

PROCEEDINGS

OF THE

SOCIETY FOR PSYCHICAL RESEARCH.

VOLUME XIV.

(CONTAINING PARTS XXXIV-XXXV.)

1898-9.

*The responsibility for both the facts and the reasonings in papers published
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SOCIETY FOR PSYCHICAL RESEARCH,

PART XXXIV.

PROCEEDINGS OF GENERAL MEETINGS.

The 91st General Meeting of the Society was held at the Westminster Town Hall on Friday, March 11th, at 8.30 p.m. ; PROFESSOR HENRY SIDGWICK in the chair.

MR. FRANK PODMORE gave an address on "The Trance Phenomena Manifested through Mrs. Piper."

The 92nd General Meeting was held in the same place on Friday, April 22nd, at 4 p.m. ; COLONEL J. HARTLEY in the chair.

DR. R. HODGSON read a paper by Miss ALICE JOHNSON on "Coincidences."

The 93rd General Meeting was held in the same place on Friday, May 20th, at 8.30 p.m. ; PROFESSOR HENRY SIDGWICK in the chair.

MR. ST. GEORGE LANE FOX read a paper by DR. C. LLOYD TUCKEY on "The Influence of Suggestion on Health, with Special Reference to 'Christian Science.'"

The 94th General Meeting was held in the same place on Friday, June 24th, at 4 p.m. ; MR. F. W. H. MYERS in the chair.

PROFESSOR W. F. BARRETT gave an address, "A Second Report on the So-called Divining Rod," the paper being a sequel to the Report published in Part XXXII., *Proceedings S.P.R.*

I.

PART OF THE PRESIDENTIAL ADDRESS DELIVERED TO
THE BRITISH ASSOCIATION AT BRISTOL, SEPT., 1898,

BY SIR WILLIAM CROOKES, F.R.S.

These, then, are some of the subjects, weighty and far-reaching, on which my own attention has been chiefly concentrated. Upon one other interest I have not yet touched—to me the weightiest and the farthest reaching of all.

No incident in my scientific career is more widely known than the part I took many years ago in certain psychic researches. Thirty years have passed since I published an account of experiments tending to show that outside our scientific knowledge there exists a Force exercised by intelligence differing from the ordinary intelligence common to mortals. This fact in my life is, of course, well understood by those who honoured me with the invitation to become your President. Perhaps among my audience some may feel curious as to whether I shall speak out or be silent. I elect to speak, although briefly. To enter at length on a still debatable subject would be unduly to insist on a topic which—as Wallace, Lodge, and Barrett have already shown—though not unfitted for discussion at these meetings, does not yet enlist the interest of the majority of my scientific brethren. To ignore the subject would be an act of cowardice—an act of cowardice I feel no temptation to commit.

To stop short in any research that bids fair to widen the gates of knowledge, to recoil from fear of difficulty or adverse criticism, is to bring reproach on science. There is nothing for the investigator to do but to go straight on, “to explore up and down, inch by inch, with the taper his reason”; to follow the light wherever it may lead, even should it at times resemble a will-o'-the wisp. I have nothing to retract. I adhere to my already published statements. Indeed, I might add much thereto. I regret only a certain crudity in those early expositions which, no doubt justly, militated against their acceptance by the scientific world. My own knowledge at that time scarcely extended beyond the fact that certain phenomena new to science had assuredly occurred, and were attested by my own sober senses, and, better still, by automatic record. I was like some two-dimensional being who might stand at the singular point of a Riemann's surface, and thus find himself in infinitesimal and inexplicable contact with a plane of existence not his own.

I think I see a little farther now. I have glimpses of something like coherence among the strange elusive phenomena; of something like continuity between those unexplained forces and laws already known. This advance is largely due to the labours of another Association of which I have also this year the honour to be President—the Society for Psychical Research. And were I now introducing for the first time these inquiries to the world of science I should choose a starting-point different from that of old. It would be well to begin with *telepathy*; with the fundamental law, as I believe it to be, that thoughts and images may be transferred from one mind to another without the agency of the recognised organs of sense—that knowledge may enter the human mind without being communicated in any hitherto known or recognised ways.

Although the inquiry has elicited important facts with reference to the Mind, it has not yet reached the scientific stage of certainty which would entitle it to be usefully brought before one of our Sections. I will therefore confine myself to pointing out the direction in which scientific investigation can legitimately advance. If telepathy take place we have two physical facts—the physical change in the brain of A, the suggester, and the analogous physical change in the brain of B, the recipient of the suggestion. Between these two physical events there must exist a train of physical causes. Whenever the connecting sequence of intermediate causes begins to be revealed the inquiry will then come within the range of one of the Sections of the British Association. Such a sequence can only occur through an intervening medium. All the phenomena of the universe are presumably in some way continuous, and it is unscientific to call in the aid of mysterious agencies when with every fresh advance in knowledge it is shown that ether vibrations have powers and attributes abundantly equal to any demand—even to the transmission of thought. It is supposed by some physiologists that the essential cells of nerves do not actually touch, but are separated by a narrow gap which widens in sleep while it narrows almost to extinction during mental activity. This condition is so singularly like that of a Branly or Lodge coherer as to suggest a further analogy. The structure of brain and nerve being similar, it is conceivable there may be present masses of such nerve coherers in the brain whose special function it may be to receive impulses brought from without through the connecting sequence of ether waves of appropriate order of magnitude. Röntgen has familiarised us with an order of vibrations of extreme minuteness compared with the smallest waves with which we have hitherto been acquainted, and of dimensions comparable with the distances between the centres of the atoms of which the material universe is built up; and there is no reason to suppose that we have here reached the limit of frequency. It is known that

the action of thought is accompanied by certain molecular movements in the brain, and here we have physical vibrations capable from their extreme minuteness of acting direct on individual molecules, while their rapidity approaches that of the internal and external movements of the atoms themselves.

Confirmation of telepathic phenomena is afforded by many converging experiments, and by many spontaneous occurrences only thus intelligible. The most varied proof perhaps, is drawn from analysis of the subconscious workings of the mind, when these, whether by accident or design, are brought into conscious survey. Evidence of a region below the threshold of consciousness has been presented, since its first inception, in the *Proceedings* of the Society for Psychical Research; and its various aspects are being interpreted and welded into a comprehensive whole by the pertinacious genius of F. W. H. Myers. Concurrently, our knowledge of the facts in this obscure region has received valuable additions at the hands of labourers in other countries. To mention a few names out of many, the observations of Richet, Pierre Janet, and Binet (in France), of Breuer and Freud (in Austria), of William James (in America), have strikingly illustrated the extent to which patient experimentation can probe subliminal processes, and can thus learn the lessons of alternating personalities, and abnormal states. Whilst it is clear that our knowledge of subconscious mentation is still to be developed, we must beware of rashly assuming that all variations from the normal waking condition are necessarily morbid. The human race has reached no fixed or changeless ideal; in every direction there is evolution as well as disintegration. It would be hard to find instances of more rapid progress, moral and physical, than in certain important cases of cure by suggestion—again to cite a few names out of many—by Liébeault, Bernheim, the late Auguste Voisin, Bérillon (in France), Schrenck-Notzing (in Germany), Forel (in Switzerland), van Eeden (in Holland), Wetterstrand (in Sweden), Milne Bramwell and Lloyd Tuckey (in England). This is not the place for details, but the *vis medicatrix* thus evoked, as it were, from the depths of the organism, is of good omen for the upward evolution of mankind.

A formidable range of phenomena must be scientifically sifted before we effectually grasp a faculty so strange, so bewildering, and for ages so inscrutable, as the direct action of mind on mind. This delicate task needs a rigorous employment of the method of exclusion—a constant setting aside of irrelevant phenomena that could be explained by known causes, including those far too familiar causes, conscious and unconscious fraud. The inquiry unites the difficulties inherent in all experimentation connected with *mind*, with tangled human temperaments and with observations dependent less on automatic record than

on personal testimony. But difficulties are things to be overcome even in the elusory branch of research known as Experimental Psychology. It has been characteristic of the leaders among the group of inquirers constituting the Society for Psychical Research to combine critical and negative work with work leading to positive discovery. To the penetration and scrupulous fair-mindedness of Professor Henry Sidgwick and of the late Edmund Gurney is largely due the establishment of canons of evidence in psychical research, which strengthen while they narrow the path of subsequent explorers. To the detective genius of Dr. Richard Hodgson we owe a convincing demonstration of the narrow limits of human continuous observation.

It has been said that "Nothing worth the proving can be proved, nor yet disproved." True though this may have been in the past, it is true no longer. The science of our century has forged weapons of observation and analysis by which the veriest tyro may profit. Science has trained and fashioned the average mind into habits of exactitude and disciplined perception, and in so doing has fortified itself for tasks higher, wider, and incomparably more wonderful than even the wisest among our ancestors imagined. Like the souls in Plato's myth that follow the chariot of Zeus, it has ascended to a point of vision far above the earth. It is henceforth open to science to transcend all we now think we know of matter, and to gain new glimpses of a profounder scheme of Cosmic Law.

An eminent predecessor in this chair declared that "by an intellectual necessity he crossed the boundary of experimental evidence, and discerned in that matter, which we in our ignorance of its latent powers and notwithstanding our professed reverence for its Creator, have hitherto covered with opprobrium, the potency and promise of all terrestrial life." I should prefer to reverse the apophthegm, and to say that in life I see the promise and potency of all forms of matter.

In old Egyptian days a well known inscription was carved over the portal of the temple of Isis: "I am whatever hath been, is, or ever will be; and my veil no man hath yet lifted." Not thus do modern seekers after truth confront Nature—the word that stands for the baffling mysteries of the universe. Steadily, unflinchingly, we strive to pierce the inmost heart of Nature, from what she is to re-construct what she has been, and to prophesy what she yet shall be. Veil after veil we have lifted, and her face grows more beautiful, august, and wonderful, with every barrier that is withdrawn.

II.

A FURTHER RECORD OF OBSERVATIONS OF CERTAIN
PHENOMENA OF TRANCE.

PART II.

A.—BY PROFESSOR WILLIAM ROMAINE NEWBOLD.

§ 1. INTRODUCTORY.

I have been present at twenty-six sittings with Mrs. Piper, and Dr. Hodgson kindly supervised seven others at which I was not present, although the communicators invoked and the topics introduced were suggested by me. Fifteen sittings, including two of those at which I was not present, were devoted for the most part to getting evidence to prove the identity of the alleged communicators; the remainder to getting from them their own theory of the phenomena and their description of the conditions under which they were working and of the life they live. While it is impossible at present to accept these statements as true, it is of the greatest importance to put them on record as affording clues for the guidance of experiments with other automatists. The material got from this latter series I shall leave entirely to Dr. Hodgson. I am myself concerned with the evidence for identity only.

Of the general character of that evidence the following pages will give a sufficient account. In making my abstract I have tried to include the more important passages which are relevant to the question of identity. I have been especially careful to bring into prominence all distinct failures and any other facts which would tend to detract from the surprising character of many of the statements made. As a rule I have not transcribed verbatim pages of confusion from which no coherent thought can be extracted. But in the cases in which such confusion immediately precedes the appearance of some surprising bit of information, it has been in several cases given in full, that the reader may form his own opinion of the methods by which such results are attained. For examples *cf.* the giving of the names of Morton, Murdoch, and the introduction of Mr. Burton (pp. 15, 27, 32). When clearly intelligible passages contained repetitions of the same word due to the inability of the sitter to decipher the first attempt, or words and

phrases which have nothing to do with the general tenor of the communication, the extraneous material has frequently been omitted without indication of the fact.

Names of persons and of places have been in nearly all cases suppressed. In the selection of pseudonyms I have taken great pains to represent familiar names by names at least as familiar, and unfamiliar names by names as unfamiliar. So also in the transcription of phonetic approximations to the real names, I have taken great care to make the representatives letter for letter analogous to their originals. A few other changes have been made in order still further to conceal identity, but nothing which could at all affect the value of the evidence.

With regard to the origin of the information given, I have no theory to offer. I can frame none to which I cannot myself allege unanswerable objections. I am satisfied, however, as is every one so far as I know who has studied the case at any length, that it was not consciously got by Mrs. Piper during waking life and then fraudulently palmed off on the sitter as supernatural. There is every reason for believing that there is no memory bond between Mrs. Piper's waking consciousness and that of her trance life.

A question more difficult to answer is that which inquires into the amount of information which Mrs. Piper's trance personalities get from the sitter. Even without resorting to the assumption of a telepathic relation between the sitter and the "medium," no one who has seen how readily an acute "medium" will construct an appropriate "spirit" message upon the suggestions furnished by a sitter's looks and words will be easily convinced by any such record as I here offer.

This is a legitimate objection, and to some extent impairs the value of the evidence. In dealing with personalities who had had much experience in writing, and occasionally with those who represented themselves as having been long dead, it was usually possible to keep complete notes of the sitter's questions and answers. The writing was relatively slow, and illegible words were readily rewritten. But the alleged spirits of those who had but recently died, or who had died a violent death, or who had been bound to the sitter by strong emotional ties, nearly always display great excitement and confusion. The time and attention of one and even two sitters is fully occupied in controlling the violent convulsions which seize the writing arm, keeping a constant flow of cheering talk going for the benefit of the communicator, replacing broken pencils and at the same time deciphering the pages of delirious nonsense which the hand scribbles off as fast as it can tear over the sheets, any misreading of which greatly increases the excitement and confusion.

Under such circumstances, our notes necessarily became fragmentary, and when the sitting was written up a few hours later, many

of our questions and answers had to be supplied from memory. It is possible that some suggestions given by sitters have escaped our notice, and the evidence is to that extent untrustworthy.

I am myself satisfied that the percentage of error thus introduced is not considerable. Both Dr. Hodgson and I have seen much of professional mediums, and are thoroughly familiar with the methods of "fishing" upon which they generally rely. Hence we always had such possibilities in mind, and it would have been impossible for any large amount of detailed information to have been extracted from us in this way without our knowledge. Occasionally our vigilance relaxed, and we made careful note of the fact. For examples see the quotation "*Fama tempus vivat*" (p. 45), and Mr. Bonney's name (p. 43). Probably it occasionally relaxed without our making any note of it, but that could not have happened very often.

The reader will observe that "yes" and "no" are often written when no questions are recorded. This is due to the fact that, the writing being exceedingly illegible and coming very rapidly, the sitter reads aloud with a slight interrogatory inflection at any convenient resting point, as at the end of a sheet or at an apparent pause in the sense. To this the writer responds with "yes" or "no," to show whether he is being correctly understood.

If these utterances are, as I believe them to be, entirely dis severed from the normal consciousness of Mrs. Piper, they as truly reveal to us a new world of mind as the microscope reveals a new world of matter. George Pelham and his companions undoubtedly record for us conscious experiences which are subjectively as real as any that you or I ever experienced. But when we ask to what metaphysical category of Being they are to be assigned, we find no satisfactory answer. Are they merely unusually stable dream states, generated in connection with Mrs. Piper's brain, interrupted perhaps during her normal life, but resuming the thread of their phantasmal existence with the recurrence of the convulsions which usher in her trance? Or are they what they profess to be, human minds, divested of their mortal bodies, and leading an independent existence in a supersensible world?—a world as real as this present world in the only true sense of real, being an inevitable portion of the common experience of conscious beings.

Of the existence of such a world we cannot satisfy ourselves by any of our usual tests. We are confined to the evidence for the identity of the alleged communicators. Of the extent and value of the evidence to be got from my series of sittings the reader can himself judge. Much of it seems to me strong, and much more I cannot reconcile with the theory of identity.

The only alternative to the "spirit" theory is the theory which ascribes the phenomena to secondary personalities, derived from the

weaving together by Mrs. Piper's nervous mechanism of all the complex suggestions of the séance room, supplemented by telepathic and clairvoyant impressions got in connection with the sitter and with the articles which he brings. For this we can find some analogies on a smaller scale; the greater part of my own experiences, if taken severally, seem to me susceptible of such an explanation, and there are a few items, such as the Morse incident (p. 24), which almost irresistibly suggest it.

Taken as a whole, however, I do not think that the phenomena can be satisfactorily explained by reference to telepathy or clairvoyance. Indeed the phenomena which those words vaguely designate are themselves too little known to provide principles for the elucidation of the less known, and although, as I have said, individual scraps of information may be ascribed with some show of plausibility to a telepathic or clairvoyant origin, the arrangement of these scraps into mosaics of thought, which, however defaced, still often irresistibly suggest the habits, tastes, and memories of some friend deceased—for this I know of no telepathic or clairvoyant analogy. For example, the demand made by "aunt Sallie" that I should identify myself by expounding the significance of "two marriages in this case, mother and aunt grandma also," admits of no satisfactory telepathic explanation. The fact was known to me and might have been got telepathically. But why is the dream personality of the only communicator who died in my childhood the only one who seeks to identify me? Why does she allude in so indirect a fashion to the mode of her death (see p. 34)? Certainly no stratum of my personality would have felt hesitation in alluding to so commonplace a matter as a laparotomy, or would have lacked suitable language in which to express the allusion. Whence came the reference to "Carson the Dr.," a circumstance which I had totally forgotten, if I ever knew it? And, finally, why was the faded personality of this almost forgotten maiden aunt evoked at all? I was not ten years old when she died, and she had been dead twenty years. She was a teacher, lived in Philadelphia, died in a hospital in New York, and was buried near Philadelphia. I do not know the exact date of her death or the exact place of her burial. Probably few persons beside her immediate relatives know that such a person ever existed, and even her relatives seldom think of her. Why were these dim memories so clearly reflected, while others, far stronger, produced no effect? Why were my memories, in process of reflection, so refracted as to come seemingly not from my masculine and adult point of view but from that of a spinster aunt who could not at first recognise me with confidence, and who, taking it for granted that her little nephew of ten had not been informed as to the precise cause of her death, expected him, although grown to man's estate, to convey a

very obvious allusion to his mother for interpretation without himself knowing what it meant ?

The telepathic interpretation of my other sittings might be criticised in much the same manner. Evidence of this sort does not suggest telepathy, it suggests the actual presence of the alleged communicators, and if it stood alone I should have no hesitation in accepting that theory.

Unfortunately it does not stand alone. It is interwoven with obscurity, confusion, irrelevancy, and error in a most bewildering fashion. I agree with Dr. Hodgson that the description given by the writers themselves of the conditions under which they are labouring would, if accepted, account for a very large part of this matter. But, even after the most generous allowance on this score, there remains much which the writers cannot explain. Easily first comes their almost total inability to observe and report the phenomena of the material world, coupled with their reiterated assertions that they can and will do so. Second should be put, perhaps, the unaccountable ignorance which they often betray of matters which upon any theory should have been well known to them. In the third place, the general intellectual, as distinguished from the moral and religious, tone of the more recent communications is far lower than we would expect of beings who had long enjoyed exceptional opportunities for the acquisition of knowledge. Concrete descriptions of the other world can be had indeed *ad infinitum*, but of organised, systematised, conceptual knowledge there is little trace.

From such inconsistent material one can draw no fixed conclusions. But there is one result which I think the investigation into Mrs. Piper's and kindred cases should achieve. For any theory some intrinsically strong evidence must be adduced, even if there be but little of it, before the theory can be given any standing in court at all. Until within very recent years the scientific world has tacitly rejected a large number of important philosophical conceptions on the ground that there is absolutely no evidence in their favour whatever. Among those popular conceptions are those of the essential independence of the mind and the body, of the existence of a supersensible world, and of the possibility of occasional communication between that world and this. We have here, as it seems to me, evidence that is worthy of consideration for all these points. It was well expressed by a friend of mine, a scholar who has been known for his uncompromising opposition to every form of supernaturalism. He had had a sitting with Mrs. Piper, at which very remarkable disclosures were made, and shortly afterwards said to me, in effect, "Scientific men cannot say much longer that there is no evidence for a future life. I have said it, but I shall say it no longer ; I know now that there is

evidence, for I have seen it. I do not believe in a future life. I regard it as one of the most improbable of theories. The evidence is scanty and ambiguous and insufficient, but it is evidence and it must be reckoned with."

If the evidence which the Mrs. Piper case affords proves sufficient to draw any considerable body of competent men into these lines of research, it will have done as much as, and more than, I can venture to expect.

DETAILED CASES.

CASE I.

Submission to Mrs. Piper of articles with which few living persons had any associations and the sitter none at all.

In the winter of 1890-1891 my interest in the problems commonly termed psychical was aroused for the first time by reading the report of sittings held with Mrs. Piper in England. Soon afterwards, February 18th, 1891, I wrote to Dr. Hodgson, with whom I had at that time no personal acquaintance, saying that I would be glad to arrange for some experiments with Mrs. Piper with a view to discover the source of the information conveyed or suggested by articles. I told him that I had in my possession articles that had belonged to persons long since dead, of whom few living persons know anything. I hoped, in the course of my work as an amateur genealogist, to discover more about some of these persons, and might be able to verify statements now made by Mrs. Piper, which are absolutely unknown to anyone living.

Dr. Hodgson replied accepting my suggestion, but asking me carefully to refrain from telling him anything whatever about the articles which I would send. On the 9th of the following May I received a letter from him asking for the articles, and I at once sent them on. Each was wrapped in paper, sealed with sealing-wax, and then wrapped in rubber cloth.

Dr. Hodgson submitted these articles to Phinuit at sittings held May 15th, 21st, 25th, June 4th, and June 5th, 1891. Phinuit talked volubly about them, but said little that was in the least relevant; a large part was distinctly false, and the balance either unverifiable or so vaguely stated as to be worthless for evidential purposes.

[The above series of sittings were those to which I referred, in connection with the articles furnished by Mr. "V.," on p. 132 of my previous Report on Mrs. Piper, *Proceedings S.P.R.*, Vol. VIII. It appeared that none of these articles fulfilled the condition of having been much handled or worn exclusively by only one or two persons;

and these experiments, with others of a like nature, incline me to think that this condition is practically necessary for any success. I shall refer to this question in Part II. of my Report.—R. H.]

CASE II.

Frederick Atkin Morton.

In the spring of 1893 I met a man whom I shall call William Morton. He was an intelligent man, by profession a dealer in real estate, loans, and mortgages, of limited education and of rather feeble health. In 1894 I found that his tactile sensibility on one side was defective. He told me that he had for some time heard rapping sounds, often saw apparitions, and at any time, by allowing himself to fall into an abstracted mood, could hear faint voices whispering to him. He found it very difficult to hear what the voices said. On one occasion, during a séance in May of 1892, he had fallen, he told me, into a trance and believed himself to have passed into another world. He described his experiences to me in detail. His hand frequently wrote automatically, and at the inception of the writing the arm was much convulsed, much as in Mrs. Piper's case. The content of the writing gave no evidence of supernormal powers, but in the whispered voices I detected what seemed to me indubitable evidence of telepathy, possibly of clairvoyance.

On March 24th, 1894, I happened to meet Mr. Morton unexpectedly, and in the course of conversation he told me that he had predicted a suicide, which was afterwards verified. I pressed him to give me names and circumstances, but he refused on the ground that the person who had committed suicide had borne the same name that he did. I then said that a friend of mine, Frederick A. Morton, had taken his own life not long before. Mr. Morton replied that that was odd, the person of whom he had been speaking was an uncle of his own, yet he thought that a person calling himself Fred Morton had presented himself to him at a séance some time in the preceding December, said that he had committed suicide by shooting himself through the head while standing before a mirror, and wished to send a message to someone whose name W. M. could not remember. The death of Mr. Frederick Atkin Morton took place under the circumstances above described, but as full accounts appeared in the papers at the time, and as I had mentioned the name, W. M.'s knowledge of it was not surprising. After this I frequently had, through the automatic writing of W. Morton, what purported to be messages from Frederick A. Morton, but found in them not a suggestion of the alleged writer's presence.

On April 13th, 1894, I had a sitting with Mrs. Piper arranged for me by Dr. Hodgson, but she was unable to go into a trance at all. W. M. knew on this occasion that I went to New York, but I have every reason for thinking that he did not know my errand. On my return, I procured from Dixon Morton, a brother of Frederick A. Morton's, a tie, a bunch of keys and two small books, which had belonged to F. A. M. I did not tell D.M. what I wished them for. On the evening of April 26th we had a sitting with W. M., at which alleged messages came from F. A. M. I asked the writer whether he would appear to me the following day through a medium I expected to see. At first he refused, on the ground that I did not believe in his identity, but after a little persuasion he agreed to do it. I offered to tell him the address to which I was going, but this he refused, saying that he knew it already. He gave an address in New York, at which Mrs. Piper had never been. Moreover, in the two weeks which had elapsed since my first sitting, Mrs. Piper had changed her address. I learned it myself late on the afternoon of April 26th and immediately destroyed the letter. I went to New York early on the morning of April 27th, taking with me the articles. The first sitting was extremely confused.

At this, as at the former sitting, I passed under the name of "Smith." Mrs. Piper went into trance almost immediately. The articles were at first grasped by Phinuit and held against the back of the neck. Later, they were attached by a rubber band to the back of the hand. (N.B.—That notes marked D. M. are by Morton's brother, whom I have termed Dixon Morton ; others are by myself.)

[Phinuit began by making some complimentary remarks about me and ventured a prophecy that I would soon go abroad. Then follow in my type-written MS. two pages of confusion, in which several persons seem to be talking at once. The names Perkins, Ransom, Clarence (*sic*), and Edith are mentioned. Such scraps as] Where are my books and how about the papers let me . . . isn't this strange to see the keys where . . . do not worry, I shall be very clear soon and then remember . . . oh, do try to remember me I am . . . my head is troubled . . . my head and face hurt terribly . . . my head was very bad . . . [might plausibly be ascribed to the speaker who afterwards becomes more coherent.]

Billie Billie E. [Very few of my friends call me Billie ; F. A. M. did, and in a letter received a short time before his death he spells the name in this way. I have always spelled it Billy.] [R. Hodgson makes some remark.] Don't worry about me sir I want to see my brother . . . also mother tell her I am . . . Billie dear old chap . . . I see you Billie . . . Billie speak . . . I have been . . . yes and . . . (Shall I tell you your name ?) No don't please Billie, speak don't please tell me my name because I'll tell it you in a clear CLEAR LIGHT for I am getting better now. [The following clause is indistinct, seems to

be] what if we will have it yet [?] my head is getting clear and I shall soon prove to you who I am J [?] Read dear D U [?] perhaps B O] B your uncle's BOY. [I was educated by an uncle who had no son of his own and was much attached to me.] Yes B. O yes b B. yes. Do you know Jack. (No.) [For the moment I had forgotten our friend Jack McKenzie Walker. He tells me that F. A. M. seldom called him Jack, but usually John.] Yes, don't you know Jack and what became of him, Billie . . . Sharpless (Did you know him?) Yes, I knew him Billie old chap. Yes, he had my book on Philosophy and never returned it look it up for me . . . (What book?) One of the old ones. Plato, yes I want [?] [F. A. M. was acquainted with Paul Sharpless. There is no truth in the account about the book.—D. M.] [N.B.—I do not know anyone named Sharpless.—W. R. N. All that follows is very illegible.]

W at att at kennn k ke kenn, connect it together and you will have part of my name. [It was not until we were copying this section that I deciphered this; the writer is clearly trying to write Atkin.] Oh yes, my head Billie is muddled a little . . . Billie what are you doing here. [hand reaches up and feels my face, strokes, and grasps my beard, pats me appreciatingly, and writes] changed a little. [I had seen F. A. M. only once in about five years. Prior to that I wore a moustache only. On that one occasion we took dinner together and I then wore a beard. The hand throughout betrayed a great deal of emotional excitement which, as well as the affectionate expressions, was very unlike the F. A. M. whom I had known. In its convulsions the hand at this point came near striking a finger which Mrs. Piper had crushed not long before and which was very painful. Hodgson warned the writer to be careful of it.] Fingers don't trouble me. . . . How are you getting on with your work, old man. (Very well. I am still at the University.) Yes, I know it and (You know I have tried to communicate with you before?) [This was a most stupid remark of mine, and consequently is duly recorded.] Yes, but did not succeed. (Where was it?) At a ladies' place. I saw you. I tried. [I had seen a medium named Miss Gaule in Baltimore, in March, 1894, but no reference was made to F. A. M. and I was satisfied that she was fraudulent.] (You tried to communicate?) Yes. I am telling you this . . . Walker . . . W A L K E R . . . don't you remember [v. sup.] (No.) No I gave you a message for him. Yes. (When did you do it?) long time How long have I been here? (Nearly a year.) yes yes S [illegible; looks like] GUPLI I can't do it [H. talks to him. To H.] Can't you leave me alone till I see B. (H. : Shall I go out?) Will you sir you make me confused, confuse the head. (H. : You mean I confuse you?) Yes I think so. [H. goes out. The hand writes more freely and betrays increasing excitement.] I am [?] longer L E W do you know me T O M I am T O M a s . . . now don't you know . . . where am I . . . what have I in mind . . . F R E D . . . [the emotional excitement appeared to reach a maximum at this point. The gestures indicative of friendliness and affection, warm handshaking, caresses about cheeks and head which had already often occurred became more violent and frequent. My hand was wrung repeatedly. Taking up the pencil again, the writing was resumed.] I am only teasing you Billie . . . I am F R E D I wanted him [pointing toward door] to go out till I could

tell you all I feel for you. [I make similar protestations.] Well don't I know that . . . I think mother and all . . . yes what can I tell them. [D. M. : The word mother was never used in address or reference by F. A. M.] (I'll let them know I have heard from you.) Good Oh Billie old chap how are you getting on do you ever see any of my folks (I have seen your brother once or twice) I . . . I told you about him, my brother . . . I . . . too hard . . . where is he . . . tell Cha— don't you know him . . . Charles I mean, what has become of him. [D. M. : Unrecognised.]

(I don't know, but I will try to bring some of your family to see you.) Good good (Shall I give his name?) No I'll tell you . . . get him . . . yes . . . Will are you still there (Yes, I'm here, go on, old man) tell George (Who is he?) don't you know? to come in and help me . . . (You mean you want George Pelham to help you?) Yes too bad [I turned to Phinuit—i. e. addressed Mrs. Piper's ear, instead of her hand—and said, "Doctor, this gentleman wants George Pelham to help him." "All right," said Phinuit, "I'll go and get him." After a slight pause, the writing recommenced.]

Billie where are you . . . tell me something . . . yes . . . how and who is Edith . . . I see her in your life . . . [Not significant.] I am not dead old chap, I told you about him D.D. yes too bad D I ck [I could not at the moment read this.] Richard over there. (Do you mean Mr. Hodgson?) No J [?] ick (Do you mean your brother Dick?) Yes [much excitement] my brother's name. [A confused medley of capitals follows, in which D, I, and M can be discerned.] F R E D [Then but not very legible, comes] Dick [Some large illegible scrawls. I asked again whether he had tried to communicate before] speak to . . . Billie speak to you do you mean did I try to speak to you . . . yes I did at a man's house (Who was he?) I don't know anything about who he was O I was going about and wrote to you . . . Yes and let me get clear and I will do all . . . (Did you promise to talk to me to-day?) I said I will when I saw my keys. I knew them immediately . . . Read Read [?] also my diary and you bet I am not dead if FRED Morton ever lived I am he and there is no use disputing my identity any longer (I am not disputing it, Fred; don't suppose I doubt you) I don't I don't Billy [much excitement and illegible scrawling.] (You need not prove your identity any further.) Yes I will Yes I will now look here Billie do you remember the little scar . . . [scrawling and excitement] yes I will . . . do you remember I had one [the hand here dropped the pencil, reached up and felt my temple to show where the scar was. At the time of writing up the sitting I thought it was my right temple but was not sure; later the hand said it was the left. When the scar was first mentioned I had no recollection of it; in a moment or two a faint recollection began to take shape in my mind and it has ever since been quite distinct. I think it must be an hallucination of memory, of which I have had several, for I have found only two other persons who had any recollection of it at all; neither is sure of it, and those who knew him best, including all the members of his family, know nothing of it]. This [?] I say to prove my identity. [I express conviction.] Good old chap, love to Dick, Alice. [D. M. : Unrecognised.]

mother . . . until I see you again . . . I must fly away old boy
 . . . be good. [Pencil is dropped.]

I then telegraphed for the brother Dick. He came on the evening of the 28th. I met him, and on the morning of the 29th we went to the house where Mrs. Piper was staying. He was introduced as Mr. Jones. The first writing was by George Pelham; after alluding to some other matters concerning some of Dr. Hodgson's earlier experiments, he said, "There is a young man here who is extremely anxious to speak to his brother. I'll help come in here and speak to him." While G. P. was writing Phinuit was talking to me. Several times he made remarks such as, "Now, don't be in a hurry, you'll have plenty of time to talk soon," which I could not understand. I asked him what he meant, saying that I was not in a hurry and never said I was. To this Phinuit replied that he was talking to a young man in the spirit who was in a great hurry to begin communicating.

[F. A. M. writes]

Oh my dear Dick I am so glad to see you here my dearest brother for I love you [D. M.: Not characteristic] . . . come nearer to me . . . I am no longer dead . . . do not shed tears for me . . . I am in another life and not far remote from your own material . . . Where are . . . oh speak to me Dick and Billie I love you both more and more spiritually, love to dear mother how is she? . . . dear Dick speak that I may hear your voice dear Dick Oh speak to me dear just as you would if you could see me I know you cannot yet trust me dear and I shall always be with you . . . do you know how I left everything . . . too bad . . . my head was in a terrible state, dear. [The autopsy revealed extensive congestion of brain.] . . . I did not suffer as you may have thought, yet . . . (Didn't you have pain?) No dear . . . no I was unconscious of pain I assure you I know and remember very well . . . there is . . . too bad . . . I [undec.] not know pain . . . I could not help it Dick . . . no, my head was in a delirious state . . . I leave all things to you, dear Dick, and I wish you to think all is for the best. God is wise and good and I leave all in his holy care, Dick, and believe me when I say I am sorry for you all—yet I can no longer worry with you. . . . I am happy now.

[In response to leading questions from D. M., the writer claims that he had been in a confused condition a few days only, that his mind was affected, that he suffered little, knew that he was out of his mind and tried to save himself. None of this is verifiable, but is not improbable. When questioned about the alleged attempt to communicate through the medium W. M. he said that the errors were due] to his mind acting . . . his own mind acted in error.

(Where were you on the Saturday and Sunday?) Where was I at the time, Dick? . . . In the room. (All the time?) All Sunday, if I remember rightly . . . Not Saturday, but Sunday. [D. M.: Not verified but not improbable.] [When asked whom he saw on Saturday,

after considerable confusion he said he saw a person named Parker Howard. He had red or sandy hair; he was in trouble and wished F. A. M. to help him. Said that he [P. H.] was a newspaper man; named the paper upon which he was employed. For further history of this incident see p. 19. After much confused and irrelevant material occurs the remark]

Who is taking my place now? [Apparently referring to paper edited by F. A. M.] (You mean on journal?) Yes. (It has been discontinued.) Too bad; I am sorry it is dropped. Could you not keep on with it? It would help you Dick, and be a success I am sure. . . . [I asked if he promised to come here.] I did promise. [to] Now if I could see you I would. (The blunders were the medium's.) [?] Yes . . . Yes. (You must have been very angry.) I never get angry nowadays; you see I cannot hold him in check, B. [Asked whether the medium cheats.] I do not think that he does. . . . I do go there sir [to R.H.] I see a light. . . . Do you know, dear fellows, you will ever be rewarded for helping me to reach you in this light and trying to free my poor imprisoning mind. [R.H. explains this remark to us. Writer is struck with his ready comprehension.] Yes. . . . Yes, exactly, sir,—who are you?— I cannot touch you sir, or reach you, sir. [R. H. moves his head forward; hand feels his head.] Do not know you, sir. [It is explained who R. H. is.] . . .

[D. M. : I want to know something more than anything else—will you tell me ?]

What!—you know I will if in my power.

(Were you disappointed about the paper?) Yes, a little.

(Did you think it wouldn't prove a success?) I feared so . . . not . . . yes, I feared very much. (Were you much depressed by it?) Certainly [?] yes . . . this is what overpowered me, and led me to take my life. . . .

(I thought you said you took your life in delirium.) You don't understand. Now listen, dear.—*Viz.* I was depressed, and very much so, but began to recover, as I thought. When I saw P. H. on Sunday, Saturday I mean, I told him I was feeling badly, but I truly did not realise how badly. When on Sunday I began to lose my mental equilibrium, then suddenly I realised nothing and nobody. This is absolutely *true*, Dick. [F. A. M. was found dead on Monday morning, but the question, "Where were you on Saturday and Sunday?" had before been asked.]

(R.H. : And what was your next experience?) I found I was in this world. . . . I did not know for the moment where I was, only I felt strange and freer. . . . my head was light in weight, also my body . . . my thoughts began to clear when I observed I had departed from my material body. Ever since then I have been trying to reach you, Dick. I saw a light and many faces beckoning me on and trying to comfort me, showing and assuring me I should soon be all right, and almost instantly I found I was. Then I called for you, and tried to tell you all,—where I was . . . after all) after all, sir,—put this) after the word all)

[N. guesses at meaning.] Not at all . . . after the) after the)

[meaning understood, viz, comma after *all*.] Yes, I never used to write badly, what's the matter with me now, Dick, don't I write well?

