled little with the metaphysical reasonings of the subject. In modern times when men began to inquire into the *nature* of light, they soon extended their knowledge of its *laws.* When this series of discoveries had come to a pause, a new series of brilliant discoveries of laws of phenomena went on, inseparably connected with a new series of views of the nature and cause of light. In like manner, the most modern discoveries in chemistry involve indispensably the idea of polar forces. The metaphysics (in M. Comte's sense) of each subject advances in a parallel line with the knowledge of physical laws. The Explication of Conceptions must go on, as we have already shown, at the same rate as the Colligation of Facts.

M. Comte will say* that Newton's discovery of gravitation only consists in exhibiting the astronomical phenomena of the universe as one single fact under different points of view. But this *fact* involves the idea of *force,* that is, of *cause.* And that this idea is not a mere modification of the ideas of time and space, we have shown: if it were so, how could it lead to the axiom that attraction is mutual, an indispensable part of the Newtonian theory? M. Comte says† that we do not know what attraction is, since we can only define it by identical phrases: but this is just as true of space, or time, or motion; and is in fact exactly the characteristick of a fundamental idea. We do not obtain such ideas from

* P. 15. † P. 16.

definitions, but we possess them not the less truly because we cannot define them.

That M. Comte's hypothesis is historically false, is obvious by such examples as I have mentioned. Metaphysical discussions have been essential steps in the progress of each science. If we arbitrarily reject all these portions of scientific history as useless trifling, belonging to the first rude attempts at knowledge, we shall not only distort the progress of things, but pervert the plainest facts. Of this we have an example in M. Comte's account of Kepler's mechanical speculations. We have seen, in the History of Physical Astronomy, that Kepler's second law, (that the planets describe areas about the sun proportional to the times,) was proved by him, by means of calculations founded on the observations of Tycho; but that the mechanical reason of it was not assigned till a later period, when it appeared as the first proposition of Newton's *Principia*. It is plain from the writings of Kepler, that it was impossible for him to show how this law resulted from the forces which were in action; since the forces which he considered were not those tending to the center, which really determine the property in question, but forces exerted by the sun *in the direction of the planet's motion*, without which forces Kepler conceived that the motion could not go on. In short, the state of mechanical science in Kepler's time was such that no demonstration of the law could be given. The terms in which such a demonstration must be expressed had not at that time acquired a precise significance; and it was in virtue of many subsequent *metaphysical* discussions (as M. Comte would term them) that these terms became capable of expressing sound mechanical reasoning. Kepler did indeed pretend to assign what he called a "physical proof" of his law, depending upon this, that the sun's force is less at greater distances; a condition which does not at all influence the result. Thus Kepler's reason for his law proves nothing but the confusion of thought in which he was involved on such subjects. Yet M. Comte assigns to Kepler the credit of having proved this law by sound mechanical reasoning, as well as established it as a matter of fact*. "This discovery by Kepler," he adds, "is the more remarkable, inasmuch as it occurred before the science of dynamics had really been created by Galileo." We may remark that inasmuch as M. Comte perceived this incongruity in the facts as he stated them, it is the more remarkable that he did not examine them more carefully.

The condemnation of the inquiry into causes which is conveyed in M. Comte's notion of the three stages of Science, he again expresses more in detail, in stating†

* M. Comte's statement is so entirely at variance with the fact that I must quote it here. (*Phil. Pos.* Vol. I. p. 705.)

"Le second théorème général de dynamique consiste dans le celébre et important *principe des aires*, dont le première idée est due à Kepler, qui découvrit et démontra forte simplement cette propriété pour le cas du mouvement d'une molecule unique, ou en d'autres terms, d'un corps dont tous les points se meuvent identiquement. Kepler établit, par les considérations les plus élémentaires, qui si la force accélératrice totale dont une molecule est animée tend constamment vers un point fixé, le rayon vecteur du mobile décrit autour de ce point des aires égales en tems egaux, de telle sorte que l'aire décrite au bout d'un temps quelconque croît proportionellement à ce temps. Il fit voir en outre que reciproquement, si une semblable rélation a été verifiée dans le mouvement d'un corps per rapport à un certain point, c'est une preuve suffisante de l'action sur le corps d'un force dirigée sans cesse vers ce point."

There is not a trace of the above propositions in the work *De Stellâ Martis*, which contains Kepler's discovery of his law, nor, I am convinced, in any other of Kepler's works. He is everywhere constant to his conceptions of the *magnetic* virtue residing in the sun, by means of which the sun, revolving on his axis, carries the planets round with him M. Comte's statement so exactly expresses *Newton's* propositions, that one is led to suspect some exraordinary mistake, by which what should have been said of the one was transferred to the other.

† Vol. II. p. 433.

what he calls his *Fundamental theory of hypotheses.* This 'theory' is, that we may employ hypotheses in our natural philosophy, but these hypotheses must always be such as admit of a positive verification. We must have no suppositions concerning the agents by which effects are produced. All such have an anti-scientific character, and can only impede the real progress of physics. There can be no use in the ethers and imaginary fluids to which some persons refer the phenomena of heat, light, electricity and magnetism. And in agreement with this doctrine, M. Comte in his account* of the Science of Optics, condemns, as utterly unphilosophical and absurd, both the theory of emission and that of undulation.

To this we reply, that theory of one or other kind is indispensable to the expression of the phenomena; and that when the laws are expressed, and apparently explained, by means of a theory, to forbid us to inquire whether it be really true or false, is a pedantic and capricious limitation of our knowledge, to which the intellect of man neither can nor should submit. If any one holds the adoption of one or other of these theories to be indifferent, let him express the *laws of phenomena* of diffraction in terms of the theory of emission†. If any one rejects the doctrine of undulation, let him point out some other way of connecting double refraction with polarization. And surely no man of science will contend that the beautiful branch of science which refers to that connexion is not a portion of our positive knowledge.

M. Comte's contempt for the speculations of the

* Vol. II. p. 640.

† I venture to offer this problem;—to express the *laws of the phenomena* of diffraction without the hypothesis of undulations;—as a challenge to any one who holds such hypothesis to be unphilosophical.

undulationists seems to have prevented his acquainting himself with their reasonings, and even with the laws of phenomena on which they have reasoned, although these form by far the most striking and beautiful addition which Science has received in modern times. He adduces, as an insuperable objection to the undulatory theory, a difficulty which is fully removed by calculation in every work on the subject:—the existence of shadow*. He barely mentions the subject of diffraction, and Young's law of interferences;—speaks of Fresnel as having applied this principle to the phenomena of coloured rings, "on which the ingenious labours of Newton left much to desire;" as if Fresnel's labours on this subject had been the supplement of those of Newton: and after regretting that "this principle of interferences has not yet been distinctly disentangled from chemical conceptions on the nature of light," concludes his chapter. He does not even mention the phenomena of dipolarization, of circular and elliptical polarization, or of the optical properties of crystals; discoveries of laws of phenomena quite as remarkable as any which can be mentioned.

M. Comte's favourite example of physical research is Thermotics, and especially Fourier's researches with regard to heat. It is shown† in the History of Thermotics, that the general phenomena of radiation required the assumption of a fluid to express them; as appears in the *theory of exchanges*‡. And the explanation of the principal laws of radiation, which Fourier gives, depends upon the conception of material molecular radiation. The *flux* of caloric, of which Fourier speaks, cannot be conceived otherwise than as implying a material flow. M. Comte apologizes§ for this expression, as too figu-

* II. p. 641. † II. p. 673.
‡ *Hist. Ind. Sci.* II. 489, B. x. c. i. § II. p. 561.

rative, and says that it merely indicates a *fact*. But what is the flow of a current of fluid except a fact? And is it not evident that without such expressions, and the ideas corresponding to them, Fourier could neither have conveyed nor conceived his theory?

In concluding this discussion, it must be recollected, that though it is a most narrow and untenable rule to say that we will admit no agency of ethers and fluids into philosophy; yet the reality of such agents is only to be held in the way, and to the extent, which the laws of phenomena indicate. It is not only allowable, but inevitable to assume, as the vehicle of heat and light, a medium possessing some of the properties of more familiar kinds of matter. But the idea of such a medium, which we possess, and on which we cannot but reason, can be fully developed only by an assiduous study of the cases in which it is applicable. It may be, that as science advances, all our knowledge may converge to one general and single aspect of the universe. We abandon and reject this hope, if we refuse to admit those ideas which must be our stepping-stones in advancing to such a point: and we no less frustrate such an expectation, if we allow ourselves to imagine that from our present position we can stride at once to the summit.

But if it is, in the sciences just mentioned, impracticable to reduce our knowledge to laws of phenomena alone, without referring to causes, media, and other agencies; how much more plainly is it impossible to confine our thoughts to phenomena, and to laws of succession and resemblance, in other sciences, as chemistry, physiology, and geology? Who shall forbid us, or why should we be forbidden, to inquire whether chemical and galvanic forces are identical; whether irritability is a peculiar vital power; whether geological causes have been uniform or paroxysmal? To exclude such inquiries, would be to secure ourselves from the poison of errour by abstaining from the banquet of truth:—it would be to attempt to feed our minds with the meagre diet of space and number, because we may find too delightful a relish in such matters as cause and end, symmetry and affinity, organization and developement.

Thus M. Comte's arrangement of the progress of science as successively metaphysical and positive, is contrary to history in fact, and contrary to sound philosophy in principle. Nor is there any better foundation for his statement that theological views are to be found only in the rude infantine condition of human knowledge, and vanish as science advances. Even in material sciences this is not the case. We have shown in the chapter on Final Causes, that physiologists have been directed in their remarks by the conviction of a purpose in every part of the structure of animals; and that this idea, which had its rise *after* the first observations, has gone on constantly gaining strength and clearness, so that it is now the basis of a large portion of the science. We have seen, too, in the Book on the palætiological sciences, that the researches of that class do by no means lead us to reject an origin of the series of events, nor to suppose this origin to be included in the series of natural laws. Science has not at all shown any reason for denying either the creation or the purpose of the universe.

This is true of those aspects of the universe which have become the subjects of rigorous science: but how small a portion of the whole do they form! Especially how minute a proportion does our knowledge bear to our ignorance, if we admit into science, as M. Comte advises, only the laws of phenomena! Even in the best explored fields of science, how few such laws do we know! Meteorology, climate, terrestrial magnetism, the colours and other properties of bodies, the conditions of musical and articulate sound, and a thousand other facts of physics, are not defined by any known laws. In physiology we may readily convince ourselves how little we know of laws, since we can hardly study one species without discovering some unguessed property, or apply the microscope without seeing some new structure in the best known organs. And when we go on to social and moral and political matters, we may well doubt whether any one single rigorous rule of phenomena has ever been stated, although on such subjects man's ideas have been busily and eagerly working ever since his origin. What a wanton and baseless assumption it would be, then, to reject those suggestions of a Governor of the universe which we derive from man's moral and spiritual nature, and from the institutions of society, because we fancy we see in the small field of our existing 'positive knowledge' a tendency to exclude 'theological views!' Because we can explain the motion of the stars by a general Law which seems to imply no hyperphysical agency, and can trace a few more limited laws in other properties of matter, we are exhorted to reject convictions irresistibly suggested to us by our bodies and our souls, by history and antiquities, by conscience and human law.

It is not merely as a speculative doctrine that M. Comte urges the necessity of our thus following the guidance of "positive philosophy." The fevered and revolutionary condition of human society at present arises, according to him*, from the simultaneous employment of three kinds of philosophy radically incompatible;—theological, metaphysical, and positive philosophy. The remedy for the evil is to reject the two former, and to refer everything to that positive philo-

* I. 50.

sophy, of which the destined triumph cannot be doubtful. In like manner, our European education*, still essentially theological, metaphysical, and literary, must be replaced by a *positive* education, suited to the spirit of our epoch.

With these practical consequences of M. Comte's philosophy we are not here concerned: but the notice of them may serve to show how entirely the rejection of the theological view pervades his system; and how closely this rejection is connected with the principles which lead him also to reject the fundamental ideas of the sciences as we have presented them.

In the detail of M. Comte's work, I do not find any peculiar or novel remarks on the induction by which the sciences are formed; except we may notice, as such, his permission of hypotheses to the enquirer, already referred to. "There can only be," he says†, "two general modes fitted to reveal to us, in a direct and entirely rational manner, the true law of any phenomenon;—either the immediate analysis of this phenomenon, or its exact and evident relation to some more extended law, previously established;—in a word, *induction*, or *deduction*. But both these ways would certainly be insufficient, even with regard to the simplest phenomenon, in the eyes of any one who fully comprehends the essential difficulties of the intimate study of nature, if we did not often begin by anticipating the result, and making a provisory supposition, at first essentially conjectural, even with respect to some of the notions which constitute the final object of enquiry. Hence the introduction, which is strictly indispensable, of hypotheses in natural philosophy." We have already seen that the "permissio intellectus" had been noticed as a requisite step in discovery, as long before as the time of Bacon.

* I. 41. † II. 433.

I do not think it necessary to examine in detail M. Comte's views of the philosophy of the different sciences; but it may illustrate the object of the present work, to make a remark upon his attempt to establish a distinction between physical and chemical science. This distinction he makes to consist in three points*;—that Physics considers general and Chemistry special properties;—that Physics considers masses and Chemistry molecules;—that in Physics the mode of arrangement of the molecules remains constant, while in Chemistry this arrangement is necessarily altered. M. Comte however allows that these lines of distinction are vague and insecure; for, among many others, magnetism, a special property, belongs to physics, and breaks down his first criterion; and molecular attractions are a constant subject of speculation in physics, so that the second distinction cannot be insisted on. To which we may add that the greater portion of chemistry does not attend at all to the arrangement of the molecules, so that the third character is quite erroneous. The real distinction of these branches of science is, as we have seen, the fundamental ideas which they employ. Physics deals with relations of space, time, and number, media, and scales of qualities, according to intensity and other differences; while Chemistry has for its subject elements and attractions as shown in composition; and polarity, though in different senses, belongs to both. The failure of this attempt at distinguishing these provinces of science by their objects, may be looked upon as an illustration of the impossibility of establishing a philosophy of the sciences on any other ground than the ideas which they involve.

We have thus traced to its extreme point, so far as the nature of science is concerned, one of those two

* *Phil. Pos.* II. 392—398.

antagonist opinions, of which the struggle began in the outset of philosophy, and has continued during the whole of her progress;—namely, the opinions which respectively make our sensations and our ideas the origin of our knowledge. The former, if it be consistent with itself, must consider all knowledge of causes as impossible, since no sensation can give us the idea of cause. And when this opinion is applied to science, it reduces it to the mere investigation of laws of phenomena, according to relations. I purposely abstain, as far as possible, from the consideration of the other consequences, not strictly belonging to the physical sciences, which were drawn from the doctrine that all our ideas are only transformed sensations. The materialism, the atheism, the sensualist morality, the anarchical polity, which some of the disciples of the Sensational School erected upon the fundamental dogmas of their sect, do not belong to our present subject, and are matters too weighty to be treated of as mere accessories.

BOOK XIII.

OF METHODS EMPLOYED IN THE FORMATION OF SCIENCE.

Chapter I.

INTRODUCTION.

1. In the last Book but one of this work, we pointed out certain general Characters of scientific knowledge which may often serve to distinguish it from opinions of a looser or vaguer kind. In the last Book we traced the steps by which men were led to a perception, more or less clear, of those characteristics; and in the course of this review, we had to consider various precepts and maxims offered by philosophers as fitted to guide us in the pursuit of exact and general truths. Other contributions of the same kind to the philosophy of science might be noticed, and some which contain more valuable suggestions, and indicate a more practical acquaintance with the subject than any which have yet been quoted. Among these, I must especially distinguish Sir John Herschel's *Discourse on the Study of Natural Philosophy*. But my object in this work is not so much to relate the history, as to present the really valuable results of preceding labours. I shall, therefore, proceed no further with the criticism of other authors; but shall endeavour to collect, both from them and from my own researches and reflections, such views and such rules as

seem best adapted to assist us in the discovery and recognition of scientific truth; or, at least, such as may enable us to understand the process by which this truth is obtained. We would present to the reader the Philosophy and, if possible, the Art, of Discovery.

2. But, in truth, we must acknowledge, before we proceed with this subject, that, speaking with strictness, an *Art of Discovery* is not possible;—that we can give no Rules for the pursuit of truth which shall be universally and peremptorily applicable;—and that the helps which we can offer to the inquirer in such cases are limited and precarious. Still, we trust it will be found that aids may be pointed out which are neither worthless nor uninstructive. The mere classification of examples of successful inquiry, to which our rules give occasion, is full of interest for the philosophical speculator. And if our maxims direct the discoverer to no operations which might not have occurred of themselves, they may still concentrate our attention on that which is most important and characteristic in these operations, and may direct us to the best mode of insuring their success. I shall, therefore, attempt to resolve the Process of Discovery into its parts, and to give an account as distinct as may be of Rules and Methods which belong to each portion of the process.

3. In the Eleventh Book we considered the three main parts of the process by which science is constructed: namely, the Decomposition and Observation of Complex Facts; the Explication of our Ideal Conceptions; and the Colligation of Elementary Facts by means of those Conceptions. The first and last of these three steps are capable of receiving additional accuracy by peculiar processes. They may further the advance of science in a more effectual manner, when directed by special technical *Methods*, of which in the present Book we must give a

brief view. In this more technical form, the observation of facts involves the *Measurement of Phenomena;* and the Colligation of Facts includes all arts and rules by which the process of Induction can be assisted. Hence we shall have here to consider *Methods of Observation*, and *Methods of Induction*, using these phrases in the widest sense. The second of the three steps above mentioned, the Explication of our Conceptions, does not admit of being much assisted by methods, although something may be done by Education and Discussion.

4. The Methods of Induction, of which we have to speak, apply only to the first step in our ascent from phenomena to laws of nature;—the discovery of *Laws of Phenomena.* A higher and ulterior step remains behind, and follows in natural order the discovery of Laws of Phenomena; namely, the *Discovery of Causes;* and this must be stated as a distinct and essential process in a complete view of the course of science. Again, when we have thus ascended to the causes of phenomena and of their laws, we can often reason downwards from the cause so discovered; and we are thus led to suggestions of new phenomena, or to new explanations of phenomena already known. Such proceedings may be termed *Applications* of our Discoveries; including in the phrase, *Verifications* of our Doctrines by such an application of them to observed facts. Hence we have the following series of processes concerned in the formation of science.

(1.) Decomposition of Facts;
(2.) Measurement of Phenomena;
(3.) Explication of Conceptions;
(4.) Induction of Laws of Phenomena;
(5.) Induction of Causes;
(6.) Application of Inductive Discoveries.

5. Of these six processes, the methods by which the

second and fourth may be assisted are here our peculiar object of attention. The treatment of these subjects in the present work must necessarily be scanty and imperfect, although we may perhaps be able to add something to what has hitherto been systematically taught on these heads. Methods of Observation and of Induction might of themselves form an abundant subject for a treatise, and hereafter probably will do so, in the hands of future writers. A few remarks, offered as contributions to this subject, may serve to show how extensive it is, and how much more ready it now is than it ever before was, for a systematic discussion.

Of the above steps of the formation of science, the first, the Decomposition of Facts, has already been sufficiently explained in the Eleventh Book: for if we pursue it into further detail and exactitude, we find that we gradually trench upon some of the succeeding parts. I, therefore, proceed to treat of the second step, the Measurement of Phenomena;—of *methods* by which this work, in its widest sense, is executed, and these I shall term Methods of Observation.

CHAPTER II.

OF METHODS OF OBSERVATION.

1. I SHALL speak, in this chapter, of Methods of exact and systematic observation, by which such facts are collected as form the materials of precise scientific propositions. These Methods are very various, according to the nature of the subject inquired into, and other circumstances: but a great portion of them agree in being processes of measurement. These I shall peculiarly con-

sider: and in the first place those referring to Number, Space, and Time, which are at the same time objects and instruments of measurement.

2. But though we have to explain how observations may be made as perfect as possible, we must not forget that in most cases complete perfection is unattainable. *Observations are never perfect.* For we observe phenomena by our senses, and measure their relations in time and space; but our senses and our measures are all, from various causes, inaccurate. If we have to observe the exact place of the moon among the stars, how much of instrumental apparatus is necessary! This apparatus has been improved by many successive generations of astronomers, yet it is still far from being perfect. And the senses of man, as well as his implements, are limited in their exactness. Two different observers do not obtain precisely the same measures of the time and place of a phenomenon; as, for instance, of the moment at which the moon occults a star, and the point of her *limb* at which the occultation takes place. Here, then, is a source of inaccuracy and errour, even in astronomy, where the means of exact observation are incomparably more complete than they are in any other department of human research. In other cases, the task of obtaining accurate measures is far more difficult. If we have to observe the tides of the ocean when rippled with waves, we can see the average level of the water first rise and then fall; but how hard is it to select the exact moment when it is at its greatest height, or the exact highest point which it reaches! It is very easy, in such a case, to err by many minutes in time, and by several inches in space.

Still, in many cases, good Methods can remove very much of this inaccuracy, and to these we now proceed.

3. (I.) *Number.*—Number is the first step of mea-

surement, since it measures itself, and does not, like space and time, require an arbitrary standard. Hence the first exact observations, and the first advances of rigorous knowledge, appear to have been made by means of number; as for example,—the number of days in a month and in a year;—the cycles according to which eclipses occur;—the number of days in the revolutions of the planets; and the like. All these discoveries, as we have seen in the History of Astronomy, go back to the earliest period of the science, anterior to any distinct tradition; and these discoveries presuppose a series, probably a very long series, of observations, made principally by means of number. Nations so rude as to have no other means of exact measurement, have still systems of numeration by which they can reckon to a considerable extent. Very often, such nations have very complex systems, which are capable of expressing numbers of great magnitude. Number supplies the means of measuring other quantities, by the assumption of a *unit* of measure of the appropriate kind: but where nature supplies the unit, number is applicable directly and immediately. Number is an important element in the Classificatory as well as in the Mathematical Sciences. The History of those Sciences shows how the formation of botanical systems was effected by the adoption of number as a leading element by Cæsalpinus; and how afterwards the Reform of Linnæus in classification depended in a great degree on his finding, in the pistils and stamens, a better numerical basis than those before employed. In like manner, the number of rays in the membrane of the gills*, and the number of rays in the fins of fish, were found to be important elements in ichthyological classification by Artedi and Linnæus. There are innumerable instances, in all parts of Natural

* *Hist. Ind. Sci.*, B. XVI. c. vii.

History, of the importance of the observation of number. And in this observation, no instrument, scale or standard is needed, or can be applied; except the scale of natural numbers, expressed either in words or in figures, can be considered as an instrument.

4. (II.) *Measurement of Space.*—Of quantities admitting of *continuous* increase and decrease, (for number is discontinuous,) space is the most simple in its mode of measurement, and requires most frequently to be measured. The obvious mode of measuring space is by the repeated application of a material measure, as when we take a foot-rule and measure the length of a room. And in this case the foot-rule is the *unit* of space, and the length of the room is expressed by the number of such units which it contains: or, as it may not contain an exact number, by a number with a *fraction.* But besides this measurement of linear space, there is another kind of space which, for purposes of science, it is still more important to measure, namely, angular space. The visible heavens being considered as a sphere, the portions and paths of the heavenly bodies are determined by drawing circles on the surface of this sphere, and are expressed by means of the parts of these circles thus intercepted: by such measures the doctrines of astronomy were obtained in the very beginning of the science. The arcs of circles thus measured, are not like linear spaces, reckoned by means of an *arbitrary* unit; for there is a *natural unit*, the total circumference, to which all arcs may be referred. For the sake of convenience, the whole circumference is divided into 360 parts or *degrees;* and by means of these degrees and their parts, all arcs are expressed. The *arcs* are the measures of the *angles at the center*, and the degrees may be considered indifferently as measuring the one or the other of these quantities.

5. In the History of Astronomy*, I have described the method of observation of celestial angles employed by the Greeks. They determined the lines in which the heavenly bodies were seen, by means either of Shadows, or of Sights; and measured the angles between such lines by arcs or rules properly applied to them. The Armill, Astrolabe, Dioptra, and Parallactic Instrument of the ancients, were some of the instruments thus constructed. Tycho Brahe greatly improved the methods of astronomical observation by giving steadiness to the frame of his instruments, (which were large *quadrants*,) and accuracy to the divisions of the *limb*†. But the application of the *telescope* to the astronomical quadrant and the fixation of the center of the field by a *cross* of fine wires placed in the focus, was an immense improvement of the instrument, since it substituted a precise visual ray, pointing to the star, instead of the coarse coincidence of Sights. The accuracy of observation was still further increased by applying to the telescope a *micrometer* which might subdivide the smaller divisions of the arc.

6. By this means, the precision of astronomical observation was made so great, that very minute angular spaces could be measured: and it then became a question whether discrepancies which appeared at first as defects in the theory, might not arise sometimes from a bending or shaking of the instrument, and from the degrees marked on the limb being really somewhat unequal, instead of being rigorously equal. Accordingly, the framing and balancing of the instrument, so as to avoid all possible tremor or flexure, and the exact division of an arc into equal parts, became great objects of those who wished to improve astronomical observations. The observer no longer gazed at the stars from a lofty

* *Hist. Ind. Sci.*, B. III. c. iv. sect. 3. † *Ibid.*, B. VII. c. vi. sect. 1.

tower, but placed his telescope on the solid ground, and braced and balanced it with various contrivances. Instead of a quadrant, an entire circle was introduced (by Ramsden;) and various processes were invented for the dividing of instruments. Among these we may notice Troughton's method of dividing; in which the visual ray of a microscope was substituted for the points of a pair of compasses, and, by *stepping* round the circle, the partial arcs were made to bear their exact relation to the whole circumference.

7. Astronomy is not the only science which depends on the measurement of angles. Crystallography also requires exact measures of this kind; and the *goniometer*, especially that devised by Wollaston, supplies the means of obtaining such measures. The science of Optics also, in many cases, requires the measurement of angles.

8. In the measurement of linear space, there is no natural standard which offers itself. Most of the common measures appear to be taken from some part of the human body; as a *foot*, a *cubit*, a *fathom*; but such measures cannot possess any precision, and are altered by convention: thus there were in ancient times many kinds of cubits; and in modern Europe, there are a great number of different standards of the foot, as the Rhenish foot, the Paris foot, the English foot. It is very desirable that, if possible, some permanent standard, founded in nature, should be adopted; for the conventional measures are lost in the course of ages; and thus, dimensions expressed by means of them become unintelligible. Two different natural standards have been employed in modern times: the French have referred their measures of length to the total circumference of a meridian of the earth; a quadrant of this meridian consists of ten million units or *metres*. The English

have fixed their linear measure by reference to the length of a pendulum which employs an exact second of time in its small oscillation. Both these methods occasion considerable difficulties in carrying them into effect; and are to be considered mainly as means of recovering the standard if it should ever be lost. For common purposes, some material standard is adopted as authority for the time: for example, the standard which in England possessed legal authority up to the year 1835 was preserved in the House of Parliament; and was lost in the conflagration which destroyed that edifice. The standard of length now generally referred to by men of science in England is that which is in the possession of the Astronomical Society of London.

9. A standard of length being established, the artifices for applying it, and for subdividing it in the most accurate manner, are nearly the same as in the case of measures of arcs: as for instance, the employment of the visual rays of microscopes instead of the legs of compasses and the edges of rules; the use of micrometers for minute measurements; and the like. Many different modes of avoiding errour in such measurements have been devised by various observers, according to the nature of the cases with which they had to deal*.

10. (III.) *Measurement of Time.*—The methods of measuring Time are not so obvious as the methods of measuring space; for we cannot apply one portion of time to another, so as to test their equality. We are obliged to begin by assuming some change as the measure of time. Thus the motion of the sun in the sky, or the length and position of the shadows of objects, were the first modes of measuring the parts of the day. But

* On the precautions employed in astrononomical instruments for the measure of space, see Sir J. Herschel's *Astronomy*, (in the *Cabinet Cyclopædia*,) Arts. 103—110.

what assurance had men, or what assurance could they have, that the motion of the sun or of the shadow was uniform? They could have no such assurance, till they had adopted some measure of smaller times; which smaller times, making up larger times by repetition, they took as the standard of uniformity;—for example, an hour-glass, or a clepsydra which answered the same purpose among the ancients. There is no apparent reason why the successive periods measured by the emptying of the hour-glass should be unequal; they are implicitly accepted as equal; and by reference to these, the uniformity of the sun's motion may be verified. But the great improvement in the measurement of time was the use of a pendulum for the purpose by Galileo, and the application of this device to clocks by Huyghens in 1656. For the successive oscillations of a pendulum are rigorously equal, and a clock is only a train of machinery employed for the purpose of counting these oscillations. By means of this invention, the measure of time in astronomical observations became as accurate as the measure of space.

11. What is the *natural unit* of time? It was assumed from the first by the Greek astronomers, that the sidereal days, measured by the revolution of a star from any meridian to the same meridian again, are exactly equal; and all improvements in the measure of time tended to confirm this assumption. The sidereal day is therefore the natural standard of time. But the solar day, determined by the diurnal revolution of the sun, although not rigorously invariable, as the sidereal day is, undergoes scarcely any perceptible variation; and since the course of daily occurrences is regulated by the sun, it is far more convenient to seek the basis of our unit of time in *his* motions. Accordingly the solar day (the *mean* solar day) is divided into 24 hours, and these, into minutes and seconds; and this is our scale of time.

Of such time, the sidereal day has 23 hours 56 minutes 4·09 seconds. And it is plain that by such a statement the length of the hour is fixed, with reference to a sidereal day. The *standard* of time (and the standard of space in like manner) equally answers its purpose, whether or not it coincides with any *whole number* of units.

12. Since the sidereal day is thus the standard of our measures of time, it becomes desirable to refer to it, constantly and exactly, the instruments by which time is measured, in order that we may secure ourselves against errour. For this purpose, in astronomical observatories, observations are constantly made of the transit of stars across the meridian; the *transit instrument* with which this is done being adjusted with all imaginable regard to accuracy*.

13. When exact measures of time are required in other than astronomical observations, the same instruments are still used, namely, clocks and chronometers. In chronometers, the regulating part is an oscillating body; not, as in clocks, a pendulum oscillating by the force of gravity, but a wheel swinging to and fro on its center, in consequence of the vibrations of a slender coil of elastic wire. To divide time into still smaller portions than these vibrations, other artifices are used; some of which will be mentioned under the next head.

14. (IV.) *Conversion of Space and Time.*—Space and time agree in being extended quantities, which are made up and measured by the repetition of homogeneous parts. If a body move uniformly, whether in the way of revolving or otherwise, the *space* which any point describes, is *proportional* to the *time* of its motion; and the space and the time may each be taken as a measure

* On the precautions employed in the measure of time by astronomers, see Herschel's *Astron.*, Art. 115—127.

of the other. Hence in such cases, by taking space instead of time, or time instead of space, we may often obtain more convenient and precise measures, than we can by measuring directly the element with which we are concerned.

The most prominent example of such a conversion, is the measurement of the Right Ascension of stars, (that is, their angular distance from a standard meridian* on the celestial sphere,) by means of the time employed in their coming to the meridian of the place of observation. Since, as we have already stated, the visible celestial sphere, carrying the fixed stars, revolves with perfect uniformity about the pole; if we observe the stars as they come in succession to a fixed circle passing through the poles, the intervals of time between these observations will be proportional to the angles which the meridian circles passing through these stars make at the poles where they meet; and hence, if we have the means of measuring time with great accuracy, we can, by watching the *times* of the transits of successive stars across some visible mark in our own meridian, determine the *angular distances* of the meridian circles of all the stars from one another.

Accordingly, now that the pendulum clock affords astronomers the means of determining time exactly, a measurement of the Right Ascensions of heavenly bodies by means of a clock and a transit instrument, is a part of the regular business of an observatory. If the sidereal clock be so adjusted that it marks the beginning of its scale of time when the first point of Right Ascension is upon the visible meridian of our observatory, the point of the scale at which the clock points when any other

* A *meridian* is a circle passing through the poles about which the celestial sphere revolves. The meridian *of any place* on the earth is that meridian which is exactly over the place.

star is in our meridian, will truly represent the Right Ascension of the star.

Thus as the motion of the stars is our measure of time, we employ time, conversely, as our measure of the places of the stars. The celestial machine and our terrestrial machines correspond to each other in their movements; and the star steals silently and steadily across our meridian line, just as the pointer of the clock steals past the mark of the hour. We may judge of the scale of this motion by considering that the full moon employs about two minutes of time in sailing across any fixed line seen against the sky, transverse to her path: and all the celestial bodies, carried along by the revolving sphere, travel at the same rate.

15. In this case, up to a certain degree, we render our measures of astronomical angles more exact and convenient by substituting time for space; but when, in the very same kind of observation, we wish to proceed to a greater degree of accuracy, we find that it is best done by substituting space for time. In observing the transit of a star across the meridian, if we have the clock within hearing, we can count the beats of the pendulum by the noise which they make, and tell exactly at which second of time the passage of the star across the visible thread takes place; and thus we measure Right Ascension by means of time. But our perception of time does not allow us to divide a second into ten parts, and to pronounce whether the transit takes place three-tenths, six-tenths, or seven-tenths of a second after the preceding beat of the clock. This, however, can be done by the usual mode of observing the transit of a star. The observer, listening to the beat of his clock, fastens his attention upon the star at each beat, and especially at the one immediately before and the one immediately after the passage of the thread: and by this means he

has these two positions and the positions of the thread so far present to his intuition at once, that he can judge in what proportion the thread is nearer to one position than the other, and can thus divide the intervening second in its due proportion. Thus if he observe that at the beginning of the second the star is on one side of the thread, and at the end of the second on the other side; and that the two distances from the thread are as two to three, he knows that the transit took place at two-fifths (or four-tenths) of a second after the former beat. In this way a second of time in astronomical observations may, by a skilful observer, be divided into ten equal parts; although when time is observed as time, a tenth of a second appears almost to escape our senses. From the above explanation, it will be seen that the reason why the subdivision is possible in the way thus described, is this:—that the moment of time thus to be divided is so small, that the eye and the mind can retain, to the end of this moment, the impression of position which it received at the beginning. Though the two positions of the star, and the intermediate thread, are seen successively, they can be contemplated by the mind as if they were seen simultaneously: and thus it is precisely the smallness of this portion of time which enables us to subdivide it by means of space.

16. There is another case, of somewhat a different kind, in which time is employed in measuring space; namely, when space, or the standard of space, is defined by the length of a pendulum oscillating in a given time. We might in this way define any space by the time which a pendulum of such a length would take in oscillating; and thus we might speak, as was observed by those who suggested this device, of five minutes of cloth, or a rope half an hour long. We may observe, however, that in this case, the space is *not proportional* to the time. And

we may add, that though we thus appear to avoid the arbitrary standard of space (for as we have seen, the standard of measures of time is a natural one,) we do not do so in fact: for we assume the invariableness of gravity, which really varies (though very slightly,) from place to place.

17. (V.) *The Method of Repetition in Measurement.* —In many cases we can give great additional accuracy to our measurements by repeatedly adding to itself the quantity which we wish to measure. Thus if we wished to ascertain the exact breadth of a thread, it might not be easy to determine whether it was one-ninetieth, or one-ninety-fifth, or one-hundredth part of an inch; but if we find that ninety-six such threads placed side by side occupy exactly an inch, we have the precise measure of the breadth of the thread. In the same manner, if two clocks are going nearly at the same rate, we may not be able to distinguish the excess of an oscillation of one of the pendulums over an oscillation of the other: but when the two clocks have gone for an hour, one of them may have gained ten seconds upon the other; thus showing that the proportion of their times of oscillation is 3610 to 3600.

In the latter of these instances, we have the principle of repetition truly exemplified, because (as has been justly observed by Sir J. Herschel*,) there is then "a juxtaposition of units without errour,"—"one vibration commences exactly where the last terminates, no part of time being lost or gained in the addition of the units so counted." In space, this juxtaposition of units without errour cannot be rigorously accomplished, since the units must be added together by material contact (as in the case of the threads,) or in some equivalent manner. Yet the principle of repetition has been applied to angular

* *Disc. Nat. Phil.*, Art. 121.

measurement with considerable success in Borda's Repeating Circle. In this instrument, the angle between two objects which we have to observe, is repeated along the graduated limb of the circle by turning the telescope from one object to the other, alternately fastened to the circle (by its *clamp*) and loose from it (by unclamping). In this manner the errours of graduation may (theoretically) be entirely got rid of: for if an angle repeated *nine* times be found to go twice round the circle, it must be *exactly* eighty degrees: and where the repetition does not give an exact number of circumferences, it may still be made to subdivide the errour to any required extent.

18. Connected with the principle of repetition, is the *Method of coincidences* or *interferences*. If we have two Scales, on one of which an inch is divided into 10, and on the other into 11 equal parts; and if, these Scales being placed side by side, it appear that the beginning of the latter Scale is between the 2nd and 3rd division of the former, it may not be apparent what fraction added to 2 determines the place of beginning of the second Scale as measured on the first. But if it appear also that the 3rd division of the second Scale *coincides* with a certain division of the first, (the 5th,) it is certain that 2 and *three-tenths* is the *exact* place of the beginning of the second Scale, measured on the first Scale. The 3rd division of the 11 Scale will coincide (or interfere with) a division of the 10 Scale, when the beginning or *zero* of the 11 divisions is three-tenths of a division beyond the preceding line of the 10 Scale; as will be plain on a little consideration. And if we have two Scales of equal units, in which each unit is divided into nearly, but not quite, the same number of equal parts (as 10 and 11, 19 and 20, 29 and 30,) and one sliding on the other, it will always happen that some one or other of the division lines will coincide, or very nearly coincide; and thus the

exact position of the beginning of one unit, measured on the other scale, is determined. A sliding scale, thus divided for the purpose of subdividing the units of that on which it slides, is called a *Vernier*, from the name of its inventor.

19. The same Principle of Coincidence or Interference is applied to the exact measurement of the length of time occupied in the oscillation of a pendulum. If a detached pendulum, of such a length as to swing in little less than a second, be placed before the seconds' pendulum of a clock, and if the two pendulums begin to move together, the former will gain upon the latter, and in a little while their motions will be quite discordant. But if we go on watching, we shall find them, after a time, to agree again exactly; namely, when the detached pendulum has gained one complete oscillation (back and forwards,) upon the clock pendulum, and again coincides with it in its motion. If this happen after 5 minutes, we know that the times of oscillation of the two pendulums are in the proportion of 300 to 302, and therefore the detached pendulum oscillates in $\frac{150}{151}$ of a second. The accuracy which can be obtained in the measure of an oscillation by this means is great; for the clock can be compared (by observing transits of the stars or otherwise) with the natural standard of time, the sidereal day. And the moment of coincidence of the two pendulums may, by proper arrangements, be very exactly determined.

We have hitherto spoken of methods of measuring time and space, but other elements also may be very precisely measured by various means.

20. (VI.) *Measurement of Weight.*—Weight, like space and time, is a quantity made up by addition of parts, and may be measured by similar methods. The principle of repetition is applicable to the measurement of weight; for if two bodies be put in the same pan of a

balance and balances the same pieces in the other pan, their weights are exactly added.

There may be difficulties of practical workmanship in carrying into effect the mathematical conditions of a perfect balance; for example, in securing an exact equality of the effective arms of the beam in all positions. These difficulties are evaded by the *Method of double weighing*; according to which the standard weights, and the body which is to be weighed, are successively put in the *same* pan, and made to balance by a third body in the opposite scale. By this means the different lengths of the arms of the beam, and other imperfections of the balance, become of no consequence*.

21. There is no natural *Standard* of weight. The conventional weight taken as the standard, is the weight of a given bulk of some known substance; for instance, a *cubic foot of water*. But in order that this may be definite, the water must not contain any portion of heterogeneous substance: hence it is required that the water be *distilled* water.

22. (VII.) *Measurement of Secondary Qualities.*—We have already seen† that secondary qualities are estimated by means of conventional Scales, which refer them to space, number, or some other definite expression. Thus the Thermometer measures heat; the Musical Scale, with or without the aid of number, expresses the pitch of a note; and we may have an exact and complete Scale of Colours, pure and impure. We may remark, however, that with regard to sound and colour, the estimates of the ear and the eye are not superseded, but only assisted: for if we determine what a note is, by

* For other methods of measuring weights acccurately, see Faraday's *Chemical Manipulation*, p. 25.

† Book III. c. ii. Of the Measure of Secondary Qualities.

comparing it with an instrument known to be in tune, we still leave the ear to decide when the note is *in unison* with one of the notes of the instrument. And when we compare a colour with our chromatometer, we judge by the eye which division of the chromatometer it *matches*. Colour and sound have their Natural Scales, which the eye and ear habitually apply; what science requires is, that those scales should be systematized. We have seen that several conditions are requisite in such scales of qualities: the observer's skill and ingenuity are mainly shown in devising such scales and methods of applying them.

23. The Method of Coincidences is employed in harmonics: for if two notes are nearly, but not quite, in unison, the coincidences of the vibrations produce an audible undulation in the note, which is called the *howl*; and the exactness of the unison is known by this howl vanishing.

24. (VIII.) *Manipulation.*—The process of applying practically methods of experiment and observation, is termed Manipulation; and the value of observations depends much upon the proficiency of the observer in this art. This skill appears, as we have said, not only in devising means and modes of measuring results, but also in inventing and executing arrangements by which elements are subjected to such conditions as the investigation requires: in finding and using some material combination by which nature shall be asked the question which we have in our minds. To do this in any subject may be considered as a peculiar Art, but especially in Chemistry; where "many experiments, and even whole trains of research, are essentially dependent for success on mere manipulation*." The changes which the chemist has to study,—compositions, decompositions, and mutual actions,

* Faraday's *Chemical Manipulation*, p. 3.

affecting the internal structure rather than the external form and motion of bodies,—are not familiarly recognized by common observers, as those actions are which operate upon the total mass of a body: and hence it is only when the chemist has become, to a certain degree, familiar with his science, that he has the power of observing. He must learn to interpret the effects of mixture, heat, and other chemical agencies, so as to see in them those facts which chemistry makes the basis of her doctrines. And in learning to interpret this language, he must also learn to call it forth;—to place bodies under the requisite conditions, by the apparatus of his own laboratory and the operations of his own fingers. To do this with readiness and precision, is, as we have said, an Art, both of the mind and of the hand, in no small degree recondite and difficult. A person may be well acquainted with all the doctrines of chemistry, and may yet fail in the simplest experiment. How many precautions and observances, what resource and invention, what delicacy and vigilance, are requisite in Chemical Manipulation, may be seen by reference to Dr. Faraday's work on that subject.

25. The same qualities in the observer are requisite in some other departments of science; for example, in the researches of Optics: for in these, after the first broad facts have been noticed, the remaining features of the phenomena are both very complex and very minute; and require both ingenuity in the invention of experiments, and a keen scrutiny of their results. We have instances of the application of these qualities in most of the optical experimenters of recent times, and certainly in no one more than Sir David Brewster. Omitting here all notice of his succeeding labours, his *Treatise on New Philosophical Instruments*, published in 1813, is an excellent model of the kind of resource and skill of

which we now speak. I may mention as an example of this skill, his mode of determining the refractive power of an *irregular* fragment of any transparent substance. At first this might appear an impossible problem; for it would seem that a regular and smooth surface are requisite, in order that we may have any measurable refraction. But Sir David Brewster overcame the difficulty by immersing the fragment in a combination of fluids, so mixed, that they had the same refractive power as the specimen. The question, *when* they had this power, was answered by noticing when the fragment became so transparent that its surface could hardly be seen; for this happened when, the refractive power within and without the fragment being the same, there was no refraction at the surface. And this condition being obtained, the refractive power of the fluid, and therefore of the fragment, was easily ascertained.

26. (IX.) *The Education of the Senses.*—Colour and Musical Tone are, as we have seen, determined by means of the Senses, whether or not Systematical Scales are used in expressing the observed fact. Systematical Scales of sensible qualities, however, not only give precision to the record, but to the observation. But for this purpose such an Education of the Senses is requisite as may enable us to apply the scale immediately. The memory must retain the sensation or perception to which the technical term or degree of the scale refers. Thus with regard to colour, as we have said already*, when we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to occur at once to our recollection without delay or search. The observer's senses, therefore, must be educated, at first by an actual exhibition of the standard, and afterwards by a familiar use of it, to understand readily and clearly each phrase

* Book VIII. c. iii. Terminology.

and degree of the scales which in his observations he has to apply. This is not only the best, but in many cases the only way in which the observation can be expressed. Thus *glassy lustre, fatty lustre, adamantine lustre,* denote certain kinds of shining in minerals, which appearances we should endeavour in vain to describe by periphrasis; and which the terms, if considered as terms in common language, would by no means clearly discriminate: for who, in common language, would say that coal has a fatty lustre? But these terms, in their conventional sense, are perfectly definite; and when the eye is once familiarized with this application of them, are easily and clearly intelligible.

27. The education of the senses, which is thus requisite in order to understand well the terminology of any science, must be acquired by an inspection of the objects which the science deals with; and is, perhaps, best promoted by the practical study of Natural History. In the different departments of Natural History, the descriptions of species are given by means of an extensive technical *terminology*: and that education of which we now speak, ought to produce the effect of making the observer as familiar with each of these terms as we are with the words of our common language. The technical terms have a much more precise meaning than other terms, since they are defined by express convention, and not learnt by common usage merely. Yet though they are thus defined, not the definition, but the perception itself, is that which the term suggests to the proficient.

In order to use the terminology to any good purpose, the student must possess it, not as a dictionary, but as a language. The terminology of his sciences must be the natural historian's most familiar tongue. He must learn to think in such language. And when this is achieved, the terminology, as I have elsewhere said, though to an uneducated eye cumbrous and pedantical, is felt to be a useful implement, not an oppressive burden*. The impatient schoolboy looks upon his grammar and vocabulary as irksome and burdensome; but the accomplished student who has learnt the language by means of them, knows that they have given him the means of expressing what he thinks, and even of thinking more precisely. And as the study of language thus gives precision to the thoughts, the study of Natural History, and especially of the descriptive part of it, gives precision to the senses.

The Education of the Senses is also greatly promoted by the practical pursuit of any science of experiment and observation, as chemistry or astronomy. The methods of manipulating, of which we have just spoken, in chemistry, and the methods of measuring extremely minute portions of space and time which are employed in astronomy, and which are described in the former part of this chapter, are among the best modes of educating the senses for purposes of scientific observation.

28. By the various Methods of precise observation which we have thus very briefly described, facts are collected, of an exact and definite kind; they are then bound together in general laws, by the aid of general ideas and of such methods as we have now to consider. It is true, that the ideas which enable us to combine facts into general propositions, do commonly operate in our minds while we are still engaged in the office of observing. Ideas of one kind or other are requisite to connect our phenomena into facts, and to give meaning to the terms of our descriptions: and it frequently happens, that long before we have collected all the facts which induction requires, the mind catches the suggestion which some of these ideas offer, and leaps forwards to a conjectural law

* *Hist. Ind. Sci.*, B. XVI. c. iv. sect. 2.

while the labour of observation is yet unfinished. But though this actually occurs, it is easy to see that the process of combining and generalizing facts is, in the order of nature, posterior to, and distinct from, the process of observing facts. Not only is this so, but there is an intermediate step which, though inseparable from all successful generalization, may be distinguished from it in our survey; and may, in some degree, be assisted by peculiar methods. To the consideration of such methods we now proceed.

Chapter III.

OF METHODS OF ACQUIRING CLEAR SCIENTIFIC IDEAS; *and first* OF INTELLECTUAL EDUCATION.

The ways in which men become masters of those clear and yet comprehensive conceptions wich the formation and reception of science require, are mainly two; which, although we cannot reduce them to any exact scheme, we may still, in a loose use of the term, call *Methods* of acquiring clear Ideas. These two ways are Education and Discussion.

1. (I.) *Idea of Space.*—It is easily seen that Education may do at least something to render our ideas distinct and precise. To learn Geometry in youth, tends, manifestly, to render our idea of space clear and exact. By such an education, all the relations, all the consequences of this idea, come to be readily and steadily apprehended; and thus it becomes easy for us to understand portions of science which otherwise we should by no means be able to comprehend. The conception of *similar triangles* was to be mastered, before the disciples of Thales could see

the validity of his method of determining the height of lofty objects by the length of their shadows. The conception of *the sphere with its circles* had to become familiar, before the annual motion of the sun and its influence upon the lengths of days could be rightly traced. The properties of circles, combined with the *pure* doctrine of motion*, were required as an introduction to the theory of Epicycles: the properties of *conic sections* were needed, as a preparation for the discoveries of Kepler. And not only was it necessary that men should possess a *knowledge* of certain figures and their properties; but it was equally necessary that they should have the *habit of reasoning* with perfect steadiness, precision, and conclusiveness concerning the relations of space. No small discipline of the mind is requisite, in most cases, to accustom it to go, with complete insight and security, through the demonstrations respecting intersecting planes and lines, dihedral and trihedral angles, which occur in solid geometry. Yet how absolutely necessary is a perfect mastery of such reasonings, to him who is to explain the motions of the moon in latitude and longitude! How necessary, again, is the same faculty to the student of crystallography! Without mathematical habits of conception and of thinking, these portions of science are perfectly inaccessible. But the early study of plane and solid geometry gives to all tolerably gifted persons, the habits which are thus needed. The discipline of following the reasonings of didactic works on this subject, till we are quite familiar with them, and of devising for ourselves reasonings of the same kind, (as, for instance, the solutions of problems proposed,) soon gives the mind the power of *discoursing* with perfect facility concerning the most complex and multiplied relations of space, and enables us to refer to the properties of all plane and solid figures as surely as

* See Book II. c. xiii.

to the visible forms of objects. Thus we have here a signal instance of the efficacy of education in giving to our Conceptions that clearness, which the formation and existence of science indispensably require.

2. It is not my intention here to enter into the details of the form which should be given to education, in order that it may answer the purposes now contemplated. But I may make a remark, which the above examples naturally suggest, that in a mathematical education, considered as a preparation for furthering or understanding physical science, Geometry is to be cultivated, far rather than Algebra:—the properties of space are to be studied and reasoned upon as they are in themselves, not as they are replaced and disguised by symbolical representations. It is true, that when the student is become quite familiar with elementary geometry, he may often enable himself to deal in a more rapid and comprehensive manner with the relations of space, by using the language of symbols and the principles of symbolical calculation: but this is an ulterior step, which may be added to, but can never be substituted for, the direct cultivation of geometry. The method of symbolical reasoning employed upon subjects of geometry and mechanics, has certainly achieved some remarkable triumphs in the treatment of the theory of the universe. These successful applications of symbols in the highest problems of physical astronomy appear to have made some teachers of mathematics imagine that it is best to *begin* the pupil's course with such symbolical generalities. But this mode of proceeding will be so far from giving the student clear ideas of mathematical relations, that it will involve him in utter confusion, and probaby prevent his ever obtaining a firm footing in geometry. To commence mathematics in such a way, would be much as if we should begin the study of a language by reading the highest strains of its lyrical poetry.

3. (II.) *Idea of Number, &c.*—The study of mathe-

matics, as I need hardly observe, develops and renders exact, our conceptions of the relations of number, as well as of space. And although, as we have already noticed, even in their original form the conceptions of number are for the most part very distinct, they may be still further improved by such discipline. In complex cases, a methodical cultivation of the mind in such subjects is needed: for instance, questions concerning cycles, and intercalations, and epacts, and the like, require very great steadiness of arithmetical apprehension in order that the reasoner may deal with them rightly. In the same manner, a mastery of problems belonging to the science of Pure Motion, or, as I have termed it, Mechanism, requires either great natural aptitude in the student, or a mind properly disciplined by suitable branches of mathematical study.

4. Arithmetic and Geometry have long been standard portions of the education of cultured persons throughout the civilized world; and hence all such persons have been able to accept and comprehend those portions of science which depend upon the idea of space: for instance, the doctrine of the globular form of the earth, with its consequences, such as the measures of latitude and longitude;—the heliocentric system of the universe in modern, or the geocentric in ancient times;—the explanation of the rainbow; and the like. In nations where there is no such education, these portions of science cannot exist as a part of the general stock of the knowledge of society, however intelligently they may be pursued by single philosophers dispersed here and there in the community.

5. (III.) *Idea of Force.*—As the idea of Space is brought out in its full evidence by the study of Geometry, so the idea of Force is called up and developed by the study of the science of Mechanics. It has already been shown, in our scrutiny of the Ideas of the Mechanical

Sciences, that Force, the Cause of motion or of equilibrium, involves an independent Fundamental Idea, and is quite incapable of being resolved into any mere modification of our conceptions of space, time, and motion. And in order that the student may possess this idea in a precise and manifest shape, he must pursue the science of Mechanics in the mode which this view of its nature demands;—that is, he must study it as an independent science, resting on solid elementary principles of its own, and not built upon some other unmechanical science as its substructure. He must trace the truths of Mechanics from their own axioms and definitions; these axioms and definitions being considered as merely means of bringing into play the Idea on which the science depends. The conceptions of force and matter, of action and reaction, of momentum and inertia, with the reasonings in which they are involved, cannot be evaded by any substitution of lines or symbols for the conceptions. Any attempts at such substitution would render the study of Mechanics useless as a preparation of the mind for physical science; and would, indeed, except counteracted by great natural clearness of thought on such subjects, fill the mind with confused and vague notions, quite unavailing for any purposes of sound reasoning. But, on the other hand, the study of Mechanics, in its genuine form, as a branch of education, is fitted to give a most useful and valuable precision of thought on such subjects; and is the more to be recommended, since, in the general habits of most men's minds, the mechanical conceptions are tainted with far greater obscurity and perplexity than belongs to the conceptions of number, space, and motion.

6. As habitually distinct conceptions of space and motion were requisite for the reception of the doctrines of formal astronomy, (the Ptolemaic and Copernican

system,) so a clear and steady conception of force is indispensably necessary for understanding the Newtonian system of physical astronomy. It may be objected that the study of Mechanics as a science has not commonly formed part of a liberal education in Europe, and yet that educated persons have commonly accepted the Newtonian system. But to this we reply, that although most persons of good intellectual culture have professed to assent to the Newtonian system of the universe, yet they have, in fact, entertained it in so vague and perplexed a manner as to show very clearly that a better mental preparation than the usual one is necessary; in order that such persons may really understand the doctrine of universal attraction. I have already spoken of the prevalent indistinctness of mechanical conceptions*; and need not here dwell upon the indications, constantly occurring in conversation and in literature, of the utter inaccuracy of thought on such subjects which may often be detected; for instance, in the mode in which many men speak of centrifugal and centripetal forces;—of projectile and central forces;—of the effect of the moon upon the waters of the ocean; and the like. The incoherence of ideas which we frequently witness on such points, shows us clearly that, in the minds of a great number of men, well educated according to the present standard, the acceptance of the doctrine of universal gravitation is a result of traditional prejudice, not of rational conviction. And those who are Newtonians on such grounds, are not at all more intellectually advanced by being Newtonians in the nineteenth century, than they would have been by being Ptolemaics in the fifteenth.

7. It is undoubtedly in the highest degree desirable that all great advances in science should become the

* B. III. c. X.

common property of all cultivated men. And this can only be done by introducing into the course of a liberal education such studies as unfold and fix in men's minds the fundamental ideas upon which the new-discovered truths rest. The progress made by the ancients in geography, astronomy, and other sciences, led them to assign, wisely and well, a place to arithmetic and geometry among the steps of an ingenuous education. The discoveries of modern times have rendered these steps still more indispensable; for we cannot consider a man as cultivated up to the standard of his times, if he is not only ignorant of, but incapable of comprehending, the greatest achievements of the human intellect. And as innumerable discoveries of all ages have thus secured to Geometry her place as a part of good education, so the great discoveries of Newton make it proper to introduce Elementary Mechanics as a part of the same course. If the education deserve to be called good, the pupil will not remain ignorant of those discoveries, the most remarkable extensions of the field of human knowledge which have ever occurred. Yet he cannot by possibility comprehend them, except his mind be previously disciplined by mechanical studies. The period appears now to be arrived when we may venture, or rather when we are bound to endeavour, to include a new class of fundamental ideas in the elementary discipline of the human intellect. This is indispensable, if we wish to educe the powers which we know that it possesses, and to enrich it with the wealth which lies within its reach*.

8. By the view which is thus presented to us of the nature and objects of intellectual education, we are led to consider the mind of man as undergoing a progress from age to age. By the discoveries which are made,

* The University of Cambridge has, by a recent law, made an examination in Elementary Mechanics requisite for the Degree of B. A.

and by the clearness and evidence which, after a time, (not suddenly nor soon,) the truths thus discovered acquire, one portion of knowledge after another becomes elementary; and if we would really secure this progress, and make men share in it, these new portions must be treated as elementary in the constitution of a liberal education. Even in the rudest forms of intelligence, man is immeasurably elevated above the unprogressive brute, for the idea of number is so far developed that he can count his flock or his arrows. But when number is contemplated in a speculative form, he has made a vast additional progress; when he steadily apprehends the relations of space, he has again advanced; when in thought he carries these relations into the vault of the sky, into the expanse of the universe, he reaches a higher intellectual position. And when he carries into these wide regions, not only the relations of space and time, but of cause and effect, of force and reaction, he has again made an intellectual advance; which, wide as it is at first, is accessible to all; and with which all should acquaint themselves, if they really desire to prosecute with energy the ascending path of truth and knowledge which lies before them. This should be an object of exertion to all ingenuous and hopeful minds. For, that exertion is necessary,—that after all possible facilities have been afforded, it is still a matter of toil and struggle to appropriate to ourselves the acquisitions of great discoverers, is not to be denied Elementary mechanics, like elementary geometry, is a study accessible to all: but like that too, or perhaps more than that, it is a study which requires effort and contention of mind,—a forced steadiness of thought. It is long since one complained of this labour in geometry; and was answered that in that region there is no *Royal Road*. The same is true of Mechanics, and must be true of all branches of solid education. But we should express the truth more appropriately in our days by saying that there is no *Popular Road* to these sciences. In the mind, as in the body, strenuous exercise alone can give strength and activity. The art of exact thought can be acquired only by the labour of close thinking.

9. (IV.) *Chemical Ideas.*—We appear then to have arrived at a point of human progress in which a liberal education of the scientific intellect should include, besides arithmetic, elementary geometry and mechanics. The question then occurs to us, whether there are any other Fundamental Ideas, among those belonging to other sciences, which ought also to be made part of such an education;—whether, for example, we should strive to develope in the minds of all cultured men the ideas of *polarity*, mechanical and chemical, of which we spoke in a former part of this work.

The views to which we have been conducted by the previous inquiry lead us to reply that it would not be well at present to make *chemical* Polarities, at any rate, a subject of elementary instruction. For even the most profound and acute philosophers who have speculated upon this subject,—they who are leading the van in the march of discovery,—do not seem yet to have reduced their thoughts on this subject to a consistency, or to have taken hold of this idea of Polarity in a manner quite satisfactory to their own minds. This part of the subject is, therefore, by no means ready to be introduced into a course of general elementary education; for, with a view to such a purpose, nothing less than the most thoroughly luminous and transparent condition of the idea will suffice. Its whole efficacy, as a means and object of disciplinal study, depends upon there being no obscurity, perplexity, or indefiniteness with regard to it, beyond that transient deficiency which at first exists in the learner's mind, and is to be removed by his studies. The idea of chemical Polarity is not yet in this condition; and therefore is not yet fit for a place in education. Yet since this idea of Polarity is the most general idea which enters into chemistry, and appears to be that which includes almost all the others, it would be unphilosophical, and inconsistent with all sound views of science, to introduce into education some chemical conceptions, and to omit those which depend upon this idea: indeed such a partial adoption of the science could hardly take place without not only omitting, but misrepresenting, a great part of our chemical knowledge. The conclusion to which we are necessarily led, therefore, is this:—that at present chemistry cannot with any advantage, form a portion of the general intellectual education*.

10. (V.) *Natural-History Ideas.*—But there remains still another class of Ideas, with regard to which we may very properly ask whether they may not advantageously form a portion of a liberal education: I mean the Ideas of definite Resemblance and Difference, and of one set of resemblances subordinate to another, which form the bases of the classificatory sciences. These Ideas are developed by the study of the various branches of Natural History, as Botany, and Zoology; and beyond all doubt, those pursuits, if assiduously followed, very materially affect the mental habits. There is this obvious advantage to be looked for from the study of Natural History, considered as a means of intellectual disci-

* I do not here stop to prove that an education (if it be so called) in which the memory only retains the verbal expression of results, while the mind does not apprehend the principles of the subject, and therefore cannot even understand the words in which its doctrines are expressed, is of no value whatever to the intellect, but rather, is highly hurtful to the habits of thinking and reasoning.

pline:—that it gives us, in a precise and scientific form, examples of the classing and naming of objects; which operations the use of common language leads us constantly to perform in a loose and inexact way. In the usual habits of our minds and tongues, things are distinguished or brought together, and names are applied, in a manner very indefinite, vacillating, and seemingly capricious: and we may naturally be led to doubt whether such defects can be avoided;—whether exact distinctions of things, and rigorous use of words be possible. Now upon this point we may receive the instruction of Natural History; which proves to us, by the actual performance of the task, that a precise classification and nomenclature are attainable, at least for a mass of objects all of the same kind. Further, we also learn from this study, that there may exist, not only an exact distinction of kinds of things, but a series of distinctions, one set subordinate to another, and the more general including the more special, so as to form a system of classification. All these are valuable lessons. If by the study of Natural History we evolve, in a clear and well defined form, the conceptions of *genus*, *species*, and of *higher* and *lower steps* of classification, we communicate precision, clearness, and method to the intellect, through a great range of its operations.

11. It must be observed, that in order to attain the disciplinal benefit which the study of Natural History is fitted to bestow, we must teach the *natural* not the artificial *classifications*; or at least the natural as well as the artificial. For it is important for the student to perceive that there are classifications, not merely arbitrary, founded upon some *assumed* character, but natural, recognized by some *discovered* character; he ought to see that our classes being collected according to one mark, are confirmed by many marks not originally stated

in our scheme; and are thus found to be grouped together, not by a single resemblance, but by a mass of resemblances, indicating a natural affinity. That objects may be collected into such groups, is a highly important lesson, which Natural History alone, pursued as the science of *natural classes*, can teach.

12. Natural History has not unfrequently been made a portion of education: and has in some degree produced such effects as we have pointed out. It would appear, however, that its lessons have, for the most part been very imperfectly learnt or understood by persons of ordinary education: and that there are perverse intellectual habits very commonly prevalent in the cultivated classes, which ought ere now to have been corrected by the general teaching of Natural History. We may detect among speculative men many prejudices respecting the nature and rules of reasoning, which arise from pure mathematics having been so long and so universally the instrument of intellectual cultivation. Pure Mathematics reasons from definitions: whatever term is introduced into her pages, as a *circle*, or a *square*, its definition comes along with it: and this definition is supposed to supply all that the reasoner needs to know, respecting the term. If there be any doubt concerning the validity of the conclusion, the doubt is resolved by recurring to the definitions. Hence it has come to pass that in other subjects also, men seek for and demand definitions as the most secure foundation of reasoning. The definition and the term defined are conceived to be so far identical, that in all cases the one may be substituted for the other; and such a substitution is held to be the best mode of detecting fallacies.

13. It has already been shown that even geometry is not founded upon definitions alone: and we shall not here again analyse the fallacy of this belief in the supreme

value of definitions. But we may remark that the study of Natural History appears to be the proper remedy for this erroneous habit of thought. For in every department of Natural History the object of our study is *kinds* of things, not one of which kinds can be rigorously defined, yet all of them are sufficiently definite. In these cases we may indeed give a specific description of one of the kinds, and may call it a definition; but it is clear that such a definition does not contain the essence of the thing. We say* that the Rose Tribe are "Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens, exalbuminous definite seeds, and alternate stipulate leaves." But no one would say that this was our essential conception of a rose, to be substituted for it in all cases of doubt or obscurity, by way of making our reasonings perfectly clear. Not only so; but as we have already seen†, the definition does not even apply to all the tribe. For the stipulæ are absent in Lowea: the albumen is present in Neillia: the fruit of Spiræa sorbifolia is capsular. If, then, we can possess any certain knowledge in Natural History, (which no cultivator of the subject will doubt,) it is evident that our knowledge cannot depend on the possibility of laying down exact definitions and reasoning from them.

14. But it may be asked, if we cannot define a word, or a class of things which a word denotes, how can we distinguish what it does mean from what it does not mean? How can we say that it signifies one thing rather than another, except we declare what is its signification?

The answer to this question involves the general principle of a natural method of classification, which has already been stated‡ and need not here be again dwelt

* Lindley's *Nat. Syst. Bot.*, p. 81. † B. VIII., c. ii. sect. 3.
‡ B. VIII., c. ii. *ibid.*

on. It has been shown that names of *kinds* of things (*genera*) associate them according to total resemblances, not partial characters. The principle which connects a group of objects in natural history is not a *definition*, but a *type*. Thus we take as the type of the Rose family, it may be, the common wild rose; all species which resemble this more than they resemble any other group of species are also roses, and form one genus. All genera which resemble Roses more than they resemble any other group of genera are of the same family. And thus the Rose family is collected about some one species, which is the type or central point of the group.

In such an arrangement, it may readily be conceived that though the nucleus of each group may cohere firmly together, the outskirts of contiguous groups may approach, and may even be intermingled, so that some species may doubtfully adhere to one group or another. Yet this uncertainty does not at all affect the truths which we find ourselves enabled to assert with regard to the general mass of each group. And thus we are taught that there may be very important differences between two groups of objects, although we are unable to tell where the one group ends and where the other begins; and that there may be propositions of indisputable truth, in which it is impossible to give unexceptionable definitions of the terms employed.

15. These lessons are of the highest value with regard to all employments of the human mind; for the mode in which words in common use acquire their meaning, approaches far more nearly to the *Method of Type* than to the method of definition. The terms which belong to our practical concerns, or to our spontaneous and unscientific speculations, are rarely capable of exact definition. They have been devised in order to express assertions, often very important, yet very vaguely con-

ceived: and the signification of the word is extended, as far as the assertion conveyed by it can be extended, by apparent connexion or by analogy. And thus, in all the attempts of man to grasp at knowledge, we have an exemplification of that which we have stated as the rule of induction, that Definition and Proposition are mutually dependent, each adjusted so as to give value and meaning to the other: and this is so, even when both the elements of truth are defective in precision: the Definition being replaced by an incomplete description or a loose reference to a Type; and the Proposition being in a corresponding degree insecure.

16. Thus the study of Natural History, as a corrective of the belief that definitions are essential to substantial truth, might be of great use; and the advantage which might thus be obtained is such as well entitles this study to a place in a liberal education. We may further observe, that in order that Natural History may produce such an effect, it must be studied by inspection of the *objects* themselves, and not by the reading of books only. Its lesson is, that we must in all cases of doubt or obscurity refer, not to words or definitions, but to things. The Book of Nature is its dictionary: it is there that the natural historian looks, to find the meaning of the words which he uses*. So long as a plant, in its most essential parts, is more like a rose than anything else, it is a rose. He knows no other definition.

* It is a curious example of the influence of the belief in definitions, that elementary books have been written in which Natural History is taught in the way of question and answer, and consequently by means of words alone. In such a scheme, of course all objects are *defined*: and we may easily anticipate the value of the knowledge thus conveyed. Thus, "Iron is a well-known hard metal, of a darkish gray colour, and very elastic:" "Copper is an orange-coloured metal, more sonorous than any other, and the most elastic of any except iron." This is to pervert the meaning of education, and to make it a business of mere words.

17. (VI.) *Well-established Ideas alone to be used.*—We may assert in general what we have above stated specially with reference to the fundamental principles of chemistry:—no Ideas are suited to become the elements of elementary education, till they have not only become perfectly distinct and fixed in the minds of the leading cultivators of the science to which they belong; but till they have been so for some considerable period. The entire clearness and steadiness of view which is essential to sound science, must have time to extend itself to a wide circle of disciples. The views and principles which are detected by the most profound and acute philosophers, are soon appropriated by all the most intelligent and active minds of their own and of the following generations; and when this has taken place, (and not till then,) it is right, by a proper constitution of our liberal education, to extend a general knowledge of such principles to all cultivated persons. And it follows, from this view of the matter, that we are by no means to be in haste to adopt, into our course of education, all new discoveries as soon as they are made. They require some time, in order to settle into their proper place and position in men's minds, and to show themselves under their true aspects; and till this is done, we confuse and disturb, rather than enlighten and unfold, the ideas of learners, by introducing the discoveries into our elementary instruction. Hence it was perhaps reasonable that a century should elapse from the time of Galileo before the rigorous teaching of mechanics became a general element of intellectual training; and the doctrine of universal gravitation was hardly ripe for such an employment till the end of the last century. We must not direct the unformed youthful mind to launch its little bark upon the waters of speculation, till all the agitation

of discovery, with its consequent fluctuation and controversy, has well subsided.

18. But it may be asked, How is it that time operates to give distinctness and evidence to scientific ideas? In what way does it happen that views and principles, obscure and wavering at first, after a while become luminous and steady? Can we point out any process, any intermediate steps, by which this result is produced? If we can, this process must be an important portion of the subject now under our consideration.

To this we reply, that the transition from the hesitation and contradiction with which true ideas are first received, to the general assent and clear apprehension which they afterwards obtain, takes place through various arguments for and against them, and various modes of presenting and testing them, all which we may include under the term Discussion, which we have already mentioned as the second of the two ways by which scientific views are developed into full maturity.

CHAPTER IV.

OF METHODS OF ACQUIRING CLEAR SCIENTIFIC IDEAS, *continued.*—OF THE DISCUSSION OF IDEAS.

1. It is easily seen that in every part of science, the establishment of a new set of ideas has been accompanied with much of doubt and dissent. And by means of discussions so occasioned, the new conceptions, and the opinions which involve them, have gradually become definite and clear. The authors and asserters of the new opinions, in order to make them defensible, have been compelled to make them consistent: in order to recommend them to others, they have been obliged to make

them more entirely intelligible to themselves. And thus the terms which formed the main points of the controversy, although applied in a loose and vacillating manner at first, have in the end become perfectly definite and exact. The opinions discussed have been, in their main features, the same throughout the debate; but they have at first been dimly, and at last clearly apprehended: like the objects of a landscape, at which we look through a telescope ill adjusted, till, by sliding the tube backwards and forwards, we at last bring it into focus, and perceive every feature of the prospect sharp and bright.

2. We have in the last Book but one* fully exemplified this gradual progress of conceptions from obscurity to clearness by means of Discussion. We have seen, too, that this mode of treating the subject has never been successful, except when it has been associated with an appeal to facts as well as to reasonings. A combination of experiment with argument, of observation with demonstration, has always been found requisite in order that men should arrive at those distinct conceptions which give them substantial truths. The arguments used led to the rejection of undefined, ambiguous, self-contradictory notions; but the reference to facts led to the selection, or at least to the retention, of the conceptions which were both true and useful. The two correlative processes, definition and true assertion, the formation of clear ideas and the induction of laws, went on together.

Thus those discussions by which scientific conceptions are rendered ultimately quite distinct and fixed, include both reasonings from principles and illustrations from facts. At present we turn our attention more peculiarly to the former part of the process; according to the distinction already drawn, between the explication of conceptions and the colligation of facts. The Discussions

* B. XI. c. ii. Of the Explication of Conceptions.

of which we here speak, are the Method (if they may be called a *method*) by which the Explication of Conceptions is carried to the requisite point among philosophers.

3. In the scrutiny of the Fundamental Ideas of the Sciences which forms the previous Part of this work, and in the *History of the Inductive Sciences*, I have, in several instances, traced the steps by which, historically speaking, these Ideas have obtained their ultimate and permanent place in the minds of speculative men. I have thus exemplified the reasonings and controversies which constitute such Discussion as we now speak of. I have stated, at considerable length, the various attempts, failures, and advances, by which the ideas which enter into the science of Mechanics were evolved into their present evidence. In like manner we have seen the conception of refracted rays of light, obscure and confused in Seneca, growing clearer in Roger Bacon, more definite in Descartes, perfectly distinct in Newton. The polarity of light, at first contemplatad with some perplexity, became very distinct to Malus, Young, and Fresnel; yet the phenomena of circular polarization, and still more, the circular polarization of fluids, leave us, even at present, some difficulty in fully mastering this conception. The related polarities of electricity and magnetism are not yet fully comprehended, even by our greatest philosophers. One of Mr. Faraday's late papers (the Fourteenth Series of his Researches) is employed in an experimental discussion of this subject, which leads to no satisfactory result. The controversy between Biot and Ampère*, on the nature of the elementary forces in electro-dynamic action, is another evidence that the discussion of this subject has not yet reached its termination. With regard to chemical polarity, I have already stated that this idea is as yet very far from being brought to an ultimate con-

* *Hist. Ind. Sci.*, B. XIII. c. vi.

dition of definiteness; and the subject of chemical forces, (for the whole subject must be included in this idea of polarity,) which has already occasioned much perplexity and controversy, may easily occasion much more, before it is settled to the satisfaction of the philosophical world. The ideas of the classificatory sciences also have of late been undergoing much, and very instructive discussion, in the controversies respecting the relations and offices of the natural and artificial methods. And with regard to physiological ideas, it would hardly be too much to say, that the whole history of physiology up to the present time has consisted of the discussion of the fundamental ideas of the science, such as vital forces, nutrition, reproduction, and the like. We have had before us at some length, in the present work, a review of the opposite opinions which have been advanced on this subject; and have attempted in some degree to estimate the direction in which these ideas are permanently settling. But without attaching any importance to this attempt, the account there given may at least serve to show, how important a share in the past progress of this subject the discussion of its fundamental ideas has hitherto had.

4. There is one reflection which is very pointedly suggested by what has been said. The manner in which our scientific ideas acquire their distinct and ultimate form being such as has been described,—always involving much abstract reasoning and analysis of our conceptions, often much opposite argumentation and debate;—how unphilosophical is it to speak of abstraction and analysis, of dispute and controversy, as frivolous and unprofitable processes, by which true science can never be benefitted; and to put such employments in antithesis with the study of facts!

Yet some writers are accustomed to talk with contempt of all past controversies, and to wonder at the blindness of those who did not at first take the view which was established at last. Such persons forget that it was precisely the controversy, which established among speculative men that final doctrine which they themselves have quietly accepted. It is true, they have had no difficulty in thoroughly adopting the truth; but that has occurred because all dissentient doctrines have been suppressed and forgotten; and because systems, and books, and language itself, have been accommodated peculiarly to the expression of the accepted truth. To despise those who have, by their mental struggles and conflicts, brought the subject into a condition in which errour is almost out of our reach, is to be ungrateful exactly in proportion to the amount of the benefit received. It is as if a child, when its teacher had with many trials and much trouble prepared a telescope so that the vision through it was distinct, should wonder at his stupidity in pushing the tube of the eye-glass out and in so often.

5. Again, some persons condemn all that we have here spoken of as the discussion of ideas, terming it *metaphysical:* and in this spirit, one writer* has spoken of the "metaphysical period" of each science, as preceding the period of "positive knowledge." But as we have seen, that process which is here termed "metaphysical," —the analysis of our conceptions and the exposure of their inconsistencies,—(accompanied with the study of facts,)—has always gone on most actively in the most prosperous periods of each science. There is, in Galileo, Kepler, Gassendi, and the other fathers of mechanical philosophy, as much of metaphysics as in their adversaries. The main difference is, that the metaphysics is of a better kind; it is more conformable to metaphysical truth. And the same is the case in other sciences. Nor can it be otherwise. For all truth, before it can be consistent

* M. Auguste Comte, *Cours de Philosophie Positive.*

with facts, must be consistent with itself: and although this rule is of undeniable authority, its application is often far from easy. The perplexities and ambiguities which arise from our having the same idea presented to us under different aspects, are often difficult to disentangle: and no common acuteness and steadiness of thought must be expended on the task. It would be easy to adduce, from the works of all great discoverers, passages more profoundly metaphysical than any which are to be found in the pages of barren *à priori* reasoners.

6. As we have said, these metaphysical discussions are not to be put in opposition to the study of facts; but are to be stimulated, nourished and directed by a constant recourse to experiment and observation. The cultivation of ideas is to be conducted as having for its object the connexion of facts; never to be pursued as a mere exercise of the subtilty of the mind, striving to build up a world of its own, and neglecting that which exists about us. For although man may in this way please himself, and admire the creations of his own brain, he can never, by this course, hit upon the real scheme of nature. With his ideas unfolded by education, sharpened by controversy, rectified by metaphysics, he may *understand* the natural world, but he cannot *invent* it. At every step, he must try the value of the advances he has made in thought, by applying his thoughts to things. The Explication of Conceptions must be carried on with a perpetual reference to the Colligation of Facts.

Having here treated of Education and Discussion as the methods by which the former of these two processes is to be promoted, we have now to explain the methods which science employs in order most successfully to execute the latter. But the Colligation of Facts, as already stated, may offer to us two steps of a very different kind,—the laws of Phenomena, and their Causes. We shall first describe some of the methods employed in obtaining truths of the former of these two kinds.

Chapter V.

ANALYSIS OF THE PROCESS OF INDUCTION.

Sect. I.—*The Three Steps of Induction.*

1. When facts have been decomposed and phenomena measured, the philosopher endeavours to combine them into general laws, by the aid of ideas and conceptions, these being illustrated and regulated by such means as we have spoken of in the last two chapters. In this task, of gathering laws of nature from observed facts, as we have already said*, the natural sagacity of gifted minds is the power by which the greater part of the successful results have been obtained; and this power will probably always be more efficacious than any Method can be. Still there are certain methods of procedure which may in such investigations give us no inconsiderable aid, and these I shall endeavour to expound.

2. For this purpose, I remark that the Colligation of ascertained facts into general propositions may be considered as containing three steps, which I shall term *the Selection of the Idea*, *the Construction of the Conception*, and *the Determination of the Magnitudes*. It will be recollected that by the word *Idea*, (or Fundamental Idea,) used in a peculiar sense, I mean certain wide and general fields of intelligible relation, such as Space, Number, Cause, Likeness; while by *Conception* I denote more special modifications of these ideas, as a *circle*, a *square number*, a *uniform force*, a *like form* of flower. Now in

* B. xi. c. vi.

order to establish any law by reference to facts, we must select the *true Idea* and the *true Conception*. For example; when Hipparchus found* that the distance of the bright star Spica Virginis from the equinoxial point had increased by two degrees in about two hundred years, and desired to reduce this change to a law, he had first to assign, if possible, the *idea* on which it depended;—whether it was regulated for instance, by *space*, or by *time;* whether it was determined by the positions of other stars at each moment, or went on progressively with the lapse of ages. And when there was found reason to select *time* as the regulative *idea* of this change, it was then to be determined how the change went on with the time;—whether uniformly, or in some other manner: the *conception*, or the rule of the progression, was to be rightly constructed. Finally, it being ascertained that the change did go on uniformly, the question then occurred what was its *amount*:—whether exactly a degree in a century, or more, or less, and how much: and thus the determination of the *magnitude* completed the discovery of the law of phenomena respecting this star.

3. Steps similar to these three may be discerned in all other discoveries of laws of nature. Thus, in investigating the laws of the motions of the sun, moon or planets, we find that these motions may be resolved, besides a uniform motion, into a series of partial motions, or Inequalities; and for each of these Inequalities, we have to learn upon what it directly depends, whether upon the progress of time only, or upon some configuration of the heavenly bodies in space; then, we have to ascertain its law; and finally, we have to determine what is its amount. In the case of such Inequalities, the fundamental element on which the Inequality depends, is called the *Argument*. And when the Inequality has been

* *Hist. Ind. Sci.*, B. III. c. iv. sect. 3.

fully reduced to known rules, and expressed in the form of a Table, the Argument is the fundamental series of numbers which stands in the margin of the Table, and by means of which we refer to the other numbers which express the Inequality. Thus, in order to obtain from a Solar Table the Inequality of the sun's annual motion, the Argument is the number which expresses the day of the year; the Inequalities for each day being (in the Table) ranged in a line corresponding to the days. Moreover, the Argument of an Inequality being assumed to be known, we must, in order to calculate the Table, that is, in order to exhibit the law of nature, know also the *Law* of the Inequality, and its *Amount*. And the investigation of these three things, the Argument, the Law, and the Amount of the Inequality, represents the three steps above described, the Selection of the Idea, the Construction of the Conception, and the Determination of the Magnitude.

4. In a great body of cases, *mathematical* language and calculation are used to express the connexion between the general law and the special facts. And when this is done, the three steps above described may be spoken of as the Selection of the *Independent Variable*, the Construction of the *Formula*, and the Determination of the *Coefficients*. It may be worth our while to attend to an exemplification of this. Suppose then, that, in such observations as we have just spoken of, namely, the shifting of a star from its place in the heavens by an unknown law, astronomers had, at the end of three successive years, found that the star had removed by 3, by 8, and by 15 minutes from its original place. Suppose it to be ascertained also, by methods of which we shall hereafter treat, that this change depends upon the time; we must then take the *time*, (which we may denote by the symbol t,) for the *independent variable*. But though the star changes

its place with the time, the change is not proportional to the time; for its motion which is only 3 minutes in the first year, is 5 minutes in the second year, and 7 in the third. But it is not difficult for a person a little versed in mathematics to perceive that the series 3, 8, 15, may be obtained by means of two terms, one of which is proportional to the time, and the other to the square of the time; that is, it is expressed by the *formula* $at + btt$. The question then occurs, what are the values of the *coefficients* a and b; and a little examination of the case shows us that a must be 2, and b, 1: so that the formula is $2t + tt$. Indeed if we add together the series 2, 4, 6, which expresses the change proportional to the time, and 1, 4, 9, which is proportional to the square of the time, we obtain the series 3, 8, 15, which is the series of numbers given by observation. And thus the three steps which give us the Idea, the Conception, and the Magnitudes; or the Argument, the Law, and the Amount, of the change; give us the Independent Variable, the Formula, and the Coefficients, respectively.

We now proceed to offer some suggestions of methods by which each of these steps may be in some degree promoted.

Sect. II.—*Of the Selection of the Fundamental Idea.*

5. When we turn our thoughts upon any assemblage of facts, with a view of collecting from them some connexion or law, the most important step, and at the same time that in which rules can least aid us, is the Selection of the Idea by which they are to be collected. So long as this idea has not been detected, all seems to be hopeless confusion or insulated facts; when the connecting idea has been caught sight of, we constantly regard the facts with reference to their connexion, and wonder that

it should be possible for any one to consider them in any other point of view.

Thus the different seasons, and the various aspects of the heavenly bodies, might at first appear to be direct manifestations from some superior power, which man could not even understand: but it was soon found that the ideas of time and space, of motion and recurrence, would give coherency to many of the phenomena. Yet this took place by successive steps. Eclipses, for a long period, seemed to follow no law; and being very remarkable events, continued to be deemed the indications of a supernatural will, after the common motions of the heavens were seen to be governed by relations of time and space. At length, however, the Chaldeans discovered that, after a period of eighteen years, similar sets of eclipses recur; and, thus selecting the idea of *time*, simply, as that to which these events were to be referred, they were able to reduce them to rule; and from that time, eclipses were recognized as parts of a regular order of things. We may, in the same manner, consider any other course of events, and may enquire by what idea they are bound together. For example, if we take the weather, years peculiarly wet or dry, hot and cold, productive and unproductive, follow each other in a manner which, at first sight at least, seems utterly lawless and irregular. Now can we in any way discover some rule and order in these occurrences? Is there, for example, in these events, as in eclipses, a certain cycle of years, after which like seasons come round again? or does the weather depend upon the force of some extraneous body—for instance, the moon—and follow in some way her aspects? or would the most proper way of investigating this subject be to consider the effect of the moisture and heat of various tracts of the earth's surface upon the ambient air? It is at our choice to *try* these and other modes of obtaining a science of the weather: that is, we may refer the phenomena to the idea of *time*, introducing the conception of a cycle;—or to the idea of external *force*, by the conception of the moon's action;—or to the idea of *mutual action*, introducing the conceptions of thermotical and atmological agencies, operating between different regions of earth, water, and air.

6. It may be asked, How are we to decide in such alternatives? How are we to select the one right idea out of several conceivable ones? To which we can only reply, that this must be done by *trying* which will succeed. If there really exist a cycle of the weather, as well as of eclipses, it must be established by comparing the asserted cycle with a good register of the seasons, of sufficient extent. Or if the moon really influence the meteorological conditions of the air, the asserted influence must be compared with the observed facts, and so accepted or rejected. When Hipparchus had observed the increase of longitude of the stars, the idea of a motion of the celestial sphere suggested itself as the explanation of the change; but this thought was verified only by observing several stars. It was conceivable that each star should have an independent motion, governed by time only, or by other circumstances, instead of being regulated by its place in the sphere; and this possibility could be rejected by trial alone. In like manner, the original opinion of the composition of bodies supposed the compounds to derive their properties from the elements according to the law of *likeness;* but this opinion was overturned by a thousand facts; and thus the really applicable idea of chemical composition was introduced in modern times. In what has already been said on the History of Ideas, we have seen how each science was in a state of confusion and darkness till the right idea was introduced.

7. No general method of evolving such ideas can be given. Such events appear to result from a peculiar sagacity and felicity of mind;—never without labour, never without preparation;—yet with no constant dependence upon preparation, or upon labour, or even entirely upon personal endowments. Newton explained the colours which refraction produces, by referring each colour to a peculiar *angle of refraction*, thus introducing the right idea. But when the same philosopher tried to explain the colours produced by diffraction, he erred, by attempting to apply the same idea, (*the course of a single ray*,) instead of applying the truer idea of the *interference of two rays*. Newton gave a wrong rule for the double refraction of Iceland spar, by making the refraction depend on the *edges* of the rhombohedron: Huyghens, more happy, introduced the idea of the *axis of symmetry* of the solid, and thus was able to give the true law of the phenomena.

8. Although the selected idea is proved to be the right one, only when the true law of nature is established by means of it, yet it often happens that there prevails a settled conviction respecting the relation which must afford the key to the phenomena, before the selection has been confirmed by the laws to which it leads. Even before the empirical laws of the tides were made out, it was not doubtful that these laws depended upon the places and motions of the sun and moon. We know that the crystalline form of a body must depend upon its chemical composition, though we are as yet unable to assign the law of this dependence.

Indeed in most cases of great discoveries, the right idea to which the facts were to be referred, was selected by many philosophers, before the decisive demonstration that it *was* the right idea, was given by the discoverer. Thus Newton showed that the motions of the planets might be explained by means of a central force in the sun: but though he established, he did not first select the idea involved in the conception of a central force. The idea had already been sufficiently pointed out, dimly by Kepler, more clearly by Borelli, Huyghens, Wren, and Hooke. Indeed this anticipation of the true idea is always a principal part of that which, in the *History of the Sciences*, we have termed the *Prelude* of a Discovery. The two steps of *proposing* a philosophical problem, and of *solving* it, are, as we have elsewhere said, both important, and are often performed by different persons. The former step is, in fact, the Selection of the Idea. In explaining any change, we have to discover first the *Argument*, and then the *Law* of the change. The selection of the Argument is the step of which we here speak; and is that in which inventiveness of mind and justness of thought are mainly shown.

9. Although, as we have said, we can give few precise directions for this cardinal process, the Selection of the Idea, in speculating on phenomena, yet there is one Rule which may have its use: it is this:—*The idea and the facts must be homogeneous:* the elementary Conceptions, into which the facts have been decomposed, must be of the same nature as the Idea by which we attempt to collect them into laws. Thus, if facts have been observed and measured by reference to space, they must be bound together by the idea of space: if we would obtain a knowledge of mechanical forces in the solar system, we must observe mechanical phenomena. Kepler erred against this rule in his attempts at obtaining physical laws of the system; for the facts which he took were the *velocities*, not the *changes of velocity*, which are really the mechanical facts. Again, there has been a transgression of this Rule committed by all chemical philosophers who have attempted to assign the relative position of the ele-

mentary particles of bodies in their component molecules. For their purpose has been to discover the *relations* of the particles *in space;* and yet they have neglected the only facts in the constitution of bodies which have a reference to space—namely, *crystalline form*, and *optical properties*. No progress can be made in the theory of the elementary structure of bodies, without making these classes of facts the main basis of our speculations.

10. The only other Rule which I have to offer on this subject, is that which I have already given:—*the Idea must be tested by the facts*. It must be tried by applying to the facts the conceptions which are derived from the idea, and not accepted till some of these succeed in giving the law of the phenomena. The justice of the suggestion cannot be known otherwise than by making the trial. If we can discover a *true law* by employing any conceptions, the idea from which these conceptions are derived is the *right* one; nor can there be any proof of its rightness so complete and satisfactory, as that we are by it led to a solid and permanent truth.

This, however, can hardly be termed a Rule; for when we would know, to conjecture and to try the truth of our conjecture by a comparison with the facts, is the natural and obvious dictate of common sense.

Supposing the Idea which we adopt, or which we would try, to be now fixed upon, we still have before us the range of many Conceptions derived from it; many Formulæ may be devised depending on the same Independent Variable, and we must now consider how our selection among these is to be made.

Chapter VI.

GENERAL RULES FOR THE CONSTRUCTION OF THE CONCEPTION.

1. In speaking of the discovery of laws of nature, those which depend upon *quantity*, as number, space, and the like, are most prominent and most easily conceived, and therefore in speaking of such researches, we shall often use language which applies peculiarly to the cases in which quantities numerically measurable are concerned, leaving it for a subsequent task to extend our principles to ideas of other kinds.

Hence we may at present consider the Construction of a Conception which shall include and connect the facts, as being the construction of a Mathematical Formula, coinciding with the numerical expression of the facts; and we have to consider how this process can be facilitated, it being supposed that we have already before us the numerical measures given by observation.

2. We may remark, however, that the construction of the right Formula for any such case, and the determination of the Coefficients of such formula, which we have spoken of as two separate steps, are in practice almost necessarily simultaneous; for the near coincidence of the results of the theoretical rule with the observed facts confirms at the same time the Formula and its Coefficients. In this case also, the mode of arriving at truth is to try various hypotheses;—to modify the hypotheses so as to approximate to the facts, and to multiply the facts so as to test the hypotheses.

The Independent Variable, and the Formula which we would try, being once selected, mathematicians have devised certain special and technical processes by which the value of the coefficients may be determined. These we shall treat of in the next Chapter; but in the mean time we may note, in a more general manner, the mode in which, in physical researches, the proper formula may be obtained.

3. A person somewhat versed in mathematics, having before him a series of numbers, will generally be able to devise a formula which approaches near to those numbers. If, for instance, the series is constantly progressive, he will be able to see whether it more nearly resembles an arithmetical or a geometrical progression. For example, MM. Dulong and Petit, in their investigation of the law of cooling of bodies, obtained the following series of measures. A thermometer, made hot, was placed in an enclosure of which the temperature was 0 degrees, and the rapidity of cooling of the thermometer was noted for many temperatures. It was found that

For the temperature	240	the rapidity of cooling was	10·69
„	220	„	8·81
„	200	„	7·40
„	180	„	6·10
„	160	„	4·89
„	140	„	3·88

and so on. Now this series of numbers manifestly increases with greater rapidity as we proceed from the lower to the higher parts of the scale. The numbers do not, however, form a geometrical series, as we may easily ascertain. But if we were to take the differences of the successive terms we should find them to be—

1·88, 1·41, 1·30, 1·21, 1·01, &c.

and these numbers are very nearly the terms of a geometric series. For if we divide each term by the succeeding one, we find these numbers,

1·33, 1·09, 1·07, 1·20, 1·27,

in which there does not appear to be any constant tendency to diminish or increase. And we shall find that a geometrical series in which the ratio is 1·165, may be made to approach very near to this series, the deviations from it being only such as may be accounted for by conceiving them as errours of observation. In this manner a certain formula* is obtained, giving results which very nearly coincide with the observed facts, as may be seen in the margin.

The physical law expressed by the formula just spoken of is this:—that when a body is cooling in an empty inclosure at a constant temperature, the quickness of the cooling, for excesses of temperature in arithmetical progression, increases as the terms of a geometrical progression, diminished by a constant number.

4. In the actual investigation of Dulong and Petit, however, the formula was not obtained in precisely the manner just described. For the quickness of cooling depends upon two elements, the temperature of the hot body and the temperature of the inclosure; not merely upon the *excess* of one of these over the other. And it was found most convenient, first, to make such experiments as should exhibit the dependence of the velocity

* The formula is $v = 2{,}037\ (a^t - 1)$ where v is the velocity of cooling, t the temperature of the thermometer expressed in degrees, and a is the quantity 1,0077.

The degree of coincidence is as follows:—

Excess of temperature of the thermometer, or values of t.	Observed values of v.	Calculated values of v.
240	10·69	10·68
220	8·81	8·89
200	7·40	7·34
180	6·10	6·03
160	4·89	4·87
140	3·88	3·89
120	3·02	3·05
100	2·30	2·33
80	1·74	1·72

of cooling upon the temperature of the enclosure; which dependence is contained in the following law:—The quickness of cooling of a thermometer in vacuo for a constant excess of temperature, increases in geometric progression, when the temperature of the inclosure increases in arithmetic progression. From this law the preceding one follows by necessary consequence*.

This example may serve to show the nature of the artifices which may be used for the construction of formulæ, when we have a constantly progressive series of numbers to represent. We must not only endeavour by trial to contrive a formula which will answer the conditions, but we must vary our experiments so as to determine, first one factor or portion of the formula, and then the other; and we must use the most probable hypothesis as means of suggestion for our formulæ.

5. In a *progressive* series of numbers, except the formula which we adopt be really that which expresses the law of nature; the deviations of the formula from the facts will generally become enormous, when the experiments are extended into new parts of the scale. True formulæ for a progressive series of results can hardly ever be obtained from a very limited range of experiments: just, as the attempt to guess the general course of a road or a river, by knowing two or three points of it in the neighbourhood of one another, would generally fail. In the investigation respecting the laws of the

* For if θ be the temperature of the inclosure, and t the excess of temperature of the hot body, it appears, by this law, that the radiation of heat is as a^θ. And hence the quickness of cooling, which is as the excess of radiation, is as $a^{\theta+t} - a^\theta$; that is, as a^θ $(a^t - 1)$ which agrees with the formula given in the last note.

The whole of this series of researches of Dulong and Petit is full of the most beautiful and instructive artifices for the construction of the proper formulæ in physical research.

cooling of bodies just noticed, one great advantage of the course pursued by the experimenters was, that their experiments included so great a range of temperatures. The attempts to assign the law of elasticity of steam deduced from experiments made with moderate temperatures, were found to be enormously wrong, when very high temperatures were made the subject of experiment. It is easy to see that this must be so: an arithmetical and a geometrical series may nearly coincide for a few terms moderately near each other: but if we take remote corresponding terms in the two series, one of these will be very many times the other. And hence, from a narrow range of experiments, we may infer one of these series when we ought to infer the other; and thus obtain a law which is widely erroneous.

6. In Astronomy, the serieses of observations which we have to study are, for the most part, not progressive, but *recurrent*. The numbers observed do not go on constantly increasing; but after increasing up to a certain amount they diminish; then, after a certain space, increase again; and so on, changing constantly through certain *cycles*. In cases in which the observed numbers are of this kind, the formula which expresses them must be a *circular function*, of some sort or other; involving, for instance, sines, tangents, and other forms of calculation, which have recurring values when the angle on which they depend goes on constantly increasing. The main business of formal astronomy consists in resolving the celestial phenomena into a series of *terms* of this kind, in detecting their *arguments*, and in determining their *coefficients*.

7. In constructing the formulæ by which laws of nature are expressed, although the first object is to assign the law of the phenomena, philosophers have, in almost all cases, not proceeded in a purely empirical manner, to

connect the observed numbers by some expression of calculation, but have been guided, in the selection of their formula, by some *hypothesis* respecting the mode of connexion of the facts. Thus the formula of Dulong and Petit above given was suggested by the theory of exchanges; the first attempts at the resolution of the heavenly motions into circular functions were clothed in the hypothesis of epicycles. And this was almost inevitable. "We must confess," says Copernicus*, "that the celestial motions are circular, or compounded of several circles, since their inequalities observe a fixed law, and recur in value at certain intervals, which could not be except they were circular: for a circle alone can make that quantity which has occurred recur again." In like manner the first publication of the *law of the sines*, the true formula of optical refraction, was accompanied by Descartes with an hypothesis, in which an explanation of the law was pretended. In such cases, the mere comparison of observations may long fail in suggesting the true formulæ. The fringes of shadows and other diffracted colours were studied in vain by Newton, Grimaldi, Comparetti, the elder Herschel, and Mr. Brougham, so long as these inquirers attempted merely to trace the laws of the facts as they appeared in themselves; while Young, Fresnel, Fraunhofer, Schwerdt, and others, determined these laws in the most rigorous manner, when they applied to the observations the hypothesis of interferences.

8. But with all the aid that hypotheses and calculation can afford, the construction of true formulæ, in those cardinal discoveries by which the progress of science has mainly been caused, has been a matter of great labour and difficulty, and of good fortune added to sagacity. In the *History of Science*, we have seen how long and how

* *De Rev.*, L. I. c. iv.

hard Kepler laboured, before he converted the formula for the planetary motions, from an epicyclical combination, to a simple ellipse. The same philosopher, labouring with equal zeal and perseverance to discover the formula of optical refraction, which now appears to us so simple, was utterly foiled. Malus sought in vain the formula determining the angle at which a transparent surface polarizes light: Sir D. Brewster*, with a happy sagacity, discovered the formula to be simply this, that the index of refraction is the tangent of the angle of polarization.

Though we cannot give rules which will be of much service when we have thus to divine the general form of the relation by which phenomena are connected, there are certain methods by which, in a narrower field, our investigations may be materially promoted;—certain special methods of obtaining laws from observations. Of these we shall now proceed to treat.

CHAPTER VII.

SPECIAL METHODS OF INDUCTION APPLICABLE TO QUANTITY.

IN cases where the phenomena admit of numerical measurement and expression, certain mathematical methods may be employed to facilitate and give accuracy to the determination of the formula by which the observations are connected into laws. Among the most usual and important of these Methods are the following:—

I. The Method of Curves.
II. The Method of Means.
III. The Method of Least Squares.
IV. The Method of Residues.

* *Hist. Ind. Sci.*, B. IX. c. vi.

Sect. I.—*The Method of Curves.*

1. The Method of Curves proceeds upon this basis; that when one quantity undergoes a series of changes depending on the progress of another quantity, (as, for instance, the Deviation of the Moon from her equable place depends upon the progress of Time,) this dependence may be expressed by means of a *curve*. In the language of mathematicians, the variable quantity, whose changes we would consider, is made the *ordinate* of the curve, and the quantity on which the changes depend is made the *abscissa*. In this manner, the curve will exhibit in its form a series of undulations, rising and falling so as to correspond with the alternate increase and diminution of the quantity represented, at intervals of space which correspond to the intervals of time, or other quantity by which the changes are regulated. Thus, to take another example, if we set up, at equal intervals, a series of ordinates representing the height of all the successive high waters brought by the tides at a given place, for a year, the curve which connects the summits of all these ordinates will exhibit a series of undulations, ascending and descending once in about each fortnight; since, in that interval, we have, in succession, the high spring tides and the low neap tides. The curve thus drawn offers to the eye a picture of the order and magnitude of the changes to which the quantity under contemplation, (the height of high water,) is subject.

2. Now the peculiar facility and efficacy of the Method of Curves depends upon this circumstance;—that order and regularity are more readily and clearly recognized, when thus exhibited to the eye in a picture, than they are when presented to the mind in any other manner. To detect the relations of Number considered directly as Number, is not easy: and we might contem-

plate for a long time a Table of recorded Numbers without perceiving the order of their increase and diminution, even if the law were moderately simple; as any one may satisfy himself by looking at a Tide Table. But if these Numbers are expressed by the magnitude of *Lines*, and if these Lines are arranged in regular order, the eye readily discovers the rule of their changes: it follows the curve which runs along their extremities, and takes note of the order in which its convexities and concavities succeed each other, if any order be readily discoverable. The separate observations are in this manner compared and generalized and reduced to rule by the eye alone. And the eye, so employed, detects relations of order and succession with a peculiar celerity and evidence. If, for example, we thus arrange as ordinates the prices of corn in each year for a series of years, we shall see the order, rapidity, and amount of the increase and decrease of price, far more clearly than in any other manner. And if there were any recurrence of increase and decrease at stated intervals of years, we should in this manner perceive it. The eye, constantly active and busy, and employed in making into shapes the hints and traces of form which it contemplates, runs along the curve thus offered to it; and as it travels backwards and forwards, is ever on the watch to detect some resemblance or contrast between one part and another. And these resemblances and contrasts, when discovered, are the images of laws of phenomena; which are made manifest at once by this artifice, although the mind could not easily catch the indications of their existence, if they were not thus reflected to her in the clear mirror of space.

Thus when we have a series of good observations, and know the argument upon which their change of magnitude depends, the Method of Curves enables us to ascertain, almost at a glance, the law of the change; and by further attention, may be made to give us a formula with great accuracy. The Method enables us to perceive, among our observations, an order, which without the method, is concealed in obscurity and perplexity.

3. But the Method of Curves not only enables us to obtain laws of nature from good observations, but also, in a great degree, from observations which are very *imperfect*. For the imperfection of observations may in part be corrected by this consideration;—that though they may appear irregular, the correct facts which they imperfectly represent, are really regular. And the Method of Curves enables us to remedy this apparent irregularity, at least in part. For when observations thus imperfect are laid down as ordinates, and their extremities connected by a line, we obtain, not a smooth and flowing curve, such as we should have if the observations contained only the rigorous results of regular laws; but a broken and irregular line, full of sudden and capricious twistings, and bearing on its face marks of irregularities dependent, not upon law, but upon chance. Yet these irregular and abrupt deviations in the curve are, in most cases, but small in extent, when compared with those bendings which denote the effects of regular law. And this circumstance is one of the great grounds of advantage in the Method of Curves. For when the observations thus laid down present to the eye such a broken and irregular line, we can still see, often with great ease and certainty, what twistings of the line are probably due to the irregular errours of observation; and can at once reject these, by drawing a more regular curve, cutting off all such small and irregular sinuosities, leaving some to the right and some to the left; and then proceeding as if this regular curve, and not the irregular one, expressed the observations.

In this manner, we suppose the errours of observation to balance each other; some of our corrected measures being too great and others too small, but with no great preponderance either way. We draw our main regular curve, not *through* the points given by our observations, but *among* them: drawing it, as has been said by one of the philosophers* who first systematically used this method, "with a bold but careful hand." The regular curve which we thus obtain, thus freed from the casual errours of observation, is that in which we endeavour to discover the laws of change and succession.

4. By this method, thus getting rid at once, in a great measure, of errours of observation, we obtain data which are *more true than the* individual *facts themselves*. The philosopher's business is to compare his hypotheses with facts, as we have often said. But if we make the comparison with separate special facts, we are liable to be perplexed or misled, to an unknown amount, by the errours of observation; which may cause the hypothetical and the observed result to agree, or to disagree, when otherwise they would not do so. If, however, we thus take the *whole mass of the facts*, and remove the errours of actual observation†, by making the curve which expresses the supposed observation regular and smooth, we have the separate facts corrected by their general tendency. We are put in possession, as we have said, of something more true than any fact by itself is.

One of the most admirable examples of the use of this Method of Curves is found in Sir John Herschel's *Investigation of the orbits of double stars*‡. The author there shows how far inferior the direct observations of the angle of position are, to the observations corrected by a curve in the manner above stated. "This curve

* Sir J. Herschel, *Ast. Soc. Trans.*, Vol. v. p. 1.

† *Ibid.*, Vol. v. p. 4.

‡ *Ibid.*

once drawn," he says, "must represent, it is evident, the law of variation of the angle of position, with the time, not only for instants intermediate between the dates of observations, but even at the moments of observation themselves, much better than the individual *raw* observations can possibly (on an average) do. It is only requisite to try a case or two, to be satisfied that by substituting the curve for the points, we have made a nearer approach to nature, and in a great measure eliminated errours of observation." "In following the graphical process," he adds, "we have a conviction almost approaching to moral certainty that we cannot be greatly misled." Again, having thus corrected the raw observations, he makes another use of the graphical method, by trying whether an ellipse can be drawn "if not *through*, at least *among* the points, so as to approach tolerably near them all; and thus approaching to the orbit which is the subject of investigation."

5. The *obstacles* which principally impede the application of the method of curves are (I.) our *ignorance of the argument* of the changes, and (II.) the *complication of several laws* with one another.

(I.) If we do not know on what quantity those changes depend which we are studying, we may fail entirely in detecting the law of the changes, although we throw the observations into curves. For the true argument of the change should, in fact, be made the abscissa of the curve. If we were to express, by a series of ordinates, the *hour* of high water on successive days, we should not obtain, or should obtain very imperfectly, the law which these times follow; for the real argument of this change is not the *solar* hour, but the hour at which the *moon* passes the meridian. But if we are supposed to be aware that *this* is the argument, (which theory suggests and trial instantly confirms) we then do immediately obtain the primary rules of the time of high water, by throwing a series of observations into a curve, with the hour of the moon's transit for the abscissa.

In like manner, when we have obtained the first great or semi-mensual inequality of the tides, if we endeavour to discover the laws of other inequalities by means of curves, we must take from theory the suggestion that the Arguments of such inequalities will probably be the *parallax* and the *declination* of the moon. This suggestion again is confirmed by trial; but if we were supposed to be entirely ignorant of the dependence of the changes of the tide on the distance and declination of the moon, the curves would exhibit unintelligible and seemingly capricious changes. For by the effect of the inequality arising from the parallax, the convexities of the curves which belong to the spring tides, are in some years made alternately greater and less all the year through; while in other years they are made all nearly equal. This difference does not betray its origin, till we refer it to the parallax; and the same difficulty in proceeding would arise if we were ignorant that the moon's declination is one of the arguments of tidal changes.

In like manner, if we try to reduce to law any meteorological changes, those of the height of the barometer for instance, we find that we can make little progress in the investigation, precisely because we do not know the Argument on which these changes depend. That there is a certain regular *diurnal* change of small amount we know; but when we have abstracted this inequality, (of which the Argument is the *time of day*,) we find far greater changes left behind, from day to day and from hour to hour; and we express these in curves, but we cannot reduce them to rule, because we cannot discover on what numerical quantity they depend. The assiduous study of barometrical observations, thrown into curves, may perhaps hereafter point out to us what are the relations of time and space by which these variations are determined; but in the mean time, this subject exemplifies to us our remark, that the method of curves is of comparatively small use, so long as we are in ignorance of the real Arguments of the Inequalities.

6. (II.) In the next place, I remark that a difficulty is thrown in the way of the method of curves by *the combination of several laws* one with another. It will readily be seen that such a cause will produce a complexity in the curves which exhibit the succession of facts. If, for example, we take the case of the tides, the height of high water increases and diminishes with the approach of the sun to, and its recess from, the syzygies of the moon. Again, this height increases and diminishes as the moon's parallax increases and diminishes; and again, the height diminishes when the declination increases, and *vice versa*; and all these Arguments of change, the distance from syzygy, the parallax, the declination, complete their circuit and return into themselves in different periods. Hence the curve which represents the height of high water has not any periodical interval in which it completes its changes and commences a new cycle. The sinuosity which would arise from each inequality separately considered, interferes with, disguises, and conceals the others; and when we first cast our eyes on the curve of observation, it is very far from offering any obvious regularity in its form. And it is to be observed that we have not yet enumerated *all* the elements of this complexity: for there are changes of the tide depending upon the parallax and declination of the sun as well as of the moon. Again; besides these changes, of which the arguments are obvious, there are others, as those depending upon the barometer and the wind, which follow no known

regular law, and which constantly affect and disturb the results produced by other laws.

In the tides, and in like manner in the motions of the moon, we have very eminent examples of the way in which the discovery of laws may be rendered difficult by the number of laws which operate to affect the same quantity. In such cases, the inequalities are generally picked out in succession, nearly in the order of their magnitudes. In this way there were successively collected, from the study of the moon's motions by a series of astronomers, those Inequalities which we term the *Equation of the Center*, the *Evection*, the *Variation*, and the *Annual Equation*. These Inequalities were not, in fact, obtained by the application of the Method of Curves; but the Method of Curves might have been applied to such a case with great advantage. The Method has been applied with great industry and with remarkable success to the investigation of the laws of the tides; and by the use of it, a series of Inequalities both of the Times and of the Heights of high water has been detected, which explain all the main features of the observed facts.

SECT. II.—*The Method of Means.*

7. THE Method of Curves, as we have endeavoured to explain above, frees us from the casual and extraneous irregularities which arise from the imperfection of observation; and thus lays bare the results of the laws which really operate, and enables us to proceed in search of those laws. But the Method of Curves is not the only one which effects such a purpose. The errours arising from detached observations may be got rid of, and the additional accuracy which multiplied observations give may be obtained, by operations upon the observed numbers without expressing them by spaces. The process of curves assumes that the errours of observation balance

each other;—that the accidental excesses and defects are nearly equal in amount;—that the true quantities which would have been observed if all accidental causes of irregularity were removed, are obtained, exactly or nearly, by selecting quantities, upon the whole, equally distant from the extremes of great and small which our imperfect observations offer to us. But when, among a number of unequal quantities, we take a quantity equally distant from the greater and the smaller, this quantity is termed the *Mean* of the unequal quantities. Hence the correction of our observations by the method of curves consists in taking the Mean of the observations.

8. Now without employing curves, we may proceed arithmetically to take the Mean of all the observed numbers of each class. Thus, if we wished to know the height of the spring tide at a given place, and if we found that four different spring tides were measured as being of the height of ten, thirteen, eleven, and fourteen feet, we should conclude that the true height of the tide was the *Mean* of these numbers,—namely, twelve feet; and we should suppose that the deviation from this height, in the individual cases, arose from the accidents of weather, the imperfections of observation, or the operation of other laws, besides the alternation of spring and neap tides.

This process of finding the Mean of an assemblage of observed numbers is much practised in discovering, and still more in confirming and correcting, laws of phenomena. We shall notice a few of its peculiarities.

9. The Method of Means requires a knowledge of the *Argument* of the changes which we would study; for the numbers must be arranged in certain Classes, before we find the Mean of each Class; and the principle on which this arrangement depends is the Argument. This knowledge of the Argument is more indispensably neces-

sary in the Method of Means than the Method of Curves; for when curves are drawn, the eye often spontaneously detects the law of recurrence in their sinuosities; but when we have collections of numbers, we must divide them into classes by a selection of our own. Thus, in order to discover the law which the heights of the tide follow, in the progress from spring to neap, we arrange the observed tides according to the *day of the moon's age;* and we then take the mean of all those which thus happen at the same period of the moon's revolution. In this manner we obtain the law which we seek; and the process is very nearly the same in all other applications of this Method of Means. In all cases, we begin by assuming the Classes of measures which we wish to compare, the Law which we could confirm or correct, the Formula of which we would determine the coefficients.

10. The Argument being thus assumed, the Method of Means is very efficacious in ridding our inquiry of errours and irregularities which would impede and perplex it. Irregularities which are altogether accidental, or at least accidental with reference to some law which we have under consideration, compensate each other in a very remarkable way, when we take the means of many observations. If we have before us a collection of observed tides, some of them may be elevated, some depressed by the wind, some noted too high and some too low by the observer, some augmented and some diminished by uncontemplated changes in the moon's distance or motion: but in the course of a year or two at the longest, all these causes of irregularity balance each other; and the law of succession, which runs through the observations, comes out as precisely as if those disturbing influences did not exist. In any particular case, there appears to be no possible reason why the deviation should be in one way, or of one moderate amount, rather than

another. But taking the mass of observations together, the deviations in opposite ways will be of equal amount, with a degree of exactness very striking. This is found to be the case in all inquiries where we have to deal with observed numbers upon a large scale. In the progress of the population of a country, for instance, what can appear more inconstant, in detail, than the causes which produce births and deaths? yet in each country, and even in each province of a country, the proportions of the whole numbers of births and deaths remain nearly constant. What can be more seemingly beyond the reach of rule than the occasions which produce letters that cannot find their destination? yet it appears that the number of "dead letters" is nearly the same from year to year. And the same is the result when the deviations arise, not from mere accident, but from laws perfectly regular, though not contemplated in our investigation*. Thus the effects of the Moon's Parallax upon the Tides, sometimes operating one way and sometimes another, according to certain rules, are quite eliminated by taking the Means of a long series of observations; the excesses and defects neutralizing each other, so far as concerns the effect upon any law of the tides which we would investigate.

11. In order to obtain very great accuracy, very large masses of observations are often employed by philosophers, and the accuracy of the result increases with the multitude of observations. The immense collections of astronomical observations which have in this manner been employed in order to form and correct the tables of the celestial motions are perhaps the most signal instances of the attempts to obtain accuracy by this accumulation of observations. Delambre's Tables of the Sun are

* Provided the argument of the law which we neglect have no coincidence with the argument of the law which we would determine.

founded upon nearly 3000 observations; Burg's Tables of the Moon upon above 4000.

But there are other instances hardly less remarkable. Mr. Lubbock's first investigations of the laws of the tides of London *, included above 13,000 observations, extending through nineteen years; it being considered that this large number was necessary to remove the effects of accidental causes †. And the attempts to discover the laws of change in the barometer have led to the performance of labours of equal amount: Laplace and Bouvard examined this question by means of observations made at the Observatory of Paris, four times every day for eight years.

12. We may remark one striking evidence of the accuracy thus obtained by employing large masses of observations. In this way we may often detect inequalities much smaller than the errours by which they are encumbered and concealed. Thus the diurnal oscillations of the barometer were discovered by the comparison of observations of many days, classified according to the hours of the day; and the result was a clear and incontestable proof of the existence of such oscillations, although the differences which these oscillations produce at different hours of the day are far smaller than the casual changes, hitherto reduced to no law, which go on from hour to hour and from day to day. The effect of

* *Phil. Trans.* 1831.

† This period of nineteen years was also selected for a reason which is alluded to in a former note. (p. 406.) It was thought that this period secured the inquirer from the errours which might be produced by the partial coincidence of the arguments of different irregularities; for example, those due to the moon's parallax and to the moon's declination. It has since been found (*Phil. Tr.* 1838. *On the Determination of the Laws of the Tides from Short Series of Observations,*) that with regard to parallax at least, the Means of one year give sufficient accuracy.

law, operating incessantly and steadily, makes itself more and more felt as we give it a longer range; while the effect of accident, followed out in the same manner, is to annihilate itself, and to disappear altogether from the result.

SECT. III.—*The Method of Least Squares.*

13. THE Method of Least Squares is in fact a method of means, but with some peculiar characters. Its object is to determine the *best Mean* of a number of observed quantities; or the *most probable Law* derived from a number of observations, of which some, or all, are allowed to be more or less imperfect. And the method proceeds upon this supposition;—that all errours are not *equally* probable, but that small errours are more probable than large ones. By reasoning mathematically upon this ground, we find that the best result is obtained (since we cannot obtain a result in which the errours vanish) by making, not the *Errours* themselves, but the *Sum of their Squares* of the *smallest* possible amount.

14. An example may illustrate this. Let a quantity which is known to increase uniformly, (as the distance of a star from the meridian at successive instants,) be measured at equal intervals of time, and be found to be successively 4, 12, 14. It is plain, upon the face of these observations that they are erroneous; for they ought to form an arithmetical progression, but they deviate widely from such a progression. But the question then occurs, what arithmetical progression do they *most probably* represent: for we may assume several arithmetical progressions which more or less approach the observed series; as for instance, these three; 4, 9, 14; 6, 10, 14; 5, 10, 15. Now in order to see the claims of each of these to the truth, we may tabulate them thus.

Observation		Errours.	Sums of Errours.	Sums of Squares of Errours.
	4, 12, 14			
Series (1)	4, 9, 14 ...	0, 3, 0 ...	3 ...	9
„ (2)	6, 10, 14 ...	2, 2, 0 ...	4 ...	8
„ (3)	5, 10, 15 ...	1, 2, 1 ...	4 ...	6

Here, although the first series gives the sum of the errours less than the others, the third series gives the sum of the squares of the errours least; and is therefore, by the proposition on which this Method depends, the *most probable* series of the three.

This Method, in more extensive and complex cases, is a great aid to the calculator in his inferences from facts, and removes much that is arbitrary in the Method of Means.

SECT. IV.—*The Method of Residues.*

15. BY either of the preceding Methods we obtain, from observed facts, such laws as readily offer themselves; and by the laws thus discovered, the most prominent changes of the observed quantities are accounted for. But in many cases we have, as we have noticed already, *several* laws of nature operating at the same time, and combining their influences to modify those quantities which are the subjects of observation. In these cases we may, by successive applications of the Methods already pointed out, detect such laws one after another: but this successive process, though only a repetition of what we have already described, offers some peculiar features which make it convenient to consider it in a separate Section, as the Method of Residues.

16. When we have, in a series of changes of a variable quantity, discovered *one* Law which the changes follow, detected its argument, and determined its magnitude so as to explain most clearly the course of observed facts, we may still find that the observed changes are not fully

accounted for. When we compare the results of our Law with the observations, there may be a difference, or as we may term it, a *Residue*, still unexplained. But this Residue being thus detached from the rest, may be examined and scrutinized in the same manner as the whole observed quantity was treated at first: and we may in this way detect in *it* also a Law of change. If we can do this, we must accommodate this new found Law as nearly as possible to the Residue to which it belongs; and this being done, the difference of our Rule and of the Residue itself, forms *a Second Residue*. This Second Residue we may again bring under our consideration; and may perhaps in *it* also discover some Law of change by which its alterations may be in some measure accounted for. If this can be done, so as to account for a large portion of this Residue, the remaining unexplained part forms a *Third Residue;* and so on.

17. This course has really been followed in various inquiries, especially in those of Astronomy and Tidology. The *Equation of the Center*, for the moon, was obtained out of the *Residue* of the Longitude, which remained when the *Mean Anomaly* was taken away. This Equation being applied and disposed of, the *Second Residue* thus obtained, gave to Ptolemy the *Evection*. The *Third Residue*, left by the Equation of the Center and the Evection, supplied to Tycho the *Variation* and the *Annual Equation*. And the Residue, remaining from these, has been exhausted by other equations, of various arguments, suggested by theory or by observation. In this case, the successive generations of astronomers have gone on, each in its turn executing some step in this Method of Residues. In the examination of the Tides, on the other hand, this method has been applied systematically and at once. The observations readily gave the *Semimensual Inequality;* the *Residue* of this

supplied the corrections due to the Moon's *Parallax* and *Declination*; and when these were determined, the *remaining Residue* was explored for the law of the Solar Correction.

18. In a certain degree, the Method of Residues and the Method of Means are *opposite* to each other. For the Method of Residues extricates Laws from their combination, *bringing them into view in succession*; while the Method of Means discovers each Law, not by bringing the others into view, but by *destroying their effect* through an accumulation of observations. By the Method of Residues we should *first* extract the Law of the Parallax Correction of the Tides, and *then*, from the Residue left by this, obtain the Declination Correction. But we might at once employ the Method of Means, and put together all the cases in which the Declination was the same; not allowing for the Parallax in each case, but taking for granted that the Parallaxes belonging to the same Declination would neutralize each other; as many falling above as below the mean parallax. In cases like this, where the Method of Means is not impeded by a partial coincidence of the Arguments of different unknown Inequalities, it may be employed with almost as much success as the Method of Residues. But still, when the Arguments of the Laws are clearly known, as in this instance, the Method of Residues is more clear and direct, and is the rather to be recommended.

19. If for example, we wish to learn whether the Height of the Barometer exerts any sensible influence on the Height of the Sea's Surface, it would appear that the most satisfactory mode of proceeding, must be to subtract, in the first place, what we know to be the effects of the Moon's Age, Parallax and Declination, and other ascertained causes of change; and to search in the *unexplained Residue* for the effects of barometrical pressure. The con-

trary course has, however, been adopted, and the effect of the barometer on the ocean has been investigated by the direct application of the Method of Means, classing the observed heights of the water according to the corresponding heights of the barometer without any previous reduction. In this manner, the suspicion that the tide of the sea is affected by the pressure of the atmosphere, has been confirmed. This investigation must be looked upon as a remarkable instance of the efficacy of the Method of Means, since the amount of the barometrical effect is much smaller than the other changes from among which it was by this process extricated. But an application of the Method of Residues would still be desirable on a subject of such extent and difficulty.

20. Sir John Herschel, in his *Discourse on the Study of Natural Philosophy* (Articles 158—161), has pointed out the mode of making discoveries by studying Residual Phenomena; and has given several illustrations of the process. In some of these, he has also considered this method in a wider sense than we have done; treating it as not applicable to quantity only, but to properties and relations of different kinds.

We likewise shall proceed to offer a few remarks on Methods of Induction applicable to other relations than those of quantity.

Chapter VIII.

METHODS OF INDUCTION DEPENDING ON RESEMBLANCE.

Sect. I.—*The Law of Continuity.*

1. The Law of Continuity is applicable to quantity primarily, and therefore might be associated with the

methods treated of in the last chapter: but inasmuch as its inferences are made by a transition from one degree to another among contiguous cases, it will be found to belong more properly to the Methods of Induction of which we have now to speak.

The *Law of Continuity* consists in this proposition,—That a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate degrees of magnitude according to the intermediate conditions. And this law may often be employed to correct inaccurate inductions, and to reject distinctions which have no real foundation in nature. For example, the Aristotelians made a distinction between motions according to nature, as that of a body falling vertically downwards, and motions contrary to nature, as that of a body moving along a horizontal plane: the former, they held, became naturally quicker and quicker, the latter naturally slower and slower. But to this it might be replied, that a horizontal line may pass, by gradual motion, through various inclined positions, to a vertical position: and thus the retarded motion may pass into the accelerated; and hence there must be some inclined plane on which the motion downwards is naturally uniform: which is false, and therefore the distinction of such kinds of motion is unfounded. Again, the proof of the First Law of Motion depends upon the Law of Continuity: for since, by diminishing the resistance to a body moving on a horizontal plane, we diminish the retardation, and this without limit, the law of continuity will bring us at the same time to the case of no resistance and to the case of no retardation.

2. The Law of Continuity is asserted by Galileo in a particular application; and the assertion which it suggests is by him referred to Plato;—namely*, that a moveable

* *Dialog.* III. 150. IV. 32.

body cannot pass from rest to a determinate degree of velocity without passing through all smaller degrees of velocity. This law, however, was first asserted in a more general and abstract form by Leibnitz*: and was employed by him to show that the laws of motion propounded by Descartes must be false. The Third Cartesian Law of Motion was this†: that when one moving body meets another, if the first body have a less momentum than the second, it will be reflected with its whole motion: but if the first have a greater momentum than the second, it will lose a part of its motion, which it will transfer to the second. Now each of these cases leads, by the Law of Continuity, to the case in which the two bodies have *equal* momentums: but in this case, by the first part of the law the body would *retain all* its motion; and by the second part of the law it would *lose* a portion of it: hence the Cartesian Law is false.

3. I shall take another example of the application of this Law from Professor Playfair's Dissertation on the History of Mathematical and Physical Science‡. "The Academy of Sciences at Paris having (in 1724) proposed, as a Prize Question, the Investigation of the Laws of the Communication of Motion, John Bernoulli presented an Essay on the subject very ingenious and profound; in which, however, he denied the existence of hard bodies, because in the collision of such bodies, a finite change of motion must take place in an instant: an event which, on the principle just explained, he maintained to be impossible." And this reasoning was justifiable: for we can form a *continuous* transition from cases in which the impact manifestly occupies a finite time, (as when we strike a large soft body) to cases in which it is apparently instantaneous. Maclaurin and others are disposed, in

* *Opera*, I. 366. † Cartes. *Prin.*, p. 35.
‡ In the *Encyc. Brit.*, p 537.

order to avoid the conclusion of Bernoulli, to reject the Law of Continuity. This, however, would not only be, as Playfair says, to deprive ourselves of an auxiliary, commonly useful though sometimes deceptive; but what is much worse, to acquiesce in false propositions, from the want of clear and patient thinking. For the Law of Continuity, when rightly interpreted, is *never* violated in actual fact. There are not really any such bodies as have been termed *perfectly hard:* and if we approach towards such cases, we must learn the laws of motion which rule them by attending to the Law of Continuity, not by rejecting it.

4. Newton used the Law of Continuity to suggest, but not to prove, the doctrine of universal gravitation. Let, he said, a terrestrial body be carried as high as the moon: will it not still fall to the earth? and does not the moon fall by the same force*? Again: if any one says that there is a material ether which does not gravitate†, this kind of matter, by condensation, may be gradually transmuted to the density of the most intensely gravitating bodies: and these gravitating bodies, by taking the internal texture of the condensed ether, may cease to gravitate; and thus the weight of bodies depends, not on their quantity of matter, but on their texture; which doctrine Newton conceived he had disproved by experiment.

5. The evidence of the Law of Continuity resides in the universality of those ideas, which enter into our apprehension of Laws of Nature. When, of two quantities, one depends upon the other, the Law of Continuity necessarily governs this dependence. Every philosopher has the power of applying this law, in proportion as he has the faculty of apprehending the ideas which he employs in his induction, with the same clearness and steadiness

* *Principia*, Lib. III. Prop. 6. † *Ib.*, Cor. 2.

which belong to the fundamental ideas of quantity, space and number. To those who possess this faculty, the Law is a Rule of very wide and decisive application. Its use, as has appeared in the above examples, is seen rather in the disproof of erroneous views, and in the correction of false propositions, than in the invention of new truths. It is a test of truth, rather than an instrument of discovery.

Methods, however, approaching very near to the Law of Continuity may be employed as positive means of obtaining new truths; and these I shall now describe.

SECT. II.—*The Method of Gradation.*

6. To gather together the cases which resemble each other, and to separate those which are essentially distinct, has often been described as the main business of science; and may, in a certain loose and vague manner of speaking, pass for a description of some of the leading procedures in the acquirement of knowledge. The selection of instances which agree, and of instances which differ, in some prominent point or property, are important steps in the formation of science. But when classes of things and properties have been established in virtue of such comparisons, it may still be doubtful whether these classes are separated by distinctions of opposites, or by differences of degree. And to settle such questions, the *Method of Gradation* is employed; which consists in taking intermediate stages of the properties in question, so as to ascertain by experiment whether, in the transition from one class to another, we have to leap over a manifest gap, or to follow a continuous road.

7. Thus for instance, one of the early *Divisions* established by electrical philosophers was that of *Electrics* and *Conductors*. But this division Faraday has overturned as an essential opposition. He takes* a *Gradation* which

* *Researches*, 12th Series, Art. 1328.

carries him from Conductors to Non-conductors. Sulphur, or lac, he says, are held to be non-conductors, but are not rigorously so. Spermaceti is a bad conductor: ice or water better than spermaceti: metals so much better that they are put in a different class. But even in metals the transit of the electricity is not instantaneous: we have in them proof of a retardation of the electric current: "and what reason," Mr. Faraday asks, "why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, [and insulation is induction*] why should we refuse the same relation to the same exhibitions of force in the metals?"

The process employed by the same sagacious philosopher to show the *identity* of Voltaic and Franklinic electricity, is another example of the same kind†. Machine [Franklinic] electricity was made to exhibit the same phenomena as Voltaic electricity, by causing the discharge to pass through a bad conductor, into a very extensive discharging train: and thus it was clearly shown that Franklinic electricity, not so conducted, differs from the other kinds, only in being in a state of successive tension and explosion instead of a state of continued current.

Again; to show that the decomposition of bodies in the Voltaic circuit was not due to the Attraction of the Poles‡, Mr. Faraday devised a beautiful series of experiments, in which these supposed *Poles* were made to assume all possible electrical conditions:—in which the decomposition took place against air, which according to common language is not a conductor, nor is decomposed;—against the metalic poles, which are excellent conduc-

* These words refer to another proposition, also established by the Method of Gradation. † *Hist. Ind. Sci.*, B. XIV. c. ix. sect. 2.

‡ *Ibid.*, *Researches*, Art. 497.

tors but undecomposable: and hence he infers that the decomposition cannot justly be considered as due to the Attraction, or Attractive Powers, of the Poles.

8. The reader of the *Novum Organon* may perhaps, in looking at such examples of the Rule, be reminded of some of Bacon's classes of instances, as his *instantiæ absentiæ in proximo*, and his *instantiæ migrantes*. But we may remark that instances classed and treated as Bacon recommends in those parts of his work, could hardly lead to scientific truth. His processes are vitiated by his proposing to himself the *form* or *cause* of the property before him, as the object of his enquiry; instead of being content to obtain, in the first place, the *law of phenomena*. Thus his example* of a migrating instance is thus given. "Let the *nature inquired into* be that of whiteness; an instance migrating to the production of this property is glass, first whole, and then pulverized; or plain water, and water agitated into a foam; for glass and water are transparent, and not white; but glass powder and foam are white, and not transparent. Hence we must inquire what has happened to the glass or water in that migration. For it is plain that the *form of whiteness* is conveyed and induced by the crushing of the glass and shaking of the water."

9. We may easily give examples from other subjects in which the method of gradation has been used to establish, or to endeavour to establish, very extensive propositions. Thus Laplace's Nebular Hypothesis,—that systems like our solar system are formed by gradual condensation from diffused masses, such as the nebulæ among the stars,—is founded by him upon an application of this Method of Gradation. We see, he conceives, among these nebulæ, instances of all degrees of condensation, from the most loosely diffused fluid, to that separation

* *Nov. Org.*, Lib. II. Aph. 28.

and solidification of parts by which suns, and satellites, and planets are formed: and thus we have before us instances of systems in all their stages; as in a forest we see trees in every period of growth. How far the examples in this case satisfy the demands of the Method of Gradation, it remains for astronomers and philosophers to examine.