— Then I called for you and tried to tell you all about where and how I

was, and with one exception, this is the only chance I have had . . .
Now you see I am making . . . taking advantage . . .

(R.H. : You are writing over the other words.) Why did you let me do it.

(R.H. : We tried to prevent it—did all we could.) . . .

don't scold me, sir, I am doing my best. . . .

. . . advantage of the opportunity . . .

[R.H. tells Phinuit that it is time to stop, etc.]

Yes, D., I wish I could . . . I want . . . I will . . . I
wish . . . and have you know all I feel, sir . . . Oh, Dick, I did
not mean to do anything wrong . . . stick . . . yes, sir, I will go
in presently.

(R.H. : You mean out.)

Out, sir . . . Dick . . . love to Ma . . . Dick, God bless
you and B. always . . . must I go . . . good bye . . . not
good bye . . . not good bye.

I'll see you again . . . fid [find ?] P [?] H.

[Hand takes pencil again later, and writes Pistol.]

[D.M. : Death resulted from a pistol shot.]

The account here given of F. A. Morton's death is correct as far as
can be verified. He had been engaged in editing a paper for a few
weeks only. One Monday morning in the summer of 1893 his body
was found lying before his bureau, a bullet through the head and a
revolver lying on the floor beside him. No reason could be assigned
for the act; no one could be found who had seen him for some days,
but those who had last seen him thought him as cheerful as usual.
The autopsy showed that the brain was much inflamed, and the
physicians said that he was probably in the first stages of brain fever
at the time of his death.

In the course of the month of May I had some sittings with an
educated young woman, not a spiritist, who had shown tendencies
to automatism. Her automatic writing was of the most rudimentary
kind and never became legible. She told me she had once seen a table
rise from the floor while no one but herself was touching it to a height
of a couple of feet or so above its normal position, but I was never
able to repeat this experiment and never myself saw anything which
would lead me to think that she had any supernormal powers. On
June 7th, 1894, Dr. Hodgson had a sitting with Mrs. Piper at
Arlington Heights at which Fred Morton presented himself. He
said he had had great difficulty in reaching Billie although he, Billie,
had been asking for him, that the light was not sufficient to enable
him to give clear and accurate messages. This light was a woman but
not so much entranced as Mrs. Piper:—"we see this one's spirit as
we see our own, but not the other's as clearly, only part of that spirit
is visible to us sir. We communicate through the mind of the half-
intelligence . . . I have seen him in two distinct places, one a

lady, the other a gentleman, the last one is a lady and the first a man." Dr. Hodgson saying something which implied that he supposed this medium was impressed by hearing only, the writer interrupted "Look here sir, I can make her write as well."

At my request, R. H. had a sitting with W. M., at which nothing of interest occurred. Upon his return, R. H. was informed by Phinuit and G. P. that W. M. had a little light, but was not altogether honest.

Being unable to discover Parker Howard, Dr. H., at my suggestion, asked the writer whether he had meant Philip Hoard, whose name had been mentioned to me by Dr. M. The writer showed excitement and claimed that this was the person whom he had had in mind, reiterated the statement that he had seen him just before his death, and that they had spoken of doing some "journal work" together.

I found that Mr. Philip Hoard was paying teller in a bank; he had known F. A. M. very slightly and had not seen or communicated with him for three years before his death. Nothing that was said of Parker Howard is at all applicable to Mr. Hoard save the allusion to sandy hair, Mr. Hoard's moustache being reddish brown, while his hair is quite dark. See the further statements of June 23rd.

Sitting of June 21st, 1894, Inter Alia.

[F. A. M. commences.] How are you B? Philip Hoard and Frank Bezay.

[I was so surprised at seeing this name appear that I interrupted him.]

(Why, I know him, did you?)

Good; give him my love and ask him if [then after some confusion] I remember the evening we went up to the office and opened our window, sat in the moonlight, talked over the subscription for our [?] paper.

[I tried to get an explanation of his statement about Mr. Philip Hoard without success. Writer insists that he did see him.]

Do you remember Jack? (Give me his whole name) McKenzie. (Is that his whole name?) two names B— at the Club yes you have it B— but let me tell you all. [I try to read] keep still . . . Sa . . . do . . . Sur [?] Sa [at this point there comes an interruption referring to one of G. P's. relatives.] Marion will be all right G. P. says—good. . . . Now about our Club. [I still fail to understand; to me "the Club" is the *University Club*, but F. M. was not a member.] Why don't you wake up B. and understand me. [Then it occurred to me that he meant the *Survivors' Club* and I asked whether that was right. Much handshaking, efforts to turn back to portion not before deciphered—writes] S U . . . and are new a d d i t i o n . . . (You mean are any more married? We had our dinner early in May). [Much handshaking—writes] I was there. * * * [Statement here made strikingly relevant to a member of the Club.] (It was at Bickford's you know I last saw you) [much excitement.] Oh, I tried and tried to think of him but no, I declare I could not before where is he, he is a good fellow. [This entire incident is curious. In May

of 1893 nine of us of whom John McKenzie Walker and F. A. M. were two, met at the house of a friend, celebrated our escape from the matrimonial snares into which so many of our friends had fallen, and formed the "Survivors' Club." We agreed to meet yearly thereafter and drum out of the club those who had in the interim engaged themselves to be married. As the "Club" had no existence save at the annual dinner and its members seldom met during the year, I hardly thought of it as a club at all.]

The writer then asked me how things were going "at the college." I gave him some items of University news which were appropriately received. He mentioned himself the surname of a University officer; when I read it with the name of the office attached and asked whether that was what he meant, he replied] yes I do, the old idiot [which was a rather emphatic expression of a sentiment which is quite widespread. I do not know whether F. A. M. shared it. The writing then becomes very much confused, and when] J. McWalker . . . Jack McKenz . . . and . . . Walker is in my mind [was written, Phinuit said the light was bad, and I put my hand over and about a half inch away from the writing hand.] Yes, thanks, I am so delighted to free and clear up my thoughts . . . How is Phl Phil, Philo (Is that all?) No Philoe oh you know [There is a literary society at the University of Pennsylvania,—the Philomathean Society, commonly known to the students as "Philo," and F. A. M. was a member. Here follows a page of confusion, scarcely any of which is intelligible. The writer asked again after his book on philosophy and repeats that he lent it to Sharpless; the remainder of the sitting is of no evidential value.]

The sitting of June 22nd was devoted to the F. A. M. case, Dr. Hodgson, Dixon Morton and myself being present. I give here merely the gist of the statements which were unverifiable or untrue, and copy in detail those which proved true.

[F. A. M. stated that his mother was at the time suffering from severe dizziness and headache: this cannot be verified. The location of the scar was given as on the left temple. It was] quite conspicuous; I parted my hair to cover it. Ask Mother if she does not remember the fall I had from the steps at Carter's years ago. She and I were together when I fell I know and remember it very well and if you ask her she will know what I mean. [Mrs. Morton remembers nothing of the kind. D. M. asked what the writer called his mother during life. After four or five attempts, during which D. M. was careful to refrain from any sort of suggestion, the writer wrote distinctly] M A M A' [with the accent upon the last syllable. D. M. read it so, asking whether that was right, and was told it was. This was correct. D. M. then asked about a difficulty which his brother had had with a certain man. The writer professed to remember it well, but when asked for the name gave it incorrectly as Henry Hollis. D. M. then asked about F. A. M.'s plans for a country house and got a good many confused statements involving this Hollis and a man named Frank Eliot. None of this can be verified and most of it is known to be untrue. No such persons as Eliot and Hollis have been found. I asked again about the Frank Bezay whom the writer had mentioned before.] (What is his profession?) medicin

[very illegible] Dr. Dr. (What does he look like?) can't get it on here B—. (Has he any hair on his face?) [Hand pulls my beard.] (Ah! so he had a beard like mine?) [Assent] (What is its color?) Dark B— Brown or dark Brown or Red . . . Red . . . Dark Brown or Red we call it. . . . Yes, Dr. B— Red . . . Why don't you say yes when you hear me? (Where did you have this talk with Bezay of which you spoke?) [In this I was wrong for it is not quite clear whether the writer intended to ascribe the talk to Mr. Hoard or Dr. Bezay] at the . . . in his office when my head was muddled. (Do you remember where it was?) Yes I do. (Could you tell us?) Yes, in—Philda. (What street?) on Broad. [Here R. H. suggests that the first thought occurring to a writer is often written.] Yes I remember this street. (Is Bezay's office on Broad street?) I can't recall this B—.

[This Bezay incident is most perplexing. In the winter of 1890-91 I met at the house of my brother-in-law a friend of his named Dr. James Bezay. I saw Dr. Bezay perhaps two or three times. In the winter of 1892-93 I saw him once or twice. He sometimes wore a beard of rather light tint but not red, as I recollect it, and sometimes a moustache only. At the time of the sitting I thought his name was Frank. Upon my return home I endeavoured to find Dr. Bezay and at first without success. But I did find that there was another physician named Frank Bezay who had an office on — street just off Broad and the above description exactly fits him. He is rather tall and heavily built and wears a beard and moustache of a dark reddish brown. Although I had no conscious acquaintance with him it is very probable that I had heard his name and had possibly seen him. Dr. Bezay's office is about five squares distant from F. A. M.'s former rooms, but Dr. Bezay tells me he never knew Mr. Morton and can recall no such visit. I afterwards found Dr. James Bezay and learned that he also did not know F. A. M. The occurrence of the name, coupled with so accurate a description, is therefore very puzzling.]

[We enquired again about Philip Hoard. Were told that Mr. Hoard has a moustache and wears eyeglasses. The moustache is described as light Mr. Hoard is tall, slight, has dark hair, dark reddish brown moustache, and wears eyeglasses. He was so kind as to read over my accounts of all these sittings, but was entirely at a loss to explain the allusions to him.

The writer now spontaneously attracts my attention to what he is about to say, and after some difficulty, during which I was very careful to give him no assistance whatever, succeeds in writing the name]

Andie Le Grand

[which was the name of a common friend of ours who was much better known to F. A. M. than to me. He lives in New Orleans; I have not seen him for more than ten years. The writer proceeded to give us a long account of a prank in connection with a bat and ball, which he says that he, A. Le G., and Jack Mackenzie Walker had played. It was impossible to verify this incident. Mr. Le Grand and Mr. Walker remember nothing about it.]

The chief points given by this communicator which would go to show his identity with the real F. A. M. are:—(1) His use of the word

Billie. (2) The giving of his own name in full. (3) The mention of Jack McKenzie Walker, Andie Le Grand, Sharpless, and the name of the University officer of whom I have spoken. (4) The mention of his brother's name Dick. (5) His evident acquaintance with the manner of F. A. M.'s death, especially in the second sitting. (6) The allusion to the Survivors' Club. (7) The spontaneous references to college news—F. A. M. having been a college friend of mine, and seldom seen elsewhere. (8) The statement that he called his mother Mamma'. As ambiguous items are to be mentioned:—(1) The statements about Mr. Hoard and Dr. Bezay, which show a curious mixture of truth and error. (2) The reference to the scar, which has neither been proved nor disproved, but is probably erroneous. (3) The "prank" episode, which is possibly true, but probably not. (4) The statements as to the circumstances of his death, which fit very well with all that is known, but cannot be demonstrated. (5) The allusion to Philo, there being nothing in the context to show what is meant by the word. (6) The fall from the steps. The most important points which are wholly false are the statements about Henry Hollis and Frank Eliot, about seeing Mr. Hoard and Dr. Bezay. The case is further complicated by the statements made about Wm. Morton. Phinuit, George Pelham, and F. A. Morton are responsible for the statement that W. Morton "had light," i.e., was a medium. I do not think anyone could draw that inference from Mr. Morton's own performances, although it is possible that he had some supernormal powers. On the other hand there is nothing intrinsically impossible in the supposition that W. Morton both had supernormal powers, and was also an hysterical automatist. A somewhat similar problem arises in the case of Baker (*see Case III*).

CASE III.

Kittie Murdoch, involving that of William Baker.

In the spring of 1894 I had the opportunity of studying at length an interesting case of automatic writing. I shall term the subject William Baker. Mr. Baker was a man of education and had been known to me for some years. He had dabbled more or less in spiritism, had seen automatic writing, and had once been told by an automatist that he could write automatically himself. He tried to do so several times without success. In the spring of 1894 he chanced to be at a séance at which no professed spiritists were present, and while sitting with his hands upon the table, suddenly felt a sharp contraction in his left upper arm. This surprised him, as he had no expectation of any "manifestations" occurring in connection with himself and was interested in another person present who was supposed to be a medium. The contraction

presently disappeared. A few moments later, after walking about the room, he sat down and took a pencil; immediately the contraction recurred in the right arm; and the hand made desperate efforts to write. It produced nothing save scrawls and a few words which happened to be in his mind. That night upon getting home he tried again without success, the hand remaining motionless. The following evening he tried again, and after waiting about forty-five minutes attempts at writing were made. The words "Yes" and "No" were written and questions were answered, but the replies were often inconsistent or untrue. Finally the hand spelled out, letter by letter, the syllable m-u-r-d, or at least Mr. Baker so read it. I examined the writing myself carefully and found the letters by no means clear, although they look more like m-u-r-d than anything else. It then flashed into Mr. Baker's mind that a certain lady named Murdoch had died about three weeks before. He had not known her but knew her brother. As soon as this occurred to him the letters och were rapidly written, and when asked whether the writer was Miss Murdoch there followed a series of violent contortions and blows, covering the paper with huge scrawls, and making a "pool" of pencil lead in the midst of the sheet. Finally the pencil was driven through the paper and the point broken. Mr. Baker was sure that this demonstration did not come from his upper consciousness, but in view of the later developments of his case I do not think his confidence is entitled to much weight. He called my attention to these phenomena and we got Miss Murdoch's brother, Mr. Edward Murdoch, to assist us in trying to determine their origin. At this time the writing was produced slowly and with difficulty, only one letter being written at a time and that usually only after many attempts. Mr. Baker distracted his attention by reading aloud. Nothing was written that would at all suggest the actual presence of Miss M. save one sentence:—"The homefolks have forgotten, Ted, before I was dead, how wrong it is to grieve." This struck Mr. M. as curious, because the name "Ted" was never used outside his home, and Mr. Baker had seldom, if ever, heard it. Many other statements were made by the hand which were found to be false. Before long the writing became more facile; other alleged spirits appeared and wrote messages which were easily shown to emanate from no other source than Mr. Baker's own consciousness. This development was attended with various unpleasant symptoms, chiefly of a choreic character, and in about three weeks Mr. Baker refused to allow his hand to write automatically any more, being convinced that spirits had nothing to do with it and that it was having a most injurious effect upon his health.

When I went to Boston for my series of sittings in June of 1894 I secured the consent of Mr. Murdoch and of Mr. Baker to attempt

getting into communication with the alleged spirit of Miss M. in order to see what explanation, if any, would be offered of Mr. Baker's experiences. I said nothing however to Dr. Hodgson of the Murdoch case.

At my first sitting of this series, June 20th, 1894, [Present: R. H. and W. R. N.] Phinuit asked me whether I knew a "lady who is a teacher. She is going over her school and college experiences." "Yes," I said, "I do; ask her to write." [I did not then know that she had been a teacher, but the allusion to college led me to reply in the affirmative.] "Lady who taught," said Phinuit, "is going over things that happened in college. She wants to come and thinks she knows you." [Miss M. had been a teacher for some years and at the time of her death was attending a well-known college. Phinuit then said:] This young lady who used to teach recognises you faintly. Young man [i.e., F. A. Morton] is trying to get her to wake up and realise she is talking to you. (How long has she been there?) Only a few years, long enough to get far from earth and we have to get her attention. [Miss M. had been dead about seven weeks.]

The following day a curious incident occurred. While experimenting with Mr. Baker I had heard Mr. Murdoch speak of a gentleman in such a context as to lead me to suppose that there might have been something in the way of an attachment between him and Miss M. I have since learned that this was not true; that the gentleman in question had been dead for several years, and that Miss M. had had only the slightest acquaintance with him. I shall term him Morse—the real name is much more unusual. Towards the end of the sitting of June 21st [Present: R. H. and W. R. N.] the writer, who was the alleged F. A. Morton, fell into hopeless confusion in the midst of which comes the following:—

Here B— . . . what is M O R D . . . go [?] to [illegible] with it . . . E S . . . M O R T I N . . . N a . . . M O R . . . yes she speaks and is saying not right yet B— . . . M O R E S E . . . M O R R E S . . . Dear M— yes . . . [More confusion] I'll speak to P— and he will say Morres Morriss. (N.: Very good, I understand.) How do you speak it . . . it sounds thus . . . He says this and knows what he is saying. (N.: Try to give her name.) [Another mass of confusion without relevance to the question.]

On June 22nd [Present: R. H. and W. R. N.] I asked the F. A. M. writer to find for me the lady who said Morse, and he said he would. On the 23rd [Present: R. H. and W. R. N.] in the midst of an alleged communication from an aunt of mine given through G. P., there came an interruption . . . Morr . . . Morrsa . . . Errsa . . . [Another interruption] Morriss (N.: Does my aunt say this?) No . . . how do you pronounce it sir? (Morse) Yes exactly. [Towards the end of the same sitting comes another mass of confusion, in which I find] his mother . . . Fred's . . . Morriss . . . Mors . . . Mortons . . . tell him his sister wishes to say this also . . .

no not at all only she calls this in connection with himself . . .
 her name . . . Morton's . . . Morres . . . (N.: Can't you
 give me her name or his name?) Oh I do not know only she keeps repeating
 E . . . oh no . . . it is too bad . . . could I not give it to
 you next time sir . . . I cannot tell [?] I'll talk with her and tell you
 all she says about . . .

[While I was in Boston I received from Mr. Murdoch by express an old copy of Shakespeare which had belonged to his sister. It contained upon the flyleaf the name of her father, but her own name was not in it. I produced it at the sitting of the 25th of June [Present: R. H. and W. R. N.] and asked G. P. to find the owner. The hand replied:] she taught when in the body and knows this and says she will try to speak to you (Could you give her name?) I'll . . . I don't believe it because I can scarcely hear her, she seems so far away. [Motions to book] let me see this again will you? [After the writing stopped and Phinuit appeared, I called his attention to the book, and he said he would find the owner, she taught. Upon his disappearance the right hand and arm were violently convulsed, arm twisted backwards, and the hand pressed in the back between the shoulders, the fingers moving as if to write. I was unable to dislodge the arm. I then put pencil in the fingers and paper under the hand against the back; a few scrawls were made, then the arm began to thresh violently about, grasping at papers and overturning the table. Becoming more quiet it began writing, slowly, and in small characters] Give me my book . . . [illegible] (what is your name?) Adelina [I read it] No . . . I cannot . . . (Tell me your brother's name) Ed . . . (Would you like to see him?) Yes yes [The writing then degenerated into mere scrawling. Hodgson took the pencil from the writer, saying that the light was nearly gone and she must wait until next time. While coming out of trance Mrs. Piper began describing what she saw.] "I saw a pretty young lady, she had her hair done up on her head back from her forehead . . . She's leading a little girl by the hand who passed out with a throat trouble, she has light curly hair . . . the lady is not very fair—rather medium." [Miss Murdoch, I understand, was short, quite stout, and had brown hair, neither very light nor very dark. This I did not know at the time. Her two sisters, whom I did know, are fair, and I had pictured her as fair also. She had lost a little sister, but the child did not die of a throat trouble, nor had she light curly hair. Her hair was light brown with a ripple in it but not noticeably curly. I knew nothing of the little sister.]

[At the opening of the sitting of June 26th [Present: R. H. and W. R. N.] Phinuit said] Oh, Hodgson, if you only knew what people said of you here! (What do they say, doctor?) They say you are a brute, Hodgson. I tell you that lady won't come back for you now. Why did you speak so roughly to her. [H. expresses his regret and says it was necessary that she should go and she did not do so when asked, etc.] You ought to coax and not drive her away. George and I have been trying to coax her to come but her feelings are hurt and I do not believe that she will. [The book was put on the back of the medium's neck. G. P. begins writing on other topics; finally I ask him about the lady to whom the book belongs. He asks] "who is this Elver whom she continually calls

for? [and adds that] Elver is her cousin. [No such person is known. In the confusion which follows May and Alice are mentioned. H. is told to go out and goes. G. P. continues:]

Here she stands and is trying to come close to me . . . Alice V (Is that N?) No . . . (Try to give her last name.) Yes I will M i c . . . M i . . . R a . . . strange name sir . . . her brother's Christian name she calls E. . . Ned . . . yes he . . . yes a queer . . . byren . . . Byron it sounds . . . [I read this] no . . . now not this my friend . . . sister Alice is hers also . . . May a sister . . . Alice I . . . and LI zzie . . . A. E. M. . . artha called Patty . . . that is your mother and . . . (Just now, Mr. Pelham, I wish to hear from this other lady.) I understand, she is trying her best to tell me something but why do they keep calling Mrse now . . . yes I do I think Mattie . . . is she one of your friends . . . give me the book will you. [Takes the book and without opening it more than was necessary inserts hand between the leaves in various parts of the book, allowing it to rest awhile in each. I watched to see whether the fly-leaf was uncovered; it was not.] No it is heard distinctly by yours [i.e., truly] A. W. M.

[Miss Murdoch has a sister Alice, a brother Ned, sometimes called Ted in the family, and a sister Mary who was formerly usually called May but for the past four or five years more commonly Mary. The allusion to Pattie is correct (cf. case. . . Lizzie is not significant to E. W. M. The initials A. E. M. probably stand for K. E. M. which are the initials as later, although erroneously, given as those of Miss M.) (Try again to give the last name.) Well it is W I R . . . no . . . M I R R . . . M I R . . . M U R . . . yes . . . M U R . . . no . . . ll . . . yes . . . I am listening [with all] my ears but . . . M o r . . . oc . . . she says this distinctly . . . (That is partly right.) I know it sir but she is still speaking it . . . Mur [scrawl] . . . good, I hear . . . Mur oc [scrawl] no oh dear . . . Mur . . . ll . . . L L . . . A. W. M. . . (I read it. Is that what you mean?) I do, this is exactly what she repeats to me A. W. M. [Motions towards book.] Give it me. [Handles it as before.] This will . . . she will have it O. K. sir . . . M U R . . . P . . . D . . . [I read this as P] yes only make it this way D . . . E . . . give [feels book] . . . C . . . Murd C . . . D C . . . yes . . . D C . . . E . . . yes . . . it is i M U R D E C [strikes out E and writes] i [after the C]. I can't quite catch it . . . yes . . . E . . . yes . . . i C . . . [scrawl] N . . . h . . . H . . . I just heard it now, wait . . . M rdoch . . . yes . . . she says yes . . . I am A. W. M U doch . . . D O C H . . . [I call to H. that time is up.] I am not going yet sir, no sir, I don't care about Hodgson . . . no sir I am struggling to bring this Ettie . . . May . . . look sir she says she has a sister May and a brother Ed or . . . (Did she know me in life?) Oh yes she knows you but only since she came out. She used to teach. . . (Did she ever try to communicate with me?) Yes, once she succeeded in giving you this, viz., "I am with you tell Ed . . . do hear . . . tell Ed I am here,

don't worry" something to this effect . . . I will try . . . you see she has a peculiar voice and it is difficult to hear all she says [?] . . . yes . . . my name sir is Miss Murdoch . . . [Then follow confused scraps purporting to be spoken by Miss M. in which she sends message to May not to worry because she lost the brooch; she will find it again. This is not intelligible to Miss Mary M. She also wishes to know what May has done with the rest of her books. It seems that some of the books which she used at college were brought home and the remainder given to her room-mate. . . . Then G.P. writes "adieu my good friend" after which comes a sudden burst] K it . . . Kittie . . . yes Kittie W. Murdoch K— Kittie W. Murdoch . . . yes . . . adieu I am perfectly exhausted H— want to go to H [scrawl] I wish to go to Heav— . . . Heaven . . . yes . . .

[Mrs. Piper began talking while coming out of trance.] I understand you . . . oh I do Kittie . . . [feels chest. Miss M. died of typhoid pneumonia.] The lady was telling me about her sister and I will tell you. I hear the voice say tell her she must tell somebody of the name of Edward to . . . he works too hard . . . Will you let go my brain . . . he'll come out all right, can't hear any more [Relapses into unconsciousness. Considerable difficulty in getting her awake.]

The name Kittie is correct. Miss M. was christened Kate, was afterwards called Kittie or Kit. For some years before her death she signed herself and was usually called Katharine, although many persons still called her Kittie. She had no middle name at all.

I telegraphed to Mr. Edward Murdoch and he came on and was present at the sitting on the following day, June 27th. [Present: R. H., W. R. N., and E. M.] Phinuit talked with him for awhile, making sundry statements about his health, none of which were strikingly correct. He also asked him point blank where he lived. The latter part of this talk was interrupted by remarks apparently addressed to a spirit, such as "All right, dear, you may, be a good girl," and "Don't be afraid." The writing now begins, at first with difficulty. The entire sitting is much confused and I give only an abstract.

My dearest Eddie [This name has not been applied to E. M. since his childhood] where is my brother Ned Murdoch [?] . . . where am I . . . Oh Ned tell me where you are dear and where is Bessie . . . and tell me are all together . . . Oh do help me do . . . I do not remember all my life but I see hear think speak and everything . . . I wish to know about mother Ned, I wish to tell you that she is long [ing] to be with [me] . . . where is my Picture and what is the matter with my scarf I made . . . where is it now and where are the brushes who has them . . . [There is no significance in the allusion to picture and brushes. For the scarf see below.]

(M. : Have you heard my voice since you left us?) I went . . . yes . . . I told you this . . . I heard you of course I did . . . Yes but don't you think I used to be a little stubborn . . . I did not mean to be did I . . . but nervous . . . give me my . . . where is

Baily [As I had had alleged communications from an aunt of this name I said, "That is for me. Never mind about it."] Thank you I just heard it [It appears that Miss M.'s trained nurse was named Miss Bailey, and her room-mate says that she often called her Bailey. The words "Give me my" . . . may be a vagrant reminiscence of the same sort.] Where is May, Ned? does her head trouble her any . . . what is the matter with her hair she looks different dear [Miss Mary M. has made no change in the mode of wearing her hair.] Oh dear this is all new to me and I wish I could tell you all I remember and feel . . . will you send your thoughts to me dear Ned and let me recall the school . . . I wish to know who is in my place dear (M. : I don't know.) Why dear don't you remember the scarf . . . it is the last thing I ever made dear. [About a half hour after this question was written, as nearly as can be ascertained, Miss M.'s mother, while going over her things, came across a pink silk scarf which had been sent her a few months before her death, and noticed how neatly it had been hemmed. It was not the last thing she did however.]

Ned, do you remember this . . . this, Ned [touching E. M.'s watch-pocket in which was her watch] do you dear (Yes.) Thank you dear, don't tell these people what I am saying, I never did like to . . . I must be [used to be?] a little reserved once in a while, dear.

[The writer then mentioned "Kittie Marston or the young girl to whom I gave the flowers"—no such person is known—and asked after John—the name is not very distinct—whether he were well. Her sister was engaged to a man of this name. When asked whether she had tried to communicate through Baker she said] yes but I could not speak distinctly dear although I tried my best [When the inquirer made it clear that it was of *writing* we asked she said] I tried, yes I do, I took his hand and wrote [Here follow scrawls such as Baker's hand made.] (How many times?) Not many but I impressed my thoughts on his hand and it was truly I your sister K . . . I wrote this [something that looks like S E V E T; is probably meant for the next word] S I S T E R but my dear little brother hardly knows or realises where I am and how clearly I can see him and Ned I see all that you do and all you say or think; when you sleep I am with you, when you walk I am with you, and now that you are going away down to the water I shall be there with you. [M. notes that such terms as "dear little brother," were occasionally but seldom used by his sister. The "down to the water" may have reference to the fact that he returned home that night with me by boat, although he had not then decided to do so, but the remark is too vague to be of value.]

(M. : Did you give us a message containing the word Ted?) Yes . . . this is the very first thing I ever said, T E D, but the rest was not my own thought dear [We read it: "The home folks have forgotten Ted, before I was dead, how wrong it is to grieve."] Oh the words are put backwards (Is any of it right?) Yes . . . Yes I said it was wrong to grieve only the folks at home do, and you got it backwards. [We then went over the message phrase by phrase, and all was accepted except the "before I was dead," of which, when asked whether she said it, the writer said] No not at all. [Then followed a good many confused allusions, among them one to a hat she did not like, which is not intelligible. M. asked her whether she

remembered a certain concert to which she had gone with him and who went with them. This was answered by a series of confused phrases, the gist of which was that she had written it already on back sheets. It was not there. She also alluded to the names Hayes and Adams. Miss M. was acquainted with persons bearing these names, but not very well. She also said] How long is it since I came here . . . I mean when I passed out [at] [here comes the name of the college town in which she died. This name had been mentioned during the sitting, but no reference had been made to her having died there. In the conclusion she asked] Where is Abbott (Miss?) Yes tell me Ned (What is her first name?) Kittio [illegible] May . . . Oh do May . . . oh May will you ever understand . . . Jessie . . . I send love [?] to Jessie Abbott. [M. remarks that a cousin named Bessie Abbott (*cf.* the occurrence of the name Bessie at the beginning of the sitting), was staying at his house at the time of Miss M.'s death, but at this time had left.]

The main points of evidence in favour of this communicator's identity with Miss Murdoch are:—(1) Phinuit's point blank allusion to the lady who had been a teacher and was going over things that happened at college. (2) The giving of the names Ed, or Ned, May and Alice—Miss M. had no other living brothers or sisters than these; (3) The giving of her own name as Kittie Murdoch. (4) The allusion to the scarf. (5) The reference to the watch. (6) The reference to Bessie Abbott. (7) The allusion to her having died at the college town.

Against the theory of identity are:—(1) The insertion of the initial W. in her name. (2) The description of the little sister as having died of throat trouble. (3) The statement that "Elver" is her cousin. (4) The message about the losing of the brooch. (5) The references to Kittie Marston, and to the hat she did not like. (6) Her inability to name the person who went with them to the concert. (7) The Morse incident.

As ambiguous:—(1) The claim that she knew me only since she came out, which is true but might obviously have been suggested by my question. (2) The claim that she tried to use Baker's hand and her asserted recognition of the message which it wrote. (3) The query about the rest of her books. (4) The message to Edward about working too hard, which is true but commonplace. (5) The allusions to the picture and brushes. (6) The reference to her own stubbornness. (7) The mention of Bailey. (8) The query about her sister's hair. (9) The references to Hayes and Adams.

The Morse incident calls for special mention. It is the one item in my series of sittings which, although represented as the remark of a spirit seen and known to be a spirit by the communicator, is indubitably of telepathic origin. I have already told how the name Morse came to be associated in my mind with Miss Murdoch. There is no

conceivable reason why Miss M. or any other spirit should be constantly alluding to a man whom Miss M. had never seen but once or twice, and who had died some years before she did. If, however, we suppose that Mrs. Piper's nervous mechanism merely catches and reflects the sitter's own subconscious memories and associations, the incident is readily explicable. It is perhaps worthy of note that William Morton told me several things which I think he must have got in this way, telepathically from my mind, although they were presented to him as the whisperings of distant voices.

At a later sitting, November 7th, 1894 [Present: Miss Edmunds] a few messages were given from the alleged Miss Murdoch, but nothing was said that was either clearly true or false. For further developments in Mr. Baker's case, see below.

CASE IV.

William Baker.

After getting these statements from the alleged Miss Murdoch, Mr. Baker was persuaded to allow some more experiments to be made with his automatic writing. He came to Boston and had a few sittings with Mrs. Piper, or rather was present at a few. George Pelham and Phinuit gave him the fullest explanation of his past experiences and minute directions for future experimentation.

He was, they said, a very fair medium but "mixed." He had "light" in the thumb, forefinger and middle finger of his right hand, and with practice could be made a very good instrument of communication. George Pelham especially was anxious to get a chance to experiment with him. He was told that he could improve his "light" by getting into better health and by abstaining from animal food; when he wished to try writing he "should keep perfectly calm, sit in as quiet a place as convenient" and call mentally for G. P. His past failures were due to the fact that his own mind interfered, so that it was almost impossible for the spirit to get his thoughts expressed independently.

We then endeavoured, with Mr. Baker's aid, to get some objective verification of these statements. Mr. Baker tried automatic writing both in Mrs. Piper's presence and at a distance from her. The writing produced when at a sitting seemed, Mr. B. said, different from that which he experienced at other times; the movements were more abrupt and were accompanied by tingling sensations which he did not feel at other times. But such sensations cannot be regarded as of any evidential value, nor did he so regard them. Our frequent attempts to get messages given to the alleged writer by Mr. Baker's hand

written by Mrs. Piper's hand or *vice versa* proved absolute failures, save in one rather questionable case. Dr. Hodgson asked George Pelham, referring to Mr. B., "Did he make any definite request of you?" The reply was, "Yes, in this line. Go to this medium soon. All I think of him." The last clause, as well as the tenor, seems to show that G. P. has misunderstood the question and supposed it to be, "Did you make any request of him"; the last clause then means, 'This is all I asked, I think, of him.' This happens to be right, as the first thing written by Mr. Baker's hand on that occasion was, "Go to Boston soon." This single success in the midst of so large a number of failures does not amount to much; it might well have been chance. Moreover it is not merely a matter of failures. G. P. claimed that he had often written by Mr. Baker's hand, said that he had heard Mr. Baker say this and that, and that he had himself said so and so. None of this was true. It might be alleged that G. P. had no means of knowing how much of what he said was understood, and he himself once stated that he thought he understood what was said to him better than what he said was understood. But in no case did he repeat what was said to him or anything like it. Whence then came the remarks which he claimed that he had heard Mr. Baker and myself and others make? Furthermore, he never succeeded in telling what Mr. Baker was doing at a given time or who was with him when he, G. P., attempted communicating, yet he always professed to see it all clearly. What then did he see, supposing him to be a spirit? These are questions which the spiritistic theory, as propounded by the alleged communicators, does not answer.

CASES V. AND VI.

Mrs. Martin and John James Burton.

Emily Stevenson, a great aunt of mine, married Robert Martin and lived in a town about thirty miles from my home. It was rather an out of the way place and in course of time we lost sight of her. I never saw her myself until the year 1889 when I went to call upon her, introduced myself and was received with open arms. I saw her only once or twice after that; last in 1891. She died two years later.

[My connection with her being so slight I was much surprised when Phinuit said to me, June 20th, 1894, [present R. H. and W. R. N.,] do you know who's Mrs. [?] Miss [?] Martin? (I do.) She's an aunt of yours and wants to speak to you. (I'd like to hear from her.) You were smaller when she passed out. [She had been dead about fifteen months. The writing then begins and it would seem that a large part of it is supposed to emanate from Mrs. Martin, but the whole page is much confused and apparently more persons than one are talking.]

[The writing contains allusions to] My will [which I have been told caused dissatisfaction in the family.]

"Ellen," "Emma," and "Eliza," [These are the names of three of Mrs. Martin's nieces, two of whom, Ellen and Elizabeth, known as Lizzie, lived together alone near her, were very intimate with her and were her chief legatees.

At the sitting of June 25th there occurs a curious interruption. G. P. is talking of confusion, etc., when the word "James" is written several times. The left hand of the medium becomes much convulsed and grasps the right hand. The right hand forces it away several times, and finally, being almost reached by it, writes] Keep it out of my reach, keep it quiet. [R. H. grasps and holds it by main force. After some confused and unrecognised messages comes] Here is a relative of mine, sir, in the person of Wm. Newbold. [Confusion] what has become of Burton? Let him and . . . both him and her free their minds. . . . my sister and aunt M— will tell you about your uncle also my son [?] . . . what and where is Burton . . . I am with . . . we are together and . . . [A certain John James Burton, whose wife was a relative of Mrs. Martin's, had died a short time before. He knew Mrs. Martin very well. I was scarcely acquainted with him, having seen him not more than two or three times in my life, and was much surprised to have the name mentioned in this connection. I said] (Is Mr. Burton there?) No, she was calling to Burton and I through mistake wrote it down as I heard her say it . . . come here B— and speak to Will . . . B— come here . . . B— and speak to Will . . .

Cass . . . Carson, the Dr. took away my medicine much against my will yet it is all right now. [This appears to be an interruption; cf. Case VII. (p. 35)].

Who is Helen . . . [Mrs. Burton's name is Helen] she is always interrupting and this I hear . . . too bad, it is not legible yet . . . I will make it clear soon . . . [illegible] . . . D N . . . D N . . . D N . . . D N . . . [I am standing; hand gently presses me down] will you sit? I see your etherial in full sir and I am not specially anxious to see you and I am more so to get these messages straight my friend. [The writer then asked R. H. to go out; then to me] Burton wishes me to say to you that if you are New . . . you will recognise me by this. [Here follows a rude diagram which looks somewhat like an open book. It is drawn several times. I said—Why can't he tell us in words?] He will do . . . No . . . [It is drawn again.] I am not good at this business, yet this is as he gives it me D i p l o m a. [Then follows confusion in which occur the words] L A W in or der of the L I B erty . . . Law and (Was he a lawyer?) No, this refers to somebody that is with you. [The drawing is again given. R. H. guesses that it is a book.] yes and it is . . . indeed yes and now he wishes to know where it is. It was a distinct part of himself in a way and he says you should know this . . . what about Andrew . . . (N. : I don't understand at all.) Oh my friend this is a friend of his Mr. Andrews or Andrew by name . . . did you hear your aunt who calls herself Mrs. Martin is connected with Burton and also you . . . she is an [?] . . . her [?] husband . . . father . . . no, your

grandpa . . . was her father—no his . . . wait I dislike any mistakes in this . . . her brother . . . is . . .

[Seeing that the communicator was getting confused I asked him whether he remembered a call I paid at his house. He said that he did, and that, after I left, he had sent me a box of fruit. I cannot learn that he did anything of the kind. He sent his love to his wife and said] she must know I live here and if she changes her place again she will hear from me. [Mrs. Burton had been thinking of moving.] (Tell me of your death.) I came here quite suddenly and on account of my head trouble. [Mr. Burton was apparently in his usual health one morning, but after breakfast complained of feeling a little ill; he lay down on a sofa, soon became unconscious and died later in the day. The cause of death was apoplexy.] (Did you suffer at all?) No conscious pain at last . . . none although she thought I did. [Mrs. Burton was heard to ask the doctor whether Mr. Burton had suffered any pain.]

[The next day Phinuit called to me and said:] I want you Billie. [Speaking in the name of a spirit] Tell gentleman in the body that, in my hazy communication to him in expressing myself I made a mistake. It was to Burton's friend Mr. Dycon or Deacon, or—something like that he says,—that the fruit was sent and he mistook you for him. Ask Helen [Phinuit pronounces the word Heleén]. At first he recalled the circumstances and connected them with you. [When Phinuit said this I had no notion whom he meant. One of Mr. Burton's sons told me that his father was an intimate friend of Mr. Anthony Deacon—*cf.* the Andrew above. I knew Mr. Deacon myself very well but did not know that he knew Mr. Burton. Mr. Deacon died after a long and painful illness only three days after Mr. Burton. The sending him a basket of fruit seems therefore plausible, but no one can be found who knows anything about it. Mrs. Deacon thought the words "Law and Order of the liberty" might possibly be connected with the fact that Mr. Deacon and Mr. Burton were both officers in the late Civil War, members of the Loyal Legion and spent much of their time together fighting their old battles over. The "diploma" may be connected with "commission."]

The evidence for the identity of these two communicators taken singly is not strong. Mrs. Martin's name and relation to me is definitely given; the other statements ascribed to her are too ambiguous to be worth much. In Mr. Burton's case the name and cause of death are correctly given. The allusion to Helen is significant, but it is not definitely stated that she is his wife. To my mind, however, the collocation of these two was one of the most striking facts developed in the course of my sittings. My acquaintance with both was so slight that I could scarcely name anyone mention of whom would have surprised me more; the manner in which Mr. Burton is introduced, his name being first mentioned in what purports to be Mrs. Martin's words as she calls to him to come and speak to me is very curious. The statement that "your aunt, Mrs. Martin, is connected with Burton" is also noteworthy.

CASE VII.

"Aunt Sally."

[At the beginning of the sitting of June 24th, (present R. H. and W. R. N.) I am informed that "Sally" wishes to be remembered to your mother [sends my mother a message with reference to something which she says my mother once did. My mother has no recollection of this. I had an aunt Sally who died early in 1875.]

This is a difficult matter to explain yet . . . yes . . . there were two marriages in this case of mother and aunt grandma also. Consequently it seems they will never cease to taunt me until I explain matters to you. Sally is the lady, elderly lady, who says she gave Marie the message . . . Maria . . . [I ask if it is Maria] not quite Martie . . . yes . . . Martha . . . yes . . . she says Martha [My mother's name is Martha] do you see these people. (R. H. : No George, not we.) Why is it possible when they are so near, yet I know you do not, yet it seems as though you must . . . Yes . . . she says this distinctly and is still coming nearer and now requests you to speak. (Can she hear me?) I can and I am the telephone [I ask for something to prove identity] Yes yes . . . Sally . . . [To me, who am still talking] don't you speak please unless you speak to H. . . . your mother had a message from her . . . ask me the question you asked before . . . now all is well, your aunt—she is your aunt—at first she could not make you out and it was as difficult for her as for you at first yet she knows you now very well [I was ten years old when she died] and the instant I understood your question she answered. [I send affectionate messages] Yes and she says Martha knows she did, ask her if she remembers what she says . . . [Here come confused statements in which the word Morse frequently occurs; (cf. p. 24) I ask whether my aunt says this word and am told] No. [Finally the hand stops writing and motions to me. After several changes of position, which seem unsatisfactory to G. P., I get on my feet and the hand feels around the lower edge of my waistcoat, pausing to write] excuse this uncanny procedure [finally presses firmly on median line about the lowest button of my waistcoat and writes] ask not [her?] if she remembers this, Will. . . . [My aunt died of the effects of an operation for the removal of an ovarian cyst. When this was written I looked over to Dr. Hodgson and said, "She refers to the cause of her death; she died of a laparotomy." The hand at once wrote] Yes, yes yes yes *yes sir*.

[After an interruption.] There is or was two marriages in the elderly lady's family ["Sally" was grey when she died] which they do not seem to be able to unravel just now (I understand, Mr. Pelham.) O.K. . . . just say this for their satisfaction so they may be quite sure you understand them and that you are you. [I explain that my paternal grandfather was twice married, that his second wife had a younger sister whom my father married many years after his father's death; she is my mother. The elder sister is still living, and is therefore both my aunt and my step-grandmother.] Yes, yes, yes, O.K. now you know what the aunt grandma meant together aunt and grandma if you recall were given at the same time. [This is a very interesting incident. My grandfather died more than forty years ago, only

eleven months after his second marriage. We only recognise the tie of blood, and many persons do not know that my aunt is also his widow. The supposed speaker was another sister.]

[At the sitting of June 26th, (present R. H. and W. R. N.) occurred the interruption which I have already given under Case V., p. 32] Cass . . . Carson, the Dr. took away my medicine much against my will yet it is all right now. [I supposed at first that this applied to Mr. Burton, but on inquiry could learn nothing of any doctor of the name in connection with him. Then it occurred to me that an old doctor named *Corson* had lived not far from our home when I was a child; I wondered whether he could have had anything to do with my aunt Sally. I knew that she had lived near Philadelphia and had died at a hospital in New York. Upon inquiry I learned that she spent two weeks at our house near New York before going to the hospital and was attended by this Dr. Corson. I must have known this at the time but have totally forgotten it. The incident of the medicine cannot now be verified. A little later on in the same sitting G. P. writes.] Sallie wishes to be remembered . . . she thinks everything of you. [I was a great favourite of hers I believe. I asked whether she remembered her hymnal which I now have, and the writer said she was] delighted to hear this. [I then asked whether she could tell me what she did just before her death. I have heard that she sang the hymn "Nearer, My God, to Thee" and hoped to elicit some reference to it.] Gave her this for you (Gave whom?) *Mattie* . . . (Is that *Mattie*?) No F . . . wait sir . . . [Apparently to the spirit.] I can't give it unless you make it plainly . . . *attie* . . . *Pattie* I called her. [This is true. My mother is always known as *Pattie*. I always understood that the hymnal was sent me as a parting gift by my aunt, but I cannot learn whether she gave it to my mother or not.]

CASE VIII.

Joseph White.

[I have lost but one relative within recent years who was closely bound to me by ties of affection. I shall call him *Joseph White*, and I had fully expected upon any theory, telepathic or otherwise, to get some messages from him. I was consequently very much surprised to hear almost nothing from him. I did not at first, however, make any attempt to open communication with him. On June 23rd, 1894, G. P. says] who is this uncle? [G. P. proceeds to say quite clearly, although with many repetitions and interruptions, that this uncle calls me *Willie* and his *Will*; that I am his nephew, and desires me to give his daughter *Edith* his love and to say that *Tom* her husband will come out all right; that the bronchial trouble will disappear. That *Tom* had feared his lungs were affected, but in fact it was only his throat and bronchial tubes. All these names and allusions are strictly pertinent, save that *Tom* had never supposed that his lungs were affected by the cold from which he was then suffering.

At the sittings of June 20th, 1895, and June 26th, 1895, I inquired after my uncle, and told G. P. that I desired to communicate with him, but without success. G. P. claimed to have seen my uncle but informed me]

as regards actual communication from him I am afraid you will not get much conversation from him as he has been extremely happy here and his thoughts have been thus far undisturbed, yet, of course, there will always remain a fondness for you, *i. e.* in his heart.

CASE IX.

W. Stainton Moses.

[At the sitting of June 19th, 1895, (Present : W. R. N.) George Pelham was telling me how the future state of the soul is affected by its earthly life] —It is only the body that sins and not the soul (Does the soul carry with it into its new life all its passions and animal appetites?) Oh no indeed, not at all. Why my good friend and scholar you would have this world of ours a decidedly material one if it were so. (Do you know of Stainton Moses?) No, not very much. Why? (Did you ever know of him or know what he did?) I only have an idea from having met him here. (Can you tell me what he said?) No, only that he was W. Stainton Moses. I found him for "E." and Hodgson. (Did you tell Hodgson this?) I do not think so. (Did he say anything about his mediumship?) No. (His writings claimed that the soul carried with it all its passions and appetites and was very slowly purified of them.) It is all untrue. (And that the souls of the bad hover over the earth goading sinners on to their own destruction.) *Not so.* Not at [all] so. I claim to understand this and it is emphatically *not so.* Sinners are sinners only in one life.

[The next day, June 20th, I said] (Can you bring Stainton Moses here?) I will do my best. (Is he far advanced?) Oh no, I should say *not.* He will have to think for awhile *yet.* (What do you mean?) Well, have you forgotten all I told you before? (You mean about progression by repentance?) Certainly I do. (Wasn't he good?) Yes, but not perfect by any means. (Was he a true medium?) *True, yes,* very true. (Had he light?) Yes. (Yet not all true?) Yes, but his light was very true, yet he made a great many mistakes and deceived himself. [At the close of the sitting I said:] (I want to see Stainton Moses.) Well, if I do not bring him do not be disappointed, because I will if I can find him.

[On the 21st, I asked again about Stainton Moses.] I cannot bring Stainton Moses because he is not in my surroundings yet. (Can you explain this further?) Well, of course I cannot bring every known person here just when you wish. (How about your surroundings?) This is a large sphere. I have the doctor after him now [To some forgotten question] No, wait patiently and I will wake him up when he arrives. (Is he asleep?) Oh, B—— you are stupid I fear at times, your mind is like a lightening . . . machine . . . I do not mean wake him up in a material sense. (Nor did I.) Well then, old man, don't be wasting light. (I'm not wasting light but I'm bound to find out what you mean.) Well, this is what I wish also. (Stainton Moses has been nearly three years in the spirit—a long time.) *Yes.* (Do you mean to say that he is not yet free from confusion?) No. (Do you mean that he will be confused in getting at the medium?) Certainly, a little, this is why I use the expression, wake him up.

[On the 22nd, Phinuit said], do you know Billie, George is talking to such a funny looking man ; he has a long double coat with a large collar and cape,—a long beard, large eyes with drooping lids, [fairly shouts with laughter]. George is shaking his fingers at me. He sent me after that gentleman. I found him in another part of our world. (Far away ?) It would be a long way to you Billie but not so far to me. George had difficulty in having him come but they had a long talk and George made it all right with him. He didn't understand what we wished of him. (Who is he ?) I don't know his name. George called me and sent me after him—you understand Billie—said, "You go and find him for me, doctor." (How did you know whom he wanted ?) He said, "I want you to find a friend of mine who used to be a medium in the body," used the light, you know. Oh he has a great deal of light, more than anybody. (Do spirits have light too ?) What d'you mean Billie ? Spirits are all light. (I mean does a person who has light in the body have in the spirit also more light than others ?) Yes indeed. (Tell me how George made you know whom he wanted.) He described him. (And his influence ?) Of course. (You know it's very hard for us to believe in spirits at all. Do you remember your life on earth, doctor ?) Oh yes, but I've been here a very long time. (Did you believe in spirits while you were on earth ?) [Phinuit gives a short derisive laugh.] Not much. Not I. (Then you should sympathise with us.) Oh, I can't put myself in your place. [The above description of S. M. answers to the notion I had of him at the time, derived from portraits.]

[G. P. writing :] Here is Stainton Moses, do you wish to see him ? (Yes.) Well, now let me give you a bit of advice. Speak slowly and distinctly, making sure that you articulate properly, or in other words well. (I know my articulation is very bad.) Yes, then he will answer to me all questions distinctly. You see he is talking to me now. Fire away. (Tell him I have read with interest his book, *Spirit Teachings*, but find in it statements apparently inconsistent with what you say and I would like to know his explanation of the fact.) Believe you in me and my teachings ? (I was much impressed with them, Mr. Moses, especially as your statements and Mr. Pelham's agree in the main. But how about the inconsistencies ?) Contradict the genuine statements made by our friend Pelham, whom I am delighted to meet. (I did not say contradict, although it appears so. Can you explain them ?) I do not understand your question. (Will you explain these seeming contradictions ?) What are they, please sir ? (You taught that evil spirits tempt sinners to their own destruction.) I have found out differently since I came over here. This particular statement given me by my friends as their medium when I was in the body is *not true*. (The second is that the soul carries its passions and appetites with it.) Material passions. U N *true*. It is not so. I have found out the difference. (Thank you) Not at all. (Would you like to make any other corrections in your book ?) There are a few. One is I believe that our thoughts were practically the same here as in the body, *i. e.*, that we had every desire after reaching this life as when it . . . but I find that we leave all such behind, in other words it dies with the body. You will understand I do not mean thoughts, but only evil [thoughts] (Are you willing to give me as tests the names of your "guides.") Guides, well I object to the expression. (Indeed.) I do

now, yet I did not before. (These names have never been made public since your death. If you are willing to give them I would be glad to know.) I will give you one [I hand a new pencil. Hand turns and twists it some moments before writing] Pencil—well, well—oh I see. (Who was “Rector?”) Dr. — (I repeat, Dr. —?) Yes sir. Rector applied for convenience instead of Dr. — (You mean the true name of the spirit Rector was Dr. —?) I do mean just this, but I had no authority to speak of Rector as Dr. — (But there was another spirit known as Doctor.) I was obliged to distinguish one from the other according to their wish. (Who was the spirit “Doctor?”) —. (Indeed. No one will be told of this save Mr. Myers and Dr. Hodgson.) Thanks. (May I tell the latter?) Certainly sir, if he is reliable. (He is.) I’ll ask Mr. Pelham . . . Certainly sir. — was a very good man sir and was always with me. Have you these? Did you hear me? (Yes. Now are you willing to tell me the name of “Imperator” also?) Well, I have never divulged this name to anyone. I’ll think it over and let you know. (These names have never been made public and they will afford excellent proof of your identity.) I understand sir. . . . I know Albert . . . I do—never mind . . . this had to do with . . . understand . . . (How about the physical phenomena produced through you?) It was not done by any effort of mine or on my part. (Could such be produced through this medium?) Oh I do not know sir. Generally the intelligences have their own phases sir and work accordingly. (In your book, Mr. Moses, you made certain statements about some historical personages, such as Abraham, Moses, the Prophets, and Jesus Christ. Do you wish to modify any of these?) *Not at all* (All are true?) To the letter sir (You recollect nothing else in your book that you would desire to change?) *Not at all sir* (Have you any messages to send to friends?) I have had a wonderful experience here sir and I am extremely happy and I consider myself extremely fortunate sir to have been brought here by this gentleman . . . Spear [I spell it, s p e a r. Hand writes] e (Oh you mean s p e e r?) Certainly . . . letter . . . my thoughts are not quite clear, sir, yet . . . Spear . . . I have a friend . . . recollection of speer [Writing is growing dreamy. I say] (You mean Charlton T. Speer, the musician?) [Excitement and pounding.] Yes, yes, why certainly, give my love to my affectionate brother worker in the body, my dearest love, *love* . . . yes sir, I do wish to give it very much this reaches every chord in my soul sir. (Do you remember Mr. F. W. H. Myers?) Oh I think I do sir. Are you he? (No. I am a stranger to you. He is editing and publishing some of your MSS.) *Good, good, good.* . . . I think I do . . . thanks sir for giving me this information regarding my book (I wished those names as proof of your identity.) [Question misunderstood] Certainly I am Stanton [only one stroke for n] Moses. (Do you remember Richard Hodgson?) No, I think not sir, are you he? (No. But he was a member of the Council of the Society for Psychical Research while you were.) [At or about the word “Society” the hand displayed great excitement.] Of course I remember him. (He went to America.) Yes, I remember he went there some time ago. (You are now in America, near Boston.) Well, I longed to go to America and this will open up a great field

to me. (Good-bye. Will you come again and speak to Dr. Hodgson?) I am of course a little strange here, yet nothing would give me greater pleasure than to prove to the world my identity I am sure. I was a great sufferer physically and I could not do altogether as I wished in consequence, yet I am strong and well here and as I can see through this light clearly I should be pleased to help you all. (You will come again?) Yes sir. (And then explain the reason for your mistakes?) Certainly sir. Oh I am so pleased to return. [Further writing, on personal matters, by G. P. At the close of the sitting Phinuit returns. Speaks with difficulty] "George has been teaching that man a lesson, showing him how to use the light."

[Sitting of June 24th, 1895. Present: R. H. and W. R. N. Mrs. Piper goes into trance easily, without the usual struggles. R. H. remarks that this is a new control. Her hands move aimlessly about, touching her eyebrows and temples with the finger tips and feeling Hodgson's face. Gasps, peculiar rattling in her throat, her face is very much contorted. Ineffectual attempts to speak, finally gasps out] Moses [Hodgson encourages communicator. Head nods] (H.: I'm Hodgson) [Head nods, she groans and grunts, hands move about. Right hand begins to write. R. H. asking questions] I am W. Stainton Moses I am he in reality. Oh my dear sir I am so very delighted to find this bright path to earth. (I'm very glad indeed.) I am here in every organ of a human body. (Yes, you're occupying the medium's body.) I am a medium also. (Yes, we know.) I did see my spirits plainly. How strange you look. Are you still in the life on earth. (Yes.) You must necessarily be I am sure. (Yes.) Do you remember one of our friends and fellow workers Dr. Wallace? (You mean Alfred R. Wallace?) Certainly, very well, my friend Wallace. (In the body?) Yes, give him my love. (I will certainly.) Also Myers (Yes indeed) whom I remember well (Yes I certainly will) all right. I had a spirit once named Wallace. You never knew — did you? (No I didn't.) He was one of my guides when on earth. (What name did you give him, i.e., —) Rector, and not Dr. as I had explained to some friend of yours. Rector was — (—?) Yes distinctly, he was Rector. (Who was Doctor?) Not Wallace, but a Dr. — whom I used to know at college. [R. H. pronounces and spells the name over.] Yes sir. It is very singular how the names of my former friends and guides run in my mind . . . run through my mind just now, at this moment. (Mr. Moses, I wish to tell you something that will interest you. Mr. F. W. H. Myers, whom you knew) quite (has been publishing a full account of your life experiences in the *Proceedings* of the Society for Psychological Research.) Viz., S.P.R. . . . good . . . oh glad I am to meet you here . . . I will help you in your work. (We shall be glad indeed. I wish to ask you one important question) let me clear up all my thoughts and I will help you. (Do you wish to write your own thoughts or answer questions?) I would like to become acquainted with these conditions. (Good.) Myers what about Myers. (Myers has been publishing a record of your experiences and has referred to Rector, Doctor and Emperor, but explains that the persons whom these names represented are not to be mentioned.) Private. (But I understand that Myers knows.) Yes, he *must*. (We are not going to

publish them.) Do not. (But you understand if we tell Myers who Imperator is, it will be strong test of your identity.) Yes . . . Rector . . . I know . . . the name was taken expressly for distinction, i.e., to distinguish one from the other, and Dr. was Dr. — whom I knew very well at college. (Could you tell us, if it will be kept private, who Imperator was?) I should hope so. Question, I did not catch sir. (Can you tell us who Imperator was?) Certainly, a young lady friend of mine. (Are you sure? I mean the famous communicator from the spirit world whom you spoke of as Imperator.) Oh no, but she in my spirit teachings is mentioned. (I mean the Imperator also mentioned in your *Spirit Teachings*.) Yes. Yes. Must I tell you who it was. (Let me explain. I wish you to use your own judgment. Mr. Myers knows) he does (and we do not know. Nobody in the United States knows. If you tell us and we send it to Myers privately, it will be a very good test of your identity, being information to him which nobody possesses on this side of the water.) —. (—?) Certainly. (—?) Yes. *Now I know wherein I speak.* I never during my illness when being helped by him told or divulged his name to *anyone* and I only left it written (—) in my MSS. (Very good, Mr. Moses. This will be a splendid test) in or among my private papers. (Good. That's first rate.) No more sir. (You are getting exhausted, aren't you?) [Here the left hand becomes convulsed and rubs Mrs. Piper's right cheek in a manner characteristic of Phinuit.] I wish to change my position sir if you please. (Yes, do so.) Help me to remain here I wish very much to continue my remembrances. (Yes, we shall be very glad too.) I remember Mrs. Speer very well. [While the last sentence was being written Phinuit remarks to Newbold:] "That gentleman's a nice fellow, he's a clergyman." Give my love to all on earth. . . . yes . . . who can deny my existence . . . oh my existence I say, who can deny that I exist? (We do not.) Stainton Moses. (Can you write your full name?) What Stainton . . . W. . . . Moses always Stainton Moses and always will be. . . .

(Now we wish your explanation of certain things. What was the origin of this mistake about evil spirits taking possession of men and leading them on to do wrong?) Experience here has taught me the difference. This was more my own theory. (You mean that when you were in the body you misunderstood the communications?) Yes often, especially when I was not feeling well. (The thoughts of the communicating spirit got confused with yours?) I mean of course to go back to the body i.e. to go back to my earthly experience . . . Yes and not so much that altogether as that I misunderstood. (You misunderstood yourself, so to speak.) Certainly, materially. (You had your own theory and misinterpreted the communicator's meaning?) Yes exactly, as I thought this very strongly I felt sure of having been told this. (Were all those physical phenomena that you got due to spirits?) No not all. They were due to material causes, etc. as well. (Do you mean persons in the body produced them?) Not at all, I mean to say that from the energy which they took from my own body, medium power etc. they were moved. (Were they moved by the action of spirits?) Action of spirits? Oh yes. (I'll state my impression. Certain spirits used the "electrical" in connection with your body to produce the physical

movements.) Yes, this is what they did. Objects etc. raps . . . (If you have anything special to say to us we shall be glad to hear it, but if not, we have something especial which we wish you to do for us.) Well [writing begins to look dreamy] I must say that I will have many things special to say to you, but I am forced to admit that this is all new to me now and it seems very strange indeed . . . I am (we shall be grateful to you for help in proving to the world the truth of spirit communications.) Yes, glad I will be to be able. (Can't read that word) enabled to communicate, giving tests etc. in my own language. (Do you think you could translate some Greek into English?) Do what? Greek . . . why I used to be as familiar with Greek as English. (Better wait for next time.) Well, yes. (Think up your Greek and the next time we will give you some to translate. Everybody knows that the medium does not know Greek and if you could translate some for us it would be good proof) what could a medium have to with me and my Greek. [R. H. explains further that proofs must be got that the medium's manifestations are not fraudulent.] Well I suppose they said the same of me. (Mr. Moses, aren't the conditions getting strange? Don't you think you had better go now and come to us another time?) Yes I do [scrawls] auf wiedersehen (auf wiedersehen.)

In this case we have the difficulties which attach to the spiritistic theory brought out in the highest relief. The general tenor of the communications, the allusion to Mr. Speer, the reception of the names of Myers and Hodgson, have an air of verisimilitude. The communicator then gives us, with the most solemn asseveration of their accuracy and with apparent consciousness of the importance of his statements to a cause which he had in life much at heart, three names which the real Mr. Moses must have known and which of all possible things would seem to be the hardest for the spirit to forget—the names of the spirit friends who, as he claims, opened his eyes while still on earth to the realities of the eternal life. And not one of those names is true or has the least semblance of truth! Furthermore, of all the points touched upon during the sitting this was the only one that was unknown to both the sitters—another item in favour of the telepathic theory. To my mind this failure on the part of the alleged Moses is an obstacle to the acceptance of the spiritistic theory which has not as yet been set aside and which must be satisfactorily explained before that theory can be regarded as meeting the requirements of the case.

CASES X. AND XI.

Aaron James and Albert Bonney.

[Sitting of January 27th, 1896. Present: W. R. N. and Mrs. Edith Waldron, the latter for the first time and under assumed name.]

At the outset of the sitting Phinuit made the most remarkably correct diagnosis that I have ever seen him make. He told Mrs. W. that she suffered much from flatulent dyspepsia, that she got dizzy sometimes and sometimes fell or felt like falling, all of which was due to dyspepsia. That she was subject to severe attacks of despondency. All this is literally true, and as Mrs. W. is vivacious and seems in the best of health, I do not think it could have been got from any superficial inspection. Phinuit then told her that her mother suffered from a cough, was very "positive," sometimes a little too "positive," that her father was a good man but "a little cranky from his rheumatism" which occasionally made him cross. This is also strictly true.

[G. P. begins writing. After a warm welcome to me and a few words on sundry topics I asked for Mrs. W.'s relatives, to me wholly unknown save by name, and even of their names I knew little.] Oh, I see. Yes. There first of all is a gentleman who is a relative of hers [lighter pressure in writing and smaller script.] Come nearer, sir, do not fear. You will be able to clear your thoughts. [To me.] Will you not leave your place for her? [Mrs. W. takes her place by the writing hand.] Thanks. He says Ask this little lady if she remembers U N C L E. [Then follows confusion connected with me. Then:] George. [Following statement seems due to G. P.'s misunderstanding speaker; cf. below.] Yes I am he. Fire away old man. Tell Edith if she is my sister's child. [She was the granddaughter of his wife's sister.] Charles . . . Charles . . . will you hear me . . . yes . . . yes . . . Give me one little answer . . . [We ask who this Charles is.] Charles in the body. [Apparently an uncle of Mrs. W.'s and nephew of speaker's wife.] And I wish you to speak to me and let me get a little clearer . . . Love to Charles . . . he is still with you. [Here comes more confusion in which several of my former communicators seem to be trying to slip in a word edgewise; in the course of it occur the curious expressions.] Speak to him for heaven's sake. Let him throw off his mantle. [It concludes with.] Your father . . . who in thunder is George. [Pointing to E. W.] He says your father. (Yes.) Well I thought all the time he was speaking to me . . . calling me I thought J A M E S . . . yes I am his brother . . . Barker . . . I know George Barker and I am his UNCLE J A M E S. (Is James his surname or Christian name?) [Great excitement and tearing of paper.] James is his surname. And I know NOW the whole world is thought, develops with reason. What has become of Mary Bacon? [Mrs. Waldron's father is George Barker. His mother's sister married one Aaron James who was always known to his nephews as Uncle James. Mary Bacon is not recognised.]

Don't you know Mary Bacon? Yes I wish to give my message distinctly and clearly . . . John [to me] this is yours B. . . . (Well, never mind, Mr. James.) Speak to Mary Bacon for me . . . Yes she will understand, Mrs. — Your mother he says is not quite well, is this so? (Yes.) Well, did you know I thought I would live somewhere and I am

really your uncle. If any other person my thoughts would not be clear. I was a level-headed man. [This is very significant. Mr. James was a clergyman; he became interested in spiritism and carried it to great excess, so much so that in the later years of his life he was regarded by his relatives as not quite sane.]

Do you remember me little girl. [She never saw him. But immediately after this comes a mass of confusion in which Mr. James' remarks seem to be mixed with those of the communicator who next comes to the front, and are interspersed with a few of G. P.'s words, as "cannot quite hear you," "who in thunder is he talking about." This other communicator knew Mrs. W. very well. It begins to get more coherent.] I wish to enquire for your mother . . . yes . . . also Mary. [The wife of the communicator who next appears is named Mary and is living.] Will you tell me where she is? I have asked, asked, asked . . . Mary, my Mary . . . tell me quick . . . I wish to know . . . I am living . . . I am . . . gentlemen I hope [A gentleman, I hope?] and I see my niece but she does not seem to recognise me. She loves me still and will do a great deal for you. You will see her soon. (E. W.: Is she coming to see me?) Yes. [Untrue of his wife.] Very happy, as happy as I could be without her . . . [Remark forgotten.] Well I know that well and I wish my wife Mary to know that I am alive. [We asked for some proof of identity.] Well, ask her what I asked her to do for me just before I left the body in regard to seeing Joseph. [Unrecognised.] . . . yes . . . about the insurance. [There was no insurance.] (Was there anything you were very fond of?) [This question was asked with the horse in mind.] I told you about this long ago viz. HORSE and you kept calling it house. [This was true; it occurred amongst the confusion which I have not transcribed. He had an old horse of which he was very fond. In connection with it came the sentence "Go and see Austin." (W. R. N.: Write your name.) Yes, of course I will. I am al Fred James. [This falls rather flat.] You don't understand. [Repeated attempts at writing the name Alfred] Barker—and I am James. Yes certainly. (N.: Is this your name?) Yes this is mine and I am Alfred. Ho . . . No . . . K . . . I am Alfred . . . Bacon . . . [Apparently G. P. now speaks to communicator.] You must clear up, clear up, clear up, clear up, clear up Bacon . . . Bacon . . . Bacon . . . Bacon . . . B U R N E R . . . [At some point hereabout Mrs. W. said in a low whisper, "It is Uncle Bonney." ku . . . ka . . . rner . . . sounds . . . [much excitement] give me [We produce an old glove that had belonged to alleged communicator and hand it to the writing hand. It feels it over.] Bennett . . . Alfred Bonney . . . [Alfred Bonney had been the husband of Mrs. W.'s paternal aunt; he had died about eighteen months before] my gloves quick [We give them back] hold it straight up in his face [I hold the glove in a place over the table to which the hand has been pointing.] Alfred Bonney, yes . . . and Uncle James both. Do let me speak, we are both alive and well. Edith, if you remember Uncle . . . speak to me as you used to do when you were a little girl. Where is Dick . . . [Unrecognised.] Never mind. Charles is still in the world with you, how is he? George too.

[I abbreviate the remainder of the sitting.] Do you remember N E B Old Tom . . . he is with me this number of years. [Unrecognised.] (E. W. : What did you die of?) What! I am not dead. Don't think I am. [Feels about E.'s chest and mine.] Heart trouble [Points at E.'s heart] there . . . Yes I lost my breath . . . in my bed . . . I went out . . . I remember sitting in my chair . . . the last thing I recall . . . do you remember my chair—and I passed out there. (E. W. : I do not think he did.) *I did.* I lost my breath in my chair and . . . [He died of heart disease but not in a chair; he had not been out of bed for some days.] . . . (E. W. : What did you and Papa and Uncle Charles do in the summers?) Go trout fishing. [True.] . . . I was most fond of . . . [E. W. reads this aright; I say there is no word "trout."] Yes, it is trout, too. (E. W. : When you passed out you left my father some things; what were they?) [This is a mistake. He did not leave him anything. Mrs. Bonney gave Mrs. Barker her choice of some of Mr. Bonney's things and she chose a rod for her little boy.] Gave him my tackle . . . Gave George [She misunderstands and says (That is wrong)] my whole outfit. (What did you give my brother?) My watch. (No.) I cannot seem to keep this in my mind but I keep this pocket in my coat, oh no this is not what I wished to say . . .

[While coming out Mrs. Piper says] Tell Edith's mother I am alive and well. Alfred Bonney. I want everybody to know.

The striking points in favor of the theory of identity are: (1) The statement that Mrs. Waldron's father is named George. (2) That the speaker is his uncle James. (3) The emphatic and characteristic statement ascribed to "Uncle James": "I know now the whole world is thought, develops with reason," and the allusion to his belief in spiritism while living. (4) The allusion to the horse. (5) The giving correctly the relationship of Mrs. Waldron to Albert Bonney. (6) The mention of heart trouble as the cause of Albert Bonney's death. (7) The allusions to trout fishing.

As ambiguous: (1) A statement that Edith is his sister's child, whereas she was his wife's sister's grandchild. (2) The allusion to Charles in the body. (3) The mention of the name "Barker" which had been mentioned in a former sitting and might have been guessed. (4) The allusion to Joseph. (5) The giving of the name "Alfred Bonney," which was, unfortunately, mentioned during the sitting. (6) The allusion to "Old Tom."

As untrue and *pro tanto* against the theory of identity: (1) The allusion to Mary Bacon. (2) The allusion to what Albert Bonney asked his wife to do for him before he left the body. (3) The reference to Austin. (4) The reference to "Old Tom." (5) The statement that he passed out in a chair. (6) The statement about his bequests.

CASE XII.

Mr. Parker.

On November 7th, 1894, Miss Edmunds submitted to Phinuit, at my request, certain articles belonging to a relative of mine who has been living abroad for a number of years, and whom I shall term Mrs. Parker. She has had many experiences of an apparently supernormal order, and I had hoped that the Mrs. Piper communicators might in some way manifest themselves to her. The articles were incorrectly ascribed by Phinuit, who had been informed at the outset that they had been sent by me, to Fred Morton and Miss Murdoch. On the following day a person unknown to me was having a sitting, and in the course of it the following remark was interpolated:—

Mr. Packer [?] (Is that name Packer?) No, *Parker* desires to send his love to his wife in the body.

[Mrs. Parker's husband has been dead some years. She secured a divorce from him some years before his death. I communicated these facts to her but she made no comment thereupon.]

CASE XIII.

Apparent Knowledge of Foreign Languages.

Frequently in my sittings George Pelham used French and Latin words and phrases—*e.g.*, “inter nos” (June 20th, 1895), “coup de main” (June 21st, 1895), “de die in diem” (June 22nd, 1895). This last phrase I could not decipher until the writer added “from day to day.” Then I read it and added, “I never saw that phrase before.” “Well,” the hand replied, “I never saw it, it just popped into my mind.” This is found in Webster's Dictionary in the list of foreign words and phrases.

[At the sitting of June 20th, at which I was alone, immediately after the occurrence of the words “inter nos” I said with reference to the matter in hand, (no one knows but Hodgson.) Hodgson, what the old chap, Fama . . . [I thought of the well-known passage in Virgil beginning with the word and said, jokingly,] (Hello, you've not forgotten your Virgil, George?) [The hand pounded excitedly and continued,] tempus viat—vivat G.P. [With unpardonable stupidity, I failed to look closely at this, merely catching the general sense, and went on with the sitting.]

While deciphering this sitting I noticed that this sentence made no sense as it stood; it occurred to Dr. Hodgson to look in the list of quotations in a dictionary, and there we found the original “Fama semper vivat.” At the sitting of June 22nd, at which I was again alone, I asked G. P. about this with very unsatisfactory results. G. P.

translated it, "May his fame live for ever." In spite of my constant questions, he was unable to note that "semper" should be substituted for "tempus." On June 21st [Present: W. R. N. and H. W. O.] I tried to get the motto of the Tavern Club (of Boston), and was given a medley of almost illegible scrawls in which I thought I could decipher the words, "offere," "datum," "duc," and "vir." When I asked whether this was "duc vir," the writer assented, and assented also to my translation, "Hero, go ahead," or, "Lead the way." This has no resemblance to the motto of the Tavern Club.

(Will you translate Greek for me?) Certainly Greek. (You remember it?) I ought to. [I then said the first scrap of Greek that happened to come into my head:—Πάτερ ἡμῶν ὁ ἐν τοῖς οὐρανόις.] (Did you catch it?) No, not exactly, slowly. (Πάτερ.) Pater . . . I say . . . Pae . . . Pater . . . pater . . . good (ἡμῶν) hemon . . . [illegible] he . . . hemon . . . urano is . . . and translation . . . Good . . . love [?] [illegible] Love [?] Love [?] . . . father is in . . . that is right . . . (All right but go ahead.) I cannot quite catch that B— . . . yes . . . Patience . . . well you have it B—. [Throughout, both Mr. O— and I frequently repeated the words and spelled them both in Greek and English.] Father is in . . . tois ou ou nois our . . . B—. Patience my boy . . . Father is in Heavens. (One word is left out, George.) Spell it slowly. (Greek or English?) Greek of course. [We do so, ἡμῶν.] Father is in the Heaven . . . I [do] not catch [it] . . . slowly now, speak those letters separately my boy . . . ae . . . emon. (Rough breathing, now, ἡμῶν.) Heaven . . . Yes . . . too bad old chap . . . [I read bad as "hard."] Bad I say, I'll catch it. [Hand points to O—.] Now you say it, let me see if it will reach me any better. [O— says it. Hand gesticulates and twists so as to get O—'s mouth close to outer side of hand just below the root of little finger.] My ear. [I explain he means that his spirit ear is located there.] Certainly, my ear . . . E M O . . . that is what bothers me . . . Father is . . . was . . . now . . . no . . . Father . . . our. [Quickly and with excitement.] O U R O U. [Then slowly and reverently, in capital letters.] O U R F A T H E R I S I N H E A V E N. (Good.) [We all shake hands over it.] (W. N.: We generally put it, "Our Father who art in heaven.") [Excitement.] Yes, I remember that too. Well, if you only knew how difficult it is to catch the sound of your voices you would wonder how I could speak at all to you because I have difficulty in making you hear also, when a thing is very clear to me. (Shall we try another?) One more (Shall it be in Latin?) . . . yes. (What pronunciation did you use, Roman, English, or Continental?) Roman. I asked for Greek, but never mind old chap . . . wait . . . I am not quite satisfied . . . But you mentioned the fact which I wished to explain. (Go ahead then and explain.) [Slowly.] W H O A R T I N . . . O.K. fire away . . . [We have scarcely given the new sentence *Tu ne cede malis sed contra audentior ito* before G. P. changes the subject by introducing two or three Latin and French words which he knows will

be significant to me but not to Mr. O—. He then asks that Mr. O— should go out, and begins writing upon a topic which he does not wish him to know of.]