Again; this method was used with great success by Macculloch and others to refute the opinion, put in currency by the Wernerian school of geologists, that the rocks called *trap rocks* must be classed with those to which a *sedimentary* origin is ascribed. For it was shown that a gradual *transition* might be traced from those examples in which trap rocks most resembled stratified rocks, to the lavas which have been recently ejected from volcanoes: and that it was impossible to assign a different origin to one portion, and to the other, of this kind of mineral masses; and as the volcanic rocks were certainly not sedimentary, it followed, that the trap rocks were not of that nature.

Again; we have an attempt of a still larger kind made by Mr. Lyell, to apply this Method of Gradation so as to disprove all distinction between the causes by which geological phenomena have been produced, and the causes which are now acting at the earth's surface. He has collected a very remarkable series of changes which have taken place, and are still taking place, by the action of water, volcanoes, earthquakes, and other terrestrial operations; and he conceives he has shown in these a *gradation* which leads, with no wide chasm or violent leap, to the state of things of which geological researches have supplied the evidence.

10. Of the value of this Method in geological speculations, no doubt can be entertained. Yet it must still require a grave and profound consideration, in so vast an

application of the Method as that attempted by Mr. Lyell, to determine what extent we may allow to the steps of our *gradation;* and to decide how far the changes which have taken place in distant parts of the series may exceed those of which we have historical knowledge, before they cease to be of the *same kind.* Those who, dwelling in a city, see, from time to time, one house built and another pulled down, may say that such *existing causes*, operating through past time, sufficiently explain the existing condition of the city. Yet we arrive at important political and historical truths, by considering the *origin* of a city as an event of a *different order* from those daily changes. The causes which are now working to produce geological results, may be supposed to have been, at some former epoch, so far exaggerated in their operation, that the changes should be paroxysms, not degrees;—that they should violate, not continue, the gradual series. And we have no kind of evidence whether the duration of our historical times is sufficient to give us a just measure of the limits of such degrees;—whether the terms which we have under our notice enable us to ascertain the average rate of progression.

11. The result of such considerations seems to be this:—that we may apply the Method of Gradation in the investigation of geological causes, provided we leave the Limits of the Gradation undefined. But, then, this is equivalent to the admission of the opposite hypothesis: for a continuity of which the successive intervals are not limited, is not distinguishable from discontinuity. The geological sects of recent times have been distinguished as *uniformitarians* and *catastrophists:* the Method of Gradation seems to prove the doctrine of the uniformitarians; but then, at the same time that it does this, it breaks down the distinction between them and the catastrophists.

There are other exemplifications of the use of gradations in Science which well deserve notice: but some of them are of a kind somewhat different, and may be considered under a separate head.

Sect. III. *The Method of Natural Classification.*

12. The method of natural classification consists, as we have seen, in grouping together objects, not according to any selected properties, but according to their most important resemblances; and in combining such grouping with the assignation of certain marks of the classes thus formed. The examples of the successful application of this method are to be found in the Classificatory Sciences through their whole extent; as, for example, in framing the Genera of plants and animals. The same method, however, may often be extended to other sciences. Thus the classification of crystalline forms, according to their degree of symmetry, (which is really an important distinction,) as introduced by Mohs and Weiss, was a great improvement upon Haüy's arbitrary division according to certain assumed primary forms. Sir David Brewster was led to the same distinction of crystals by the study of their optical properties; and the scientific value of the classification was thus strongly exhibited. Mr. Howard's classification of clouds appears to be founded in their real nature, since it enables him to express the laws of their changes and successions. As we have elsewhere said, the criterion of a true classification is, that it makes general propositions possible. One of the most prominent examples of the beneficial influence of a right classification, is to be seen in the impulse given to geology by the distinction of strata according to the organic fossils which they contain*:

* *Hist. Ind. Sci.*, B. XVIII. c. ii. sect. 3.

which, ever since its general adoption, has been a leading principle in the speculations of geologists.

13. The mode in which, in this and in other cases, the Method of Natural Classification directs the researches of the philosopher, is this:—his arrangement being adopted, at least as an instrument of inquiry and trial, he follows the course of the different members of the classification, according to the guidance which Nature herself offers; not prescribing beforehand the marks of each part, but distributing the facts according to the total resemblances, or according to those resemblances which he finds to be most important. Thus, in tracing the course of a series of strata from place to place, we identify each stratum, not by any single character, but by all taken together;—texture, colour, fossils, position, and any other circumstances which offer themselves. And if, by this means, we come to ambiguous cases, where different indications appear to point different ways, we decide so as best to preserve undamaged those general relations and truths which constitute the value of our system. Thus although we consider the organic fossils in each stratum as its most important characteristic, we are not prevented, by the disappearance of some fossils, or the addition of others, or by the total absence of fossils, from identifying strata in distant countries, if the position and other circumstances authorize us to do so. And by this Method of Classification, the doctrine of *Geological Equivalents** has been applied to a great part of Europe.

14. We may further observe, that the same method of natural classification which thus enables us to identify strata in remote situations, notwithstanding there may be great differences in their material and contents, also forbids us to assume the identity of the series of rocks which

* *Hist. Ind. Sci.*, B. XVIII. c. iii. sect. 4.

occur in different countries, when this identity has not been verified by such a continuous exploration of the component members of the series. It would be in the highest degree unphilosophical to apply the special names of the English or German strata to the rocks of India, or America, or even of southern Europe, till it has appeared that in those countries the geological series of northern Europe really exists. In each separate country, the divisions of the formations which compose the crust of the earth must be made out, by applying the Method of Natural Arrangement *to that particular case*, and not by arbitrarily extending to it the nomenclature belonging to another case. It is only by such precautions, that we can ever succeed in obtaining geological propositions, at the same time true and comprehensive; or can obtain any sound general views respecting the physical history of the earth.

15. The Method of Natural Classification, which we thus recommend, falls in with those mental habits which we formerly described as resulting from the study of natural history. The method was then termed the *Method of Type*, and was put in opposition to the *Method of Definition*.

The Method of Natural Classification is directly opposed to the process in which we assume and apply *arbitrary* definitions; for in the former Method, we find our classes in nature, and do not make them by marks of our own imposition. Nor can any advantage to the progress of knowledge be procured, by laying down our characters when our arrangements are as yet quite loose and unformed. Nothing was gained by the attempts to *define* Metals by their weight, their hardness, their ductility, their colour; for to all these marks, as fast as they were proposed, exceptions were found, among bodies which still could not be excluded from the list of Metals. It was only when elementary substances were divided into *Natural Classes*, of which classes Metals were one, that a true view of their distinctive characters was obtained Definitions in the outset of our examination of nature are almost always, not only useless, but prejudicial.

16. When we obtain a law of nature by induction from phenomena, it commonly happens, as we have already seen, that we introduce, at the same time, a Proposition and a Definition. In this case, the two are correlative, each giving a real value to the other. In such cases, also, the Definition, as well as the Proposition, may become the basis of rigorous reasoning, and may lead to a series of deductive truths. We have examples of such Definitions and Propositions in the laws of motion, and in many other cases.

17. When we have established Natural Classes of objects, we seek for Characters of our classes; and these Characters may, to a certain extent, be called the *Definitions* of our classes. This is to be understood, however, only in a limited sense: for these Definitions are not absolute and permanent. They are liable to be modified and superseded. If we find a case which manifestly belongs to our Natural Class, though violating our Definition, we do not shut out the case, but alter our definition. Thus, when we have made it part of our Definition of the *Rose* family, that they have *alternate stipulate leaves*, we do not, therefore, exclude from the family the genus *Lowœa*, which has *no stipulæ*. In Natural Classifications, our Definitions are to be considered as temporary and provisional only. When Mr. Lyell established the distinctions of the tertiary strata, which he termed *Eocene*, *Miocene*, and *Pliocene*, he took a numerical criterion (the proportion of recent species of shells contained in those strata) as the basis of his division. But now that those kinds of strata have become, by their

application to a great variety of cases, a series of Natural Classes, we must, in our researches, keep in view the natural connexion of the formations themselves in different places; and must by no means allow ourselves to be governed by the numerical proportions which were originally contemplated; or even by any amended numerical criterion equally arbitrary; for however amended, Definitions in natural history are never immortal. The etymologies of *Pliocene* and *Miocene* may, hereafter, come to have merely an historical interest; and such a state of things will be no more inconvenient, provided the natural connexions of each class are retained, than it is to call a rock *oolite* or *porphyry*, when it has no roelike structure and no fiery spots.

The Methods of Induction which are treated of in this and the preceding chapter, and which are specially applicable to causes governed by relations of Quantity or of Resemblance, commonly lead us to *Laws of Phenomena* only. Inductions founded upon other ideas, those of Substance and Cause for example, appear to conduct us somewhat further into a knowledge of the essential nature and real connexions of things. But before we speak of these, we shall say a few words respecting the way in which inductive propositions, once obtained, may be verified and carried into effect by their application.

Chapter IX.

OF THE APPLICATION OF INDUCTIVE TRUTHS.

1. By the application of inductive truths, we here mean, according to the arrangement given in Chap. I. of this Book, those steps, which in the natural order of science, follow the discovery of each truth. These steps

are, the *verification* of the discovery by additional experiments and reasonings, and its *extension* to new cases, not contemplated by the original discoverer. These processes occupy that period, which, in the history of each great discovery, we have termed the *Sequel* of the epoch; as the collection of facts, and the elucidation of conceptions, form its Prelude.

2. It is not necessary to dwell at length on the processes of the verification of discoveries. When the law of nature is once stated, it is far easier to devise and execute experiments which prove it, than it was to discern the evidence before. The truth becomes one of the standard doctrines of the science to which it belongs, and is verified by all who study or who teach the science experimentally. The leading doctrines of chemistry are constantly exemplified by each chemist in his *Laboratory;* and an amount of verification is thus obtained of which books give no adequate conception. In astronomy, we have a still stronger example of the process of verifying discoveries. Ever since the science assumed a systematic form, there have been *Observatories*, in which the consequences of the theory were habitually compared with the results of observation. And to facilitate this comparison, *Tables* of great extent have been calculated, with immense labour, from each theory, showing the place which the theory assigned to the heavenly bodies at successive times; and thus, as it were, challenging nature to deny the truth of the discovery. In this way, as I have elsewhere stated, the continued prevalence of an errour in the systematic parts of astronomy is impossible*. An errour, if it arise, makes its way into the tables, into the ephemeris, into the observer's nightly list, or his sheet of reductions; the evidence of sense flies in its face in a thousand Observatories; the dis-

* *Hist. Ind. Sci.*, B. VII. c. vi. sect. 6.

crepancy is traced to its source, and soon disappears for ever.

3. In these last expressions, we suppose the theory, not only to be tested, but also to be corrected when it is found to be imperfect. And this also is part of the business of the observing astronomer. From his accumulated observations, he deduces more exact values than had previously been obtained, of the *Coefficients* of these Inequalities of which the *Argument* is already known. This he is enabled to do by the methods explained in the fifth chapter of this Book; the Method of Means, and especially the Method of Least Squares. In other cases, he finds, by the Method of Residues, some new Inequality; for if no change of the Coefficients will bring the Tables and the observation to a coincidence, he knows that a new Term is wanting in his formula. He obtains, as far as he can, the law of this unknown Term; and when its existence and its law have been fully established, there remains the task of tracing it to its cause.

4. The condition of the science of Astronomy, with regard to its security and prospect of progress, is one of singular felicity. It is a question well worth our consideration, as regarding the interests of science, whether, in other branches of knowledge also, *a continued and connected system of observation and calculation*, imitating the system employed by astronomers, might not be adopted. But the discussion of this question would involve us in a digression too wide for the present occasion.

5. There is another mode of application of true theories after their discovery, of which we must also speak; I mean the process of showing that facts, not included in the original induction, and apparently of a different kind, are explained by reasonings founded upon the theory. The history of physical astronomy is full of such events.

Thus after Bradley and Wargentin had observed a certain cycle among the perturbations of Jupiter's satellites, Laplace explained this cycle by the doctrine of universal gravitation*. The long inequality of Jupiter and Saturn, the diminution of the obliquity of the ecliptic, the acceleration of the moon's mean motion, were in like manner accounted for by Laplace. The coincidence of the nodes of the moon's equator with those of her orbit was proved to result from mechanical principles by Lagrange. The motions of the recently-discovered planets, and of comets, shown by various mathematicians to be in exact accordance with the theory, are verifications and extensions still more obvious.

6. In many of the cases just noticed, the consistency between the theory, and the consequences thus proved to result from it, is so far from being evident, that the most consummate command of all the powers and aids of mathematical reasoning is needed, to enable the philosopher to arrive at the result. In consequence of this circumstance, the labours just referred to, of Laplace, Lagrange, and others, have been the object of very great and very just admiration. Moreover, the necessary connexion of new facts, at first deemed inexplicable, with principles already known to be true;—a connexion utterly invisible at the outset, and yet at last established with the certainty of demonstration;—strikes us with the delight of a new discovery; and at first sight appears no less admirable than an original induction. Accordingly, men sometimes appear tempted to consider Laplace and other great mathematicians as persons of a kindred genius to Newton. We must not forget, however, that there is a great and essential difference between inductive and deductive processes of the mind. The discovery of a *new* theory, which is true, is a step widely distinct

* *Hist. Ind. Sci.*, B. VII. c. IV. sect. 3.

from any mere developement of the consequences of a theory already invented and established.

7. As an example, in another field, of the extension of a discovery by applying it to the explanation of new phenomena, we may adduce Wells's *Inquiry into the Cause of Dew*. For this investigation, although it has sometimes been praised as an original discovery, was, in fact, only resolving the phenomenon into principles already discovered. The atmologists of the last century were aware* that the vapour which exists in air in an invisible state may be condensed into water by cold; and they had noticed that there is always a certain temperature, lower than that of the atmosphere, to which if we depress bodies, water forms upon them in fine drops. This temperature is the limit of that which is necessary to constitute vapour, and is hence called the *constituent temperature*. But these principles were not generally familiar in England till Dr. Wells introduced them into his *Essay on Dew*, published in 1814; having indeed been in a great measure led to them by his own experiments and reasonings. His explanation of Dew, —that it arises from the coldness of the bodies on which it settles,—was established with great ingenuity; and is a very elegant confirmation of the Theory of Constituent Temperature.

8. The example of all the best writers who have previously treated of the philosophy of sciences, from Bacon to Herschel, draws our attention to those instances of the application of scientific truths, which are subservient to the uses of practical life; to the support, the preservation, the pleasure of man. It is well known in how large a degree the furtherance of these objects constituted the merit of the *Novum Organon* in the eyes of its author; and the enthusiasm with which men regard

* *Hist. Ind. Sci.*, B. x. c. iii. sect. 5.

these visible and tangible manifestations of the power and advantage which knowledge may bring, has gone on increasing up to our own day. Such useful inventions as we here refer to must always be objects of great philosophical, as well as practical interest; and it might be well worth our while, did our present limits allow, to discuss the bearing of such inventions upon the formation and progress of science. For the present, it must suffice to observe that those practical inventions which are of most importance in the Arts, are rarely or never of any material consequence to Science; for they are either mere practical processes, which the artist practises, but which the scientist cannot account for: or at most, they depend upon some of the inferior generalizations of science for their reason, and do not tend to confirm or illustrate the higher points at which theory has arrived. These considerations must be our apology for not entering into this discussion at the present advanced stage of our undertaking. As we have already said, knowledge is power; but its interest for us in the present work, is not that it is power, but that it is knowledge. The effect which the application of science to general practical uses has in *diffusing* a knowledge of theoretical principles, and thus in giving to men's minds an intellectual culture, is indeed well worthy our attention; but the consideration of this subject must be reserved for some future occasion.

We must now conclude our task by a few words on the subject of inductions involving Ideas ulterior to those already considered.

CHAPTER X.

OF THE INDUCTION OF CAUSES.

1. WE formerly* stated the objects of the researches of Science to be Laws of Phenomena and Causes; and showed the propriety and the necessity of not resting in the former object, but extending our inquiries to the latter also. Inductions, in which phenomena are connected by relations of Space, Time, Number and Resemblance, belong to the former class; and of the Methods applicable to such Inductions we have treated already. In proceeding to Inductions governed by any ulterior Ideas, we can no longer lay down any Special Methods by which our procedure may be directed. A few general remarks are all that we shall offer.

The principal Maxim in such cases of Induction is the obvious one:—that we must be careful to possess and to apply, with perfect clearness and precision, the Fundamental Idea on which the Induction depends.

We may illustrate this in a few cases.

2. *Induction of Substance.*—The Idea of Substance† involves this axiom, that the weight of the whole compound must be equal to the weights of the separate elements, whatever changes the composition or separation of the elements may have occasioned. The application of this Maxim we may term the *Method of the Balance.* We have seen‡ how the memorable revolution in Chemistry, the overthrow of phlogiston, and the establishment of the oxygen theory, was produced by the application of this Method. We have seen too§ that the same Idea leads us to this Maxim;—that *Imponderable Fluids* are not to be admitted as *chemical* elements of bodies.

* Book XI. c. vii.

† *Ibid.*, VI. c. iii.

‡ *Ibid.*, B. VI. c. iv.

§ *Ibid.*

Whether those which have been termed *Imponderable Fluids*,—the supposed fluids which produce the phenomena of Light, Heat, Electricity, Galvanism, Magnetism,—really exist or no, is a question, not merely of the *Laws*, but of the *Causes* of Phenomena. It is, as has already been shown, a question which we cannot help discussing, but which is at present involved in great obscurity. Nor does it appear at all likely that we shall obtain a true view of the cause of Light, Heat, and Electricity, till we have discovered precise and general laws connecting optical, thermotical, and electrical *phenomena* with those chemical doctrines to which the Idea of Substance is necessarily applied.

3. *Induction of Force.*—The inference of *Mechanical Forces* from phenomena has been so abundantly practised, that it is perfectly familiar among scientific inquirers. From the time of Newton, it has been the most common aim of mathematicians; and a persuasion has grown up among them, that mechanical forces,—attraction and repulsion,—are the only modes of action of the particles of bodies which we shall ultimately have to consider. I have attempted to show that this mode of conception is inadequate to the purposes of sound philosophy;—that the particles of crystals, and the elements of chemical compounds, must be supposed to be combined in some other way than by mere mechanical attraction and repulsion. Dr. Faraday has gone further in shaking the usual conceptions of the force exerted, in well-known cases. Among the most noted and conspicuous instances of attraction and repulsion exerted at a distance, were those which take place between electrized bodies. But the eminent electrician just mentioned has endeavoured to establish, by experiments of which it is very difficult to elude the weight, that the action in these cases does not take place at a distance, but is the result of a chain

of intermediate particles connected at every point by forces of another kind.

4. *Induction of Polarity.*—The forces to which Mr. Faraday ascribes the action in these cases are *Polar Forces.* We have already endeavoured to explain the Idea of Polar Forces; which implies† that at every point forces exactly equal act in opposite directions; and thus, in the greater part of their course, neutralize and conceal each other; while at the extremities of the line, being by some cause liberated, they are manifested, still equal and opposite. And the criterion by which this polar character of forces is recognized, is implied in the reasoning of Faraday, on the question of one or two electricities, of which we formerly spoke‡. The maxim is this:—that in the action of polar forces, along with every manifestation of force or property, there exists a corresponding and simultaneous manifestation of an equal and opposite force or property.

5. As it was the habit of the last age to reduce all action to mechanical forces, the present race of physical speculators appears inclined to reduce all forces to polar forces. Mosotti has endeavoured to show that the positive and negative electricities pervade all bodies, and that gravity is only an apparent excess of one of the kinds over the other. As we have seen, Faraday has given strong experimental grounds for believing that the supposed remote actions of electrized bodies are really the effects of polar forces among contiguous particles. If this doctrine were established with regard to all electrical, magnetical, and chemical forces, we might ask, whether, while all other forces are polar, gravity really affords a single exception to the universal rule? Is not the universe pervaded by an omnipresent anta-

* *Researches*, 12th Series. † B. v. c. i.

‡ *Ibid.*

gonism, a fundamental conjunction of contraries, everywhere opposite, nowhere independent? We are, as yet, far from the position in which Inductive Science can enable us to answer such inquiries.

6. *Induction of Ulterior Causes.*—The first Induction of a Cause does not close the business of scientific inquiry. Behind proximate causes, there are ulterior causes, perhaps a succession of such. Gravity is the cause of the motions of the planets; but what is the cause of gravity? This is a question which has occupied men's minds from the time of Newton to the present day. Earthquakes and volcanoes are the causes of many geological phenomena; but what is the cause of those subterraneous operations? This inquiry after ulterior causes is an inevitable result from the intellectual constitution of man. He discovers mechanical causes, but he cannot rest in them. He must needs ask, whence it is that matter has its universal power of attracting matter. He discovers polar forces: but even if these be universal, he still desires a further insight into the cause of this polarity. He sees, in organic structures, convincing marks of adaptation to an end: whence, he asks, is this adaptation? He traces in the history of the earth a chain of causes and effects operating through time: but what, he inquires, is the power which holds the end of this chain?

Thus we are referred back from step to step, in the order of causation, in the same manner as, in the palætiological sciences, we were referred back in the order of time. We make discovery after discovery in the various regions of science; each, it may be, satisfactory, and in itself complete, but none final. Something always remains undone. The last question answered, the answer suggests still another question. The strain of music from the lyre of Science flows on, rich and sweet, full and harmonious, but never reaches a close: no cadence is heard with which the intellectual ear can feel satisfied.

Of the Supreme Cause.—In the utterance of Science, no cadence is heard with which the human mind can feel satisfied. Yet we cannot but go on listening for and expecting a satisfactory close. The notion of a cadence appears to be essential to our relish of the music. The idea of some closing strain seems to lurk among our own thoughts, waiting to be articulated in the notes which flow from the knowledge of external nature. The idea of something ultimate in our philosophical researches, something in which the mind can acquiesce, and which will leave us no further questions to ask, of *whence*, and *why*, and *by what power*, seems as if it belonged to us;—as if we could not have it withheld from us by any imperfection or incompleteness in the actual performances of science. What is the meaning of this conviction? What is the reality thus anticipated? Whither does the developement of this Idea conduct us?

We have already seen that a difficulty of the same kind, which arises in the contemplation of causes and effects considered as forming an historical series, drives us to the assumption of a First Cause, as an Axiom to which our Idea of Causation in time necessarily leads. And as we were thus guided to a First Cause in order of Succession, the same kind of necessity directs us to a Supreme Cause in order of Causation.

On this most weighty subject it is difficult to speak fitly; and the present is not the proper occasion, even for most of that which may be said. But there are one or two remarks which flow from the general train of the contemplations we have been engaged in, and with which this Work must conclude.

We have seen how different are the kinds of cause to which we are led by scientific researches. *Mechanical*

Forces are insufficient without *Chemical Affinities*; Chemical agencies fail us, and we are compelled to have recourse to *Vital Powers;* Vital Powers cannot be merely physical, and we must believe in something hyperphysical, something of the nature of a *Soul.* Not only do biological inquiries lead us to assume an animal soul, but they drive us much further; they bring before us *Perception,* and *Will* evoked by Perception. Still more, these inquiries disclose to us *Ideas* as the necessary forms of Perception, in the actions of which we ourselves are conscious. We are aware, we cannot help being aware, of our Ideas and our Volitions as belonging to *us,* and thus we pass from *things* to *persons;* we have the idea of *Personality* awakened. And the idea of Design and *Purpose,* of which we are conscious in our own minds, we find reflected back to us, with a distinctness which we cannot overlook, in all the arrangements which constitute the frame of organized beings.

We cannot but reflect how widely diverse are the kinds of principles thus set before us;—by what vast strides we mount from the lower to the higher, as we proceed through that series of causes which the range of the sciences thus brings under our notice. Yet we know how narrow is the range of these sciences when compared with the whole extent of human knowledge. We cannot doubt that on many other subjects, besides those included in physical speculation, man has made out solid and satisfactory trains of connexion;—has discovered clear and indisputable evidence of causation. It is manifest, therefore, that, if we are to attempt to ascend to the Supreme Cause—if we are to try to frame an idea of the Cause of all these subordinate causes;—we must conceive it as more different from any of them, than the most diverse are from each other;—more elevated above the highest, than the highest is above the lowest.

But further;—though the Supreme Cause must thus be inconceivably different from all subordinate causes, and immeasurably elevated above them all, it must still include in itself all that is essential to each of them, by virtue of that very circumstance that it is the Cause of their Causality. Time and Space,—Infinite Time and Infinite Space,—must be among its attributes; for we cannot but conceive Infinite Time and Space as attributes of the Infinite Cause of the Universe. Force and Matter must depend upon it for their efficacy; for we cannot conceive the activity of Force, or the resistance of Matter, to be independent powers. But these are its lower attributes. The Vital Powers, the Animal Soul, which are the Causes of the actions of living things, are only the Effects of the Supreme Cause of Life. And this Cause, even in the lowest forms of organized bodies, and still more in those which stand higher in the scale, involves a reference to Ends and Purposes, in short, to manifest Final Causes. Since this is so, and since, even when we contemplate ourselves in a view studiously narrowed, we still find that we have Ideas, and Will and Personality, it would render our philosophy utterly incoherent and inconsistent with itself, to suppose that Personality, and Ideas, and Will, and Purpose, do not belong to the Supreme Cause from which we derive all that we have and all that we are.

But we may go a step further;—though, in our present field of speculation, we confine ourselves to knowledge founded on the facts which the external world presents to us, we cannot forget, in speaking of such a theme as that to which we have thus been led, that these are but a small, and the least significant portion of the facts which bear upon it. We cannot fail to recollect that there are facts belonging to the world within us, which more readily and strongly direct our thoughts to the Supreme Cause of all things. We can plainly discern that we have Ideas elevated above the region of mechanical causation, of animal existence, even of mere choice and will, which still have a clear and definite significance, a permanent and indestructible validity. We perceive as a fact, that we have a Conscience, judging of Right and Wrong; that we have Ideas of Moral Good and Evil; that we are compelled to conceive the organization of the moral world, as well as of the vital frame, to be directed to an end and governed by a purpose. And since the Supreme Cause is the cause of these facts, the Origin of these Ideas, we cannot refuse to recognize Him as not only the Maker, but the Governor of the World; as not only a Creative, but a Providential Power; as not only a Universal Father, but an Ultimate Judge.

We have already passed beyond the boundary of those speculations which we proposed to ourselves as the basis of our conclusions. Yet we may be allowed to add one other reflection. If we find in ourselves Ideas of Good and Evil, manifestly bestowed upon us to be the guides of our conduct, which guides we yet find it impossible consistently to obey;—if we find ourselves directed, even by our natural light, to aim at a perfection of our moral nature from which we are constantly deviating through weakness and perverseness;—if, when we thus lapse and err, we can find, in the region of human philosophy, no power which can efface our aberrations, or reconcile our actual with our ideal being, or give us any steady hope and trust with regard to our actions, after we have thus discovered their incongruity with their genuine standard;—if we discern that this is our condition, how can we fail to see that it is in the highest degree consistent with all the indications supplied by such a philosophy as that of which we have been attempting to lay the foundations, that the Supreme Cause, through whom man exists as a moral being of vast capacities and infinite hopes, should have Himself provided a teaching for our ignorance, a propitiation for our sin, a support for our weakness, a purification and sanctification of our nature?

And thus, in concluding our long survey of the grounds and structure of science, and of the lessons which the study of it teaches us, we find ourselves brought to a point of view in which we can cordially sympathize, and more than sympathize, with all the loftiest expressions of admiration and reverence and hope and trust, which have been uttered by those who in former times have spoken of the elevated thoughts to which the contemplation of the nature and progress of human knowledge gives rise. We can not only hold with Galen, and Harvey, and all the great physiologists, that the organs of animals give evidence of a purpose; —not only assert with Cuvier that this conviction of a purpose can alone enable us to understand every part of every living thing;—not only say with Newton that "every true step made in philosophy brings us nearer to the First Cause, and is on that account highly to be valued;"—and that "the business of natural philosophy is to deduce causes from effects, till we come to the very First Cause, which certainly is not mechanical:"—but we can go much further, and declare, still with Newton, that "this beautiful system could have its origin no other way than by the purpose and command of an intelligent and powerful Being, who governs all things, not as the soul of the world, but as the Lord of the Universe; who is not only God, but Lord and Governor."

When we have advanced so far, there yet remains one step. We may recollect the prayer of one, the master in this school of the philosophy of science: "This also we humbly and earnestly beg;—that human things may not prejudice such as are divine;—neither that from the unlocking of the gates of sense, and the kindling of a greater natural light, anything may arise of incredulity or intellectual night towards divine mysteries; but rather that by our minds thoroughly purged and cleansed from fancy and vanity, and yet subject and perfectly given up to the divine oracles, there may be given unto faith the things that are faith's." When we are thus prepared for a higher teaching, we may be ready to listen to a greater than Bacon, when he says to those who have sought their God in the material universe, "Whom ye ignorantly worship, him declare I unto you." And when we recollect how utterly inadequate all human language has been shown to be, to express the nature of that Supreme Cause of the Natural, and Rational, and Moral, and Spiritual world, to which our Philosophy points with trembling finger and shaded eyes, we may receive, with the less wonder but with the more reverence, the declaration which has been vouchsafed to us:

ΕΝ ΑΡΧΗ ΗΝ ʽΟ ΛΟΓΟΣ, ΚΑΙ ʽΟ ΛΟΓΟΣ ΗΝ ΠΡΟΣ ΤΟΝ ΘΕΟΝ, ΚΑΙ ΘΕΟΣ ΗΝ ʽΟ ΛΟΓΟΣ.

APHORISMS.

THE following Aphorisms exhibit some of the principal results of the views and discussions contained in the preceding pages of this work, expressed in a compact manner, and detached from the reasonings on which they rest. At the end of each Aphorism reference is made to the Book and Chapter where its import is discussed in the work.

Along with these, I shall add some other Aphorisms on the subject of the Language of Science; a subject in which it appears to be time to collect, from the usage of the most judicious writers, some rules which may tend to preserve the purity and analogies of scientific language from wanton and needless violation. As this subject is not discussed in the work itself, I have given, along with these Aphorisms, such examples as may tend to confirm and illustrate them, and have applied them to some cases at present unsettled.

APHORISMS CONCERNING IDEAS.

I.

Man is the Interpreter of Nature, Science the right interpretation. (Book I. Chapter 1.)

II.

The *Senses* place before us the *Characters* of the Book of Nature; but these convey no knowledge to us, till we have discovered the Alphabet by which they are to be read. (I. 2.)

III.

The *Alphabet*, by means of which we interpret Phenomena, consists of the *Ideas* existing in our own minds; for these give to the phenomena that coherence and significance which is not an object of sense. (I. 2.)

IV.

The antithesis of *Sense* and *Ideas* is the foundation of the Philosophy of Science. No knowledge can exist without the union, no philosophy without the separation, of these two elements. (I. 2.)

V.

Fact and *Theory* correspond to Sense on the one hand, and to Ideas on the other, so far as we are *conscious* of our Ideas: but all facts involve ideas *unconsciously;* and thus the distinction of Facts and Theories is not tenable, as that of Sense and Ideas is. (I. 2.)

VI.

Sensations and Ideas in our knowledge are like Matter and Form in bodies. Matter cannot exist without Form, nor Form without Matter: yet the two are altogether distinct and opposite. There is no possibility either of separating, or of confounding them. The same is the case with Sensations and Ideas. (I. 2.)

VII.

Ideas are not *trans*formed, but *in*formed Sensations; for without ideas, sensations have no form. (I. 2.)

VIII.

The Sensations are the *Objective*, the Ideas the *Subjective* part of every act of perception or knowledge. (I. 2.)

IX.

General Terms denote *Ideal Conceptions*, as a *circle*, an *orbit*, a *rose*. These are not *Images* of real things, as was held by the Realists, but Conceptions: yet they are conceptions, not bound together by mere *Name*, as the Nominalists held, but by an Idea. (I. 2.)

X.

It has been said by some, that all Conceptions are merely *states* or *feelings of the mind*, but this assertion only tends to confound what it is our business to distinguish. (I. 2.)

XI.

Observed Facts are connected so as to produce new truths, by superinducing upon them an Idea: and such truths are obtained *by Induction*. (I. 2.)

XII.

Truths once obtained by legitimate Induction are Facts: these Facts may be again connected, so as to produce higher truths: and thus we advance to *Successive Generalizations*. (I. 2.)

XIII.

Truths obtained by Induction are made compact and permanent by being expressed in *Technical Terms.* (I. 3.)

XIV.

Experience cannot conduct us to universal and necessary truths:—Not to universal, because she has not tried all cases:—Not to necessary, because necessity is not a matter to which experience can testify. (I. 5.)

XV.

Necessary truths derive their necessity from the *Ideas* which they involve; and the existence of necessary truths proves the existence of Ideas not generated by experience. (I. 5.)

XVI.

In Deductive Reasoning, we cannot have any truth in the conclusion which is not virtually contained in the premises. (I. 6.)

XVII.

In order to acquire any exact and solid knowledge, the student must possess with perfect precision the ideas appropriate to that part of knowledge: and this precision is tested by the student's *perceiving* the axiomatic evidence of the *axioms* belonging to each *Fundamental Idea*. (I. 6.)

XVIII.

The Fundamental Ideas which it is most important to consider, as being the Bases of the Material Sciences, are the Ideas of *Space*, *Time* (including Number), *Cause* (including Force and Matter), *Outness* of Objects, and *Media* of Perception of Secondary Qualities, *Polarity*

(Contrariety), Chemical *Composition* and *Affinity*, *Substance*, *Likeness* and Natural *Affinity*, *Means and Ends* (whence the notion of Organization), *Symmetry*, and the Ideas of *Vital Powers*. (I. 8.)

XIX.

The Sciences which depend upon the Ideas of Space and Number are *Pure* Sciences, not *Inductive* Sciences: they do not infer special Theories from Facts, but deduce the conditions of all theory from Ideas. The Elementary Pure Sciences, or Elementary Mathematics, are Geometry, Theoretical Arithmetic and Algebra. (II. 1.)

XX.

The Ideas on which the Pure Sciences depend, are those of *Space* and *Number*; but Number is a modification of the conception of Repetition, which belongs to the Idea of *Time*. (II. 1.)

XXI.

The *Idea of Space* is not derived from experience, for experience of external objects *pre*supposes bodies to exist in Space. Space is a condition under which the mind receives the impressions of sense, and therefore the relations of space are necessarily and universally true of all perceived objects. Space is a *form* of our perceptions, and regulates them, whatever the *matter* of them may be. (II. 2.)

XXII.

Space is not a General Notion collected by abstraction from particular cases; for we do not speak of *Spaces* in general, but of universal or absolute *Space*. Absolute Space is infinite. All special spaces are *in* absolute space, and are parts of it. (II. 3.)

XXIII.

Space is not a real object or thing, distinct from the objects which exist in it; but it is a real condition of the existence of external objects. (II. 3.)

XXIV.

We have an *Intuition* of objects in space; that is, we contemplate objects as *made up* of spatial parts, and apprehend their spatial relations by the same act by which we apprehend the objects themselves. (II. 3.)

XXV.

Form or Figure is space limited by boundaries. Space has necessarily *three* dimensions, length, breadth, depth; and no others which cannot be resolved into these. (II. 3.)

XXVI.

The Idea of Space is exhibited for scientific purposes, by the *Definitions* and *Axioms* of Geometry; such, for instance, as these:—the *Definition of a Right Angle*, and *of a Circle*;—the *Definition of Parallel Lines*, and the *Axiom* concerning them;—the Axiom that *two straight lines cannot inclose a space*. These Definitions are necessary, not arbitrary; and the Axioms are needed as well as the Definitions, in order to express the necessary conditions which the Idea of Space imposes. (II. 4.)

XXVII.

The Definitions and Axioms of Elementary Geometry do not *completely* exhibit the Idea of Space. In proceeding to the Higher Geometry, we may introduce other additional and independent Axioms; such as that of Archimedes, that *a curve line which joins two points is less than any broken line joining the same points and including the curve line*. (II. 4.)

XXVIII.

The perception of a *solid object* by sight requires that *act* of mind by which, from figure and shade, we infer distance and position in space. The perception of *figure* by sight requires that act of mind by which we give an outline to each object. (II. 6.)

XXIX.

The perception of Form by touch is not an impression on the passive sense, but requires an *act* of our muscular frame by which we become aware of the position of our own limbs. The perceptive faculty involved in this act has been called *the muscular sense*. (II. 6.)

XXX.

The *Idea of Time* is not derived from experience, for experience of changes *pre*supposes occurrences to take place in Time. Time is a condition under which the mind receives the impressions of sense, and therefore the relations of time are necessarily and universally true of all perceived occurrences. Time is a *form* of our perceptions, and regulates them, whatever the *matter* of them may be. (II. 7.)

XXXI.

Time is not a General Notion collected by abstraction from particular cases. For we do not speak of particular *Times* as examples of time in general, but as parts of a single and infinite *Time*. (II. 8.)

XXXII.

Time, like Space, is a form, not only of perception, but of *Intuition*. We consider the whole of any time as *equal* to the *sum* of the parts; and an occurrence as *coinciding* with the portion of time which it occupies. (II. 8.)

XXXIII.

Time is analogous to Space of *one dimension:* portions of both have a beginning and an end, are long or short. There is nothing in Time which is analogous to Space of two, or of three, dimensions, and thus nothing which corresponds to Figure. (II. 8.)

XXXIV.

The Repetition of a set of occurrences, as, for example, strong and weak, or long and short sounds, according to a steadfast order, produces *Rhythm*, which is a conception peculiar to Time, as Figure is to Space. (II. 8.)

XXXV.

The simplest form of Repetition is that in which there is no variety, and thus gives rise to the conception of *Number*. (II. 8.)

XXXVI.

The simplest numerical truths are seen by Intuition; when we endeavour to deduce the more complex from these simplest, we employ such maxims as these:—*If equals be added to equals the wholes are equal:—If equals be subtracted from equals the remainders are equal:—The whole is equal to the sum of all its parts.* (II. 9.)

XXXVII.

The Perception of Time involves a constant and latent kind of memory, which may be termed a *Sense of Succession.* The Perception of Number also involves this Sense of Succession, although in small numbers we appear to apprehend the units simultaneously and not successively. (II. 10.)

XXXVIII.

The Perception of Rhythm is not an impression on the passive sense, but requires an *act* of thought by which we connect and group the strokes which form the Rhythm. (II. 10.)

XXXIX.

Intuitive is opposed to *Discursive* reason. In intuition, we obtain our conclusions by dwelling upon *one* aspect of the fundamental Idea; in discursive reasoning, we combine *several* aspects of the Idea, (that is, several axioms,) and reason from the combination. (II. 11.)

XL.

Geometrical deduction (and deduction in general) is called *Synthesis*, because we introduce, at successive steps, the results of new principles. But in reasoning on the relations of space, we sometimes go on *separating* truths into their component truths, and these into other component truths; and so on; and this is geometrical *Analysis*. (II. 11.)

XLI.

Among the foundations of the Higher Mathematics, is the *Idea of Symbols* considered as general *Signs* of Quantity. This idea of a Sign is distinct from, and independent of other ideas. The Axiom to which we refer in reasoning by means of Symbols of quantity is this:—*The interpretation of such symbols must be perfectly general.* This Idea and Axiom are the bases of Algebra in its most general form. (II. 12.)

XLII.

Among the foundations of the Higher Mathematics is also the *Idea of a Limit.* The Idea of a Limit cannot be superseded by any other definitions or Hypotheses.

The Axiom which we employ in introducing this Idea into our reasoning is this:—*What is true up to the Limit is true at the Limit.* This Idea and Axiom are the bases of all Methods of Limits, Fluxions, Differentials, Variations, and the like. (II. 12.)

XLIII.

There is a *pure* Science of Motion, which does not depend upon observed facts, but upon the Idea of motion. It may also be termed *Pure Mechanism*, in opposition to Mechanics Proper, or *Machinery*, which involves the mechanical conceptions of force and matter. It has been proposed to name this Pure Science of Motion, *Kinematics.* (II. 13.)

XLIV.

The pure Mathematical Sciences must be successfully cultivated, in order that the progress of the principal Inductive Sciences may take place. This appears in the case of Astronomy, in which Science, both in ancient and in modern times, each advance of the theory has depended upon the previous solution of problems in pure mathematics. It appears also inversely in the Science of the Tides, in which, at present, we cannot advance in the theory, because we cannot solve the requisite problems in the Integral Calculus. (II. 14.)

XLV.

The *Idea of Cause*, modified into the conceptions of mechanical cause, or Force, and resistance to force, or Matter, is the foundation of the Mechanical Sciences; that is, Mechanics, (including Statics and Dynamics,) Hydrostatics, and Physical Astronomy. (III. 1.)

XLVI.

The Idea of Cause is not derived from experience; for in judging of occurrences which we contemplate, we consider them as being, universally and necessarily, Causes and Effects, which a finite experience could not authorize us to do. The Axiom, that every event must have a cause, is true independently of experience, and beyond the limits of experience. (III. 2.)

XLVII.

The Idea of Cause is expressed for purposes of science by these three Axioms:—*Every Event must have a Cause:—Causes are measured by their Effects:—Reaction is equal and opposite to Action.* (III. 4.)

XLVIII.

The Conception of Force involves the Idea of Cause, as applied to the motion and rest of bodies. The conception of *force* is suggested by muscular action exerted: the conception of *matter* arises from muscular action resisted. We necessarily ascribe to all bodies solidity and inertia, since we conceive Matter as that which cannot be compressed or moved without resistance (III. 5.)

XLIX.

Mechanical Science depends on the Conception of Force; and is divided into *Statics*, the doctrine of Force preventing motion, and *Dynamics*, the doctrine of Force producing motion. (III. 6.)

L.

The Science of Statics depends upon the Axiom, that Action and Reaction are equal, which in Statics assumes this form:—*When two equal weights are supported on the middle point between them, the pressure on the fulcrum is equal to the sum of the weights.* (III. 6.)

LI.

The Science of Hydrostatics depends upon the Fundamental Principle that *fluids press equally in all di-*

rections. This principle necessarily results from the conception of a Fluid, as a body of which the parts are perfectly moveable in all directions. For since the Fluid is a body, it can transmit pressure; and the transmitted pressure is equal to the original pressure, in virtue of the Axiom that Reaction is equal to Action. That the Fundamental Principle is not derived from experience, is plain both from its evidence and from its history. (III. 6.)

LII.

The Science of Dynamics depends upon the three Axioms above stated respecting Cause. The First Axiom, —that every change must have a Cause,—gives rise to the First Law of Motion,—that *a body not acted upon by a force will move with a uniform velocity in a straight line*. The Second Axiom,—that Causes are measured by their Effects,—gives rise to the Second Law of Motion,—that *when a force acts upon a body in motion, the effect of the force is compounded with the previously existing motion*. The Third Axiom,—that *Reaction is equal and opposite to Action*,—gives rise to the Third Law of Motion, which is expressed in the same terms as the Axiom; Action and Reaction being understood to signify momentum gained and lost (III. 7).

LIII.

The above Laws of Motion, historically speaking, were established by means of experiment: but since they have been discovered and reduced to their simplest form, they have been considered by many philosophers as self-evident. This result is principally due to the introduction and establishment of terms and definitions, which enable us to express the Laws in a very simple manner. (III. 7.)

LIV.

In the establishment of the Laws of Motion, it happened, in several instances, that Principles were assumed as self-evident which do not now appear evident, but which have since been demonstrated from the simplest and most evident principles. Thus it was assumed that *a perpetual motion is impossible*;—that *the velocities of bodies acquired by falling down planes or curves of the same vertical height are equal*;—that *the actual descent of the center of gravity is equal to its potential ascent.* But we are not hence to suppose that these assumptions were made without ground: for since they really follow from the laws of motion, they were probably, in the minds of the discoverers, the results of undeveloped demonstrations which their sagacity led them to divine. (III. 7.)

LV.

It is a *Paradox* that Experience should lead us to truths confessedly universal, and apparently necessary, such as the Laws of Motion are. The *Solution* of this paradox is, that these laws are interpretations of the Axioms of Causation. The axioms are universally and necessarily true, but the right interpretation of the terms which they involve, is learnt by experience. Our Idea of Cause supplies the *Form*, Experience, the *Matter*, of these Laws. (III. 8.)

LVI.

Primary Qualities of Bodies are those which we can conceive as directly perceived; *Secondary* Qualities are those which we conceive as perceived by means of a Medium. (IV. 1.)

LVII.

We necessarily perceive bodies as *without* us: the Idea of *Externality* is one of the conditions of perception. (IV. 1.)

LVIII.

We necessarily assume a *Medium* for the perceptions of Light, Colour, Sound, Heat, Odours, Tastes; and this Medium *must* convey impressions by means of its mechanical attributes. (IV. 1.)

LIX.

Secondary Qualities are not *extended* but *intensive;* their effects are not augmented by addition of parts, but by increased operation of the medium. Hence they are not measured directly, but by *scales;* not by *units,* but by *degrees.* (IV. 4.)

LX.

In the Scales of Secondary Qualities, it is a condition (in order that the scale may be complete,) that every example of the quality must either *agree* with one of the degrees of the Scale, or lie between two *contiguous* degrees. (IV. 4.)

LXI.

We perceive *by means of* a medium and *by means of* impressions on the nerves: but we do not (by our senses,) perceive either the medium or the impressions on the nerves. (IV. 1.)

LXII.

The *Prerogatives of the Sight* are, that by this sense we necessarily and immediately apprehend the *position* of its objects: and that from visible circumstances, we *infer* the *distance* of objects from us, so readily that we seem to perceive and not to infer. (IV. 2.)

LXIII.

The *Prerogatives of the Hearing* are, that by this sense we perceive relations perfectly precise and definite between two notes, namely, *Musical Intervals* (as an *Octave*, a *Fifth*); and that when two notes are perceived together, they are apprehended as distinct, (a *Chord*,) and as having a certain relation, (*Concord* or *Discord*.) (IV. 2.)

LXIV.

The Sight cannot decompose a compound colour into simple colours, or distinguish a compound from a simple colour. The Hearing cannot directly perceive the place, still less the distance, of its objects. We infer these obscurely and vaguely from audible circumstances. (IV. 2.)

LXV.

The *First Paradox of Vision* is, that we see objects *upright*, though the images on the retina are *inverted*. The solution is, that we do not see the image on the retina at all, we only see by means of it. (IV. 2.)

LXVI.

The *Second Paradox of Vision* is, that we see objects *single*, though there are two images on the retinas, one in each eye. The explanation is, that it is a Law of Vision that we see (small or distant) objects single, when their images fall on *corresponding points* of the two retinas. (IV. 2.)

LXVII.

The law of single vision for *near* objects is this:— When the two images in the two eyes are situated, part for part, nearly but not exactly, upon corresponding points, the object is apprehended as single and solid if the two objects are such as would be produced by a single solid object seen by the eyes separately. (IV. 2.)

LXVIII.

The ultimate object of each of the Secondary Mechanical Sciences is, to determine the nature and laws of the processes by which the impression of the Secondary Quality treated of is conveyed: but before we discover the *cause*, it may be necessary to determine the *laws* of the phenomena; and for this purpose a *Measure* or *Scale* of each quality is necessary. (IV. 4.)

LXIX.

Secondary qualities are measured by means of such effects as can be estimated in number or space. (IV. 4.)

LXX.

The Measure of Sounds, as high or low, is the *Musical Scale*, or *Harmonic Canon*. (IV. 4.)

LXXI.

The Measures of Pure Colours are the *Prismatic Scale*; the same, including *Fraunhofer's Lines*; and *Newton's Scale* of Colours. The principal Scales of Impure Colours are *Werner's Nomenclature* of Colours, and *Merimée's Nomenclature* of Colours. (IV. 4.)

LXXII.

The Idea of *Polarity* involves the conception of contrary properties in contrary directions:—the properties being, for example, attraction and repulsion, darkness and light, synthesis and analysis; and the contrary directions being those which are directly opposite, or, in some cases, those which are at right angles. (V. 1.)

LXXIII. (*Doubtful.*)

Coexistent polarities are fundamentally identical. (v. 2.)

LXXIV.

The Idea of Chemical *Affinity*, as implied in Elementary Composition, involves peculiar conceptions. It is not properly expressed by assuming the qualities of bodies to *resemble* those of the elements, or to depend on the *figure* of the elements, or on their *attractions*. (vi. 1.)

LXXV.

Attractions take place between bodies, Affinities between the particles of a body. The former may be compared to the alliances of states, the latter to the ties of family. (vi. 2.)

LXXVI.

The governing principles of Chemical Affinity are, that it is *elective*; that it is *definite*; that it *determines the properties* of the compound; and that *analysis is possible.* (vi. 2.)

LXXVII.

We have an idea of *Substance*: and an axiom involved in this Idea is, that *the weight of a body is the sum of the weights of all its elements.* (vi. 3).

LXXVIII.

Hence Imponderable Fluids are not to be admitted as chemical elements. (vi. 4.)

LXXIX.

The Doctrine of Atoms is admissible as a mode of expressing and calculating laws of nature; but is not proved by any fact, chemical or physical, as a philosophical truth. (vi. 5.)

LXXX.

We have an Idea of *Symmetry;* and an axiom involved in this Idea is, that a symmetrical natural body, if there be a tendency to modify any member in any manner, there is a tendency to modify all the corresponding members in the same manner. (VII. 1.)

LXXXI.

All hypotheses respecting the manner in which the elements of inorganic bodies are arranged in space, must be constructed with regard to the general facts of crystallization. (VII. 3.)

LXXXII.

When we consider any object as *One*, we give unity to it by an act of thought. The condition which determines what this unity shall include, and what it shall exclude, is this;—that assertions concerning the one thing shall be possible. (VIII. 1.)

LXXXIII.

We collect individuals into *Kinds* by applying to them the Idea of Likeness. Kinds of things are not determined by definitions, but by this condition;—that general assertions concerning such kinds of things shall be possible. (VIII. 1.)

LXXXIV.

The *Names* of kinds of things are governed by their use; and that may be a right name in one use which is not so in another. A whale is not a *fish* in natural history, but it is a *fish* in commerce and law. (VIII. 1.)

LXXXV.

We take for granted that each kind of things has a special *character* which may be expressed by a Definition. The ground of our assumption is this;—that reasoning must be possible. (VIII. 1.)

LXXXVI.

The "Five Words," *Genus, Species, Difference, Property, Accident*, were used by the Aristotelians, in order to express the subordination of kinds, and to describe the nature of definitions and propositions. In modern times, these technical expressions have been more referred to by Natural Historians than by Metaphysicians. (VIII. 1.)

LXXXVII.

The construction of a Classificatory Science includes *Terminology*, the formation of a descriptive language; —*Diataxis*, the Plan of the System of Classification, called also the *Systematick;—Diagnosis*, the Scheme of the Characters by which the different Classes are known, called also the *Characteristick*. *Physiography* is the knowledge which the System is employed to convey. Diataxis includes *Nomenclature*. (VIII. 2.)

LXXXVIII.

Terminology must be conventional, precise, constant; copious in words, and minute in distinctions, according to the needs of the science. The student must understand the terms, *directly* according to the convention, not through the medium of explanation or comparison. (VIII. 2.)

LXXXIX.

The *Diataxis*, or Plan of the System, may aim at a Natural or an Artificial System. But no classes can be absolutely artificial, for if they were, no assertions could be made concerning them. (VIII. 2.)

XC.

An *Artificial System* is one in which the *smaller* groups (the Genera) are *natural;* and in which the *wider* divisions (Classes, Orders) are constructed by the *peremptory* application of selected Characters; (selected, however, so as not to break up the smaller groups.) (VIII. 2.)

XCI.

A *Natural System* is one which attempts to make *all* the divisions *natural*, the widest as well as the narrowest; and therefore applies *no* characters *peremptorily*. (VIII. 2.)

XCII.

Natural Groups are best described, not by any definition which marks their boundaries, but by a *Type* which marks their center. The Type of any natural group is an example which possesses in a marked degree all the leading characters of the class. (VIII. 2.)

XCIII.

A Natural Group is steadily fixed, though not precisely limited; it is given in position, though not circumscribed; it is determined, not by a boundary without, but by a central point within;—not by what it strictly excludes, but by what it eminently includes;—by a Type, not by a Definition. (VIII. 2.)

XCIV.

The prevalence of Mathematics as an element of education has made us think Definition the philosophical mode of fixing the meaning of a word: if (Scientific) Natural History were introduced into education, men might become familiar with the fixation of the signification of words by Types; and this process agrees more nearly with the common processes by which words acquire their significations. (VIII. 2.)

XCV.

The attempts at Natural Classification are of three sorts; according as they are made by the process of *blind trial*, of *general comparison*, or of *subordination of characters*. The process of Blind Trial professes to make its classes by attention to *all* the characters, but without proceeding methodically. The process of General Comparison professes to enumerate all the characters, and forms its classes by the *majority*. Neither of these methods can really be carried into effect. The method of Subordination of Characters considers some characters as *more important* than others; and this method gives more consistent results than the others. This method, however, does not depend upon the Idea of Likeness only, but introduces the Idea of Organization or Function. (VIII. 2.)

XCVI.

A *Species* is a collection of individuals which are descended from a common stock, or which resemble such a collection as much as these resemble each other: the resemblance being opposed to a *definite* difference. (VIII. 2.)

XCVII.

A *Genus* is a collection of species which resemble each other more than they resemble other species: the resemblance being opposed to a *definite* difference. (VIII. 2.)

XCVIII.

The *Nomenclature* of a Classificatory Science is the collection of the names of the Species, Genera, and other divisions. The *binary* nomenclature, which denotes a species by the *generic* and *specific* name, is now commonly adopted in Natural History. (VIII. 2.)

XCIX.

The *Diagnosis*, or Scheme of the Characters, comes, in the order of philosophy, after the Classification. The characters do not *make* the classes, they only enable us to *recognize* them. The Diagnosis is an Artificial Key to a Natural System. (VIII. 2.)

C.

The basis of all Natural Systems of Classification is the Idea of Natural Affinity. The Principle which this Idea involves is this:—Natural arrangements, obtained from *different* sets of characters, must *coincide* with each other. (VIII. 4.)

CI.

In order to obtain a Science of Biology, we must analyze the Idea of Life. It has been proved by the biological speculations of past time, that Organic Life cannot rightly be solved into Mechanical or Chemical Forces, or the operation of a Vital Fluid, or of a Soul. (IX. 2.)

CII.

Life is a System of Vital Forces; and the conception of such Forces involves a peculiar Fundamental Idea. (IX. 3.)

CIII.

Mechanical, chemical, and vital Forces form an ascending progression, each including the preceding. Chemical Affinity includes in its nature Mechanical Force, and may often be practically resolved into Mechanical Force. (Thus the ingredients of gunpowder, liberated from their chemical union, exert great mechanical Force: a galvanic battery acting by chemical process does the like.) Vital Forces include in their nature both chemical Affinities and mechanical Forces: for

Vital Powers produce both chemical changes, (as digestion,) and motions which imply considerable mechanical force, (as the motion of the sap and of the blood. (IX. 4.)

CIV.

In *voluntary* motions, Sensations produce Actions, and the connexion is made by means of Ideas: in *reflected* motions, the connexion neither seems to be nor is made by means of Ideas: in *instinctive* motions, the connexion is such as requires Ideas, but we cannot believe the Ideas to exist. (IX. 5.)

CV.

The assumption of a Final Cause in the structure of each part of animals and plants is as inevitable as the assumption of an Efficient Cause for every event. The maxim that in organized bodies nothing is *in vain*, is as necessarily true as the maxim that nothing happens *by chance*. (IX. 6.)

CVI.

The idea of living beings as subject to *disease* includes a recognition of a Final Cause in organization; for disease is a state in which the vital forces do not attain their *proper ends*. (IX. 6.)

CVII.

The Palætiological Sciences depend upon the Idea of Cause; but the leading conception which they involve is that of *historical cause*, not mechanical cause. (X. 1.)

CVIII.

Each Palætiological Science, when complete, must possess three members: the *Phenomenology*, the *Ætiology*, and the *Theory*. (X. 2.)

CIX.

There are, in the Palætiological Sciences, two antagonist doctrines: *Catastrophes* and *Uniformity*. The doctrine of a *uniform course of nature* is tenable only when we extend the notion of Uniformity so far that it shall include Catastrophes. (x. 3.)

CX.

The Catastrophist constructs Theories, the Uniformitarian demolishes them. The former adduces evidence of an Origin, the latter explains the evidence away. The Catastrophist's dogmatism is undermined by the Uniformitarian's skeptical hypotheses. But when these hypotheses are asserted dogmatically, they cease to be consistent with the doctrine of Uniformity. (x. 3.)

CXI.

In each of the Palætiological Sciences, we can ascend to remote periods by a chain of causes, but in none can we ascend to a *beginning* of the chain. (x. 3.)

CXII.

In contemplating the series of causes and effects which constitutes the world, we necessarily assume a *First Cause* of the whole series. (x. 5.)

CXIII.

The Palætiological Sciences point backwards with lines which are broken, but which all converge to the *same* invisible point: and this point is the Origin of the Moral and Spiritual, as well as of the Natural World. (x. 5.)

APHORISMS CONCERNING SCIENCE.

I.

THE two processes by which Science is constructed are the *Explication of Conceptions* and the *Colligation of Facts*. (Book XI. Chap. 1.)

II.

The Explication of Conceptions, as requisite for the progress of science, has been effected by means of discussions and controversies among scientists; often by debates concerning definitions; these controversies have frequently led to the establishment of a Definition; but along with the Definition, a corresponding Proposition has always been expressed or implied. The essential requisite for the advance of science is the clearness of the Conception, not the establishment of a Definition. The construction of an exact Definition is often very difficult. The requisite conditions of clear Conceptions may often be expressed by Axioms as well as by Definitions. (XI. 2.)

III.

Conceptions, for purposes of science, must be *appropriate* as well as clear: that is, they must be modifications of that Fundamental Idea, by which the phenomena can really be interpreted. This maxim may warn us from errour, though it may not lead to discovery. Discovery depends upon the previous cultivation or natural clearness of the appropriate Idea, and therefore *no discovery is the work of accident*. (XI. 2.)

IV.

Facts are the materials of science, but all Facts involve Ideas. Since, in observing Facts, we cannot exclude Ideas, we must, for the purposes of science, take care that the Ideas are clear and rigorously applied. (XI. 3.)

V.

The last Aphorism leads to such Rules as the following:—That Facts, for the purposes of material science, must involve Conceptions of the Intellect only, and not Emotions:—That Facts must be observed with reference to our most exact conceptions, Number, Place, Figure, Motion:—That they must also be observed with reference to any other exact conceptions which the phenomena suggest, as Force, in mechanical phenomena, Concord, in musical. (XI. 3.)

VI.

The resolution of complex Facts into precise and measured partial Facts, we call the *Decomposition of Facts*. This process is requisite for the progress of science, but does not necessarily lead to progress. (XI. 3.)

VII.

Science begins with *common* observation of facts; but even at this stage, requires that the observations be precise. Hence the sciences which depend upon space and number were the earliest formed. After common observation, come Scientific *Observation* and *Experiment*. (XI. 4.)

VIII.

The Conceptions by which Facts are bound together, are suggested by the sagacity of discoverers. This sagacity cannot be taught. It commonly succeeds by guessing; and this success seems to consist in framing several *tentative hypotheses* and selecting the right one. But a supply of appropriate hypotheses cannot be constructed by rule, nor without inventive talent. (xi. 4.)

IX.

The truth of tentative hypotheses must be tested by their application to facts. The discoverer must be ready, carefully to try his hypotheses in this manner, and to reject them if they will not bear the test, in spite of indolence and vanity. (xi. 4.)

X.

The process of scientific discovery is cautious and rigorous, not by abstaining from hypotheses, but by rigorously comparing hypotheses with facts, and by resolutely rejecting all which the comparison does not confirm. (xi. 5.)

XI.

Hypotheses may be useful, though involving much that is superfluous, and even erroneous: for they may supply the true bond of connexion of the facts; and the superfluity and errour may afterwards be pared away. (xi. 5.)

XII.

It is a test of true theories not only to account for, but to predict phenomena. (xi. 5.)

XIII.

Induction is a term applied to describe the *process* of a true Colligation of Facts by means of an exact and appropriate Conception. *An Induction* is also employed to denote the *proposition* which results from this process. (xi. 5.)

XIV.

The Consilience of Inductions takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This Consilience is a test of the truth of the Theory in which it occurs. (XI. 5.)

XV.

An Induction is not the mere *sum* of the Facts which are colligated. The Facts are not only brought together, but seen in a new point of view. A new mental Element is *superinduced;* and a peculiar constitution and discipline of mind are requisite in order to make this Induction. (XI. 5.)

XVI.

Although in Every Induction a new conception is superinduced upon the Facts; yet this once effectually done, the novelty of the conception is overlooked, and the conception is considered as a part of the fact. (XI. 5.)

XVII.

The *Logic of Induction* consists in stating the Facts and the Inference in such a manner, that the Evidence of the Inference is manifest; just as the Logic of Deduction consists in stating the Premises and the Conclusion in such a manner that the Evidence of the Conclusion is manifest. (XI. 6.)

XVIII.

The Logic of Deduction is exhibited by means of a certain Formula; namely, a Syllogism; and every train of deductive reasoning, to be demonstrative, must be capable of resolution into a series of such Formulæ legitimately constructed. In like manner, the Logic of Induction may be exhibited by means of certain *Formulæ;*

and every train of inductive inference, to be sound, must be capable of resolution into a scheme of such Formulæ, legitimately constructed. (xi. 6.)

XIX.

The *inductive act of thought* by which several Facts are colligated into one Proposition, may be expressed by saying: *The several Facts are exactly expressed as one Fact, if, and only if, we adopt the Conceptions and the Assertion* of the Proposition. (xi. 6.)

XX.

The One Fact, thus inductively obtained from several Facts, may be combined with other Facts, and colligated with them by a new act of Induction. This process may be indefinitely repeated: and these successive processes are the *Steps* of Induction, or of *Generalization*, from the lowest to the highest. (xi. 6.)

XXI.

The relation of the successive Steps of Induction may be exhibited by means of an *Inductive Table*, in which the several Facts are indicated, and tied together by a Bracket, and the Inductive Inference placed on the other side of the Bracket; and this arrangement repeated, so as to form a genealogical Table of each Induction, from the lowest to the highest. (xi. 6.)

XXII.

The Logic of Induction is the *Criterion of Truth* inferred from Facts, as the Logic of Deduction is the Criterion of Truth deduced from necessary Principles. The Inductive Table enables us to apply such a Criterion; for we can determine whether each Induction is verified and justified by the Facts which its Bracket includes; and if each induction in particular be sound, the highest, which merely combines them all, must necessarily be sound also. (XI. 6.)

XXIII.

The distinction of *Fact* and *Theory* is only relative. Events and phenomena, considered as particulars which may be colligated by Induction, are *Facts;* considered as generalities already obtained by colligation of other Facts, they are *Theories.* The same event or phenomenon is a Fact or a Theory, according as it is considered as standing on one side or the other of the Inductive Bracket. (XI. 6.)

XXIV.

Inductive truths are of two kinds, *Laws of Phenomena*, and *Theories of Causes.* It is necessary to begin in every science with the Laws of Phenomena; but it is impossible that we should be satisfied to stop short of a Theory of Causes. In Physical Astronomy, Physical Optics, Geology, and other sciences, we have instances showing that we can make a great advance in inquiries after true Theories of Causes. (XI. 7.)

XXV.

Art and Science differ. The object of Science is Knowledge; the objects of Art, are Works. In Art, truth is a means to an end; in Science, it is the only end. Hence the Practical Arts are not to be classed among the Sciences. (XI. 8.)

XXVI.

Practical Knowledge, such as Art implies, is not Knowledge such as Science includes. Brute animals have a practical knowledge of relations of space and force; but they have no knowledge of Geometry or Mechanics. (XI. 8.)

XXVII.

The Methods by which the constructions of Science is promoted are, *Methods of Observation*, *Methods of obtaining clear Ideas*, and *Methods of Induction*. (XII. 1.)

XXVIII.

The Methods of Observation of Quantity in general are, *Numeration*, which is precise by the nature of Number; the *Measurement of Space* and *of Time*, which are easily made precise; the *Conversion of Space and Time*, by which each aids the measurement of the other; the *Method of Repetition;* the *Method of Coincidences* or *Interferences*. The measurement of Weight is made precise by the *Method of Double-weighing*. Secondary Qualities are measured by means of *Scales of Degrees;* but in order to apply these Scales, the student requires the *Education of the Senses*. The Education of the Senses is forwarded by the practical study of *Descriptive Natural History*, *Chemical Manipulation*, and *Astronomical Observation*. (XII. 2.)

XXIX.

The Methods by which the acquisition of clear Scientific Ideas is promoted, are mainly two; *Intellectual Education* and *Discussion of Ideas*. (XII. 3.)

XXX.

The Idea of Space becomes more clear by studying *Geometry;* the Idea of Force, by studying *Mechanics;* the Ideas of Likeness, of Kind, of Subordination of Classes, by studying *Natural History*. (XII. 3.)

XXXI.

Elementary Mechanics should now form a part of intellectual education, in order that the student may understand the Theory of Universal Gravitation: for an intellectual education should cultivate such ideas as enable the student to understand the most complete and admirable portions of the knowledge which the human race has attained to. (XII. 3.)