This case is more significant than the others because it does seem that the writer has some knowledge of Greek, whereas the familiar phrase before used might be picked up by anybody. It is also difficult to explain this translation by the telepathic theory. The writer seems not to recognise the familiar words but to translate afresh from the words he hears; if it were merely reflected from my mind one would hardly expect it to take this new form.

In order to test G. P.'s knowledge of Greek still further I wrote a sentence, making the first three words give the keynote of the whole, using very simple and familiar words, and purposely choosing the thought from the group that was uppermost in the minds of the writers. The sentence was:— *Οὐκ ἔστι θάνατος· αἱ γὰρ τῶν θνητῶν ψυχὰι ζῶν ζῶσιν ἀθάνατον, αἰδιον, μακάριον.* We first gave this to G. P. at the sitting of June 25th, 1895 [Present: R. H. and W. R. N.]. At our suggestion G. P. calls the alleged Mr. Moses to help translate it. The result is confusion worse confounded. Apparently the writers cannot hear what we say, *θάνατος* is at first written *fanais*. In this confusion words and sentences occur which appear to emanate from Moses, such as "I could in time recall all the Greek I ever taught and why should I not," "It seems like awaking from a dream to recall this to mind." When the writer finally gets the word *οὐκ* he translates it "light," apparently from association with the Latin word "lux." On June 26th and 27th, further unsuccessful attempts at translation were made. G. P. said that he remembered his Greek well enough when he was away from the "light" but the effort of communicating confused him and drove it out of his head. On July 1st, at a sitting at which Dr. Hodgson only was present, and in the midst of a communication from G. P. upon another topic, the following interruption occurs:—

Who said there was no death? [Hand moves forward as though "speir- ing" into the "vacant space."] Moses (Ask Moses what he means by that.) Well, you interrupt me. Well, I must say old chap (I did not mean to interrupt you.) No not you H . . . Moses . . . Ouk esti thanatos. Moses (that's first rate. Is this Mr. Moses translating?) Ouk esti thanatos. There's no death. Repeat it to me in Greek Hodgson for him. [R. H. repeats, says it is correct and suggests getting the rest of the passage translated.] Come H. Come here a moment. Hurry up H. [R. H. repeats the rest of the passage.]

Again . . . Good oh good may God preserve you always H., and keep you alive on earth until you have accomplished a thorough work. I'll help you in every way possible (Shall I repeat the Greek again?) Yes, something new . . . Yes he's listening . . . too fast H . . . wait . . . ready he has it very nearly . . . not the last H . . . no before . . . yes . . . not quite . . . got it. [R. H. had been repeating the first five words only several times.] I'll go now and translate it and return sir.

This promise was never kept and we heard no more of the Greek. At later sittings other matters came to the front and Moses did not reappear to complete the translation.

At a sitting held June 27th, 1895, at which I alone was present, G. P. told me that he had understood Romanic (*sic*) and a little Celtic, and told me to ask Mr. Howard about it. Dr. Hodgson asked about the Romanic and found that Mr. Howard knew nothing whatever about it. G. P. spent some time in Ireland and wrote a book on Irish affairs.

CASE XIV.

Apparent Clairvoyance or Telepathy.

Several times I got apparent evidence for the acquisition of knowledge in some supernormal way which seemed to suggest rather telepathy or clairvoyance than extraneous intelligences. Thus (June 20th, 1894, present R. H. and W. R. N.) Phinuit said to me :—

Who is this Edith in your surroundings. (There are several.) Yes, I know it [Counts on fingers] one, two, three. This is the young one, dark hair, pretty complexion, nice young lady. [This struck me at the time as a description of a young cousin of mine *as I last saw her*. She is usually lacking in colour, but was then flushed with exertion.]

At another time, on or about June 26th, 1894, while in Dr. Hodgson's study, I picked up a volume of poems by G. P. with a preface by Rogers. I had not known that G. P. had written poems, and exchanged a few words with Dr. Hodgson about it. At the sitting of the 27th, Dr. Hodgson was endeavouring to get from G. P. some evidence of having heard a message which he had tried to give him. The writer seemed to know nothing about it, but wrote :—

No,—but I did hear you tell about the memoriam Rogers . . . [We make some remark.] Yes, I caught it as you were telling him and it attracted me.

Just a year later a similar incident occurred. As the alleged Walter Scott was concluding a sitting he told me that there were monkeys in the sun. That night while writing up the sitting at Dr. Hodgson's rooms, ten miles from Mrs. Piper, Dr. Hodgson and I fell to laughing over this preposterous statement; so loudly indeed did we laugh that I finally cautioned Dr. H. that we would be wakening the whole block. The next morning the writer, without my saying anything about it, explained that he did not mean to say there were monkeys in the sun; the light of the medium was failing him and gave rise to this error. He meant to say that we would follow the light of the sun as far as the tropic of Capricorn and there we would see the monkeys flying in and out of sand caves. I do not see that this explanation betters the matter very much. A little later on, as the writer was professing to show me the moon, the hand suddenly stopped :—

Excuse me sir, a moment. Who was the gentleman with whom I saw you seemingly laughing over my journeys with you! Actually laughing . . . yes sir . . . and roaring enough to split the canopy of space. [I confess I was much taken aback by having my sins thus unexpectedly brought to light; I explained who it was and how absurd the statement about the sun had seemed to us. I begged the writer's pardon.] Not at all, sir, thank you sir . . . exceedingly kind sir. No intelligent spirit would convey for a moment this impression.

At a sitting held June 21st, 1895, at which Mr. H. W. O. was present for the second time, G. P. towards the close, when getting dreamy, said to him.

Give my love to Norton when you see him again. [I ask O. whether he knows Norton; he says yes.] Of course you will and tell him I do not care about Jane Austin. [*sic*] . . . The old essays, read them and you will find out . . . Norton, the dear old chap. Adieu. [Professor C. E. Norton is meant. Mr. O. was under a false name at both sittings.] [See *Proceedings*, Vol. XIII., pp. 333-4.—R. H.]

III.

DISCUSSION OF THE TRANCE-PHENOMENA OF
MRS. PIPER.

I.—BY FRANK PODMORE.

In considering the phenomena presented by Mrs. Piper's trances, I do not propose to follow Dr. Hodgson in his speculations on the ultimate significance of the trance-utterances. Not indeed that those speculations can fairly be held illegitimate, or even premature. It is difficult, indeed, to discuss the content of these utterances at all apart from the dramatic form in which they are cast; and it would be ridiculous to pretend that the invariable assumption by the entranced Mrs. Piper of an alien personality—a personality of the type of the entranced Mrs. Piper—is a fact wholly without evidential weight. But before entering upon the difficult problems suggested by these dramatic impersonations, there is a preliminary question to be asked—a question which Dr. Hodgson himself, and those other members of the Society whose acquaintance with Mrs. Piper's trances has given them the best right to form an opinion—to wit, Professor William James, Professor Oliver Lodge, Mr. Frederic Myers and Dr. Leaf—have already answered to their own satisfaction. But it is well, perhaps, in view of the importance of the issues now clearly seen to be involved, that this preliminary question should once more be definitely stated, and the answer already given, as above indicated, be reviewed. In brief, is it certain that we have to deal with supernormal faculty—say telepathy, to put it at its lowest—at all? Is it not conceivable that the whole of the information given in the trances may have been acquired by normal means, either by unconscious elaboration of hints undesignedly furnished by the sitter, or by a deliberate system of private inquiry?

Now the obvious preliminary remark is that Mrs. Piper does not stand alone. She is a member of a large class. Clairvoyante mediums of one type or another have been numerous, since, at any rate, the days of Mesmer. And, speaking generally, the claims put forward by them, or on their behalf, have been the same as the claims now advanced for Mrs. Piper—the power of diagnosing and prescribing for the diseases, even of absent persons; of seeing events and places at a distance; and of communicating with the world of spirits.

But in attempting to compare the utterances of Mrs. Piper with those of previous mediums, we encounter at the outset a serious—and in most cases an insuperable—difficulty, the meagreness, namely, of the records. Of some clairvoyants, no doubt, no such deficiency can be asserted; he would be an insatiable student, indeed, who would demand an ampler unfoldment of the *Arcana Cœlestia*; or would desire to add to the volumes of the *Great Harmonia*. But whatever the philosophic or theologic value of the utterances of Emanuel Swedenborg and Andrew Jackson Davis—and in ranking the two names together as members of the same class no comparison of the two men, or judgment on their respective systems, is intended to be conveyed—it is clear that their writings offer little external evidence to support their claims to inspiration. Some few instances, indeed, are alleged in which Swedenborg clairvoyantly became aware of events at a distance; but it would scarcely be profitable at the present time to inquire into the sufficiency of the evidence. And I am not aware that Davis' claims to the possession of information supernormally acquired have ever been substantiated in concrete cases. Again, there were many German ecstasies in the first few decades of the present century, of whom the Seeress of Prevorst may be taken as the type, whose delineation of the spiritual spheres, journeys to the moon and the interstellar spaces, and the like, fill many closely printed volumes. But here, again, evidence for terrestrial clairvoyance, though not altogether wanting, is hardly sufficient to call in itself for serious consideration. With these exceptions then, which are scarcely profitable for our present purpose, the records of clairvoyance are for the most part both meagre and sporadic; so that we are unable to judge how much of the alleged success might have been due to information surreptitiously obtained at the time from the sitters themselves; nor what proportion of the statements made on any single occasion were correct, and what were inaccurate or irrelevant; nor again have we any data for estimating the proportion of successful and unsuccessful sittings. Naturally, it is "good" sittings which are recorded; the rest are forgotten. Moreover, an additional element of uncertainty is introduced by the fact that the accounts given are frequently undated, and were certainly in many cases written down months or years after the events. In a few cases, however, there seems to have been a fairly full record of the sittings of some one medium, based upon contemporary notes. In no case, indeed, with which I am acquainted, does the record profess to approach, in accuracy or completeness, the practically verbatim series of reports edited by Dr. Hodgson. But in a few cases—of which the best known are Mr. Stainton Moses, Alexis Didier, and Adèle Maginot—the accounts given are sufficiently full and appear to have been prepared with sufficient care to admit of some useful comparison being made.

I propose then to give a brief outline of each of these series of trance-utterances, in its purely evidential aspect, and to consider how far the information displayed can plausibly be assigned to normal sources.

Some account of the alleged spirit communications made through the medium of Stainton Moses—based almost exclusively on his own statements of what took place—is given by Mr. Myers in *Proceedings*, Vol. XI., pp. 24–113. To the comments made by Mr. Myers I have to add one important detail, viz., that all the information as to the names, ages and dates of death of the three Jones children which was given at the séance of February 10th, 1874, had, it has since been ascertained, appeared in the obituary column of the *Times* (London) on February 4th, 1874, six days before the séance.¹ In the article referred to, 38 spirits are enumerated as having communicated through the medium of Stainton Moses, either by raps or by automatic writing. Of these 38 communicators, 8 or 9 had been personages of some historical importance; 13 were individuals of no special distinction apparently unknown even by name to Mr. Moses or any of the circle; the remainder had been personal acquaintances of the medium or of the Speer family, who formed the usual sitters. The historical personages sometimes signed their names in handwriting which was reported to be more or less characteristic, and sometimes communicated facts in their life history which could have been ascertained, and were actually verified, by reference to their biographies; the unknown persons gave their names, the date of their death, and the disease of which they died, in many cases reproducing verbatim the obituary notice which had appeared shortly before in the daily papers. Of the personal friends, one was a friend of Mrs. Speer, of whom Mr. Moses writes that he had never previously heard; the only facts given were the initials and surname in full—A. P. Kirkland. Another was a sister of Dr. Speer, who had died as an infant before his birth; three Christian names and a surname and particulars as to the death were eventually given. Other relations of the Speers gave their names and relationship correctly; the medium's grandmother and other friends conversed with him, and gave him particulars of incidents known to him, or subsequently verified.

Finally, in one of Mr. Moses' notebooks there were discovered, after his death, communications from President Garfield and a well-known lady, called by Mr. Myers "Blanche Abercromby," which communications purported to have been made in each case some hours before news of the death had reached Mr. Moses through ordinary channels.

Setting aside the last two facts—for which we have no other evidence than the medium's own statement—there is nothing to

¹ See *Proceedings*, Vol. XI., pp. 75-90.

forbid the supposition that the whole of the information given at these séances was the reproduction of facts consciously or unconsciously acquired by the medium from the daily papers, from books, or from conversations with his friends.

Nor do the alleged communications, it is interesting to note, bear in most cases any marked resemblance to those made through Mrs. Piper's organism. Apart from the initial difference that Mr. Moses' spirits mostly communicated by raps, and, when they wrote, wrote not infrequently in a hand simulating the characteristic handwriting of the alleged communicator, it is to be remembered that the names, dates and other obituary particulars, which is practically all of an evidential nature that the "spirits" offer, are precisely the details which are least conspicuous in Mrs. Piper's trance-utterances; whilst conversely Dr. and Mrs. Speer do not seem to have been favoured with any of the curiously subtle character sketches, personal descriptions, odd bits of old family history, etc., etc., which form the staple of the later medium's communications.

There have been numerous exponents in England of "travelling" clairvoyance, especially in the decade 1840-1850. But the records are in most cases fragmentary and sporadic, or contain too little detail. Of one clairvoyant medium of that period, however, we have several tolerably full records by competent observers; and the fact that the clairvoyant was well paid for his performances, and that some of his phenomena, at any rate, were almost without question to be accounted for by the exercise of normal vision, render his case a valuable one for our present purposes.

Alexis Didier was a young Frenchman who was brought to this country in the summer of 1844 and exhibited by one Marcillet, whom Elliotson and others vouched for as a gentleman of high character and undoubted integrity. Alexis was apparently in the first instance thrown into a deep trance; his eyes were then bandaged, generally as follows:—A pad of leather would be placed over each eye, and then two handkerchiefs would be tied diagonally across; over these a third handkerchief would be tied horizontally, and the interstices would be filled up with cotton wool. In these circumstances he would play *écarté* with great skill and rapidity; would know not only his own cards but frequently those in his adversary's hand as well; would play correctly with his own cards face downwards on the table; would frequently, by request, pick out any named card when the whole pack was face downwards. Further he would—though generally with his eyes unbandaged and merely closed—read words written in sealed envelopes, describe the contents of closed packets, and read words and sentences several pages deep in any book which might be presented to him.

From the detailed descriptions of these performances given by various observers in the *Zoist* itself and in the periodical literature of the time, we can gather many indications that the power exercised by Alexis was perfectly normal.¹ In any case bandaging such as that described could not have been accepted as satisfactory. But a writer in the *Morning Chronicle* tells us that he had himself been bandaged by a friend in the same way and had managed to read distinctly. It was noticed, moreover, by several persons that Alexis contorted his face both during and after the process of bandaging; that he frequently touched or fidgeted with the bandages; that he held the objects given to him at curious angles, and often changed their position as if trying to get a better view; envelopes and closed paper packages would be carried, for instance, to the stomach or the top of the head. Moreover, the card-playing appears to have been the only form of experiment which was pretty uniformly successful; even here there were many failures, but the failures seem to have predominated over the successes in other cases. He appears to have selected the packages which he was to read from amongst a large number presented to him; the contents of sealed envelopes could not be read in the hands of a sceptic—the seal must be broken and the contents shown to a sympathetic witness. Marcillet was present in the room throughout the performance. Of all the feats, that of reading the words several pages deep in a book was the most strongly suggestive of trickery. This appears not to have been attempted, as a rule, until Alexis had already read some words on the open page, the book in his hand, with the text covered by a piece of paper or a handkerchief placed there by himself. Alexis would then separate a number of pages, from ten to 150, holding them edgewise before him, and offer to read some words on a particular part of the page several pages further on. It is not stated in any account which I have seen whether Alexis or the audience chose the particular spot on the page; but it is certain that Alexis could not indicate with even approximate correctness the number of pages deep. In one case the words read were found 80 and 150 pages further on respectively.

If this were all that Alexis had to show, we could only wonder at the simplicity of the numerous witnesses—lawyers, medical men, members of Parliament and others—cited in the *Zoist*, who vouched for his performances. But there are two considerations which give us pause. In the first place, though it would have been difficult to prove

¹ See especially *Zoist* II. pp. 393-409, 477-529; the detailed, and, on the whole, impartial account by Dr. Forbes, F.R.S., in the *Lancet*, August 3rd, 1844; a letter in the *Morning Chronicle*, June 28th, 1844, signed *No Go*; the *Times*, June 25th, 1844; *Medical Times*, July 27th, 1844, and subsequent dates; various articles in the *Critic* for 1844 and 1845; etc.

this even at the time, and of course no certain proof is now possible, there are indications that his trance was genuine; and if genuine, it is permissible to suppose, though the knowledge which he displayed had clearly been acquired by the exercise of the normal senses, that he himself was innocent of deception in the matter. In the second place, at every séance, together with this display of conscious or unconscious jugglery, there occurred instances of "travelling clairvoyance" and thought-reading, which, if not genuine, involved deception of a more hazardous and complicated nature. Of course, fraud is the first explanation in a case of this kind. Alexis was a professional—he received five guineas a séance; there is no strong improbability in the assumption that the respectable M. Marcillet was a confederate; and perhaps the most probable, though not necessarily the correct, explanation of his card-playing performances is that of deliberate fraud. But in the following instances, which are selected from numerous other accounts in the literature of that time as having apparently been reported with due care, the fraud must have been of a different kind. The first account is compiled from notes made by Lord Adare of a sitting with Alexis which took place on July 2nd, 1844, at the house of M. Dupuis, in Welbeck-street. A corresponding, but rather fuller and more dramatic account of the incident is given by the Rev. G. Sandby, in a letter to the *Medical Times*, dated July 8th.

Col. Llewellyn, who was, I believe, rather sceptical, produced a morocco case, something like a surgical instrument case. Alexis took it, placed it to his stomach and said, "The object is a hard substance, not white, enclosed in something more white than itself; it is a bone taken from a greater body; a human bone—yours. It has been separated, and cut so as to leave a flat side." Alexis opened the case, took out a piece of bone wrapped in silver paper, and said "The ball struck here; it was an extraordinary ball in effect; you received three separate injuries at the same moment; the bone was broken in three pieces; you were wounded early in the day whilst engaged in charging the enemy." He also described the dress of the soldiers, and was right in all these particulars. This excited the astonishment of all the bystanders, especially the gallant colonel. This account is drawn up, not only from my own notes, but from Colonel Llewellyn's statement made after the séance, and from a written account given me by a lady who was sitting close by.—(*Zoist* II., pp. 510-11.)

On the hypothesis that the information given was normally obtained, we must suppose that Colonel Llewellyn was a garrulous old gentleman, who had betrayed his secret to someone in the room at the time; or that Marcillet or Alexis had by some means acquired beforehand knowledge of his history, and of his intention to attend the séance. Neither supposition can, of course, be dismissed as altogether improbable.

The next case is taken from a letter by Dr. W. B. Costello, which appeared in the *Medical Times* of July 27th, 1844. Dr. Costello explains that, though personally unknown to Marcillet, he had obtained an introduction to him (apparently through a friend), and had accordingly presented himself with Marcillet's card at a séance held in a private house. It may be presumed, therefore, that Marcillet was aware of Dr. Costello's intention to be present, and had had the opportunity of working up his *dossier*. After relating two "clairvoyant" descriptions of distant scenes and houses given to other guests, which were apparently recognised as substantially correct, Dr. Costello proceeds:—

The sitting was now drawing to a close, and . . . I asked permission to put a question as to a fact in which I was engaged early on Monday morning. I said I was aware that coming there under the auspices of the mesmeriser, and, moreover, labouring under the disadvantage of not being known to himself, I could well understand that if the answer was satisfactory it could be only to myself, but if it were otherwise I should state it to be so without hesitation. Permission having been courteously granted, I took Alexis by the hand and asked him to describe the persons, the room, and the act in which I was engaged on Monday morning. He answered, after brief musing, "You are in a room with a person, not on a bed, but a couchette; this person has suffered a great deal; you have been doing something to his head—there is another person also—there are instruments with screws laid out on the table (*des objets mécaniques et à vis*)." While speaking he kept moving his hands over the front of his person, till he reached the lower part of the abdomen, when he exclaimed suddenly, "Tiens, vous l'opérez aussi au bas ventre, vous opérez pour la pierre." I was astonished at the minuteness and truthfulness of his description. I asked him if the patient was old or young. He answered young. The truth was I was performing the operation of lithotripsy on a gentleman, not young, but eighty years of age, at Clifton, on the morning of that day, and the room, sofa and position of the table on which my instruments lay were as correctly described as if he had been present. It is, moreover, not a little singular that the patient has an ulceration behind the ear, which his servant dresses for him. This closed the sitting. Of course the correctness of Alexis' answers to me had no influence on the mind of Mr. —. It might, however have puzzled him, as it has me, had I been known to him, as I trust I am to the rest of the members of my own profession.

The next account is extracted from a long letter, dated *Mon Loisir, Lausanne*, November 25th, 1851, written by the Rev. Chauncey Hare Townshend to Dr. Elliotson, and containing a minute description of a séance with Alexis, at which Marcillet was not present. Townshend relates that he had paid a surprise visit to Marcillet in passing through Paris the previous month; he had introduced himself as a friend of Elliotson, and found that both his name and the fact that he had written a book on mesmerism were unknown to Marcillet. The latter, at Townshend's earnest request, had sent a messenger for Alexis, and

brought him round to Townshend's hotel some hours later. As soon as Alexis was in the "mesmeric" trance Marcillet left the room, and

I began to test the clairvoyance of Alexis, in the matter of seeing distant places. I asked him if he would visit my house (in thought). He immediately asked, "Which, for you have two? You have a house in London and one in the country. Which shall I go to first?"

I said, "To the house in the country."

After a pause, Alexis said, "*J'y suis!*" and then, to my surprise, he opened wide both his eyes, and stared about him. I saw, however, at once, that he had the fixed rigid gaze of a sleep waker. As far as I could perceive, he never once altered the fixed position of the lids during the whole time that he was in distant clairvoyance. The pupil looked dilated dull, and without any movement of conscious activity.

"Well," I asked, "what do you see?"

"*Je vois,*" said he, "*une maison d'un moyen apparence. C'est une maison, pas un château. Il y a un jardin autour. A côté gauche il y a une maison, plus petite, sur la propriété.*"

All this was said in breaths, with some effort, and with a hurried gasp, as it were, between each sentence. I own I was surprised at the accuracy of the description of my house near Lausanne, particularly at the mention of the *small house on the left-hand side*, where, according to Swiss custom, dwells my landlady. It was, in fact, a marking feature of the place, not to be guessed at by a stranger, and, as such, brought much conviction to my mind. "Now," said I to Alexis, "What sort of view do you see?" "*De l'eau, de l'eau,*" said he hurriedly, as if he saw the lake which indeed spreads out before my windows. Then "*Il y a des arbres en face tous près de la maison*" (all true). "Well now," I said, "we will go into the drawing-room" (*salon*). "What do you see?" He looked about, and said (where my memory fails as to the exact words I give the sense in English), "You have a good many pictures on the walls. But now, this is curious—they are all modern, *except two.*"

"And these two," said I; "can you see the subjects?"

"Oh, yes! One is a sea-piece; the other is *un sujet religieux.*"

I really felt something of a shudder at this extreme precision. How then was I astonished when Alexis went on to describe minutely the *sujet religieux*, which was a picture I had lately bought of an Italian refugee, and which had many striking peculiarities. He said at once, "There are three figures in the picture—an old man, a woman, and a child. Can the woman be the Virgin? (he asked of himself musingly) No! she is too old (preceded he, answering his own question, while I remained perfectly silent). The woman has a book upon her lap, and the child *points with its finger to something in the book! There is a distaff in the corner.*" Effectively, the picture represented St. Ann teaching the Virgin to read, and every particular respecting it was correct.

I asked, "On what is the picture painted?"

Alexis answered, "It is neither on canvas nor copper (metal). It is on a curious substance." After some consideration he began to rap on the table with his knuckles, as if trying to ascertain the nature of the substance. Then he called out "*C'est sur pierre.*" (The picture is in fact on black

marble.) "Now," said he, "I am looking at it behind. It is of a curious colour *entre noirâtre et gris* (the exact colour it is behind). *It is also rough behind. Et tiens,*" added he, "*c'est bombé.*"

This last peculiarity would have convinced the most incredulous. The picture, from a warp or curve in the stone, had been very difficult to frame. (*Zoist IX.*, pp. 405-6.)

Alexis then proceeded to give an equally detailed description of Townshend's house in Norfolk Street, London; of the maid-servants there, the horse in the stables, etc.; and furnished various other proofs of clairvoyance.

The obvious remark on a case of this kind is that Townshend was a well-known writer on mesmerism; that it was practically certain that he would at some time or another come to see Alexis; and that it would probably be worth while for Alexis and his agents to "get up" as many facts as possible in connection with him, in order to afford a convincing proof of supernormal faculty. That the description of the maidservants in the house in London, and the grey horse in the stable with sores on its flanks, applied accurately to the time of the séance would only prove, on this hypothesis, that Alexis' Intelligence Department was up-to-date. It is true that this explanation becomes more and more difficult as it has to be applied to a wider and wider circle. But though successful clairvoyant descriptions of the kind appear to have been given at every séance, the reports which we possess are mostly at secondhand, or insufficiently detailed, and the names of persons concerned are frequently not given. As a "sitting" with Alexis appears to have been a kind of levée attended by some thirty or forty persons, it is clear that Alexis had considerable chances of utilising any information which he might have surreptitiously acquired; and the supposition that his display of apparent clairvoyance was, in fact, to be so explained, though it certainly implies the possession of highly-trained confederates and singular good fortune in the chance of sitters, is not perhaps to be summarily dismissed. It is interesting to remark that Alexis himself expressly disclaimed any assistance from spirits in the matter.

The revelations of the two clairvoyants so far considered bear little resemblance to those of Mrs. Piper. Our next seer is of a different type, and presents, so far as I am aware, as close a parallel as any in later spiritualistic literature to the American clairvoyante.

Alphonse Cahagnet describes himself as a simple *ouvrier*. He was, in fact, as we learn from an authoritative account of him in the *Journal du Magnétisme*,¹ originally a journeyman cabinet-maker, and subsequently took up the trade of restoring old furniture. In January, 1848, he published at Paris the first volume of his *Arcanes de la vie*

¹ Vol. XIII., p. 340.

future dévoilés, in which he gave an account of communications received through eight somnambules, which purported to proceed from thirty-six persons of various stations, who had died at different epochs, some of them more than two centuries previously. The séances had extended over some years. This first volume contained "revelations" of the usual post-Swedenborgian kind about the constitution of the spirit spheres, the occupations of the deceased, the bliss of the after life, and so on, together with more personal messages from deceased friends of those persons whom Cahagnet admitted to witness the manifestations. But there is little or nothing to show that the so-called communications did not emanate exclusively from the imagination of the medium, and we are dependent solely upon Cahagnet's good faith and competence for the accuracy of the reports given. Cahagnet appears, however, to have been a man of quite unusual sincerity and teachableness. The criticisms on his earlier work showed him where the evidence was defective; and in the later séances described in his second volume, which was published in January, 1849, he appears to have done his utmost to establish the authenticity of the alleged spirit communications by procuring, wherever possible, the written attestations of the other persons present. The medium in all these later sittings was a young woman named Adèle Maginot, whom he had known for many years. A natural somnambulist from her childhood, she had allowed Cahagnet to "magnetise" her, in order that he might put a stop to the spontaneous attacks which were impairing her health. He soon found her an excellent clairvoyante, especially for the diagnosis and cure of diseases.

In the later séances, however, which took place in the spring and summer of 1848, Adèle was chiefly consulted by persons who wished for interviews with deceased friends. Cahagnet drew up a statement of the communications made at each of these sittings, and asked the sitters to sign the statement, indicating how far the particulars given were true or false. These statements, with the signed attestations, are published. In the few cases where the names are not given in full, Cahagnet explains that for sufficient reasons the sitters had desired that their names should be withheld from the general public, but that they were at the disposal of any private inquirer who might wish to satisfy himself of the genuineness of the accounts. Of course these reports, which do not profess to be verbatim, do not show what indications the clairvoyante may have received from leading questions or undesigned hints by the sitters.

Cahagnet, indeed, seems to admit a certain amount of editing on his part. His words are:—

"Cet ouvrage est loin d'offrir l'intérêt du roman par son style forcément coupé, accidenté. Aussi conviendrait-il mieux aux amateurs de la science

qu'aux lecteurs passionnés des descriptions poétiques de nos romans du jour. *J'ai cherché à rendre le style le plus clair possible en le dépouillant de cet entourage de questions, de scènes étrangères à ce genre de révélations. Je tiens moins à bien écrire qu'à bien persuader . . . Je suis resté dans les limites de l'austère vérité, du rôle impartial de l'historien, présentant à la philosophie du jour, des faits dans toute leur nudité, mais aussi dans toute leur sincérité.*" (Vol. II., p. 233).

But it is evident from the accounts given that many of the sitters, at any rate, were sceptical, and on their guard against deception. And in some cases it is clear that no hints received from the sitters could have furnished information, as in the case of Rostan, quoted below. Another possible evidential defect is that, though Cahagnet tells us that he has recorded all the somnambule's mistakes as well as all her correct statements (Vol. II. p. 126), he does not expressly say that he has published the records of every séance. As, however, we have numbered records of forty-six séances in the interval between going to press with the first volume in the autumn of 1847, and the end of August, 1848, twenty-eight of which sittings took place between the 6th of March and the latter date, it may, I think, fairly be assumed that the sittings here recorded represent at least a substantial proportion of those which actually took place. Lastly, to complete the enumeration of the more prominent evidential defects, very few dates are given. In this respect also, however, the second volume shows a marked improvement over the first. The ninety-six séances there recorded contain hardly a single date. But of the later séances several are dated, and the rest, from internal evidence, appear to be printed in chronological order. I can pay M. Cahagnet no higher compliment than to say that, taken all together, he seems more nearly to approach the evidential standard which the investigators of the S.P.R., after long years of work, have elaborated for their own guidance, than any previous worker in these obscure regions.

The following are a few representative records:—

No. 129.¹—M. Petiet asks for M. Jérôme Petiet. Adèle sees a young man, about twenty-four or twenty-six years of age (he was thirty), not so tall as his brother now present; auburn hair, rather long; open forehead, arched and very pronounced eyebrows, brown and rather sunken eyes, nose rather long, pretty well formed; complexion fresh, skin very white and delicate, medium sized mouth, round dimpled chin. "He was weak in the chest; he would have been very strong had it not been for this. He wears a rough grey vest, buttons with a shank and eye such as are no longer worn. I do not think they are brass ones, nor of the same stuff as the vest. They don't look to me very bright. His pantaloons are of a dark colour, and he wears low quartered shoes without any instep.

¹ Vol. II., pp. 170-2. The sittings are numbered in one series through the two volumes: Vol. II. begins with Séance No. 98.

“This man was of a stubborn disposition, selfish, without any fine feelings, had a sinister look, was not very communicative, devoid of candour, and had but little affection for anyone. He had suffered with his heart. His death was natural, but sudden. He died of suffocation.” Adèle chokes as this man choked, and coughed as he did. She says that “he must have had moxas or a plaster applied to his back, and this accounts for the sore I see there. He had no disease, however, in that part. The spine was sound. Those who applied this remedy did not know the seat of the disease. He holds himself badly. His back is round without being humped.”

M. Petiet finds nothing to alter in these details, which are very exact, and confirm him in his belief that the application of this plaster, advised by a man who was not a doctor, brought on his brother's death, which was almost sudden.

“Signed the present report as very exact.

PETIET,

19, Rue Neuve-Coquenard.”

Note.—The buttons that Adèle was unable to describe were of metal, a dirty white ground, and surrounded by a blue circle. In this apparition there is a remarkable fact to be noted—viz., that Adèle experienced the same kind of illness as this man. I was obliged to release her by passes, she suffered terribly.

In the sitting next to be quoted, M. du Potet, a well-known writer on Animal Magnetism, and editor at that time of the *Journal du Magnétisme* in Paris, had brought with him the Prince de Kourakine, who is described as being “Secretary to the Russian Ambassador.” The Prince had asked for his sister-in-law, and a striking personal description had been given by Adèle, which was acknowledged by the Prince, in the hearing of M. du Potet and two other witnesses, to be accurate. Unfortunately, the Prince's signed attestation was not procured on the spot; he had promised to come again, but—as Cahagnet delicately puts it—“les événements survenus en France l'ont forcé de partir,” and the promised testimony was never obtained. After the apparition of the Russian Princess, however, the record continues:—

No. 117¹.—M. du Potet wishes in his turn to call up M. Dubois, a doctor, a friend of his who had been dead about fifteen months.

Adèle said: “I see a grey-headed man, he has very little hair on the front of his head; his forehead is bare and prominent at the temples, making his head appear square. He may be about sixty years of age. He has two wrinkles on either side of his cheeks, a crease under his chin, making it look double; he is short-necked and stumpy; has small eyes, a thick nose, a rather large mouth, a flat chin, and small thin hands. He does not look to me quite so tall as M. du Potet; if he is not stouter he is more broad-shouldered. He wears a brown frock-coat with side pockets. I see him

¹ Vol. II., pp. 118-120.

draw a snuff-box out of one of them and take a pinch. He has a very funny walk, he does not carry himself well, and has weak legs; he must have suffered from them. He has rather short trousers. Ah! he does not clean his shoes every day, for they are covered with mud. Taking it altogether, he is not well dressed. He has asthma, for he breathes with difficulty. I see, too, that he has a swelling in the abdomen, he has something to support it. I have told him that it is M. du Potet who asked for him. He talks to me of magnetism with incredible volubility; he talks of everything at once; he mixes everything up; I cannot understand any of it; it makes him sputter saliva."

M. du Potet asks that the apparition may be asked why he has not appeared to him before as he had promised? He answers: "Wait till I find out my whereabouts; I have only just arrived, I am studying everything I see. I want to tell you all about it when I appear, and I shall have many things to tell you."

"Which day did you promise me you would do so?" "On a Wednesday." Adèle adds: "This man must be forgetful; I am sure that he was very absent-minded." M. du Potet asks further: "When will you appear to me?" "I cannot fix the time, I shall try to do so in six weeks." "Ask him if he was fond of the Jesuits?" At this name he gives such a leap in the air, stretching out his arms and crying "The Jesuits," that Adèle draws back quickly, and is so startled that she does not venture to speak to him again.

M. du Potet declares that all these details are very accurate, that he cannot alter a syllable. He says that this man's powers of conversation were inexhaustible; he mixed up all the sciences to which he was devoted, and spoke with such volubility that, as the clairvoyante says, he sputtered in consequence. He took little pains with his appearance; he was so absent-minded that he sometimes forgot to eat. When anyone mentioned the Jesuits to him he jumped as Adèle has described. He was always covered with mud like a spaniel. It is not surprising that the clairvoyante should see him with muddy shoes. He had, in fact, promised M. du Potet that he would appear to him on a Wednesday or a Saturday. M. du Potet has acknowledged the accuracy of this apparition in No. 75 of the *Journal du Magnétisme*.

In effect, in the *Journal* of August 10th of the same year, in reviewing the first volume, Du Potet gives handsome testimony to the striking nature of the impersonation, "si bien que je croyais le voir moi-même, tant le tableau en était saisissant. Bientôt cette ombre s'est enfuie en effrayant la somnambule; un seul mot avait causé cette disparition subite, et mon étonnement en fut porté a son comble, car ce même mot le mettait toujours en fureur." But Du Potet, for all that, is inclined to attribute the phenomenon to transmission of thought from his own mind;¹ and a few months later,² in reviewing the second volume, he takes occasion to give the result of his

¹ *Journal du Magnétisme*, Vol. VII., p. 89.

² *Journal du Magnétisme*, Vol. VIII., p. 24.

further inquiries on this séance. Generally, the minute description of the personal appearance and other particulars which were prominent in Du Potet's own mind at the time were correct; and other details were correctly given which Du Potet might have heard, but had certainly not remembered at the time. He had ascertained, however, from the widow and children, that Dr. Dubois took no tobacco; never had a *redingote* of the colour described; had no hernia, and consequently wore no bandage. Moreover, the apparition predicted never came off. Du Potet, however, adds expressly that Dr. Dubois was unknown in life to Cahagnet and his somnambule.

The explanation by telepathy becomes a little more difficult in the séance next quoted. The sitter in this case, Dejean de la Bastie, Delegate to the Government from the Isle of Bourbon, had come a few days previously, and received a personal description of his father, which he acknowledged to be exact with a few trifling exceptions, together with much excellent paternal advice.

No. 141.¹—M. Dejean de la Bastie, already quoted in Séance 138, desires another apparition. He asks for M. Marie-Joseph-Théodore de Guigné. Adèle sees a man about forty years of age, rather tall, with brown hair. M. Dejean interrupts Adèle by saying that this is not the portrait of the person for whom he asks. We see that this gentleman wishes for perfectly accurate information. At the words "rather tall, with brown hair," he says, "He was tall and not brown-haired." Adèle answers that the person whose appearance she is describing must have the same name and belong to his family, that she is conscious that it is so; but he again asks for this gentleman, and a second person appears. The first remains. "The newcomer," she says, "is thirty years of age and over; he is tall and thin, has dark-flaxen hair, a pale face, with rather sweet, dark-blue eyes; a long nose, a mouth that is large rather than small, a long chin. I see he wears a sort of great coat, such as is no longer worn. It is not at all becoming; it resembles a dressing-gown, but is not one; it is dark-blue or black. This garb proclaims him to be a man in orders—a priest or something of the kind. He looks stern. He must have had chest complaint. I see that his lungs are distended with blood. He has been ailing a long time. He is very weak. I think that privations have caused this, and made his chest so delicate. I do not see, however, that he has the germs of any fatal disease, and this makes me believe that his death was violent, accidental, unexpected. His hand is large and thin. I see a medal on his breast, the size of the palm of a hand. He wears low-cut shoes, such as are not worn now. He will not speak to me, so I conclude that he did not speak French."

The following remarks precede the signature of M. Dejean:—"This person had more of gentleness and kindness than severity in his disposition. He died of a malignant fever, accompanied by delirium lasting several days,

¹ Vol II., pp. 219-220.

and attributed by the doctor to the needs of a vigorous constitution thwarted by absolute continence.”

“These details acknowledged to be accurate.

(Signed) DEJEAN DE LA BASTIE,

This 25th August, 1848.

18, Rue Neuve de Luxembourg.”

In some cases, with the express object of excluding thought-transference, the sitter came armed with the name of some dead person of whom he knew nothing—as in the following case. M. l'Abbé A——, mentioned at the beginning of the record, had had a successful experiment of the same kind at a previous sitting (No. 112).

No. 122¹.—Pastor Rostan, who is referred to in the preceding séance in connection with the conversion of M. l'Abbé A——, desired in his turn to obtain an apparition. He asked for a person unknown to him, whose name had been given to him; but there had been a mistake made in giving him this name; in consequence a person appeared whose description we took, but who could not be recognised. At least, such is this gentleman's version, and I do not imagine that I was imposed upon. I suggested a second séance to him, especially as he persisted in asking for a person entirely unknown to him, to such an extent had he been influenced by M. Hébert's arguments. He then asked his maid-servant to give him a name of one of her acquaintances who had been dead some time; he came armed with this name, and asked for Jeannette Jex. Adèle replied: “I see a woman who is not tall, she may be between thirty and forty years of age; if she is not hump-backed she must be crook-backed, for she carries herself very badly. I cannot make her turn round. Her hair is auburn, approaching to red; she has small grey eyes, a thick nose. She is not good-looking. She has a prominent chin, a receding mouth, thin lips; her dress is countrified. I see that she has a cap with two flat bands, rounded over the ears. She must have suffered from a flow of blood to the head, she has had indigestion. I see she has a swelling in the abdomen on the left side and in the glands of one breast. She has been ill a long time.”

M. Rostan handed over the report to his servant, and gave it back to me after adding his signature and the following remarks:—

“This is correct as regards stature, age, dress, carriage, the disease and deformed figure.

(Signed) J. J. ROSTAN.”

But if M. Rostan was staggered by the result of his test, his friends apparently still ascribed the results to thought-transference, which gives Cahagnet occasion for some argument on the subject.

There are, indeed, if a digression from the main argument may be permitted, indications that some at least of the alleged apparitions were subjective—inspired, that is, by the imagination of the medium, supplemented occasionally by telepathic drafts from the sitter. We

¹ Vol. II., pp. 142-144.

should probably be justified in assuming—in default of any corroborative evidence as to their reality—that the accounts of Heaven and of the occupations of the spirits therein, given in the first volume, had no more remote origin than the medium's own mind, whose workings were no doubt directed, now by memories of lessons learnt in childhood, now by hints of the Swedenborgian philosophy received from Cahagnet himself.

Here, for instance, is an account of a heavenly vision vouchsafed to Bruno, Cahagnet's first ecstatic, a young man of 27, of mild disposition, and apparently limited education.

No. 10.¹—Bruno is in a deep ecstasy ; he enters heaven for the first time, and there sees his father, who is seated at a small table reading. “What is the book which your father is holding ?” “It is like ours, but the printing is not the same.” “What is it like ?” “It has letters in the shape of a D, then others shaped like little hooks and crescents. I can hardly describe it to you. My father closes the book and says that we can understand nothing in this writing. Allow me to return to heaven for a moment.” After a quarter of an hour he awakes, looks at his bed with contempt, and exclaims : “Oh ! I understand why the dead do not regret the earth. Who would wish to vegetate on this mud-heap after seeing what I have just seen ?” “What have you seen, then ?” “Heaven.” “Yes, well, what is heaven like ?” “Oh ! I was in a place without any horizon, illuminated by a superb light. Before me was a being who, I believe, was God, seated on a throne ; his head was covered with a shining turban, his beard was grey. I think his arm was resting on the arm of his chair. He was robed in crimson velvet studded with golden fleurs-de-lis. His mien was majestic ; he was speaking to his ministers, six or eight in number. I did not count them. They were all seated on the steps of the throne, and were clothed in robes of the same material and the same colour as the robe of God ; but I do not think there was any gold embroidery on them. All round them and in the distance walked a multitude of beings. Oh ! how ugly are the men of the earth in comparison with those beautiful faces, those fair skins ! A gauze-like scarf covered one shoulder, and, besides that, they had a little skirt of such transparent gauze that every limb was easily distinguishable. Their feet were shod with sandals, fastened with broad laces (*cothurnes*) ; but, oh, God ! how beautiful it was ! I was lifted up into the air, I beheld the earth under my feet, and all these little men, so proud, so vain-glorious, how ill-favoured and poor they seemed to me by the side of those divine beings around me.

At a later sitting, it should be observed, it was revealed to Bruno that the figure seated on the throne was the angel Gabriel.

And here are some extracts from Adèle's visions of Heaven. Louise, her spirit niece, comes to the entranced Adèle to announce the appearance of her second brother [her first had appeared at a previous sitting.]

¹ Vol. I., pp. 18-19.

No. 40.¹—"Which brother is this?" "It is Jean-Marie; he, too, died in Africa three years ago." Adèle looks with joy on these three members of her family; the latter is also in a dragoon's uniform; as in previous séances, she talks to them for a very long time without informing me of the subject of their conversation. "What are your brothers doing up in heaven?" "They enjoy themselves, they walk about." "Eternity cannot be spent in aimlessly walking up and down and enjoying oneself?" "Oh! they play music, study sciences; they are better and more pleasurably occupied than we."

No. 41.—"Here they are." "Do they seem pleased to see you?" "I should think so, indeed." "Is your niece with them?" "No." "Are they glad that they are dead?" "Who would not be glad! they are so happy. They tell me I shall see my mother."

Adèle waits a moment, then all of a sudden she stretches out her arms, seems to embrace her mother; her heart beats violently, her face expresses emotion, she is very joyful and sheds tears. "Does your mother seem as glad to see you as you are to see her?" "Oh! yes." "What does she do up in heaven?" "She is with my father, my brothers, my sister, in short with all the family; she is very anxious about me, she is very happy; she reads, and takes pleasure in hearing my brothers play." "Then there are books in heaven?" "Yes, certainly, and they are not romances such as those of earth." "What do they tell of?" "They tell of the mysteries of God, of science; but they are not written as on earth, so my mother tells me."

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No. 45².—"Are all your relations along with you?" "Four of them are." "Do you expect any others?" "I expect my sister who is also dead, and whom I have not seen as yet. Oh, there she is; how beautiful she is! Oh, God! how beautiful one is after death!" "How is she clothed?" "In her betrothal garments; she died on the eve of the day fixed for her marriage; she is in white, her hair is fastened back; she, too, is barefoot like my little niece; how curious that is!" "Ask them why they have no shoes?" "My mother answers that where they are there are no stones." "On what are they standing now?" "On a beautiful green sward." "By what are they surrounded?" "By a vast and beautiful blue horizon." "What kind of light have they?" "A very pure light which I can only compare to the light at the close of a fine summer's day."

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No. 49³.—Second apparition of Adèle's little godson. She seemed to take the greatest interest in this little creature, and when she saw him go away it seemed to her that he was going to fall; she followed him, and then entered upon the state of complete ecstasy from which I did not find it easy to rouse her; she signified her displeasure by saying to me, as she did before, "Why compel me to return to this globe of mud and wretchedness? I was so happy following this little creature with his pretty little white wings, as

¹ Vol. I., pp. 89, 90, 91.

² Vol. I., pp. 96, 97.

³ Vol. I., p. 107.

M. Bruno saw him. It is I who was wrong in this respect."¹ "How far did you go?" "I ascended very high up; then I went through a great archway, at the end of which there were very beautiful gardens, and everything in them was remarkable for its grace and freshness. There were many people walking in the alleys; some were reading, others played music; they all seemed very happy. Such perfect harmony seemed to exist among them that one could not but long to be with them. I saw there my father, my mother, all my family, which is a very large one. I wished to remain, but my mother and your will obliged me to descend." "How were all these people clothed?" "They all had a kind of gauze robe in all sorts of colours; their physiognomy was quite different, but I easily recognised my relations, although if they were to appear to us thus dressed they would not be easily recognisable."

But there are other accounts which, while they point to the action of telepathy, are extremely difficult to reconcile with the theory of spirit-intercourse held by the recorder.

On two occasions Adèle was asked to search for a long-lost relative of the sitter. On each occasion she found the man *alive*, and conversed with his spirit.

M. Lucas, a carrier (*messenger*), of Rambouillet, came to inquire after the fate of his brother-in-law, who had disappeared after a quarrel some 12 years previously.

Adèle in the trance found the man at once, said that he was alive, and that she saw him in a foreign country, where there were trees like those in America, and that he was busy gathering seeds from small shrubs, about 3ft. high. He would not answer her question, and she asked to be awoke, as she was afraid of wild beasts.² M. Lucas returned a few days afterwards, bringing with him the mother of the missing man.

No. 99³.—Adèle, as soon as she was asleep, said:—"I see him." "Where do you see him?" "Here." "Give us a description of him again and also of the place where he is." "He is a fair man, tanned by the heat of the sun; he is very stout, his features are fairly regular; brown eyes, large mouth; he appears gloomy and meditative. He is dressed as a workman, in a sort of short blouse. He is occupied at present, as he was last time, in gathering seed, which resembles pepper-corns, but I do not think it is pepper; it is larger. This seed grows on small shrubs about one mètre high. There is a little negro with him occupied in the same way." "Try to obtain some answer to-day. Get him to tell you the name of the country where you see him." "He will not answer." "Tell him that his good mother, for whom he had a great affection, is with you, and asks for news of him." "Oh! at the mention of his mother he turned round and said to me, 'My mother! I shall not die without seeing her again. Comfort her, and tell

¹ Adèle had on a previous occasion differed from Bruno—another subject of Cabagnet's—as to the existence of wings on the little child.

Vol. II., pp. 32-33.

³ Vol. II., pp. 34-37.

her that I always think of her. I am not dead!" "Why does he not write to her?" "He has written to her, but the vessel has no doubt been wrecked—at least he supposes this to be so, since he has received no answer. He tells me that he is in Mexico. He has followed the emperor, Don Pedro; he has been imprisoned for five years, he has suffered a great deal, and will use every effort to return to France; they will see him again." "Can he name the place in which he is living?" "No; it is very far inland, those countries have no names." "Is he living with a European?" "No, with a coloured man." "Why does he not write to his mother?" "Because no vessels come to the place where he is. He does not know to whom to turn. Besides, he only knew how to write a very little, and has almost forgotten. There is no one with him who can render him this service; no one speaks his language; he makes himself understood with great difficulty. Besides that, he has never been of a communicative disposition or a talker. He seems to be rather a surly fellow. It is very difficult to get these few words out of him. One would think he were dumb." "In short, how can one manage to write to him or hear news of him?" "He knows nothing about it. He can only say these three things: I am in Mexico, I am not dead, they will see me again." "Why did he leave his parents in this manner, without saying anything to them, as he was happy at home?" "This man was very reserved; he hardly ever spoke. He loved his mother very much, but he had not the same affection for his father, who was a passionate, surly man, and often treated him brutally. The cup had long since been full. It was not the trifling dispute that he had had with his father the day before his departure that made him decide to go away; it had been his fixed determination for some time past. He told no one of it. He went away on the sly. Having kissed them all the evening before, he made good his escape next day, without another word. Do not be uneasy, madam; you will see him again!" This good woman burst into tears, because she recognised the truth of every detail given her by Adèle. She did not find anything at fault in the description. The disposition, the education, and the departure of her son were as Adèle said; but a greater semblance of probability is given to the clairvoyante's account by the fact that his relations had an idea that he had enlisted in Don Pedro's army, and at one time took some steps to ascertain the truth of it. M. Lucas told me of this detail on a journey which he afterwards made to Paris. No information was, however, obtainable. What no less contributed to the astonishment of this good woman, of M. Lucas, and the other people present at this curious séance, was to see Adèle put up her hand to the left side of her face to keep off the fiery rays of the sun in those countries, and appear to be suffocated with heat; but the most extraordinary part of this scene was that she had a severe sunstroke which turned the whole of that side of her face, from forehead to shoulder, bluish red, whilst the other side remained dead white. This dark colour did not begin to disappear till twenty-four hours later. At the time the heat of it was so great that one could not hold one's hand on it.

This simulation, by the subliminal consciousness, of the effects of severe sunburn is no doubt not more incredible than the production in hypnosis of mimic stigmata. Such physical effects of the imagination,

if rare, are well authenticated. But if Cahagnet's last sentence refers to the heat of the medium's skin, I am afraid we must admit that the imagination of the recorder possibly played as prominent a part in the marvel as that of the patient.

Shortly after this incident, M. Mirande, the head of the printing-office in which the first volume of the *Arcanes* had been printed, came to Cahagnet, and asked for a sitting. He was much impressed with what he saw and heard, and finally

No. 105¹.— . . begged Adèle to ask for the apparition of his brother, who, he believed, had died in the Russian campaign. As Adèle did not see him in the spirit world, she said that he was not dead, that she saw him on earth; she described him thus: "I see a fine stout man, with brown hair, black eyes, bright, beautiful, and with a gentle expression; a medium-sized mouth, with good teeth, a well-shaped nose, fresh-coloured cheeks; he is courteous and lively. He looks to me about thirty years of age, he is somewhat taller than M. Mirande. His uniform is that of a non-commissioned officer (I think), for I only see one epaulette, a blue coat, blue trousers with red braid, a shako with a small peak; taking him altogether he is a fine man." "Ask him in what country he is?" "He does not know, or will not tell me. He replies that he has suffered much. He has been made prisoner and sent into the interior of Russia, to the country bordering on China, I think; he says that his brother will see him again." "Why has he not written to him?" "He has done so, but the letters have either been lost or gone astray." "Why does he no longer write?" "He does not know whether his brother is still alive." "Tell him that he is alive, and is making it his business to discover the place of his retreat." "He answers that he cannot tell me that, but that he is in hopes of surprising his brother some day." "What is he doing over there?" "He is very happy, he has some estates of his own which he farms, and employs many people. If he had found the means of getting rid of them by selling them he would have returned to France already, but there purchasers are not to be as easily found as they are with us: he does not know when he will be able to do this." "Ask him to write to his brother." "He has no chance. You think it is easy; he is far away from the sea, and then he is anxious to surprise his brother by returning to him rich and happy. He is not selfish, he is good-hearted, but he is not free from ambition, and never was. The one and only dream of his life has been to end his days as a rich man, and he has reached the height of his desires." "Is he married?" "Yes; but he has no children. Next time I will ask him to let me see the country in which he lives, and also his house and his wife." Adèle is tired, and wishes to be awakened. M. Mirande asks for some further information concerning his brother's uniform. Adèle repeats what she has already said, and adds that she thinks she saw some lace, what we call *brandebourgs*, on his breast, and also that he had white facings to his coat. M. Mirande acknowledged that all the details of the physique were very accurate, as well as those of the disposition, and his brother's ambition for a fortune, his good-heartedness,

¹ Vol. II., pp. 60-63.

liveliness, affection, etc., were all quite true. He does not so clearly recollect the details of his dress; however he is satisfied that it is substantially correct. This gentleman's brother had served in the departmental guard before the Russian campaign. M. Mirande believes that this was the uniform belonging to it, only he never knew him as non-commissioned officer. He is much surprised that his brother, who must be fifty-six, only looks thirty. Adèle reminds him that she sees him at the time of his leaving France. "Then he was only about twenty-one," replies M. Mirande, "you see him about thirty years of age; doubtless the fatigue he has gone through has aged him very much." Adèle replies: "Whilst you only knew him as a soldier at the age of twenty-one, he appears to me as an officer and older. Several years may have gone by between that time and the time at which I see him, that is quite admissible. If I saw him at the age of twenty-one, it would be said that I see in your mind. If I, on the contrary, saw him at his present age of fifty-six, his brother would not recognise him, which M. Mirande understands well enough. He in no wise doubts that it is his brother who appeared. He cannot recover from his astonishment."

We have, unfortunately, no corroboration of the truth of the statements made about those two persons. A third volume of the *Arcanes* were published some years later, which went into a second edition in 1860, and it is perhaps fair to assume that, if news had come that either of the missing persons was still alive, and had passed through the experiences described by Adèle, Cahagnet would not have missed the opportunity of making public such a striking testimony to his subjects' clairvoyance. It follows, then, that in these two séances all that we are entitled to say is that Adèle was able to divine with, it may be admitted, singular accuracy, the ideas present in the minds of her interlocutors. It was a striking example of telepathy; but we have no kind of proof that it was anything more, and from internal evidence it seems very unlikely that it was anything more. In our total ignorance of all conditions and limitations, it is perhaps fair to say that the assumption that the spirits of the dead are ready to attend at any moment the summons of the living does not in itself constitute an additional obstacle to accepting the accounts of Adèle's séances in general as evidence of spirit intercourse. But it is quite another matter when we have to deal, as in the two cases now in question, with the spirits of men still living. How did Adèle manage to discover the whereabouts of those two persons? And, still more, how did she contrive that they should speak with her, and that at a time when one of them, at least, was wide awake and engaged in earning his living by the work of his hands? And was Adèle's power of communicating with the spirits of the living restricted to persons who had gone away to distant climes in order to escape from their relations? If Adèle, or any other of Cahagnet's clairvoyantes, really had possessed the power of conversing with the living at a

distance, I cannot doubt that Cahagnet, in the course of his many years' experiments, would have been able to present us with some evidence of such a power that was not purely hypothetical. Nothing would be so easy to prove. The fact that no such evidence is forthcoming affords, I submit, a strong presumption that Adèle did not possess that power, and that the conversations here detailed were purely imaginary, the authentic or plausible details which they contained being filched telepathically from the minds of those present. The curious similarity of the two accounts also points in the same direction. Both men profess to have written home, but the letters must have miscarried. Neither can write now, because they are far from the sea, in the interior. Both have suffered much; both have been prisoners; both protest that their relations will see them before they die; neither, however, is in a hurry to come back; and neither is willing to discover the name of his present place of abiding.

To suppose, as the recorder supposes, that these narratives are authentic revelations obtained from actual conversations with the spirits of men living in unnamed, and—as Cahagnet explains at length—probably nameless localities in the interior of Mexico or Asiatic Russia, is to strain credulity to the breaking-point. But if these two narratives are not what they seem to be, what are we to say of the other narratives in the book, which are cast in the same dramatic form, and contain similar details harmonising with the expectations or memories of the interlocutors? If those are not authentic messages from the distant living, we require some further warrant for the assumption that these are authentic messages from the spirits of the dead. Considered in conjunction with the almost certainly subjective visions of Heaven and dead playmates which characterised the earlier trances, these later séances certainly point to an exclusively mundane origin.

But, after all, to enquire too curiously whether the information displayed by Adèle reached her hidden consciousness from the minds of the dead or the living, is hardly germane to our present purpose.

It is enough, here, to note that all the witnesses cited by Cahagnet seem to have been satisfied that nothing less than thought-transference would explain the revelations, and that any candid reader now must find it hard to resist the same conviction.

To turn now to the consideration of Mrs. Piper's trance-utterances. The first point to be brought out is that the abundance of the material, the fulness of the records, the watchful supervision exercised over the medium herself for some years past, and the extraordinary and almost uniformly high level of success, make these records much more noteworthy than any previous accounts of the kind. Practically we are justified by the fulness and accuracy of the records in leaving altogether out of account certain sources of error which vitiate to a considerable

extent any conclusions which might be based even on the sets of documents which I have already cited, and which render almost worthless in themselves the great mass of similar narratives in the literature of the past century. It is tolerably clear that Mrs. Piper's success, at any rate, cannot be plausibly attributed to the unconscious reproduction of knowledge normally acquired; nor to the skilful manipulation of information extracted at the time from the sitters themselves; nor to misrepresentation and exaggeration as to what was actually said at the sittings; nor—if we may trust Dr. Hodgson's honesty—to the selection of the "good" sittings and the suppression of the failures. "Fishing" for information, indeed, as Dr. Leaf pointed out nine years ago, may reasonably have been supposed to operate to some extent at the sittings then given in this country; but it is clearly inadequate to explain even a small fraction of the later records. Our choice now seems clearly defined between deliberate and systematic fraud on the one hand and supernormal faculty on the other.

Now as regards fraud, there is, of course, no *à priori* improbability involved in such an assumption. The position of the Society in such investigations has always been that, while no dishonesty is necessarily imputed to the medium, every possible precaution should be taken against dishonesty; and that no experiment can be regarded as conclusive in which the conditions allowed the honesty or dishonesty of the medium to be a factor in the problem. The mere fact that Mrs. Piper has been paid—at the rate lately of 10 dollars a sitting—has in no way affected the precautions taken. The motive to dishonesty in such matters, as we know from long experience, is not necessarily the anticipation of pecuniary reward. The precautions taken in Mrs. Piper's case were not increased because Mrs. Piper was paid, and should not have been relaxed if she had given her services for nothing. But nevertheless, the fact that Mrs. Piper has received payment at the rate of something like £200 a year for about five years past is of some importance, because fraud of the kind here supposed,—the employment of private inquiry agents,—would have necessarily involved considerable expenditure.

If we turn to the case of the three other mediums cited, we see that the motive for fraud was *prima facie* stronger in the case of Alexis Didier, who received a handsome payment for his performances, than in that of Stainton Moses, whose reward was certainly not of the substantial kind, and whose whole career is difficult to reconcile with the assumption of dishonesty.

Again, if we defer for the present the consideration of the internal evidence afforded by the trance utterances, the presumption of fraud in the case of Alexis Didier, based upon the extremely dubious character of his demonstrations of clairvoyance at close quarters is, as has been

shown, overwhelmingly strong; whilst the fact that Stainton Moses' spirit communications were associated with physical phenomena of a kind which are known in other cases to have been fraudulently produced will afford to some minds a presumption hardly less cogent of fraud in the trance utterances also. There is no such presumption in the case of Adèle Maginot or of Mrs. Piper.

There are, indeed, three preliminary arguments against dishonesty on Mrs. Piper's part, to which some weight should be allowed. (1) Mrs. Piper has produced on nearly all those who have come into contact with her—even those who, (see *e.g.*, Report, p. 524), were predisposed to think her an impostor—the impression of transparent honesty. (2) By an almost universal consensus of opinion her trance is a genuine one, and the association of a genuine trance condition with preconcerted fraud of the kind here supposed would be unusual, if not altogether without precedent. It should, perhaps, be added that, whilst we have no independent evidence of any value as to the nature of Stainton Moses' trance, the descriptions given would seem to indicate that in the case of Alexis Didier the trance was genuine, and the contortions observed both on entering and leaving the trance state seem to have been not unlike those observed in Mrs. Piper. There would appear to be no reason to doubt the genuineness of the somnambulist state in the case of Adèle. (3) In all these years—now thirteen or more—during which Mrs. Piper has been under the close observation, first of Professor William James, and afterwards of Dr. Hodgson and other competent persons—though she has been shadowed by detectives, though her personal luggage, as Professor Lodge has told us, has been searched, her correspondence read, her goings-out and comings-in closely watched—during all these years not the smallest circumstance has come to light reflecting in any way upon her honesty. Certainly no other medium has been exposed to so stringent an ordeal. How much weight should be attached to general considerations of this kind it is difficult to say, but in view especially of the fact that the researches of Dr. Hodgson himself, and of many less competent inquirers, have succeeded in bringing home the charge of dishonesty to very many professional mediums, that this medium should have passed through the most searching and prolonged inquiries without even a rumour of an exposure, or the discovery of any suspicious circumstances, is a fact entitled to some weight.

Let us now consider how far we can account for these various trance-utterances without having recourse to supposed supernormal sources of information. As regards Stainton Moses, the case seems to me quite clear. Practically all the particulars which his "spirits" furnished were names, dates, and other concrete facts, such as could have been culled from the daily papers, published biographies, and

conversations with his friends. In fact there could be no difficulty in accounting for the whole of these communications, with two exceptions, on the hypothesis that they merely reproduced facts already present in his subconscious memory. The two exceptions—the entry of communications from the spirits of President Garfield and “Blanche Abercromby,” at an hour when their deaths could hardly have been known by normal means—seem to preclude this simple explanation, and compel us to choose, for these two cases at any rate, between supernormal faculty and deliberate fraud. In the absence of any corroborative evidence we should not be justified in pressing the former explanation.

The problem presented by Alexis Didier is more difficult. It is true that the kind of information given—description of scenes, the exterior and interior of houses, and occasionally the recent occupations of the sitters—is not beyond the competence of a smart inquiry agent; and the circumstances were no doubt favourable; there were nearly always a crowd of persons present at the séances; no doubt he and Marcillet could form a pretty shrewd guess beforehand at some of the sitters; and probably Alexis was free to choose at each séance from amongst the thirty or forty expectant recipients the two or three about whom he had the most to tell. But his frequent and conspicuous success makes this explanation extremely difficult. If fraud is really the explanation of Alexis' clairvoyance at a distance, I think we must regard it as the high-water mark of achievement in this line.

With Adèle Maginot we are carried one step further. We are no longer concerned with names and dates merely; or merely with descriptions of houses and parks; nor is the medium any longer free, within wide limits, to choose what information she will give, and to whom. Adèle, as we have seen, had only one or two sitters at a time; and she had to fulfil the tests which were prescribed to her by them. And the particulars given in response to the requests of her sitters—minute descriptions of the personal appearance, the ailments, the character, and so on of persons often dead many years before—sometimes of persons not known even to the sitters themselves—were not such as any inquiry agent could have ascertained, one supposes, without grave risk of detection; even had the sittings been fixed some time beforehand, and all the other circumstances been favourable to the undertaking of such inquiries. On the whole, notwithstanding the various defects already enumerated in the record, I find it almost impossible to doubt that Adèle's success was due to some kind of supernormal faculty.

But now turn to Mrs. Piper, and note that the conditions of the experiment are in her case incomparably more stringent than in any previous clairvoyante. She could not pour out information at her own

fancy; she could not, even within the narrowest limits, select her sitters; and,—if we admit that the precautions taken were effectual to that end,—she did not even know their names. Practically, of course, the proof of her supernormal powers very largely depends upon the effectiveness of those precautions to secure the anonymity of the sitters in the first sittings,—and I do not propose here to consider any others. What those precautions were are described in general terms in the Report. The arrangements for sittings were made by letter or verbally with Dr. Hodgson; the correspondence and diary of engagements were kept in a locked desk at Dr. Hodgson's office. The sittings were fixed sometimes a fortnight, sometimes only two or three days beforehand; the dates were sometimes changed; in one case for instance (p. 527, Dr. F. H. K., Illinois) the sitting was fixed at two or three days' notice only, the sitter coming from a distant State, and being a stranger even to Dr. Hodgson. Moreover, one series of sittings were held in Cambridge (U.S.A.), under the direction of Professor James; another in New York, under Dr. Thaw; and similar precautions against revealing the sitters' names were taken in this country; at Liverpool by Professor Lodge, at Cambridge (England) by Mr. Myers, in London by Dr. Leaf. That in one or other of these instances the precautions taken may have been insufficient; that letters may have been left lying about; desks left open; false keys found serviceable;—that by some carelessness or malign chance there may have been a loophole for fraud, is, of course, conceivable. But it would be very difficult to suppose that that loophole was always left open, that that malign chance favored Mrs. Piper for nine years so punctually that the sittings which have to be written down as failures now number barely 10 per cent. The case in that respect could easily be made very much stronger; in England, for instance, there were several successful sitters who came as chance callers and were introduced without any previous notice at all. And it is at least worth remarking that the one series of sittings where it would have been least difficult to anticipate the names of the probable sitters and to provide for their advent—the well-known Professors of Harvard, who came when Mrs. Piper was under Professor James' direction,—was one of the least successful here recorded.

But let us dismiss the initial difficulty and assume that Mrs. Piper, by skill of her own, by fatuity of the investigators, or by some incredible chance, had kept herself through all these years posted up in the names of the sitters and the dates of their coming. She would then be in the same position as Adèle Maginot. But if we find it difficult to believe that the information given by Adèle could have been acquired by normal means, what shall we say of the much more detailed and intimate revelations of Mrs. Piper? There are two ways

in which details of the kind given might conceivably have been obtained; either from other mediums, or, directly, by means of inquiries made for the express purpose. Now it is no doubt permissible to assume a freemasonry amongst professional mediums leading to a continual interchange of useful information about persons who are in the habit of resorting to clairvoyants. There is evidence that some such system does exist. And it is the case that several of the sitters mentioned in Dr. Hodgson's Report had previously paid visits to other professional mediums. But, so far as we can judge, that applies only to a small portion of the sitters; and in any case this would not be a complete explanation of the matter. Mrs. Piper's unquestioned superiority to all other professional mediums is in itself sufficient proof that she is not dependent solely on common sources of information. We are driven, then, ultimately to the supposition that Mrs. Piper has in her employment one or more inquiry agents or private detectives.

Suppose, then, Mrs. Piper's agent, armed with the name and address in some distant State, to go on the quest of information about an intending sitter. He would find no difficulty in ascertaining such bare external facts as the locality of the house, nature of business, social standing, etc. The local papers, the public registers, the family lot in the cemetery, the gossip of the local tradesmen would furnish him with additional particulars. In the rôle of book-cavasser, say, he would obtain entrance to the house, and would thus be competent to furnish accurate descriptions of the living rooms and the servants' quarters. By chatting with a sympathetic nursemaid he could learn more personal details—names, age, appearance, disposition, etc., of children, near relatives or intimate friends of the household, and recent accidents, illness or death amongst them. By the more hazardous process of bribing servants to read letters and open desks, and so on, he could in some cases no doubt obtain more intimate details of family troubles, distant friends, relatives dead years ago. But it is clear that the further he pushed his inquiries by such methods—even did time permit—the greater the risk (which on the average of so many cases would amount to a certainty) of ultimate detection. Moreover, such proceedings would be costly and the results very uncertain.

To turn now to the contents of the messages. The first salient point is that Mrs. Piper is weak precisely where Stainton Moses was strong—in names and dates. Dates appear to be given very rarely. Names, of course, appear frequently; but the Christian names are given as a rule first; and both Christian and surnames emerge frequently piecemeal, and obviously with considerable effort. This tentative exhibition of important information was naturally regarded at the earlier sittings as a suspicious circumstance, pointing to "fishing";

and indeed I imagine that Phinuit's evil reputation rests mainly on this characteristic. But in so far as it is founded on this circumstance, that reputation is apparently undeserved. For this same tentative and piecemeal emergence of proper names appears in the most fully reported sittings, where it is tolerably certain that no hints were given, and even in those sittings where the communication is made entirely by writing. So that now it may fairly be urged that the sparing use of dates and the difficulty in eliciting proper names are arguments so far as they go against fraud—against fraud, that is, of the only kind that can reasonably be supposed to have operated.

There are indeed several cases referred to in Dr. Hodgson's previous report (Vol. VIII., pp. 37-42, p. 104, etc.) in which the information volunteered by the trance-intelligence (without any reference, or only an indirect reference, to the sitter) was such as might easily have been got from the newspapers, inscriptions on tombstones, etc. Such are the cases of Porter Brewster, William N—, Gracie X—, and the Rev. Robert West. Now it is interesting to note that in these cases no precise dates are given; the approximate date furnished in one case is four years out; the names and other details in the first case are hopelessly jumbled up and incorrect; whilst in the second case the "spirit" can only indicate the place of his death as "some Western City." The obvious comment is that the "spirit-guides" of Stainton Moses did this sort of thing much better.

Again, in marked contrast, not only to Alexis Didier, but to the great majority of clairvoyantes, Mrs. Piper comparatively seldom gives descriptions of distant localities, houses, rooms and so on; and her success in such delineations, when they have been attempted, seems not to have been conspicuous.

It is hardly necessary to point out what Mrs. Piper's trance-utterances do include—detailed personal descriptions of deceased persons, their diseases and manner of death, their moral and intellectual characteristics; dramatic and lifelike representations of such persons, their mode of address, their attitude towards, and relation with, others still living, references to cherished personal possessions, conversations on various intimate and private matters; revival of forgotten family histories; and so on, and so on.

And in all this enormous mass of information poured out by one to whom—on the assumption of fraud—it represents only indifferent details learned by rote, we find no hint of self-betrayal. The dramatic impersonations are almost uniformly consistent; the complex relationships and varying attitudes are kept distinct; there are indeed irrelevancies and incoherencies; but they are not such as to suggest confusion between different family histories or the attachment of *dossiers* to the wrong person, or any of the innumerable mistakes of

omission and commission into which an impostor, one cannot but suppose, must necessarily fall.

On the almost inconceivable hypothesis that Mrs. Piper has obtained all this information fraudulently, we can but view with amazement her artistic restraint in the use of proper names; her masterly reticence on dates and descriptions of houses and such concrete matters, which form the stock-in-trade of the common clairvoyante; the consummate skill which has enabled her to portray hundreds of different characters without ever confusing the rôle, to utilise the stores of information so laboriously acquired without ever betraying the secret of their origin. In a word, if Mrs. Piper's trance-utterances are entirely founded on knowledge acquired by normal means, Mrs. Piper must be admitted to have inaugurated a new departure in fraud. Nothing to approach this has ever been done before. On the assumption that all so-called clairvoyance is fraudulent, we have seen the utmost which fraud has been able to accomplish in the past, and at its best it falls immeasurably short of Mrs. Piper's achievements. Now, that in itself requires explanation. We know somewhat of the conditions and the limits of fraud, and if all clairvoyants are simply tricksters it has to be explained why Mrs. Piper is so incomparably superior to all her fellows. For whatever differences there may be in the conditions and opportunities, be it noted, are differences which must have operated to the disadvantage of Mrs. Piper. On the assumption of fraud the tremendous gulf between her and them is an almost insuperable obstacle. But, on the other hand, if it be conceded that Mrs. Piper has genuine supernormal powers, the concession is no bar to recognising similar powers in a greatly inferior degree in other reputed clairvoyants. For whilst we know something of fraud, we know nothing at all of the limits and conditions in which such supernormal faculties must be supposed to operate.

IV.

A CONTRIBUTION TO THE STUDY OF HYSTERIA AND
HYPNOSIS; BEING SOME EXPERIMENTS ON TWO
CASES OF HYSTERIA, AND A PHYSIOLOGICO-
ANATOMICAL THEORY OF THE NATURE OF
THESE NEUROSES.

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[*Preliminary Note.*—The following observations were made some eight or nine years ago, while the paper itself was written seven years ago, and was presented at that time to the American Neurological Association at its annual meeting in 1891. It was then laid aside, with the intention on the part of the writer of further carrying on the experiments with the expectation of obtaining more light on the matter, and either substantiating the theory of hysteria propounded or controverting it. Other work has prevented this intention from being carried out, and the existence of the paper was forgotten. It is now published, at the request of Dr. Hodgson, in its original form, excepting that certain parts have been expanded with the view of making the theory more intelligible to the reader who is not an expert in neurology. This much is due in explanation, as, while the observations themselves have long since lost their novelty, the phraseology would imply the contrary, and would seem, perhaps, not to take cognisance of the work that has been done of late years in this field.