XXXII.

Natural History ought to form a part of intellectual education, in order to correct certain prejudices which arise from cultivating the intellect by means of mathematics alone; and in order to lead the student to see that the division of things into Kinds, and the attribution and use of Names, are processes susceptible of great precision. (XII. 3.)

XXXIII.

The conceptions involved in scientific truths have attained the requisite degree of clearness by means of the *Discussions* respecting ideas which have taken place among discoverers and their followers. Such discussions are very far from being unprofitable to science. They are metaphysical, and must be so: the difference between discoverers and barren reasoners is, that the former employ good, and the latter bad metaphysics. (XII. 4.)

XXXIV.

The Process of Induction may be resolved into three steps; the *Selection of the Idea*, the *Construction of the Conception*, and the *Determination of the Magnitudes*. (XII. 5.)

XXXV.

These three steps correspond to the determination of the *Independent Variable*, the *Formula*, and the *Coefficients*, in mathematical investigations; or to the *Argu-*

ment, the *Law*, and the *Numerical Data*, in a Table of an Inequality. (XII. 5.)

XXXVI.

The Selection of the Idea depends mainly upon inventive sagacity: which operates by suggesting and trying various hypotheses. Some inquirers try erroneous hypotheses; and thus, exhausting the forms of errour, form the Prelude to Discovery. (XII. 5.)

XXXVII.

The following Rules may be given, in order to the selection of the Idea for purposes of Induction:—the Idea and the Facts must be *homogeneous;* and the Rule must be *tested by the Facts.* (XII. 5.)

XXXVIII.

The Construction of the Conception very often includes, in a great measure, the Determination of the Magnitudes. (XII. 6.)

XXXIX.

When a series of *progressive* numbers is given as the result of observation, it may generally be reduced to law by combinations of arithmetical and geometrical progressions. (XII. 6.)

XL.

A true formula for a progressive series of numbers cannot commonly be obtained from a *narrow range* of observations. (XII. 6.)

XLI.

Recurrent series of numbers must, in most cases, be expressed by circular formulæ. (XII. 6.)

XLII.

The true construction of the conception is frequently suggested by some hypothesis; and in these cases, the hypothesis may be useful, though containing superfluous parts. (XII. 6.)

XLIII.

There are special Methods of Induction applicable to Quantity; of which the principal are, the *Method of Curves*, the *Method of Means*, the *Method of Least Squares*, and the *Method of Residues*. (XII. 7.)

XLIV.

The Method of Curves consists in drawing a curve, of which the observed quantities are the ordinates, the quantity on which the change of these quantities depends being the abscissa. The efficacy of this Method depends upon the faculty which the eye possesses, of readily detecting regularity and irregularity in forms. The Method may be used to detect the laws which the observed quantities follow; and also, when the observations are inexact, it may be used to correct these observations, so as to obtain data more true than the observed facts themselves. (XII. 7.)

XLV.

The Method of Means gets rid of irregularities by taking the arithmetical mean of a great number of observed quantities. Its efficacy depends upon this; that in cases in which observed quantities are affected by other inequalities, besides that of which we wish to determine the law, the excesses *above* and defects *below* the quantities which the law in question would produce, will, in a collection of many observations, *balance* each other. (XII. 7.)

XLVI.

The Method of Least Squares is a Method of Means, in which the mean is taken according to the condition, that the sum of the squares of the errours of observation shall be the least possible which the law of the facts allows. It appears, by the doctrine of chances, that this is the *most probable* mean. (XII. 7.)

XLVII.

The Method of Residues consists in subtracting, from the quantities given by observation, the quantity given by any law already discovered; and then examining the remainder, or *Residue*, in order to discover the leading law which it follows. When this second law has been discovered, the quantity given by it may be subtracted from the first Residue; thus giving a *Second Residue*, which may be examined in the same manner; and so on. The efficacy of this Method depends principally upon the circumstance of the laws of variation being successively smaller and smaller in amount (or at least in their mean effect); so that the ulterior undiscovered laws do not prevent the law in question from being *prominent* in the observations. (XII. 7.)

XLVIII.

The Method of Means and the Method of Least Squares cannot be applied without our *knowing the Arguments* of the Inequalities which we seek. The Method of Curves and the Method of Residues, when the Arguments of the principal Inequalities are known, often make it easy to find the others. (XII. 7.)

XLIX.

The Law of Continuity is this:—that a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate magnitudes according to the intermediate conditions. This Law may often be employed to disprove distinctions which have no real foundation. (XII. 8.)

L.

The Method of Gradation consists in taking a number of stages of a property in question, intermediate between two extreme cases which appear to be different. This Method is employed to determine whether the extreme cases are really distinct or not. (XII. 8.)

LI.

The Method of Gradation, applied to decide the question, whether the existing geological phenomena arise from existing causes, leads to this result:—That the phenomena do appear to arise from existing causes, but that the action of existing causes may, in past times, have transgressed, to any extent, their recorded limits of intensity. (XII. 8.)

LII.

The Method of Natural Classification consists in classing cases, not according to any assumed definition, but according to the connexion of the facts themselves, so as to make them the means of asserting general truths. (XII. 8.)

LIII.

In the *Induction of Causes* the principal Maxim is, that we must be careful to possess, and to apply, with perfect clearness, the Fundamental Idea on which the Induction depends. (XII. 10.)

LIV.

The Induction of Substance, of Force, of Polarity, go beyond mere laws of phenomena, and may be considered as the Induction of Causes. (XII. 10.)

LV.

The Cause of certain phenomena being inferred, we are led to inquire into the Cause of this Cause, which inquiry must be conducted in the same manner as the previous one; and thus we have the Induction of Ulterior Causes. (XII. 10.)

LVI.

In contemplating the series of Causes which are themselves the effects of other causes, we are necessarily led to assume a Supreme Cause in the Order of Causation, as we assume a First Cause in Order of Succession. (XII. 10.)

APHORISMS

CONCERNING THE LANGUAGE OF SCIENCE.

INTRODUCTION.

It has been shown in the *History of Science*, and has further appeared in the course of the present work, that almost every step in the progress of science is marked by the formation or appropriation of a technical term. Common language has, in most cases, a certain degree of looseness and ambiguity; as common knowledge has usually something of vagueness and indistinctness. In common cases too, knowledge usually does not occupy the intellect alone, but more or less interests some affection, or puts in action the fancy; and common language, accommodating itself to the office of expressing such knowledge, contains, in every sentence, a tinge of emotion or of imagination. But when our knowledge becomes perfectly exact and purely intellectual, we require a language which shall also be exact and intellectual;—which shall exclude alike vagueness and fancy, imperfection and superfluity;—in which each term shall convey a meaning steadily fixed and rigorously limited. Such a language that of science becomes, through the use of Technical Terms. And we must now endeavour to lay down some maxims and suggestions, by attention to which Technical Terms may be better fitted to answer their purpose. In order to do this, we shall in the first place take a rapid survey of the manner in which Technical Terms have been employed from the earliest periods of scientific history.

The progress of the use of technical scientific language offers to our notice two different and successive periods; in the first of which, technical terms were formed casually, as convenience in each case prompted; while in the second period, technical language was constructed intentionally, with set purpose, with a regard to its connexion, and with a view of constructing a system. Though the casual and the systematic formation of technical terms cannot be separated by any precise date of time, (for at all periods some terms in some sciences have been framed unsystematically,) we may, as a general description, call the former the *Ancient* and the latter the *Modern* Period. In illustrating the two following Aphorisms, I will give examples of the course followed in each of these periods.

APHORISM I.

In the Ancient Period of Science, Technical Terms were formed in three different ways:—by appropriating common words and fixing their meaning;—by constructing terms containing a description;—by constructing terms containing reference to a theory.

THE earliest sciences offer the earliest examples of technical terms. These are Geometry, Arithmetic, and Astronomy; to which we have soon after to add Harmonics, Mechanics, and Optics. In these sciences, we may notice the above-mentioned three different modes in which technical terms were formed.

I. The simplest and first mode of acquiring technical terms, is to take words current in common usage, and by rigorously defining or otherwise fixing their meaning, to fit them for the expression of scientific truths. In this manner almost all the fundamental technical terms of Geometry were formed. A *sphere*, a *cone*, a *cylinder*, had among the Greeks, at first, meanings less precise than those which geometers gave to these words, and besides the mere designation of form, implied some use or application. A *sphere* (σφαῖρα) was a hand-ball used in games; a *cone* (κῶνος) was a boy's spinning-top, or the crest of a helmet; a *cylinder* (κύλινδρος) was a roller; a *cube* (κύβος) was a die: till these words were adopted by the geometers, and made to signify among them pure modifications of space. So an *angle* (γωνία) was only a corner; a *point* (σημεῖον) was a signal; a *line* (γραμμή) was a mark; a *straight* line (εὐθεῖα) was marked by an adjective which at first meant only *direct.* A *plane* (ἐπίπεδον) is the neuter form of an adjective, which by its derivation means *on the ground*, and hence *flat.* In all these cases, the word adopted as a term of science has its sense rigorously fixed; and where the common use of the term is in any degree vague, its meaning may be modified at the same time that it is thus limited. Thus a *rhombus* (ῥόμβος) by its derivation, might mean any figure which is *twisted* out of a regular form; but it is confined by geometers to that figure which has four equal sides, its angles being oblique. In like manner, a *trapezium* (τραπέζιον) originally signifies a *table*, and thus might denote any form; but as the tables of the Greeks had one side shorter than the opposite one, such a figure was at first called a *trapezium.* Afterwards the term was made to signify any figure with four unequal sides; a name being more needful in geometry for this kind of figure than for the original form.

This class of technical terms, namely, words adopted from common language, but rendered precise and determinate for purposes of science, may also be exemplified in other sciences. Thus, as was observed in the early portion of the history of astronomy*, a *day*, a *month*,

* *Hist. Ind. Sci.*, B. III. c. i.

a *year*, described at first portions of time marked by familiar changes, but afterwards portions determined by rigorous mathematical definitions. The conception of the heavens as a revolving sphere, is so obvious, that we may consider the terms which involve this conception as parts of common language; as the *pole* (πόλος); the *arctic circle*, which includes the stars that never set*; the *horizon* (ὁρίζων) a boundary, applied technically to the circle bounding the visible earth and sky. The *turnings of the sun* (τροπαὶ ἠελίοιο), which are mentioned by Hesiod, gave occasion to the term *tropics*, the circles at which the sun in his annual motion turns back from his northward or southward advance. The *zones* of the earth, (the *torrid*, *temperate*, and *frigid*;) the *gnomon* of a dial; the *limb* (or border) of the moon, or of a circular instrument, are terms of the same class. An *eclipse* (ἔκλειψις) is originally a deficiency or disappearance, and joined with the name of the luminary, an *eclipse of the sun* or *of the moon*, described the phenomenon; but when the term became technical, it sufficed, without addition, to designate the phenomenon.

In Mechanics, the Greeks gave a scientific precision to very few words: we may mention *weights* (βαρέα), the *arms of a lever* (μήκεα), its *fulcrum* (ὑπομόχλιον), and the verb *to balance* (ἰσοῤῥοπεῖν). Other terms which they used, as *momentum* (ῥοπή) and *force* (δύναμις), did not acquire a distinct and definite meaning till the time of Galileo, or later. We may observe that all abstract terms, though in their scientific application expressing mere conceptions, were probably at first derived from some word describing external objects. Thus the Latin word for force, *vis*, seems to be connected with a Greek word, ἶς, or Fίς, which often has nearly the same meaning; but originally, as it would seem, signified a sinew or muscle, the obvious seat of animal strength.

* *Hist. Ast.*, B. III. c. i. sect. 8.

In later times, the limitation imposed upon a word by its appropriation to scientific purposes, is often more marked than in the cases above described. Thus the *variation* is made to mean, in astronomy, the second inequality of the moon's motion; in magnetism, the *variation* signifies the angular deviation of the compass-needle from the north; in pure mathematics, the *variation* of a quantity is the formula which expresses the result of any small change of the most general kind. In like manner, *parallax* (παράλλαξις) denotes a *change* in general, but is used by astronomers to signify the change produced by the spectator's being removed from the center of the earth, his theoretical place, to the surface. *Alkali* at first denoted the ashes of a particular plant, but afterwards, all bodies having a certain class of chemical properties; and, in like manner, *acid*, the class opposed to alkali, was modified in signification by chemists, so as to refer no longer to the taste.

Words thus borrowed from common language, and converted by scientific writers into technical terms, have some advantages and some disadvantages. They possess this great convenience, that they are understood after a very short explanation, and retained in the memory without effort. On the other hand, they lead to some inconvenience; for since they have a meaning in common language, a careless reader is prone to disregard the technical limitation of this meaning, and to attempt to collect their import in scientific books, in the same vague and conjectural manner in which he collects the purpose of words in common cases. Hence the language of science, when thus resembling common language, is liable to be employed with an absence of that scientific precision which alone gives it value. Popular writers and talkers, when they speak of *force*, *momentum*, *action and reaction*, and the like, often afford examples of the inaccuracy thus arising from the scientific appropriation of common terms.

II. Another class of technical terms, which we find occurring as soon as speculative science assumes a distinct shape, consists of those which are intentionally constructed by speculators, and which contain some description or indication distinctive of the conception to which they are applied. Such are a *parallelogram* (παραλληλόγραμμον), which denotes a plane figure bounded by two pairs of parallel lines; a *parallelopiped* (παραλληλοπίπεδον), which signifies a solid figure bounded by three pairs of parallel planes. A *triangle* (τρίγωνος, *trigon*) and a *quadrangle* (τετράγωνος, *tetragon*) were perhaps words invented independently of the mathematicians: but such words extended to other cases, *pentagon*, *decagon*, *heccædecagon*, *polygon*, are inventions of scientific men. Such also are *tetrahedron*, *hexahedron*, *dodecahedron*, *tesseracontaoctohedron*, *polyhedron*, and the like. These words being constructed by speculative writers, explain themselves, or at least require only some conventional limitation, easily adopted. Thus *parallelogram* might mean a figure bounded by any number of sets of parallel lines, but it is conventionally restricted to a figure of *four* sides. So a *great circle* in a sphere means one which passes through the center of the sphere; and a *small circle* is any other. So in trigonometry, we have the *hypotenuse* (ὑποτενίουσα), or *subtending* line, to designate the line subtending an angle, and especially a right angle. In this branch of mathematics we have many invented technical terms; as *complement*, *supplement*, *cosine*, *cotangent*, a *spherical angle*, the *pole of a circle*, or of a sphere. The word *sine* itself appears to belong to the class of terms already described as scientific appropriations of common terms, although its origin is somewhat obscure.

Mathematicians were naturally led to construct these and many other terms by the progress of their speculations. In like manner, when astronomy took the form of a speculative science, words were invented to denote distinctly the conceptions thus introduced. Thus the sun's annual path among the stars, in which not only solar, but also all lunar eclipses occur, was termed the *ecliptic.* The circle which the sun describes in his diurnal motion, when the days and nights are equal, the Greeks called the *equidiurnal* (ἰσημερινὸς,) the Latin astronomers the *equinoctial*, and the corresponding circle on the earth was the *equator.* The ecliptic intersected the equinoctial in the *equinoctial points.* The *solstices* (in Greek, τροπαὶ) were the times when the sun arrested his motion northwards or southwards; and the *solstitial points* (τὰ τροπίκα σημεῖα) were the places in the ecliptic where he then was. The name of *meridians* was given to circles passing through the poles of the equator; the *solstitial colure* (κόλουρος, curtailed), was one of these circles, which passes through the solstitial points, and is intercepted by the horizon.

We have borrowed from the Arabians various astronomical terms, as *Zenith*, *Nadir*, *Azimuth*, *Almacantar*. And these words, which among the Arabians probably belonged to the first class, of appropriated scientific terms, are for us examples of the second class, invented scientific terms; although they differ from most that we have mentioned, in not containing an etymology corresponding to their meaning in any language with which European cultivators of science are generally familiar. Indeed, the distinction of our two classes, though convenient, is in a great measure, casual. Thus most of the words we formerly mentioned, as *parallax*, *horizon*, *eclipse*, though appropriated technical terms among the Greeks, are to us invented technical terms.

In the construction of such terms as we are now considering, those languages have a great advantage which possess a power of forming words by composition. This was eminently the case with the Greek language; and hence most of the ancient terms of science in that language, when their origin is once explained, are clearly understood and easily retained. Of modern European languages, the German possesses the greatest facility of composition; and hence scientific authors in that language are able to invent terms which it is impossible to imitate in the other languages of Europe. Thus Weiss distinguishes his various systems of crystals as *zwei-und-zwei-gliedrig*, *ein-und-zwei-gliedrig*, *drey-und-drey-gliedrig*, *&c.*, (two-and-two-membered, one-and-two-membered, three-and-three-membered.) And Hessel, also a writer on crystallography, speaks of *doubly-one-membered edges*, *four-and-three spaced rays*, and the like.

How far the composition of words, in such cases, may be practised in the English language, and the general question, what are the best rules and artifices in such cases, I shall afterwards consider. In the mean time, I may observe that this list of invented technical terms might easily be much enlarged. Thus in harmonics we have the various intervals, as a *Fourth*, a *Fifth*, an *Octave*, (*Diatessaron*, *Diapente*, *Diapason*,) a *Comma*, which is the difference of a *Major* and *Minor Tone;* we have the various *Moods* or *Keys*, and the notes of various lengths, as *Minims*, *Breves*, *Semibreves*, *Quavers*. In chemistry, *Gas* was at first a technical term invented by Van Helmont, though it has now been almost adopted into common language. I omit many words which will perhaps suggest themselves to the reader, because they belong rather to the next class, which I now proceed to notice.

III. The third class of technical terms consists of such as are constructed by men of science, and involve some theoretical idea in the meaning which their derivation implies. They do not merely describe, like the class last spoken of, but describe with reference to some doctrine or hypothesis which is accepted as a portion of science. Thus *latitude* and *longitude*, according to their origin, signify breadth and length; they are used, however, to denote measures of the distance of a place on the earth's surface from the equator, and from the first meridian, of which distances, one cannot be called *length* more properly than the other. But this appropriation of these words may be explained by recollecting that the earth, as known to the ancient geographers, was much further extended from east to west than from north to south. The *Precession* of the equinoxes is a term which implies that the stars are fixed, while the point which is the origin of the measure of celestial longitude moves backward. The *Right Ascension* of a star is a measure of its position corresponding to terrestrial longitude; this quantity is identical with the angular ascent of the equinoctial point, when the star is in the horizon in a *right* sphere; that is, a sphere which supposes the spectator to be at the equator. The *Oblique Ascension* (a term now little used), is derived in like manner from an oblique sphere. The motion of a planet is *direct* or *retrograde*, *in consequentia* (*signa*), or *in antecedentia*, in reference to a certain assumed standard direction for celestial motions, namely, the direction opposite to that of the sun's daily motion, and agreeing with his annual motion among the stars; or with what is much more evident, the moon's monthly motion. The *equation of time* is the quantity which must be added to or subtracted from the time marked by the sun, in order to reduce it to a theoretical condition of equable progress. In like manner the *equation of the center* of the sun or of the moon is the angle which must be added to, or subtracted from, the actual advance of the luminary in the heavens, in order to make its motion equable. Besides the equation of the center of the moon, which represents the first and greatest of her deviations from equable motion, there are many other *equations*, by the application of which her motion is brought nearer and nearer to perfect uniformity. The second of these equations is called the *evection*, the third the *variation*, the fourth the *annual equation*. The motion of the sun as affected by its inequalities is called his *anomaly*, which term denotes inequality. In the History of Astronomy, we find that the inequable motions of the sun, moon, and planets were, in a great measure, reduced to rule and system by the Greeks, by the aid of an hypothesis of circles, revolving, and carrying in their motion other circles which also revolved. This hypothesis introduced many technical terms, as *deferent*, *epicycle*, *eccentric*. In like manner, the theories which have more recently taken the place of the theory of epicycles have introduced other technical terms, as the *elliptical orbit*, the *radius vector*, and the *equable description of areas* by this radius, which phrases express the true laws of the planetary motions.

There is no subject on which theoretical views have been so long and so extensively prevalent as astronomy, and therefore no other science in which there are so many technical terms of the kind we are now considering. But in other subjects also, so far as theories have been established, they have been accompanied by the introduction or fixation of technical terms. Thus, as we have seen in the examination of the foundations of mechanics, the terms *force* and *inertia* derive their precise meaning from a recognition of the first law of motion; *accelerating force* and *composition of motion* involve the second law; *moving force*, *momentum*, *action* and *reaction*, are expressions which imply the third law. The term *vis viva* was introduced to express a general property of moving bodies; and other terms have been introduced for like purposes, as *impetus* by Smeaton, and *work done*, by other engineers. In the recent writings of several French engineers, the term *travail* is much employed, to express the work done and the force which does it: this term has been rendered by *labouring force*. The proposition which was termed the *hydrostatic paradox* had this name in reference to its violating a supposed law of the action of forces. The verb to *gravitate*, and the abstract term *gravitation*, sealed the establishment of Newton's theory of the solar system.

In some of the sciences, opinions, either false, or disguised in very fantastical imagery, have prevailed; and the terms which have been introduced during the reign of such opinions, bear the impress of the time. Thus in the days of alchemy, the substances with which the operator dealt were personified; and a metal when exhibited pure and free from all admixture was considered as a little king, and was hence called a *regulus*, a term not yet quite obsolete. In like manner, a substance from which nothing more of any value could be extracted, was dead, and was called a *caput mortuum*. Quick silver, that is, live silver (*argentum vivum*), was killed by certain admixtures, and was *revived* when restored to its pure state.

We find a great number of medical terms which bear the mark of opinions formerly prevalent among physicians; and though these opinions hardly form a part of the progress of science, and were not presented in our History, we may notice some of these terms as examples of the mode in which words involve in their derivation obsolete opinions. Such words as *hysterics*, *hypochondriac*, *melancholy*, *cholera*, *colic*, *quinsey* (*squinantia*, συνάγχη, a suffocation), *megrim*, *migraine* (*hemicranium*, the middle of the skull), *rickets*, (*rachitis*, from ῥαχὶς, the backbone), *palsy*, (*paralysis*, παραλύσις,) *apoplexy* (ἀποπληξία, a stroke), *emrods*, (αἱμοῤῥοΐδες, *hemorrhoids*, a flux of blood), *imposthume*, (corrupted from *aposteme*, ἀπόστημα, an abscess), *phthisic* (φθίσις, consumption), *tympany* (τυμπανία, swelling), *dropsy* (*hydropsy*, ὕδρωψ,) *sciatica*, isciatica (ἰσχιαδικὴ, from ἰσχίον, the hip), *catarrh* (κατάῤῥους, a flowing down), *diarrhœa* (διαῤῥοία, a flowing through), *diabetes* (διαβήτης, a passing through), *dysentery* (δυσεντερία, a disorder of the entrails), *arthritic* pains (from ἄρθρα, the joints), are names derived from the supposed or real seat and circumstances of the diseases. The word from which the first of the above names is derived (ὑστερα, the last place,) signifies the womb, according to its order in a certain systematic enumeration of parts. The second word, *hypochondriac*, means something affecting the viscera below the cartilage of the breastbone, which cartilage is called χόνδρος; *melancholy* and *cholera* derive their names from supposed affections of χολὴ, the bile. *Colic* is that which affects the *colon* (κῶλον), the largest member of the bowels. A disorder of the eye is called *gutta serena* (the "drop serene" of Milton), in contradistinction to *gutta turbida*, in which the impediment to vision is perceptibly opake. Other terms also record the opinions of the ancient anatomists, as *duodenum*, a certain portion of the intestines, which they estimated as twelve inches long. We might add other allusions, as the *tendon of Achilles*.

Astrology also supplied a number of words founded upon fanciful opinions; but this study having been expelled from the list of sciences, such words now survive only so far as they have found a place in common language. Thus men were termed *mercurial*, *martial*, *jovial*, or *saturnine*, accordingly as their characters were supposed to be determined by the influence of the planets, Mercury, Mars, Jupiter, or Saturn. Other expressions, such as *disastrous*, *ill-starred*, *exorbitant*, *lord of the ascendant*, and hence *ascendancy*, *influence*, a *sphere of action*, and the like, may serve to show how extensively astrological opinions have affected language, though the doctrine is no longer a recognized science.

The preceding examples will make it manifest that opinions, even of a recondite and complex kind, are often implied in the derivation of words; and thus will show how scientific terms, framed by the cultivators of science, may involve received hypotheses and theories. When terms are thus constructed, they serve not only to convey with ease, but to preserve steadily and to diffuse widely, the opinions which they thus assume. Moreover, they enable the speculator to employ these complex conceptions, the creations of science, and the results of much labour and thought, as readily and familiarly as if they were convictions borrowed at once from the senses. They are thus powerful instruments in enabling philosophers to ascend from one step of induction and generalization to another; and hereby contribute powerfully to the advance of knowledge and truth.

It should be noticed, before we proceed, that the names of natural objects, when they come to be considered as the objects of a science, are selected according to the processes already enumerated. For the most part, the natural historian adopts the common names of animals, plants, mineral, gems, and the like, and only endeavours to secure their steady and consistent application. But many of these names imply some peculiar, often fanciful, belief respecting the object.

Various plants derive their names from their supposed virtues, as *herniaria*, *rupture-wort;* or from legends, as *herba Sancti Johannis*, *St. John's wort*. The same is the case with minerals: thus the *topaz* was asserted to come from an island so shrouded in mists that navigators could only *conjecture* (τοπάζειν) where it was. In these latter cases, however, the legend is often not the true origin of the name, but is suggested by it.

The privilege of constructing names where they are wanted, belongs to natural historians no less than to the cultivators of physical science; yet in the ancient world, writers of the former class appear rarely to have exercised this privilege, even when they felt the imperfections of the current language. Thus Aristotle repeatedly mentions classes of animals which have no name, as co-ordinate with classes that have names; but he hardly ventures to propose names which may supply these defects*. The vast importance of nomenclature in natural history was not recognized till the modern period.

We have, however, hitherto considered only the formation or appropriation of single terms in science; except so far as several terms may in some instances be connected by reference to a common theory. But when the value of technical terms began to be fully appreciated, philosophers proceeded to introduce them into their sciences more copiously and in a more systematic manner. In this way, the modern history of technical language has some features of a different aspect from the ancient; and must give rise to a separate Aphorism.

APHORISM II.

In the Modern Period of Science, besides the three processes anciently employed in the formation of technical terms, there have been introduced Systematic Nomen-

* In his *History of Animals*, (Book I. chap. vi.), he says, that the great classes of animals are Quadrupeds, Birds, Fishes, Whales (*Cetaceans*), Oysters (*Testaceans*), animals like crabs which have no general name (*Crustaceans*), soft animals (*Mollusks* and *Insects*). He does, however, call the Crustacea by a name (*Malacostraca*, soft-shelled) which has since been adopted by Naturalists.

*clature, Systematic Terminology, and the Systematic Modification of Terms to express theoretical relations**.

WRITERS upon science have gone on up to modern times forming such technical terms as they had occasion for, by the three processes above described;—namely, appropriating and limiting words in common use;—constructing for themselves words descriptive of the conception which they wished to convey;—or framing terms which by their signification imply the adoption of a theory. Thus among the terms introduced by the study of the connexion between magnetism and electricity, the word *pole* is an example of the first kind; the name of the subject, *electro-magnetism*, of the second; and the term *current*, involving an hypothesis of the motion of a fluid, is an instance of the third class. In chemistry, the term *salt* was adopted from common language, and its meaning extended to denote any compound of a certain kind; the term *neutral* salt implied the notion of a balanced opposition in the two elements of the compound; and such words as *subacid* and *superacid*, invented on purpose, were introduced to indicate the cases in which this balance was not attained. Again, when the phlogistic theory of chemistry was established, the term *phlogiston* was introduced to express the theory, and from this such terms as *phlogisticated* and *dephlogisticated* were derived, exclusively words of science. But in such instances as have just been given, we approach towards a systematic modification of terms, which is a peculiar process of modern times. Of this, modern chemistry forms a prominent example, which we shall soon consider, but we shall first notice the other processes mentioned in the Aphorism.

* On the subject of Terminology and Nomenclature, see also Aphorisms LXXXVIII. and XCVIII. concerning Ideas, and Book VIII. chap. ii. of the *Philosophy*.

I. In ancient times, no attempt was made to invent or select a Nomenclature of the objects of Natural History which should be precise and permanent. The omission of this step by the ancient naturalists gave rise to enormous difficulty and loss of time when the sciences resumed their activity. We have seen in the history of the sciences of classification, and of botany in especial*, that the early cultivators of that study in modern times endeavoured to identify all the plants described by Greek and Roman writers with those which grow in the north of Europe; and were involved in endless confusion†, by the multiplication of names of plants, at the same time superfluous and ambiguous. The *Synonymies* which botanists (Bauhin and others) found it necessary to publish, were the evidences of these inconveniences. In consequence of the defectiveness of the ancient botanical nomenclature, we are even yet uncertain with respect to the identification of some of the most common trees mentioned by classical writers‡. The ignorance of botanists respecting the importance of nomenclature operated in another manner to impede the progress of science. As a good nomenclature presupposes a good system of classification, so, on the other hand, a system of classification cannot become permanent without a corresponding nomenclature. Cæsalpinus, in the sixteenth century§, published an excellent system of arrangement for plants; but this, not being connected with any system of names, was never extensively accepted, and soon fell into oblivion. The business of framing a scientific botanical classification was in this way delayed for about a century. In the same manner, Willoughby's classification of fishes,

* *Hist. Ind. Sci.*, B. XVI. c. ii.

† For instance, whether the *fagus* of the Latins be the beech or the chestnut.

‡ *Hist. Ind. Sci.*, B. XVI. c. iii. sect. 3.

§ *Ibid.*, B. XVI. c. iii. sect. 2.

though, as Cuvier says, far better than any which preceded it, was never extensively adopted, in consequence of having no nomenclature connected with it.

II. Probably one main cause which so long retarded the work of fixing at the same time the arrangement and the names of plants, was the great number of minute and diversified particulars in the structure of each plant which such a process implied. The stalks, leaves, flowers, and fruits of vegetables, with their appendages, may vary in so many ways, that common language is quite insufficient to express clearly and precisely their resemblances and differences. Hence botany required not only a fixed system of *names* of plants, but also an artificial system of phrases fitted to *describe* their parts: not only a *Nomenclature*, but also a *Terminology*. The Terminology was, in fact, an instrument indispensably requisite in giving fixity to the Nomenclature. The recognition of the kinds of plants must depend upon the exact comparison of their resemblances and differences; and to become a part of permanent science, this comparison must be recorded in words.

The formation of an exact descriptive language for botany was thus the first step in that systematic construction of the technical language of science, which is one of the main features in the intellectual history of modern times. The ancient botanists, as Decandolle* says, did not make any attempt to select terms of which the sense was rigorously determined; and each of them employed in his descriptions the words, metaphors, or periphrases which his own genius suggested. In the History of Botany†, I have noticed some of the persons who contributed to this improvement. "Clusius," it is there stated, "first taught botanists to describe well.

* *Theor. Elem. de Bot.*, p. 327.
† *Hist. Ind. Sci.*, B XVI c. iii. sect. 3.

He introduced exactitude, precision, neatness, elegance, method: he says nothing superfluous; he omits nothing necessary." This task was further carried on by Jung and Ray*. In these authors we see the importance which began to be attached to the exact definition of descriptive terms; for example, Ray quotes Jung's definition of *Caulis*, a stalk.

The improvement of descriptive language, and the formation of schemes of classification of plants, went on gradually for some time, and was much advanced by Tournefort. But at last Linnæus embodied and followed out the convictions which had gradually been accumulating in the breasts of botanists; and by remodelling throughout both the terminology and the nomenclature of botany, produced one of the greatest reforms which ever took place in any science. He thus supplied a conspicuous example of such a reform, and a most admirable model of a language, from which other sciences may gather great instruction. I shall not here give any account of the terms and words introduced by Linnæus. They have been exemplified in the *History of Science*†; and the principles which they involve I shall consider separately hereafter. I will only remind the reader that the great simplification in *nomenclature* which was the result of his labours, consisted in designating each kind of plant by a *binary* term consisting of the name of the *genus* combined with that of the *species:* an artifice seemingly obvious, but more convenient in its results than could possibly have been anticipated.

Since Linnæus, the progress of Botanical Anatomy and of Descriptive Botany have led to the rejection of several inexact expressions, and to the adoption of several new terms, especially in describing the structure of the

* *Hist. Ind. Sci.*, B. XVI. c. iii. sect. 3. (about A. D. 1660).

† *Ib.*, c. iv. sect. 1—3.

fruit and the parts of cryptogamous plants. Hedwig, Medikus, Necker, Desvaux, Mirbel, and especially Gærtner, Link, and Richard, have proposed several useful innovations, in these as in other parts of the subject; but the general mass of the words now current consists still, and will probably continue to consist, of the terms established by the Swedish Botanist*.

When it was seen that botany derived so great advantages from a systematic improvement of its language, it was natural that other sciences, and especially classificatory sciences, should endeavour to follow its example. This attempt was made in Mineralogy by Werner, and afterwards further pursued by Mohs. Werner's innovations in the descriptive language of Mineralogy were the result of great acuteness, an intimate acquaintance with minerals, and a most methodical spirit: and were in most respects great improvements upon previous practices. Yet the introduction of them into Mineralogy was far from regenerating that science, as Botany had been regenerated by the Linnæan reform. It would seem that the perpetual scrupulous attention to most minute differences, (as of lustre, colour, fracture,) the greater part of which are not really important, fetters the mind, rather than disciplines it or arms it for generalization. Cuvier has remarked† that Werner, after his first *Essay on the Characters of Minerals*, wrote little; as if he had been afraid of using the system which he had created, and desirous of escaping from the chains which he had imposed upon others. And he justly adds, that Werner dwelt least, in his descriptions, upon that which is really the most important feature of all, the crystalline structure. This, which is truly a definite character, like those of Botany, does, when it can be clearly discerned, determine the place of the mineral in a system. This, there-

* De Candolle, *Th. Elem.*, p 307.

† *Eloges*, II. 314.

fore, is the character which, of all others, ought to be most carefully expressed by an appropriate language. This task, hardly begun by Werner, has since been fully executed by others, especially by Romé de l'Isle, Haüy, and Mohs. All the forms of crystals can be described in the most precise manner by the aid of the labours of these writers and their successors. But there is one circumstance well worthy our notice in these descriptions. It is found that the language in which they can best be conveyed is not that of words, but of *symbols*. The relations of space which are involved in the forms of crystalline bodies, though perfectly definite, are so complex and numerous, that they cannot be expressed, except in the language of mathematics: and thus we have an extensive and recondite branch of mathematical science, which is, in fact, only a part of the Terminology of the mineralogist.

The Terminology of Mineralogy being thus reformed, an attempt was made to improve its Nomenclature also, by following the example of Botany. Professor Mohs was the proposer of this innovation. The names framed by him were, however, not composed of two but of three elements, designating respectively the Species, the Genus, and the Order*: thus he has such species as *Rhombohedral Lime Haloide, Octahedral Fluor Haloide, Prismatic Hal Baryte*. These names have not been generally adopted; nor is it likely that any names constructed on such a scheme will find acceptance among mineralogists, till the higher divisions of the system are found to have some definite character. We see no real mineralogical significance in Mohs's Genera and Orders, and hence we do not expect them to retain a permanent place in the science.

The only systematic names which have hitherto been

* *Hist. Ind. Sci.*, B. xv. c. ix.

generally admitted in Mineralogy, are those expressing the chemical constitution of the substance; and these belong to a system of technical terms different from any we have yet spoken of, namely to terms formed by systematic modification.

III. The language of Chemistry was already, as we have seen, tending to assume a systematic character, even under the reign of the phlogiston theory. But when oxygen succeeded to the throne, it very fortunately happened that its supporters had the courage and the foresight to undertake a completely new and systematic recoinage of the terms belonging to the science. The new nomenclature was constructed upon a principle hitherto hardly applied in science, but eminently commodious and fertile; namely, the principle of indicating a modification of relations of elements, by a change in the termination of the word. Thus the new chemical school spoke of sulph*uric* and sulph*urous* acids; of sulph*ates* and sulph*ites* of bases; and of sulph*urets* of metals; and in like manner, of phos*phoric* and phos*phorous* acids, of phos*phates*, phos*phites*, phos*phurets*. In this manner a nomenclature was produced, in which the very name of a substance indicated at once its constitution and place in the system.

The introduction of this chemical language can never cease to be considered one of the most important steps ever made in the improvement of technical terms; and as a signal instance of the advantages which may result from artifices apparently trivial, if employed in a manner conformable to the laws of phenomena, and systematically pursued. It was, however, proved that this language, with all its merits, had some defects. The relations of elements in composition were discovered to be more numerous than the modes of expression which the terminations supplied. Besides the sulphurous and sulphuric acids, it appeared there were others; these were called the *hyposulphurous* and *hyposulphuric:* but these names, though convenient, no longer implied, by their form, any definite relation. The compounds of Nitrogen and Oxygen are, in order, the *Protoxide*, the *Deutoxide* or *Binoxide; Hyponitrous* Acid, *Nitrous* Acid, and *Nitric* Acid. The nomenclature here ceases to be systematic. We have three oxides of Iron, of which we may call the first the *Protoxide*, but we cannot call the others the *Deutoxide* and *Tritoxide*, for by doing so we should convey a perfectly erroneous notion of the proportions of the elements. They are called the *Protoxide*, the *Black* Oxide, and the *Peroxide*. We are here thrown back upon terms quite unconnected with the system.

Other defects in the nomenclature arose from errours in the theory; as for example the names of the muriatic, oxymuriatic, and hyperoxymuriatic acids; which, after the establishment of the new theory of chlorine, were changed to *hydrochloric* acid, *chlorine*, and *chloric* acid.

Thus the chemical system of nomenclature, founded upon the oxygen theory, while it shows how much may be effected by a good and consistent scheme of terms, framed according to the real relations of objects, proves also that such a scheme can hardly be permanent in its original form, but will almost inevitably become imperfect and anomalous, in consequence of the accumulation of new facts, and the introduction of new generalizations. Still, we may venture to say that such a scheme does not, on this account, become worthless; for it not only answers its purpose in the stage of scientific progress to which it belongs:—so far as it is not erroneous, or merely conventional, but really systematic and significant of truth, its terms can be translated at once into the language of any higher generalization which is afterwards arrived at. If terms express relations really ascertained to be true, they can never lose their value by any change of the received theory. They are like coins of pure metal, which, even when carried into a country which does not recognize the sovereign whose impress they bear, are still gladly received, and may, by the addition of an explanatory mark, continue part of the common currency of the country.

These two great instances of the reform of scientific language, in Botany and in Chemistry, are much the most important and instructive events of this kind which the history of science offers. It is not necessary to pursue our historical survey further. Our remaining Aphorisms respecting the Language of Science will be collected and illustrated indiscriminately, from the precepts and the examples of preceding philosophers of all periods.

We may, however, remark that Aphorisms III., IV., V., VI., VII., respect peculiarly the Formation of Technical Terms by the Appropriation of Common Words, while the remaining ones apply to the Formation of New Terms.

It does not appear possible to lay down a system of rules which may determine and regulate the construction of all technical terms, on all the occasions on which the progress of science makes them necessary or convenient. But if we can collect a few maxims such as have already offered themselves to the minds of philosophers, or such as may be justified by the instances by which we shall illustrate them, these maxims may avail to guide us in doubtful cases, and to prevent our aiming at advantages which are unattainable, or being disturbed by seeming imperfections which are really no evils. I shall therefore state such maxims of this kind as seem most sound and useful.

APHORISM III.

In framing scientific terms, the appropriation of old words is preferable to the invention of new ones.

THIS maxim is stated by Bacon in his usual striking manner. After mentioning *Metaphysic*, as one of the divisions of Natural Philosophy, he adds*: "Wherein I desire it may be conceived that I use the word *metaphysic* in a differing sense from that that is received: and in like manner I doubt not but it will easily appear to men of judgment that in this and other particulars, wheresoever my conception and notion may differ from the ancient, yet I am studious to keep the ancient terms. For, hoping well to deliver myself from mistaking by the order and perspicuous expressing of that I do propound; I am otherwise zealous and affectionate to recede as little from antiquity, either in terms or opinions, as may stand with truth, and the proficience of knowledge, . . . To me, that do desire, as much as lieth in my pen, to ground a sociable intercourse between antiquity and proficience, it seemeth best to keep a way with antiquity *usque ad aras;* and therefore to retain the ancient terms, though I sometimes alter the uses and definitions; according to the moderate proceeding in civil governments, when, although there be some alteration, yet that holdeth which Tacitus wisely noteth, *eadem magistratuum vocabula.*"

We have had before us a sufficient number of examples of scientific terms thus framed; for they formed the first of three classes which we described in the First Aphorism. And we may again remark, that science, when she thus adopts terms which are in common use, always limits and fixes their meaning in a technical manner. We may also repeat here the warning already given respecting terms of this kind, that they are peculi-

* *De Augm.*, Lib. III. c. iv.

arly liable to mislead readers who do not take care to understand them in their technical instead of their common signification. *Force, momentum, inertia, impetus, vis viva,* are terms which are very useful, if we rigorously bear in mind the import which belongs to each of them in the best treatises on Mechanics; but if the reader content himself with conjecturing their meaning from the context, his knowledge will be confused and worthless.

In the application of this Third Aphorism, other rules are to be attended to, which I add.

Aphorism IV.

When common words are appropriated as technical terms, their meaning and relations in common use should be retained as far as can conveniently be done.

I will state an example in which this rule seems to be applicable. Mr. Davies Gilbert* has recently proposed the term *efficiency* to designate the work which a machine, according to the force exerted upon it, is capable of doing; the work being measured by the weight raised, and the space through which it is raised, jointly. The usual term employed among engineers for the work which a machine actually does, measured in the way just stated, is *duty*. But as there appears to be a little incongruity in calling that work *efficiency* which the machine *ought* to do, when we call that work *duty* which it really does, I have proposed to term these two quantities *theoretical efficiency* and *practical efficiency*, or *theoretical duty* and *practical duty*†.

Since common words are often vague in their mean-

* *Phil. Trans.* 1827, p. 25.

† The term *travail* is used by French engineers, to express *efficiency* or *theoretical duty*. This term has been rendered in English by *labouring force*.

ing, I add as a necessary accompaniment to the Third Aphorism the following:—

APHORISM V.

When common words are appropriated as technical terms, their meaning may be modified, and must be rigorously fixed.

THIS is stated by Bacon in the above extract: "to retain the ancient terms, though I sometimes *alter the uses and definitions*." The scientific use of the term is in all cases much more precise than the common use. The loose notions of *velocity* and *force* for instance, which are sufficient for the usual purposes of language, require to be fixed by exact measures when these are made terms in the science of Mechanics.

This scientific fixation of the meaning of words is to be looked upon as a matter of convention, although it is in reality often an inevitable result of the progress of science. *Momentum* is conventionally defined to be the product of the numbers expressing the weight and the velocity; but then, it could be of no use in expressing the laws of motion if it were defined otherwise.

Hence it is no valid objection to a scientific term that the word in common language does not mean exactly the same as in its common use. It is no sufficient reason against the use of the term *acid* for a class of bodies, that all the substances belonging to this class are not sour. We have seen that a *trapezium* is used in geometry for any four-sided figure, though originally it meant a figure with two opposite sides parallel and the two others equal. A certain stratum which lies below the chalk is termed by English geologists *the green sand.* It has sometimes been objected to this denomination that the stratum has very frequently no tinge of green, and that it is often composed of lime with little or no sand. Yet the term is a good technical term in spite of these apparent improprieties; so long as it is carefully applied to that stratum which is geologically equivalent to the greenish sandy bed to which the appellation was originally applied.

When it appeared that *geometry* would have to be employed as much at least about the heavens as the earth, Plato exclaimed against the folly of calling the science by such a name; since the word signifies "earth-measuring;" yet the word *geometry* has retained its place and answered its purpose perfectly well up to the present day.

But though the meaning of the term may be modified or extended, it must be rigorously fixed when it is appropriated to science. This process is most abundantly exemplified by the terminology of Natural History, and especially of Botany, in which each term has a most precise meaning assigned to it. Thus Linnæus established exact distinctions between *fasciculus*, *capitulum*, *racemus*, *thyrsus*, *paniculus*, *spica*, *amentum*, *corymbus*, *umbella*, *cyma*, *verticillus*; or, in the language of English Botanists, *a tuft*, *a head*, *a cluster*, *a bunch*, *a panicle*, *a spike*, *a catkin*, *a corymb*, *an umbel*, *a cyme*, *a whorl*. And it has since been laid down as a rule*, that each organ ought to have a separate and appropriate name; so that the term *leaf*, for instance, shall never be applied to *a leaflet*, *a bractea*, or *a sepal* of the calyx.

Botanists have not been content with fixing the meaning of their terms by verbal definition, but have also illustrated them by figures, which address the eye. Of these, as excellent modern examples, may be mentioned those which occur in the works of Mirbel†, and Lindley‡.

* De Candolle, *Theor. El.*, 328.

† *Elémens de Botanique.*

‡ *Elements of Botany.*

APHORISM VI.

When common words are appropriated as technical terms, this must be done so that they are not ambiguous in their application.

AN example will explain this maxim. The conditions of a body, as a solid, a liquid, and an air, have been distinguished as different *forms* of the body. But the word *form*, as applied to bodies, has other meanings; so that if we were to inquire in *what form* water exists in a snow-cloud, it might be doubted whether the forms of crystallization were meant, or the different forms of ice, water, and vapour. Hence I have proposed* to reject the term *form* in such cases, and to speak of the different *consistence* of a body in these conditions. The term *consistence* is usually applied to conditions between solid and fluid; and may without effort be extended to those limiting conditions. And though it may appear more harsh to extend the term *consistence* to the state of air, it may be justified by what has been said in speaking of Aphorism V.

I may notice another example of the necessity of avoiding ambiguous words. A philosopher who makes method his study, would naturally be termed a *methodist;* but unluckily this word is already appropriated to a religious sect: and hence we could hardly venture to speak of Cæsalpinus, Ray, Morison, Rivinus, Tournefort, Linnæus, and their successors, as *botanical methodists*. Again, by this maxim, we are almost debarred from using the term *physician* for a cultivator of the science of physics, because it already signifies a practiser of physic. We might, perhaps, still use *physician* as the equivalent of the French *physicien,* in virtue of Aphorism V.; but probably it would be better to form a new word. Thus

* *Hist. Ind. Sci.*, B. x. c. ii. sect. 2.

we may say, that while the Naturalist employs principally the ideas of resemblance and life, the *Physicist* proceeds upon the ideas of force, matter, and the properties of matter.

Whatever may be thought of this proposal, the maxim which it implies is frequently useful. It is this.

APHORISM VII.

It is better to form new words as technical terms, than to employ old ones in which the last three Aphorisms cannot be complied with.

THE principal inconvenience attending the employment of new words constructed expressly for the use of science, is the difficulty of effectually introducing them. Readers will not readily take the trouble to learn the meaning of a word, in which the memory is not assisted by some obvious suggestion connected with the common use of language. When this difficulty is overcome, the new word is better than one merely appropriated; since it is more secure from vagueness and confusion. And in cases where the inconveniences belonging to a scientific use of common words become great and inevitable, a new word must be framed and introduced.

The Maxims which belong to the construction of such words will be stated hereafter; but I may notice an instance or two tending to show the necessity of the Maxim now before us.

The word *Force* has been appropriated in the science of Mechanics in two senses: as indicating the cause of motion; and again, as expressing certain measures of the effects of this cause, in the phrases *accelerating force* and *moving force*. Hence we might have occasion to speak of the accelerating or moving force *of* a certain *force;* for instance, if we were to say that the force which governs the motions of the planets resides in the sun; and that the accelerating force *of* this *force* varies only with the distance, but its moving force varies as the product of the mass of the sun and the planet. This is a harsh and incongruous mode of expression; and might have been avoided, if, instead of *accelerating force* and *moving force*, single abstract terms had been introduced by Newton: if, for instance, he had said that the velocity generated in a second measures the *accelerativity* of the force which produces it, and the momentum produced in a second measures the *motivity* of the force.

The science which treats of heat has hitherto had no special designation: treatises upon it have generally been termed treatises *On Heat*. But this practice of employing the same term to denote the property and the science which treats of it, is awkward, and often ambiguous. And it is further attended with this inconvenience, that we have no adjective derived from the name of the science, as we have in other cases, when we speak of *acoustical* experiments and *optical* theories. This inconvenience has led various persons to suggest names for the Science of Heat. M. Comte terms it *Thermology*. In the *History of the Sciences*, I have named it *Thermotics*, which appears to me to agree better with the analogy of the names of other corresponding sciences, *Acoustics* and *Optics*.

Electricity is in the same condition as Heat; having only one word to express the property and the science. M. Le Comte proposes *Electrology:* for the same reason as before, I should conceive *Electrics* more agreeable to analogy. The coincidence of the word with the plural of Electric would not give rise to ambiguity; for *Electrics*, taken as the name of a science, would be singular, like *Optics* and *Mechanics*. But a term offers itself to express *common* or *machine Electrics*, which appears worthy of admission, though involving a theoretical view. The received doctrine of the difference between Voltaic and Common Electricity is, that in the former case the fluid must be considered as in motion, in the latter as at rest. The science which treats of the former class of subjects is commonly termed *Electrodynamics*, which obviously suggests the name *Electrostatics* for the latter.

The subject of the Tides is, in like manner, destitute of any name which designates the science concerned about it. I have ventured to employ the term *Tidology*, having been much engaged in tidological researches.

Many persons possess a peculiarity of vision, which disables them from distinguishing certain colours. On examining many such cases, we find that in all such persons the peculiarities are the same; all of them confounding scarlet with green, and pink with blue. Hence they form a class, which, for the convenience of physiologists and others, ought to have a fixed designation. Instead of calling them, as has usually been done, "persons having a peculiarity of vision," we might take a Greek term implying this meaning, and term them *Idiopts*.

But my business at present is not to speak of the selection of new terms when they are introduced, but to illustrate the maxim that the necessity for their introduction often arises. The construction of new terms will be treated of subsequently.

Aphorism VIII.

Terms must be constructed and appropriated so as to be fitted to enunciate simply and clearly true general propositions.

This Aphorism may be considered as the fundamental principle and supreme rule of all scientific terminology. It is asserted by Cuvier, speaking of a particular case.

Thus he says* of Gmelin, that by placing the lamantin in the genus of morses, and the siren in the genus of eels, he had rendered every general proposition respecting the organization of those genera impossible.

The maxim is true of words appropriated as well as invented, and applies equally to the mathematical, chemical, and classificatory sciences. With regard to most of these, and especially the two former classes, it has been abundantly exemplified already, in what has previously been said, and in the *History of the Sciences*. For we have there had to notice many technical terms, with the occasions of their introduction; and all these occasions have involved the intention of expressing in a convenient manner some truth or supposed truth. The terms of Astronomy were adopted for the purpose of stating and reasoning upon the relations of the celestial motions, according to the doctrine of the sphere, and the other laws which were discovered by astronomers. The few technical terms which belong to Mechanics, *force*, *velocity*, *momentum*, *inertia*, &c., were employed from the first with a view to the expression of the laws of motion and of rest; and were, in the end, limited so as truly and simply to express those laws when they were fully ascertained. In Chemistry, the term *phlogiston* was useful, as has been shown in the *History*, in classing together processes which really are of the same nature; and the nomenclature of the *oxygen* theory was still preferable, because it enabled the chemist to express a still greater number of general truths.

To the connexion here asserted, of theory and nomenclature, we have the testimony of the author of the oxygen theory. In the Preface to his *Chemistry*, Lavoisier says:—"Thus while I thought myself employed only in forming a Nomenclature, and while I proposed to

* *Règne Animal*, Introd. viii.

myself nothing more than to improve the chemical language, my work transformed itself by degrees, without my being able to prevent it, into a Treatise on the Elements of Chemistry." And he then proceeds to show how this happened.

It is, however, mainly through the progress of Natural History in modern times, that philosophers have been led to see the importance and necessity of new terms in expressing new truths. Thus Harvey, in the Preface to his work on Generation, says:—"Be not offended if in setting out the History of the Egg I make use of a new method, and sometimes of unusual terms. For as they which find out a new plantation and new shores call them by names of their own coining, which posterity afterwards accepts and receives, so those that find out new secrets have good title to their compellation. And here, methinks, I hear Galen advising: If we consent in the things, contend not about the words."

The Nomenclature which answers the purposes of Natural History is a Systematic Nomenclature, and will be further considered under the next Aphorism. But we may remark, that the Aphorism now before us governs the use of words, not in science only, but in common language also. Are we to apply the name *fish* to animals of the whale kind? The answer is determined by our present rule: we are to do so, or not, accordingly as we can best express true propositions. If we are speaking of the internal structure and physiology of the animal, we must not call them *fish*; for in these respects they deviate widely from fishes: they have warm blood, and produce and suckle their young as land quadrupeds do. But this would not prevent our speaking of the *whale-fishery*, and calling such animals *fish* on all occasions connected with this employment; for the relations thus arising depend upon the animal's living in the water, and being caught in a manner similar to other fishes. A plea that human laws which mention fish do not apply to whales, would be rejected at once by an intelligent judge.

Aphorism IX.

In the Classificatory Sciences, a Systematic Nomenclature is necessary; and the System and the Nomenclature are each essential to the utility of the other.

The inconveniences arising from the want of a good Nomenclature were long felt in Botany, and are still felt in Mineralogy. The attempts to remedy them by *Synonymies* are very ineffective, for such comparisons of synonymes do not supply a systematic nomenclature; and such a one alone can enable us to state general truths respecting the objects of which the classificatory sciences treat. The *System* and the *Names* ought to be introduced together; for the former is a collection of asserted analogies and resemblances, for which the latter provide simple and permanent expressions. Hence it has repeatedly occurred in the progress of Natural History, that good Systems did not take root, or produce any lasting effect among naturalists, because they were not accompanied by a corresponding Nomenclature. In this way, as we have already noticed, the excellent botanical System of Cæsalpinus was without immediate effect upon the science. The work of Willoughby, as Cuvier says*, forms an epoch, and a happy epoch in Ichthyology; yet because Willoughby had no Nomenclature of his own, and no fixed names for his genera, his immediate influence was not great. Again, in speaking of Schlotheim's work containing representations of fossil vegetables, M. Adolphe Brongniart observes† that the figures and descriptions are so good, that if the author

* *Hist. des Poissons*, Pref. † *Prodrom. Veg. Foss.*, p. 3.

had established a nomenclature for the objects he describes, his work would have become the basis of all succeeding labours on the subject.

As additional examples of cases in which the improvement of classification, in recent times, has led philosophers to propose new names, I may mention the term *Pœcilite*, proposed by Mr. Conybeare to designate the group of strata which lies below the oolites and lias, including the new red or variegated sandstone, with the keuper above, and the magnesian limestone below it. Again, the transition districts of our island have recently been reduced to system by Professor Sedgwick and Mr. Murchison; and this step has been marked by the terms *Cambrian* system, and *Silurian* system, applied to the two great groups of formations which they have respectively examined, and by several other names of the subordinate members of these formations.

Thus System and Nomenclature are each essential to the other. Without Nomenclature, the system is not permanently incorporated into the general body of knowledge, and made an instrument of future progress. Without System, the names cannot express general truths, and contain no reason why they should be employed in preference to any other names.

This has been generally acknowledged by the most philosophical naturalists of modern times. Thus Linnæus begins that part of his Botanical Philosophy in which names are treated of, by stating that the foundation of botany is twofold, *Disposition* and *Denomination*; and he adds this Latin line,

Nomina si nescis perit et cognitio rerum.

And Cuvier, in the Preface to his *Animal Kingdom*, explains, in a very striking manner, how the attempt to connect zoology with anatomy led him, at the same time, to reform the classifications, and to correct the nomenclature of preceding zoologists.

I have stated that in Mineralogy we are still destitute of a good nomenclature generally current. From what has now been said, it will be seen that it may be very far from easy to supply this defect, since we have, as yet, no generally received system of mineralogical classification. Till we know what are really different species of minerals, and in what larger groups these species can be arranged, so as to have common properties, we shall never obtain a permanent mineralogical nomenclature. Thus *Leucocyclite* and *Tesselite* are minerals previously confounded with Apophyllite, which Sir John Herschel and Sir David Brewster distinguished by those names, in consequence of certain optical properties which they exhibit. But are these properties definite distinctions? and are there any external differences corresponding to them? If not, can we consider them as separate species? and if not separate species, ought they to have separate names? In like manner, we might ask if *Augite* and *Hornblende* are really the same species, as Gustavus Rose has maintained? if *Diallage* and *Hypersthene* are not definitely distinguished, which has been asserted by Kobell? Till such questions are settled, we cannot have a fixed nomenclature in mineralogy. What appears the best course to follow in the present state of the science, I shall consider when we come to speak of the form of technical terms.

I may, however, notice here that the main Forms of systematic nomenclature are two:—terms which are produced by combining words of higher and lower generality, as the binary names, consisting of the name of the genus and the species, generally employed by natural historians since the time of Linnæus;—and terms in which some relation of things is indicated by a change in the form of the word, for example, an alteration of its termination, of which kind of nomenclature we have a conspicuous example in the modern chemistry.

Aphorism X.

New terms and changes of terms, which are not needed in order to express truth, are to be avoided.

As the Seventh Aphorism asserted that novelties in language may be and ought to be introduced, when they aid the enunciation of truths, we now declare that they are not admissible in any other case. New terms and new systems of terms are not to be introduced, for example, in virtue of their own neatness or symmetry, or other merits, if there is no occasion for their use.

I may mention, as an old example of a superfluous attempt of this kind, an occurrence in the history of Astronomy. In 1628 John Bayer and Julius Schiller devised a *Cœlum Christianum*, in which the common names of the planets, &c., were replaced by those of Adam, Moses, and the Patriarchs. The twelve Signs became the twelve Apostles, and the constellations became sacred places and things. Peireskius, who had to pronounce upon the value of this proposal, praised the piety of the inventors, but did not approve, he said*, the design of perverting and confounding whatever of celestial information from the period of the earliest memory is found in books.

Nor are slight anomalies in the existing language of science sufficient ground for a change, if they do not seriously interfere with the expression of our knowledge. Thus Linnæus says† that a fair generic name is not to be exchanged for another though apter one: and‡ if we separate an old genus into several, we must try to find

* Gassendi, *Vita Peireskii*, 300. † *Phil. Bot.*, 246. ‡ *Ib.*, 247.

names for them among the synonyms which describe the old genus. This maxim excludes the restoration of ancient names long disused, no less than the needless invention of new ones. Linnæus lays down this rule*; and adds, that the botanists of the sixteenth century well nigh ruined botany by their anxiety to recover the ancient names of plants. In like manner Cuvier† laments it as a misfortune, that he has had to introduce many new names; and declares earnestly that he has taken great pains to preserve those of his predecessors.

The great bulk which the Synonymy of botany and of mineralogy have attained, shows us that this maxim has not been universally attended to. In these cases, however, the multiplication of different names for the same kind of object has arisen in general from ignorance of the identity of it under different circumstances, or from the want of a system which might assign to it its proper place. But there are other instances, in which the multiplication of names has arisen not from defect, but from excess, of the spirit of system. The love which speculative men bear towards symmetry and completeness is constantly at work, to make them create systems of classification more regular and more perfect than can be verified by the facts: and as good systems are closely connected with a good nomenclature, systems thus erroneous and superfluous lead to a nomenclature which is prejudicial to science. For although such a nomenclature is finally expelled, when it is found not to aid us in expressing the true laws of nature, it may obtain some temporary sway, during which, and even afterwards, it may be a source of much confusion.

We have a conspicuous example of such a result in the geological nomenclature of Werner and his school. Thus it was assumed, in Werner's system, that his *First,*

* *Phil. Bot.*, 248. † *Règne Anim.*, Pref. p. xvi.

Second, and *Third Flötz Limestone*, his *Old* and *New Red Sandstone*, were universal formations; and geologists looked upon it as their business to detect these strata in other countries. Names were thus assigned to the rocks of various parts of Europe, which created immense perplexity before they were again ejected. The geological terms which now prevail, for instance, those of Smith, are for the most part not systematic, but are borrowed from accidents, as localities, or popular names; as *Oxford Clay* and *Cornbrash;* and hence they are not liable to be thrust out on a change of system. On the other hand we do not find sufficient reason to accept the system of names of strata proposed by Mr. Conybeare in the *Introduction to the Geology of England and Wales*, according to which the *Carboniferous Rocks* are the *Medial Order*,—having above them the *Supermedial Order* (*New Red Sand, Oolites and Chalk*), and above these the *Superior Order* (*Tertiary Rocks*); and again, —having below, the *Submedial Order* (the *Transition Rocks*), and the *Inferior Order* (*Mica Slate, Gneiss, Granite*). For though these names have long been proposed, it does not appear that they are useful in enunciating geological truths. We may, it would seem, pronounce the same judgment respecting the system of geological names proposed by M. Alexander Brongniart, in his *Tableau des Terrains qui composent l'écorce du Globe*. He divides these strata into nine classes, which he terms *Terrains Alluviens, Lysiens, Pyrogenes, Clysmiens, Yzemiens, Hemilysiens, Agalysiens, Plutoniques, Vulcaniques*. These classes are again variously subdivided: thus the Terrains Yzemiens are *Thalassiques, Pelagiques*, and *Abyssiques;* and the Abyssiques are subdivided into *Lias, Keuper, Conchiliens, Pœciliens, Peneens, Rudimentaires, Entritiques, Houillers, Carbonifers* and *Gres Rouge Ancien*. Scarcely any amount of new truths would induce geologists to burthen themselves at once with this enormous system of new names: but in fact, it is evident that any portion of truth, which any author can have brought to light, may be conveyed by means of a much simpler apparatus. Such a nomenclature carries its condemnation on its own face.

Nearly the same may be said of the systematic nomenclature proposed for mineralogy by Professor Mohs. Even if all his Genera be really natural groups, (a doctrine which we can have no confidence in till they are confirmed by the evidence of chemistry,) there is no necessity to make so great a change in the received names of minerals. His proceeding in this respect, so different from the temperance of Linnæus and Cuvier, has probably ensured a speedy oblivion to this part of his system. In crystallography, on the other hand, in which Mohs's improvements have been very valuable, there are several terms introduced by him, as *rhombohedron*, *scalenohedron*, *hemihedral*, *systems* of crystallization, which will probably be a permanent portion of the language of science.

I may remark, in general, that the only persons who succeed in making great alterations in the language of science, are not those who make names arbitrarily and as an exercise of ingenuity, but those who have much new knowledge to communicate; so that the vehicle is commended to general reception by the value of what it contains. It is only eminent discoverers to whom the authority is conceded of introducing a new system of names; just as it is only the highest authority in the state which has the power of putting a new coinage in circulation.

I will here quote some judicious remarks of Mr. Howard, which fall partly under this Aphorism, and partly under some which follow. He had proposed, as names for the kinds of clouds, the following: *Cirrus, Cirrocumulus, Cirrostratus, Cumulostratus, Cumulus, Nimbus, Stratus.* In an abridgment of his views, given in the Supplement to the *Encyclopædia Britannica*, English names were proposed as the equivalents of these; *Curlcloud, Sondercloud, Wanecloud, Twaincloud, Stackencloud, Raincloud, Fallcloud.* Upon these Mr. Howard observes: "I mention these, in order to have the opportunity of saying that I do not adopt them. The names for the clouds which I deduced from the Latin, are but seven in number, and very easy to remember. They were intended as *arbitrary terms* for the *structure* of clouds, and the meaning of them was carefully fixed by a definition. The observer having once made himself master of this, was able to apply the term with correctness, after a little experience, to the subject under all its varieties of form, colour, or position. The new names, if meant to be another set of arbitrary terms, are superfluous; if intended to convey in themselves an explanation in English, they fail in this, by applying to some part or circumstance only of the definition; the *whole* of which must be kept in view to study the subject with success. To take for an example the first of the modifications. The term *cirrus* very readily takes an abstract meaning, equally applicable to the rectilinear as to the flexuous forms of the subject. But the name of *curl-cloud* will not, without some violence to its *obvious sense*, acquire this more extensive one: and will therefore be apt to mislead the reader rather than further his progress. Others of these names are as devoid of a meaning obvious to the English reader, as the Latin terms themselves. But the principal objection to English or any other local terms, remains to be stated. They take away from the nomenclature its general advantage of constituting, as far as it goes, an universal language, by means of which the intelligent of every country may convey to each other their ideas without the necessity of translation."

I here adduce these as examples of the arguments against changing an established nomenclature. As grounds of selecting a new one, they may be taken into account hereafter.

Aphorism XI.

Terms which imply theoretical views are admissible, as far as the theory is proved.