During the last decade much work has been done, especially in France, in the field of hysteria, resulting in very extensive contributions to our knowledge of the subject. The studies of M. Janet in particular have given us a new insight into this neurosis, and it may be said that as a result of the accumulated facts we have been obliged to recast our conceptions of the disease. This new work, however, has been for the most part along psychological lines. The theory here offered is an attempt to find a physiologico-anatomical basis for certain psychological phenomena. It can be regarded only as suggestive and tentative, but it seems to the writer that the later

additions to our knowledge of the subject tend to strengthen this theory rather than oppose it.

One of the greatest obstacles to finding a satisfactory explanation of hysterical and hypnotic phenomena is the tacit assumption that the psychological and physical condition must be respectively always the same, and hence the hunt for a universal law governing these phenomena—for an explanation that will comprehend all the phenomena of hysteria in the one case, and another that will do likewise for all phenomena of hypnosis. The phenomena of hysteria are so complex, so various, and involve so many different functions, that in individual cases we must invoke more than one psychological and physical principle. In a given case, for example, the phenomena may depend in part upon the contraction of the field of consciousness and conscious or subconscious fixed ideas; in part upon the lack of cerebral inhibition, on the law of association of nervous processes and organic nervous memories (association neuroses); in part upon the abnormal diffusion of nervous stimuli beyond their customary channels; in part upon auto- or external-suggestion, and so on.

Likewise in hypnosis, in consequence of the complexity of the human brain, we have to deal with such varied phenomena that no single law will cover all cases. For instance, even in somnambulism the phenomena in individual instances are so fundamentally dissimilar that any attempt to refer them all to a single physiologico-anatomical basis must necessarily be unsuccessful. To be more specific, there may be a complete loss of memory, in the somnambulist state, of the whole previous normal life, including even a loss of educational acquirements, as for instance in the case of Mary Reynolds.¹ This young lady not only had forgotten all her previous life but had forgotten even how to read and write, and was obliged to learn these accomplishments over again. In either state, normal or somnambulist, she had no knowledge of the other. The cerebral process affected in such a case, and the functioning centres, or association tracts remaining, must be very different from that of a case like the well-known Félicité, reported by Azam. Félicité in her second state not only remembers the whole of her past life, but has more acute powers than in the normal state. Again there are fundamental differences between these cases and that of M. Mesnet's sergeant, who in his normal state had lost every sense excepting that of touch; and all these cases of so-called somnambulism show marked differences from the induced somnambulism as ordinarily observed in healthy subjects. It may be convenient, in view of our deficient knowledge of the nervous processes involved, to define all

¹ This case, originally reported by Dr. Mitchell, of New York, is the one referred to by M. Binet in his *Alterations of Personality*, p. 4. The fullest account of it will be found in *Harper's Magazine* for May, 1860.

these states as somnambulism. A single term emphasises the analogies between in other respects different classes of facts ; but it should not be lost sight of when we seek a physiological explanation, that, in spite of the general similarities, the physiological processes or cerebral areas involved must be dissimilar, and the adequacy of an explanation designed for one set of facts must not be measured by its adequacy for another. It is probably after all a question of cerebral localisation. Just as an impairment of the functions of the Rolandic region will exhibit itself by loss of muscular power, slight or absolute, limited to the hand, or involving the whole side of the body, and an impairment of the function of the occipital lobe by impairment of vision, according to the extent of cortical territory involved and intensity of the morbid change, so probably different varieties of somnambulism and hysterical states may be obtained, according to the extent of area or number of centres in the highest level whose function is suppressed.

A point which may be parenthetically raised here is whether in some cases of hysteria with physical defects like anæsthesia and paralysis, the so-called somnambulist condition with restored functions is really a somnambulist condition at all, but rather is not the normal state, the subject having been restored to a normal state out of a hystero-hypnotic state. It must not, then, be assumed that the anatomico-physiological theory here advanced is intended to cover all forms of hysteria or hypnosis. The theory supposes a localised "going to sleep" of certain portions of the brain (highest level of Hughlings-Jackson). Such an explanation is applicable only to the classical but probably most numerous class of cases, viz., to that "automatic condition" to which the ordinary but fully hypnotised person is reduced. In those more elaborate states to which so much prominence has been given of late years, and which have been described as spontaneous somnambulism, double personalities, etc., disturbances of other portions or localised areas of the brain are probably involved.

In this connection the recent anatomical investigations of Flechsig, and the theory which he has propounded are of interest. In his late address "Gehirn und Seele," Flechsig claims to have demonstrated the existence of regions in the brain unconnected with the projection system of fibres ; but in connection with the various sensory centres by means of association fibres. These regions, the so-called silent areas of the brain, Flechsig looks upon as association centres, where the various sensory impressions are associated together to form memories. The frontal lobe is one of these association centres, and here, perhaps, in particular are stored the memories which make up a person's individuality. This is only putting in different language the same conception advocated by Hughlings-Jackson, the "association centres" taking the place of the "highest level." It is noteworthy that

Flechsig by the anatomical method should have arrived at conclusions practically identical with the inductions which Hughlings-Jackson reached from the clinical side. Bianchi¹ has also offered a similar theory, viz., "that the frontal lobes are the seat of coördination and fusion of the incoming and outgoing products of the several sensory and motor areas of the cortex. . . . The frontal lobes would thus sum up into series the products of the sensori-motor regions as well as the motive states which accompany all the perceptions, the fusion of which constitutes what has been called the psychical tone of the individual."

If Flechsig's observations shall be confirmed, it may be that many of the phenomena of hysteria and hypnosis are to be explained by a suppression of function of these association centres, the variations in the phenomena depending on the extent to which the centres are affected, or the number of centres involved. In principle this would be identical with the theory advocated in my original paper, to which I now proceed.]

The cases on which the theory mentioned in the title to this paper is based, if not unique, nevertheless are worth reporting because of the light they help to throw upon the nature of certain forms of hysterical anæsthesia and paralysis, and if they do not make clear the exact cerebral defect underlying this affection, they at least indicate what it is not.

More than this, it may be said that the one truth taught by cases of this sort is that the brain of such hysterics really does react to external impressions, notwithstanding the apparent presence of anæsthesia. In other words, that an hysteric who has, to all outward appearances, lost the perception of sensation over a part of the body as well as the special senses, really does feel and see and hear. We may say that such a person feels, but is not conscious that he feels. The corresponding portion of the brain functions, but may be said (for purposes of making the clinical facts more comprehensible) not to be in physiological connection with other portions of the brain. Paradoxical as this may seem, a few simple experiments will easily demonstrate the fact. The cases I have to relate present many points of interest, but I shall confine myself to such parts of their histories as bear directly upon the subject matter of this paper.

The first case, Mrs. B., is one of traumatic hysteria and neuritis. The patient fell off a railway baggage truck, striking upon her left shoulder. In consequence of the accident, a number of mental symptoms developed, such as insomnia, melancholia with mild suicidal

¹ Quoted by Thomas, *American Journal Medical Science*, November, 1896.

tendencies, inability to concentrate the attention, etc. ; but besides the hysterical symptoms there is present a neuritis involving a portion of the brachial plexus. This neuritis has been of rather moderate intensity but irritative in character, affecting the sensory more than the motor nerves, so that the patient has suffered almost continuously ever since the accident (a period now of about two years) from most intense pain about the shoulder and down the arm. Almost any use of the arm causes severe pain, so that it is difficult to determine how much of the inability to use the arm should be ascribed to the effects of the pain, and how much to real paralysis. There can be no doubt, however, that decided functional paresis of nearly the whole arm exists. The absence of all trophic and electrical changes in the muscles shows that the paresis is not due to the neuritis, but is hysterical. Besides this, nearly the only other objective symptom that was present from the first has been a very slight anæsthesia, detected only by very careful and delicate examination over the whole inner side of the arm and hand, corresponding to the distribution of the inner cord of the brachial plexus.

After this condition had existed for about a year and a half, it was noticed that this anæsthesia suddenly had become profound, so that the skin over the ulnar side of the hand could be most severely pinched and pricked without being felt. Most careful and accurate tests were made to demonstrate the actual presence of this loss of sensation. As the patient was a good hypnotic subject it was seen at once that this was a chance not to be lost.

After placing a screen between the patient's face and her hand, I took a pin and pricked the hand several times, then laid gently upon it a pair of small nippers with flat surfaces (such as are used in microscopical work) and pinched the skin with the same. She did not feel the pricks of the pin, nor did she know that anything had been done to her hand. It should be said that care was taken not to give the patient any hint of the nature of the experiment, or even that any experiment was being made. She was then hypnotised. While in the trance, I asked her, "What did I do to your hand?"

"You pricked it."

"How many times?"

"A good many times, more than twelve."

"Where did I prick it? Show me."

[Patient indicated correctly with her finger the part that was pricked.]

"What else did I do?"

"You laid something on it."

"What?"

"Something long and flat."

"What else did I do?"

"Pinched it."

"With what?"

"Something you had in your hand. I don't know what it is."

The patient was then awakened and the experiment repeated with variations. After being again hypnotised, she was asked what was done.

"You pricked my hand."

"How many times?"

"Eighteen."

"All at once?"

"No; first five times, then thirteen."

"What else was done?"

"You pinched it."

"How many times?"

"Five."

"What did I pinch it with?"

"Your fingers."

These answers were all correct.

These experiments were afterwards repeated publicly before the Boston Society for Medical Improvement. Another fact of some importance, though one that has been noticed before in similar cases, should be mentioned here, namely, that during the hypnotic trance, sensation completely returned in the previously anæsthetic hand, so that any manipulation of the skin was immediately noticed by the patient. It may be said incidentally, and as further evidence of the merely functional nature of the anæsthesia, that normal sensation was restored later by means of hypnotic suggestion.

Before commenting on this case I will describe my second patient, who presented somewhat similar phenomena. Only a few of the more prominent features need be referred to here.

Case 2. Mrs. R.'s most prominent disability is hemi-anæsthesia. The loss of sensation over the right half of the body is only slight in intensity, but in the arm it is marked. In the hand there is an absolute loss of tactile, thermal, muscular and pain sense. You may pinch, prick and rub the skin, twist and bend her fingers without her having any knowledge of the fact. The wrist may also be bent without her knowledge, unless the movement be rough. You may place in her hand objects, such as a knife, pencil and a pair of scissors (the thumb and forefinger being thrust through the holes), without her being aware of the fact. She fingers them, handles them, but does not feel them. A bracelet-like line limits the absolute anæsthesia of the hand at the wrist joint. Right hemianopsia is also present. [The line of division passes through the centre of vision.]

Hearing, smell and taste are diminished on the right side ; in fact smell and taste are almost lost.

There is no paralysis, although there has been a history of impaired muscular power on the right side in the past.

There is gray atrophy of both discs. This has been confirmed by Drs. Wadsworth and Williams. The acuteness of vision is nearly normal.¹

In considering the nature of the affection from which this woman suffers, I think there are few who would not say at first sight that we have to do with a lesion of the internal capsule. The hemianopsia with optic atrophy certainly means a focal disease somewhere between the cortex and the optic chiasm, and if it be placed in the neighbourhood of Charcot's sensory crossway it would cause the hemianæsthesia in the classical way. But it is very easy to demonstrate that the hemianæsthesia, at least, whatever be the cause of defect of vision, is not due to a lesion in this situation, or at any rate to one that causes an interruption to the centripetal fibres that pass upwards through this portion of the capsule on the way to the cortex. Indeed, it is very easy to demonstrate that in one sense of the word there is no loss of sensation in this case at all, in spite of all the tests that have been given. In one sense of the word this patient feels perfectly, and if she feels, if it can be shown that by changing the conditions of the experiment the patient is conscious of every impression given to her hand and has a thorough knowledge of the location, etc., of her fingers, it must be admitted that the paths of conduction from the periphery to the cortex, as well as the cortex itself, must be intact. To show this, we have only to hypnotise her. Now touch her hand or body or foot ever so lightly and she feels it perfectly. You may place objects in her hand, the same that she failed to recognise before, and she tells their names immediately, without hesitation. She says at once knife, scissors, rubber bands, etc. The translation from an insensible hand to one that is perfectly normal, is startling. The special senses have returned as well. In fact so far as her physical powers are concerned, she is perfectly normal. Wake her up and the hand and the whole right side lose at once what they recovered. One symptom alone persists during the hypnotic state—she does not see out of the right half of each eye ; a further proof that the hemianopsia is due to an organic lesion.

It is a necessary induction from these facts that the centripetal fibres to the inner capsule and the sensory centres in the cortex must be intact, and the anæsthesia must be functional.

It may be suggested that, although there is no organic lesion causing the hemianæsthesia, yet there may be some change, dynamic

¹The *macula* apparently was not involved in the atrophy.

or vaso-motor, which prevents the passage of impressions in the waking state and disappears during hypnosis, allowing sensory stimuli to be felt. In other words, under hypnosis the cortex and sensory paths resume their activity but lose it again when the subject is awake. But it is not difficult to show that this is not the condition of affairs we have to deal with, for it can be demonstrated that this patient feels with her right hand, not only when hypnotised, but when awake.

The following is one of numbers of experiments made for this purpose :—While Mrs. R. is awake I squeeze the fingers of the right hand, prick the hand with a pin, close and open the fingers, place a pencil, scissors and my knife in the palm,—and she has no knowledge of what has been done. She says she feels nothing. She is now hypnotised.

“What did I do to your hand when you were awake?” I ask.

“You stuck a pin in it.”

“How many times?”

“One, two, three, four, five.”

“What else?”

“You put a pencil, scissors, and your finger in it.”

“What else?”

“Nothing.”

“What else did I do?”

“Pinched it.”

“What else?”

“Doubled it up.”

“Anything else?”

“No.”

“Sure?”

“Yes.”

These answers were correct.¹ The anæsthetic hand then really did perceive, so to speak, the impressions, and that, too, as intelligently as a normal hand, although at that time the patient was not conscious of it. From this it seems to me that the conclusion is inevitable that the sensory conducting fibres from the periphery to the brain, as well as the cortical sensory centres, are physiologically normal in this case. These cases seem to me to show that in hysterical anæsthesia of this sort the sensory cortical centres receive and record external impressions in a perfectly healthy way, and whatever may be at fault in such cases the defect is not to be found in these regions.

There is another piece of evidence which goes to show that the anæsthetic hand of such patients really, so to speak, feels. It will be

¹ The only answer about which there may be doubt is the number of times the hand was pinched.

remembered that Mrs. R. was described as deftly fingering and turning over in her hand any object placed in it, but was unconscious of the nature of the object. Unless the hand "felt" the object she could not possibly use the hand in this way.

One naturally asks for an explanation of such phenomena as these. Can we find any physiological basis upon which they may rest?

Hughlings-Jackson has pointed out that three different levels of evolution may be distinguished in the central nervous system. These three strata are both anatomically and physiologically distinct, but are arranged as an hierarchy. In the lowest level are to be found the most organised, the most automatic, and the least complex nervous arrangements. The highest level contains the least organised, the least automatic (or most voluntary) and most complex arrangements. The array of evidence which Hughlings-Jackson marshals in support of this plan of development is so strong and convincing as almost to take it out of the field of hypothesis. I shall not be able here, however, to refer to the mass of facts physiological and pathological on which it is based, but shall only state briefly so much of the general scheme as may be necessary for my purposes.

In the *lowest level* are included the spinal cord, medulla, and the brain-stem as high as the nuclei of the cerebral nerves. The *middle level* contains Ferrier's motor region and certain portions of the sensory region. The *highest level* includes much of the sensory region and all that portion of the brain anterior to the so-called motor region, that is, the greater part of the frontal lobes. Ferrier's motor region (central convolutions, etc.) Hughlings-Jackson has named "middle motor," because he believes all the frontal lobes anterior to Ferrier's motor region to be also motor in character. These are the highest motor centres. The highest level is concerned with the more complex forms of ideation.

The centres in the lowest level are the most simple and the most organised. They represent comparatively limited regions of the body. For example, a nucleus in the lumbar enlargement represents only a limited number of movements. These movements are simple and they are most automatic, in the sense that they may go on by themselves with greater or less independence of all other centres. These centres are also most organised. Comparatively simple combinations of movement are developed at an early period, which persist with relatively little modification.

In the middle stratum, Ferrier's motor region, the movements are more complex, and they represent wider regions of the body. The same parts are represented as below in the lowest level, but in a more complicated manner. They are less automatic, and the centres are less organised, as development is continually taking place in them,

allowing new combinations of movements. It is here, probably, that originate the movements engaged in writing, sewing, typewriting, and the various manual occupations *after* they have been once acquired.

In the highest level, in the anterior lobes, all parts of the body are represented, but in more complex combinations still. This level represents over again what has been represented in the middle motor region, which re-represented in its turn what was represented in the lowest level. In passing then from the lowest level (the spinal cord) to the highest level (the anterior lobe), we meet with "increasing complexity or greater intricacy of representation, so that ultimately the highest motor centres co-ordinate movements of all parts of the body in the most special and complex combinations."

The highest centres, like all other parts of the brain, are sensori-motor, and it is probable that they supply the greater part of the physical basis of consciousness.

Looked at from a purely physical point of view, all parts of the brain must be "sensori-motor." The term sensori-motor is not synonymous with sensation and volition, but refers to the physical cerebral process alone. It is impossible to conceive that at some particular point, the brain processes cease to be "sensori-motor," and become something else. It would be opposed to the doctrine of evolution. The highest centres can differ from the lower centres only in more intricate combinations of movements and impressions. There is every reason to believe that the highest centres represent movements as well as impressions. This conclusion is based on a study of many clinical facts, and particularly those of epilepsy. I will not dwell on the evidence which may be adduced in support of this statement, as we are more particularly concerned with the sensory centres. It is mentioned here only for the sake of completeness.

The highest sensory centres likewise represent all parts of the body in intricate combinations. Sensory impressions received in the middle centres are co-ordinated in the highest centres with other impressions and with movements, and there have their psychical counterparts as ideation, volition, emotion, and other states of consciousness.

Accepting, then, this anatomical scheme as approximately correct, let us see if we can build upon it a psycho-physiological scheme which will correspond with normal psychological experience on the one hand, and pathological facts on the other.

In the first place, it is a familiar fact that there is a sort of duality to consciousness, which exhibits itself in two ways. One way is that while intently thinking about one thing we may be doing another of an entirely irrelevant character, and the thing we are doing may not only involve movements of considerable complication, but these movements may be constantly corrected and guided by sensory impressions

from our environment. Furthermore, these impressions, although thus acting on the organism, may not be perceived by the individual. Movements of this kind are recognised as in a greater or less degree automatic, according to the vividness with which they and the incoming sensory impressions enter into consciousness.

The second way in which duality is exhibited is in compound movements which are primarily and as a whole the result of direct volition, yet are made up of a series of simpler and co-ordinated movements; these simple movements are not directly willed, but seem to take place automatically, without our being conscious of any effort in, or with almost no knowledge of, their production. In fact we may have to direct special attention to learn how we make such movements, and to analyse the successive steps in the synchronous occurrences. As familiar instances of this may be mentioned the movements concerned in buttoning a coat, or those of a well-drilled soldier doing the manual at arms, or in shaving the beard. When we button our coats, we may do it while we are thinking of something else, in which case we have an example of the first form of dual consciousness; or the act may be initiated by volition, and thus be an act represented in consciousness, but in most instances we do not consciously move the different fingers by successive efforts of volition, as I do now,—first the thumb and forefinger, then removing the forefinger, bring into play the thumb and middle finger, and then removing the latter, complete the act by the tip of the thumb. The co-ordination of these different movements seems to take place unconsciously and automatically. In fact I did not know how I did it in detail until I watched my hand and observed the sensory impressions guiding each step. My conscious mind wills the resultant act as a whole, and the component movements are made unconsciously. A little observation will show that a very large number, if not the majority, of our acts are thus made up of these two kinds of movements, the volitional and the unconscious (automatic). It would seem as if there were two minds, one the self-conscious mind which gave the general order, and the unconscious mind which carried out the details. A very pretty experiment in evidence of this was the following. Writing is an art which is partly volitional and partly unconscious. Very few of us probably have any idea what contractions of the fingers we make in forming the letters, or, for that matter, are conscious of crossing our t's, dotting our i's, or forming our letters. Now it is very easy to obtain automatic writing with Mrs. R. On one occasion the automatic and anæsthetic hand wrote the verse, "Mary had a little lamb," etc., without Mrs. R. being conscious of it. She was then hypnotised and asked what the hand had written. She answered correctly. When asked if she had made any mistakes she said "Yes," and added that

she had left out such a letter, failed to cross such a *t*, and dot such an *i*, which was correct. This would indicate that such details were directly controlled by one part of the mind.¹

This duality of consciousness (the conscious or volitional and the unconscious or automatic), exhibits itself in the greater part of our daily acts.

Now if Hughlings-Jackson's anatomical scheme is correct, it would presumably follow that the conscious movements originate in the fore brain, or highest level, and the greater part² of the unconscious movements, in the middle level, or Rolandic-motor area. Otherwise this scheme is unintelligible.

Let us now go one step further, and see if we can make out the probable relationship between the middle motor and sensory regions and the frontal lobes, on the basis of our present physiological and pathological knowledge.

In the first place, we know that sensory impressions arriving simultaneously from the eye, the ear, the skin, and various parts of the body, are primarily received and recorded as sensations in the second level. Here we also fairly may claim to know that they are associated together among themselves and to a certain extent with the vestiges (memories) of similar impressions of the past. At this level then, our daily experiences may be recorded and become chains of memories, or associated impressions which may make up a personality, but a personality of limited attainments. The activity of such a personality is comparatively simple and automatic. That is to say, the sensory impressions received at the second level may result at once in outward expression or muscular action (through its Rolandic-motor centres); but such immediate response means automaticity and simplicity. The movements are a little complicated. It is probable, for example, that the elementary movements concerned in writing, sewing, speaking, warding off an expected blow, changing the posture, altering the direction of our walk to avoid obstacles, playing games, like tennis, etc., etc., are all performed at this level. But the sensory impressions received at the middle level may not result in immediate movement, or whether they do or not they (generally) are transmitted to the highest level (the frontal lobes), where they become associated with that great network of vestiges (memories) of past impressions which constitutes the whole experience (intellectual and physical) of the individual. The activity of this level is the dominant consciousness for the time being of the individual, and so long as it is in activity, *is* the personality of

¹ The conditions were such that Mrs. R. could not see the writing. Of course in this experiment all the writing was done automatically, but in ordinary conscious writing we are not conscious of such details.

² Not all, for many undoubtedly originate in the third level.

the individual. Now, in so far as a sensory impression at the middle level is transmitted, received, and associated with the complicated processes of the highest level, is it perceived (recognised) by the personality, becomes a part of the dominant state of consciousness, and the "person" is said to perceive this or that. If the highest level is in activity, even though a sensory impression is received at the middle level, and therefore "felt," so to speak, by this part of the brain, it is not felt by the individual unless it becomes associated with the processes in the fore brain (highest level). If it stops at the middle level, it becomes a part of a definite personality if the highest level is not in activity; or, as associations are necessarily formed in the middle level, whenever the activity of the frontal lobes alone is suppressed the dominant consciousness becomes that of the middle brain, and all the impressions that have been there received and welded together stand out as a personality which has "felt" all the sensory impressions composing it.

Now it may be that the highest level and the second level may act more or less independently and simultaneously, like two distinct but connected brains. While the highest level is carrying on a complicated train of thought and even expressing these thoughts in words, the second level may be automatically doing something else, like guiding the individual in his walk through a crowd, or sewing, knitting, playing a game, etc.

Although, for the most part, the highest level is so in rapport with the second level that it is cognisant of the greater part of that which the second level feels and does, and maintains a guiding control over its actions, this is not wholly nor always so. Many sensory impressions, —sounds, sights and tactile feelings,—may enter and be recorded at the second level which never reach the highest level, and therefore do not enter the dominant consciousness. This is particularly exemplified in absent mindedness, *i.e.*, where a person is not conscious of impressions from his environment; and yet such impressions were recorded as sensations, for they afterwards may be remembered in dreams and hypnosis. Similarly, the union may be so severed temporarily that the middle brain may perform many acts of which the frontal lobes are unconscious. Such acts are typically automatic. The more unconscious the highest level is of the middle level, or, to speak more precisely, the less the activity of the latter enters into association with that of the former, the less controlled and less complex and the more "unconsciously" automatic are the actions of the middle level. This dual activity of the two levels may be illustrated on the sensory side by the well-known observation of Dessoir. A gentleman was absorbed in reading a book while a conversation was going on about him in the same room. On being questioned it was found that he was

entirely unconscious of what had been said. He was then hypnotised, and while in hypnosis was able to repeat the conversation. Here we may assume that the highest level was entirely occupied with the thoughts suggested by the book, while the middle level was receiving and recording the language of the conversation. The association between the two levels was temporarily suspended. Dual activity, of which that of the second level is motor and purely automatic, probably has been experienced by every one on similar occasions; as when engaged in deep thought while dressing, the wrong clothes have been put on, or, at other times, objects have been taken up and misplaced, or, to use more common illustrations, sewing, knitting and other mechanical work has been done. This dual activity, when persisting with complete independence by the middle level of the highest level, is called absent mindedness. A good illustration is an observation of himself by the writer. On a late occasion, being impatient to learn the news, he read the newspaper while walking along the street. After a time he became conscious of the fact that, although completely absorbed in the newspaper he maintained his direction and avoided obstacles as accurately as under ordinary circumstances, while occasionally his attention would be awakened to exceptional objects in the sidewalk (hydrant covers). It required only a moment's observation to discover that, although central vision was upon the newspaper, the peripheral parts of the retina saw the sidewalk and the houses on the one side, and the curbing and street on the other. Walking was here probably directed automatically by the occipital lobes (middle level). Similar experiences are well known and have happened to us all, as when engaged in deep thought or animated conversation we have walked along a crowded street, avoided the passers by and vehicles, perhaps nodded to acquaintances, all automatically, without remembrance of the facts. These unconscious experiences may, nevertheless, come out afterwards in dreams. Parenthetically I may remark that in this way may be explained many of those so-called extraordinary dreams in which a person has dreamed of the arrival of a person, or of some accident happening to a person of which he imagines he never heard, and yet, as a matter of fact, he "unconsciously" saw that person or heard of the accident under conditions which made no impression upon the dominant consciousness; that is, there was a dual activity of the two levels.

Thus far I have emphasised the independent activity of these levels. But of course, such independence is not the rule. The *inter*-dependence of one level upon the other is essential for mentation that shall completely subserve the intellectual wants of the individual. This interdependence and co-working of the two is probably very complex, and with our present knowledge cannot be understood in its details.

It may be, and possibly justly, denied that the different levels ever function absolutely independently of each other. I think this is very likely true. At one moment or another, in a normal healthy individual, the sensory impressions and movements of the middle level make connection with the highest level, and thereby come into consciousness, and what would have been purely automatic movements are re-ordinated and adjusted as volitional movements. And conversely, the conscious ideation of the individual is being constantly corrected by sensory messages from below. Nevertheless, approximately or relatively, the middle level may act as an independent organised centre.

Ordinarily, as I have said, there is mutual interaction of which the details can be but imperfectly understood. We cannot more than grasp the general scheme, but there are three principles in this relationship which are important and have a practical bearing on the present problem.

First, the highest level requires, and is entirely dependent upon, the second level for all intercourse with the outer world. That consciousness which we call self sees and hears and feels only *through* the consciousness of the middle level, and also acts only *through* this level; that is, the highest level has to make use of the so-called sensory and motor centres of the middle level to feel on the one hand and act on the other. This would seem to be a necessary inference from the loss of function—of sensation and movement—which always follows injury to these centres. The middle level, therefore, would know a good deal of the conscious life of the highest level.

Second. As the second level feels and acts directly without any intervention of the highest level, when acting automatically it would receive a great many impressions and do a great many things of which the highest level was unconscious; that is, this conscious life of the second level would not always enter into that group of mental states which we call personal consciousness.

Third. The more complex states of consciousness which make up the conscious waking state of the individual have their seat in the highest level, and in order that an impression received in the lower level may be *perceived*, it must be united in the highest level with all the other impressions which constitute consciousness at any given moment; or, in other words, inasmuch as in the last analysis consciousness must consist of a compound of feelings or sensations (vivid or faint), any given state of consciousness must depend upon the combination of numerous sensory [and motor] centres. And unless a given centre A in the middle level is physiologically associated with that group of centres B, C, D, in the highest level which for the time being subserves consciousness, the sensation corresponding to the given sensory centre

A cannot be brought into the consciousness of the individual and cannot be perceived and recognised as belonging to the individual.

If, further, all the highest centres were removed or their power to function suppressed, then consciousness would be limited to the activity of the middle level, which would constitute a second personality, and would be of a more or less automatic character.

In this psycho-physiological scheme which has just been outlined, it may be well at this place to point out that there is only one assumption that is novel. The three automatic levels of evolution we owe to Hughlings-Jackson. The duality of consciousness, that is, the division of mental processes into the higher complex processes of ideation and volitional movements on the one hand and into the automatic subconscious, or almost subconscious, processes on the other, is well recognised as characteristic of every mind. This duality has been more precisely worked out of late as conceptions of personality;—the most complex associated states of consciousness and chains of memories constituting the self conscious personality of the ordinary waking state,—and the more automatic subconscious states constituting a second personality which plays a constant, but more or less hidden, part in the mental drama of life. Subliminal consciousness is but another term to specify certain particular associations of this second personality.

The thesis here put forward is the identification, or rather, if it is preferred, the correlation of the higher complex states of self consciousness (first personality) with the highest level of Hughlings-Jackson, and the correlation of the automatic more or less subconscious states with the second level (the middle motor and sensory region). It must be admitted that it is hardly possible, with our present knowledge, to offer any satisfactory proof of this correlation, such as we are accustomed to look for in the experimental sciences. Nor should it be forgotten that the same is true of our conceptions of anatomical levels and personalities. The best we can do is to offer a theory, provisional perhaps, which shall harmonise our anatomical conceptions on the one hand and our psycho-physiological experiences on the other. In so far as it does this and explains what are otherwise paradoxical phenomena is it of value. It seems to the writer that the theory here suggested, so far as it goes, fulfils the demands of the problem.

Accepting this scheme, then, the further question arises, how does it correspond with observed pathological facts? Will it render intelligible the phenomena of hysterical anæsthesia as observed in the two cases above reported? It seems to the writer that the apparently paradoxical phenomena are just what would be expected from this scheme; in truth they are the direct corollary of it. The explanation has already been outlined above. It was observed in the cases of Mrs. B. and Mrs. R. that, notwithstanding apparent loss of

sensation, tactile impressions were really felt but not perceived, that is, did not enter into the group of conscious states which constituted the self-conscious personality. How can a person at the same time both feel and not perceive that he feels? Now according to this hypothesis, *anaesthesia of hysteria is the inhibition or going to sleep of certain limited areas (or centres) of the highest level (frontal lobes), while hypnosis is the more or less (according to the stage) complete inhibition or going to sleep of the frontal lobes as a whole.*

In hysteria there is a local suppression of function; in complete hypnosis, a total suppression of function of the highest level.

Take the case of Mrs. R. I prick her anaesthetic hand a definite number of times; I place in it a pair of scissors, which she fingers. Neither the pricks nor the touch of the object does she "feel." What is the matter? Where is the fault? We know it is not in the nerves. It cannot be in the middle tactile centre, for then not only would any given tactile impression, a prick, be not perceived, but it would not be received at all, and by no device (like hypnosis) could what never had been received be revived as a memory. If on the other hand we suppose that there is a local suppression of function in the frontal lobes (highest level), Mrs. R. would not "feel" the pricks given her hand nor recognise the scissors, for although these sensory impressions were recorded in the middle level, they stopped here. They did not reach the frontal lobes, and consequently become associated with all the other impressions of movement and sensation which, as normally associated together, constitute perception. Her consciousness is therefore minus any given tactile impression which stopped at the middle level.

Now we hypnotise Mrs. R. What happens? We recognise at once two phenomena.

First. Sensation has returned to her hand. That is, she perceives tactile impressions, etc. Why? Evidently because (under the terms of our theory) the activity of the frontal lobes being suppressed as a whole, the states of consciousness which are "awake" and in rapport with the outer world are those of the middle level. The association of these states constitutes a "second personality," which feels all impressions.

Second. This personality remembers the pricks before given the hand. Why? Evidently because they were received here (although they did not reach the frontal lobes), and were associated with the other conscious states of the personality of this level, and therefore are now revived as memories.

Third. The hypnotised subject is largely devoid of spontaneity, and her acts are more or less automatic. Why? Because these are the characteristics of the middle level.

Thus by our theory we have an intelligible explanation of the anæsthesia of hysteria and its relation to hypnosis. It becomes comprehensible why a sensory impression is recorded but not perceived during the waking state and yet is remembered during the hypnotic trance.

Regarding the nature of the suppression of function of hysteria, I do not think we are in a position to speak with any positiveness. I termed it an "inhibition" or "going to sleep," for the reason that the process seems to have more analogy with sleep than any other condition; but I do not wish to be understood as insisting upon any precise condition as the cause of the suppression of function. Even if the process were like that of going to sleep we have little idea what it is, but it would seem plausible that if this process should affect definite and limited areas of the brain it would account for the phenomenon of hysteria.

There are a number of other phenomena allied to those of hysteria which seem to harmonise very well with this explanation. Among them may be mentioned those of double consciousness and automatic writing. I have succeeded in obtaining automatic writing from both these subjects. I will not describe these experiments at length, as they have already been published¹ elsewhere, but will merely mention a few of the more important phenomena elicited. There are several ways of obtaining writing of this sort, but a simple method is to hypnotise a subject and during the trance state to tell her she is to write something—a verse or anything one pleases—when awake. After being awakened, a pencil is put in her hand and she is given a book to read aloud, or told to count backwards, or do some mental problem. While her attention is occupied with this, the hand holding the pencil, if placed over a sheet of paper, will write what was desired. The subject herself has absolutely no knowledge of what her hand writes. It was found by Professor William James that the automatic hand while writing became perfectly anæsthetic. I found the same thing with my subject, Mrs. B., after her anæsthesia was cured. Her hand could be severely pricked without her being conscious of it. It seems to me that this phenomenon may be explained by the theory of hysterical anæsthesia just advanced. If there is a local inhibition of the highest sensory and motor centres, the hand would be moved automatically by the remaining middle centres. The pain following the pricks would not be felt, because it would not be associated with the conscious states induced by reading the book, or whatever the mental problem was. The impression of pain would remain isolated on a lower platform.

¹ *Boston Medical and Surgical Journal*, May 15th and 22nd, 1890.

There are also a number of very remarkable phenomena connected with hysterical anæsthesia, which have been observed by Binet. Binet, following an entirely different method, has also shown that the anæsthetic hands of hysterics do feel, or at least in some way do record the impressions given to them. He has succeeded in obtaining very elaborate movements from the hands simply by means of tactile impressions given to the anæsthetic skin. But even more interesting, he has found that many hysterics perceive *as a visual sensation* any tactile or muscular impression. Such an hysteric sees, visually, any figure or letter written on the skin. If the finger be bent three times, the number three is seen as an indistinct image. The subject can also tell the position given to the hand by seeing it as a visual image. But after seeing the image of the hand in this way he cannot modify the image by any mental effort, so long as the operator does not change the position of the hand.

All these phenomena, it seems to me, are physiologically intelligible, if we assume that in anæsthesia of this kind there is an inhibition of the highest centres, while the middle centres still react. Although in this case the impressions received at the middle level cannot excite the highest *tactile* centres and be perceived, they may still continue to excite the highest *visual* centres which are normally co-ordinated with the tactile centres in the perception of one's hand. The perception of one's own hand includes a number of associated sensations, some vivid and some faint. Among them is the visual sense. Though the tactile sense fails, one of the others may be excited by an impression in the middle centres.

SUPPLEMENT.

I.

THE BRITISH MEDICAL ASSOCIATION AND HYPNOTISM.

I.—ADDRESS BY DR. J. MILNE BRAMWELL.

At the annual meeting of the British Medical Association, held in Edinburgh last July, the subject of hypnotism was again brought before the medical profession. A discussion, as to its phenomena and theories, was opened in the Psychological Section, July 29th, by Dr. J. Milne Bramwell (by invitation of the Council of the British Medical Association), followed by Mr. F. W. H. Myers (who was invited by the Council to read a paper), Drs. Yellowlees, Benedikt, Woods, Mercier, etc.

Dr. Bramwell commenced his address with a short account of a few of his hypnotic cases, selected for the following reasons:—(1) The patients were natives of our own country. (2) All the observations had been checked by independent witnesses. (3) Sufficient time had elapsed to enable the permanence of the therapeutic results to be fairly estimated.