It is not unfrequently stated that the circumstances from which the names employed in science borrow their meaning, ought to be facts and not theories. But such a recommendation implies a belief that facts are rigorously distinguished from theories and directly opposed to them; which belief, we have repeatedly seen, is unfounded. When theories are firmly established, they become facts; and names founded on such theoretical views are unexceptionable. If we speak of the *minor axis* of Jupiter's *orbit*, or of his *density*, or of *the angle of refraction*, or *the length of an undulation* of red light, we assume certain theories; but inasmuch as the theories are now the inevitable interpretation of ascertained facts, we can have no better terms to designate the conceptions thus referred to. And hence the rule which we must follow is, not that our terms must involve no theory, but that they imply the theory only in that sense in which it is the interpretation of the facts.

For example, the term *polarization* of light was objected to, as involving a theory. Perhaps the term was at first suggested by conceiving light to consist of particles having poles turned in a particular manner. But among intelligent speculators, the notion of polarization soon reduced itself to the simple conception of opposite properties in opposite positions, which is a bare statement of the fact: and the term being understood to have this meaning, is a perfectly good term, and indeed the best which we can imagine for designating what is intended.

I need hardly add the caution, that names involving theoretical views not in accordance with facts are to be rejected. The following instances exemplify both the positive and the negative application of this maxim.

The distinction of *primary* and *secondary* rocks in geology was founded upon a theory; namely, that those which do not contain any organic remains were first deposited, and afterwards, those which contain plants and animals. But this theory was insecure from the first. The difficulty of making the separation which it implied, led to the introduction of a class of *transition* rocks. And the recent researches of geologists lead them to the conclusion, that those rocks which are termed *primary*, may be the newest, not the oldest, productions of nature.

In order to avoid this incongruity, other terms have been proposed as substitutes for these. Mr. Lyell remarks*, that granite, gneiss, and the like, form a class which should be designated by a common name; which name should not be of chronological import. He proposes *hypogene*, signifying "nether-formed;" and thus he adopts the theory that they have not assumed their present form and structure at the surface, but determines nothing of the period when they were produced.

These hypogene rocks, again, he divides into unstratified or *plutonic*, and altered, stratified, or *metamorphic*; the latter term implying the hypothesis that the stratified rocks to which it is applied have been altered, by the

* *Princ. Geol.*, IV. 386.

effect of fire or otherwise, since they were deposited. That fossiliferous strata, in some cases at least, have undergone such a change, is demonstrable from facts*.

The modern nomenclature of chemistry implies the oxygen theory of chemistry. Hence it has sometimes been objected to. Thus Davy, in speaking of the Lavoisierian nomenclature, makes the following remarks, which, however plausible they may sound, will be found to be utterly erroneous†. "Simplicity and precision ought to be the characteristics of a scientific nomenclature: words should signify *things*, or the *analogies* of things, and not *opinions*. . . . A substance in one age supposed to be simple, in another is proved to be compound, and *vice versâ*. A theoretical nomenclature is liable to continual alterations: *oxygenated muriatic acid* is as improper a term as *dephlogisticated marine acid*. Every school believes itself to be in the right: and if every school assumes to itself the liberty of altering the names of chemical substances in consequence of *new ideas* of their composition, there can be no permanency in the language of the science; it must always be confused and uncertain. Bodies which are *similar* to each other should always be classed together; and there is a presumption that their composition is *analogous*. *Metals*, *earths*, *alkalis*, are appropriate names for the bodies they represent, and independent of all speculation: whereas *oxides*, *sulphurets*, and *muriates* are terms founded upon opinions of the composition of bodies, some of which have been already found erroneous. The least dangerous mode of giving a systematic form to a language seems to be to signify the analogies of substances by some common sign affixed to the beginning or the termination of the word. Thus as the metals have been distinguished by a termination in *um*, as

* *Elem. Geol.*, p. 17. † *Elements of Chem. Phil.*, p. 46.

aurum, so their calciform or oxidated state might have been denoted by a termination in *a*, as *aura:* and no progress, however great, in the science could render it necessary that such a mode of appellation should be changed."

These remarks are founded upon distinctions which have no real existence. We cannot separate *things* from their *properties*, nor can we consider their properties and analogies in any other way than by having *opinions* about them. By contrasting *analogies* with *opinions*, it might appear as if the author maintained that there were certain analogies about which there was no room for erroneous opinions. Yet the analogies of chemical compounds, are, in fact, those points which have been most the subject of difference of opinion, and on which the revolutions of theories have most changed men's views. As an example of analogies which are still recognized under alterations of theory, the writer gives the relation of a metal to its oxide or calciform state. But this analogy of metallic oxides, as Red Copper or Iron Ore, to Calx, or burnt lime, is very far from being self-evident;—so far indeed, that the recognition of the analogy was a great step in chemical *theory*. The terms which he quotes, *oxygenated muriatic acid* (and the same may be said of *dephlogisticated marine acid*,) if improper, are so not because they involve theory, but because they involve false theory;—not because those who framed them did not endeavour to express analogies, but because they expressed analogies about which they were mistaken. Unconnected names, as *metals*, *earths*, *alkalis*, are good as the *basis* of a systematic nomenclature, but they are not substitutes for such a nomenclature. A systematic nomenclature is an instrument of great utility and power, as the modern history of chemistry has shown. It would be highly unphilosophical to reject the use of such an instrument, because, in the course of the revolutions of science, we may have to modify, or even to remodel it altogether. Its utility is not by that means destroyed. It has retained, transmitted, and enabled us to reason upon, the doctrines of the earlier theory, so far as they are true; and when this theory is absorbed into a more comprehensive one, (for this, and not its refutation, is the end of a theory so far as it is true,) the nomenclature is easily translated into that which the new theory introduces. We have seen, in the history of astronomy, how valuable the theory of *epicycles* was, in its time: the nomenclature of the relations of a planet's orbit, which that theory introduced, was one of Kepler's resources in discovering the *elliptical* theory; and, though now superseded, is still readily intelligible to astronomers.

This is not the place to discuss the reasons for the *form* of scientific terms; otherwise we might ask, in reference to the objections to the Lavoisierian nomenclature, if such forms as *aurum* and *aura* are good to represent the absence or presence of oxygen, why such forms as *sulphite* and *sulphate* are not equally good to represent the presence of what we may call a smaller or larger dose of oxygen, so long as the oxygen theory is admitted in its present form; and to indicate still the difference of the same substances, if under any change of theory it should come to be interpreted in a new manner.

But I do not now dwell upon such arguments, my object in this place being to show that terms involving theory are not only allowable, if understood so far as the theory is proved, but of great value, and indeed of indispensable use, in science. The objection to them is inconsistent with the objects of science. If, after all that has been done in chemistry or any other science, we have arrived at no solid knowledge, no permanent truth;—if all that we believe now may be proved to be false to-morrow;—then indeed our opinions and theories are corruptible elements, on which it would be unwise to rest any thing important, and which we might wish to exclude, even from our names. But if our knowledge has no more security than this, we can find no reason why we should wish to have names of things, since the names are needed mainly that we may reason upon and increase our knowledge such as it is. If we are condemned to endless alternations of varying opinions, then, no doubt, our theoretical terms may be a source of confusion; but then, where would be the advantage of their being otherwise? what would be the value of words which should express in a more precise manner opinions equally fleeting? It will perhaps be said, our terms must express facts, not theories: but of this distinction so applied we have repeatedly shown the futility. Theories firmly established are facts. Is it not a fact that the rusting of iron arises from the metal combining with the oxygen of the atmosphere? Is it not a fact that a combination of oxygen and hydrogen produces water? That our terms should express *such* facts, is precisely what we are here inculcating.

Our examination of the history of science has led us to a view very different from that which represents it as consisting in the succession of hostile opinions. It is, on the contrary, a progress, in which each step is recognized and employed in the succeeding one. Every theory, so far as it is true, (and all that have prevailed extensively and long, contain a large portion of truth,) is taken up into the theory which succeeds and seems to expel it. All the narrower inductions of the first are included in the more comprehensive generalizations of the second. And this is performed mainly by means of such terms as we are now considering;—terms involving the previous theory. It is by means of such terms, that the truths at first ascertained become so familiar and manageable, that they can be employed as elementary facts in the formation of higher inductions.

These principles must be applied also, though with great caution, and in a temperate manner, even to descriptive language. Thus the mode of describing the forms of crystals adopted by Werner and Romé de l'Isle was to consider an original form, from which other forms are derived by *truncations* of the edges and the angles. Haüy's method of describing the same forms, was to consider them as built up of rows of small solids, the angles being determined by the *decrements* of these rows. Both these methods of description involve hypothetical views; and the last was intended to rest on a true physical theory of the constitution of crystals. Both hypotheses are doubtful or false: yet both these methods are good as modes of description: nor is Haüy's terminology vitiated, if we suppose (as in fact we must suppose in many instances,) that crystalline bodies are not really made up of such small solids. The mode of describing an octahedron of fluor spar, as derived from the cube, by decrements of one row on all the edges, would still be proper and useful as a description, whatever judgment we should form of the material structure of the body. But then, we must consider the solids which are thus introduced into the description as merely hypothetical geometrical forms, serving to determine the angles of the faces. It is in this way alone that Haüy's nomenclature can now be retained.

In like manner we may admit theoretical views into the descriptive phraseology of other parts of Natural History: and the theoretical terms will replace the obvious images, in proportion as the theory is generally accepted and familiarly applied. For example, in speaking of the Honeysuckle, we may say that the upper leaves are *perfoliate*, meaning that a single orbicular leaf is perforated by the stalk, or threaded upon it. Here is an image which sufficiently conveys the notion of the form. But it is now generally recognized that this apparent single leaf is, in fact, two opposite leaves joined together at their bases. If this were doubted, it may be proved by comparing the upper leaves with the lower, which are really separate and opposite. Hence the term *connate* is applied to these conjoined opposite leaves, implying that they grow together; or they are called *connato-perfoliate*. Again; formerly the corolla was called *monopetalous* or *polypetalous*, as it consisted of one part or of several: but it is now agreed among botanists that those corollas which appear to consist of a single part, are, in fact, composed of several soldered together; hence the term *gamopetalous* is now employed (by Decandolle and his followers) instead of monopetalous*.

In this way the language of Natural History not only expresses, but inevitably implies, general laws of nature; and words are thus fitted to aid the progress of knowledge in this, as in other provinces of science.

APHORISM XII.

If terms are systematically good, they are not to be rejected because they are etymologically inaccurate.

TERMS belonging to a system are defined, not by the meaning of their radical words, but by their place in the system. That they should be appropriate in their signi-

* On this subject, see Illiger, *Versuch einer Systematischen Vollständigen Terminologie für das Thierreich und Pflanzenreich.* (1810.) De Candolle, *Théorie Elémentaire de la Botanique.*

fication, aids the processes of introducing and remembering them, and should therefore be carefully attended to by those who invent and establish them; but this once done, no objections founded upon their etymological import are of any material weight. We find no inconvenience in the circumstance that *geometry* means the measuring of the earth, that the name *porphyry* is applied to many rocks which have no fiery spots, as the word implies, and *oolite* to strata which have no roelike structure. In like manner, if the term *pœcilite* were already generally received, as the name of a certain group of strata, it would be no valid ground for quarreling with it, that this group was not always variegated in colour, or that other groups were equally variegated: although undoubtedly in *introducing* such a term, care should be taken to make it as distinctive as possible. It often happens, as we have seen, that by the natural progress of changes in language, a word is steadily confirmed in a sense quite different from its etymological import. But though we may accept such instances, we must not wantonly attempt to imitate them. I say, not wantonly: for if the progress of scientific identification compel us to follow any class of objects into circumstances where the derivation of the term is inapplicable, we may still consider the term as an unmeaning sound, or rather an historical symbol, expressing a certain member of our system. Thus if, in following the course of the *mountain* or *carboniferous* limestone, we find that in Ireland it does not form mountains nor contain coal, we should act unwisely in breaking down the nomenclature in which our systematic relations are already expressed, in order to gain, in a particular case, a propriety of language which has no scientific value.

All attempts to act upon the maxim opposite to this, and to make our scientific names properly descriptive of the objects, have failed and must fail. For the marks which really distinguish the natural classes of objects, are by no means obvious. The discovery of them is one of the most important steps in science; and when they are discovered, they are constantly liable to exceptions, because they do not contain the essential differences of the classes. The natural order *Umbellatæ*, in order to be a natural order, must contain some plants which have not umbels, as *Eryngium* *. "In such cases," said Linnæus, "it is of small import what you *call* the order, if you take a proper series of plants, and give it some name which is clearly understood to apply to the plants you have associated." "I have," he adds, "followed the rule of borrowing the name *à fortiori*, from the principal feature."

The distinction of crystals into systems according to the degree of symmetry which obtains in them, has been explained elsewhere. Two of these systems, of which the relation as to symmetry might be expressed by saying that one is *square pyramidal* and the other *oblong pyramidal*, or the first *square prismatic* and the second *oblong prismatic*, are termed by Mohs, the first, *Pyramidal*, and the second *Prismatic*. And it may be doubted whether it is worth while to invent other terms, though these are thus defective in characteristic significance. As an example of a needless rejection of old terms in virtue of a supposed impropriety in their meaning, I may mention the attempt made in the last edition of Haüy's *Mineralogy*, to substitute *autopside* and *heteropside* for *metallic* and *unmetallic*. It was supposed to be proved that all bodies have a metal for their basis; and hence it was wished to avoid the term *unmetallic*. But the words *metallic* and *unmetallic* may mean that minerals *seem* metallic and unmetallic, just as well as

* See *Hist. Ind. Sci.*, B. XVI. c. iv. sect. 5.

if they contained the element *opside* to imply this seeming. The old names express all that the new express, and with more simplicity, and therefore should not be disturbed.

The maxim on which we are now insisting, that we are not to be too scrupulous about the etymology of scientific terms, may, at first sight, appear to be at variance with our Fourth Aphorism, that words used technically are to retain their common meaning as far as possible. But it must be recollected, that in the Fourth Aphorism we spoke of *common* words *appropriated* as technical terms; we here speak of words *constructed* for scientific purposes. And although it is, perhaps, impossible to draw a broad line between these two classes of terms, still the rule of propriety may be stated thus: In technical terms, deviations from the usual meaning of words are bad in proportion as the words are more familiar in our own language. Thus we may apply the term *Cirrus* to a cloud composed of filaments, even if these filaments are straight; but to call such a cloud a *Curl cloud* would be much more harsh.

Since the names of things, and of classes of things, when constructed so as to involve a description, are constantly liable to become bad, the natural classes shifting away from the descriptive marks thus prematurely and casually adopted, I venture to lay down the following maxim.

Aphorism XIII.

The fundamental terms of a system of Nomenclature may be conveniently borrowed from casual or arbitrary circumstances.*

* I may refer back to Book viii., chap. ii., sect. 6, for some further remarks on Nomenclature. It will be seen, that besides the maxims of botanical writers concerning names, to which reference is there made

For instance, the names of plants, of minerals, and of geological strata, may be taken from the places where they occur conspicuously or in a distinct form; as *Parietaria, Parnassia, Chalcedony, Arragonite, Silurian* system, *Purbeck* limestone. These names may be considered as at first supplying standards of reference; for in order to ascertain whether any rock be *Purbeck* limestone, we might compare it with the rocks in the Isle of Purbeck. But this reference to a local standard is of authority only till the place of the object in the system, and its distinctive marks, are ascertained. It would not

some others are suggested by the considerations there offered;—especially these two:—

APHORISM XIII. (*a*).

The Binary method of Nomenclature (names by genus and species) is the most convenient hitherto employed in Classification.

APHORISM XIII. (*b*).

Numerical names in Classification are bad. For, besides that such names offer nothing for the memory to take hold of, new discoveries will probably alter the numeration, and make the names erroneous. Thus, if we call the species of a genus 1, 2, 3, &c., a new species intermediate between 1 and 2, 2 and 3, &c., cannot be put in its place without deranging the numbers.

The geological term *Trias*, lately introduced to designate the group consisting of the *three* members (Bunter Sandstein, Muschelkalk, and Keuper) becomes improper if, as some geologists hold, two of these members cannot be separated.

In like manner the names assigned by Mr. Rickman to the successive styles of Gothic architecture in England,—*Early English, Decorated*, and *Perpendicular*,—cannot be replaced by numerical designations, *First Pointed, Second Pointed, Third Pointed.* For—besides that he who first distinctly establishes classes has the right of naming them, and that Mr. Rickman's names are really appropriate and significant—these new names would confound all meaning of language. We should not be able to divide Early English, or Decorated, or Perpendicular into sub-styles;—for who could talk of *First Second Pointed* and *Second Second Pointed;* and what should we call *that* pointed style—the *Transition* from the Norman—which precedes the *First Pointed?*

vitiate the above names, if it were found that the *Parnassia* does not grow on Parnassus; that *Chalcedony* is not found in Chalcedon; or even that *Arragonite* no longer occurs in Arragon; for it is now firmly established as a mineral species. Even in geology such a reference is arbitrary, and may be superseded, or at least modified, by a more systematic determination. *Alpine* limestone is no longer accepted as a satisfactory designation of a rock, now that we know the limestone of the Alps to be of various ages.

Again, names of persons, either casually connected with the object, or arbitrarily applied to it, may be employed as designations. This has been done most copiously in botany, as for example, *Nicotiana*, *Dahlia*, *Fuchsia*, *Jungermannia*, *Lonicera*. And Linnæus has laid down rules for restricting this mode of perpetuating the memory of men, in the names of plants. Those generic names, he says*, which have been constructed to preserve the memory of persons who have deserved well of botany, are to be religiously retained. This, he adds, is the sole and supreme reward of the botanist's labours, and must be carefully guarded and scrupulously bestowed, as an encouragement and an honour. Still more arbitrary are the terms borrowed from the names of the gods and goddesses, heroes and heroines of antiquity, to designate new genera in those departments of natural history in which so many have been discovered in recent times as to weary out all attempts at descriptive nomenclature. Cuvier has countenanced this method. "I have had to frame many new names of genera and sub-genera," he says†, "for the sub-genera which I have established were so numerous and various, that the memory is not satisfied with numerical indications. These I have chosen either so as to indicate some charac-

* *Phil. Bot.*, 241. † *Règne An.*, p. 16.

ter, or among the usual denominations, which I have latinized, or finally, after the example of Linnæus, among the names of mythology, which are in general agreeable to the ear, and which are far from being exhausted."

This mode of framing names from the names of persons to whom it was intended to do honour, has been employed also in the mathematical and chemical sciences; but such names have rarely obtained any permanence, except when they recorded an inventor or discoverer. Some of the constellations, indeed, have retained such appellations, as *Berenice's Hair;* and the new star which shone out in the time of Cæsar, would probably have retained the name given to it, of the *Julian Star*, if it had not disappeared again soon after. In the map of the Moon, almost all the parts have had such names imposed upon them by those who have constructed such maps, and these names have very properly been retained. But the names of new planets and satellites thus suggested have not been generally accepted; as the *Medicean* stars, the name employed by Galileo for the satellites of Jupiter, the *Georgium Sidus*, the appellation proposed by Herschel for Uranus when first discovered*; Ceres *Ferdinandea*, the name which Piazzi wished to impose

* In this case, the name *Uranus*, selected with a view to symmetry according to the mythological order of descent of the persons (*Uranus*, *Saturn*, *Jupiter*, *Mars*) was adopted by astronomers in general, though not proposed or sanctioned by the discoverer of the new planet. In the cases of the smaller planets, *Ceres*, *Pallas*, *Juno*, and *Vesta*, the names were given either by the discoverer, or with his sanction. Following this rule, Bessel gave the name of *Astræa* to a new planet discovered in the same region by Mr. Hencke, as mentioned in Note (N) to Book VII. of the *History* (2nd Ed.) Following the same rule, and adhering as much as possible to mythological connexion, the astronomers of Europe have, with the sanction of M. Le Verrier, given the name of *Neptune* to the planet revolving beyond Uranus, and discovered in consequence of his announcement of its probable existence, which had been inferred by

on the small planet Ceres. The names given to astronomical Tables by the astronomers who constructed them have been most steadily adhered to, being indeed names of books, and not of natural objects. Thus there were the *Ilchanic*, the *Alphonsine*, the *Rudolphine*, the *Carolinian* Tables. Comets which have been ascertained to be periodical, have very properly had assigned to them the name of the person who established this point; and of these we have thus, *Halley's*, *Encke's Comet*, and *Biela's* or *Gambart's Comet*.

In the case of discoveries in science or inventions of apparatus, the name of the inventor is very properly employed as the designation. Thus we have the *Torricellian* Vacuum, the *Voltaic* Pile, *Fahrenheit's* Thermometer. And in the same manner with regard to laws of nature, we have *Kepler's* Laws, *Boyle* or *Mariotte's* law of the elasticity of air, *Huyghens's* law of double refraction, *Newton's* scale of colours. *Descartes'* law of refraction is an unjust appellation; for the discovery of the law of sines was made by Snell. In deductive mathematics, where the invention of a theorem is generally a more definite step than an induction, this mode of designation is more common, as *Demoivre's* Theorem, *Maclaurin's* Theorem, *Lagrange's* Theorem, *Eulerian* Integrals.

In the *History of Science** I have remarked that in the discovery of what is termed galvanism, Volta's office was of a higher and more philosophical kind than that of Galvani; and I have, on this account, urged the propriety of employing the term *voltaic*, rather than *galvanic* electricity. I may add that the electricity of the common machine is often placed in contrast with this,

by Mr. Adams and him (calculating in ignorance of each other's purpose) from the perturbations of Uranus; as I have stated in the Preface to the Second Edition of the *History*.

* B. XIII. c. 1.

and appears to require an express name. Mr. Faraday calls it *common* or *machine* electricity; but I think that *franklinic* electricity would form a more natural correspondence with *voltaic*, and would be well justified by Franklin's place in the history of that part of the subject.

APHORISM XIV.

In forming a Terminology, words may be invented when necessary, but they cannot be conveniently borrowed from casual or arbitrary circumstances.*

It will be recollected that Terminology is a language employed for describing objects, Nomenclature, a body of names of the objects themselves. The *names*, as was stated in the last maxim, may be arbitrary; but the *descriptive terms* must be borrowed from words of suitable meaning in the modern or the classical languages. Thus the whole terminology which Linnæus introduced into botany, is founded upon the received use of Latin words, although he defined their meaning so as to make it precise when it was not so, according to Aphorism V. But many of the terms were invented by him and other botanists, as *Perianth*, *Nectary*, *Pericarp;* so many, indeed, as to form, along with the others, a considerable language. Many of the terms which are now become familiar were originally invented by writers on botany. Thus the word *Petal*, for one division of the corolla, was introduced by Fabius Columna. The term *Sepal* was devised by Neckar to express each of the divisions of the calyx. And up to the most recent times, new deno-

* I may also refer to B. VIII. c. ii. sect. 2, for some remarks on Terminology. The following Aphorism contains one of the most important maxims:—

APHORISM XIV. (*a*).

The meaning of Technical Terms must be fixed by convention, not by casual reference to the ordinary meaning of the words.

minations of parts and conditions of parts have been devised by botanists, when they found them necessary. in order to mark important differences or resemblances. Thus the general *Receptacle* of the flower, as it is termed by Linnæus, or *Torus*, by Salisbury, is continued into organs which carry the stamina and pistil, or the pistil alone, or the whole flower; this organ has hence been termed* *Gonophore*, *Carpophore*, and *Anthophore*, in these cases.

In like manner when Cuvier had ascertained that the lower jaws of Saurians consisted always of six pieces having definite relations of form and position, he gave names to them, and termed them respectively the *Dental*, the *Angular*, the *Coronoid*, the *Articular*, the *Complementary*, and the *Opercular* Bones.

In all these cases, the descriptive terms thus introduced have been significant in their derivation. An attempt to circulate a perfectly arbitrary word as a means of description would probably be unsuccessful. We have, indeed, some examples approaching to arbitrary designations, in the Wernerian names of colours, which are a part of the terminology of Natural History. Many of these names are borrowed from natural resemblances, as *Auricula purple*, *Apple green*, *Straw yellow*; but the names of others are taken from casual occurrences, mostly, however, such as were already recognized in common language, as *Prussian blue*, *Dutch orange*, *King's yellow*.

The extension of arbitrary names in scientific terminology is by no means to be encouraged. I may mention a case in which it was very properly avoided. When Mr. Faraday's researches on Voltaic electricity had led him to perceive the great impropriety of the term *poles*, as applied to the apparatus, since the processes have not

* De Candolle's *Th. El.*, 405.

reference to any opposed points, but to two opposite directions of a path, he very suitably wished to substitute for the phrases *positive pole* and *negative pole*, two words ending in *ode*, from ὅδος, a way. A person who did not see the value of our present maxim, that descriptive terms should be descriptive in their origin, might have proposed words perfectly arbitrary, as *Alphode*, and *Betode:* or, if he wished to pay a tribute of respect to the discoverers in this department of science, *Galvanode* and *Voltaode.* But such words would very justly have been rejected by Mr. Faraday, and would hardly have obtained any general currency among men of science. *Zincode* and *Platinode*, terms derived from the metal which, in one modification of the apparatus, forms what was previously termed the pole, are to be avoided, because in their origin too much is casual; and they are not a good basis for derivative terms. The pole at which the zinc is, is the Anode or Cathode, according as it is associated with different metals. Either the *Zincode* must sometimes mean the pole at which the Zinc is, and at other times that at which the Zinc is not, or else we must have as many names for poles as there are metals. *Anode* and *Cathode*, the terms which Mr. Faraday adopted, were free from these objections; for they refer to a natural standard of the direction of the voltaic current, in a manner which, though perhaps not obvious at first sight, is easily understood and retained. *An*ode and *Cath*ode, the *rising* and the *setting* way, are the directions which correspond to east and west in that voltaic current to which we must ascribe terrestrial magnetism. And with these words it was easy to connect *Anïon* and *Cathïon*, to designate the opposite elements which are separated and liberated at the two *Electrodes.*

The following Aphorisms respect the Form of Technical Terms.

By the *Form* of Terms, I mean their philological conditions; as, for example, from what languages they may be borrowed, by what modes of inflexion they must be compounded, how their derivatives are to be formed, and the like. In this, as in other parts of the subject, I shall not lay down a system of rules, but shall propose a few maxims.

Aphorism XV.

The two main conditions of the Form of technical terms are, that they must be generally intelligible, and susceptible of such grammatical relations as their scientific use requires.

These conditions may at first appear somewhat vague, but it will be found that they are as definite as we could make them, without injuriously restricting ourselves. It will appear, moreover, that they have an important bearing upon most of the questions respecting the form of the words which come before us; and that if we can succeed in any case in reconciling the two conditions, we obtain terms which are practically good, whatever objections may be urged against them from other considerations.

1. The former condition, for instance, bears upon the question whether scientific terms are to be taken from the learned languages, Greek and Latin, or from our own. And the latter condition very materially affects the same question, since in English we have scarcely any power of inflecting our words; and therefore must have recourse to Greek or Latin in order to obtain terms which admit of grammatical modification. If we were content with the term *Heat* to express the science of heat, still it would be a bad technical term, for we cannot derive from it an adjective like *thermotical*. If *bed* or *layer* were an equally good term with

stratum, we must still retain the latter, in order that we may use the derivative *Stratification*, for which the English words cannot produce an equivalent substitute. We may retain the words *lime* and *flint*, but their adjectives for scientific purposes are not *limy* and *flinty*, but *calcareous* and *siliceous;* and hence we are able to form a compound, as *calcareo-siliceous*, which we could not do with indigenous words. We might fix the phrases *bent back* and *broken* to mean (of optical rays) that they are *reflected* and *refracted;* but then we should have no means of speaking of the angles of *Reflection* and *Refraction*, of the *Refractive* Indices, and the like.

In like manner, so long as anatomists described certain parts of a vertebra as *vertebral laminæ*, or *vertebral plates*, they had no adjective whereby to signify the properties of these parts; the term *Neurapophysis*, given to them by Mr. Owen, supplies the corresponding expression *neurapophysial*. So again, the term *Basisphenoid*, employed by the same anatomist, is better than *basilar* or *basial process of the sphenoid*, because it gives us the adjective *basisphenoidal*. And the like remark applies to other changes recently proposed in the names of portions of the skeleton.

Thus one of the advantages of going to the Greek and Latin languages for the origin of our scientific terms is, that in this way we obtain words which admit of the formation of adjectives and abstract terms, of composition, and of other inflexions. Another advantage of such an origin is, that such terms, if well selected, are readily understood over the whole lettered world. For this reason, the descriptive language of science, of botany for instance, has been, for the most part, taken from the Latin; many of the terms of the mathematical and chemical sciences have been derived from the Greek; and when occasion occurs to construct a new term, it is generally to that language that recourse is had. The advantage of such terms is, as has already been intimated, that they constitute an universal language, by means of which cultivated persons in every country may convey to each other their ideas without the need of translation.

On the other hand, the advantage of indigenous terms is, that so far as the language extends, they are intelligible much more clearly and vividly than those borrowed from any other source, as well as more easily manageable in the construction of sentences. In the descriptive language of botany, for example, in an English work, the terms *drooping*, *nodding*, *one-sided*, *twining*, *straggling*, appear better than *cernuous*, *nutant*, *secund*, *volubile*, *divaricate*. For though the latter terms may by habit become as intelligible as the former, they cannot become more so to any readers; and to most English readers they will give a far less distinct impression.

2. Since the advantage of indigenous over learned terms, or the contrary, depends upon the balance of the capacity of inflexion and composition on the one hand, against a ready and clear significance on the other, it is evident that the employment of scientific terms of the one class or of the other may very properly be extremely different in different languages. The German possesses in a very eminent degree that power of composition and derivation, which in English can hardly be exercised at all, in a formal manner. Hence German scientific writers use native terms to a far greater extent than do our own authors. The descriptive terminology of botany, and even the systematic nomenclature of chemistry, are represented by the Germans by means of German roots and inflexions. Thus the description of *Potentilla anserina*, in English botanists, is that it has *Leaves interruptedly pinnate*, *serrate*, *silky*, *stem creeping*, *stalks axillar*, *one-*

flowered. Here we have words of Saxon and Latin origin mingled pretty equally. But the German description is entirely Teutonic. *Die Blume in Achsel; die Blätter unterbrochen gefiedert, die Blättchen scharf gesagt, die Stämme kriechend, die Bluthenstiele einblumig.* We could imitate this in our own language, by saying *brokenly-feathered, sharp-sawed;* by using *threed* for *ternate*, as the Germans employ *gedreit;* by saying *fingered-feathered* for *digitato-pinnate*, and the like. But the habit which we have, in common as well as scientific language, of borrowing words from the Latin for new cases, would make such usages seem very harsh and pedantic.

We may add that, in consequence of these different practices in the two languages, it is a common habit of the German reader to impose a scientific definiteness upon a common word, such as our Fifth Aphorism requires; whereas the English reader expects rather that a word which is to have a technical sense shall be derived from the learned languages. *Die Kelch* and *die Blume* (the cup and the flower) easily assume the technical meaning of *calyx* and *corolla; die Griffel* (the pencil) becomes *the pistil;* and a name is easily found for the *pollen*, the *anthers*, and the *stamens*, by calling them the dust, the dust-cases, and the dust-threads (*der Staub, die Staub-beutal*, or *Staub-fächer*, and *die Staub-fäden*). This was formerly done in English to a greater extent than is now possible without confusion and pedantry. Thus, in Grew's book on the *Anatomy of Plants*, the calyx is called the *impalement*, and the sepals the *impalers;* the petals are called the *leaves of the flower;* the stamens with their anthers are the *seminiform attire*. But the English language, as to such matters, is now less flexible than it was; partly in consequence of its having adopted the Linnæan terminology almost entire, without any eudeavour to naturalize it. Any attempt at idiomatic description would interfere with the scientific language now generally received in this country. In Germany, on the other hand, those who first wrote upon science in their own language imitated the Latin words which they found in foreign writers, instead of transferring new roots into their own language. Thus the *Numerator* and *Denominator* of a fraction they call the *Namer* and the *Counter* (*Nenner* and *Zähler*). This course they pursued even where the expression was erroneous. Thus that portion of the intestines which ancient anatomists called *Duodenum*, because they falsely estimated its length at twelve inches, the Germans also term *Zwölffingerdarm* (twelve-inch-gut), though this intestine in a whale is twenty feet long, and in a frog not above twenty lines. As another example of this process in German, we may take the word *Muttersackbauchblatte*, the *uterine peritonæum.*

It is a remarkable evidence of this formative power of the German language, that it should have been able to produce an imitation of the systematic chemical nomenclature of the French school, so complete, that it is used in Germany as familiarly as the original system is in France and England. Thus Oxygen and Hydrogen are *Sauerstoff* and *Wafferstoff;* Azote is *Stickstoff* (suffocating matter); Sulphuric and Sulphurous Acid are *Schwefel-säure* and *Schwefelichte-säure.* The Sulphate and Sulphite of Baryta, and Sulphuret of Baryum, are *Schwefel-säure Baryterde*, *Schwefelichte-säure Baryterde*, and *Schwefel-baryum.* Carbonate of Iron is *Kohlen-säures Eisenoxydul;* and we may observe that, in such cases, the German name is much more agreeable to analogy than the English one; for the Protoxide of Iron, (*Eisenoxydul,*) and not the Iron itself, is the base of the salt. And the German language has not only thus imitated the established nomenclature of chemistry, but has shown itself capable of supplying new forms to meet the demands which the progress of theory occasions. Thus the Hydracids are *Wasserstoff-säuren;* and of these, the Hydriodic Acid is *Iodwasserstoff-säure*, and so of the rest. In like manner, the translator of Berzelius has found German names for the sulpho-salts of that chemist; thus he has *Wasserstoffschwefliges Schewefel-lithium*, which would be (if we were to adopt his theoretical view,) hydro-sulphuret of sulphuret of lithium: and a like nomenclature for all other similar cases.

3. In English we have no power of imitating this process, and must take our technical phrases from some more flexible language, and generally from the Latin or Greek. We are indeed so much accustomed to do this, that except a word has its origin in one of these languages, it hardly seems to us a technical term; and thus by employing indigenous terms, even descriptive ones, we may, perhaps, lose in precision more than we gain in the vividness of the impression. Perhaps it may be better to say *cuneate*, *lunate*, *hastate*, *sagittate*, *reniform*, than *wedge-shaped*, *crescent-shaped*, *halbert-headed*, *arrow-headed*, *kidney-shaped*. *Ringent* and *personate* are better than any English words which we could substitute for them; *labiate* is more precise than *lipped* would readily become. *Urceolate*, *trochlear*, are more compact than *pitcher-shaped*, *pulley-shaped;* and *infundibuliform*, *hypocrateriform*, though long words, are not more inconvenient than *funnel-shaped* and *salver-shaped*. In the same way it is better to speak (with Dr. Prichard*,) of *repent* and *progressive* animals, than of *creeping* and progressive: the two Latin terms make a better pair of correlatives.

4. But wherever we may draw the line between the proper use of English and Latin terms in descriptive phraseology, we shall find it advisable to borrow almost all other technical terms from the learned languages.

* *Researches*, p. 69.

We have seen this in considering the new terms introduced into various sciences in virtue of our Ninth Maxim. We may add, as further examples, the names of the various animals of which a knowledge has been acquired from the remains of them which exist in various strata, and which have been reconstructed by Cuvier and his successors. Such are the *Palæotherium*, the *Anoplotherium*, the *Megatherium*, the *Dinotherium*, the *Chirotherium*, the *Megalichthys*, the *Mastodon*, the *Ichthyosaurus*, the *Plesiosaurus*, the *Pterodactylus*. To these others are every year added; as, for instance, very recently, the *Toxodon*, *Zeuglodon*, and *Phascolotherium* of Mr. Owen, and the *Thylacotherium* of M. Valenciennes. Still more recently the terms *Glyptodon*, *Mylodon*, *Dicynodon*, *Paloplotherium*, *Rhynchosaurus*, have been added by Mr. Owen to designate fossil animals newly determined by him.

The names of species, as well as of genera, are thus formed from the Greek: as the Plesiosaurus *dolichodeirus*, (long-necked), Ichthyosaurus *platyodon* (broad-toothed), the Irish elk, termed Cervus *megaceros* (large-horned). But the descriptive specific names are also taken from the Latin, as Plesiosaurus *brevirostris*, *longirostris*, *crassirostris;* besides which there are arbitrary specific names, which we do not here consider.

These names being all constructed at a period when naturalists were familiar with an artificial system, the standard language of which is Latin, have not been taken from modern language. But the names of living animals, and even of their classes, long ago formed in the common language of men, have been in part adopted in the systems of naturalists, agreeably to Aphorism Third. Hence the language of systems in natural history is mixed of ancient and modern languages. Thus Cuvier's divisions of the vertebrated animals are

Mammifères (Latin), *Oiseaux, Reptiles, Poissons; Bimanes, Quadrumanes, Carnassières, Rongeurs, Pachydermes* (Greek), *Ruminans* (Latin), *Cétacés* (Latin). In the subordinate divisions the distribution being more novel, the names are less idiomatic: thus the kinds of Reptiles are *Cheloniens, Sauriens, Ophidiens, Batraciens,* all which are of Greek origin. In like manner, Fish are divided into *Chondropterygiens, Malacopterygiens, Acanthopterygiens.* The unvertebrated animals are *Mollusques, Animaux articulés,* and *Animaux rayonnés;* and the Mollusques are divided into six classes, chiefly according to the position or form of their foot; namely, *Cephalopodes, Pteropodes, Gasteropodes, Acephales, Brachiopodes, Cirrhopodes.*

In transferring these terms into English, when the term is new in French as well as English, we have little difficulty; for we may take nearly the same liberties in English which are taken in French; and hence we may say *mammifers* (rather *mammals*), *cetaceans* or *cetaces, batracians* (rather *batrachians*), using the words as substantives. But in other cases we must go back to the Latin: thus we say *radiate* animals, or *radiata* (rather *radials*), for *rayonnées.* These changes, however, rather refer to another Aphorism.

[Mr. Kirby has proposed *Radiary, Radiaries,* for *Radiata.*]

5. When new Mineral Species have been established in recent times, they have generally had arbitrary names a signed to them, derived from some person or places. In some instances, however, descriptive names have been selected; and then these have been generally taken from the Greek, as *Augite, Stilbite, Diaspore, Dichroite, Dioptase.* Several of these Greek names imposed by Haüy, refer to some circumstances, often fancifully selected, in his view of the crystallization of the substance, as *Epi-*

dote, *Peridote*, *Pleonast*. Similar terms of Greek origin have been introduced by others, as *Orthite*, *Anorthite*, *Periklin*. Greek names founded on casual circumstances are less to be commended. Berzelius has termed a mineral *Eschynite*, from αἰσχυνὴ, *shame*, because it is, he conceives, a shame for chemists not to have separated its elements more distinctly than they did at first.

6. In Botany, the old names of genera of Greek origin are very numerous, and many of them are descriptive, as *Glycyrhiza* (γλυκὺς and ῥίζα, sweet root) liquorice, *Rhododendron* (rose-tree), *Hæmatoxylon* (bloody wood), *Chrysocoma* (golden hair), *Alopecurus* (fox-tail), and many more. In like manner there are names which derive a descriptive significance from the Latin, either adjectives, as *Impatiens*, *Gloriosa*, *Sagittaria*, or substantives irregularly formed, as *Tussilago* (à tussis domatione), *Urtica* (ab urendo tactu), *Salsola* (à salsedine). But these, though good names when they are established by tradition, are hardly to be imitated in naming new plants. In most instances, when this is to be done, arbitrary or local names have been selected, as *Strelitzia*.

7. In Chemistry, new substances have of late had names assigned them from Greek roots, as *Iodine*, from its violet colour, *Chlorine* from its green colour. In like manner fluorine has by the French chemists been called *Phthor*, from its destructive properties. So the new metals, *Chrome*, *Rhodium*, *Iridium*, *Osmium*, had names of Greek derivation descriptive of their properties. Some such terms, however, were borrowed from localities, as *Strontia*, *Yttria*, the names of new earths. Others have a mixed origin, as *Pyrogallic*, *Pyroacetic*, and *Pyroligneous* Spirit. In some cases the deviation has been extravagantly capricious. Thus in the process for making Pyrogallic Acid, a certain substance is left behind, from which M. Braconnot extracted an acid

which he called *Ellagic* Acid, framing the root of the name by reading the word *Galle* backwards.

The new laws which the study of Electro-chemistry brought into view, required a new terminology to express their conditions: and in this case, as we have observed in speaking of the Twelfth Maxim, arbitrary words are less suitable. Mr. Faraday very properly borrowed from the Greek his terms *Electrolyte*, *Electrode*, *Anode*, *Cathode*, *Anīon*, *Cathīon*, *Dielectric*. In the mechanico-chemical and mechanical sciences, however, new terms are less copiously required than in the sciences of classification, and when they are needed, they are generally determined by analogy from existing terms. *Thermo-electricity* and *Electro-dynamics* were terms which very naturally offered themselves; Nobili's *thermo-multiplier*, Snow Harris's *unit-jar*, were almost equally obvious names. In such cases, it is generally possible to construct terms both compendious and descriptive, without introducing any new radical words.

8. The subject of Crystallography has inevitably given rise to many new terms, since it brings under our notice a great number of new relations of a very definite but very complex form. Haüy attempted to find names for all the leading varieties of crystals, and for this purpose introduced a great number of new terms, founded on various analogies and allusions. Thus the forms of calc-spar are termed by him *primitive*, *equiaxe*, *inverse*, *metastatique*, *contrastante*, *imitable*, *birhomboidale*, *prismatique*, *apophane*, *uniternaire*, *bisunitaire*, *dodécaèdre*, *contractée*, *dilatée*, *sexduodecimale*, *bisalterne*, *binoternaire*, and many others. The want of uniformity in the origin and scheme of these denominations would be no valid objection to them, if any general truth could be expressed by means of them: but the fact is, that there is no definite distinction of these forms. They pass into

each other by insensible gradations, and the optical and physical properties which they possess are common to all of them. And as a mere enunciation of laws of form, this terminology is insufficient. Thus it does not at all convey the relation between the *bisalterne* and the *binoternaire*, the former being a combination of the *metastatique* with the *prismatique*, the latter, of the *metastatique* with the *contrastante*: again, the *contrastante*, the *mixte*, the *cuboide*, the *contractée*, the *dilatée*, all contain faces generated by a common law, the index being respectively altered so as to be in these cases, 3, $\frac{3}{2}$, $\frac{4}{3}$, $\frac{9}{4}$, $\frac{5}{3}$; and this, which is the most important geometrical relation of these forms, is not at all recorded or indicated by the nomenclature. The fact is, that it is probably impossible, the subject of crystallography having become so complex as it now is, to devise a system of names which shall express the relations of form. Numerical symbols, such as those of Weiss or Naumann, or Professor Miller, are the proper ways of expressing these relations, and are the only good crystallographic terminology for cases in detail.

The terms used in expressing crystallographic laws have been for the most part taken from the Greek by all writers except some of the Germans. These, we have already stated, have constructed terms in their own language, as *zwei-und-ein gliedrig*, and the like.

In Optics we have some new terms connected with crystalline laws, as *uniaxal* and *biaxal* crystals, *optical axes*, which offered themselves without any effort on the part of the discoverers. In the whole history of the undulatory theory, very few innovations in language were found necessary, except to fix the sense of a few phrases, as *plane-polarized* light in opposition to *circularly-polarized*, and the like.

This is still more the case in Mechanics, Astronomy, and pure mathematics. In these sciences, several of the primary stages of generalization being already passed over, when any new steps are made, we have before us some analogy by which we may frame our new terms. Thus when the *plane of maximum areas* was discovered, it had not some new arbitrary denomination assigned it, but the name which obviously described it was fixed as a technical name.

The result of this survey of the scientific terms of recent formation seems to be this;—that indigenous terms may be employed in the descriptions of facts and phenomena as they at first present themselves; and in the first induction from these; but that when we come to generalize and theorize, terms borrowed from the learned languages are more readily fixed and made definite, and are also more easily connected with derivatives. Our native terms are more impressive, and at first more intelligible; but they may wander from their scientific meaning, and are capable of little inflexion. Words of classical origin are precise to the careful student, and capable of expressing, by their inflexions, the relations of general ideas; but they are unintelligible, even to the learned man, without express definition, and convey instruction only through an artificial and rare habit of thought.

Since in the balance between words of domestic and of foreign origin so much depends upon the possibility of inflexion and derivation, I shall consider a little more closely what are the limits and considerations which we have to take into account in reference to that subject.

APHORISM XVI.

In the composition and inflexion of technical terms, philological analogies are to be preserved if possible, but modified according to scientific convenience.

In the language employed or proposed by writers upon subjects of science, many combinations and forms of derivation occur, which would be rejected and condemned by those who are careful of the purity and correctness of language. Such anomalies are to be avoided as much as possible; but it is impossible to escape them altogether, if we are to have a scientific language which has any chance of being received into general use. It is better to admit compounds which are not philologically correct, than to invent many new words, all strange to the readers for whom they are intended: and in writing on science in our own language, it is not possible to avoid making additions to the vocabulary of common life; since science requires exact names for many things which common language has not named. And although these new names should, as much as possible, be constructed in conformity with the analogies of the language, such extensions of analogy can hardly sound, to the grammarian's ear, otherwise than as solecisms. But, as our maxim indicates, the analogy of science is of more weight with us than the analogy of language: and although anomalies in our phraseology should be avoided as much as possible, innovations must be permitted wherever a scientific language, easy to acquire, and convenient to use, is unattainable without them.

I shall proceed to mention some of the transgressions of strict philological rules, and some of the extensions of grammatical forms, which the above conditions appear to render necessary.

1. The combination of different languages in the derivation of words, though to be avoided in general, is in some cases admissible.

Such words are condemned by Quintilian and other grammarians, under the name of *hybrids*, or things of a mixed race; as *biclinium*, from *bis* and *κλίνη*; *epitogium*, from ἐπὶ and *toga*. Nor are such terms to be unnecessarily introduced in science. Whenever a homogeneous word can be formed and adopted with the same ease and convenience as a hybrid, it is to be preferred. Hence we must have *ichthyology*, not *piscology*, *entomology*, not *insectology*, *insectivorous*, not *insectophagous*. In like manner, it would be better to say *unoculus* than *monoculus*, though the latter has the sanction of Linnæus, who was a purist in such matters. Dr. Turner, in his *Chemistry*, speaks of *protoxides* and *binoxides*, which combination violates the rule for making the materials of our terms as homogeneous as possible; *protoxide* and *deutoxide* would be preferable, both on this and on other accounts.

Yet this rule admits of exceptions. *Mineralogy*, with its Greek termination, has for its root *minera*, a medieval Latin word of Teutonic origin, and is preferable to *Oryctology*. *Terminology* appears to be better than *Glossology*: which according to its derivation would be rather the science of language in general than of technical terms; and *Horology*, from ὅρος, a term, would not be immediately intelligible, even to Greek scholars; and is already employed to indicate the science which treats of horologes, or time-pieces.

Indeed, the English reader is become quite familiar with the termination *ology*, the names of a large number of branches of science and learning having that form. This termination is at present rather apprehended as a formative affix in our own language, indicating a science, than as an element borrowed from a foreign language. Hence, when it is difficult or impossible to find a Greek term which clearly designates the subject of a science, it is allowable to employ some other, as in *Tidology*, the doctrine of the Tides.

The same remark applies to some other Greek elements of scientific words: they are so familiar to us that in composition they are almost used as part of our own language. This naturalization has taken place very decidedly in the element *arch*, (ἀρχὸς, a leader,) as we see in *archbishop*, *archduke*. It is effected in a great degree for the preposition *anti:* thus we speak of *anti-slavery* societies, *anti-reformers*, *anti-bilious*, or *anti-acid* medicines, without being conscious of any anomaly. The same is the case with the Latin preposition *præ* or *pre*, as appears from such words as *pre-engage*, *pre-arrange*, *pre-judge*, *pre-paid;* and in some measure with *pro*, for in colloquial language we speak of *pro-catholics* and *anti-catholics*. Also the preposition *ante* is similarly used, as *ante-nicene* fathers. The preposition *co*, abbreviated from *con*, and implying things to be simultaneous or connected, is firmly established as part of the language, as we see in *coexist*, *coheir*, *coordinate;* hence I have called those lines *cotidal* lines which pass through places where the high water of the tide occurs simultaneously.

2. As in the course of the mixture by which our language has been formed, we have thus lost all habitual consciousness of the difference of its ingredients, (Greek, Latin, Norman-French, and Anglo-Saxon): we have also ceased to confine to each ingredient the mode of grammatical inflexion which originally belonged to it. Thus the termination *ive* belongs peculiarly to Latin adjectives, yet we say *sportive*, *talkative*. In like manner, *able* is added to words which are not Latin, as *eatable*, *drinkable*, *pitiable*, *enviable*. Also the termination *al* and *ical* are used with various roots, as *loyal*, *royal*, *farcical*, *whimsical;* hence we may make the adjective *tidal* from tide. This ending, *al*, is also added to abstract terms in *ion*, as *occasional*, *provisional*, *intentional*, *national;* hence we may, if necessary, use such words as *educa-*

tional, terminational. The ending *ic* appears to be suited to proper names, as *Pindaric, Socratic, Platonic;* hence it may be used when scientific words are derived from proper names, as *Voltaic* or *Galvanic* electricity: to which I have proposed to add *Franklinic.*

In adopting scientific adjectives from the Latin, we have not much room for hesitation; for, in such cases, the habits of derivation from that language into our own are very constant; *ivus* becomes *ive,* as *decursive; inus* becomes *ine,* as in *ferine; atus* becomes *ate,* as *hastate;* and *us* often becomes *ous,* as *rufous; aris* becomes *ary,* as *axillary; ens* becomes *ent,* as *ringent.* And in adopting into our language, as scientific terms, words which in another language, the French for instance, have a Latin origin familiar to us, we cannot do better than form them as if they were derived directly from the Latin. Hence the French adjectives *cétacé, crustacé, testacé,* may become either *cetaceous, crustaceous, testaceous,* according to the analogy of *farinaceous, predaceous,* or else *cetacean, crustacean, testacean,* imitating the form of *patrician.* Since, as I shall soon have to notice, we require substantives as well as adjectives from these words, we must, at least for that use, take the forms last suggested.

In pursuance of the same remark, *rongeur* becomes *rodent;* and *edenté* would become *edentate,* but that this word is rejected on another account: the adjectives *bimane* and *quadrumane* are *bimanous* and *quadrumanous.*

3. There is not much difficulty in thus forming adjectives: but the purposes of Natural History require that we should have substantives corresponding to these adjectives; and these cannot be obtained without some extension of the analogies of our language. We cannot in general use adjectives or participles as singu-

lar substantives. *The happy* or *the doomed* would, according to good English usage, signify those who are happy and those who are doomed. Hence we could not speak of a particular scaled animal as *the squamate*, and still less could we call any such animal *a squamate*, or speak of *squamates* in the plural. Some of the forms of our adjectives, however, do admit of this substantive use. Thus we talk of *Europeans*, *plebeians*, *republicans;* of *divines* and *masculines;* of the *ultramontanes;* of *mordants* and *brilliants;* of *abstergents* and *emollients;* of *mercenaries* and *tributaries;* of *animals*, *manuals*, and *officials;* of *dissuasives* and *motives*. We cannot generally use in this way adjectives in *ous*, nor in *ate* (though *reprobates* is an exception), nor English participles, nor adjectives in which there is no termination imitating the Latin, as *happy*, *good*. Hence, if we have, for purposes of science, to convert adjectives into substantives, we ought to follow the form of examples like these, in which it has already appeared in fact, that such usage, though an innovation at first, may ultimately become a received part of the language.

By attention to this rule we may judge what expressions to select in cases where substantives are needed. I will take as an example the division of the mammalian animals into Orders. These Orders, according to Cuvier, are *Bimanes*, *Quadrumanes*, *Carnassiers*, *Rongeurs*, *Edentés*, *Ruminans*, *Pachydermes*, *Cétacés;* and of these, *Bimanes*, *Quadrumanes*, *Rodents*, *Ruminants*, *Pachyderms* are admissible as English substantives on the grounds just stated. *Cetaceous* could not be used substantively; but *Cetacean* in such a usage is sufficiently countenanced by such cases as we have mentioned, *patrician*, &c.; hence we adopt this form. We have no English word equivalent to the French *Carnassiers:* the English translator of Cuvier has not provided English words for his technical terms; but has formed a Latin word, *Carnaria*, to represent the French terms. From this we might readily form *Carnaries;* but it appears much better to take the Linnæan name *Feræ* as our root, from which we may take *Ferine*, substantive as well as adjective; and hence we call this order *Ferines.* The word for which it is most difficult to provide a proper representation, is *Edenté*, *Edentata:* for, as we have said, it would be very harsh to speak of the order as the *Edentates;* and if we were to abbreviate the word into *edent*, we should suggest a false analogy with *rodent*, for as *rodent* is *quod rodit*, that which gnaws, *edent* would be *quod edit*, that which eats. And even if we were to take *edent* as a substantive, we could hardly use it as an adjective: we should still have to say, for example, the *edentate* form of head. For these reasons it appears best to alter the form of the word, and to call the Order the *Edentals*, which is quite allowable, both as adjective and substantive.

[An objection might be made to this term, both in its Latin, French and English form: namely, that the natural group to which it is applied includes many species, both existing and extinct, well provided with teeth. Thus the armadillo is remarkable for the number of its teeth; the megatherium, for their complex structure. But the analogy of scientific language readily permits us to fix, upon the word *edentata*, a special meaning, implying the absence of one particular kind of teeth, namely, incisive teeth. Linnæus called the equivalent order *Bruta.* We could not apply in this case the term *Brutes;* for common language has already attached to the word a wider meaning, too fixedly for scientific use to trifle with it.]

There are several other words in *ate* about which there is the same difficulty in providing substantive forms. Are we to speak of *Vertebrates?* or would it not be better, in agreement with what has been said above, to call these *Vertebrals*, and the opposite class *Invertebrals?*

There are similar difficulties with regard to the names of subordinate portions of zoological classification; thus the Ferines are divided by Cuvier into *Cheiroptéres*, *Insectivores*, *Carnivores;* and these latter into *Plantigrades*, *Digitigrades*, *Amphibies*, *Marsupiaux*. There is not any great harshness in naturalizing these substantives as *Chiropters*, *Insectivores*, *Carnivores*, *Plantigrades*, *Digitigrades*, *Amphibians*, and *Marsupials*. These words *Carnivores* and *Insectivores* are better, because of more familiar origin, than Greek terms; otherwise we might, if necessary, speak of *Zoophagans* and *Entomophagans*.

It is only with certain familiar adjectival terminations, as *ous* and *ate*, that there is a difficulty in using the word as substantive. When this can be avoided, we readily accept the new word, as *Pachyderms*, and in like manner *Mollusks*.

If we examine the names of the Orders of Birds, we find that they are in Latin, *Predatores* or *Accipitres*, *Passeres*, *Scansores*, *Rasores* or *Gallinæ*, *Grallatores*, *Palmipedes* and *Anseres:* Cuvier's Orders are, *Oiseaux de Proie*, *Passereaux*, *Grimpeurs*, *Gallinacés*, *Echassiérs*, *Palmipedes*. These may be englished conveniently as *Predators*, *Passerines*, *Scansors*, *Gallinaceans*, (rather than *Rasors*,) *Grallators*, *Palmipedans*, [or rather *Palmipeds*, like *Bipeds*]. *Scansors*, *Grallators*, and *Rasors*, are better, as technical terms, than *Climbers*, *Waders*, and *Scratchers*. We might venture to anglicize the terminations of the names which Cuvier gives to the divisions of these Orders: thus the Predators are the *Diurnals* and the *Nocturnals;* the Passerines are the *Dentirostres*, the *Fissirostres*, the *Conirostres*, the *Tenuirostres*, and the *Syndactyls*: the word *lustre* showing that the former termination is allowable. The Scansors are not sub-divided, nor are the Gallinaceans. The Grallators are *Pressirostres*, *Cultrirostres*, *Macrodactyls*. The Palmipeds are the *Plungers*, the *Longipens*, the *Totipalmes* and the *Lamellirostres*.

The next class of Vertebrals is the *Reptiles*, and these are either *Chelonians*, *Saurians*, *Ophidians*, or *Batrachians*. Cuvier writes *Batraciens*, but we prefer the spelling to which the Greek word directs us.

The last or lowest class is the *Fishes*, in which province Cuvier has himself been the great systematist, and has therefore had to devise many new terms. Many of these are of Greek or Latin origin, and can be anglicized by the analogies already pointed out, as *Chondropterygians*, *Malacopterygians*, *Lophobranchs*, *Plectognaths*, *Gymnodonts*, *Sclerodermis*. *Discoboles* and *Apodes* may be English as well as French. There are other cases in which the author has formed the names of Families, either by forming a word in *ides* from the name of a genus, as *Gadoides*, *Gobiöides*, or by gallicizing the Latin name of the genus, as *Salmones* from *Salmo*, *Clupes* from *Clupea*, *Esoces* from *Esox*, *Cyprins* from *Cyprinus*. In these cases Agassiz's favourite form of names for families of fishes has led English writers to use the words *Gadoids*, *Gobioids*, *Salmonoids*, *Clupeoids*, *Lucioids* (for *Esocés*) *Cyprinoids*, &c. There it a taint of hybridism in this termination, but it is attended with this advantage, that it has begun to be characteristic of the nomenclature of family groups in the class *Pisces*. One of the orders of fishes, co-ordinate with the Chondropterygians and the Lophobranchs, is termed *Osseux* by Cuvier. It appears hardly worth while to invent a substantive word for this, when *Bony Fishes* is so simple a phrase, and may readily be understood as a technical name of a systematic order.

The Mollusks are the next Class; and these are divided into *Cephalopods*, *Gasteropods*, and the like. The Gasteropods are *Nudibranchs*, *Inferobranchs*, *Tectibranchs*, *Pectinibranchs*, *Scutibranchs*, and *Cyclobranchs*. In framing most of these terms Cuvier has made hybrids by a combination of a Latin word with *branchiæ*, which is the Greek name for the gills of a fish; and has thus avoided loading the memory with words of an origin not obvious to most naturalists, as terms derived from the Greek would have been. Another division of the Gasteropods is *Pulmonés*, which we must make *Pulmonians*. In like manner the subdivisions of the Pectinibranchs are the *Trochoidans* and *Buccinoidans*, (*Trochoïdes*, *Buccinoïdes*). The *Acéphales*, another order of Mollusks, may be *Acephals* in English.

After these comes the third grand division, *Articulated Animals*, and these are *Annelidans*, *Crustaceans*, *Arachnidans*, and *Insects*. I shall not dwell upon the names of these, as the form of English words which is to be selected must be sufficiently obvious from the preceding examples.

Finally, we have the fourth grand division of animals, the *Rayonnés*, or *Radiata*; which, for reasons already given, we may call *Radials*, or *Radiaries*. These are *Echinoderms*, *Intestinals*, (or rather *Entozoans*,) *Acalephes*, and *Polyps*. The Polyps, which are composite animals in which many gelatinous individuals are connected so as to have a common life, have, in many cases, a more solid framework belonging to the common part of the animal. This framework, of which coral is a special example, is termed in French *Polypier*; the word has been anglicized by the word *polypary*, after the analogy of *aviary* and *apiary*. Thus Polyps are either *Polyps with Polyparies* or *Naked Polyps*.

Any common kind of Polyps has usually in the English language been called *Polypus*, the Greek termination being retained. This termination in *us*, however, whether Latin or Greek, is to be excluded from the English as much as possible, on account of the embarassment which it occasions in the formation of the plural. For if we say *Polypi* the word ceases to be English, while *Polypuses* is harsh: and there is the additional inconvenience, that both these forms would indicate the plural of individuals rather than of classes. If we were to say, "The Corallines are a Family of the *Polypuses with Polyparies*," it would not at once occur to the reader that the three last words formed a technical phrase.

This termination *us*, which must thus be excluded from the names of families, may be admitted in the designation of genera; of animals, as *Nautilus*, *Echinus*, *Hippopotamus*; and of plants, as *Crocus*, *Asparagus*, *Narcissus*, *Acanthus*, *Ranunculus*, *Fungus*. The same form occurs in other technical words, as *Fucus*, *Mucus*, *Œsophagus*, *Hydrocephalus*, *Callus*, *Calculus*, *Uterus*, *Fœtus*, *Radius*, *Focus*, *Apparatus*. It is, however, advisable to retain this form only in cases where it is already firmly established in the language; for a more genuine English form is preferable. Hence we say, with Mr. Lyell, *Icthyosaur*, *Plesiosaur*, *Pterodactyl*. In like manner Mr. Owen anglicizes the termination *erium*, and speaks of the *Anoplothere* and *Paleothere*.

Since the wants of science thus demand adjectives which can be used also as substantive names of classes, this consideration may sometimes serve to determine our selection of new terms. Thus Mr. Lyell's names for the subdivisions of the tertiary strata, *Miocene*, *Pliocene*, can be used as substantives; but if such words as *Mioneous*, *Plioneous*, had suggested themselves, they must have been rejected, though of equivalent signification, as not fulfilling this condition.

4. (*a.*) Abstract substantives can easily be formed from adjectives: from electric we have *electricity;* from galvanic, *galvanism;* from organic, *organization; velocity*, *levity*, *gravity*, are borrowed from Latin adjectives. *Caloric* is familiarly used for the matter of heat, though the form of the word is not supported by any obvious analogy.

(*b.*) It is quite intolerable to have words regularly formed, in opposition to the analogy which their meaning offers; as when bodies are said to have conduct*ibility* or condu*cibility* with regard to heat. The bodies are conduct*ive*, and their property is conduct*ivity*.

(*c.*) The terminations *ize* (rather than *ise*), *ism*, and *ist*, are applied to words of all origins: thus we have to *pulverize*, to *colonize*, *Witticism*, *Heathenism*, *Journalist*, *Tobacconist*. Hence we may make such words when they are wanted. As we cannot use *physician* for a cultivator of physics, I have called him a *Physicist*. We need very much a name to describe a cultivator of science in general. I should incline to call him a *Scientist*. Thus we might say, that as an Artist is a Musician, Painter, or Poet, a Scientist is a Mathematician, Physicist, or Naturalist.

(*d.*) Connected with verbs in *ize*, we have abstract nouns in *ization*, as *polarization*, *crystallization*. These it appears proper to spell in English with *z* rather than *s;* governing our practice by the Greek verbal termination ίζω which we imitate. But we must observe that verbs and substantives in *yse*, (*analyse*,) belong to a different analogy, giving an abstract noun in *ysis* and an adjective *ytic* or *ytical*; (*analysis*, *analytic*, *analytical*). Hence *electrolyse* is more proper than *electrolyze**.

(*e*.) The names of many sciences end in *ics* after the analogy of *Mathematics*, *Metaphysics*; as *Optics*, *Mechanics*. But these in most other languages, as in our own formerly, have the singular form *Optice*, *l'Optique*, *Optik*, *Optick*: and though we now write *Optics*, we make such words of the singular number: "Newton's Opticks is an example." As, however, this connexion in new words is startling, as when we say "Thermo-electrics is now much cultivated," it appears better to employ the singular form, after the analogy of *Logic* and *Rhetoric*, when we have words to construct. Hence we may call the science of languages *Linguistic*, as it is called by the best German writers, for instance, William von Humboldt.

5. In the derivation of English from Latin or Greek words, the changes of letters are to be governed by the rules which have generally prevailed in such cases. The Greek οι and αι, the Latin *oe* and *ae*, are all converted into a simple *e*, as in *E*conomy, Geod*e*sy, p*e*nal, C*e*sar. Hence, according to common usage, we should write ph*e*nomena, not ph*œ*nomena, pal*e*ontology, not pal*œ*ontology, mioc*e*ne not mioc*œ*ne, p*e*klite not p*œ*klite. But in order to keep more clearly in view the origin of our terms, it may be allowable to deviate from these rules of change, especially so long as the words are still new and unfamiliar. Dr. Buckland speaks of the *poikilitic*, not *pecilitic*, group of strata: *palæontology* is the spelling commonly adopted; and in imitation of this I have written *palætiology*. The diphthong ει was by the Latins changed into *i*, as in Arist*i*des; and hence this has been the usual

* I fear I have, in some of the preceding pages, neglected this distinction.

form in English. Some recent authors indeed (Mr. Mitford for instance) write Aristeides; but the former appears to be the more legitimate. Hence we write m*i*ocene, pl*i*ocene, not m*ei*ocene, pl*ei*ocene. The Greek *υ* becomes *y*, and *ου* becomes *u*, in English as in Latin, as cr*y*stal, col*u*re. The consonants *κ* and *χ* become *c* and *ch* according to common usage. Hence we write *crystal*, not *chrystal*, batra*ch*ian, not batra*c*ian, *c*ryolite, not *ch*ryolite. As, however, the letter *c* before *e* and *i* differs from *k*, which is the sound we assign to the Greek *κ*, it may be allowable to use *k* in order to avoid this confusion. Thus, as we have seen, poi*k*ilite has been used, as well as pecilite. Even in common language some authors write s*k*eptic, which appears to be better than sceptic with our pronunciation, and is preferred by Dr. Johnson. For the same reason, namely to avoid confusion in the pronunciation, and also, in order to keep in view the connexion with *cathode*, the elements of an electrolyte which go to the anode and cathode respectively may be termed the anion and cat*h*ion; although the Greek would suggest catïon, (*κατίον*).

6. The example of chemistry has shown that we have in the terminations of words a resource of which great use may be made in indicating the relations of certain classes of objects: as sulphur*ous* and sulphur*ic* acids; sulph*ates*, sulph*ites*, and sulph*urets*. Since the introduction of the artifice by the Lavoisierian school, it has been extended to some new cases. The Chlor*ine*, Fluor*ine*, Brom*ine*, Iod*ine*, had their names put into that shape in consequence of their supposed analogy: and for the same reason have been termed Chlore, Phtore, Brome, Iode, by French chemists. In like manner, the names of metals in their Latin form have been made to end in *um*, as Osmium, Palladium; and hence it is better to say Platin*um*, Molybden*um*, than Platin*a*, Molyb-

den*a*. It has been proposed to term the basis of Boracic acid Bor*on*; and those who conceive that the basis of Silica has an analogy with Boron have proposed to term it Silic*on*, while those who look upon it as a metal would name it Silici*um*. Seleni*um* was so named when it was supposed to be a metal: as its analogies are now acknowledged to be of another kind, it would be desirable, if the change were not too startling, to term it Sel*en*, as it is in German. Phosph*orus* in like manner might be Phosph*ur*, which would indicate its analogy with Sulph*ur*.

The resource which terminations offer has been applied in other cases. The names of many species of minerals end in *lite*, or *ite*, as Stauro*lite*, Aug*ite*. Hence Adolphe Brongniart, in order to form a name for a genus of fossil plants, has given this termination to the name of the recent genus which they nearly resemble, as Zam*ites* from Zamia, Lycopod*ites* from Lycopodium.

Names of different genera which differ in termination only are properly condemned by Linnæus*; as Alsine, Alsinoides, Alsinella, Alsinastrum; for there is no definite relation marked by those terminations. Linnæus gives to such genera distinct names, Alsine, Bufonia, Sagina, Elatine.

Terminations are well adapted to express definite systematic relations, such as those of chemistry, but they must be employed with a due regard to all the bearings of the system. Davy proposed to denote the combinations of other substances with chlorine by peculiar terminations; using *ane* for the smallest proportion of Chlorine, and *anea* for the larger, as Cupr*ane*, Cupr*anea*. In this nomenclature, common salt would be *Sodane*, and Chloride of Nitrogen would be *Azotane*. This suggestion never found favour. It was objected that it was

* *Phil. Bot.*, 231.

contrary to the Linnæan precept, that a specific name must not be united to a generic as a termination. But this was not putting the matter exactly on its right ground; for the rules of nomenclature of natural history do not apply to chemistry; and the Linnæan rule might with equal propriety have been adduced as a condemnation of such terms as Sulphur*ous*, Sulphur*ic*. But Davy's terms were bad; for it does not appear that Chlorine enters, as Oxygen does, into so large a portion of chemical compounds, that its relations afford a key to their nature, and may properly be made an element in their names.

This resource, of terminations, has been abused, wherever it has been used wantonly, or without a definite significance in the variety. This is the case in M. Beudant's Mineralogy. Among the names which he has given to new species, we find the following (besides many in *ite*), Scolexer*ose*, Opsim*ose*, Exanthel*ose*, &c.; Diacr*ase*, Panab*ase*, Neopl*ase*; Neocl*ese*; Rhodo*ise*, Stibicon*ise*, &c.; Marcel*ine*, Whilelm*ine*, &c.; Exit*ele*, and many others. In addition to other objections which might be made to these names, their variety is a material defect: for to make this variety depend on caprice alone, as in those cases it does, is to throw away a resource of which chemical nomenclature may teach us the value.

Aphorism XVII.

When alterations in technical terms become necessary, it is desirable that the new term should contain in its form some memorial of the old one.

We have excellent examples of the advantageous use of this maxim in Linnæus's reform of botanical nomenclature. His innovations were very extensive, but they were still moderated as much as possible, and connected in many ways with the names of plants then in use. He has himself given several rules of nomenclature, which tend to establish this connexion of the old and new in a reform. Thus he says, "Generic names which are current, and are not accompanied with harm to botany, should be tolerated*." "A passable generic name is not to be changed for another, though more apt†." "New generic names are not to be framed so long as passable synonyms are at hand‡." "A generic name of one genus, except it be superfluous, is not to be transferred to another genus, though it suit the other better§." "If a received genus requires to be divided into several, the name which before included the whole, shall be applied to the most common and familiar kind‖." And though he rejects all *generic* names which have not a Greek or Latin root¶, he is willing to make an exception in favour of those which from their form might be supposed to have such a root, though they are really borrowed from other languages, as *Thea*, which is the Greek for goddess; *Coffea*, which might seem to come from a Greek word denoting silence (κωφὸς); *Cheiranthus*, which appears to mean hand-flower, but is really derived from the Arabic *Keiri*: and many others.

As we have already said, the attempt at a reformation of the nomenclature of Mineralogy made by Professor Mohs will probably not produce any permanent effect, on this account amongst others, that it has not been conducted in this temperate mode; the innovations bear too large a proportion to the whole of the names, and contain too little to remind us of the known appellations. Yet in some respects Professor Mohs has acted upon this maxim. Thus he has called one of his classes

* *Philosophia Botanica*, Art. 242. † P. 246.
‡ *Phil. Bot.*, p. 247. § P. 249. ‖ P. 249. ¶ P. 232.

Spar, because *Felspar* belongs to it. I shall venture to offer a few suggestions on this subject of Mineralogical Nomenclature.

It has already been remarked that the confusion and complexity which prevail in this subject render a reform very desirable. But it will be seen, from the reasons assigned under the Ninth Aphorism, that no permanent system of names can be looked for, till a sound system of classification be established. The best mineralogical systems recently published, however, appear to converge to a common point; and certain classes have been formed which have both a natural-historical and a chemical significance. These Classes, according to Naumann, whose arrangement appears the best, are Hydrolytes, Haloids, Silicides, Oxides of Metals, Metals, Sulphurides (Pyrites, Glances, and Blendes), and Anthracides. Now we find; —that the Hydrolytes are all compounds, such as are commonly termed *Salts*;—that the Haloids are, many of them, already called *Spars*, as *Calc Spar*, *Heavy Spar*, *Iron Spar*, *Zinc Spar*;—that the *Silicides*, the most numerous and difficult class, are denoted for the most part, by single words, many of which end in *ite*;— that the other classes, or sub-classes, *Oxides*, *Pyrites*, *Glances*, and *Blendes*, have commonly been so termed: as *Red Iron Oxide*, *Iron Pyrites*, *Zinc Blende*;—while pure metals have usually had the adjective *native* prefixed, as *Native Gold*, *Native Copper*. These obvious features of the current names appear to afford us a basis for a systematic nomenclature. The Salts and Spars might all have the word *salt* or *spar* included in their name, as *Natron Salt*, *Glauber Salt*, *Rock Salt*; *Calc Spar*, *Bitter Spar*, (Carbonate of Lime and Magnesia), *Fluor Spar*, *Phosphor Spar*, (Phosphate of Lime), *Heavy Spar*, *Celestine Spar* (Sulphate of Strontian), *Chromic Lead Spar* (Chromate of Lead); the *Silicides* might all have the name constructed so as to be a single word ending in *ite*, as *Chabasite* (Chabasie), *Natrolite* (Mesotype), *Sommite* (Nepheline), *Pistacite* (Epidote); from this rule might be excepted the *Gems*, as *Topaz*, *Emerald*, *Corundum*, which might retain their old names. The Oxides, Pyrites, Glances, and Blendes, might be so termed; thus we should have *Tungstic Iron Oxide* (usually called Tungstate of Iron), *Arsenical Iron Pyrites* (Mispickel), *Tetrahedral Copper Glance* (Fahlerz), *Quicksilver Blende* (Cinnabar), and the Metals might be termed *native*, as *Native Copper*, *Native Silver*.

Such a nomenclature would take in a very large proportion of commonly received appellations, especially if we were to select among the synonyms, as is proposed above in the case of *Glauber Salt*, *Bitter Spar*, *Sommite*, *Pistacite*, *Natrolite*. Hence it might be adopted without serious inconvenience. It would make the name convey information respecting the place of the mineral in the system; and by imposing this condition, would limit the extreme caprice, both as to origin and form, which has hitherto been indulged in imposing mineralogical names.

The principle of a mineralogical nomenclature determined by the place of the species in the system, has been recognized by Mr. Beudant as well as Mr. Mohs. The former writer has proposed that we should say *Carbonate Calcaire*, *Carbonate Witherite*, *Sulphate Couperose*, *Silicate Stilbite*, *Silicate Chabasie*, and so on. But these are names in which the part added for the sake of the system, is not incorporated with the common name, and would hardly make its way into common use.

We have already noticed Mr. Mohs's designations for two of the Systems of Crystallization, the *Pyramidal* and the *Prismatic*, as not characteristic. If it were thought advisable to reform such a defect, this might be done by calling them the *Square Pyramidal* and the *Oblong Prismatic*, which terms, while they expressed the real distinction of the systems, would be intelligible at once to those acquainted with the Mohsian terminology.

I will mention another suggestion respecting the introduction of an improvement in scientific language. The term *Depolarization* was introduced, because it was believed that the effect of certain crystals, when polarized light was incident upon them in certain positions, was to destroy the peculiarity which polarization had produced. But it is now well known that the effect of the second crystal in general is to divide the polarized ray of light into two rays, polarized in different planes. Still this effect is often spoken of as *Depolarization*, no better term having been yet devised. I have proposed and used the term *Dipolarization*, which well expresses what takes place, and so nearly resembles the older word, that it must sound familiar to those already acquainted with writings on this subject.

I may mention one term in another department of literature which it appears desirable to reform in the same manner. The theory of the Fine Arts, or the philosophy which speculates concerning what is beautiful in painting, sculpture or architecture, and other arts, often requires to be spoken of in a single word. Baumgarten and other German writers have termed this province of speculation *Æsthetics*; αἰσθάνεσθαι, *to perceive*, being a word which appeared to them fit to designate the perception of beauty in particular. Since, however, *æsthetics* would naturally denote the Doctrine of Perception in general; since this Doctrine requires a name;

since the term *æsthetics* has actually been applied to it by other German writers (as Kant); and since the essential point in the philosophy now spoken of is that it attends to Beauty;—it appears desirable to change this name. In pursuance of the maxim now before us, I should propose the term *Callæsthetics*, or rather (in agreement with what was said in page 561) *Callæsthetic*, the science of the perception of beauty.

APPENDIX.

PHILOSOPHICAL ESSAYS

PREVIOUSLY PUBLISHED.

ESSAY I.

ON THE NATURE OF THE TRUTH OF THE LAWS OF MOTION (1834)*.

1. The long continuance of the disputes and oppositions of opinion which have occurred among theoretical writers concerning the elementary principles of Mechanics, may have made such discussions appear to some persons wearisome and unprofitable. I might, however, not unreasonably plead this very circumstance as an apology for offering a new view of the subject; since the extent to which these discussions have already gone shews that some men at least take a great interest in them; and it may be stated, I think, without fear of contradiction, that these controversies have not terminated in the general and undisputed establishment of any one of the antagonist opinions.

The question to which my remarks at present refer is this: "What is the kind and degree of cogency of the best proofs of the laws of motion, or of the fundamental principles of mechanics, exprest in any other way?" Are these laws, philosophically considered, *necessary*, and capable of demonstration by means of self-evident axioms, like the truths of geometry; or are they *empirical*, and only known to be true by trial and observation, like such general rules as we obtain in natural history?

It certainly appears, at first sight, very difficult to answer the arguments for either side of this alternative. On the one hand it is said, the laws of motion cannot be necessarily true, for if they were so, the denial of them would involve a contradiction. But this it does not, for we can readily conceive them to be other than they are. We can conceive that a body in motion should have a natural tendency to move slower and slower. And we know that, historically speaking, men did at first suppose the laws of motion to be different from what they are now proved to be. This would have been impossible if the negation of these laws had involved a contradiction of self-evident principles, and consequently had been not only false but

* From the *Transactions of the Cambridge Philosophical Society*, Vol. v. Part II.

inconceivable. These laws, therefore, cannot be necessary; and can be duly established in no other way than by a reference to experience.

On the other hand, those who deduce their mechanical principles without any express reference to experiment, may urge, on their side, that, by the confession even of their adversaries, the laws of motion are proved to be true beyond the limits of experience;—that they are assumed to be true of any new kind of motion when first detected, as well as of those already examined;—and that it is inexplicable how such truths should be established empirically. They may add that the consequences of these laws are allowed to hold with the most complete and absolute universality; for instance, the proposition that "the quantity of motion in the world in a given direction cannot be either increased or diminished," is conceived to be rigorously exact; and to have a degree and kind of certainty beyond and above all mere facts of experience; what other kind of truth than necessary truth this can be, it is difficult to say. And if the conclusions be necessarily true, the principles must be so too.

This apparent contradiction therefore, that a law should be necessarily true and yet the contrary of it conceivable, is what I have now to endeavour to explain; and this I must do by pointing out what appear to me the true grounds of the laws of motion.

2. The science of Mechanics is concerned about motions as determined by their causes, namely, forces; the nature and extent of the truth of the first principles of this science must therefore depend upon the way in which we can and do reason concerning *causes*. In what manner we obtain the conception of cause, is a question for the metaphysician, and has been the subject of much discussion. But the general principle which governs our mode of viewing occurrences with reference to this conception, so far as our present subject is concerned, does not appear to be disturbed by any of the arguments which have been adduced in this controversy. This principle I shall state in the form of an axiom, as follows.

Axiom I.—*Every change is produced by a cause.*

It will probably be allowed that this axiom expresses a universal and constant conviction of the human mind; and that in looking at a series of occurrences, whether for theoretical or practical purposes, we inevitably and unconsciously assume the truth of this axiom. If a body at rest moves, or a body in motion stops, or turns to the right or the left, we cannot conceive otherwise than that there is some cause for this change. And so far as we can found our mechanical principles on this axiom, they will rest upon as broad and deep a basis as any truths which can come within the circle of our knowledge.

I shall not attempt to analyse this axiom further. Different persons may, according to their different views of such subjects, call it a law of our nature that we should think thus, or a part of the constitution of the human mind, or a result of our power of seeing the true relations of things. Such variety of opinion or expression would not affect the fundamental and universal character of the conviction which the axiom expresses; and would therefore not interfere with our future reasonings.

3. There is another axiom connected with this, which is also a governing and universal principle in all our reasoning concerning causes. It may be thus stated.

AXIOM II.—*Causes are measured by their effects.*

EVERY effect, that is, every change in external objects, implies a cause, as we have already said: and the existence of the cause is known only by the effects it produces. Hence the intensity or magnitude of the cause cannot be known in any other manner than by these effects: and, therefore, when we have to assign a measure of the cause, we must take it from the effects produced.

In what manner the effects are to be taken into account, so as to measure the cause for any particular purpose, will have to be further considered; but the axiom, as now stated, is absolutely and universally true, and is acted upon in all parts of our knowledge in which causes are measured.

4. But something further is requisite. We not only consider that all changes of motion in a body have a cause, but that this cause may reside in other bodies. Bodies are conceived to act upon one another, and thus to influence each other's motions, as when one billiard ball strikes another. But when this happens, it is also supposed that the body struck influences the motion of the striking body. This is included in our notion of body or matter. If one ball could strike and

affect the motions of any number of others without having its own motion in any degree affected, the struck balls would be considered, not as bodies, but as mere shapes or appearances. Some reciprocal influence, some resistance, in short some *reaction*, is necessarily involved in our conception of action among bodies. All mechanical action upon matter implies a corresponding reaction; and we might describe matter as that which resists or reacts when acted on by force. Not only must there be a reaction in such cases, but this reaction is defined and determined by the action which produces it, and is of the same kind as the action itself. The action which one body exerts upon another is a blow, or a pressure; but it cannot press or strike without receiving a pressure or a blow in return. And the reciprocal pressure or blow depends upon the direct, and is determined altogether and solely by that. But this action being mutual, and of the same kind on each body, the effect on each body will be determined by the effect on the other, according to the same rule; each effect in turn being considered as action and the other as reaction. But this cannot be otherwise than by the equality and opposite direction of the action and reaction. And since this reasoning applies in all cases in which bodies influence each others motions, we have the following axiom which is universally true, and is a fundamental principle with regard to all mechanical relations.

Axiom III.—*Action is always accompanied by an equal and opposite Reaction.*

5. I now proceed to shew in what manner the Laws of Motion depend upon these three axioms.

Bodies move in lines straight or curved, they move more or less rapidly, and their motions are variously affected by other bodies. This succession of occurrences suggests the conceptions of certain properties or attributes of the motions of bodies, as their direction and velocity, by means of which the laws of such occurrences may be exprest. And these properties or attributes are conceived as belonging to the body at each *point* of its motion, and as changing from one point to another. Thus the body, at each point of its path, moves in a certain direction, and with a certain velocity.

These properties, direction and velocity for instance, are subject to the rule stated in the first axiom: they cannot change without some cause; and when any changes in the motions of a body are seen to depend on its position relative to another body or to any part of space, such other body, or such other part of space, is said to exert a *force* upon the moving body. Also the force exerted upon the moving body is considered to be of a certain value at each point of the body's motion; and though it may change from one point to another, its changes must depend upon the position of the points only, and not upon the velocity and direction of the moving body. For the force which acts upon the body is conceived as a property of the bodies, or points, or lines, or surfaces among which the moving body is placed; the force at all points therefore depends upon the position with regard to the bodies and spaces of which the force is a property; but remains the same, whatever be the circumstances of the body moved. The circumstances of the body moved cannot be a cause which shall change the force acting at any point of space, although they may alter the *effect* which that force produces upon the body. Thus, gravity is the same force at the same point of space, whether it have to act upon a body at rest or in motion; although it still remains to be seen whether it will produce the same effect in the two cases.

6. This being established, we can now see of what nature the laws of motion must be, and can state in a few words the proofs of them. We shall have a law of motion corresponding to each of the above three axioms; the first law will assert that when no force acts, the properties of the motion will be constant; the second law will assert that when a force acts, its quantity is measured by the effect produced; the third law will assert that, when one body acts upon another, there will be a reaction, equal and opposite to the action. And so far as the laws are announced in this form, they will be of absolute and universal truth, and independent of any particular experiment or observation whatever.

But though these laws of motion are necessarily and infallibly true, they are, in the form in which we have stated them, entirely useless and inapplicable. It is impossible to deduce from them any definite and positive conclusions, without some additional knowledge or assumption. This will be clear by stating, as we can now do in a very small compass, the proofs of the laws of motion in the form in which they are employed in mechanical reasonings.

7. First, of the first Law;—that *a body not acted upon by any force will go on in a straight line with an invariable velocity.*

The body will go on in a straight line: for, at any point of its motion, it has a certain direction, which direction will, by Axiom I, continue unchanged, except some cause make it deviate to one side or other of its former position. But any cause which should make the direction deviate towards any part of space would be a force, and the body is not acted upon by any force. Therefore, the direction cannot change, and the body will go on in the same straight line from the first.

The body will move with an invariable velocity. For the velocity at any point will, by Axiom I, continue unchanged, except some cause make it increase or decrease. And since, by supposition, the body is not acted upon by any force, there can be no such cause depending upon position, that is, upon relations of *space;* for any cause of change of motion which has a reference to space is force.

Therefore there can be no cause of change of motion, except there be one depending upon *time*, such, for instance, as would exist if bodies had a natural tendency to move slower and slower, according to a rate depending on the time elapsed.

But if such cause existed, its effects ought to be considered separately; and it would still be requisite to assume the permanence of the same velocity, as the first law of motion; and to obtain, in addition to this, the laws of the retardation depending on the time.

Whether there is any such cause of retardation in the actual motions of bodies, can be known only by a reference to experience; and by such reference it appears that there is no such cause of the diminution of velocity depending on time alone; and therefore that the first law of motion may, in all cases in which bodies are exempt from the action of external forces, be applied without any addition or correction depending upon the time elapsed.

It is not here necessary to explain at any length in what manner we obtain from experience the knowledge of the truth just stated, that there is not in the mere lapse of time any cause of the retardation of moving bodies. The proposition is established by shewing that in all the cases in which such a cause appears to exist, the cause of retardation resides in surrounding bodies and not in time alone, and is therefore an external force. And as this can be shewn in every instance, there remains only the negation of all ground for the assumption of such a cause of retardation. We therefore reject it altogether.

Thus it appears that in proving the first law of motion, we obtain from our conception of cause the conviction that velocity will be uniform except some cause produce a change in it; but that we are compelled to have recourse to experience in order to learn that time alone is not a cause of change of velocity.

8. I now proceed to the second Law:—that *when a force acts upon a body in motion, the effect is the same as that which the same force produces upon a body at rest.*

This law requires some explanation. How is the effect produced upon a moving body to be measured, so that we may compare it with the effect upon a body at rest? The answer to this is, that we here take for the measure of the effect of the force, that motion which must be *compounded* with the motion existing before the change, in order to produce the motion which exists after the change: the rules for the composition of motion being established on independent grounds by the aid of definition alone. Thus if gravity act upon a body which is falling vertically, the effect of gravity upon the body is measured by the velocity *added* to that which the body already has: if gravity act upon a body which is moving horizontally, its effect is measured by the distance to which the body falls below the horizontal line.

The effect of the force which we consider in the second Law of motion, is its effect upon velocity only: and it is proper to mark this restriction by an appropriate term: we shall call this the *accelerative effect* of force; and the cause, as measured by this effect, may be termed the *accelerative quantity* of the force*.

* The accelerative quantity of a force (the *quantitas acceleratrix vis cujusvis* of Newton) is often called *the accelerating force;* and we may thus have to speak of *the accelerating force of a certain force,*

 which

A law of motion which necessarily results from our second Axiom is, that the accelerative quantity of a force is measured by the accelerative effect. But whether the accelerative effect depends upon the velocity and direction of the moving body, cannot be known independently of experience. It is very conceivable, for instance, that the force of gravity being every where the same, shall yet produce, upon falling bodies, a smaller accelerative effect in proportion to the velocity which they already have in a downward direction. Indeed if gravity resembled in its operation the effect of any other mode of mechanical agency, the result would be so. If a body moved downwards in consequence of the action of a hand pushing it with a constant effort, or of a spring, or of a stream of fluid rushing in the same direction, the accelerative effect of such agents would be smaller and smaller as the velocity of the body propelled was larger and larger. We can learn from experience alone that the effects of the action of gravity do not follow the same rule.

We assert that the accelerative quantity of the *same force* of gravity is the same whatever be the motion of the body acted on. It may be asked how we know that the force of gravity *is* the same in cases so compared; for instance, when it acts on a body at rest and in motion? The answer to this question we have given already. By the very process of considering gravity as a force, we consider it as an attribute of something independent of the body acted on. The amount of the force may depend upon place, and even time, for any thing we know *à priori;* but we do not find that the weight of bodies depends on these circumstances, and therefore, having no evidence of a difference in the force of gravity, we suppose it the same at different times and places. And as to the rest, since the force is a force which acts on the body, it is considered as the same force, whatever be the circumstances of the passive body, although the *effects* may vary with these circum-

which is at any rate an awkward phraseology. It would perhaps have been fortunate if Newton, or some other writer of authority, at the time when the principles of mechanics were first clearly developed, had invented an abstract term for this quantity: it might for instance have been called *accelerativity.* And the second law of motion would then have been, that the *accelerativity* of the same force is the same, whatever be the motion of the body acted on.

stances. If the effects are liable to such change, this change must be considered separately, and its laws investigated; but it cannot be allowed to unsettle our assumption of the permanence of the force itself. It is precisely this assumption of a constant cause, which gives us a fixed term, as a means of estimating and expressing by what conditions the effects are regulated.

It appears by observation and experiment, that the accelerative quantity of the same force is not affected by the velocity or direction of the body acted on: for instance, a body falling vertically receives, in any second of time, an accession of velocity as great as that which it received in the first second, notwithstanding the velocity with which it is already moving. The proof of this and similar assertions from experiment produced, historically speaking, the establishment of the second law of motion in the sense in which we now assert it. And here, as in the case of the first law, we may observe that an important portion of the process of proof consisted in shewing that in those cases in which the *accelerative effect* of a force appeared to be changed by the circumstances of the motion of the body acted on, the change was, in fact, due to other external forces; so that all evidence of a cause of change residing in those circumstances was entirely negatived; and thus the law, that the accelerative effect of the same force is the same, appeared to be absolutely and rigorously true.

9. When the motions of bodies are not effected merely by forces like gravity, which are only perceived by their effects, but are acted upon by other bodies, the case requires other considerations.

It is in such cases that we originally form the conception of force; we ourselves pull and push, thrust and throw bodies, with a view, it may be, either to put them in motion, or to prevent their moving, or to alter their figure. Such operations, and the terms by which they are described, are all included in the term *force*, and in other terms of cognate import. And in using this term, we necessarily assume and imply the co-existence of these various effects of force which we have observed universally to accompany each other. Thus the same kind of force which is the cause of motion, may also be the cause of a body having a form different from its natural form; when we draw a bow, the same kind of pull is needed to move the string, and to hold it steady, when the bow is bent. And a weight might be hung to the string, so as to produce either the one or the other of these effects. By an infinite multiplicity of experiments of this kind, we become imbued with the conviction that the same pressure may be the cause of tension and of motion. Also as the cause can be known by its effects only, each of these effects may be taken as its measure; and therefore, so long as one of them is the same, since the cause is the same, the other must be the same also. That is, so long as the pressure or force which shews itself in tension is the same, the motion which it would produce must, under the same circumstances, be the same also. This general fact is not a result of any particular observations, but of the general observation or suggestion arising unavoidably from universal experience, that both tension and motion may be referred to force as their cause, and have no other cause.

We come therefore to this principle with regard to the actions of bodies upon each other, that so long as the tension or pressure is the same, the force, as shewn by its effect in producing motion, must also be the same.

10. This force or action of bodies upon one another, is that which is meant in the Third Axiom, and we now proceed to consider the application of this axiom in mechanics.

Pressures or forces such as I have spoken of, may be employed in producing tension only, and not motion; in this case, each force prevents the motion which would be produced by the others, and the forces are said to balance each other, or to be in equilibrium. The science which treats of such cases is called Statics, and it depends entirely upon the above third axiom, applied to pressures producing rest. It follows from that axiom, that pressures, which acting in opposite directions thus destroy each other's effects, must be equal, each measuring the other. Thus if a man supports a stone in his hand, the force or effort exerted by the man upwards is equal to the weight or force of the stone downwards. And if a second stone, just equal to the first, were supported at the same time in the same hand, the force or effort must be twice as great; for the two stones may be considered as one body of twice the magnitude, and of twice the weight; and therefore the effort which supports it must also be twice as great. And thus we see in what manner statical forces are to be measured in virtue of this third axiom; and no further principle is requisite to enable us to establish the whole doctrine of statics.

11. The third axiom, when applied to the actions of bodies in motion, gives rise to the third law of motion, which we must now consider. Here, as in the cases of the other axioms, we must inquire how we are to measure the quantities to which the axiom applies. What is the measure of the *action* which takes place when a body is put in motion by pressure or force? In order to answer this question, we must consider what circumstances make it requisite that the force should be greater or less. If we have to lift a stone, the force which we exert must be greater when the stone is greater: again, we must exert a greater force to lift it quickly than slowly. It is clear, therefore, that that property of a force with which we are here concerned, and which we may call the *motive quantity* of the force*, increases both when the velocity communicated, and when the mass moved, increase, and depends upon both these quantities, though we have not yet shewn what is the law of this dependence.

The condition that a quantity P shall increase when each of two others V and M does so, may be satisfied in many ways: for instance, by supposing P proportional to the sum $M + V$, (all the quantities being expressed in numbers), or to the product, MV, or to MV^2, or in many other ways.

When, however, the quantities V and M are altogether heterogeneous, as when one is velocity, and the other weight, the first of the above suppositions, that P varies as $M + V$, is inadmissible. For the law of variation of the formula $M + V$ depends upon the relation of the units by which M and V respectively are measured; and as these units are arbitrary in each case, the result is, in like manner, arbitrary, and therefore cannot express a law of nature.

12. The supposition that the motive quantity of a force

* The motive quantity of a force (*vis cujusvis quantitas motrix* of Newton) is sometimes called *moving force;* we are thus led to speak of the moving force of a force, as we have already observed concerning accelerating force. Hence, as in that case, we might employ a single term, as *motivity*, to denote this property of force; and might thus speak of it and of its measures without the awkwardness which arises from the usual phrase.

varies as $M + V$, where M is the mass moved and V the velocity, being thus inadmissible, we have to select upon due grounds, among the other formulæ MV, MV^2, M^2V, &c.

And in the first place I observe that the formula must be proportional to M simply (excluding M^2, &c.) for both the forces which produce motion and the masses in which motion is produced are capable of addition by juxtaposition, and it is easily seen by observation that such addition does not modify the motion of each mass. If a certain pressure upon one brick (as its own weight) cause it to fall with a certain velocity, an equal pressure on another equal brick will cause it also to fall with the same velocity; and these two bricks being placed in contact, may be considered as one mass, which a double force will cause to fall with still the same velocity. And thus all bodies, whatever be their magnitude, will fall with the same velocity by the action of gravity. Those who deny this (as the Aristotelians did) must maintain, that by establishing between two bodies such a contact as makes them one body, we modify the motion which a certain pressure will produce in them. And when we find experimentally (as we do find) that large bodies and small ones fall with the same velocity, excluding the effects of extraneous forces, this result shews that there is not, in the union of small bodies into a larger one, any cause which affects the motion produced in the bodies.

It appears, therefore, that the motive quantity of force which puts a body in motion is, *cæteris paribus*, proportional to the mass of the body; so that for a double mass a double force is requisite, in order that the velocity produced may be the same. Mass considered with reference to this rule, is called *Inertia.*

13. The measure of mass which is used in expressing a law of motion, must be obtained in some way independent of motion, otherwise the law will have no meaning. Therefore, mass measured in order to be considered as *Inertia* must be measured by the statical effects of bodies, for instance, by comparison of weights. Thus two masses are equal which each balance the same weight in the same manner; and a mass is double of one of them which produces the same effect as the two. And we find, by universal observations, that the weight of a mass is not affected by the figure or the arrangement of parts, so long as the matter continues the same. Hence it

appears that the mass of bodies must be compared by comparing their weights, and Inertia is proportional to weight at the same place.

Since all bodies, small or large, light or heavy, fall downwards with equal velocities, when we remove or abstract the effect of extraneous circumstances, the motive quantity of the force of gravity on equal bodies is as their masses; or as their weight, by what has just been said.

14. For the measure of the motive quantity of force, or of the action and reaction of bodies in motion, we have, therefore, now to choose among such expressions as MV, and MV^2. And our choice must be regulated by finding what is the measure which will enable us to assert, in all cases of action between bodies in motion, that action and reaction are equal and opposite.

Now the fact is, that either of the above measures may be taken, and each has been taken by a large body of mathematicians. The former however (MV) has obtained the designation which naturally falls to the lot of such a measure; and is called *momentum*, or sometimes simply *quantity of motion:* the latter quantity (MV^2) is called *vis viva* or *living force*.

I have said that either of these measures may be taken: the former must be the measure of action, if we are to measure it by the effect produced *in a given time;* the latter is the measure if we take the *whole* effect produced. In either way the third law of motion would be true.

Thus if a ball B, lying on a smooth table, be drawn along by a weight A hanging by a thread over the edge of the table, the motion of B is produced by the action of A, and on the other hand the motion of A is diminished by the reaction of B; and the equality of action and reaction here consists in this, that the momentum (MV) which B acquires in any time is equal to that which A loses: that is, so much is taken from the momentum which A would have had, if it had fallen freely *in the same time;* so that A falls more slowly by just so much.

But if the weight A fall through a given space from rest, as 1 foot, and then cease to act, the equality of action and reaction consists in this, that the *vis viva* which B acquires on the whole, is equal to the *vis viva* which A loses; that is, the *vis viva* of A thus acting on B is smaller by so much than it would have been, if A had fallen freely *through the same space*.

15. In fact, these two propositions are necessarily connected, and one of them may be deduced from the other. The former way of stating the third law of motion appears, however, to be the simplest mode of treating the subject, and we may put the third law of motion in this form.

In the direct mutual action of bodies, the momentum gained and lost in any time are equal.

This law depends upon experiment, and is perhaps best proved by some of its consequences. It follows from the law so stated, that the motive quantity of a force is proportional to the momentum generated in a given time; since the motive quantity of force is to be equivalent to that action and reaction which is understood in the third law of motion. Now, if the pressure arising from the weight of a body P produce motion in a mass Q, since the momentum gained by Q and that lost by P in any time are equal, the momentum of the whole at any time will be the same as if P's weight had been employed in moving P alone. Therefore, the velocity of the mass Q will be less, in the same proportion in which the mass or inertia is greater: and thus the accelerating quantity of the force is inversely proportioned to the mass moved. This rule enables us to find the accelerative quantity of the force in various cases, as for instance, when bodies oscillate, or when a smaller weight moves a large mass; and we can hence calculate the circumstances of the motion, which are found to agree with the consequences of the above law.

16. But the argument may be reduced to a simpler form. Our object is to shew that, for an equal mass, the velocity produced by a force acting for a given time is as the pressure which produces the motion; for instance, that a double pressure will produce a double velocity. Now a double pressure may be considered as the union of two equal pressures, and if these two act *successively*, the first will communicate to the body a certain velocity, and the second will communicate an additional velocity, equal to the first, by the second law of motion; so that the whole velocity thus communicated will be the double of the first. Therefore, if the velocity communicated be not also the double of the first when the two pressures act *together*, the difference must arise from this, that the effect of one force is modified by the simultaneous action of the other. And when we find by experience (as we do find) that there is no such

difference, but that the velocity communicated in a given time is as the pressure which communicates it, this result shews that there is nothing in the circumstance of a body being already acted on by one pressure, which modifies the effect of an additional pressure acting along with the first.

17. I have above asserted the law, of the *direct* action of bodies only. But it is also true when the action is indirect, as when by turning a winch we move a wheel, the main mass of which is farther from the axis than the handle of the winch. In this case the pressure we exert acts at a mechanical disadvantage on the main mass of the wheel, and we may ask whether this circumstance introduces any new law of motion. And to this we may reply, that we can *conceive* pressure to produce different effects in moving bodies, according as it is exerted directly or by the intervention of machines; but that we *find* no reason to believe that such a difference exists. The relations of the pressures in different parts of a machine are determined by considering the machine at rest. But if we suppose it to be put in motion by such pressures, we see no reason to expect that these pressures should have a different relation to the motions produced from what they would have done if they were direct pressures. And as we find in experiment a negation of all evidence of such a difference, we reject the supposition altogether. We assert, therefore, the third law of motion to be true, whatever be the mechanism by the intervention of which action and reaction are opposed to each other.

From this consideration it is easy to deduce the following rule, which is known by the designation of D'Alembert's principle, and may be considered as a fourth law of motion.

When any forces produce motion in any connected system of matter, the motive quantities of force gained and lost by the different parts must balance each other according to the connexion of the system.

By the motive quantity of force *gained* by any body, is here meant the quantity by which that motive force which the body's motion implies (according to the measures already established) exceeds the quantity of motive force which acts immediately upon the body. It is the excess of the *effective* above the *impressed* force, and of course arises from the force transmitted from the other bodies of the system in consequence of the connexion of the parts. The motive quantity of force *lost* is in like manner the excess of the impressed above the effective force. And these two excesses, in different parts of the system, must balance each other according to the mechanical advantage or disadvantage at which they act for each part.

This completes our system of mechanical principles, and authorizes us to extend to bodies of any size and form the rules which the second law of motion gives for the motion of bodies considered as points. And by thus enabling us to trace what the motions of bodies will be according to the rule asserted in the third law of motion, (namely, that the motive quantity of forces is as the momentum produced in a given time,) it leads us to verify that supposition by experiments in which bodies oscillate or revolve or move in any regular and measurable manner, as has been done by Atwood, Smeaton, and many others.

18. We have thus a complete view of the nature and extent of the fundamental principles of mechanics; and we now see the reason why the laws of motion are so many and no more, in what way they are independent of experience, and in what way they depend upon experiment. The form, and even the language of these laws is of necessity what it is; but the interpretation and application of them is not possible without reference to fact. We may imagine many rules according to which bodies might move (for many sets of rules, different from the existing ones, are, so far as we can see, possible) and we should still have to assert—that velocity could not change without a cause,—that change of action is proportional to the force which produces it,—and that action and reaction are equal and opposite. The truth of these assertions is involved in those notions of causation and matter, which the very attempt to know any thing concerning the relations of matter and motion presupposes. But, according to the facts which we might find, in such imaginary cases as I have spoken of, we should settle in a different way—what is a cause of change of velocity,—what is the measure of the force which changes motion,—and what is the measure of action between bodies. The law is necessary, if there is to be a law; the meaning of its terms is decided by what we find, and is therefore regulated by our special experience.

19. It may further illustrate this matter to point out that this view is confirmed by the history of mathematics. The laws of motion were assented to as soon as propounded; but were

yet each in its turn the subject of strenuous controversy. The terms of the law, the form, which is necessarily true, were recognized and undisputed; but the meaning of the terms, the substance of the law, was loudly contested; and though men often tried to decide the disputed points by pure reasoning, it was easily seen that this could not suffice; and that since it was a case where experience *could* decide, experience *must* be the proper test: since the matter came within her jurisdiction, her authority was single and supreme.

Thus with regard to the first law of motion, Aristotle allowed that *natural* motions continue unchanged, though he asserted the motions of terrestrial bodies to be *constrained* motions, and therefore, liable to diminution. Whether this was the cause of their diminution was a question of fact, which was, by examination of facts, decided against Aristotle. In like manner, in the first case of the second law of motion which came under consideration, both Galileo and his opponent agree that falling bodies are *uniformly* accelerated; that is, that the force of gravity accelerates a body uniformly whatever be the velocity it has already; but the question arises, what is uniform acceleration? It so happened in this case, that the first conjecture of Galileo, afterwards defended by Casræus, (that the velocity was proportional to the space from the beginning of the motion,) was not only contradictory to fact, but involved a self-contradiction; and was, therefore, easily disposed of. But this accident did not supersede the necessity of Galileo and his pupils verifying their assertion by reference to experiment, since there were many suppositions which were different from theirs, and still possible, though that of Casræus was not.

The mistake of Aristotle and his followers, in maintaining that large bodies fall more quickly than small ones, in exact proportion to their weight, arose from perceiving half of the third law of motion, that the velocity increases with the force which produces it; and from overlooking the remaining half, that a greater force is required for the same velocity, according as the mass is larger. The ancients never attained to any conception of the force which moves and the body which is moved, as distinct elements to be considered when we enquire into the subject of motion, and therefore could not even propose to themselves in a clear manner the questions which the third law of motion answered.

But, when, in more modern times, this distinction was brought into view, the progress of opinion in this case was nearly the same as with regard to the other laws.

It was allowed at once, and by all, that action and reaction are equal; but the controversy concerning the sense in which this law is to be interpreted, was one of the longest and fiercest in the history of mathematics, and the din of the war has hardly yet died away. The disputes concerning the measure of the force of bodies in motion, or the *vis viva*, were in fact a dispute which of two measures of action that I have mentioned above should be taken; the effect in a given time, or the whole effect: in the one case the *momentum* (MV), in the other the *vis viva* (MV^2), was the proper measure.

20. It may be observed that the word *momentum*, which one party appropriated to their views, was employed to designate the motive quantity of force, or the action of bodies in motion, before it was determined what the true measure of such action was. Thus Galileo, in his "Discorso intorno alle cose che stanno in su l'Acqua," says, that momentum "is the force, efficacy, or virtue with which the motion moves and the body moved resists; depending not on weight only, but on the velocity, inclination, and any other cause of such virtue."

The adoption of the phrase *vis viva* is another instance of the extent to which men are tenacious of those terms which carry along with their use a reference to the fundamental laws of our thought on such matters. The party which used this phrase maintained that the mass multiplied into the square of the velocity was the proper measure of the force of bodies in motion; but finding the term *moving force* appropriated by their opponents, they still took the same term *force*, with the peculiar distinction of its being *living* force, in opposition to *dead* force or pressure, which they allowed to be rightly measured by the momentum generated in a given time. The same tendency to adopt, in a limited and technical sense, the words of most general and fundamental use in the subject, has led some writers (Newton for instance,) to employ the term *motion* or *quantity of motion* as synonymous with momentum, or the product of the numbers which express the mass and the velocity. And this use being established, the quantities of motion gained and lost are always equal and opposite; and, therefore the quantity which exists in any given direction cannot be increased or diminished by any mutual action of bodies. Thus we are led to the assertion which has already been noticed, that the quantity of motion in the world is always the same. And we now see how far the necessary truth of this proposition can be asserted. The proposition is necessarily true according to our notions of material causation; but the measure of "quantity of motion," which is a condition of its truth, is inevitably obtained from experience.

21. It is not surprizing that there should have been a good deal of confusion and difference of opinion on these matters: for it appears that there is, in the intellectual constitution and faculties of man, a source of self-delusion in such reasonings. The actual rules of the motion and mutual action of bodies are, and must be, obtained from observation of the external world: but there is a constant wish and propensity to express these rules in such terms as shall make them appear self-evident, because identical with the universal and necessary rules of causation. And this propensity is essential to the progress of our knowledge; and in the success of this effort consists, in a great measure, the advance of the science to its highest point of simplicity and generality.

22. The nature of the truth which belongs to the laws of motion will perhaps appear still more clearly, if we state, in the following tabular form, the analysis of each law into the part which is necessary, and the part which is empirical.

	Necessary.	*Empirical.*
First Law.	Velocity does not change without a cause.	The time for which a body has already been in motion is not a cause of change of velocity.
Second Law.	The accelerating quantity of a force is measured by the acceleration produced.	The velocity and direction of the motion which a body already possesses are not, either of them, causes which change the acceleration produced.
Third Law.	Reaction is equal and opposite to action.	The connexion of the parts of a body, or of a system of bodies, and the action to which the body or system is already subject, are not, either of them, causes which change the effects of any additional action.

Of course, it will be understood that, when we assert that the connexion of the parts of a system does not change the effect of any action upon it, we mean that this connexion does not introduce any *new* cause of change, but leaves the effect to be determined by the previously established rules of equilibrium and motion. The connexion will modify the application of such rules; but it introduces no additional rule: and the same observation applies to all the above stated empirical propositions.

This being understood, it will be observed that the part of each law which is here stated as empirical, consists, in each case, of a negation of the supposition that the condition of the moving body with respect to motion and action, is a cause of any change in the circumstances of its motion; and from this it follows that these circumstances are determined entirely by the forces extraneous to the body itself.

23. This mode of considering the question shows us in what manner the laws of motion may be said to be proved by their simplicity, which is sometimes urged as a proof. They undoubtedly have this distinction of the greatest possible simplicity, for they consist in the negation of all causes of change, except those which are essential to our conception of such causation. We may conceive the motions of bodies, and the effect of forces upon them, to be regulated by the lapse of time, by the motion which the bodies have, by the forces previously acting; but though we may imagine this as possible, we do not find that it is so in reality. If it were, we should have to consider the effect of these conditions of the body acted on, and to combine this effect with that of the acting forces; and thus the motion would be determined by more numerous conditions and more complex rules than those which are found to be the laws of nature. The laws which, in reality, govern motion are the fewest and simplest possible, because all are excluded, except those which the very nature of laws of motion necessarily implies. The prerogative of simplicity is possessed by the actual laws of the universe, in the highest perfection which is imaginable or possible. Instead of having to take into account all the circumstances of the moving bodies, we find that we have only to reject all these circumstances. Instead of having to combine empirical with necessary laws, we learn empirically that the necessary laws are entirely sufficient.

24. Since all that we learn from experience is, that she

has nothing to teach us concerning the laws of motion, it is very natural that some persons should imagine that experience is not necessary to their proof. And accordingly many writers have undertaken to establish all the fundamental principles of mechanics by reasoning alone. This has been done in two ways:—sometimes by attending only to the necessary part of each law (as the parts are stated in the last paragraph but one) and by overlooking the necessity of the empirical supplement and limitation to it;—at other times by asserting the part which I have stated as empirical to be self-evident, no less than the other part. The former way of proceeding may be found in many English writers on the subject; the latter appears to direct the reasonings of many eminent French mathematicians. Some (as Laplace) have allowed the empirical nature of two out of the three laws; others, as M. Poisson, have considered the first as alone empirical; and others, as D'Alembert, have assumed the self-evidence of all the three independently of any reference whatever to observation.

25. The parts of the laws which I have stated as empirical, appear to me to be clearly of a different nature, as to the cogency of their truth, from the parts which are necessary; and this difference is, I think, established by the fact that these propositions were denied, contested, and modified, before they were finally established. If these truths could not be denied without a self-contradiction, it is difficult to understand how they could be (as they were) long and obstinately controverted by mathematicians and others fully sensible to the cogency of necessary truth.

I will not however go so far as to assert that there may not be some point of view in which that which I have called the empirical part of these laws, (which, as we have seen, contains negatives only,) may be properly said to be self-evident. But however this may be, I think it can hardly be denied that there is a difference of a fundamental kind in the nature of these truths,—which we can, in our imagination at least, contradict and replace by others, and which, historically speaking, have been established by experiment;—and those other truths, which have been assented to from the first, and by all, and which we cannot deny without a contradiction in terms, or reject without putting an end to all use of our reason on this subject.

26. On the other hand, if any one should be disposed to maintain that, inasmuch as the laws are interpreted by the aid of experience only, they must be considered as entirely empirical laws, I should not assert this to be placing the science of mechanics on a wrong basis. But at the same time I would observe, that the form of these laws is not empirical, and would be the same if the results of experience should differ from the actual results. The laws may be considered as a formula derived from *à priori* reasonings, where experience assigns the value of the terms which enter into the formula.

Finally, it may be observed, that if any one can convince himself that matter is either necessarily and by its own nature determined to move slower and slower, or necessarily and by its own nature determined to move uniformly, he must adopt the latter opinion, not only of the truth, but of the necessity of the truth of the first law of motion, since the former branch of the alternative is certainly false: and similar assertions may be made with regard to the other laws of motion.

27. This inquiry into the nature of the laws of motion, will, I hope, possess some interest for those who attach any importance to the logic and philosophy of science. The discussion may be said to be rather metaphysical than mechanical; but the views which I have endeavoured to present, appear to explain the occurrence and result of the principal controversies which the history of this science exhibits; and, if they are well founded, ought to govern the way in which the principles of the science are treated of, whether the treatise be intended for the mathematical student or the philosopher.

ESSAY II.

REMARKS ON MATHEMATICAL REASONING AND ON THE LOGIC OF INDUCTION*.

Sect. I.—*On the Grounds of Mathematical Reasoning.*

1. The study of a science, treated according to a rigorous system of mathematical reasoning, is useful, not only on account of the positive knowledge which may be acquired on the subjects which belong to the science, but also on account of the collateral effects and general bearings of such a study, as a discipline of the mind and an illustration of philosophical principles.

Considering the study of the mathematical sciences with reference to these latter objects, we may note two ways in which it may promote them;—by habituating the mind to strict reasoning,—and by affording an occasion of contemplating some of the most important mental processes and some of the most distinct forms of truth. Thus mathematical studies may be useful in teaching practical logic and theoretical metaphysics. We shall make a few remarks on each of these topics.

2. The study of Mathematics teaches strict reasoning—by bringing under the student's notice prominent and clear examples of trains of demonstration:—by exercising him in the habits of attentive and connected thought which are requisite in order to follow these trains;—and by familiarizing him with the peculiar and distinctive conviction which demonstration produces, and with the rigorous exclusion of all considerations which do not enter into the demonstration.

3. Logic is a system of doctrine which lays down rules for determining in what cases pretended reasonings are and are not demonstrative. And accordingly, the teaching of strict reasoning by means of the study of logic is often recommended and practised. But in order to show the superiority of the study of mathematics for this purpose, we may consider,—that reasoning, as a practical process, must be learnt by practice, in the same manner as any other practical art, for example, riding,

* From the *Mechanical Euclid*, 1837.

or fencing;—that we are not secured from committing fallacies by such a classification of fallacies as logic supplies, as a rider would not be secured from falls by a classification of them;—and that the habit of attending to our mental processes while we are reasoning, rather interferes with than assists our reasoning well, as the horseman would ride worse rather than better, if he were to fix his attention upon his muscles when he is using them.

4. To this it may be added, that the peculiar habits which enable any one to follow a *chain* of reasoning are excellently taught by mathematical study, and are hardly at all taught by logic. These habits consist in not only apprehending distinctly the demonstration of a proposition when it is proved, but in retaining all the propositions thus proved, and using them in the ulterior steps of the argument with the same clear conviction, readiness, and familiarity, as if they were self-evident principles. Writers on Logic seldom give examples of reasoning in which several syllogisms follow each other; and they never give examples in which this progressive reasoning is so exemplified as to make the process familiar. Their chains generally consist only of two or three links. In mathematics, on the contrary, every theorem is an example of such a chain: every proof consists of a series of assertions, of which each depends on the preceding, but of which the last inferences are no less evident or less easily applied than the simplest first principles. The language contains a constant succession of short and rapid references to what has been proved already; and it is justly assumed that each of these brief movements helps the reasoner forwards in a course of infallible certainty and security. Each of these hasty glances must possess the clearness of intuitive evidence, and the certainty of mature reflection; and yet must leave the reasoner's mind entirely free to turn instantly to the next point of his progress. The faculty of performing such mental processes well and readily is of great value, and is in no way fostered by the study of logic.

5. It is sometimes objected to the study of Mathematics as a discipline of reasoning, that it tends to render men insensible to all reasoning which is not mathematical, and leads them to demand, in other subjects, proofs such as the subject does not admit of, or such as are not appropriate to the matter.

To this it may be replied, that these evil results, so far as they occur, arise either from the student pursuing too exclusively one particular line of mathematical study, or from erroneous notions of the nature of demonstration.

The present volume is intended to assist, in some measure, in remedying the too exclusive pursuit of one particular line of Mathematics, by shewing that the same simplicity and evidence which are seen in the Elements of Geometry may be introduced into the treatment of another subject of a kind very different; and it is hoped that we may thus bring the subject within the reach of those who cultivate the study of Mathematics as a discipline only. The remarks now offered to the reader are intended to aid him in forming a just judgment of the analogy between mathematical and other proof; which is to be done by pointing out the true grounds of the evidence of Geometry, and by exhibiting the views which are suggested by the extension of mathematical reasoning to sciences concerned about physical facts.

6. We shall therefore now proceed to make some remarks on the nature and principles of reasoning, especially as far as they are illustrated by the mathematical sciences.

Some of the leading principles which bear upon this subject are brought into view by the consideration of the question, "What is the foundation of the certainty arising from mathematical demonstration?" and in this question it is implied that mathematical demonstration is recognized as a kind of reasoning, possessing a peculiar character and evidence, which make it a definite and instructive subject of consideration.

7. Perhaps the most obvious answer to the question respecting the conclusiveness of mathematical demonstration is this;—that the certainty of such demonstration arises from its being founded upon *Axioms;* and conducted by steps, of which each might, if required, be stated as a rigorous *Syllogism.*

This answer might give rise to the further questions, What is the foundation of the conclusiveness of a Syllogism? and, What is the foundation of the certainty of an Axiom? And if we suppose the former inquiry to be left to Logic, as being the subject of that science, the latter question still remains to be considered. We may also remark upon this answer, that mathematical demonstration appears to depend upon Definitions, at least as much as upon Axioms. And thus we are led to these questions:—Whether mathematical demonstration is founded upon Definitions, or upon Axioms, or upon both? and, What is the real nature of Definitions and of Axioms?

8. The question, What is the foundation of mathematical demonstration? was discussed at considerable length by Dugald Stewart*; and the opinion at which he arrived was, that the certainty of mathematical reasoning arises from its depending upon *definitions*. He expresses this further, by declaring that mathematical truth is hypothetical, and must be understood as asserting only, that *if* the definitions are assumed, the conclusion follows. The same opinion has, I think, prevailed widely among other modern speculators on the same subject, especially among mathematicians themselves.

9. In opposition to this opinion, I urge, in the first place that no one has yet been able to construct a system of mathematical truth by means of definitions alone, to the exclusion of axioms; although attempts having this tendency have been made constantly and earnestly. It is, for instance, well known to most readers, that many mathematicians have endeavoured to get rid of Euclid's "Axioms" respecting straight lines and parallel lines; but that none of these essays has been generally considered satisfactory. If these axioms could be superseded, by definition or otherwise, it was conceived that the whole structure of Elementary Geometry would rest merely upon definitions; and it was held by those who made such essays, that this would render the science more pure, simple, and homogeneous. If these attempts had succeeded, Stewart's doctrine might have required a further consideration; but it appears strange to assert that Geometry is supported by definitions, and not by axioms, when she cannot stir four steps without resting her foot upon an axiom.

10. But let us consider further the nature of these attempts to supersede the axioms above mentioned. They have usually consisted in endeavours so to frame the definitions, that these might hold the place which the axioms hold in Euclid's reasoning. Thus the axiom, that "two straight lines cannot enclose a space," would be superfluous, if we were to take the following definition:—"A line is said to be *straight*, when two such lines cannot coincide in *two* points without coinciding *altogether*."

But when such a method of treating the subject is proposed,

* *Elements of the Philosophy of the Human Mind*, Vol. II.

we are unavoidably led to ask,—whether it is allowable to lay down such a definition. It cannot be maintained that we may propound any form of words whatever as a definition, without any consideration whether or not it suggests to the mind any intelligible or possible conception. What would be said, for instance, if we were to state the following as a definition, "A line is said to be *straight* (or any other term) when two such lines cannot coincide in *one* point without coinciding altogether?" It would inevitably be remarked, that no such lines exist; or that such a property of lines cannot hold good without other conditions than those which this definition expresses; or, more generally, that the definition does not correspond to any conception which we can call up in our minds, and therefore can be of no use in our reasonings. And thus it would appear, that a definition, to be admissible, must necessarily refer to and agree with some conception which we can distinctly frame in our thoughts.

11. This is obvious, also, by considering that the definition of a straight line could not be of any use, except we were entitled to apply it in the cases to which our geometrical propositions refer. No definitions of straight lines could be employed in Geometry, unless it were in some way certain that the lines so defined are those by which angles are contained, those by which triangles are bounded, those of which parallelism may be predicated, and the like.

12. The same necessity for some general conception of such lines accompanying the definition, is implied in the terms of the definition above suggested. For what is there meant by "*such* lines?" Apparently, lines having some general character in which the property is necessarily involved. But how does it appear that lines may have such a character? And if it be self-evident that there may be such lines, this evidence is a necessary condition of this (or any equivalent) definition. And since this self-evident truth is the ground on which the course of reasoning must proceed, the simple and obvious method is, to state the property *as* a self-evident truth; that is, as an axiom. Similar remarks would apply to the other axiom above mentioned; and to any others which could be proposed on any subject of rigorous demonstration.

13. If it be conceded that such a conception accompanying the definition is necessary to justify it, we shall have made a

step in our investigation of the grounds of mathematical evidence. But such an admission does not appear to be commonly contemplated by those who maintain that the conclusiveness of mathematical proof results from its depending on definitions. They generally appear to understand their tenet as if it implied *arbitrary* definitions. And something like this seems to be held by Stewart, when he says that mathematical truths are true *hypothetically*. For we understand by an hypothesis a supposition, not only which we may make, but may abstain from making, or may replace by a different supposition.

14. That the fundamental conceptions of Geometry are not arbitrary definitions, or selected hypotheses, will, I think, be clear to any one who reasons geometrically at all. It is impossible to follow the steps of any single proposition of Geometry without conceiving a straight line and its properties, whether or not such a line be defined, and whether or not its properties be stated. That a straight line should be distinguished from all other lines, and that the axiom respecting it should be seen to be true, are circumstances indispensable to any clear thought on the subject of lines. Nor would it be possible to frame any coherent scheme of Geometry in which straight lines should be excluded, or their properties changed. Any one who should make the attempt, would betray, in his first propositions, to all men who can reason geometrically, a reference to straight lines.

15. If, therefore, we say that Geometry depends on definitions, we must add, that they are *necessary*, not arbitrary definitions,—such definitions as we must have in our minds, so far as we have elements of reasoning at all. And the elementary hypotheses of Geometry, if they are to be so termed, are not hypotheses which are requisite to enable us to reach this or that conclusion; but hypotheses which are requisite for *any* exercise of our thoughts on such subjects.

16. Before I notice the bearing of this remark on the question of the necessity of axioms, I may observe that Stewart's disposition to consider definitions, and not axioms, as the true foundation of Geometry, appears to have resulted, in part, from an arbitrary selection of certain axioms, as specimens of all. He takes, as his examples, the axioms, "that if equals be added to equals the wholes are equal," that "the whole is greater than its part;" and the like. If he had, instead of these, considered

the more properly geometrical axioms,—such as those which I have mentioned; "that two straight lines cannot enclose a space;" or any of the axioms which have been made the basis of the doctrine of parallels; for instance, Playfair's axiom, "that two straight lines which intersect each other cannot both of them be parallel to a third straight line;"—it would have been impossible for him to have considered axioms as holding a different place from definitions in geometrical reasoning. For the properties of triangles are proved from the axiom respecting straight lines, as distinctly and directly, as the properties of angles are proved from the definition of a right angle. Of the many attempts made to prove the doctrine of parallels, almost all professedly, all really, assume some axiom or axioms which are the basis of the reasoning.

17. It is therefore very surprizing that Stewart should so exclusively have fixed his attention upon the more general axioms, as to assert, following Locke, "that from [mathematical] axioms it is not possible for human ingenuity to draw a single inference*;" and even to make this the ground of a contrast between geometrical axioms and definitions. The slightest examination of any treatise of Geometry might have shown him that there is no sense in which this can be asserted of axioms, in which it is not equally true of definitions; or rather, that while Euclid's definition of a straight line leads to no truth whatever, his axiom respecting straight lines is the foundation of the whole of Geometry; and that, though we can draw some inferences from the definition of parallel straight lines, we strive in vain to complete the geometrical doctrine of such lines, without assuming some axiom which enables us to prove the converse of our first propositions. Thus, that which Stewart proposes as the distinctive character of axioms, fails altogether; and with it, as I conceive, the whole of his doctrine respecting mathematical evidence.

18. That Geometry (and other sciences when treated in a method equally rigorous) depends upon axioms as well as definitions, is supposed by the form in which it is commonly presented. And after what we have said, we shall assume this form to be a just representation of the real foundations of such sciences, till we can find a tenable distinction between axioms and definitions, in their nature, and in their use; and till we

* *Elements of the Philosophy of the Human Mind*, Vol. II. p. 38.

have before us a satisfactory system of Geometry without axioms. And this system, we may remark, ought to include the Higher as well as the Elementary Geometry, before it can be held to prove that axioms are needless; for it will hardly be maintained, that the properties of circles depend upon definitions and hypotheses only, while those of ellipses require some additional foundation; or that the comparison of curve lines requires axioms, while the relations of straight lines are independent of such principles.

19. Having then, I trust, cleared away the assertion, that mathematical reasoning rests ultimately upon definitions only, and that this is the ground of its peculiar cogency, I have to examine the real evidence of the truth of such axioms as are employed in the exact Mathematical Sciences. And we are, I think, already brought within view of the answer to this question. For if the definitions of Mathematics are not arbitrary, but necessary, and must, in order to be applicable in reasoning, be accompanied by a conception of the mind through which this necessity is seen; it is clear, that this apprehension of the necessity of the properties which we contemplate, is really the ground of our reasonings and the source of their irresistible evidence. And where we clearly apprehend such necessary relations, it can make no difference whatever in the nature of our reasoning, whether we express them by means of definitions or of axioms. We define a straight line vaguely;—that it is that line which lies evenly between two points: but we forthwith remedy this vagueness, by the axiom respecting straight lines: and thus we express our conception of a straight line, so far as is necessary for reasoning upon it. We might, in like manner, begin by defining a right angle to be the angle made by a line which stands evenly between the two portions of another line; and we might add an axiom, that all right angles are equal. Instead of this, we define a right angle to be that which a line makes with another when the two angles on the two sides of it are equal. But in all these cases, we express our conception of a necessary relation of lines; and whether this be done in the form of definitions or axioms, is a matter of no importance.

20. But it may be asked, If it be thus unimportant whether we state our fundamental principles as axioms or definitions, why not reduce them all to definitions, and thus give to

our system that aspect of independence which many would admire, and with which none need be displeased? And to this we answer, that if such a mode of treating the subject were attempted, our definitions would be so complex, and so obviously dependent on something not expressed, that they would be admired by none. We should have to put into each definition, as conditions, all the axioms which refer to the things defined. For instance, who would think it a gain to escape the difficulties of the doctrine of parallels by such a definition as this: "Parallel straight lines are those which being produced indefinitely both ways do not meet; and which are such that if a straight line intersects one of them it must somewhere meet the other?" And in other cases, the accumulation of necessary properties would be still more cumbersome and more manifestly heterogeneous.

21. The reason of this difficulty is, that our fundamental conception of lines and other relations of space, are capable of being contemplated under several various aspects, and more than one of these aspects are needed in our reasonings. We may take one such aspect of the conception for a definition; and then we must introduce the others by means of axioms. We may define parallels by their not meeting; but we must have some positive property, besides this negative one, in order to complete our reasonings respecting such lines. We have, in fact, our choice of several such self-evident properties, any of which we may employ for our purpose, as geometers well know; but with our naked definition, as they also know, we cannot proceed to the end. And in other cases, in like manner, our fundamental conception gives rise to various elementary truths, the connexion of which is the basis of our reasonings: but this connexion resides in our thoughts, and cannot be made to follow, as a logical result, from any assumed form of words, presented as a definition.

22. If it be further demanded, What is the nature of this bond in our thoughts by which various properties of lines are connected? perhaps the simplest answer is to say, that it resides in *the idea of space*. We cannot conceive things in space without being led to consider them as determined and related in some way or other to straight lines, right angles, and the like; and we cannot contemplate these determinations and relations distinctly, without assuming those properties of straight lines,

of right angles, and of the rest, which are the basis of our Geometry. We cannot conceive or perceive objects at all, except as existing in space; we cannot contemplate them geometrically, without conceiving them in space which is subjected to geometrical conditions; and this mode of contemplation is, by language, analysed into definitions, axioms, or both.

23. The truths thus seen and known, may be said to be known by *intuition*. In English writers this term has, of late, been vaguely used, to express all convictions which are arrived at without conscious reasoning, whether referring to relations among our perceptions, or to conceptions of the most derivative and complex nature. But if we were allowed to restrict the use of this term, we might conveniently confine it to those cases in which we necessarily apprehend relations of things truly, as soon as we conceive the objects distinctly. In this senso axioms may be said to be known *by intuition;* but this phraseology is not essential to our purpose.

24. It appears, then, that the evidence of the axioms of Geometry depends upon a distinct possession of the idea of space. These axioms are stated in the beginning of our Treatises, not as something which the reader is to learn, but as something which he already knows. No proof is offered of them; for they are the beginning, not the end of demonstration. The student's clear apprehension of the truth of these, is a condition of the possibility of his pursuing the reasonings on which he is invited to enter*. Without this mental capacity,

* In this statement respecting the nature of Axioms, I find myself agreeing with the acute author of "*Sematology*." See the "*Sequel to Sematology*," p. 103. "An Axiom does not account for an intellection; it does but describe the requisite competency for it." It appears to me that this view is not familiar among English metaphysicians. I may here quote what I said at a former period, "However we may *define* force, it is necessary in order to understand the elementary reasonings of this portion of science, that we should *conceive* it distinctly. Do we wish for a test of the distinctness of our conceptions? The test is, our being able to see the necessary truth of the Axioms on which our reasonings rest...These principles (the Axioms of Statics) are all perfectly evident as soon as we have formed the general conception of pressure; but without that act of thought, they can have no evidence whatever given them by any form of words, or reference to other truths;—by definitions, or by illustrations from other kinds of quantity."—*Thoughts on the Study of Mathematics*, p. 25.

and the power of referring to it, in the reader, the writer's assertions and arguments are empty and unmeaning words; but then, this capacity and power are what all rational creatures alike possess, though habit may have developed it in very various degrees in different persons.

25. It has been common in the school of metaphysicians of which I have spoken, to describe some of the elementary convictions of our minds as *fundamental laws* of belief; and it appears to have been considered that this might be taken as a final and sufficient account of such convictions. I do not know whether any persons would be tempted to apply this formula, as a solution of our question respecting the nature of axioms. If this were proposed, I should observe, that this form of expression seems to me, in such a case, highly unsatisfactory. For *laws* require and enjoin a conjunction of things which can be contemplated separately, and which would be disjoined if the law did not exist. It is a law of nature that terrestrial bodies, when free, fall downwards; for we can easily conceive such bodies divested of such a property. But we cannot say, in the same sense, that the impossibility of two straight lines inclosing a space arises from a law; for if they are straight lines, they need no law to compel this result. We cannot conceive straight lines exempt from such a law. To speak of this property as imposed by a law, is to convey an inadequate and erroneous notion of the close necessity, inviolable even in thought, by which the truth clings to the conception of the lines.