Of these the following are examples:—

Case 2.—Pruritus and eczema. Mrs. A., aged 49. Suffered from pruritus and eczema, which four years' careful treatment had failed to relieve. At night the irritation was intolerable, and produced insomnia. The disease, in the opinion of a well-known surgeon, was due to an organic cause which interfered with the circulation. An operation, performed in order to relieve this, proved unsuccessful. In August, 1889, Dr. Bramwell tried to hypnotise the patient, other treatment being abandoned. The attempt failed, and was repeated unsuccessfully on sixty-six occasions during the next four months, the condition meanwhile growing steadily worse. At the sixty-eighth séance somnambulism was induced. All irritation vanished immediately, and the patient slept soundly on that and the following nights. In a fortnight all trace of disease had disappeared, and treatment was abandoned. Three years later there had been no relapse.

Case 3.—Hyperhidrosis. Miss B., aged 15, January, 1890. On the back of the left arm, just above the wrist, a patch of skin, two and a half inches long by one and a half broad, was the seat of constant perspiration. This had existed from early childhood, was always excessive, and invariably rendered more so by emotion or exertion. The forearm was always enveloped in bandages, but these rapidly became saturated, and the perspiration dripped upon the floor. On January 10th, the patient was hypnotised for the first time, and somnambulism induced. By the following day the symptoms had markedly diminished; the patient was again hypnotised, when the perspiration ceased. Two years later there had been no relapse.

Case 5.—Neurasthenia ; suicidal tendencies. Mr. D., aged 34, June 2nd, 1890. Barrister. Formerly strong and athletic. His health commenced to fail in 1877, after typhoid fever. He was compelled to abandon work in 1882, and had since been a chronic invalid. He was constantly depressed, and suffered from anæmia, dyspepsia, insomnia, etc. He had frequent suicidal impulses, and once attempted suicide. The least exertion produced acute pain in the lower part of the spine, and he was unable to walk 100 yards without severe suffering. He had constant medical treatment, including six months' rest in bed, without benefit. He was hypnotised from June 2nd to September 20th, 1890. By the end of July all morbid symptoms disappeared, and he amused himself by working on a farm. He has not relapsed since, and can walk forty miles a day without undue fatigue.

Case 6.—Dipsomania. Mr. E., aged 33, April 30th, 1890. He had a family history of intemperance, and commenced to take stimulants in excess at seventeen. In 1884, his friends induced him to place himself under control. This was repeated thrice without good results. In 1887, he entered a retreat for a year, but soon after leaving it began to drink as badly as ever. From this time he does not appear to have taken stimulants daily, and, according to his own account, struggled hard against the temptation to do so. Any physical pain or mental trouble, however, would start a drinking bout, and of these he had on an average one a week. He was hypnotised from April 30th, to May 17th, 1890, and during this time kept sober. He returned home and relapsed in less than a month. He was again hypnotised daily for a week, and from that date, June 1890, until the present time, he has not relapsed.

Case 7.—Dipsomania. A patient, aged 47, with bad family history of alcoholism. He had taken stimulants to excess for seventeen years, had had three attacks of delirium tremens and seven of epilepsy. He was first hypnotised on April 22nd, 1895, and has not relapsed since.

Case 8.—Neuralgia of the leg. Miss F., aged 28, July 17th, 1896. She complained of pain in the leg of five years' duration, supposed to be due to sciatica. During the first two years of her illness she never walked more than a quarter of a mile, then this was abandoned, and she took to a bath-chair. Treatment :—Rest on back in bed for two months ; careful drugging ; Weir Mitchell ; massage ; electricity ; baths at Droitwich and Bath ; Paquelin's cautery to leg, seventy applications daily from July, 1895, to May, 1896, about 20,000 in all. During this time her condition steadily grew worse ; all treatment was then abandoned, and she was considered incurable. When Dr. Bramwell saw her she was emaciated ; complained of constant pain ; was unable to walk ; suffered from insomnia ; had lost all interest in life. She was hypnotised on July 17th, 1896 ; completely recovered in two days, and learned to cycle. At the present date she is well, active, and strong.

In March, 1890, Mr. Turner, of Leeds, performed many painless dental operations upon Dr. Bramwell's patients, and recorded the results in the *Journal of the British Dental Association*. The most remarkable case was that of a young girl, suffering from valvular disease of the heart, from whom he extracted five teeth.

On March 25th, 1890, a number of Dr. Bramwell's patients were operated on at Leeds, in the presence of some sixty medical men, including

such well-known surgeons as Pridgin Teale, Mayo Robson, etc. A delicate girl was put to sleep by written order, while Dr. Bramwell remained in another room, and sixteen teeth were extracted. She showed no signs of pain, there was no corneal reflex, and the pulse fell during the operation. A boy, aged 8, suffering from exostosis of the great toe, had only been hypnotised on two previous occasions. Mr. Mayo Robson first performed evulsion of the great toe-nail, then removed the bony growth, and part of the first phalanx. Some of the patients were strong, healthy labouring men, others were weak, nervous women. None suffered from pain afterwards, and all returned home by train—a journey of over an hour. In every instance the healing process was remarkably rapid.

Dr. Bramwell then gave a short account of some of his experimental researches in reference to automatic writing, the appreciation of time by somnambules, and more especially the condition of the volition in hypnotised subjects. Taking his personal observations as a basis, he discussed how far the theories of others were satisfactory in explaining them. It is unnecessary to refer to this portion of the paper in detail, as the greater part of it has already appeared in the *Proceedings* of our Society. It is worthy of note, however, that although two years have elapsed since then, further experiment has not caused Dr. Bramwell to alter his opinions.

He still regards the theories of the Nancy school, especially in reference to so-called automatism, and the possibility of successfully suggesting crime, as untenable. The further evidence he has collected shows, he thinks, more and more clearly, that the phenomena of hypnotism are due to the intelligent and voluntary action of a secondary consciousness.

II.—FREDERIC W. H. MYERS,

Hon. Secretary of the Society for Psychological Research.

THE PSYCHOLOGY OF HYPNOTISM.

I must begin what I have to say by warmly thanking the officers of this Association for allowing a layman in this place to say anything at all, and by thanking this learned audience also for showing themselves thus willing to hear me. I feel a quite special diffidence in undertaking my present task; but I can best show my sense of the honour done me by speaking frankly on, without further preface or apology.

I understand that what is asked from me is some attempt at a coherent psychological presentation of the multifarious and perplexing facts now commonly grouped under the name of hypnotism. Purely physiological explanations thereof have, by common admission, thus far failed; and little attempt has yet been made by the able practical hypnotists, to whom the recent advance in our knowledge is owing, to correlate their ever-growing observations from a purely psychological point of view. My late friends, Edmund Gurney and Professor Delbœuf of Liège, did, indeed, apply to these problems minds of unusual acuteness. They and others have said much of value; but much more remains to be said than any of us in this generation can hope to say. My own attempt at synthesis will fall far short both of certainty and of completeness; but its very imperfections may

indicate how great the difficulties are which have to be faced ; how wide and penetrating a survey is needed if these phenomena, often so superficially described, are to carry us, as they ought some day to do, deep into the mysteries of human personality.

I say then, as my first and surest remark, that in order to understand hypnotism we must bring into comparison a great mass of cognate life-phenomena, both spontaneous and induced. From the lack of wide comparison, from the confinement of attention to some few of the commoner and more obvious manifestations of hypnosis, have sprung most of those narrow and misleading theories of which Dr. Bramwell has spoken. But there is now no excuse for such narrowness. Far more facts have become matter of common knowledge than Despine, or Heidenhain, or Charcot knew ; and even a cursory glance at the recent annals of hypnotism will show at how many points its problems touch on problems already familiar in other fields of research.

Let us briefly dwell on each main group of these problems in turn. In hypnotic records we find, to begin with, abundant instances of isolated losses and gains of faculty resembling the fantastic associations and dissociations, dynamogenies and inhibitions, which characterise hysteria. We find, too, that uprush into consciousness of ideas or impulses, matured beneath the conscious threshold, which forms, in my view, the best definition of genius. We find that interruption of external attention, along with that profound organic recuperation, which are the marks of sleep. We find the change of personality, the intercurrency of memories, which in their slighter forms are called somnambulisms, or sleep-waking conditions, and which may exist quite apart from hysteria. Finally, we observe in hypnosis—rarely indeed, yet, as I hold, unmistakably—certain forms of supernormal sensibility which bring hypnotism into connection with all those facts (as I must needs regard them) to which I have given the names of “telepathy” and “telesthesia.”

HYPNOTISM AND HYSTERIA.

Let us begin with one of the most obvious of these analogies,—the analogy between hypnotism and hysteria. How far does this analogy extend ? In broad outline the answer is plain enough. Both in hypnosis and in hysteria there is a disaggregation of the personality. Instead of the continuous personality of common life, with its one familiar alternation of sleep and waking, there are minor changes of phase, interruptions of memory, irregularities of will, inhibitions of faculty, something capricious and mutilated in the manifestations of the self. In the case of hysteria these changes are plainly degenerative. They indicate what Dr. Pierre Janet calls *misère psychologique*, some weakness in the grasp which holds together all our separate fragments of motor and sensory capacity under that central dominance which makes the organism a unity. Scraps of capacity drop out from that dominance ; the muscles, for instance, which are innervated by some special centre, sink down beneath the threshold of conscious will. The hysteric, let us say, gets a fixed idea that he cannot move his left arm. So long as this idea persists,—and there is no trustworthy medical method of removing it,—the man's arm is in effect lost to him ; it is psychically paralysed, and as useless as if the governing brain-centres were

actually destroyed. And here the thought at once occurs that this is exactly the kind of effect which the platform hypnotist produces by suggestion. His suggestion, like the hysteric's self-suggestion, deprives the man of the use of his arm; he induces a form, though a brief and curable form, of psychical paralysis. The recognition by the Paris school of analogies of this kind between hypnotism and hysteria was originally, as I conceive it, a forward step—a result mainly of the increased care and intelligence with which hysteria had been studied by Charcot and his disciples.

But the last word on the matter has certainly not yet been said. If we look a little deeper into those analogies, we shall discover the point at which they begin to be misleading—the point at which hysterical and hypnotic workings, although *in pari materia*, begin to be in effect almost exactly opposite the one to the other.

Expressed in a sentence, the difference is this: In hysteria we lose from supraliminal control portions of faculty which we do not wish to lose, and we cannot recover them at will. In hypnotism we lose from supraliminal control portions of faculty which we wish to lose, or are indifferent to losing, and we can recover them the moment that we will.

A good way, I think, of presenting these two notions together is to call that stratum into which the faculties submerged in hysteria sink the hypnotic stratum, and to describe hysteria as a disease of that stratum; or say an undue permeability of the psychical diaphragm which separates ordinary consciousness from the deeps below. This or that group of sensory or motor ability drops out of waking knowledge or out of control of waking will.

In hypnotism, on the other hand, we gain instead of losing control. Instead of losing control over the supraliminal stratum, we gain control over the hypnotic stratum. We purposely increase the permeability of the psychical diaphragm in such a way as to push down beneath it various forms of pain and annoyance which we are anxious to get rid of from our waking consciousness; while, on the other hand, we stimulate in the depths of our being many sanative and recuperative operations whose results rise presently into the perception of our waking life.

An example will make my meaning clear. One frequent symptom in hysteria is a retrenchment or deformation of the visual field. The hysteric, with his weakened central authority, his enfeebled attention, has lost the use of the outer margin of his normal field; he cannot see a moving finger till it is almost directly in front of him. But now, instead of the finger, bring within the disused margin of the visual field some object associated with the patient's hysterical terrors, as a lighted match or a stuffed mouse. In that case, as Professor Janet has ingeniously shown, the patient will scream and go off into an *accès* long before the mouse has reached the point at which the mere finger would have been noticed. This means that the power of vision over the margin of the field, although lost for practical use, is retained in the hypnotic stratum, and manifests itself again at the bidding of a hurtful hysterical caprice.

Now compare the result if a hypnotic suggestion is made to the patient with the diminished visual field. Professor Janet, for instance, suggests to her that she can really see with the whole normal field. Apparently this is nothing more than an empty remark from which no results can be expected

to follow. But if the patient is suggestible, if a certain appeal—whose nature we must discuss further on—is effectively made to the patient's subliminal region of personality, to the hypnotic stratum in particular, then a result does follow. The power of vision begins to return over the outer parts of the field—fitfully, perhaps, and with relapses; but if the suggestion is repeated the sight is often permanently restored. That is to say, hypnotism has exactly repaired the mischief which hysteria has done. It has acted on the same stratum, but with an increase instead of a diminution of central control. In just the ways in which hysteria can destroy hypnotism can fulfil.

SUBMERGENCE AND EMERGENCE.

In describing that most complex of known entities, the human mind, any physical illustration whatever seems even absurdly inadequate. Yet such illustrations we are compelled to use, and, when not pressed too far, they may be more suggestive than long abstract phrases could be. The notion of upper level, middle level, and lower level nerve centres has started many a fruitful thought. And somewhat similarly this present notion—in which the terms upper and lower are used in a different metaphorical sense, this notion of a kind of stratification of the personality above and below the threshold of ordinary consciousness—will be found to suggest new and practical questions to which it is possible to find some beginning of answer.

Thus, it is now natural to ask whether, if faculty which has existed above the conscious threshold and has been unduly submerged may then emerge again above that threshold, other faculty which has not ordinarily existed above the threshold may ever emerge from beneath it? Does this happen in the psychical storm of hysteria? Does it happen in normal waking life? Does it happen as a result of hypnotic suggestion?

Our answer must be, Yes, it happens in all these cases. Even in hysteria we often observe great tactile hyperæsthesia, as in Binet's experiments, where the hysteric perceived the relieved surfaces on an unknown coin pressed to her skin with a delicacy immensely—fifty times, as Binet thought—beyond the range of normal sensibility.

I do not say that this hyperæsthesia was of any practical use, but it was faculty; it was faculty previously unknown, but borne upwards into the conscious stratum by that same psychical disturbance which carried downwards, and away from conscious control, many more important fragments of that patient's sensory and motor power. From the practical or therapeutic side the hysteria brought nothing but loss to the patient; from the psychological side it brought also a certain gain. For the physician man's faculties exist for the benefit of man; if he comes upon rare and useless capacities, he dismisses them as probably morbid, and at any rate uninteresting. For the psychologist, on the other hand, man's faculties exist for the knowledge of man; the more rare and useless the faculty, the more interest it has for him as a possible inlet into some human mystery yet unexplored.

HYPNOTISM AND GENIUS.

And here, in this very question of the emergence of unfamiliar faculty from a subconscious stratum, our next step shows us faculty thus emerging which is of real use; products of subliminal mentation uprushing into

ordinary consciousness with actual benefit to the waking life. This is the reply to our question asked a little way back : Does this emergence occur in normal life ? My answer is that it does, and that when it does it constitutes genius.

Here again the psychological view of human faculty will differ from the æsthetic criterion in much the same manner as we just now saw that it differed from the medical. The æsthetic critic asks whether the thoughts and images which surge up ready-made into the artist's or poet's mind—his inspirations, as he calls them—are such as to give delight to other men. Unless they are such, the critic refuses to him the name of genius. To the psychologist, on the other hand, it matters little whether other men find joy in the artist's inspirations or no. The question which interests him is, how those inspirations arise ? Can we prove that they were matured by subliminal mentation, beyond the artist's conscious control, and then presented to him as finished products from his subterranean workshops ? If so, I submit they all share a certain definite character, to which the name of genius might with real significance, although not with practical convenience, be given. And this is true, although the results which can most easily be proved to be of subliminal manufacture are not likely to be results in the highest order of art. Psychologically the best specimen of genius may be the calculating boy's vision of the product of two factors of six figures each, seen in a flash upon a mental blackboard, with no consciousness of the process by which it was attained. The calculating boy's achievement, indeed, may seem a mere curiosity ; but without the type of faculty which that boy has shown, the inspirations of a Shakespeare or a Raphael could never have arisen to bring joy to mankind.

Thus prepared, let us go on to the next question, which to-day most concerns us, the question whether hypnotism succeeds in bringing up faculty from submerged strata into conscious control or enjoyment. My answer is that to do this very thing is of the essence of hypnotism. We have seen how hypnotism brings up again to the surface the portions of faculty which hysteria has submerged. We have seen also—it is an obvious inference from Dr. Bramwell's cases—how hypnotism acts in parallel fashion to genius, by elaborating subliminally certain intellectual results which are then presented ready-made to the waking intelligence. Dr. Bramwell's milliner, computing subconsciously the far-off dates when her suggestions fell due, is a precise parallel to the calculating boy inspired with arithmetical results reached by no conscious working. Or again, when she solved in the trance a difficulty in her work, and that solution came to her in waking hours as an inspiration, she underwent exactly the poet's experience, though the subject-matter may have been only the set of a skirt or the trimming of a bonnet.

That which the gift of Nature does in certain limited directions for some few delicately constructed men, that can hypnotic suggestion do for the ordinary clodhopper, with results of course grotesque in comparison to the triumphs of art, but yet quite as striking in proportion to the common man's inferior powers. That "*objectivation de types*," for example, which Professor Richet has best described, that assumption in speech, writing, demeanour, of some suggested character which is one of the commonest platform phenomena, is in fact an inspiration of genius as remarkable for

the rustic as the triumphs of a Duse or a Sarah Bernhardt are for their much higher organisations.

And here I come to one of the commonest phenomena of hypnotism—usually spoken of as a mere jest or a mere indication of the hypnotiser's power over his patient—but which the psychologist, I think, is bound to regard as one of the most striking of all our indications of latent faculty. I speak of the hallucinations which the hypnotiser suggests in the entranced, or sometimes even in the waking, subject. Such a hallucinatory image is, strictly speaking, an inspiration of genius, even if it represents nothing better than a black cat. The painter's highest joy consists in the sudden emergence into perceptibility of some fair form created below the threshold : the "flash upon the inward eye" of some remembered or transmuted image which deliberate efforts could neither shape nor recall. As the Sistine Madonna was to Raphael, so to the hypnotised girl is the delusive cat. The girl in her ordinary state can no more conceive with deceptive distinctness that feline image than Raphael every day could hang in heaven a form of supernal beauty. Hypnotised, the girl can see the creature's very crouch and spring ; she has evoked from her subterranean treasure-house of imagination a picture incomparably more vivid than waking imaginative effort could have afforded her.

Yet all this lies, so to say, but on the fringe of hypnotic power. The main interest of hypnotism lies in a still deeper evocation of latent faculty. The fact which is bringing hypnotism before the medical profession is not its power to imitate and in some sense to outdo the achievements of genius, but its power to imitate, and greatly to outdo, the achievements of sleep.

HYPNOTISM AND SLEEP.

The relation of hypnotism to sleep was the next point marked out for our discussion. Here, again, I must begin by giving to sleep a psychological definition.

I regard sleep as an alternating phase of our personality, distinguished from the waking phase by the shutting off of the supraliminal life of relation, of external attention, and by the concentration of subliminal attention upon the profounder organic life. To sleep's concentrated inward attention I ascribe its unique recuperative power. Our entry into this phase of our personality is not wholly a voluntary thing. Sometimes it is hindered by physical causes, as by pain ; always (as some hold) it needs to be helped on by physical causes, as by the accumulation of waste products in the brain. The first obvious effect of hypnotism is to bring sleep more fully under our control. Under hypnotic suggestion people fall asleep without fatigue to help them, and sleep on so that no tortures can wake them—sleep on in the dentist's chair or through the great pain and peril of childbirth. They can remain at will in that regenerative phase of personality which for us needs so much physiological preparation, and is subject to so many pathological checks.

It is, of course, from my point of view, perfectly natural that an increase of power over the personality should facilitate our transitions between its different phases. But when a phase so profoundly withdrawn from outward stimulus can thus be produced, ought we to give it the name of

sleep, or rather of trance? Trance is the better word; for to identify hypnotic with ordinary sleep is to underrate the modifiability of this almost infinite complex of sentient units which we call a man. I have called the act of falling asleep a passage into another phase of personality; but such passages are like the irregular heatings and coolings of a molten mass; the gaseous, the liquid, the solid state each has certain stable laws of its own; but every transition involves millions of molecular changes which can never occur twice in just the same way. There is something in sleep that reminds one of some increase of internal heat caused by the collapse of a cooled surface; for in sleep, along with the decrease of responsiveness to the external world, certain inward movements become more active, more profoundly recuperative; fulfil more subtly the organism's hidden need. In this respect hypnotic trance is like a further stage of sleep. Not that it is necessarily more lethargic, more comatose; it has forms so alert as to be hardly distinguishable from waking. Trance is a further stage of sleep in the sense that it accomplishes more powerfully sleep's characteristic task; the subliminal plasticity is more marked, the subliminal control intenser; until hypnosis sometimes seems to be to sleep what sleep is to waking.

HYPNOTISM AND SLEEP-WAKING STATES.

Nay, more. In ordinary sleep, neither hysterical nor hypnotic, certain phenomena from time to time occur which the physician may sometimes wish to check as inconvenient, but which to the psychologist should yield lessons much deeper than he has yet drawn from them. I speak of somnambulisms or sleep-waking states, which in my view are rudiments of new phases of personality, useless for the most part, and destined to be abortive and to die away. To these nascent rearrangements of personality the hypnotic trance furnishes abundant parallels. Sometimes it has even happened that for long periods—in one case at least for a lifetime—the new phase of personality, developed by hypnotic suggestion, has been more salutary for the patient than the old. Here also hypnotic artifice has improved upon the hints which Nature spontaneously gave, and has shown that there is no real certainty that the particular disposition of personality with which each man is born must be absolutely the best for all his life on earth, no proof that the kaleidoscope of his being may not be sometimes shaken into a more satisfactory pattern. And setting aside those extreme cases where one phase of personality is marked off from another by an actual break in the chain of memory, many of the sanative changes which hypnotism effects are morally and physically so profound as to deserve the name of regenerations. The suicidal melancholic, the brutalised morphinomaniac are hardly the same persons as the active hospital nurse, the successful man of business, into whom hypnotic suggestion transforms them. And be it remembered that these sanative regenerations are both for physician and for psychologist the *leading facts* of hypnotism—the facts which call most pressing both for explanation and for development.

I have now briefly reviewed the relation between hypnotism and certain comparable, if not strictly cognate, conditions—namely, hysteria, genius, sleep, somnambulism. I have shown by each comparison that the essential meaning of hypnotism is always the same—a fuller control over subliminal plasticity.

SUGGESTION AND SELF-SUGGESTION.

But how, after all, is this fuller control effected? How is this subliminal plasticity—this *vis medicatrix Nature*—actually reached? On this question Dr. Bramwell has demonstrated, with all the advantage of actual experience, what some of us foreshadowed long ago—I mean the absolute insufficiency at present of any purely physiological explanation. No such explanation, indeed, now survives with sufficient vitality to be worth the trouble of killing anew. The main consensus of living hypnotists declares that hypnotic phenomena are due to *suggestion* almost or quite alone. We need not reject their dictum, but we must make it our task to try and find out what that word suggestion can mean. One thing the word certainly cannot mean, if it is to have any explanatory value at all; and that is—more ordinary persuasiveness. Dr. Bramwell (to take his own instance) is not the first person who has advised the dipsomaniac not to drink. If he succeeds in reforming such a patient, it is because he has managed to touch not his supraliminal reason, but his subliminal plasticity. He has set going some intelligent organic faculty in the man which has laid dormant till that moment, and which proves more effectual for healing than the man's conscious will. How then, has he done this? He has either infused power, or he has merely evoked it. Either he has added power by some influence, such as Cuvier and many competent men have believed in—some influence itself subliminal, and akin to what I have called telepathy; or else in some empirical way, not as yet understood, he has simply started a self-suggestion; has unlocked, as I say, some fountain of energy which was latent within the man's own being.

Now for my part I certainly believe that subliminal relations between man and man—influences and transmissions as yet unknown to science—do play a real part in hypnotic phenomena, especially and manifestly in the rare cases of the induction of trance from a distance. But I shall not here insist on these transmissive influences; for I believe that the great mass of hypnotic results can be, and are, accomplished without them, especially and manifestly in the fairly common cases where the subject can bring on the trance for himself, with no external suggestion to help him.

Whether, indeed, those results are accomplished without invoking the same agency, whatever it be, which manifests itself in telepathy, is quite another question. What we see in hypnotic recuperation is an apparent dynamogeny, or increase in organic energy; but whence that new energy comes we can only conjecture. As to this, all that our review of hypnotism and allied conditions has made plain is that it is from the subliminal region that any new energy or new modification of energy must needs proceed. Beneath the threshold of waking consciousness there lies, not merely an unconscious complex of organic processes, but an intelligent vital control. To incorporate that profound control with our waking will is the great evolutionary end which hypnotism, by its group of empirical artifices, is beginning to help us to attain.

In waking consciousness I am like the proprietor of a factory whose machinery I do not understand. My foreman—my subliminal self—weaves for me so many yards of broadcloth per diem (my ordinary vital processes) as a matter of course. If I want any pattern more complex, I have to shout my orders in the din of the factory, where only two or three inferior

workmen hear me, and shift their looms in a small and scattered way. Such are the confined and capricious results of the first, the more familiar stages of hypnotic suggestion.

At certain intervals, indeed, the foreman stops most of the looms, and uses the freed power to stoke the engine and to oil the machinery. This, in my metaphor, is sleep, and it will be effective hypnotic trance if I can get the foreman to stop still more of the looms, come out of his private room, and attend to my orders—my self-suggestions—for their repair and rearrangement.

The question for us proprietors, then, is how we can best get at our potent but secluded foremen; in what way we can make to our subliminal selves effective suggestions. And here, I think, we are for the present at the end of theory. We must look for guidance to actual experience; not to the experience of the hypnotic clinic alone, but to all forms of self-suggestion which are practically found to remove and soothe the pains and weariness of large masses of common men. Apart from recognised forms of hypnotic suggestion by others, two popular forms of self-suggestion may here stand, for brevity's sake, as types of the rest. Each of these appeals to deep-seated faiths; each is preached in a crude extravagant form from which the man of science turns with repulsion.

The healing fountain of Lourdes draws its prestige from the ancient belief in beings higher than man who concern themselves with even the details of man's physical welfare. So-called Christian Science—or mind-cure, to use its less presumptuous name—while still relying on Christian revelation, appeals most directly to the optimistic instinct in man, maintaining that pain and evil are non-existent shadows, and that the only realities are love and joy. The conception of the miracles of Lourdes is too mediæval, perhaps, to maintain itself for long. The vaguer impulse which prompts to mind-cure may prove more capable of adaptation to whatever may hereafter be learnt as to the true relation between man's central will and his bodily organism.

Questions like these as to the relative efficacy of various self-suggestions belong indeed to the inmost meaning of hypnotism; but they lie beyond the facts with which the physician needs at once to deal. What is now to be desired is that medical science should recognise that a new task has opened before her; that these hypnotic artifices, empirical as they may be, do yet lie in the true path of therapeutic progress; that hypnotism is no more a trick than education is a trick; but that just as education develops observation and memory, through alert attention, so hypnotism develops organic concentration and recuperation, through their adjuvant phase of sleep.

Finally, if beneath the fanaticism and the extravagance of men blindly seeking relief from pain some glimmering truth makes way, that truth also it must be for science to adopt and to utilise, to clarify and to interpret. By one method or other—and her familiar method of widespread cautious experiment should surely be the best—science must subject to her own deliberate purposes that intelligent vital control, that reserve of energy, which lies beneath the conscious threshold, and works obscurely for the evolution of man.

III.—DAVID YELLOWLEES, LL.D., M.D.,

Physician Superintendent, Glasgow Royal Asylum, Gartnavel, Glasgow.

THE USES AND DANGERS OF HYPNOTISM.

I must express the highest appreciation alike of the striking and forcible address of Dr. Bramwell and of the highly philosophic and admirable paper of Mr. Myers. I believe the popular impression that some mysterious power resides in the operator is unfounded, and that the President, for instance, could exert the same influence as Dr. Bramwell over a hypnotised patient. The essential thing is the abnormal condition induced in a patient. My own experience enables me to believe even in the extraordinary cures related by Dr. Bramwell. I have obtained some remarkable results from my own endeavours. On the whole, however, these have been disappointing, chiefly because I have failed to hypnotise the very patients whom I could most have benefited thereby. I have not persisted in my attempts more than six or seven times, and the attempts have been chiefly among the insane, hence perhaps the very partial success. I cannot regard hypnotism as free from danger, nor think it possible that we could often reach and awaken "the subliminal strata of mind" without the risk of injury. I therefore utterly condemn the use of hypnotism for mere experiment or amusement. As to crime in relation to hypnotism, it may be true, as Dr. Bramwell has said, that a hypnotised patient would revolt from any manifest or gross crime, but might she not be told to sign a document, and very great evil thus be wrought?

IV.—JOHN F. WOODS, M.D.,

Medical Superintendent, Hoxton House Asylum, London, N.

HYPNOTISM IN ORGANIC DISEASE.

Having now treated over 1,000 cases of disease by so-called suggestion, I have, like Dr. Bramwell, come to the conclusion that it is a potent remedy, and that much good may be done by it if rightly employed. It is commonly thought that only functional nervous disorders, such as neuralgia, are benefited by this treatment. This is not my experience, and my chief object in speaking is to draw attention to the fact that it is applicable to a much wider field of disease. Let us not forget that the nervous system is implicated in almost all disease, and in so far as we can influence it for good we can benefit the disease. Take the case of organic heart mischief. At first sight it may appear irrational—I had almost said ridiculous—to treat it by suggestion. We must remember, however, that in all cases of serious organic disease of the heart there is a strong nervous element; the patient is apt to be agitated, perhaps he sleeps badly, or there may be pain. Now, if we can soothe the nervous system, secure sleep, and remove pain—we can do all this—we are going a long way to improve the patient's condition. Moreover, in addition to these general effects we can produce a direct and specific effect upon the heart by placing one hand upon the epigastrium and suggesting that the heart shall quiet down, and beat more slowly and

calmly. Again, let us take such a disease as rheumatic fever. It might, perhaps, be thought that this was the very last disease to be benefited by suggestion, and yet, when we come to think of it, we shall find that the nervous symptoms in it are very pronounced; they are evidently produced by the toxins of the disease. Now, although we are powerless to touch the root of the disease by suggestion, we may do much to alleviate symptoms; by removing pain and mental distress, quieting restlessness, and securing sleep. We may even lower the temperature. This I have succeeded in doing not only in rheumatic fever, but in pleurisy, pneumonia, and typhoid. Nothing could more eloquently show the value of suggestive treatment in diseases other than the purely nervous. I will instance just one other organic disease that can be benefited by this treatment—*tabes dorsalis*—a very unlikely malady, one might think, to be improved by it. In a patient now under observation I can always remove the girdle and lightning pains, and improve his appetite, spirits, and sleep. As the result of this treatment, he can now walk much more steadily than before, and considers himself a great deal better than when he first came under observation. One word as to the necessity of sending the patient to sleep. This is, in my experience, by no means essential. I have cured many cases without it; we may often succeed in getting the patient into a receptive condition by making him relax all his muscles to the utmost, and composing himself to complete mental and bodily inactivity. I do not agree with Mr. Myers that hypnotic phenomena cannot be explained on physiological lines. Mr. Myers would appear to assume that the subliminal processes to which he refers are purely psychic. I contend that they must have a physiological basis. Time does not permit me to say more than this concerning Mr. Myers's valuable and interesting remarks.

V.—CHAS. A. MERCIER, M.B., F.R.C.S. Eng.,

Lecturer on Neurology and Insanity, Westminster Hospital Medical School.

SUGGESTION AND CRIME.

I desire to point out that the cases of suggested crime instanced by Dr. Bramwell are entirely beside the question. He has instanced cases in which it was suggested to a patient that the patient should commit a crime as a crime; but this is not the point. The point is, and the fear is, that a patient may be made to commit a crime which has been suggested to him as a purely innocent act. The common exhibition of the itinerant mesmerists is to make a patient eat a tallow candle on the suggestion that he is eating a stick of celery, or to drink soap and water under the suggestion that he is drinking beer. Why, then, might not a butcher cut the throat of a child under the suggestion that he is cutting the throat of a sheep? Why should he not be made to poleaxe a man under the suggestion that he is poleaxing a bullock? Or, to put a more probable and more practical case, why should not a man be induced to sign an important document under the suggestion that he was signing something of a totally different character, and of no importance?

II.

NOTE ON "FISHER'S GHOST."

BY H. ARTHUR SMITH.

I think I am right in saying that in the general experience of psychical investigators, the more dramatic and interesting a ghost story is, the less likelihood there is of its being well authenticated. "Fisher's Ghost" is, however, certainly an exception to this rule. It has been as carefully examined as any event so distant in time well can be, and this by enquirers who have brought to the task special and varied qualifications. It is probably unique, in that it in some degree, at least, connects the question of ghostly phantasms with the severe principles of evidence which regulate the proceedings of a Court of Law.¹ Many years ago De Morgan, though he had probably never heard of this case, conceived and considered the possibility of some such association. In his introduction to "*From Matter to Spirit*," he wrote: "If the ghost of a murdered man were to make his appearance in court in a form which no one could possibly attribute either to imposture, optics, or chemistry, and were solemnly to declare that the prisoner was not the murderer, and then to vanish through the roof, the judge would, no doubt, instruct the jury that they must dismiss the respectable apparition from their minds altogether; that even if the spirit had offered to be sworn and to stand cross-examination, there would be very grave doubt whether his evidence could be received, from his probable want of belief in a future state; but that as matters stood, it was clearly their duty to take the vision *pro non viso*. To which the jury would reply, if they believed the ghost, by a verdict of not guilty." It is true that in Fisher's case the ghost was considerate enough not to embarrass the court by any such entry as Mr. De Morgan supposes, but it adopted a no less effective way of working its ends, the difference being that in this case the ghost was on the side of the prosecution.

The story, which was investigated a year or two ago by Mr. Andrew Lang, and fully related by him in *Blackwood's Magazine* for July, 1897, has quite recently been minutely examined from a lawyer's point of view by Mr. G. B. Barton, the official historian of New South Wales, who has of course had special opportunities of testing it by reference to the records of the colony. The bare facts, as now stated by Mr. Barton, may be very briefly summarised.

In 1826, a ticket-of-leave holder, Frederick Fisher by name, was farming a tract of land at Campbell-town, near Sydney, with the assistance of one George Worrell, who acted as overseer, and was thoroughly conversant with all Fisher's concerns. On or about the 17th of June, Fisher suddenly

¹ Since writing this sentence I have observed that Mr. Andrew Lang, in the paper referred to below, gives some analogous cases from the English Courts.

disappeared, and in answer to enquiries as to what had become of him, Worrell gave it out that he had taken passage in the *Lord St. Vincent* and gone to England. Some of the neighbours were rather incredulous as to this, inasmuch as nothing in Fisher's proceedings had pointed to any such sudden removal, and this incredulity was by no means decreased when it was observed that Worrell was taking steps to sell off some of Fisher's horses and farm stock. When asked for his authority for this procedure, Worrell produced a paper purporting to be a receipt signed by Fisher for the purchase money of the horses; but the signature was, to say the least, doubtful, and the sale was not effected. Other features in Worrell's conduct fomented the suspicion that there had been some foul play in the business, and when three months had passed by without any news of Fisher, the police authorities were moved to offer a reward of £20 for the discovery of his body, or of £5 for proof that he had quitted the colony. This naturally drew attention again to the case, and set many men to work, searching the bush in every direction; but a month elapsed without any discovery. At last, however, a settler named Farley created a sensation by alleging that when driving home at night from Campbell-town and passing the corner of a paddock that had belonged to Fisher, he suddenly saw the figure of a man, to all appearance Fisher himself, seated on the top of the fence. Taking it at first to be Fisher, he went up to speak to him, but as he approached, the figure slowly rose from the fence, raised its right arm in the direction of a small creek, and gradually disappeared, apparently following the windings of the channel. This was at once reported to the police and the magistrates, who eventually determined to have a search made on the spot. On the last day of October two constables, taking with them a native tracker named Gilbert, proceeded to the spot. They at once found appearances as of blood stains on the fence indicated by Farley, and, with confidence increased by this discovery, proceeded to the creek. On coming to a pool of water covered with scum, Gilbert took a corn stalk from the neighbouring field, passed it over the scum, put it to his nose and said "White man's fat." Then he led them up the creek about 40 rods to some swampy ground, took the ramrod from his gun, drove it into the ground, drew it out, smelt it, and again said "White man's fat, dig there." Spades were procured, and in a few minutes the body of a man was discovered. The features were not recognisable, but on getting one of the hands clear, constable Newland¹ said "That's the hand of Frederick Fisher; I will swear to it." He also recognised the body by its general appearance and by the clothes.

Of course arrangements were at once made for an inquest. Medical evidence shewed that the skull had been fractured in several places. The identification of the body as Fisher's was complete; but though the suspicion against Worrell was keener than ever, the verdict was an open one—"wilful murder against some person or persons unknown." Worrell was, however, arrested, and when before the magistrates made a declaration in which he accused four men who were in his kitchen on June 17th, as the guilty persons. This story, inconsistent as it was with his previous statements, made little impression, and he was committed for trial at the Criminal Sessions at Sydney.

¹ Mr. Lang gives this witness's name as Leonard.