26. This expression, of "laws of belief," appears to have found favour, on this account among others, that it recognized a kind of analogy between the grounds of our reasoning on very abstract subjects, and the principles to which we have recourse in other cases when we manifestly derive our fundamental truths from facts, and when it is supposed to be the ultimate and satisfactory account of them to say, that they are laws of nature learnt by observation. But such an analogy can hardly be considered as a real recommendation by the metaphysician; since it consists in taking a case in which our knowledge is obviously imperfect and its grounds obscure, and in erecting this case into an authority which shall direct the process and control the enquiry of a much more profound and penetrating kind of speculation. It cannot be doubted that we are likely to see the true grounds and evidence of our doctrines much more clearly in the

case of Geometry and other rigorous systems of reasoning, than in collections of mere empirical knowledge, or of what is supposed to be such. It is both an unphilosophical and an indolent proceeding, to take the latter cases as a standard for the former.

27. I shall therefore consider it as established, that in Geometry our reasoning depends upon axioms as well as definitions,—that the evidence of the truth of the axioms and of the propriety of the definitions resides in the idea of space,—and that the distinct possession of this idea, and the consequent apprehension of the truth of the axioms which are its various aspects, is supposed in the student who is to pursue the path of geometrical reasoning. This being understood, I have little further to observe on the subject of Geometry. I will only remark—that all the conclusions which occur in the science follow purely from those first principles of which we have spoken; —that each proposition is rigorously proved from those which have been proved previously from such principles;—that this process of successive proof is termed *Deduction*;—and that the rules which secure the rigorous conclusiveness of each step are the rules of *Logic*, which I need not here dwell upon.

28. But I now proceed to consider some other questions to which our examination of the evidence of Geometry was intended to be preparatory;—How far do the statements hitherto made apply to other sciences? for instance, to such sciences as are treated of in the present volume, Mechanics and Hydrostatics? To this I reply, that some such sciences at least, as for example Statics, appear to me to rest on foundations exactly similar to Geometry:—that is to say, that they depend upon axioms,—self-evident principles, not derived in any immediate manner from experiment, but involved in the very nature of the conceptions which we must possess, in order to reason upon such subjects at all. The proof of this doctrine must consist of several steps, which I shall take in order.

29. In the first place, I say that the axioms of Statics are *self-evidently true*. In the beginning of the Treatise I have stated these barely as axioms, without addition or explanation, as the axioms of Geometry are stated in treatises on that subject. And such is the proper and orderly mode of exhibiting axioms; for, as has been said, they are to be understood as an expression of the condition of conception of the student. They are not to be learnt from without, but from

within. They necessarily and immediately flow from the distinct possession of that idea, which if the student do not possess distinctly, all conclusive reasoning on the subject under notice is impossible. It is not the business of the deductive reasoner to communicate the apprehension of these truths, but to deduce others from them.

30. But though it may not be the author's business to elucidate the truth of the axioms as a deductive reasoner, it may still be desirable that he should do so as a philosophical teacher; and though it may not be possible to add anything to their evidence in the mind of him who possesses distinctly the idea from which they flow, it may be in our power to assist the beginner in obtaining distinct possession of this idea and unfolding it into its consequences. I shall therefore make a few remarks, tending to illustrate the self-evident nature of the "Axioms" of Statics, of Hydrostatics, and of the Doctrine of Motion.

31. Omitting, for the present, the consideration of the First Axiom of Statics (see paragraph 36); the Second is, "If two equal forces act perpendicularly at the extremities of equal arms of a straight line to turn it opposite ways, they will keep each other in equilibrium." This is often, and properly, further confirmed, by observing that there is no reason why one of the forces should preponderate rather than the other, and that, as both cannot preponderate, neither will do so. All the circumstances on which the result (equilibrium or preponderance) can depend, are equal on the two sides;—equal arms, equal angles, equal forces. If the forces are not in equilibrium, *which* will preponderate? no answer can be given, because there is no circumstance left by which either can be distinguished.

32. The argument which we have just used, is often applicable, and may be expressed by the formula, "there is no reason why one of the two opposite cases should occur, which is not equally valid for the other; and as both cannot occur (for they are opposite cases) neither will occur." This argument is called "the principle of sufficient reason;" it puts in a general form the considerations on which several of our axioms depend; and to persons who are accustomed to such generality, it may make their truth more clear.

The same principle might be applied to other cases, for example, to Axiom 7, that the effect produced on a bent lever does not depend on the direction of the arm. For if we suppose two forces acting perpendicularly on two equal arms of a bent lever to turn it opposite ways, these forces will balance, whatever be the angle which they make, since there is no reason why either should preponderate: but it would thus appear, that the force which would be balanced by Q in the figure to Axiom 7, would also be balanced by R, and therefore these two forces produce the same effect; which is what the axiom asserts.

33. The same reasoning might be applied to Axiom 9; for if two equal forces act at right angles at equal arms, in planes perpendicular to the axis of a rigid body, and tend to turn it opposite ways, they will balance each other, since all the conditions are the same for both.

34. Nearly the same might be said of Axiom 10;—if a string pass freely round a fixed body, equal forces acting at its two ends will balance each other; for if it pass with perfect freedom, its passing round the point cannot give an advantage to either force. Therefore the force which will be balanced by the string at its second extremity is exactly equal to the force which acts at its first extremity*.

35. The axioms which are perhaps least obvious are Axioms 4 and 5; for instance, the former;—that "the pressure upon the fulcrum is equal to the sum of the weights." Yet this becomes evident when we consider it steadily. It will then be seen that we consider pressure or weight as something which must be supported, so that the whole support must be equal to the whole pressure. The two weights which act upon the lever must be somehow balanced and counteracted, and the length of the lever cannot at all remove or alter this necessity. Their pressure will be the same as if the two arms of the lever were shortened till the weights coincided at the fulcrum; but in this case, it is clear that the pressure on the fulcrum would be equal to the sum of the weights: therefore it will be so in every other case.

36. This principle, that in statical equilibrium, a force is necessarily supported by an equal force, is expressed in Axiom 1, with regard to forces acting at any point; and the two forces are then called action and re-action. The principle, as stated in Axiom 1, may be considered as an expression of the conception of equality as applied to forces, or, if any one chooses, as a

* The same principle may be applied to prove Ax. 6.

definition of equal forces. This principle is implied in the conception of any comparison of forces; for equilibrium and addition of forces are modes in which forces are compared, as superposition and addition of spaces are modes in which geometrical quantities are compared.

We may further observe, that this fundamental conception of action and reaction is equivalent to the conception of force and matter, which are ideas necessarily connected and correlative. Matter is that which can resist the action of force. In Mechanics at least, we know matter only as the subject on which force acts.

37. But matter not only receives, it also transmits the action of force; and it is impossible to reason respecting the mechanical results of such transmission, without laying down the fundamental principles by which it operates. And this accordingly is the purpose of Axioms 7, 8, 9, 10, 13 [of the *Mechanical Euclid*]. When the body is supposed to be perfectly rigid, it transmits force without any change or yielding. This rigidity of a body is contemplated under different aspects, in the Axioms just referred to. In Axiom 8, it is the rigidity of a rod pushed endways; in Axiom 7, the rigidity of a plane turned about a fixed point; in Axiom 9, the rigidity of a solid twisted about an axis. Axiom 10 defines the manner in which a flexible string transmits pressure, and in like manner Axiom 1 of the Hydrostatics, defines the manner in which a fluid transmits pressure. Any one who chooses may call Axioms 7, 8, 9 of the Statics, collectively, the *Definition* of a rigid body. The place of these principles in our reasoning will not be thereby altered; nor the necessity superseded, of their being accompanied by distinct mechanical conceptions.

38. Axioms 14, 15, 16, of the Statics, are all included in the general consideration that material bodies may be supposed to consist of material parts, and that the weight of the whole is equal to the weight of all the parts; but they are stated separately, because they are used separately, and because they are at least as evident in these more particular cases as they are in the more general form.

By considerations of this nature it appears, and I trust quite satisfactorily, that the axioms, as above stated, are evident in their nature, in virtue of the conceptions which we necessarily form, in order to reason upon mechanical subjects.

39. Some persons may be surprized to find the Axioms of Mechanics represented as so numerous; especially if they look for analogy to Geometry, where the necessary axioms are confessedly few, and according to some writers, none; and they may be led to think that many of the axioms here given must be superfluous, by observing that in most mechanical works the fundamental principles are stated as much fewer than these. But I believe that very few of those which I have stated are superfluous *in effect*. From the very circumstance that they *are* axioms, they are assented to when they are adduced in the reasoning, whether they have been before asserted or not; but to make our reasoning formally correct (which was one of my objects) every proposition which is assumed should be previously stated. And when we examine them, we see that the various modifications and combinations of the ideas of force, body, and equilibrium, along with the ideas of space of one, two, or three dimensions, readily branch out into as many heads as appear in this part of the present work.

40. Some persons may be disposed at first to say, that our knowledge of such elementary truths as are stated in the Axioms of Statics and Hydrostatics, is collected *from observation and experience*. But in refutation of this I remark, that we cannot experimentally verify these elementary truths, without assuming other principles which require proof as much as these do. If, for instance, Archimedes had wished to ascertain by trial whether two equal weights at the equal arms of a lever would balance each other, how could he know that the weights *were* equal, by any more simple criterion than that they *did* balance? But in fact, it is perfectly certain that of the thousands of persons who from the time of Archimedes to the present day have studied Statics as a mathematical science, a very few have received or required any confirmation of his axioms from experiment; and those who have needed such help have undoubtedly been those in whom the apprehension of the real nature and force of the evidence of the subject was most obscure.

41. I by no means intend to assert that the axioms as stated in this Treatise are given in the only exact form; or that they may not be improved, simplified, and reduced in number. But I do not think it likely that this can be done to any great extent, consistently with the rigour of deductive proof. The Fourth Axiom of Statics is one which attempts have been

made to supersede: for example, Lagrange* has endeavoured to deduce it from the preceding ones. But it will be found that his proof, if distinctly stated, involves some such axiom as this:—that "If two forces, acting at the extremities of a straight line, and a single force, acting at an intermediate point of the straight line, produce the same effect to turn a body about another line, the two forces produce at the intermediate point an effect equal to the single force." And though this axiom may be self-evident, it will hardly be considered as more simple than that which it replaces.

42. Thus, Statics, like Geometry, rests upon axioms which are neither derived directly from experience, nor capable of being superseded by definitions, nor by simpler principles. In this science, as in that previously considered, the evidence of these fundamental truths resides in those convictions, to which an attentive and steady consideration of the subject necessarily leads us. The axioms with regard to pressures, action and reaction, equilibrium and preponderance, rigid and flexible bodies, result necessarily from the conceptions which are involved in all exact reasoning on such matters. The axioms do not flow *from* the definitions, but they flow irresistibly *along with* the definitions, from the distinctness of our ideas upon the subjects thus brought into view. These axioms are not arbitrary assumptions, nor selected hypotheses; but truths which we must see to be necessarily and universally true, before we can reason on to anything else; and here, as in Geometry, the capacity of seeing that they are thus true, is required in the student, in order that he and the writer may be able to proceed together.

43. It was stated that the Axioms of Geometry, are derived from the idea of space; in like manner the Axioms of Statics are derived from the *idea of statical force* or *pressure*, and the *idea of body* or *matter*, which, as we have said, is correlative with the idea of force. We must possess distinctly this idea of force acting upon body and body sustaining force;—of body resisting, and while it resists, transmitting the action of force;—of body, with this mechanical property, in the various forms of straight line, lever, plane, solid, flexible line, flexible surface, and fluid; and if we possess distinctly the ideas thus pointed out, the truth of the Axioms of Statics and Hydrostatics will be seen as self-evident, and we shall be in a condition to

* *Mècanique Analytique.* Introduction.

go on with the reasonings [of the *Mechanical Euclid*], seeing both the cogency of the proof, and its necessary and independent character.

44. As the Axioms which are the basis of the Statics of Solids depend upon the idea of body, considered as transmitting force, so the axioms of Hydrostatics depend on the idea of a fluid, considered as a body which transmits pressure in all directions; or, as we may express it more briefly, upon the *idea of fluid pressure*. It is not enough to conceive a fluid as a body, the parts of which are perfectly moveable; for, as I have elsewhere observed*, "this definition cannot be a sufficient basis for the doctrines of the pressure of fluids; for how can we evolve, out of the mere notion of mobility, which includes no conception of force, the independent conception of pressure." But the conception of fluid as transmitting pressure, supplies us with the requisite axioms. The First Axiom of our Hydrostatics—that if a fluid be contained in a tube of which the two ends are similar and equal planes acted on by equal pressures, it will be kept in equilibrium—follows from the principle of sufficient reason, for there is no reason why either pressure should preponderate. If, for example, the curvature of the tube, or any such cause, affected the pressure at either end, this condition would be a limitation of the property of transmitting pressure in all directions, and would imply imperfect fluidity; whereas the fluidity is supposed to be *perfect*. And for the like reasons, we might assume as an *Axiom* the Third *Proposition* of the Hydrostatics, that fluids transmit pressure *equally* in all directions, from one part of their boundary to the other; for if the pressure transmitted were different according to the direction, this difference might be referred to some cohesion or viscosity of the fluid; and the fluidity might be made more perfect, by conceiving the difference removed. Therefore the proposition would be necessarily and evidently true of a perfect fluid.

45. But instead of laying down this axiom, I have taken the axiom that any part of a fluid which is in equilibrium, may be supposed to become rigid. This axiom leads immediately to the proposition, and it is, besides, of great use in all parts of Hydrostatics. If we had to reason concerning flexible bodies, we might conveniently and properly assume a corresponding axiom for them;—namely, that, of a flexible body which is in

* *Thoughts on the Study of Mathematics.*

equilibrium, any part may be supposed to become rigid. And we might give a reason for this, by saying that rigidity implies forces which resist a tendency to change of form, when any such tendency occurs; but in a body which is in equilibrium, there is no tendency to change of form, and therefore the resisting forces vanish. It is of no consequence what forces *would* act *if there were* a stress to bend the body: since there *is not* any such stress, the rigidity is not called into play, and therefore it makes no difference whether we suppose it to exist or not.

46. The same kind of reasons may be given, in order to shew the admissibility of introducing, in the case of equilibrium of a fluid, rigidity, instead of that still greater susceptibility of change of figure which fluidity implies. Since the mass is perfectly fluid, its particles exert no constraint on each other's motions; but then, because they are in equilibrium, no constraint is needed to keep them in their places. They are as steadily kept there (so long as the same forces continue to act) as if they were held by the insurmountable forces which connect the parts of a perfectly rigid body. We may therefore suppose the inoperative forces of rigidity to be present or absent among the particles, without altering the other forces or their relations. And hence we see the truth of Axiom 2 of the Hydrostatics.

47. The above considerations (Art. 44) arising from the properties which we assume being perfect, may be applied in other cases; for instance, to shew that the force exerted by a *perfectly* smooth surface is perpendicular to the surface. (*Mech. Euc.* B. I. Ax. 13.) For if it were not, the force might be resolved into a force perpendicular to the surface, and a force acting along the surface; and the latter force might be referred to some friction or cohesion of the surface. Therefore we should not have supposed the surface perfectly smooth, without imagining this force to vanish: and thus the only force exerted by such a perfectly smooth surface would necessarily be a normal force.

48. The last axiom of Hydrostatics (Ax. 7) is in fact a substitute for an idea which we must exclude in Elementary Mathematics;—the idea of a *Limit*. The attempt to proceed far in Geometry without the use of this idea, gave rise to a series of well-known embarrassments among the ancients. The mode of evading the difficulty which I have adopted, by means

of the axiom just referred to, appeared to me the best. The axiom is readily assented to, if it be considered that, since we may make the particles as small as we please, we may make as small as we please the errour arising from the neglect of one particle. We may make it microscopic, and then throw away the microscope; and thus the errour vanishes.

49. Some of the Axioms which are stated in Book III., on the Laws of Motion, give occasion to remarks similar to those already made. Thus Axiom 4, which asserts that if particles move in such a manner as always to preserve the same relative distances and positions, their motions will not be altered by supposing them rigidly connected, is evident by the same considerations as the Axioms concerning flexible and fluid bodies, already noticed in Articles 45 and 46. For the forces of rigidity are forces which would prevent a change of the distances and relative positions of the particles if there were a tendency to any such change; and if there be no such tendency, it makes no difference whether the potential resistance to it be present or absent.

50. The 5th Axiom of Book III., which asserts that forces producing parallel and equal velocities at the same time, may be conceived to be added; and the 6th Axiom, which asserts that in systems in motion the action and reaction are equal and opposite, are applications of what is stated in the second sentence of this third Book;—that the Definitions and Axioms of Statics are adopted and assumed in the case of bodies in motion. In the third Book, as in the first, forces are conceived as capable of addition, and matter is conceived as that which can resist force, and transmit it unaltered.

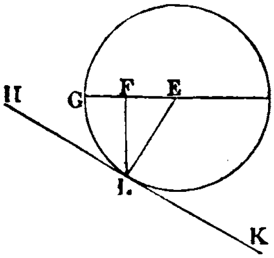
The 3d, 8th, and 9th Axioms of Book III., like the 7th of Book II., are introduced to avoid the reasoning which depends on Limits.

51. In the case of Mechanics, as in the case of Geometry, the distinctness of the idea is necessary to a full apprehension of the truth of the axioms; and in the case of mechanical notions it is far more common than in Geometry, that the axioms are imperfectly comprehended, in consequence of the want of distinctness and exactness in men's ideas. Indeed this indistinctness of mechanical notions has not only prevailed in many individuals at all periods, but we can point out whole centuries, in which it has been, so far as we can trace, universal.

And the consequence of this was, that the science of Statics, after being once established upon clear and sound principles, again fell into confusion, and was not understood as an exact science for two thousand years, from the time of Archimedes to that of Galileo and Stevinus.

52. In order to illustrate this indistinctness of mechanical ideas, I shall take from an ancient Greek writer an attempt to solve a mechanical problem; namely, the Problem of the Inclined Plane. The following is the mode in which Pappus professes* to answer this question:—"To find the force which will support a given weight *A* upon an inclined plane."

Let *HK* be the plane; let the weight *A* be formed into a sphere: let this sphere be placed in contact with the plane *HK*, touching it in the point *L*, and let *E* be its center. Let *EG* be a horizontal radius, and *LF* a vertical line which meets it. Take a weight *B* which is to *A* as *EF* to *FG*. Then if *A* and *B* be suspended at *E* and *G* to the lever *EFG* of which the center of motion is *F*, they will balance; being supported, as it were, by the fulcrum *LF*. And the sphere, which is equal to the weight *A*, may be supposed to be collected at its center. If therefore *B* act at *G*, the weight *A* will be supported.



It may be observed that in this attempt, the confusion of ideas is such, that the author assumes a weight which acts at *G*, on the lever *EFG*, and which is therefore a vertical force, as identical with a force which acts at *G*, to support the body in the inclined plane, and which is parallel to the plane.

53. When this kind of confusion was remedied, and when men again acquired distinct notions of pressure, and of the transmission of pressure from one point to another, the science of Statics was formed by Stevinus, Galileo, and their successors†.

The fundamental ideas of Mechanics being thus acquired, and the requisite consequences of them stated in axioms, our

* Pappus, B. VIII. Prop. ix. I purposely omit the confusion produced by this author's mode of treating the question, in which he inquires the force which will *draw* a body *up* the inclined plane.

† See *History of the Inductive Sciences*, B. VI. chap. i. sect. 2, On the Revival of the Scientific Idea of Pressure.

reasonings proceed by the same rigorous line of demonstration, and under the same logical rules as the reasonings of Geometry; and we have a science of Statics which is, like Geometry, an exact deductive science.

Sect. II.—*On the Logic of Induction.*

54. There are other portions of Mechanics which require to be considered in another manner; for in these there occur principles which are derived directly and professedly from experiment and observation. The derivation of principles by reasoning from facts is performed by a process which is termed *Induction*, which is very different from the process of Deduction already noticed, and of which we shall attempt to point out the character and method.

It has been usual to say of any general truths, established by the consideration and comparison of several facts, that they are obtained by *Induction;* but the distinctive character of this process has not been well pointed out, nor have any rules been laid down which may prescribe the form and ensure the validity of the process, as has been done for Deductive reasoning by common Logic. The *Logic of Induction* has not yet been constructed; a few remarks on this subject are all that can be offered here.

55. The Inductive Propositions, to which we shall here principally refer as examples of their class, are those elementary principles which occur in considering the motion of bodies, and of which some are called the Laws of Motion *. They are such as these;—a body not acted on by any force will move on for ever uniformly in a straight line;—gravity is a uniform force;—if a body in motion be acted upon by any force, the effect of the force will be compounded with the previous motion;—when a body communicates motion to another directly, the momentum lost by the first body is equal to the momentum gained by the second. And I remark, in the first place, that in collecting such propositions from facts, there occurs a step corresponding to the term "Induction," (ἐπαγωγή, *inductio*). Some notion is *superinduced* upon the observed facts. In each inductive process, there is some general idea introduced, which is given,

* Inductive Propositions [in the *Mechanical Euclid*] are, Book II. Propositions 25, 26, 32, 36, 37: Book III. Prop. 2, 3, 8, 13.

not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a new generality. In order to obtain our inference, we travel beyond the cases we have before us; we consider them as exemplifications of, or deviations from, some ideal case in which the relations are complete and intelligible. We take a standard, and measure the facts by it; and this standard is created by us, not offered by Nature. Thus we assert, that a body left to itself will move on with unaltered velocity, not because our senses ever disclosed to us a body doing this, but because (taking this as our ideal case) we find that all actual cases are intelligible and explicable by means of the notion of forces which cause change of motion, and which are exerted by surrounding bodies. In like manner, we see bodies striking each other, and thus moving, accelerating, retarding, and stopping each other; but in all this, we do not, by our senses, perceive that abstract quantity, momentum, which is always lost by one as it is gained by another. This momentum is a creation of the mind, brought in among the facts, in order to convert their apparent confusion into order, their seeming chance into certainty, their perplexing variety into simplicity. This the idea of momentum gained and lost does; and, in like manner, in any other case in which inductive truths are established, some idea is introduced, as the means of passing from the facts to the truth.

56. The process of mind of which we here speak can only be described by suggestion and comparison. One of the most common of such comparisons, especially since the time of Bacon, is that which speaks of induction as the *interpretation* of facts. Such an expression is appropriate; and it may easily be seen that it includes the circumstance which we are now noticing;—the superinduction of an idea upon the facts by the interpreting mind. For when we read a page, we have before our eyes only black and white, form and colour; but by an act of the mind, we transform these perceptions into thought and emotion. The letters are nothing of themselves; they contain no truth, if the mind does not contribute its share: for instance, if we do not know the language in which the words are written. And if we are imperfectly acquainted with the language, we become very clearly aware how much a certain activity of the mind is requisite in order to convert the words into propositions, by the extreme

effort which the business of interpretation requires. Induction, then, may be conveniently described as the interpretation of phenomena.

57. But I observe further, that in thus inferring truths from facts, it is not only necessary that the mind should contribute to the task its own idea, but, in order that the propositions thus obtained may have any exact import and scientific value, it is requisite that the idea be perfectly *distinct* and precise. If it be possible to obtain some vague apprehension of truths, while the ideas in which they are expressed remain indistinct and ill-defined, such knowledge cannot be available for the purposes we here contemplate. In order to construct a science, all our fundamental ideas must be distinct; and among them, those which Induction introduces.

58. This necessity for distinctness in the ideas which we employ in Induction, makes it proper to *define*, in a precise and exact manner, each idea when it is thus brought forwards. Thus, in establishing the propositions which we have stated as our examples in these cases, we have to define *force* in general; *uniform force*; *compounding* of motions; *momentum*. The construction of these definitions is an essential part of the process of Induction, no less than the assertion of the inductive truth itself.

59. But in order to justify and establish the inference which we make, the ideas which we introduce must not only be distinct, but also *appropriate*. They must be exactly and closely applicable to the facts; so that when the idea is in our possession, and the facts under our notice, we perceive that the former includes and takes up the latter. The idea is only a more precise mode of apprehending the facts, and it is empty and unmeaning if it be anything else; but if it be thus applicable, the proposition which is asserted by means of it is true, precisely because the facts *are* facts. When we have defined force to be the cause of change of motion, we see that, as we remove external forces, we do, in actual experiments, remove all the change of motion; and therefore the proposition that there is in bodies no internal cause of change of motion, is true. When we have defined momentum to be the product of the velocity and quantity of matter, we see that in the actions of bodies, the effect increases as the momentum increases; and by measurement, we find that the effect may consistently be measured by the momen-

tum. The ideas here employed are not only distinct in the mind, but applicable in the world; they are the elements, not only of relations of thought, but of laws of nature.

60. Thus an inductive inference requires an idea from within, facts from without, and a coincidence of the two. The idea must be distinct, otherwise we obtain no scientific truth; it must be appropriate, otherwise the facts cannot be steadily contemplated by means of it; and when they are so contemplated, the Inductive Proposition must be seen to be verified by the evidence of sense.

It appears from what has been said, that in establishing a proposition by Induction, the definition of the idea and the assertion of the truth, are not only both requisite, but they are correlative. Each of the two steps contains the verification and justification of the other. The proposition derives its meaning from the definition; the definition derives its reality from the proposition. If they are separated, the definition is arbitrary or empty, the proposition is vague or verbal.

61. Hence we gather, that in the Inductive Sciences, our Definitions and our Elementary Inductive Truths ought to be introduced together. There is no value or meaning in definitions, except with reference to the truths which they are to express. Discussions about the definitions of any science, taken separately, cannot therefore be profitable, if the discussion do not refer, tacitly or expressly, to the fundamental truths of the science; and in all such discussions it should be stated what are taken as the fundamental truths. With such a reference to Elementary Inductive Truths clearly understood, the discussion of Definitions may be the best method of arriving at that clearness of thought, and that arrangement of facts, which Induction requires.

I will now note some of the differences which exist between Inductive and Deductive Reasoning, in the modes in which they are presented.

62. One leading difference in these two kinds of reasoning is, that in Deduction we infer particular from general truths; in Induction, on the contrary, we infer general from particular. Deductive proofs consist of many steps, in each of which we apply known general propositions in particular cases;—"all triangles have their angles equal to two right angles, therefore this triangle has; therefore, &c." In Induction, on the other hand, we have a single step in which we pass from many par-

ticular Propositions to one general proposition; "This stone falls downwards; so do those others;—all stones fall downwards." And the former inference flows necessarily from the relation of general and particular; but the latter, as we have seen, derives its power of convincing from the introduction of a new idea, which is distinct and appropriate, and which supplies that generality which the particulars cannot themselves offer.

63. I observe also that this difference of process in inductive and deductive proofs, may be most properly marked by a difference in the form in which they are stated. In Deduction, the *Definition* stands at the beginning of the proposition; in Induction, it may most suitably stand at or near the end. Thus the definition of a uniform force is introduced in the course of the proposition that gravity is a uniform force. And this arrangement represents truly the real order of proof; for, historically speaking, it was taken for granted that gravity was a uniform force; but the question remained, what was the right definition of a uniform force. And in the establishment of other inductive principles, in like manner, definitions cannot be laid down for any useful purpose, till we know the propositions in which they are to be used. They may therefore properly come each at the conclusion of its corresponding proposition.

64. The ideas and definitions which are thus led to by our inductive process, may bring with them Axioms. Such Axioms may be self-evident as soon as the inductive idea has been distinctly apprehended, in the same manner as was explained respecting the fundamental ideas of Geometry and Statics. And thus *Axioms*, as well as Definitions, may come at the end of our Inductive Propositions; and they thus assume their proper place at the beginning of the deductive propositions which follow them, and are proved from them. Thus, in Book III., Axioms 8 and 9, come after the definition of Accelerating Force, and stand between Props. 14 and 15.

65. Another peculiarity in inductive reasoning may be noticed. In a deductive demonstration, the reference is always to what has been already proved; in establishing an Inductive Principle, it is most convenient that the reference should be to subsequent propositions. For the proof of the Inductive Principle consists in this;—that the principle being adopted, consequences follow which agree with fact; but the demonstration of these consequences may require many steps, and several special

propositions. Thus the Inductive Principle, that gravity is a uniform force, is established by shewing that the law of descent, which falling bodies follow in fact, is explained by means of this principle; namely, the law that the space is as the square of the time from the beginning of the motion. But the proof of such a property, from the definition of a uniform force, requires many steps, as may be seen [in the *Mechanical Euclid*], B. III. Pr. 5: and this proof must be referred to, along with several others, in order to establish the truth, that gravity is a uniform force.

66. It may be suggested, that, this being the case, the propositions might be transposed, so that the inductive proof might come after those propositions to which it refers. But if this were done, all the propositions which depend upon the laws of motion must be proved hypothetically only. For instance, we must say, "If, in the communication of motion, the momentum lost and gained be equal, the velocity acquired by a body falling down an inclined plane, will be equal to that acquired by falling down the height." This would be inconvenient, and even if it were done, that completeness in the line of demonstration which is the object of the change, could not be obtained; for the transition from the particular cases to the general truth, which must occur in the Inductive Proposition, could not be in any way justified according to rules of Deductive Logic.

I have, therefore, in the preceding pages, placed the Inductive Principle first in each line of reasoning; and have ranged after it the Deductions from it, which justify and establish it as their first office, but which are more important as its consequences and applications, after it is supposed to be established.

67. I have used one common *formula* in presenting the proof of each of the Inductive Principles which I have introduced;—namely, after stating or exemplifying the facts which the induction includes, I have added "These results can be clearly explained and rigorously deduced by introducing the *Idea* or the *Definition*," which belongs to each case, "and the *Principle*," which expresses the inductive truth. I do not mean to assert that this formula is the only right one, or even the best; but it appears to me to bring under notice the main circumstances which render an induction systematic and valid.

68. It may be observed, however, that this formula does not express the full cogency of the proof. It declares only that the results *can* be clearly explained and rigorously deduced by

the employment of a certain definition and a certain proposition. But in order to make the conclusion demonstrative, we ought to be able to declare that the results can be clearly explained and rigorously deduced *only* by the definition and proposition which we adopt. And, in reality, the mathematician's conviction of the truth of the Laws of Motion does depend upon his seeing that they (or laws equivalent to them) afford the *only* means of clearly expressing and deducing the actual facts. But this conviction, that no other law than those proposed can account for the known facts, finds its place in the mind gradually, as the contemplation of the consequences of the law and the various relations of the facts becomes steady and familiar. I have therefore not thought it proper to require such a conviction along with the first assent to the inductive truths which I have here stated.

69. The propositions established by Induction are termed *Principles*, because they are the starting points of trains of deductive reasoning. In the system of deduction, they occupy the same place as axioms; and accordingly they are termed so by Newton—"Axiomata sive leges motus." Stewart objects strongly to this expression*: and it would be difficult to justify it; although to draw the line between axioms and inductive principles may be a harder task than at first appears.

70. But from the consideration that our Inductive Propositions are the principles or beginnings of our deductive reasoning, and so far at least stand in the place of axioms, we may gather *this* lesson,—that they are not to be multiplied without necessity. For instance, if in a treatise on Hydrostatics, we should state as two separate propositions, that "air has weight;" and that "the mercury in the barometer is sustained by the weight of the air;" and should prove both the one and the other by reference to experiment; we should offend against the maxims of Logic. These propositions are connected; the latter may be demonstrated deductively from the former; the former may be inferred inductively from the facts which prove the latter. One of these two courses ought to be adopted; we ought not to have two ends of our reasoning upwards, or two beginnings of our reasoning downwards.

71. I shall not now extend these Remarks further. They may appear to many barren and unprofitable speculations; but

* *Elem. Phil. Human Mind*, Vol. II. p. 44.

those who are familiar with such subjects, will perhaps find in them something which, if well founded, is not without some novelty for the English reader. Such will, I think, be the case, if I have satisfied him,—that mathematical truth depends on axioms as well as definitions,—that the evidence of geometrical axioms is to be found only in the distinct possession of the idea of space,—that other branches of mathematics also depend on axioms,—and that the evidence of these axioms is to be sought in some appropriate idea;—that the evidence of the axioms of statics, for instance, resides in the ideas of force and matter;—that in the process of induction the mind must supply an idea in addition to the facts apprehended by the senses;—that in each such process we must introduce one or more definitions, as well as a proposition;—that the definition and the proposition are correlative, neither being useful or valid without the other;—and that the formula of inductive reasoning must be in many respects the reverse of the common logical formulæ of deduction.

ESSAY III.

DEMONSTRATION THAT ALL MATTER IS HEAVY*.

THE discussion of the nature of the grounds and proofs of the most general propositions which the physical sciences include, belongs rather to Metaphysics than to that course of experimental and mathematical investigation by which the sciences are formed. But such discussions seem by no means unfitted to occupy the attention of the cultivators of physical science. The ideal, as well as the experimental side of our knowledge must be carefully studied and scrutinized, in order that its true import may be seen; and this province of human speculation has been perhaps of late unjustly depreciated and neglected by men of science. Yet it can be prosecuted in the most advantageous manner by them only: for no one can speculate securely and rightly respecting the nature and proofs of the truths of science without a steady possession of some large and solid portions of such truths. A man must be a mathematician, a mechanical philosopher, a natural historian, in order that he may philosophize well concerning mathematics, and mechanics, and natural history; and the mere metaphysician who without such preparation and fitness sets himself to determine the grounds of mathematical or mechanical truths, or the principles of classification, will be liable to be led into error at every step. He must speculate by means of general terms, which he will not be able to use as instruments of discovering and conveying philosophical truth, because he cannot, in his own mind, habitually and familiarly, embody their import in special examples.

Acting upon such views, I have already laid before the Philosophical Society of Cambridge essays on such subjects as I here refer to; especially a memoir "On the Nature of the Truth of the Laws of Motion," which was printed by the Society in its Transactions. This memoir appears to have

* From the *Transactions of the Cambridge Philosophical Society*, Vol. VII. Part II. No. 12. [1841.]

excited in other places, notice of such a kind as to shew that the minds of many speculative persons are ready for and inclined towards the discussion of such questions. I am therefore the more willing to bring under consideration another subject of a kind closely related to the one just mentioned.

The general questions which all such discussions suggest, are (in the existing phase of English philosophy) whether certain proposed scientific truths, (as the laws of motion,) be *necessary* truths; and if they are necessary, (which I have attempted to shew that in a certain sense they are,) *on what ground* their necessity rests. These questions may be discussed in a general form, as I have elsewhere attempted to shew. But it may be instructive also to follow the general arguments into the form which they assume in special cases; and to exhibit, in a distinct shape, the incongruities into which the opposite false doctrine leads us, when applied to particular examples. This accordingly is what I propose to do in the present memoir, with regard to the proposition stated at the head of this Essay, namely, that *all matter is heavy.*

At first sight it may appear a doctrine altogether untenable to assert that this proposition is a necessary truth: for, it may be urged, we have no difficulty in conceiving matter which is not heavy; so that matter without weight is a conception not inconsistent with itself; which it must be if the reverse were a necessary truth. It may be added, that the possibility of conceiving matter without weight was shewn in the controversy which ended in the downfall of the phlogiston theory of chemical composition; for some of the reasoners on this subject asserted phlogiston to be a body with positive levity instead of gravity, which hypothesis, however false, shews that such a supposition is possible. Again, it may be said that *weight* and *inertia* are two separate properties of matter: that mathematicians measure the quantity of matter by the inertia, and that we learn by experiment only that the weight is proportional to the inertia; Newton's experiments with pendulums of different materials having been made with this very object.

I proceed to reply to these arguments. And first, as to the possibility of conceiving matter without weight, and the argument thence deduced, that the universal gravity of matter is not a necessary truth, I remark, that it is indeed just, to say that we cannot even distinctly conceive the contrary of a necessary truth to be true; but that this impossibility can be asserted only of those perfectly distinct conceptions which result from a complete development of the fundamental idea and its consequences. Till we reach this stage of development, the obscurity and indistinctness may prevent our perceiving absolute contradictions, though they exist. We have abundant store of examples of this, even in geometry and arithmetic; where the truths are universally allowed to be necessary, and where the relations which are impossible, are also inconceivable, that is, not conceivable distinctly. Such relations, though not distinctly conceivable, still often appear conceivable and possible, owing to the indistinctness of our ideas. Who, at the first outset of his geometrical studies, sees any impossibility in supposing the side and the diagonal of a square to have a common measure? Yet they can be rigorously proved to be incommensurable, and therefore the attempt distinctly to conceive a common measure of them must fail. The attempts at the geometrical duplication of the cube, and the supposed solutions, (as that of Hobbes) have involved absolute contradictions; yet this has not prevented their being long and obstinately entertained by men, even of minds acute and clear in other respects. And the same might be shewn to be the case in arithmetic. It is plain, therefore, that we cannot, from the supposed possibility of conceiving matter without weight, infer that the contrary may not be a necessary truth.

Our power of judging, from the compatibility or incompatibility of our conceptions, whether certain propositions respecting the relations of ideas are true or not, must depend entirely, as I have said, upon the degree of development which such ideas have undergone in our minds. Some of the relations of our conceptions on any subject are evident upon the first steady contemplation of the fundamental idea by a sound mind: these are the *axioms* of the subject. Other propositions may be deduced from the axioms by strict logical reasoning. These propositions are no less *necessary* than the axioms, though to common minds their *evidence* is very different. Yet as we become familiar with the steps by which these ulterior truths are deduced from the axioms, *their* truth also becomes evident, and the contrary becomes inconceivable. When a person has familiarized himself with the first twenty-six propositions of Euclid, and not till then, it becomes evident to him, that parallelograms on the same base and between the same parallels are equal; and he cannot even conceive the contrary. When he has a little further cultivated his geometrical powers, the equality of the square on the hypothenuse of a right-angled triangle to the squares on the sides, becomes also evident; the steps by which it is demonstrated being so familiar to the mind as to be apprehended without a conscious act. And thus, the contrary of a necessary truth cannot be distinctly conceived; but the incapacity of forming such a conception is a condition which depends upon cultivation, being intimately connected with the power of rapidly and clearly perceiving the connection of the necessary truth under consideration with the elementary principles on which it depends. And thus, again, it may be that there is an absolute impossibility of conceiving matter without weight; but then, this impossibility may not be apparent, till we have traced our fundamental conceptions of matter into some of their consequences.

The question then occurs, whether we can, by any steps of reasoning, point out an inconsistency in the conception of matter without weight. This I conceive we may do, and this I shall attempt to shew.

The general mode of stating the argument is this:—the quantity of matter is measured by those sensible properties of matter which undergo quantitative addition, subtraction and division, as the matter is added, subtracted and divided. The quantity of matter cannot be known in any other way. But this mode of measuring the quantity of matter, in order to be true at all, must be universally true. If it were only partially true, the limits within which it is to be applied would be arbitrary; and therefore the whole procedure would be arbitrary, and, as a method of obtaining philosophical truth, altogether futile.

We may unfold this argument further. Let the contrary be supposed, of that which we assert to be true: namely, let it be supposed that while all other kinds of matter are heavy, (and of course heavy in proportion to the quantity of matter) there is one kind of matter which is absolutely destitute of weight; as, for instance, phlogiston, or any other element. Then where this *weightless* element (as we may term it) is mixed with *weighty* elements, we shall have a compound, in which the weight is no longer proportional to the quantity of matter. If, for example, 2 measures of heavy matter unite with 1 measure of phlogiston, the weight is as 2, and the quantity of matter as 3. In all such cases, therefore, the weight ceases to be the measure of the quantity of matter. And as the proportion of the weighty and the weightless matter may vary in innumerable degrees in such compounds, the weight affords no criterion at all of the quantity of matter in them. And the smallest admixture of the weightless element is sufficient to prevent the weight from being taken as the measure of the quantity of matter.

But on this hypothesis, how are we to distinguish such compounds from bodies consisting purely of heavy matter? How are we to satisfy ourselves that there is not, in every body, some admixture, small or great, of the weightless element? If we call this element *phlogiston*, how shall we know that the bodies with which we have to do are, any of them, absolutely free from phlogiston?

We cannot refer to the weight for any such assurance; for by supposition the presence and absence of phlogiston makes no difference in the weight. Nor can any other properties secure us at least from a very small admixture; for to assert that a mixture of 1 in 100 or 1 in 10 of phlogiston would always manifest itself in the properties of the body, must be an arbitrary procedure, till we have proved this assertion by experiment: and we cannot do this till we have learnt some mode of measuring the quantities of matter in bodies and parts of bodies; which is exactly what we question the possibility of, in the present hypothesis.

Thus, if we assume the existence of an element, *phlogiston*, devoid of weight, we cannot be sure that every body does not contain some portion of this element; while we see that if there be an admixture of such an element, the weight is no longer any criterion of the quantity of matter. And thus we have proved, that if there be any kind of matter which is not heavy, the weight can no longer avail us, *in any case or to any extent*, as a measure of the quantity of matter.

I may remark, that the same conclusion is easily extended to the case in which phlogiston is supposed to have absolute levity; for in that case, a certain mixture of phlogiston and of heavy matter would have no weight, and might be substituted for phlogiston in the preceding reasoning.

I may remark, also, that the same conclusion would follow by the same reasoning, if any kind of matter, instead of being void of weight, were heavy, indeed, but not *so* heavy, in proportion to its quantity of matter, as other kinds.

On all these hypotheses there would be no possibility of measuring quantity of matter by weight at all, in any case, or to any extent.

But it may be urged, that we have not yet reduced the hypothesis of matter without weight to a contradiction; for that mathematicians measure quantity of matter, not by weight, but by the other property, of which we have spoken, inertia.

To this I reply, that, practically speaking, quantity of matter is always measured by weight, both by mechanicians and chemists: and as we have proved that this procedure is utterly insecure in all cases, on the hypothesis of weightless matter, the practice rests upon a conviction that the hypothesis is false. And yet the practice is universal. Every experimenter measures quantity of matter by the balance. No one has ever thought of measuring quantity of matter by its inertia practically: no one has constructed a measure of quantity of matter in which the matter produces its indications of quantity by its motion. When we have to take into account the inertia of a body, we inquire what its weight is, and assume this as the measure of the inertia; but we never take the contrary course, and ascertain the inertia first in order to determine by that means the weight.

But it may be asked, Is it not then true, and an important scientific truth, that the *quantity of matter* is measured by the *inertia?* Is it not true, and proved by experiment, that the *weight* is *proportional* to the *inertia?* If this be not the result of Newton's experiments mentioned above, what, it may be demanded, do they prove?

To these questions I reply: It is true that quantity of matter is measured by the inertia, for it is true that inertia is as the quantity of matter. This truth is indeed one of the laws of motion. That weight is proportional to inertia is proved by experiment, as far as the laws of motion are so proved: and Newton's experiments prove one of the laws of motion, so far as any experiments can prove them, or are needed to prove them.

That inertia is proportional to weight, is a law equivalent to that law which asserts, that when pressure produces motion in a given body, the velocity produced in a given time is as the pressure. For if the velocity be as the pressure, when the body is given, the velocity will be constant if the inertia also be as the pressure. For the inertia is understood to be that property of bodies to which, *ceteris paribus*, the velocity impressed is *inversely* proportional. One body has twice as much inertia as another, if, when the same force acts upon it for the same time, it acquires but half the velocity. This is the fundamental conception of *inertia*.

In Newton's pendulum experiments, the pressure producing motion was a certain resolved part of the weight, and was proportional to the weight. It appeared by the experiments, that whatever were the material of which the pendulum was formed, the rate of oscillation was the same; that is, the velocity acquired was the same. Hence the inertia of the different bodies must have been in each case as the weight: and thus this assertion is true of all different kinds of bodies.

Thus it appears that the assertion, that inertia is universally proportional to weight, is equivalent to the law of motion, that the velocity is as the pressure. The conception of inertia (of which, as we have said, the fundamental conception is, that the velocity impressed is inversely proportional to the inertia,) connects the two propositions so as to make them identical.

Hence our argument with regard to the universal gravity of matter brings us to the above law of motion, and is proved by Newton's experiments in the same sense in which that law of motion is so proved.

Perhaps some persons might conceive that the identity of weight and inertia is obvious at once; for both are merely resistance to motion;—inertia, resistance to all motion (or change of motion)—weight, resistance to motion upwards.

But there is a difference in these two kinds of resistance to motion. Inertia is instantaneous, weight is continuous resistance. Any momentary impulse which acts upon a free body overcomes its inertia, for it changes its motion; and this change once effected, the inertia opposes any return to the former condition, as well as any additional change. The inertia is thus overcome by a momentary force. But the weight can only be overcome by a continuous force like itself. If an impulse act in opposition to the weight, it may for a moment neutralize or overcome the weight; but if it be not continued, the weight resumes its effect, and restores the condition which existed before the impulse acted.

But weight not only produces rest, when it is resisted, but motion, when it is not resisted. Weight is measured by the reaction which would balance it; but when unbalanced, it produces motion, and the velocity of this motion increases constantly. Now what determines the velocity thus produced in a given time, or its rate of increase? What determines it to have one magnitude rather than another? To this we must evidently reply, *the inertia.* When weight produces motion, the inertia is the reaction which makes the motion determinate. The accumulated motion produced by the action of unbalanced weight is as determinate a condition as the equilibrium produced by balanced weight. In both cases the condition of the body acted on is determined by the opposition of the action and reaction.

Hence inertia is the reaction which opposes the weight, when unbalanced. But by the conception of action and reaction, (as mutually determining and determined,) they are measured by each other: and hence the inertia is necessarily proportional to the weight.

But when we have reached this conclusion, the original objection may be again urged against it. It may be said, that there must be some fallacy in this reasoning, for it proves a state of things to be necessary when we can so easily conceive a contrary state of things. Is it denied, the opponent may ask, that we can readily imagine a state of things in which bodies have no weight? Is not the uniform tendency of all bodies in the same direction not only not necessary, but not even true? For they do in reality tend, not with equal forces in parallel lines, but to a center with unequal forces, according to their position: and we can conceive these differences of intensity and direction in the force to be greater than they really are; and can with equal ease suppose the force to disappear altogether.

To this I reply, that certainly we may conceive the weight of bodies to vary in intensity and direction, and by an additional effort of imagination, may conceive the weight to vanish: but that in all these suppositions, even in the extreme one, we must suppose the rule to be universal. If *any* bodies have weight, *all* bodies must have weight. If the direction of weight be different in different points, this direction must still vary according to the *law of continuity;* and the same is true of the intensity of the weight. For if this were not so, the rest and motion, the velocity and direction, the permanence and change of bodies, as to their mechanical condition, would be arbitrary and incoherent: they would not be subject to mechanical ideas; that is, not to ideas at all: and hence these conditions of objects would in fact be inconceivable. In order that the universe may be possible, that is, may fall under the conditions of intelligible conceptions, we must be able to conceive a body at rest. But the rest of bodies (except in the absolute negation of all force) implies the equilibrium of opposite forces. And one of these opposite forces must be a *general* force, as weight, in order that the universe may be governed by general conditions. And this general force, by the conception of force, may produce motion, as well as equilibrium; and this motion again must be determined, and determined by general conditions; which cannot be, except the communication of motion be regulated by an inertia proportional to the weight.

But it will be asked, Is it then pretended that Newton's experiment, by which it was intended to prove inertia proportional to weight, does really prove nothing but what may be demonstrated *à priori?* Could we know, without experiment, that all bodies,—gold, iron, wood, cork,—have inertia proportional to their weight? And to this we reply, that experiment holds the same place in the establishment of this, as of the other fundamental doctrines of mechanics. Intercourse with the external world is requisite for developing our ideas; measurement of phenomena is needed to fix our conceptions and to render them precise: but the result of our experimental studies is, that we reach a position in which our convictions do not rest upon experiment. We learn by observation truths of which we afterwards see the necessity. This is the case with the laws of motion, as I have repeatedly endeavoured to shew. The same will appear to be the case with the proposition, that bodies of different kinds have their inertia proportional to their weight.

For bodies *of the same kind* have their inertia proportional to their weight, both quantities being proportional to the quantity of matter. And if we compress the same quantity of matter into half the space, neither the weight nor the inertia is altered, because these depend on the quantity of matter alone. But in this way we obtain a body of *twice the density;* and in the same manner we obtain a body of any other density. Therefore whatever be the density, the inertia is proportional to the quantity of matter. But the mechanical relations of bodies cannot depend upon any difference of *kind*, *except* a difference of density. For if we suppose any fundamental difference of mechanical nature in the particles or component elements of bodies, we are led to the same conclusion, of arbitrary, and therefore impossible, results, which we deduced from this supposition with regard to weight. Therefore all bodies of different density, and hence, all bodies whatever, must have their inertia proportional to their weight.

Hence we see, that the propositions, that all bodies are heavy, and that inertia is proportional to weight, necessarily follow from those fundamental ideas which we unavoidably employ in all attempts to reason concerning the mechanical relations of bodies. This conclusion may perhaps appear the more startling to many, because they have been accustomed to expect that fundamental ideas and their relations should be self-evident at our first contemplation of them. This, however, is far from being the case, as I have already shewn. It is not the *first*, but the most complete and developed condition of our conceptions, which enables us to see what are axiomatic truths in each province of human speculation. Our fundamental ideas are necessary conditions of knowledge, universal forms of intuition, inherent types of mental development; they may even be termed, if any one chooses, results of connate intellectual tendencies; but we cannot term them *innate* ideas, without calling up a large array of false opinions. For innate ideas were considered as capable of composition, but by no means of simplification: as most perfect in their original condition; as to be found, if any where, in the most uneducated and most uncultivated minds; as the same in all ages, nations, and stages of intellectual culture; as capable of being referred to at once, and made the basis of our reasonings, without any special acuteness or effort: in all which circumstances the Fundamental Ideas of which we have spoken, are opposed to Innate Ideas so understood.

I shall not, however, here prosecute this subject. I will only remark, that Fundamental Ideas, as we view them, are not only not innate, in any usual or useful sense, but they are not necessarily *ultimate* elements of our knowledge. They are the results of our analysis so far as we have yet prosecuted it; but they may themselves subsequently be analysed. It may hereafter appear, that what we have treated as different Fundamental Ideas have, in fact, a connexion, at some point below the structure which we erect upon them. For instance, we treat of the mechanical ideas of force, matter, and the like, as distinct from the idea of substance. Yet the principle of measuring the quantity of matter by its weight, which we have deduced from mechanical ideas, is applied to determine the substances which enter into the composition of bodies. The idea of substance supplies the axiom, that the whole quantity of matter of a compound body is equal to the sum of the quantities of matter of its elements. The mechanical ideas of force and matter lead us to infer that the quantity both of the whole and its parts must be measured by their weights. *Substance* may, for some purposes, be described as that to which properties belong; *matter* in like manner may be described as that which resists force. The former involves the Idea of permanent Being; the latter, the Idea of Causation. There may be some elevated point of view from which these ideas may be seen to run together. But even if this be so, it will by no means affect the validity of reasonings founded upon these notions, when duly determined and developed. If we once adopt a view of the nature of knowledge which makes necessary truth possible at all, we need be little embarrassed by finding how closely connected different necessary truths are; and how often, in exploring towards their roots, different branches appear to spring from the same stem.

ESSAY IV.

DISCUSSION OF THE QUESTION:—ARE CAUSE AND EFFECT SUCCESSIVE OR SIMULTANEOUS*?

I HAVE at various times laid before this Society dissertations on the metaphysical grounds and elements of our knowledge, and especially on the foundations of the science of Mechanics. As these speculations have not failed to excite some attention, both here and elsewhere, I am tempted to bring forward in the same manner some additional disquisitions of the same kind. Indeed, the immediate occasion of the present memoir is of itself an evidence that such subjects are not supposed to be without their interest for the general reader; for I am led to the views and reasonings which I am now about to lay before the Society, by some remarks in one of our most popular Reviews, (*The Quarterly Review*, Article on the History and Philosophy of the Inductive Sciences. June 1841.) A writer of singular acuteness and comprehensiveness of view has there made remarks upon the doctrines which I had delivered in the "Philosophy of the Inductive Sciences," which remarks appear to me in the highest degree instructive and philosophical. I am not, however, going here to discuss fully the doctrines contained in this critique. With respect to its general tentency, I will only observe, that the author does not accept, in the form in which I had given it, the account of the origin and ground of necessary and universal truths. I had stated that our knowledge is derived from Sensations and Ideas; and that Ideas, which are the conditions of perception, such as *space*, *time*, *likeness*, *cause*, make universal and necessary knowledge possible; whereas, if knowledge were derived from Sensation alone, it could not have those characters. I have moreover enumerated a long series of Fundamental Ideas as the bases of a corresponding series of sciences, of which sciences I have shown also, by an historical survey, that they claim to possess universal truths, and have their claims allowed. I have gone

* From the *Transactions of the Cambridge Philosophical Society*, Vol. VII. Part III. No. 18. [1842.]

further: for I have stated the Axioms which flow from these Fundamental Ideas, and which are the logical grounds of necessity and universality in the truths of each science, when the science is presented in the form of a demonstrated system. The Reviewer does not assent to this doctrine, nor to the argument by which it is supported; namely, that Experience cannot lead to universal truths, except by means of a universal Idea supplied by the mind, and infused into the particular facts which observation ministers. He considers that the existence of universal truths in our knowledge may be explained otherwise. He holds that it is a sufficient account of the matter to say that we pass from special experience to universal truth in virtue of "the inductive propensity—the irresistible impulse of the mind to generalize *ad infinitum*." I shall not here dwell upon very strong reasons which may be assigned, as I conceive, for not accepting this as a full and satisfactory explanation of the difficulty. Instead of doing so, I shall here content myself with remarking, that even if we adopt the Reviewer's expressions, we must still contend that there are *different forms* of the *impulse of the mind to generalize*, corresponding to each of the Fundamental Ideas of our system. These Fundamental Ideas, if they be nothing else, must at least be accepted as a classification of the modes of action of the Inductive Propensity,—as so many different paths and tendencies of the Generalizing Impulse: and the Axioms which I have stated as the express results of the Fundamental Ideas, and as the steps by which those Ideas make universal truths possible, are still no less worthy of notice, if they are stated as the results of our Generalizing Impulse; and as the steps by which that Impulse, in its many various forms, makes universal truths possible. The Generalizing Impulse in that operation by which it leads us to the Axioms of geometry, and to those of mechanics, takes very different courses; and these courses may well deserve to be separately studied. And perhaps, even if we accept this view of the philosophy of our knowledge, no simpler or clearer way can be found of describing and distinguishing these fundamentally different operations of the Inductive Propensity, than by saying, that in the one case it proceeds according to the Idea of Space, in another according to the Idea of Mechanical Cause; and the like phraseology may be employed for all the other cases.

This then being understood, my present object is to consider some very remarkable, and, as appears to me, novel views of the Idea of Cause which the Reviewer propounds. And these may be best brought under our discussion by considering them as an attempt to solve the question, Whether, according to our fundamental apprehensions of the relation of Cause and Effect, effect follows cause in the order of time, or is simultaneous with it.

At first sight, this question may seem to be completely decided by our fundamental convictions respecting cause and effect, and by the axioms which have been propounded by almost all writers, and have obtained universal currency among reasoners on this subject. That the cause must precede the effect,—that the effect must follow the cause,—are, it might seem, self-evident truths, assumed and assented to by all persons in all reasonings in which those notions occur. Such a doctrine is commonly asserted in general terms, and seems to be verified in all the applications of the idea of cause. A heavy body produces motion by its weight; the motion produced is subsequent in time to the pressure which the weight exerts. In a machine, bodies push or strike each other, and so produce a series of motions; each motion, in this case, is the result of the motions and configurations which have preceded it. The whole series of such motions employs time; and this time is filled up and measured by the series of causes and effects, the effects being, in their turn, causes of other effects. This is the common mode of apprehending the universal course of events, in which the chain of causation, and the progress of time, are contemplated as each the necessary condition and accompaniment of the other.

But this, the Critic remarks, is not true in *direct* causation. "If the antecedence and consequence in question be understood as the interposition of an interval of time, however small, between the action of the cause and the production of the effect, we regard it as inadmissible. In the production of motion by force, for instance, though the effect be cumulative with continued exertion of the cause, yet each elementary or individual action is, to our apprehension, *instanter* accompanied with its corresponding increment of momentum in the body moved. In all dynamical reasonings no one has ever thought of interposing an instant of time between the action and its resulting momentum; nor does it appear necessary." This is so evident, that it appears strange it should have the air of novelty; yet, so far as I am aware, the matter has never before been put in the same point of view. But this being the case, the question occurs, how it is that time *seems* to be employed in the progress from cause to effect? How is it that the opinion of the effect being subsequent to the cause has generally obtained? And to this the Critic's answer is obvious:—it is so in cases of indirect or of *cumulative* effect. If a ball *A* strikes another, *B*, and puts it in motion, and *B* strikes *C*, and puts it in motion, *A*'s impact may be considered as the cause, though not the direct cause, of *C*'s motion. Now time, namely the time of *B*'s motion after it is struck by *A*, and before it strikes *C*, intervenes between *A*'s impact and the beginning of *C*'s motion: that is, between the cause and its effect. In this sense, the effect is subsequent to the cause. Again, if a body be put in motion by a series of impulses acting at finite intervals of time, all in the same direction, the motion at the end of all these intervals is the effect of all the impulses, and exists after they have all acted. It is the accumulated effect, and subsequent to each separate action of the cause. But in this case, each impulse produces its effect instantaneously, and the time is employed, not in the transition from any cause to its effect, but in the intervals between the action of the several causes, during which intervals the body goes on with the velocity already communicated to it. In each impulse, force produces motion: and the motion goes on till a new change takes place, by the same kind of action. The force may be said, in the language employed by the Critic, to be transformed into momentum; and in the successive impulses, successive portions of force are thus transformed; while in the intervening intervals, the force thus transformed into momentum is carried by the body from one place to another, where a new change awaits it. "The cause is absorbed and transformed into effect, and therein treasured up." Hence, as the Writer says, "The time lost in cases of indirect physical causation is that consumed in the movements which take place among the parts of the mechanism set in action, by which the active forces so transformed into mechanism are transported over intervals of space to new points of action, the motion of matter in such cases being regarded as a mere carrier of force":—and when force is directly counteracted by force, their mutual destruction must be conceived, as the Reviewer says, to be instantaneous.

We can therefore hardly resist his conclusion, that men have been misled in assuming sequence as a feature in the relation of cause and effect; and we may readily assent to his suggestion, that sequence, when observed, is to be held as a sure indication of indirect action, accompanied with a movement of parts.

But yet if we turn for a moment to other kinds of causation, we seem to be compelled at every step to recognize the truth of the usual maxim upon this subject, that effects are subsequent to causes. Is not poison, taken at a certain moment, the cause of disorder and death which follow at a *subsequent* period? Is not a man's early prudence often the cause of his prosperity in *later* life, and his folly, though for a moment it may produce gratification, *finally* the cause of his ruin? And even in the case of mechanism, if, in a clock which goes rightly, we alter the length of the pendulum, is not this alteration the cause of an alteration which *afterwards* takes place in the rate of the clock's going? Are not all these, and innumerable other cases, instances in which the usual notion of the effect following the cause is verified? and are they not irreconcileable with the new doctrine of cause and effect being simultaneous?

In order to disentangle this apparent confusion, let us first consider the case last mentioned, of a clock, in which some alteration is made which affects the rate of going.

So long as the parts of the clock remain unaltered, its rate will remain unaltered; and any part which is considered as capable of alteration, may be considered as, if we please, the cause of the unaltered rate, by being itself unaltered. But we do not usually introduce the positive idea of cause, to correspond with this negation of change. If we speak of the rate as unaltered, we may also say that it is so because there is *no cause* of alteration. The steady rate is the indication of the absence of any cause of alteration; and the rate of going measures the progress of time, in a state of things in which causes of change are thus excluded. If an alteration takes place in any part of the clock, once for all, the rate is altered; but the new rate is steady as the old rate was, and, like it, measures the uniform progress of time. But the difference between the new rate and the old is occasioned by the difference of the parts of the clock; and the new rate may very properly be said to be caused by the change of the parts, and to be subsequent to it: for it does prevail after the change, and does not prevail before.

But how is this view to be reconciled with the one just quoted from the Reviewer, and, as it appeared, satisfactorily proved by him; according to which all mechanical effects are simultaneous with their causes, and not subsequent to them? We have here the two views in close contact, and in seeming opposition.

In the going of a clock, the parts are in motion; and these motions are determined by forces arising from the form and connexion of the parts of the mechanism. Each of the forces thus exerted at any instant produces its effect at the same instant; and thus, so far as the term *cause* refers to such instantaneous forces, the cause and the effect are simultaneous. But if such instantaneous forces act at successive intervals of time, the motion during each interval is unaltered, and by its uniform progress measures the progress of time. And thus the motion of the machine consists of a series of intervals, during each of which the motion is uniform, and measures the time; separated from each other by a series of changes, at each of which the change measures the instantaneous force, and is simultaneous with it. And if, in this case, we suppose, at any point of time, the instantaneous forces to cease, the succession of them being terminated, from that point of time the motion would be uniform. And since the rate of the motion in each interval of time is determined by the instantaneous force which last acted and by the preceding motion, the rate of the motion in each interval of time is determined by all the preceding instantaneous forces. Hence, when the series of instantaneous forces stops, the rate at which the motion goes on permanently, from that point of time, is determined by the antecedent series of such forces, which series may be considered as an aggregate cause; and hence it appears, that the *permanent* effect is determined by the *aggregate* cause; and in this sense the effect is subsequent to the cause.

Thus we obtain, in this case, a solution of the difficulty which is placed before us. The instantaneous effect or change is simultaneous with the instantaneous force or cause by which it is produced. But if we consider a series of such instantaneous forces as a single aggregate cause, and the final condition as a permanent effect of this cause, the effect is subsequent to the cause. In this case, the cause is immediately succeeded by the effect. The cause acts in time: the effect goes on in time.

The times occupied by the cause and by the effect succeed each other, the one ending at the point of time at which the other begins. But the time which the cause occupies is really composed of a series of instants of uniform motion interposed between instantaneous forces; and during the time that this series of causes is going on, to make up the aggregate cause, a series of effects is going on to make up the final effect. There is a progressive cause and a progressive effect which go on together, and occupy the same finite time; and this simultaneous progression is composed of all the simultaneous instantaneous steps of cause and effect. The aggregate cause is the sum of the progression of causes; the final effect is the last term of the progression of effects. At each step, as the Reviewer says, cause is transformed into effect; and it is treasured up in the results during the intermediate intervals; and the time occupied is not the time which intervenes between cause and effect at each step, but the time which intervenes beween these transformations.

I have supposed forces to act at distinct instants, and to cease to act in the intervals between; and then, the aggregate of such intervals to make up a finite time, during which an aggregate force acts. But if the action of the force be rigorously continuous, it will easily be seen that all the consequences as to cause and effect will be the same; the discontinuous action being merely the usual artifice by which, in mathematical reasonings, we obtain results respecting continuous changes. It will still be true, that the uniform motion which takes place after a continuous force has acted, is the effect subsequent to the cause; while the change which takes place at any instant by the action of the force, is the instantaneous effect simultaneous with the cause.

It may be objected, that this solution does not appear immediately to apply: for the motion of a clock is not uniform during any portion of the time. The parts move by intervals of varied motion and of rest; or by oscillations backwards and forwards; and the succession of forces which acts during any oscillation, or any cycle of motion, is repeated during the succeeding oscillation or cycle, and so on indefinitely; and if an alteration be made in the parts, it is not a change once for all, but recurs in its operation in every cycle of the motion.

But it will be found that this circumstance does not prevent the same explanation from being still applicable with a slight modification. Instead of uniform motion in the intervals of causation, we shall have to speak of *steady going:* and instead of considering all the forces which affect the motion as causes of change of uniform motion, we shall have to speak of changes in the parts of the mechanism as causes of *change of rate of going.* With this modification, it will still be true, that any instantaneous cause produces its instantaneous effect simultaneously, while the permanent effect is subsequent to the change which is its cause. The steady going of the clock is assumed as a normal condition, in which it measures the progress of time; and in this assumption, the notion of cause and effect is not brought into view. But a steady rate thus denoting the mean passage of time, a change in the rate indicates a cause of change. The *change of rate,* as an instantaneous *transition* from one rate to another, is *simultaneous* with the change in the parts. But then the *changed rate* as a continued *condition* in which, no new change supervening, the rate again measures the progress of time, is *subsequent* to the change of parts, for it begins when that ends, and continues when the progress of that has ceased.

If, however, this be a satisfactory solution of the difficulty in the case of mechanism, how shall we apply the same views to the other cases? Growth, the effect of food, is subsequent to the act of taking food; disorder, the effect of poison, is subsequent to the introduction of poison into the system. Can we say that the animal would continue unchanged if it were not to take food; and that food is the cause of a change, namely, of growth? This is manifestly false; for if the animal were not to take food, it would soon perish. But the analogy of the former case, of the clock, will enable us to avoid this perplexity. As we assumed a steady rate of going in the clock to be the measure of time when we considered the effect of mechanism, so we assume a steady rate of action in the animal functions to be the measure of the progress of time when we consider the causes which act upon the development and health of animals. Digestion, and of course nutrition, are a part of this normal condition; they are involved in the steady going of the animal mechanism, and we must suppose these functions to go regularly on, in order that the animal may preserve its character of animal. Food and digestion may be considered as causes of the continued existence of the animal, in the same way in which the form of the parts of a clock is the cause of the steady going of a clock. And when we come to consider causes of change, this kind of causation, which produces a normal condition of things, merely measuring the flow of time, is left out of our account. We can conceive an uniform condition of animal existence, the animal neither growing nor wasting. This being taken as the normal condition, any deviation from this condition indicates a cause, and is taken as the evidence and measure of the cause of change. And thus, in a growing animal, the food partly keeps the animal in continued animal existence, and partly, and in addition to this, causes its growth. Food, in the former view, is always circulating in the system, and is supposed to be uniformly administered; the cycles of nutrition being merged in the notion of uniform existence, as the oscillations of the pendulum in a clock are merged in tho notion of uniform going; and the elementary steps of nutrition which are, in this view, supposed to take place at each instant, produce their instantaneous effect, for they are requisite in the cycle of animal processes which goes on from instant to instant. But on the other hand, in considering growth, we compare the state of an animal with a preceding state, and consider the nutriment taken in the intervening time as the cause of the change: hence this nutriment, as an aggregate, is considered as the cause of growth of the animal; and in this view the effect is subsequent to the cause. But yet here, as in the case of mechanism, the progressive effect is simultaneous, step by step, with the progressive cause. There is a series of operations; as for instance, intussusception, digestion, assimilation, growth: each of these is a progressive operation; and in the progress of each operation, the steps of the effect and the instantaneous forces are simultaneous. But the end of one operation is the beginning of the next, or at least in part, and hence we have time occupied by the succession. The end of intussusception is the beginning of digestion, the end of digestion the beginning of assimilation, and so on. These aggregate effects suceeed each other; and hence growth is subsequent to the taking of food; though each instantaneous force of animal life, no less than of mechanism, produces an effect simultaneous with its action. Each of these separate operations is an aggregate operation, and occupies time; and each aggregate effect is a condition of the action of the cause in the next operation.

Again; if an animal in a permanent condition, neither waxing nor wasting, may be taken as the normal state in which the functions of life measure time, in order that we may consider growth as an effect, to be referred to food as cause; we may, for other purposes, consider, as the normal condition, an animal waxing and then wasting, according to the usual law of animal life: and we must take this, the healthy progress of an animal, as our normal condition, if we have to consider causes which produce disease. If we have to refer the morbid condition of an animal to the influence of poison, for example, we must consider how far the condition deviates from what it would have been if the poison had not been taken into the frame. The usual progress of the animal functions including its growth, is the measure of *time;* the deviation from this usual progress is the indication of *cause;* and the effect of the poison is subsequent to the cause, because the poison acts through the cycle of the animal functions just mentioned, which occupies time; and because the taking the poison into the system, not any subsequent action of the animal forces in the system, is considered as the event which we must contemplate as a cause. To resume the analogy of the clock: the rate of the clock is altered by altering the parts; but this alteration itself may occupy time; as if we alter the rate of a clock by applying a drop of acid, which gradually eats off a part of the pendulum, the corrosion, as an aggregate effect, occupies time; and the rates before and after the change are separated by this time. But the application of the drop is the cause; and thus, in this case the final effect is subsequent to the cause, though here, as in the case of mechanism, the instantaneous forces always produce a simultaneous effect.

Thus we have in every case a *uniform* state, or a state which is considered as uniform, or at least *normal;* and which is taken as the indication and measure of *time;* and we have also *change,* which is contemplated as a deviation from uniformity, and is taken as the indication and measure of *cause.* The uniform state may be one which never exists, being purely imaginary; as the case in which no forces act; and the case in which animal functions go on permanently, the animal neither growing nor wasting. The normal state may also be a state in which change is constantly taking place, as, in fact, even a state of motion is a state of change; such states also are, in a further sense, that of a clock going by starts, and that of an animal constantly growing: in these cases the changes are all merged in a wider view of uniformity, so that these are taken as the normal states. And in all these cases, successive changes which take place are separated by intervals of time, measured by the normal progress; and each change is produced by some *simultaneous* instantaneous cause. But taking the cause in a larger sense, we group these instantaneous causes, and perhaps omit in our contemplation some of the intervening intervals; and thus assign the cause to a *preceding*, and the effect to a *succeeding* time.

I may observe further, as a corollary from what has been said, that the measure of time is different, when we consider different kinds of causation; and in each case, is *homogeneous* with the changes which causation effects. In the consideration of mechanical causes, we measure time by mechanical changes;—by uniform motion, or uniform succession of cycles of motion; by the rotation of a wheel, or the oscillation of a pendulum. But if we have to consider physiological changes, the progress of time is physiologically measured;—by the normal progress of vital operations; by the circulation, digestion or development of the organized body; by the pulse, or by the growth. These different measures of time give to time, so far as it is exhibited by facts and events, a different character in the different cases. Phenomenal time has a different nature and essence according to the kind of the changes which we consider, and which gives us our sole phenomenal indication of cause.

I fear that I am travelling into matters too abstruse and metaphysical for the occasion: but before I conclude, I will present one other aspect of the subject.

In stating the difficulty, I referred to cases of moral as well as physical causation; as when prudence produces prosperity, or when folly produces ruin. It may be asked, whether we are here to apply the same explanation;—whether we are to assume a normal condition of human existence, in which neither prudence nor folly are displayed, neither prosperity nor adversity produced;—whether we are to conceive the progress of such a state to measure the progress of time, and deviations from it to denote causes of the kind mentioned. It may be asked further, whether, if we do make this supposition, we can resolve the influence of such causes as prudence or imprudence into instantaneous acts, which produce their effects immediately: and which occupy time only by being separated by intervals of the inactive normal moral condition. To this I must here reply, that the discussion of such questions would carry me too far, and would involve speculations not included within the acknowledged domain of this Society, from which I therefore abstain. But I may say, before quitting the subject, that I do not think the suppositions above suggested are untenable; and that in order to include moral causation under the maxims of causation in general, we must necessarily make some such hypothesis. The peculiarity of that kind of causation which the will and the character exert, and which is exerted upon the will and the character, would make this case far more complex and difficult than those already considered; but, at the same time, would offer us the means of explaining what may seem harsh, in the above analogy. For instance, we should have to assume such a maxim as this: that in moral causation, time is not to be measured by the flow of mechanical or physiological events;—not by the clock, or by the pulse. Moral causation has its own clock, its own pulse, in the progress of man's moral being; and by this measure of time is the relation of moral cause and effect to be defined.

That in estimating moral causation, the progress of time is necessarily estimated by moral changes, and not by machinery,—by the progress of events, and not by the going of the clock,—is a truth familiar as a practical maxim to all who give their thoughts to dramatic or narrative fictions. Who feels any thing incongruous or extravagantly hurried in the progress of events in that great exhibition of moral causation, the tragedy of Othello? If we were asked what time those vast and terrible and complex changes of the being and feelings of the characters occupy, we should say, that, measured on its own scale, the event is of great extent;—that the transaction is of considerable magnitude in all ways. But if, with previous critics, we look into the progress of time by the day and the hour—what is the measure of this history? Forty-eight hours.

But I am going beyond the boundaries of the speculations which we usually follow in this room, and will conclude.

ESSAY V.

ON THE FUNDAMENTAL ANTITHESIS OF PHILOSOPHY*.

I HAVE upon former occasions laid before the Society dissertations on certain questions which may be termed metaphysical:—on the nature of the truth of the laws of motion:—on the question whether all matter is heavy:—and on the question whether cause and effect are successive or simultaneous. As these dissertations have not failed to excite some interest, I hope that I shall have the indulgence of the Society in making a few remarks on another question of the same kind. In doing this, as my object is to throw some light if possible on a matter of considerable obscurity and difficulty, I shall not attempt to avoid the occasional repetition of a sentence or two which I may have, in substance, delivered elsewhere.

1. All persons who have attended in any degree to the views generally current of the nature of reasoning are familiar with the distinction of necessary truths and truths of experience; and few such persons, or at least few students of mathematics, require to have this distinction explained or enforced. All geometricians are satisfied that the geometrical truths with which they are conversant are necessarily true: they not only *are* true, but they *must be* true. The meaning of the terms being understood, and the proof being gone through, the truth of the proposition must be assented to. That parallelograms upon the same base and between the same parallels are equal;—that angles in the same segment are equal;—these are propositions which we learn to be true by demonstrations deduced from definitions and axioms; and which, when we have thus learnt them, we see could not be otherwise. On the other hand, there are other truths which we learn from experience; as for instance, that the stars revolve round the pole in one day; and that the moon goes through her phases from full to full again in thirty days. These truths we see to be true; but we know them only by experience. Men never could have dis-

* From the *Transactions of the Cambridge Philosophical Society*, Vol. VIII. Part II. No. 14. [1844].

covered them without looking at the stars and the moon; and having so learnt them, still no one will pretend to say that they are necessarily true. For aught we can see, things might have been otherwise; and if we had been placed in another part of the solar system, then, according to the opinions of astronomers, experience would have presented them otherwise.

2. I take the astronomical truths of experience to contrast with the geometrical necessary truths, as being both of a familiar definite sort; we may easily find other examples of both kinds of truth. The truths which regard numbers are necessary truths. It is a necessary truth, that 27 and 38 are equal to 65; that half the sum of two numbers added to half their difference is equal to the greater number. On the other hand, that sugar will dissolve in water; that plants cannot live without light; and in short, the whole body of our knowledge in chemistry, physiology, and the other inductive sciences, consists of truths of experience. If there be any science which offer to us truths of an ambiguous kind, with regard to which we may for a moment doubt whether they are necessary or experiential, we will defer the consideration of them till we have marked the distinction of the two kinds more clearly.

3. One mode in which we may express the difference of necessary truths and truths of experience, is, that necessary truths are those of which we cannot distinctly conceive the contrary. We can very readily conceive the contrary of experiential truths. We can conceive the stars moving about the pole or across the sky in any kind of curves with any velocities; we can conceive the moon always appearing during the whole month as a luminous disk, as she might do if her light were inherent and not borrowed. But we cannot conceive one of the parallelograms on the same base and between the same parallels larger than the other; for we find that, if we attempt to do this, when we separate the parallelograms into parts, we have to conceive one triangle larger than another, both having all their parts equal; which we cannot conceive at all, if we conceive the triangles distinctly. We make this impossibility more clear by conceiving the triangles to be placed so that two sides of the one coincide with two sides of the other; and it is then seen, that in order to conceive the triangles unequal, we must conceive the two bases which have the same extremities both ways, to be different lines, though both

straight lines. This it is impossible to conceive: we assent to the impossibility as an axiom, when it is expressed by saying, that two straight lines cannot inclose a space; and thus we cannot distinctly conceive the contrary of the proposition just mentioned respecting parallelograms.

4. But it is necessary, in applying this distinction, to bear in mind the terms of it;—that we cannot *distinctly* conceive the contrary of a necessary truth. For in a certain loose, indistinct way, persons conceive the contrary of necessary geometrical truths, when they erroneously conceive false propositions to be true. Thus, Hobbes erroneously held that he had discovered a means of geometrically doubling the cube, as it is called, that is, finding two mean proportionals between two given lines; a problem which cannot be solved by plane geometry. Hobbes not only proposed a construction for this purpose, but obstinately maintained that it was right, when it had been proved to be wrong. But then, the discussion showed how indistinct the geometrical conceptions of Hobbes were; for when his critics had proved that one of the lines in his diagram would not meet the other in the point which his reasoning supposed, but in another point near to it; he maintained, in reply, that one of these points was large enough to include the other, so that they might be considered as the same point. Such a mode of conceiving the opposite of a geometrical truth, forms no exception to the assertion, that this opposite cannot be distinctly conceived.

5. In like manner, the indistinct conceptions of children and of rude savages do not invalidate the distinction of necessary and experiential truths. Children and savages make mistakes even with regard to numbers; and might easily happen to assert that 27 and 38 are equal to 63 or 64. But such mistakes cannot make such arithmetical truths cease to be necessary truths. When any person conceives these numbers and their addition distinctly, by resolving them into parts, or in any other way, he sees that their sum is necessarily 65. If, on the ground of the possibility of children and savages conceiving something different, it be held that this is not a necessary truth, it must be held on the same ground, that it is not a necessary truth that 7 and 4 are equal to 11; for children and savages might be found so unfamiliar with numbers as not to reject the assertion that 7 and 4 are 10, or even that 4 and 3 are 6, or 8.

But I suppose that no persons would on such grounds hold that these arithmetical truths are truths known only by experience.

6. Necessary truths are established, as has already been said, by demonstration, proceeding from definitions and axioms, according to exact and rigorous inferences of reason. Truths of experience are collected from what we see, also according to inferences of reason, but proceeding in a less exact and rigorous mode of proof. The former depend upon the relations of the ideas which we have in our minds: the latter depend upon the appearances or phenomena, which present themselves to our senses. Necessary truths are formed from our thoughts, the elements of the world within us; experiential truths are collected from things, the elements of the world without us. The truths of experience, as they appear to us in the external world, we call Facts; and when we are able to find among our ideas a train which will conform themselves to the apparent facts, we call this a Theory.

7. This distinction and opposition, thus expressed in various forms; as Necessary and Experiential Truth, Ideas and Senses, Thoughts and Things, Theory and Fact, may be termed the *Fundamental Antithesis of Philosophy*; for almost all the discussions of philosophers have been employed in asserting or denying, explaining or obscuring this antithesis. It may be expressed in many other ways; but is not difficult, under all these different forms, to recognize the same opposition: and the same remarks apply to it under its various forms, with corresponding modifications. Thus, as we have already seen, the antithesis agrees with that of Reasoning and Observation: again, it is identical with the opposition of Reflection and Sensation: again, sensation deals with Objects; facts involve Objects, and generally all things without us are Objects:—Objects of sensation, of observation. On the other hand, we ourselves who thus observe objects, and in whom sensation is, may be called the Subjects of sensation and observation. And this distinction of Subject and Object is one of the most general ways of expressing the fundamental antithesis, although not yet perhaps quite familiar in English. I shall not scruple however to speak of the Subjective and Objective element of this antithesis, where the expressions are convenient.

8. All these forms of antithesis, and the familiar references to them which men make in all discussions, shew the fundamental

and necessary character of the antithesis. We can have no knowledge without the union, no philosophy without the separation, of the two elements. We can have no knowledge, except we have both impressions on our senses from the world without, and thoughts from our minds within:—except we attend to things, and to our ideas;—except we are passive to receive impressions, and active to compare, combine, and mould them. But on the other hand, philosophy seeks to distinguish the impressions of our senses from the thoughts of our minds;—to point out the difference of ideas and things;—to separate the active from the passive faculties of our being. The two elements, sensations and ideas, are both requisite to the existence of our knowledge, as both matter and form are requisite to the existence of a body. But philosophy considers the matter and the form separately. The properties of the form are the subject of geometry, the properties of the matter are the subject of chemistry or mechanics.

9. But though philosophy considers these elements of knowledge separately, they cannot really be separated, any more than can matter and form. We cannot exhibit matter without form, or form without matter; and just as little can we exhibit sensations without ideas, or ideas without sensations;—the passive or the active faculties of the mind detached from each other.

In every act of my knowledge, there must be concerned the things whereof I know, and thoughts of me who know: I must both passively receive or have received impressions, and I must actively combine them and reason on them. No apprehension of things is purely ideal: no experience of external things is purely sensational. If they be conceived as *things*, the mind must have been awoke to the conviction of things by sensation: if they be *conceived* as things, the expressions of the senses must have been bound together by conceptions. If we *think* of any *thing*, we must recognize the existence both of thoughts and of things. *The fundamental antithesis of philosophy is an antithesis of inseparable elements.*

10. Not only cannot these elements be separately exhibited, but they cannot be separately conceived and described. The description of them must always imply their relation; and the names by which they are denoted will consequently always bear a relative significance. And thus *the terms which denote the*

fundamental antithesis of philosophy cannot be applied absolutely and exclusively in any case. We may illustrate this by a consideration of some of the common modes of expressing the antithesis of which we speak. The terms Theory and Fact are often emphatically used as opposed to each other: and they are rightly so used. But yet it is impossible to say absolutely in any case, This is a Fact and not a Theory; this is a Theory and not a Fact, meaning by Theory, true Theory. Is it a fact or a theory that the stars appear to revolve round the pole? Is it a fact or a theory that the earth is a globe revolving round its axis? Is it a fact or a theory that the earth revolves round the sun? Is it a fact or a theory that the sun attracts the earth? Is it a fact or a theory that a loadstone attracts a needle? In all these cases, some persons would answer one way and some persons another. A person who has never watched the stars, and has only seen them from time to time, considers their circular motion round the pole as a theory, just as he considers the motion of the sun in the ecliptic as a theory, or the apparent motion of the inferior planets round the sun in the zodiac. A person who has compared the measures of different parts of the earth, and who knows that these measures cannot be conceived distinctly without supposing the earth a globe, considers its globular form a fact, just as much as the square form of his chamber. A person to whom the grounds of believing the earth to revolve round its axis and round the sun, are as familiar as the grounds for believing the movements of the mail-coaches in this country, conceives the former events to be facts, just as steadily as the latter. And a person who, believing the fact of the earth's annual motion, refers it distinctly to its mechanical course, conceives the sun's attraction as a fact, just as he conceives as a fact the action of the wind which turns the sails of a mill. We see then, that in these cases we cannot apply absolutely and exclusively either of the terms, Fact or Theory. Theory and Fact are the elements which correspond to our Ideas and our Senses. The Facts are Facts so far as the Ideas have been combined with the sensations and absorbed in them: the Theories are Theories so far as the Ideas are kept distinct from the sensations, and so far as it is considered as still a question whether they can be made to agree with them. A true Theory is a fact, a Fact is a familiar theory.

In like manner, if we take the terms Reasoning and Observation; at first sight they appear to be very distinct. Our observation of the world without us, our reasonings in our own minds, appear to be clearly separated and opposed. But yet we shall find that we cannot apply these terms absolutely and exclusively. I see a book lying a few feet from me: is this a matter of observation? At first, perhaps, we might be inclined to say that it clearly is so. But yet, all of us, who have paid any attention to the process of vision, and to the mode in which we are enabled to judge of the distance of objects, and to judge them to be distant objects at all, know that this judgment involves inferences drawn from various sensations;—from the impressions on our two eyes;—from our muscular sensations; and the like. These inferences are of the nature of reasoning, as much as when we judge of the distance of an object on the other side of a river by looking at it from different points, and stepping the distance between them. Or again: we observe the setting sun illuminate a gilded weathercock; but this is as much a matter of reasoning as when we observe the phases of the moon, and infer that she is illuminated by the sun. All observation involves inferences, and inference is reasoning.

11. Even the simplest terms by which the antithesis is expressed cannot be separated: ideas and sensations, thoughts and things, subject and object, cannot in any case be applied absolutely and exclusively. Our sensations require ideas to bind them together, namely, ideas of space, time, number, and the like. If not so bound together, sensations do not give us any apprehension of things or objects. All things, all objects, must exist in space and in time—must be one or many. Now space, time, number, are not sensations or things. They are something different from, and opposed to sensations and things. We have termed them ideas. It may be said they are *relations* of things, or of sensations. But granting this form of expression, still a *relation* is not a thing or a sensation; and therefore we must still have another and opposite element, along with our sensations. And yet, though we have thus these two elements in every act of perception, we cannot designate any portion of the act as absolutely and exclusively belonging to one of the elements. Perception involves sensation, along with ideas of time, space, and the like; or, if any one prefers the expression, we may say, Perception involves sensations along with the apprehension of relations. Perception is sensation, along with

such ideas as make sensation into an apprehension of things or objects.

12. And as perception of objects implies ideas,—as observation implies reasoning;—so, on the other hand, ideas cannot exist where sensation has not been: reasoning cannot go on when there has not been previous observation. This is evident from the necessary order of development of the human faculties. Sensation necessarily exists from the first moments of our existence, and is constantly at work. Observation begins before we can suppose the existence of any reasoning which is not involved in observation. Hence, at whatever period we consider our ideas, we must consider them as having been already engaged in connecting our sensations, and as having been modified by this employment. By being so employed, our ideas are unfolded and defined; and such development and definition cannot be separated from the ideas themselves. We cannot conceive space without boundaries or forms; now forms involve sensations. We cannot conceive time without events which mark the course of time; but events involve sensations. We cannot conceive number without conceiving things which are numbered; and things imply sensations. And the forms, things, events, which are thus implied in our ideas, having been the objects of sensation constantly in every part of our life, have modified, unfolded and fixed our ideas, to an extent which we cannot estimate, but which we must suppose to be essential to the processes which at present go on in our minds. We cannot say that objects create ideas; for to perceive objects we must already have ideas. But we may say, that objects and the constant perception of objects have so far modified our ideas, that we cannot, even in thought, separate our ideas from the perception of objects.

We cannot say of any ideas, as of the idea of space, or time, or number, that they are absolutely and exclusively ideas. We cannot conceive what space, or time, or number would be in our minds, if we had never perceived any thing or things in space or time. We cannot conceive ourselves in such a condition as never to have perceived any thing or things in space or time. But, on the other hand, just as little can we conceive ourselves becoming acquainted with space and time or numbers as objects of sensation. We cannot reason without having the operations of our minds affected by previous sensations; but we cannot conceive reasoning to be merely a series of sensations. In order to be used in reasoning, sensation must become observation; and, as we have seen, observation already involves Reasoning. In order to be connected by our ideas, sensations must be Things or objects, and things or objects already include ideas. And thus none of the terms by which the fundamental antithesis is expressed can be absolutely and exclusively applied.

13. I will make a remark suggested by the views which have thus been presented. Since, as we have just seen, none of the terms which express the fundamental antithesis can be applied absolutely and exclusively, the absolute application of the antithesis in any particular case can never be a conclusive or immoveable principle. This remark is the more necessary to be borne in mind, as the terms of this antithesis are often used in a vehement and peremptory manner. Thus we are often told that such a thing is a *Fact* and not a Theory, with all the emphasis which, in speaking or writing, tone or italics or capitals can give. We see from what has been said, that when this is urged, before we can estimate the truth, or the value of the assertion, we must ask to whom is it a fact? what habits of thought, what previous information, what Ideas does it imply, to conceive the fact as a fact? Does not the apprehension of the fact imply assumptions which may with equal justice be called theory, and which are perhaps false theory? in which case, the fact is no fact. Did not the ancients assert it as a Fact, that the earth stood still, and the stars moved? and can any Fact have stronger apparent evidence to justify persons in asserting it emphatically than this had? These remarks are by no means urged in order to shew that no Fact can be certainly known to be true; but only to shew that no Fact can be certainly shown to be a Fact merely by calling it a Fact, however emphatically. There is by no means any ground of general skepticism with regard to truth involved in the doctrine of the necessary combination of two elements in all our knowledge. On the contrary, Ideas are requisite to the essence, and Things to the reality of our knowledge in every case. The proportions of Geometry and Arithmetic are examples of knowledge respecting our Ideas of space and number, with regard to which there is no room for doubt. The doctrines of Astronomy are examples of truths not less certain respecting the Facts of the external world.

14. I remark further, that since in every act of knowledge, observation or perception, both the elements of the fundamental antithesis are involved, and involved in a manner inseparable even in our conceptions, it must always be possible to derive one of these elements from the other, if we are satisfied to accept, as proof of such derivation, that one always co-exists with and implies the other. Thus an opponent may say, that our ideas of space, time, and number, are derived from our sensations or perceptions, because we never were in a condition in which we had the ideas of space and time, and had not sensations or perceptions. But then, we may reply to this, that we no sooner perceive objects than we perceive them as existing in space and time, and therefore the ideas of space and time are not derived from the perceptions. In the same manner, an opponent may say, that all knowledge which is involved in our reasonings is the result of experience; for instance, our knowledge of geometry. For every geometrical principle is presented to us by experience as true; beginning with the simplest, from which all others are derived by processes of exact reasoning. But to this we reply, that experience cannot be the origin of such knowledge; for though experience shows that such principles are true, it cannot show that they *must be* true, which we also know. We never have seen, as a matter of observation, two straight lines inclosing a space; but we venture to say further, without the smallest hesitation, that we never shall see it; and if any one were to tell us that, according to his experience, such a form was often seen, we should only suppose that he did not know what he was talking of. No number of acts of experience can add to the certainty of our knowledge in this respect; which shows that our knowledge is not made up of acts of experience. We cannot test such knowledge by experience; for if we were to try to do so, we must first know that the lines with which we make the trial *are* straight; and we have no test of straightness better than this, that two such lines cannot inclose a space. Since then, experience can neither destroy, add to, nor test our axiomatic knowledge, such knowledge cannot be derived from experience. Since no one act of experience can affect our knowledge, no numbers of acts of experience can make it.

15. To this a reply has been offered, that it is a characteristic property of geometric forms that the ideas of them exactly resemble the sensations; so that these ideas are as fit

subjects of experimentation as the realities themselves; and that by such experimentation we learn the truth of the axioms of geometry. I might very reasonably ask those who use this language to explain how a particular class of ideas can be said to resemble sensations; how, if they do, we can know it to be so; how we can prove this resemblance to belong to geometrical ideas and sensations; and how it comes to be an especial characteristic of those. But I will put the argument in another way. Experiment can only show what is, not what must be. If experimentation on ideas shows what must be, it is different from what is commonly called experience.

I may add, that not only the mere use of our senses cannot show that the axioms of geometry *must be* true, but that, without the light of our ideas, it cannot even show that they *are* true. If we had a segment of a circle a mile long and an inch wide, we should have two lines inclosing a space; but we could not, by seeing or touching any part of either of them, discover that it was a bent line.

16. That mathematical truths are not derived from experience is perhaps still more evident, if greater evidence be possible, in the case of numbers. We assert that 7 and 8 are 15. We find it so, if we try with counters, or in any other way. But we do not, on that account, say that the knowledge is derived from experience. We refer to our conceptions of seven, of eight, and of addition, and as soon as we possess these conceptions distinctly, we see that the sum must be fifteen. We cannot be said to make a trial, for we should not believe the apparent result of the trial if it were different. If any one were to say that the multiplication table is a table of the results of experience, we should know that he could not be able to go along with us in our researches into the foundations of human knowledge; nor, indeed, to pursue with success any speculations on the subject.

17. Attempts have also been made to explain the origin of axiomatic truths by referring them to the "association of ideas." But this is one of the cases in which the word *association* has been applied so widely and loosely, that no sense can be attached to it. Those who have written with any degree of distinctness on the subject, have truly taught, that the habitual association of the Ideas leads us to believe a connexion of the Things: but they have never told us that this association gave us the power

of forming the ideas. Association may determine belief, but it cannot determine the possibility of our conceptions. The African king did not believe that water could become solid, because he had never seen it in that state. But that accident did not make it impossible to conceive it so, any more than it is impossible for us to conceive frozen quicksilver, or melted diamond, or liquefied air; which we may never have seen, but have no difficulty in conceiving. If there were a tropical philosopher really incapable of conceiving water solidified, he must have been brought into that mental condition by abstruse speculations on the necessary relations of solidity and fluidity, not by the association of ideas.

18. To return to the results of the nature of the Fundamental Antithesis. As by assuming universal and indissoluble connexion of ideas with perceptions, of knowledge with experience, as an evidence of derivation, we may assert the former to be derived from the latter, so might we, on the same ground, assert the latter to be derived from the former. We see all forms in space; and we might hence assert all forms to be mere modifications of our idea of space. We see all events happen in time; and we might hence assert all events to be merely limitations and boundary-marks of our idea of time. We conceive all collections of things as two or three, or some other number: it might hence be asserted that we have an original idea of number, which is reflected in external things. In this case, as in the other, we are met at once by the impossibility of this being a complete account of our knowledge. Our ideas of space, of time, of number, however distinctly reflected to us with limitations and modifications, must be reflected, limited and modified by something different from themselves. We must have visible or tangible forms to limit space, perceived events to mark time, distinguishable objects to exemplify number. But still, in forms, and events, and objects, we have a knowledge which they themselves cannot give us. For we know, without attending to them, that whatever they are, they will conform and must conform to the truths of geometry and arithmetic. There is an ideal portion in all our knowledge of the external world; and if we were resolved to reduce all our knowledge to one of its two antithetical elements, we might say that all our knowledge consists in the relation of our ideas. Wherever there is necessary truth, there must be something more than sensation can supply: and the

necessary truths of geometry and arithmetic show us that our knowledge of objects in space and time depends upon necessary relations of ideas, whatever other element it may involve.

19. This remark may be carried much further than the domain of geometry and arithmetic. Our knowledge of matter may at first sight appear to be altogether derived from the senses. Yet we cannot derive from the senses our knowledge of a truth which we accept as universally certain;—namely, that we cannot by any process add to or diminish the quantity of matter in the world. This truth neither is nor can be derived from experience; for the experiments which we make to verify it pre-suppose its truth. When the philosopher was asked what was the weight of smoke, he bade the inquirer subtract the weight of the ashes from the weight of the fuel. Every one who thinks clearly of the changes which take place in matter, assents to the justice of this reply: and this, not because any one had found by trial that such was the weight of the smoke produced in combustion, but because the weight lost was assumed to have gone into some other form of matter, not to have been destroyed. When men began to use the balance in chemical analysis, they did not prove by trial, but took for granted, as self-evident, that the weight of the whole must be found in the aggregate weight of the elements. Thus it is involved in the idea of matter that its amount continues unchanged in all changes which takes places in its consistence. This is a necessary truth: and thus our knowledge of matter, as collected from chemical experiments, is also a modification of our idea of matter as the material of the world incapable of addition or diminution.

20. A similar remark may be made with regard to the mechanical properties of matter. Our knowledge of these is reduced, in our reasonings, to principles which we call the laws of motion. These laws of motion, as I have endeavoured to shew in a paper already printed by the Society, depend upon the idea of Cause, and involve necessary truths, which are necessarily implied in the idea of cause;—namely, that every change of motion must have a cause—that the effect is measured by the cause;—that re-action is equal and opposite to action. These principles are not derived from experience. No one, I suppose, would derive from experience the principle, that every event must have a cause. Every attempt to see the traces of cause in the world assumes this principle. I do not say that these

principles are anterior to experience; for I have already, I hope, shewn, that neither of the two elements of our knowledge is, or can be, anterior to the other. But the two elements are co-ordinate in the development of the human mind; and the ideal element may be said to be the origin of our knowledge with the more propriety of the two, inasmuch as our knowledge is the relation of ideas. The other element of knowledge, in which sensation is concerned, and which embodies, limits, and defines the necessary truths which express the relations of our ideas, may be properly termed Experience; and I have, in the Memoir just quoted, endeavoured to shew how the Principles concerning mechanical causation, which I have just stated, are, by observation and experiment, limited and defined, so that they become the Laws of Motion. And thus we see that such knowledge is derived from ideas, in a sense quite as general and rigorous, to say the least, as that in which it is derived from experience.

21. I will take another example of this; although it is one less familiar, and the consideration of it perhaps a little more difficult and obscure. The objects which we find in the world, for instance, minerals and plants, are of different kinds; and according to their kinds, they are called by various names, by means of which we know what we mean when we speak of them. The discrimination of these kinds of objects, according to their different forms and other properties, is the business of chemistry and botany. And this business of discrimination, and of consequent classification, has been carried on from the first periods of the development of the human mind, by an industrious and comprehensive series of observations and experiments; the only way in which any portion of the task could have been effected. But as the foundation of all this labour, and as a necessary assumption during every part of its progress, there has been in men's minds the principle, that objects are so distinguishable by resemblances and differences, that they may be named, and known by their names. This principle is involved in the idea of a Name; and without it no progress could have been made. The principle may be briefly stated thus:—Intelligible Names of Kinds are possible. If we suppose this not to be so, language can no longer exist, nor could the business of human life go on. If instead of having certain definitekinds of minerals, gold, iron, copper, and the like, of which the external forms and characters

are constantly connected with the same properties and qualities, there were no connexion between the appearance and the properties of the object;—if what seemed externally iron might turn out to resemble lead in its hardness; and what seemed to be gold during many trials, might at the next trial be found to be like copper; not only all the uses of these minerals would fail, but they would not be distinguishable kinds of things, and the names would be unmeaning. And if this entire uncertainty as to kind and properties prevailed for all objects, the world would no longer be a world to which language was applicable. To man, thus unable to distinguish objects into kinds, and call them by names, all knowledge would be impossible, and all definite apprehension of external objects would fade away into an inconceivable confusion. In the very apprehension of objects as intelligibly sorted, there is involved a principle which springs within us, contemporaneous, in its efficacy, with our first intelligent perception of the kinds of things of which the world consists. We assume, as a necessary basis of our knowledge, that things are of definite kinds; and the aim of chemistry, botany, and other sciences is, to find marks of these kinds; and along with these, to learn their definitely-distinguished properties. Even here, therefore, where so large a portion of our knowledge comes from experience and observation, we cannot proceed without a necessary truth derived from our ideas, as our fundamental principle of knowledge.

22. What the Marks are, which distinguish the constant differences of Kinds of things (definite marks, selected from among many unessential appearances), and what their definite properties are, when they are so distinguished, are parts of our knowledge to be learnt from observation, by various processes; for instance, among others, by chemical analysis. We find the differences of bodies, as shown by such analysis, to be of this nature:—that there are various elementary bodies, which, combining in different definite proportions, form kinds of bodies definitely different. But, in arriving at this conclusion, we introduce a new idea, that of Elementary Composition, which is not extracted from the phenomena, but supplied by the mind, and introduced in order to make the phenomena intelligible. That this notion of elementary composition is not supplied by the chemical phenomena of combustion, mixture, &c. as merely an observed fact, we see from this; that men had in ancient

times performed many experiments in which elementary composition was concerned, and had not seen the fact. It never was truly seen till modern times; and when seen, it gave a new aspect to the whole body of known facts. This Idea of Elementary Composition, then, is supplied by the mind, in order to make the facts of chemical analysis and synthesis intelligible *as* analysis and synthesis. And this idea being so supplied, there enters into our knowledge along with it a corresponding necessary principle;—That the elementary composition of a body determines its kind and properties. This is, I say, a principle assumed, as a consequence of the idea of composition, not a result of experience; for when bodies have been divided into their kinds, we take for granted that the analysis of a single specimen may serve to determine the analysis of all bodies of the same kind: and without this assumption, chemical knowledge with regard to the kinds of bodies would not be possible. It has been said that we take only one experiment to determine the composition of any particular kind of body, because we have a thousand experiments to determine that bodies of the same kind have the same composition. But this is not so. Our belief in the principle that bodies of the same kind have the same composition is not established by experiments, but is assumed as a necessary consequence of the ideas of Kind and of Composition. If, in our experiments, we found that bodies supposed to be of the same kind had not the same composition, we should not at all doubt of the principle just stated, but conclude at once that the bodies were *not* of the same kind;—that the marks by which the kinds are distinguished had been wrongly stated. This is what has very frequently happened in the course of the investigations of chemists and mineralogists. And thus we have it, not as an experiential fact, but as a necessary principle of chemical philosophy, that the Elementary Composition of a body determines its Kind and Properties.

23. How bodies differ in their elementary composition, experiment must teach us, as we have already said that experiment has taught us. But as we have also said, whatever be the nature of this difference, Kinds must be definite, in order that Language may be possible: and hence, whatever be the terms in which we are taught by experiment to express the elementary composition of bodies, the result must be conformable to this principle, That the Differences of elementary composition are

definite. The law to which we are led by experiment is, that the elements of bodies continue in definite proportions according to weight. Experiments add other laws; as for instance, that of multiple proportions in different kinds of bodies composed of the same elements; but of these we do not here speak.

24. We are thus led to see that in our knowledge of mechanics, chemistry, and the like, there are involved certain necessary principles, derived from our ideas, and not from experience. But to this it may be objected, that the parts of our knowledge in which these principles are involved has, in historical fact, all been acquired by experience. The Laws of Motion, the Doctrine of Definite Proportions, and the like, have all become known by experiment and observation; and so far from being seen as necessary truths, have been discovered by long-continued labours and trials, and through innumerable vicissitudes of confusion, error, and imperfect truth. This is perfectly true: but does not at all disprove what has been said. Perception of external objects and experience, experiment and observation, are needed, not only, as we have said, to supply the objective element of all knowledge—to embody, limit, define, and modify our ideas; but this intercourse with objects is also requisite to unfold and fix our ideas themselves. As we have already said, ideas and facts can never be separated. Our ideas cannot be exercised and developed in any other form than in their combination with facts; and therefore the trials, corrections, controversies, by which the Matter of our knowledge is collected, is also the only way in which the Form of it can be rightly fashioned. Experience is requisite to the clearness and distinctness of our ideas, not because they are derived from experience, but because they can only be exercised upon experience. And this consideration sufficiently explains how it is that experiment and observation have been the means, and the only means, by which men have been led to a knowledge of the laws of nature. In reality, however, the necessary principles which flow from our ideas, and which are the basis of such knowledge, have not only been inevitably assumed in the course of such investigations, but have been often expressly promulgated in words by clear-minded philosophers, long before their true interpretation was assigned by experiment. This has happened with regard to such principles as those above mentioned; That every event must have a cause; That reaction is equal and opposite to action; That

the quantity of matter in the world cannot be increased or diminished: and there would be no difficulty in finding similar enunciations of the other principles above mentioned;—That the kinds of things have definite differences, and that these differences depend upon their elementary composition.' In general, however, it may be allowed, that the necessary principles which are involved in those laws of nature of which we have a knowledge become then only clearly known, when the laws of nature are discovered which thus involve the necessary ideal element.

25. But since this is allowed, it may be further asked, how we are to distinguish between the necessary principle which is derived from our ideas, and the law of nature which is learnt by experience. And to this we reply, that the necessary principle may be known by the condition which we have already mentioned as belonging to such principles:—that it is impossible distinctly to conceive the contrary. We cannot conceive an event without a cause, except we abandon all distinct idea of cause; we cannot distinctly conceive two straight lines inclosing space; and if we seem to conceive this, it is only because we conceive indistinctly. We cannot conceive 5 and 3 making 7 or 9; if a person were to say that he could conceive this, we should know that he was a person of immature or rude or bewildered ideas, whose conceptions had no distinctness. And thus we may take it as the mark of a necessary truth, that we cannot conceive the contrary distinctly.

26. If it be asked what is the test of distinct conception (since it is upon the distinctness of conception that the matter depends), we may consider what answer we should give to this question if it were asked with regard to the truths of geometry. If we doubted whether any one had these distinct conceptions which enable him to see the necessary nature of geometrical truth, we should inquire if he could understand the axioms *as* axioms, and could follow, as demonstrative, the reasonings which are founded upon them. If this were so, we should be ready to pronounce that he had distinct ideas of space, in the sense now supposed. And the same answer may be given in any other case. That reasoner has distinct conceptions of mechanical causes who can see the axioms of mechanics as axioms, and can follow the demonstrations derived from them as demonstrations. If it be said that the science, as presented to him, may be erroneously constructed; that the axioms may not be axioms,

and therefore the demonstrations may be futile, we still reply, that the same might be said with regard to geometry: and yet that the possibility of this does not lead us to doubt either of the truth or of the necessary nature of the propositions contained in Euclid's Elements. We may add further, that although, no doubt, the authors of elementary books may be persons of confused minds, who present as axioms what are not axiomatic truths; yet that in general, what is presented as an axiom by a thoughtful man, though it may include some false interpretation or application of our ideas, will also generally include some principle which really is necessarily true, and which would still be involved in the axiom, if it were corrected so as to be true instead of false. And thus we still say, that if in any department of science a man can conceive distinctly at all, there are principles the contrary of which he cannot distinctly conceive, and which are therefore necessary truths.

27. But on this it may be asked, whether truth can thus depend upon the particular state of mind of the person who contemplates it; and whether that can be a necessary truth which is not so to all men. And to this we again reply, by referring to geometry and arithmetic. It is plain that truths may be necessary truths which are not so to all men, when we include men of confused and perplexed intellects; for to such men it is not a necessary truth that two straight lines cannot inclose a space, or that 14 and 17 are 31. It need not be wondered at, therefore, if to such men it does not appear a necessary truth that reaction is equal and opposite to action, or that the quantity of matter in the world cannot be increased or diminished. And this view of knowledge and truth does not make it depend upon the state of mind of the student, any more than geometrical knowledge and geometrical truth, by the confession of all, depend upon that state. We know that a man cannot have any knowledge of geometry without so much of attention to the matter of the science, and so much of care in the management of his own thoughts, as is requisite to keep his ideas distinct and clear. But we do not, on that account, think of maintaining that geometrical truth depends merely upon the state of the student's mind. We conceive that he knows it because it is true, not that it is true because he knows it. We are not surprized that attention and care and repeated thought should be requisite to the clear apprehension of truth. For such care and such repetition are

requisite to the distinctness and clearness of our ideas: and yet the relations of these ideas, and their consequences, are not produced by the efforts of attention or repetition which we exert. They are in themselves something which we may discover, but cannot make or change. The idea of space, for instance, which is the basis of geometry, cannot give rise to any doubtful propositions. What is inconsistent with the idea of space cannot be truly obtained from our ideas by any efforts of thought or curiosity; if we blunder into any conclusion inconsistent with the idea of space, our knowledge, so far as this goes, is no knowledge; any more than our observation of the external world would be knowledge, if, from haste or inattention, or imperfection of sense, we were to mistake the object which we see before us.

28. But further: not only has truth this reality, (which makes it independent of our mistakes,) that it must be what is really consistent with our ideas; but also, a further reality, to which the term is more obviously applicable, arising from the principle already explained, that ideas and perceptions are inseparable. For since, when we contemplate our ideas, they have been frequently embodied and exemplified in objects, and thus have been fixed and modified; and since this compound aspect is that under which we constantly have them before us, and free from which they cannot be exhibited; our attempts to make our ideas clear and distinct will constantly lead us to contemplate them as they are manifested in those external forms in which they are involved. Thus in studying geometrical truth, we shall be led to contemplate it as exhibited in visible and tangible figures;—not as if these could be sources of truth, but as enabling us more readily to compare the aspects which our ideas, applied to the world of objects, may assume. And thus we have an additional indication of the reality of geometrical truth, in the necessary possibility of its being capable of being exhibited in a visible or tangible form. And yet even this test by no means supersedes the necessity of distinct ideas, in order to a knowledge of geometrical truth. For in the case of the duplication of the cube by Hobbes, mentioned above, the diagram which he drew made two points appear to coincide, which did not really, and by the nature of our idea of space, coincide; and thus confirmed him in his errour.

Thus the inseparable nature of the Fundamental Antithesis of

Ideas and Things gives reality to our knowledge, and makes objective reality a corrective of our subjective imperfections in the pursuit of knowledge. But this objective exhibition of knowledge can by no means supersede a complete development of the subjective condition, namely, distinctness of ideas. And that there is a subjective condition, by no means makes knowledge altogether subjective, and thus deprives it of reality; because, as we have said, the subjective and the objective elements are inseparably bound together in the fundamental antithesis.

29. It would be easy to apply these remarks to other cases, for instance, to the case of the principle we have just mentioned, that the differences of elementary composition of different kinds of bodies must be definite. We have stated that this principle is necessarily true;—that the contrary proposition cannot be distinctly conceived. But by whom? Evidently, according to the preceding reasoning, by a person who distinctly conceives Kinds, as marked by intelligible names, and Composition, as determining the kinds of bodies. Persons new to chemical and classificatory science may not possess these ideas distinctly; or rather, cannot possess them distinctly; and therefore cannot apprehend the impossibility of conceiving the opposite of the above principle; just as the schoolboy cannot apprehend the impossibility of the numbers in his multiplication table being other than they are. But this inaptitude to conceive, in either case, does not alter the necessary character of the truth: although, in one case, the truth is obvious to all except schoolboys and the like, and the other is probably not clear to any except those who have attentively studied the philosophy of elementary compositions. At the same time, this difference of apprehension of the truth in different persons does not make the truth doubtful or dependent upon personal qualifications; for in proportion as persons attain to distinct ideas, they will see the truth; and cannot, with such ideas, see anything as truth which is not truth. When the relations of elements in a compound become as familiar to a person as the relations of factors in a multiplication table, he will then see what are the necessary axioms of chemistry, as he now sees the necessary axioms of arithmetic.

30. There is also one other remark which I will here make. In the progress of science, both the elements of our knowledge are constantly expanded and augmented. By the exercise of observation and experiment, we have a perpetual accumulation

of facts, the materials of knowledge, the objective element. By thought and discussion, we have a perpetual development of man's ideas going on: theories are framed, the materials of knowledge are shaped into form; the subjective element is evolved; and by the necessary coincidence of the objective and subjective elements, the matter and the form, the theory and the facts, each of these processes furthers and corrects the other: each element moulds and unfolds the other. Now it follows, from this constant development of the ideal portion of our knowledge, that we shall constantly be brought in view of new Necessary Principles, the expression of the conditions belonging to the Ideas which enter into our expanding knowledge. These principles, at first dimly seen and hesitatingly asserted, at last become clearly and plainly self-evident. Such is the case with the principles which are the basis of the laws of motion. Such may soon be the case with the principles which are the basis of the philosophy of chemistry. Such may hereafter be the case with the principles which are to be the basis of the philosophy of the connected and related polarities of chemistry, electricity, galvanism, magnetism. That knowledge is possible in these cases, we know; that our knowledge may be reduced to principles gradually more simple, we also know; that we have reached the last stage of simplicity of our principles, few cultivators of the subject will be disposed to maintain; and that the additional steps which lead toward very simple and general principles will also lead to principles which recommend themselves by a kind of axiomatic character, those who judge from the analogy of the past history of science will hardly doubt. That the principles thus axiomatic in their form, do also express some relation of our ideas, of which experiment and observation have given the true and real interpretation, is the doctrine which I have here attempted to establish and illustrate in the most clear and undoubted of the existing sciences; and the evidence of this doctrine in those cases seems to be unexceptionable, and to leave no room to doubt that such is the universal type of the progress of science. Such a doctrine, as we have now seen, is closely connected with the views here presented of the nature of the Fundamental Antithesis of Philosophy, which I have endeavoured to illustrate.

ESSAY VI.

REMARKS ON A REVIEW OF THE PHILOSOPHY OF THE INDUCTIVE SCIENCES*.

Trinity Lodge, April 11*th*, 1844.

MY DEAR HERSCHEL,

BEING about to send you a copy of a paper on a philosophical question just printed in the Transactions of our Cambridge Society, I am tempted to add, as a private communication, a few Remarks on another aspect of the same question. These Remarks I think I may properly address to you. They will refer to an Article in the "Quarterly Review" for June 1841, respecting my "History" and "Philosophy" of the Inductive Sciences; and without assigning any other reason, I may say that the interest I know you to take in speculations on such subjects makes me confident that you will give a reasonable attention to what I may have to say on the subject of that Article. With the Reviewal itself, I am so far from having any quarrel, that when it appeared I received it as affording all that I hoped from Public Criticism. The degree and the kind of admiration bestowed upon my works by a writer so familiar with science, so comprehensive in his views, and so equitable in his decisions, as the Reviewer manifestly was, I accepted as giving my work a stamp of acknowledged value which few other hands could have bestowed.

You may perhaps recollect, however, that the Reviewer dissented altogether from some of the general views which I had maintained, and especially from a general view which is also, in the main, that presented in the preceding Essay, namely, that, besides Facts, Ideas are an indispensable source of our knowledge; that Ideas are the ground of necessary truth; that the Idea of Space, in particular, is the ground of the necessary truths of geometry. This question, and especially as limited to the last form, will be the subject of my Remarks in the first place; and I wish to consider the Reviewer's objections with the respect which their subtlety and depth of thought well deserve.

The Reviewer makes objections to the account which I have

* *A Letter to Sir John F. W. Herschel, Bart.*

given of the source whence geometrical truth derives its characters of being necessary and universal; but he is not one of those metaphysicians who deny those characters to the truths of geometry. He allows in the most ample manner that the truths of geometry *are* necessary. The question between us therefore is, From what this character is derived? The Reviewer prefers, indeed, to have it considered that the question is not concerning the necessity, but, as he says, the universality of these truths; or rather, the nature and grounds of our conviction of their universality. He might have said, with equal justice, the nature and grounds of our conviction of their necessity. For his objection to the term *necessity* in this case—"that all the propositions about realities are necessarily true, since every reality must be consistent with itself," (p. 206)—does not apply to our conviction of necessity, since we may not be able to see what are the properties of real things; and therefore may have no conviction of their necessity. It may be a necessary property of salt to be soluble, but we see no such necessity; and therefore the assertion of such a property is not one of the necessary truths with which we are here concerned. But to turn back to the necessary or universal truths of geometry, and the ground of those attributes:—The main difference between the Author and the Reviewer is brought into view, when the Reviewer discusses the general argument which I had used, in order to show that truths which we see to be necessary and universal cannot be derived from experience. The argument is this,—

"Experience must always consist of a limited number of observations; and however numerous these may be, they can show nothing with regard to the infinite number of cases in which the experiment has not been made..... Truths can only be known to be general, not universal, if they depend upon experience alone. Experience cannot bestow that universality which she herself cannot have; nor that necessity of which she has no comprehension." (*Phil.* i. pp. 63, 64.)

Here is that which must be considered as the cardinal argument on this subject. It is therefore important to attend to the answer which the Reviewer makes to it. He says,—

"We conceive that a full answer to this argument is afforded by the nature of the inductive propensity,—by the irresistible impulse of the mind to generalize *ad infinitum*, when nothing in the nature of limitation or opposition offers itself to the imagination; and by our involuntary application of the law of continuity to fill up, by the same ideal substance of truth, every interval which uncontradicted experience may have left blank in our inductive conclusion." (p. 207.)

Now here we have two rival explanations of the same thing,—the conviction of the universality of geometrical truths. The one explanation is, that this universality is imposed upon such truths by their involving a certain element, derived from the universal mode of activity of the mind when apprehending such truths, which element I have termed an Idea. The other explanation is, that this universality arises from the *inductive propensity*—from the *irresistible impulse to generalize ad infinitum*—from the *involuntary application of the law of continuity*—from the *filling up all intervals with the same ideal substance of truth.*

With regard to these two explanations, I may observe, that so far as they are thus stated they do not necessarily differ. They both agree in expressing this; that the ground of the universality of geometrical truths is a certain law of the mind's activity, which determines its procedure when it is concerned in apprehending the external world. One explanation says, that we impress upon the external world the relations of our ideas, and thus believe more than we see,—the other says, that we have an irresistible impulse to introduce into our conviction a relation between what we do observe and what we do not, namely, to generalize *ad infinitum* from what we do see. One explanation says, that we perceive all external objects as included in absolute ideal space,—the other, that we fill up the intervals of the objects which we perceive with the same ideal substance of truth. Both sets of expressions may perhaps be admissible; and if admitted, may be understood as expressing the same opinions, or opinions which have much in common. The Author's expressions have the advantage, which ought to belong to them, as the expressions employed in a systematic work, of being fixed expressions, technical phrases, intentionally selected, uniformly and steadily employed whenever the occasion recurs. The Reviewer's expressions are more lively and figurative, and such as well become an occasional composition; but hardly such as could be systematically applied to the subject in a regular treatise. We could not, as a standard and technical phrase, talk of "filling up the intervals of observation with the same ideal substance of truth;" and the "inevitable impulse to generalize" would hardly sufficiently express that we generalize according to a certain idea, namely, the idea of space. Perhaps that which is suggested to us as the common import of the two sets of expressions may be conveyed by some other phrase, in a manner free from the objections which lie against both the Author's and the Critic's terms. Perhaps the mental Idea governing our experience, and the irresistible Impulse to generalize our observation, may both be superseded by our speaking of a Law of the mind's *Activity*, which is really implied in both. There operates, in observing the external world, a law of the mind's activity, by which it connects its observations; and this law of the mind's activity may be spoken of either as the Idea of space, or as the irresistible Impulse to generalize the relations of space which it observes. And this expression—*the laws of the mind's activity*—thus opposed to that merely *passive* function by which the mind receives the impressions of sense, may be applied to other ideas as well as to the idea of space, and to the impulse to generalize in other truths as well as those of geometry.

So far, it would seem, that the Author and the Critic may be brought into much nearer agreement than at first seemed likely, with regard to the grounds of the necessity and universality in our knowledge. But even if we adopt this conciliatory suggestion, and speak of the necessity and universality of certain truths as arising from the Laws of the mind's Activity, we cannot, without producing great confusion, allow ourselves to say, as the Critic says, that these truths are thus derived from *experience*, or from *observation*. It will, I say, be found fatal to all philosophical precision of thought and language, to say that the fundamental truths of geometry, the axioms, with the conviction of their necessary truth, are derived from experience. Let us take any axiomatic truth of geometry, and ask ourselves if this is not so.

It is, for example, an axiom in geometry that if a straight line cut one of two parallel straight lines, it must cut the other also. Is this truth derived or derivable from observation of actual parallel lines, and a line cutting them, exhibited to our senses? Let those who say that we do acquire this truth by observation, imagine to themselves the mode in which the observation must be made. We have before us two parallel straight lines, and we see that a straight line which cuts the one cuts the other also. We see this again in another case, it may be, the angles and the distances being different, and in a third, and in a fourth; and so on; and generalizing, we are irresistibly led to believe the assertion to be universally true. But can any one really imagine this to be the mode in which we arrive at this truth? "We see," says this explanation, "two parallel straight lines, cut by a third." But how do we know that the observed lines *are* parallel? If we apply any test of parallelism, we must assume some property of parallels, and thus involve some axiom on the subject, which we have no more right to assume than the one now under consideration. We should thus destroy our explanation as an account of the mode of arriving at independent geometrical axioms. But probably those who would give such an explanation would not do this. They would not suppose that in observing this property of parallels we try by measurement whether the lines are parallel. They would say, I conceive, that we *suppose* lines to be parallel, and that then we *see* that the straight line which cuts the one must cut the other. That when we make this supposition, we are persuaded of the truth of the conclusion, is certain. But what I have to remark is, that this being so, the conclusion is the result, not of observation, but of the hypothesis. The geometrical truth here spoken of, after this admission, no longer flows from experience, but from supposition. It is not that we *ascertain* the lines to be parallel, and then *find* that they have this property: but we *suppose* the lines to be parallel, and *therefore* they have this property. This is not a truth of experience.

This, it may be said, is so evident that it cannot have been overlooked by a very acute reasoner, such as you describe your Critic to be. What, it may be asked, is the answer which he gives to so palpable an objection as this? How does he understand his assertion that we learn the truth of geometrical axioms from experience (p. 208), so as to make it tenable on his own principles? What account does he give of the origin of such axioms which makes them in any sense to be derived from experience?

In justice to the Reviewer's fairness (which is unimpeachable throughout his argumentation) it must be stated that he does give an account in which he professes to show how this is done. And the main step of his explanation consists in introducing the conception of *direction*, and *unity of direction*. He says (p. 208), "The *unity of direction*, or that we cannot march from a given point by more than one path *direct to the same object*, is a matter of practical experience, long before it can by possibility become matter of abstract thought." We might ask here, as in the former case, how this can be a matter of experience, except we have some independent test of directness? and we might demand to know what this test is. Or do we not rather, here as in the other case, *suppose* the directness of the path; and is not the singleness of the direct path a consequence, not of its observed form, but of its hypothetical directness; and thus by no means a result of experience? But we may put our remark upon this deduction of the geometrical axiom in another form. We generalize, it is said, the observations which we have made ever since we were born. But this term "generalize" is far too vague to pass for an explanation, without being itself explained. We are impelled to believe that to be true in general which we see to be true in particular. But how do we see any truth? How do we pick out any proposition with respect to a diagram which we see before us? We see in particular, and state in general, some truth respecting straight lines or parallel lines, or concerning direction. But where do we find the conception of straightness, or parallelism, or direction? These conceptions are not upon the surface of things. The child does not, from his birth, see straightness and parallelism so as to know that he sees them. How then does his experience bear upon a proposition in which these conceptions are involved? It is said that it is a matter of experience long before it is a matter of abstract thought. But how can there be any experience by which we learn these properties of a straight line, till our thoughts are at least so abstract as to conceive what straightness is? If it be said that this conception grows with our experience, and is gradually unfolded with our unfolding materials of knowledge, so as to give import and significance to them: I need make no objection to such a statement, except this—that this power of unfolding out of the mind conceptions which give meaning to our experience, is something in addition to the mere employment of our senses upon the external world. It is what I have called the *ideal* part of our knowledge. It implies, not only an impulse to generalize from experience, but also an impulse to form conceptions by which generalization is possible. It requires, not only that nothing should oppose the tendency, but that the direction in which the tendency is to operate should be determined by the laws of the mind's activity; by an internal, not by any external agency.

One main ground on which the Reviewer is disposed to quarrel with and reject several of the expressions used in the Philosophy;—such as that space is an Idea, a Form of our perception, and the like,—is this; that such expressions appear to deprive the external world of its reality; to make it, or at least most of its properties, a creation of the observing mind. He quotes the following argument which is urged in the "Philosophy," in order to prove that space is not a notion obtained from experience: "Experience gives us information concerning things without us, but our apprehending them *as* without us takes for granted their existence in space. Experience acquaints us with the form, position, magnitude, &c. of particular objects, but that they *have* form, position, magnitude, pre-supposes that they are in space." From this statement he altogether dissents. "No," says he, "the reason why we apprehend things as without us is that they *are* without us. We take for granted that they exist in space, because they *do* so exist, and because such their existence is a matter of direct perception, which can neither be explained in words nor contravened in imagination: because, in short, space is a *reality*, and not a mere matter of convention or imagination."

Now, if by calling space an *idea*, we suggest any doubt of its reality and of the reality of the external world, we certainly run the risk of misleading our readers; for the external world is real if anything be real: the bodies which exist in space are things, if things are anywhere to be found. That bodies *do* exist in space, and that *that* is the reason why we apprehend them as existing in space, I readily grant. But I conceive that the term Idea ought not to suggest any such doubt of the reality of the knowledge in which it is involved. Ideas are always, in our knowledge, conjoined with facts. Our *real knowledge* is *knowledge*, because it involves Ideas, *real*, because it involves Facts. We apprehend things as existing in space because they do so exist: and our idea of space enables us so to observe them, and so to conceive them.

But we want, further, a reason why, apprehending them as they are, we also apprehend, that in certain relations they could not be otherwise (that two straight linear objects could not inclose a space, for instance). This circumstance is no way accounted for by saying that we apprehend them as they are; and is, I presume to say, inexplicable, except by supposing that it arises from some property of the observing mind:—an Idea, as I have termed it,—an irresistible Impulse to generalize, as the Reviewer expresses it. Or, as I have suggested, we may adopt a third phrase, a Law of the Mind's Activity: and in order that no question may remain, whether we ascribe reality to the objects and relations which we observe, we may describe it as "a Law of the mind's activity in apprehending what is." And thus the real existence of the object, and the ideal element which our apprehension of it introduces, would both be clearly asserted.

I am ready to use expressions which recognize the reality of space and other external things more emphatically than those expressions which I have employed in the "Philosophy," if expressions can be found which, while they do this, enable us to explain the possibility of knowledge, and to analyse the structure of truth. It is, indeed, extremely difficult to find, in speaking of this subject, expressions which are satisfactory. The reality of the objects which we perceive is a profound, apparently an insoluble problem*. We cannot but suppose that existence is something different from our knowledge of existence:—that which exists, does not exist merely in our knowing that it does:—truth is truth whether we know it or not. Yet how can we conceive truth, otherwise than as something known? How can we conceive things as existing, without conceiving them as objects of perception? Ideas and Things are constantly opposed, yet necessarily co-existent. How they are thus opposite and yet identical, is the ultimate problem of all philosophy. The successive phases of philosophy have consisted in separating and again uniting these two opposite elements; in dwelling sometimes upon the one and sometimes upon the other, as the principal or original or only element; and then in discovering that such an account of the state of the case was insufficient. Knowledge requires Ideas. Reality requires Things. Ideas and Things co-exist. Truth *is*, and is known. But the complete explanation of these points appears to be beyond our reach. At least it is not necessary for the

* These remarks were written in 1841. The preceding Essay contains a further discussion of this problem.

purposes of our philosophy. The *separation* of Ideas and Sensations in order to discover the conditions of Knowledge is our main task. How Ideas and Sensations are *united* so as to form Things, does not so immediately concern us.

I have stated that we may, without giving up any material portion of the Philosophy of Science to which I have been led, express the conclusions in other phraseology; and that instead of saying that all our knowledge involves certain Fundamental Ideas, the sources from which all universal truth is derived, we may say that there are certain Laws of Mental Activity according to which alone all the real relations of things are apprehended. If this alteration in the phraseology will make the doctrines more generally intelligible or acceptable, there is no reason why it should not be adopted. But I may remark, that a main purpose of the "Philosophy" was not merely to prove that there *are* such Fundamental Ideas or Laws of mental activity, but to *enumerate* those of them which are involved in the existing sciences; and to state the fundamental truths to which the fundamental ideas lead. This was the task which was attempted; and if this have been executed with any tolerable success, it may perhaps be received as a contribution to the philosophy of science, of which the value is not small, in whatever terms it be expressed. And this enumeration of fundamental ideas, and of truths derived from them, must have something to correspond to it, in any other mode of expressing that view of the nature of knowledge which we are led to adopt. If instead of *Fundamental Ideas*, we speak of Impulses of generalization, or of *Laws of mental activity*, we must still distinguish such Impulses, or such Laws, according to the distinctions of ideas to which the survey of science led us. We shall thus have a series of groups of Laws, or of classes of generalizing Impulses, corresponding to the series of Fundamental Ideas already given. If we employ the language of the Reviewer, we shall have one generalizing Impulse which suggests relations of Space; another which directs us to properties of Numbers; another which deals with Time; another with Cause: another which groups objects according to Likeness; another which suggests a Purpose as a necessary relation among them; to which may be added, even while we confine ourselves to the physical sciences, several others, as may be seen in the "Philosophy." Now when the fundamental conditions and elements of truth are thus arranged into groups, it is not a matter of so much consequence to decide whether each group shall be said to be bound together by an Idea or by an Impulse of generalization; as it is to see that, if this happen in virtue of Ideas, here are so many distinct Ideas which enter into the structure of science, and give universality to its matter; and again, if this happen in virtue of an irresistible Impulse of generalization in each case, we have so many different kinds of Impulses of generalization. The main purpose in the "Philosophy" was to analyse scientific truth into its conditions and elements; and I did not content myself with saying that those elements are Sensations and Ideas; the Ideas being that element which makes universal knowledge conceivable and possible. I went further: I enumerated the Ideas which thus enter into science. I showed that in the sciences which I passed in review, the most acute and profound inquirers had taken for granted that certain truths in each science are of universal and necessary validity, and I endeavoured to select the idea in which this universality and necessity resided, and to separate it from all other ideas involved in other sciences. If therefore it be thought better to say that those Principles in each science upon which, as upon the axioms in geometry, the universality and necessity of scientific truth depends, are arrived at, not by Ideas, but by an irresistible Impulse of generalization, those who employ such phraseology, if they make a classification of such Impulses corresponding to my classification of Ideas, will still adopt the greater part of my philosophy, altering only the phraseology. Or if, as I suggested, instead of "Fundamental Ideas," we use the phrase "Laws of Mental Activity," then our primary intellectual Code—the Constitution of our minds, as it may be termed—will consist of a Body of Laws of which the Titles correspond with the Fundamental Ideas of the "Philosophy."

My object was, from the writings of the most sagacious and profound philosophers who have laboured on each science, to extract such a code, such a constitution. If I have in any degree succeeded in this, the result must have a reality and a value independently of all forms of expression. Still, I do not think that any language can ever serve for such legislation, in which the two elements of truth are not distinguished. Even if we adopt the phraseology which I have just employed, we shall have to recollect that Law and Fact must be kept distinct, and that the Constitution has its Principles as well as its History.

But I will not longer detain you by seeking other modes of expressing the Fundamental Antithesis to which the preceding Essay refers. The Remarks which I here send you were written three years ago, on the appearance of the Review which I have quoted. If I succeed in obtaining for them a few minutes' attention from you and a few other friends, I shall be glad that they have been preserved.

I am, my dear Herschel,
always truly yours,
W. WHEWELL.

P.S. I have spared you a large portion of my Remarks as originally written. I had gone on to show that, in my "Philosophy," I had not only enumerated and analysed a great number of different Fundamental Ideas which belong to the different existing sciences, but that I had also shown in what manner these ideas enter into their respective sciences; namely, by the statement or use of Axioms, which involve the ideas, and which form the basis of each science when systematically exhibited. A number of these Axioms, belonging to most of the physical sciences, are stated in the "Philosophy." I might have added also, that I have attempted to classify the historical steps by which such Axioms are brought into view and applied. But it is not necessary to dwell upon these points, in order to illustrate the difference and the agreement between the Reviewer and me.

Sir John F. W. Herschel, Bart. &c.

THE END.