The trial took place on February 2nd, 1827, before Chief Justice Forbes and a military jury of six officers, the prosecution being conducted by Acting-Attorney-General Moore, and the defence by an attorney named Rowe. Farley was not called as a witness, he having nothing to depose which the rules of evidence could admit. Worrell's conduct, however, was so unsatisfactorily explained that the jury had no difficulty in convicting. The verdict was "guilty" and the death sentence pronounced.

The prisoner was attended by the Rev. W. Cowper, to whom he made a qualified confession, denying his declaration as to the four men whom he had accused, but representing that he had accidentally struck and killed Fisher while aiming a blow at a horse which he saw straying among the wheat crop. He was, however executed on a scaffold in front of the old gaol off Lower George Street in the presence of an enormous crowd.

Such, in outline, are the facts as narrated by Mr. Barton after examination of the court and official records; and they agree in every important point with the story as it was first told in R. Montgomery Martin's *History of the British Colonies* published in London in 1835; and a little later in *Tegg's Magazine* published in Sydney, under the title of "Fisher's Ghost, a Legend of Campbell-Town." It appears also in Rusden's *History of Australia* (1883) and with more or less intentional embellishment in *Household Words* for 1859.

A friend of my own who was Secretary of the Navigation and Pilotage Boards in Sydney in 1852, has recently given me his recollections of the story as it was told him by his father-in-law, who was present at the trial and well conversant with the facts. His version again is in substantial agreement, but there are certain variations of detail which it may not be uninteresting to indicate. In the first place his impression was that the locality of the murder was near Parramatta, on the opposite side of Sydney from Campbell-town. In this he is doubtless wrong. Again in his version he represents Worrell as having despatched to England a letter addressed to himself and signed in Fisher's name, in which letter Fisher is made to say that, having come into a good fortune in England, he does not intend to return to Australia, and that for old acquaintance sake he makes Worrell a present of the farm and stock. This letter was to be posted in England so as to confirm Worrell's alleged title. There is nothing in Mr. Barton's narrative to contradict this, but obviously such a letter, if written, could have had no effect whatever on the case, inasmuch as the interval between the murder and the trial was not sufficient for it to have been despatched and returned. If it happened at all it could only have come to light afterwards. My friend's recollection is quite distinct as to the "White man's fat" expression. In fact this is a point which would be little likely to escape any memory. The identification of the body he ascribes partly to the peculiar "basket" pattern on the buttons, and partly to the discovery in the pockets of a large comprehensive sort of knife with corkscrew and other attachments, which Fisher was known to have recently received from England. My friend is under the impression that the prosecution was conducted by Mr. Alfred Stephen. This is not inconsistent with Mr. Barton's story, as Mr. Stephen must have been a young man at the time, and may well have held a junior brief with the Attorney-General.

The most important deviation, however, concerns the confession. According to his account, a petition for a reprieve was got up by some people who were convinced of Worrell's innocence, and presented to Governor Darling; and the Governor, strongly impressed with the peculiar circumstances which led up to the conviction, determining at the last moment to stay the execution, despatched an orderly to the gaol for the purpose. Before his arrival, however, Worrell had made and signed not a qualified but a complete confession, giving details of the murderous assault which explained some of appearances on the spot. On this confession being at once reported to the Governor, the reprieve was withdrawn and the execution proceeded with. One can scarcely hesitate in choosing between this and the narrative given by Mr. Barton, whose sources of information are much more trustworthy. It is just one of those dramatic embellishments which are likely to find their way in course of time into any story which has excited strong public interest.

On the whole the various accounts of the event which have appeared are singularly free from decorative additions, and though of course there is room for sceptical criticism as to the real origin of Farley's story, there is a *vraisemblance* therein which has been admitted by every narrator. One would like to know whether Farley received the £20 reward offered by the police, for one cannot help suspecting that he may have known the details of the murder from the beginning, and that on hearing of the reward he ingeniously invented the ghost to explain the four months' delay in making the disclosure. If so, being on the spot, he would have little difficulty in assisting the tracker to identify the locality of the grave. Mr. Lang considers this hypothesis, but rejects it as an improbable explanation of Farley's conduct. But is the apparition less improbable?

NOTE ON A CASE IN "PHANTASMS OF THE LIVING."

The Editor of these *Proceedings* is of course not responsible for statements appearing in any other book; but by the wish of the two surviving part-authors of *Phantasms of the Living*, and considering the close connection between that book and the S.P.R., under the auspices of whose Council it was published, we think it right to mention here that one of the cases in the "Additional Chapter," Vol. II., p. 671, must now be withdrawn. There is no reason to doubt the *bona fides* of Mr. Sparks, the principal informant; but Mr. Cleave, then 18 years of age, whose evidence is essential to the case, has admitted that the alleged apparition of himself, when entranced, to a young lady in London, was a hoax.

III.

REVIEWS.

Some cases recorded in the "Annales des Sciences Psychiques."

Professor Grasset published in the *Annales des Sciences Psychiques* for Nov.-Dec., 1897, and in the *Semaine Médicale* of Dec. 1st, 1897, an account of an experiment made by him in Oct. 1897, with a subject of Dr. Ferroul's of Narbonne, in reading through opaque bodies. Reports of this experiment have appeared in most of the chief English and American newspapers, and as these reports were in the main accurate it will be unnecessary here to enter into details. Suffice it to say that the subject read the contents of a sealed envelope sent by Professor Grasset to Dr. Ferroul with substantial accuracy, and Professor Grasset was satisfied that the envelope had not been tampered with in any way. Sufficient precautions had been taken to render it impossible to see the writing through the envelope, and the words written were known only to Professor Grasset and not to Dr. Ferroul.

I propose to comment on two points only in the record of this experiment, and then to give a brief account of a second experiment undertaken by a Committee appointed by the Académie des Sciences et Lettres of Montpellier at the request of Professor Grasset.

(1.) Dr. Ferroul writes to Professor Grasset as follows:—

"When your packet arrived here this morning my subject was not at hand . . . Having to go my rounds I decided to arrange for my subject to come to my house about 4 o'clock, and I went to her house to make the appointment. Having learnt what I wanted she proposed to read the contents of the envelope immediately."

Now in view of the evidence of the committee of investigation which will be given later, it would be of interest to know whether Dr. Ferroul proceeded directly to the subject's house 300 mètres distant, after leaving the envelope on his study table, or whether any considerable interval of time elapsed—say 20 minutes or half-an-hour—before he went there; and also whether the subject was at home on Dr. Ferroul's arrival at her house.

(2.) Professor Grasset opened the envelope before a meeting of the Académie des Sciences et Lettres of Montpellier, and the members present did not find any trace of the borders of the envelope having been tampered with, after examining the inside of it.

Professor Richet has had the goodness to send two photographs of the envelope, and another of the sheet of note paper which was enclosed in it.

Too much importance must not be attached to criticisms founded on a photograph, the opinion of the members of the Academy who saw the original being naturally entitled to more weight. But with this proviso, the following observations may be of some value.

A safety pin which had been inserted in the envelope, and which Professor Grasset himself states had only caught some silver paper wrapped

round the document, did not pierce the document itself, so that if the envelope had been opened, the document could have been removed without causing a tear.

The appearance of the inside of the envelope strongly suggests that the flap most remote from the safety pin was loosened (by steaming or some similar method) and withdrawn from under the seal and afterwards replaced. There are three separate indications of this at least.

- (a) A torn hole in that flap of the envelope through which the greater part of the safety pin ran.
- (b) A considerable tear in the (supposed) removed flap.
- (c) A crumpled margin of one of the side flaps, perhaps caused by dragging on replacement.

Also the margins of two of the flaps have darkened aspects, which somewhat suggests that fresh gum may have been used to keep the removed flap in place. The supposed removed flap also looks as if it might be dirtier, and there appear to be indications that fresh gum has diffused itself beyond the edges where two of the flaps join.

So much for the first experiment.

The report of the committee appointed by the Académie des Sciences et Lettres of Montpellier, appeared in the *Semaine Médicale*, pp. 18-20, 1898, and in the January-February number of the *Annales des Sciences Psychiques*, 1898. It is admirably written, and with much detail, and is signed by all the members of the Committee, including Professor Grasset himself. I attempt no more than to give the salient points. Three envelopes were prepared by the Committee before leaving Montpellier; but as two only were used in the experiments we may dismiss the third from our minds. Most elaborate precautions were adopted in the matter of sealing, gumming and wrapping, and the documents placed inside the envelopes were so selected from many others that their contents were unknown to any of the Committee or in fact to anybody, a condition which is strongly criticised by Dr. Dariex in the *Annales* in his comments on the report. But it should be borne in mind that the subject claimed not the faculty of telepathy but of reading through opaque bodies. Arrived at Narbonne, the committee proceeded to Dr. Ferroul's house. Contrary to their wish, Dr. Ferroul insisted that the first experiment at least should be made under the conditions already familiar to the subject; namely, that the envelope should be placed on the writing table in his study, and that no one should be present in the room, not even if concealed; as the subject found the presence of any body in the room, where the envelope was, disconcerting. Prepared for some such difficulty, the Committee had placed one of the envelopes in a box filled with shavings and wrapped in stout paper; and the additional precaution had been taken of inserting a sensitive plate inside the envelope, so that if the envelope were opened the plate would be affected by the exposure.

The box was placed on the writing table. Dr. Ferroul then proposed to seal and lock the door of his study. The Committee saw no use in sealing the door, as they had no private seal with them: and on search being made for the key it could not be found—a significant fact. A start was then made for the subject's house, which, although but 300 mètres distant, it took 7 or 8 minutes to reach. At the moment of their arrival the subject was out.

She arrived about 3 minutes afterwards and was at once hypnotised by Dr. Ferroul. No mention is made of any means having been taken to test the genuineness of the trance.

The trance began about 2.5. The subject almost at once stated that the box contained shavings.

At 2.15 she was awakened.

At 2.25 her sister, who, though present on the arrival of the Committee at 2 o'clock, had left the house almost immediately, re-entered the room.

At 2.27 the subject was re-hypnotised, and at 2.35 stated that the box was made of wood.

At 2.40 a servant who had gone out just as the Committee arrived, re-entered the house.

During the next hour the subject, who was awakened and re-hypnotised several times, had ample opportunities of conversing with her sister away from the observation of the Committee.

About 3.30 the sister left the house taking with her some paint brushes, saying that she was going to do some photo-miniature work. A few moments before she had had in her hand a small bottle containing a transparent liquid of a yellowish tinge.

At 3.40 the subject, when in trance, described the contents of the box in disconnected sentences. She mentioned black sealing-wax (correct), a large paper—shavings—glass (*i.e.* the sensitive plate)—green paper . . . “the letter ‘f’—I think—an ‘r’ a ‘g’ or an ‘f’—and something else—I’m not sure if there aren’t numbers—then a ‘4’, I think.”

The document inside the envelope really bore the following inscription. At the top, the letters “a d f” on one line—on the line below, the letters “g r.” At the bottom are placed the numbers 8, 7, 4, upside down.

At 3.50 the subject was awakened and wished to walk alone to Dr. Ferroul’s house. The Committee followed and entered the study at the same time.

Immediately on entering the room two members of the Committee independently noticed that the box was not in exactly the same position as it had been left in, and that the seals on the top were no longer intact.

The subject tried to tear the envelope—presumably with the purpose of destroying all traces of injury to the seals etc., but the envelope was quickly recovered, before she had time to do it more than a trifling injury, which was carefully noted.

At 4.15 an experiment was made with the second envelope, which was held all the time by one of the Committee. The subject made three absolutely unsuccessful attempts to read the contents. The third attempt was distinctly amusing as it opened with a repetition of the words written in Professor Grasset’s envelope—“The deep heavens reflect in stars our tears : then they said to themselves as they wrote it—What does it matter, since she has read it at a distance once.”

The real contents of the envelope were a Knave of Clubs with the word “amour” written on one side of it, and a plain white card with the number “24” in one corner, and these words : “It is a square courtyard with nothing peculiar about it.”

The young lady then had an attack of nerves, which brought the séance to an abrupt conclusion. Dr. Ferroul and the subject explained the failure by the sensitive plate which acted on the subject, who at such moments is stored with electricity, as an insulator.

The Committee conclude their report as follows :—

“ We made two experiments with Dr. Ferroul’s subject.

In one, the envelope remained in the possession of one of us all the time, and the subject’s answers were completely wide of the mark.

In the other, if the subject succeeded in indicating more or less correctly, though not fully, the contents of the box, it is essential to a proper appreciation of the value of this experiment, to collate from the evidence given above with regard to the condition of the box and its contents the following facts :—

(1) The box had to be left unwatched.

(2) The subject was absent from her house at the moment of our arriving here after having relinquished the box.

(3) The details relating to the contents of the box were furnished only after an interval of 1 hour 40 minutes, and after the subject had communicated several times with her sister, to whose frequent comings and goings we have already drawn attention.

This summary will enable anyone to draw his own inferences and to find an explanation both simple and natural of the more than surprising keenness of vision of the subject whom we examined in deference to the request of the Academy.”

I need only add that the seals on one side of the box were impaired, the paper-cover of the box was torn and had holes in it, and fresh gum had been used to refasten it, one of the seals had been bodily removed and stuck back again, and the sensitive plate showed unmistakably signs of having been exposed to the light.

Dr. Dariex, the editor of the *Annales*, pleads for a suspense of judgment on various grounds, chiefly on the ground that Professor Grasset’s first experiment remains unaffected by the evidence of the committee. It would be hard, however, to believe that anyone will be found inclined to waste time on further experiments with Dr. Ferroul’s subject of the “ X rays eyes,” as the *New York Herald* called her, at least on experiments of this nature, though if the evidence of some other experiments made with her in 1894 can be relied upon, there is reason to suppose that she is not always fraudulent, but occasionally clairvoyante. (v. *Annales*, May-June, July-August, 1896.)

I now proceed to consider two articles on table-turning phenomena by M. Rouillon, Professor of German at Périgueux College, which appeared in the *Annales des Sciences Psychiques* for Sept.-Oct., 1897, and Jan.-Feb., 1898, respectively. I give the séances in their chronological order, thus slightly altering M. Rouillon’s arrangement, and I discuss only those sittings at which two gentlemen, MM. Etienne Vidal and Loze, were present, either alone or with others.

Séance on the 28th November, 1893, in M. Loze’s room at Limoges College.

Translation.

“Seated at the table, MM. Vidal père—Etienne and Marcel Vidal, his sons, Loze and Rouillon. We make a chain with our hands on the edge of the table.

At the end of 35 minutes movements begin.

I ask for the name [of the Saint] on the almanac under the date of the 12th June. *None of us know it*, and the same remark applies to all our séances. The almanac is on a writing table at three paces from us, resting against the lamp (the only light in the room) and so serves us as a screen. This almanac has six months on each side. When the question was put, the side containing the first six months of the year was turned towards the light of the lamp; none of us can see it. The answer is ‘Trinité,’ we proceed to verify, and this is found to be correct.

I leave the circle. The other four keep one hand only on the table, without forming a chain.

Question. ‘The name under the date of the 2nd January.’ *Answer.* ‘Basile.’ Correct.

Question. The name given for the 2nd September? *Answer.* ‘Firmin.’

I proceed to verify the answer. The month of September is on the side of the almanac placed in the shade. The answer is wrong, but M. Loze observes that Firmin does come in the month of September, that the word is written in large characters, and that there is a 2 in the date [of S. Firmin], the 25th. The almanac is put back in its place, with the second six months towards the light.

M. Vidal asks for the name under the date of the 5th December.

Answer. Sabas.

This name, absolutely unknown to any of us, strikes us all with astonishment, and we regard the result as negative, as simply a fortuitous combination of letters. A verification is made, and Sabas is given under the date in question.

At the conclusion of the séance, we all signed a record vouching for the reality of these occurrences.”

The presence of the almanac in the room to my mind deprives the results obtained of all value, as it is practically impossible to decide with certainty what is or is not within the range of vision of five different persons, especially as the 35 minutes' concentration of mind before the table began to move may well have produced hyperæsthesia in the visual organs of any or all of the experimenters. It would have seemed the obvious precaution not to have had an almanac at all in the room. The answers could have been written down and verified afterwards. The remark that the almanac was on a writing-table at “three paces from us” is very loose evidence, it being clear that the writing-table could not be equidistant from the five different persons sitting at the table. Out of the four questions asked, the name of the questioner is given in two instances, once M. Rouillon, once M. Vidal, in the two remaining instances the name of the questioner is not given. This is a point of some importance. M. Rouillon does not state whether these four dates were the only dates asked, and it is possible that he has recorded the successes or partial successes only.

M. Rouillon does not say what position relatively to the almanac he took up after leaving the circle at the table; nor does he say, except in one instance, who verified the answer by referring to the almanac. The phrases employed are "nous vérifions," "on vérifie," once "je vérifie," and in one instance the verification is not expressly mentioned, but left to be inferred. And even when M. Rouillon says "je vérifie," it does not amount to much, for the expression does not exclude the co-operation of the other experimenters. M. Rouillon is looking to see if the almanac gives Firmin as the saint for September 2nd. It does not; whereupon M. Loze draws attention to the fact that S. Firmin's day does occur in the month of September, but on the 25th, instead of on the 2nd. Now we gather from M. Rouillon's repeated assertions that neither M. Loze nor any other person knew the dates of the various Saints' days. It follows then that although M. Rouillon is said to have verified the reference, still M. Loze or M. Vidal may have been examining the almanac as well at the same time. He only once mentions the act of replacing the almanac after verification, and then uses a charmingly vague French expression "on replace." Obviously the person who handled the almanac would have an opportunity of consulting it either consciously or sub-consciously, and would then perhaps choose the next or some subsequent date for the table to answer. And even in those instances when one experimenter is stated to have replaced the almanac, and another experimenter to have put the subsequent question, there is no reason against supposing, for any evidence to the contrary, that the person who replaced the almanac may not have suggested a date to the next or some subsequent questioner.

I now pass on to the second séance, held in M. Loze's room on October 30th, 1893, two days later.

Translation.

"At the table, MM. Etienne Vidal, Loze, Martin, licentiate in mathematics and science, and tutor at Limoges College, and Duris, tutor.

Same conditions as before, no one either in the room or at the table knew the names asked for. The following questions were put by M. Vidal, who appears to have the greatest influence on the table.

<i>The name for</i>	Jan. 14.	<i>Answer.</i>	Hilaire. Correct.
"	" Feb. 21.	"	Pépin. Correct.
"	" Jan. 28.	"	Charlemagne. Correct.
"	" Dec. 27.	"	Innocents. (Error of one day, Innocents' Days falling on December 28th.)
"	" May. 16.	<i>Answer.</i>	Cyriaque. (Error of one month, Cyriaque being June 16th.)
"	" May. 26.	<i>Answer.</i>	Clet. (Error of one month, Clec being April 26th.)

A more curious thing still. Abdon was given us for July 30th. This name does not occur on the almanac that we used, neither on the date named, nor at any other date. In the evening being in the bursar's office I consulted an almanac hanging on the wall and found under the date of July 30th the name 'Abdon.' *The same thing has happened several times in the course of my experiments with my two children alone.* We have sometimes had to consult

a dictionary to find the name indicated by the table (the name 'Cunégonde' for instance) or another almanac (for the name 'Rose.') The name dictated by the table always corresponded perfectly with the date named by us."

My observations on the first séance apply equally to this second séance. The conditions we are told were the same. Are we to understand that the almanac was still three paces from the persons sitting at the table? If so, it follows that the table was in *precisely* the same position as two days before. The errors of one day and in two instances of exactly one month do not seem to have struck M. Rouillon as suspicious. They can hardly fail to strike others so. The fact that the name "Abdon" had to be sought for in another almanac in the bursary, and other names in a dictionary, are also accepted by M. Rouillon without misgiving.

It will be noticed that other persons besides the actual experimenters were present in the room.

It is difficult to accept M. Rouillon's assurance that the date, or at least the approximate date, of Holy Innocents' day was unknown to any person present, particularly in a Roman Catholic country. Five out of ten persons of education would be aware that Innocents' day falls soon after Christmas and before the beginning of January.

As in the two remaining séances at which MM. Vidal and Loze assisted no further experiments were made with the almanac, it is only fair to state now that M. Rouillon had obtained similar successes with other persons at the table, but the success was intermittent and except in the case of one séance, at which he and his ten year old daughter alone were present, the proportion of failures to successes is not given. In this latter séance, out of 30 dates 24 saints were correctly named by the table; but this séance was held on January 3rd, 1894, and M. Rouillon had been engaged with his daughters and others in these almanac experiments off and on ever since September 1893. In three or four months he ought to have become an expert in Saints' Days, and at the least he ought not to expect his readers to believe that the saints corresponding to the dates that were chosen were absolutely unknown to him or others present in every instance after the game had been played so often.

I now pass on to the most remarkable of the series of table-turning séances, which is described as follows in *Annales des Sciences Psychiques*, Jan-Febr, 1898, pp. 1-5.

Translation.

A CASE FROM LIMOGES. BY M. ROUILLON.

An account is here given, drawn from careful notes and circumstantial reminiscences, of two experiences, which, considering the excellent conditions under which they took place, constitute a contribution of great value (as I venture to believe) to psychical research. The two experimenters have been personally known to me for several years; their sincerity and critical habit of mind offer an unimpeachable guarantee of their accuracy.

Identification of a name with a dead person unknown to the experimenters, obtained by direct writing.

On Christmas evening 1893 M. Loze, bursary clerk at Limoges College, had invited his friend M. Etienne Vidal to come to his rooms to take a glass

of white wine. It was about 10 o'clock. There was a good wood fire burning in the grate, in front of which stood a small round table with a bottle and a plate upon it. M. Vidal had his forearm, and M. Loze his elbow, on the top of the small table. The conversation was in no way connected with the subject of psychical experiments. All of a sudden M. Vidal felt the table rise once, then twice, and so forcibly that there was only just time to clear the tray. "Hullo," said M. Vidal, "one would think the table wants to talk. Let's wait a minute." They placed their hands on the table and asked,

"Who is there?" "Demi-Siphon."

"Dead or alive?" ("Mort ou vivant?") "Dead." ("Morte.")

"Ah, it's a woman then! What were you?" "A dancer."

"Where?" "Moulin Rouge."

"What did you die of?" "An accident."

"What accident?" "Rupture of the perinæum—in doing 'a split.'"

"Will you provide an interesting séance for our benefit?"

Thereupon followed some extremely curious phenomena, but of a nature which cannot be recorded here; and afterwards the table spelt out "Slates."

Some time before this M. Loze had procured two slates in thick wooden frames, which he had fixed together by means of two brass screws.¹ The screws were unfastened and the slates separated. M. Loze washed them with a wet sponge and put them to dry by the fire in a good light. Then he went to look in a drawer for a piece of pencil, and placed it between the two slates. M. Vidal, after examining the slates again on both sides, in front of the lamp and in the presence of M. Loze, who made a simultaneous and equally careful examination, laid them one against the other and screwed them together. They were then placed flat on the table away from all contact with the experimenters' hands. The table seemed to rap impatiently. On being asked what it wanted, the reply came: "Remove the lamp." M. Loze carried the lamp into an alcove about two paces away, whence it still threw some light, while at the same time the glow from the fire was fairly bright. It would have been quite easy to read the time by a watch. All this time M. Loze did not lose the slates, which were resting on the table, from sight, nor did M. Vidal touch them.²

"Write something, and when the writing is finished, rap once."

The table remained motionless for a moment, then gave two raps signifying, according to the usual code, "No."

"There is something not quite right?" "Yes."

"What?" "Hands."

"Do you want our hands on the slates?" "Yes."

This was done,³ and after a moment the table rapped once.

"Is the writing finished?" "Yes."

¹ These were stout screws, at least four centimètres long.

² This mutual supervision on the part of both experimenters was the method of procedure adopted throughout.

³ It must be understood that during these experiments the slates rested on the table, and were held (*maintenus*) by both experimenters simultaneously in an amply good enough light. Each time that they were opened, it was in a strong light and under the closest supervision of both operators. (Note by M. VIDAL.)

The lamp was then brought close, and on the slates being unscrewed by M. Vidal, extremely distinct tracings were discovered on one of them resembling figures of 8 interlaced. Encouraged by this result, M. Vidal, without rubbing anything out, screwed up the slates again, and put them back on the table.

The lamp was again removed, and a request was made for something more definite. The table remained stationary, but the movement of the pencil was heard, and one rap was given. By the light of the lamp M. Vidal unscrewed the slates and on one of them some characters were seen which at first sight seemed to have no meaning. On questioning the table it replied: "reversed." M. Vidal thereupon made out an L, and then M. Loze a flourish, which he occasionally uses as a signature. With his eyes away from the slate the latter took a sheet of thin paper, on which he traced in his ordinary style the flourish in question; and on placing it, with the back side towards himself, against the lamp, he stated that the strokes which he had just made were practically identical with those on the slate.

The slate was held before a glass, and M. Loze's signature was perfectly recognisable.

Without effacing anything the slates were again screwed together and the table was requested to be good enough to write in the ordinary way. The noise of the pencil was noticed, and one rap was made. The slates were opened by M. Loze, and on one of them was found written in most satisfactorily clear characters "Demi-Siphon." The slates were closed again by M. Loze.

"By one single word, applicable strictly and solely to yourself, indicate the kind of life that you led." The noise of the pencil was once more heard, and once more there was a rap. The slates were unscrewed, and on one of them was found very legibly written the word "Vadrouille." The writing was remarkably clear and firm.¹

Some more questions were put to the table, which all of a sudden came to a standstill, and did not budge again.

Neither M. Vidal nor M. Loze had ever heard of Demi-Siphon, and they were puzzled both by the name and the details connected with it, of which they knew absolutely nothing.

The next day, when talking of their experience before several colleagues, they learnt that a few days before, *Le Temps* had announced the death of a dancer at the Moulin Rouge who went by the nickname of Demi-Siphon. M. Vidal told me at the time that he did not know whether the cause of death (rupture of the perinæum) as indicated by the table was correct. But I feel sure I remember that this point was corroborated by inquiries made at the time. During the whole evening M. Vidal and M. Loze were alone in the room where the séance took place, and also at the following sitting. By way of invitation to a fresh séance, the intelligent force was requested to be so good as to give a palpable manifestation.

"Cross." An explanation was asked for.

¹ It will be observed that in these various experiments MM. Vidal and Loze took it in turn to open and shut the slates, one opening and the other shutting, and vice versa on the next occasion, without affecting the result in any way. (Note by M. VIDAL.)

“Wall,”—but nothing was to be seen.

Some days later, about half-past seven in the evening, M. Vidal was alone in his room, which no one else could have entered.¹ All of a sudden on turning round he saw on the glass a cross about 10 centimètres long, traced, as if by a finger, in ink. The ink was still wet. About three mètres from the glass, on a table, stood an inkpot containing ink identical with that in the drawing. Now M. Vidal had not left his room for half an hour, and some ten minutes or so previously he had just completed his toilet in front of the glass, in a strong light, before going to the theatre. It can be said then that this cross was produced in his presence.

About the same time M. Loze also had found several crosses in chalk in his room, and in various other places, but in this latter case the conditions were not sufficiently stringent (le manque de contrôle) to warrant us attaching the same importance to it. Still it seems likely, after taking into consideration all the circumstances, that these marks proceeded from the same cause as the cross in ink. I append the attestations of MM. Vidal and Loze, and can only add that these incidents were related to me by the two witnesses the very day after they took place (le lendemain même de leur constatation) with all the details as given above.

M. ROUILLON,

Professeur d'allemand au lycée de Périgueux.

Périgueux, *January 2nd, 1898.*

This account is scrupulously accurate.

E. VIDAL.

This report of the various experiences related (slates and cross) is in strict conformity with the truth.

L. LOZE.

In connection with this case Professor Charles Richet writes as follows in a letter addressed to Mr. F. W. H. Myers.

“I draw your attention to the paper by M. Rouillon in the *Annales des Sciences Psychiques*. The value of the experiences depends wholly on the worth and honourable character, and above all on the scientific abilities of the witnesses. M. Vidal, who was one of the witnesses and probably the medium, is a man of great intelligence. At the time of the experiences he was only 20 years old. Since then he has passed his medical examinations with great brilliancy, and is now a Doctor of Medicine. He worked for three years in my laboratory, and I consider that he possesses a scientific mind of an high order. The strange thing is that he should have had these mediumistic experiences during 6 weeks, and then never again.” M. Richet adds that he has no personal acquaintance with M. Loze.

It is much to be regretted that the report was not written by M. Vidal or by M. Loze, or by both in collaboration, as M. Rouillon's paper exhibits the usual deficiencies of second-hand evidence. Regrettable also is the interval

¹ The rooms of M. Vidal's family are separated from the rest of the college by a glass partition. Access is gained by a door which, from the outside, can be opened only by a latchkey. This door leads into a large lobby with rooms on either side. One of these rooms is M. E. Vidal's.

of four years between the date of the séances and the publication of the report. This interval of four years must detract substantially from the value and reliability of M. Rouillon's record. Mention is made of contemporaneous notes, by the aid of which the report published in the *Annales* was written. But although not directly admitted, it seems a fair inference that the report was not written by M. Rouillon until quite recently. Nor did M. Rouillon depend entirely upon written notes; he speaks of "des souvenirs précis." After a lapse of three or four years it is quite true that our memory of any event may be precise, or distinct or circumstantial, however we prefer to translate the word; in fact it not seldom happens that our recollections become more precise, even too precise, as time goes on; but it does not follow that because our recollections are distinct they are necessarily accurate. It would be interesting to know how far these contemporaneous notes preserve a record of the *conditions*, and how far the information about the conditions given in M. Rouillon's paper, incomplete though it is, has been drawn from the memory of the experimenters 3 or 4 years after the occurrences.

M. Rouillon makes a point of the fact that prior to the first and sudden appearance of Demi-Siphon the conversation was in no way whatever connected with matters psychical. But we know that both M. Vidal and M. Loze had been attending table-turning séances quite recently, and so it is not unfair to presuppose a state of mental expectancy. An accidental movement of M. Vidal's forearm or of M. Loze's elbow, both of which are recorded to have been resting on the table just before it began to move, may well have supplied the necessary stimulus to start the table on its wild career.

M. Loze had "some time before" procured two slates joined together by brass screws, presumably for experimenting at some séance which he had in view. Note the strange coincidence. M. Loze has "some time before" ("*depuis quelque temps*")—a vague expression like many others used by M. Rouillon—provided himself with two slates and screwed them together with brass screws. Then lo! and behold an obliging spirit named Demi-Siphon arrives in the most opportune manner and performs upon them.

It is a pity that M. Rouillon did not take the trouble to search the issues of *Le Temps* about Christmas 1893 to make *quite certain* whether the details there given of Demi-Siphon's death were identical with those given by the slate, or whether the table supplied any information which did not appear in the newspaper.

Also it would be most desirable to know whether *Le Temps* alone among French newspapers contained a reference to the death of Demi-Siphon, and whether MM. Vidal and Loze were in the habit of reading *Le Temps*, as were evidently some of their colleagues. For it is quite conceivable that, though the experimenters did not consciously acquire the information, it may have been acquired subconsciously by either or both of them.

One would like to know too where exactly the screws were placed in the slates, what was the size of the piece of pencil, and what was the size of the slates; especially the latter, because unless they were fairly large, the communications must have got a little mixed before the end of the séance, as we are told that nothing written on them was rubbed out.

Were the slates shown afterwards to any other persons, with the writing still on them? Before the slate-writing began, the table, or rather the influence that controlled it, impatiently demanded the removal of the lamp, but it is noticeable that, though the table insisted on the removal of the lamp at the first séance, yet one would infer that the material manifestation of the ink cross, which, by the way, could not be produced then and there at the second séance, occurred in a strong light, for the cross was on the glass before which M. Vidal had been dressing "*en pleine lumière.*"

The day after the first séance MM. Loze and Vidal spoke of their experiences before several colleagues. One may naturally suppose then that they spoke of the unexplained incident of the "Cross" "Wall," which occurred at the second séance, before other persons also. If this was the case, there arises a formidable suspicion of a practical joke. Neither the fact that M. Vidal did not see the cross on entering his room and when occupied with his toilet, nor that the ink was still wet, nor that the ink on the wall and the ink in M. Vidal's ink-pot were identical, exclude such an explanation; and no capable practical joker would be baffled by so slight an obstacle as a door which could be opened from the outside only by a latchkey.

And be it noted in this connection that it was not the door of M. Etienne Vidal's room that could only be opened by a latchkey, but merely the front door of the suite of rooms occupied by M. Vidal père and his family.

Again, to assume, as M. Rouillon does, that, because M. Vidal only noticed the ink cross after having been some time in his room, therefore the cross was actually inked on the wall in his presence, is to assume a very great deal too much.

Note also that M. Vidal did not see the cross in process of being drawn, only when completed.

It is difficult to understand whether the "Vadrouille" incident was recorded simply because of the production of clear and distinct writing when the slates were screwed together, or whether because the answer was considered particularly apposite to the question asked. If the latter, it may be remarked that this term might have been applied to many dancers at the Moulin Rouge without much risk of its proving inappropriate.

Lastly Professor Richet excites our curiosity by fixing on M. Vidal as "probably the medium," but, like the wise judge, does not give his reasons, which we should very much like to hear.

I come now to the last séance of the series. It is remarkable for having produced a communication very similar in style to the communication made by "Demi-Siphon." I translate the account given in *Annales des Sciences Psychiques*, Sept.-Oct., 1897, p. 261.

"The following séance was held about January 13th, 1894, in M. Loze's room at the College. I give below a passage from a letter that I addressed to Professor Charles Richet on January 21st, as it correctly records what took place:—

'MONSIEUR LE DOCTEUR,—Eight days ago we made some new experiments in psychic force. One startling incident remains in my mind, which after several days' consideration I have decided to communicate to you.

At the table were MM. E. Vidal, Loze and myself.

Without any preliminary question the table dictates :

Jeanne Eymery.

Are you dead? Yes.

Where? Barnabé.

What did you die of? Murdered.

By whom? Husband.

When? January 10th, 1894.

Has your husband been arrested? Yes.

Where is he? Périgueux.

We took the whole thing as a joke, merely astonished at the part played by the unconscious self with its faculty of inventing elaborate stories, and we didn't even think of verifying these fantastic lucubrations.

In the evening, at supper, M. Vidal père said to his son : 'Well, have you seen anything?' M. Etienne Vidal replied with a laugh, 'Oh nothing. We had a visit from a woman named Jeanne Eymery who was murdered by her husband, etc.'

Whereupon Mme. Vidal exclaims, 'Why, it's in to-day's *Petite Gironde*.'

Amazement of M. E. Vidal who, after ascertaining that this affair was related at length in the *Petite Gironde*, runs off to find M. Loze in order to acquaint him with this strange discovery.

I send you the newspaper in question.

Your first idea, Monsieur le Docteur, will certainly be that one of us had read or learned in some way or other the story so strangely communicated to us. This is not the case, and we append our three signatures at the end of this letter in support of our unanimous assertion.'

"From inquiries that I made by letter of the schoolmaster at Trélassac, of which Barnabé is a suburb, and also of the secretary of the mayoralty of the parish where Jeanne Eymery was born, it appears that her only Christian names were Marie and Françoise. But it may be observed that the husband's name was Jean Eymery and that the wife is often known by the name and Christian name of her husband. It is true that 'Jeanne' is not 'Jean,' but perhaps we may have borrowed the letter E of the surname Eymery and tacked it on to the Christian name 'Jean.' This would give the pronunciation 'Jeanne,' though one letter would be wanting to complete the proper spelling of the word. Assuming this to have been the case, there would be no contradiction on any point between the facts communicated by the table and those given in the newspaper."

Again a most important detail in the evidence is omitted by M. Rouillon, for as before he does not state who put the questions to the table. No difficulty seems to have been felt at the time when the table rapped out the name "Jeanne." The question "Are you dead?" "Vous êtes *Morte*?" (feminine) follows pat. The attempted explanation of the confusion between "Jean" and "Jeanne" strikes me as particularly feeble. I am afraid that the unsympathetic sceptic would point out that the substitution of the name "Jeanne" for "Marie Françoise" was far more probably caused by a slip of the memory on the part of one of the experimenters, who had read the account of the murder in the *Petite Gironde* and confused the names of the husband and wife, or who had read a report in some other newspaper where the murdered woman's name was incorrectly given as "Jeanne." For we have

no reason to suppose that the affair was reported only in the *Petite Gironde*. And surely a spirit who three days after death could make such admirably lucid communications would not be likely to fail in such a personal detail as its own Christian name. Does not the inquiry "Has your husband been arrested?" look like a "leading question"? It suggests the methods of a counsel examining a well primed witness. Perhaps the parts of examining counsel and witness in this case were played by the same person. Strange too that the experimenters should have regarded what M. Rouillon seven days afterwards in his letter to Professor Richet calls "a striking incident," as a joke. Why, little more than a fortnight before, they had been treated to a communication from Demi-Siphon, the details of which had been verified by means of a paragraph in *Le Temps*. These two spirits, Demi-Siphon, late of the Moulin Rouge, and Jeanne Eymery, were really most accommodating. With a thoughtfulness, which I am sure Dr. Hodgson would like to see emulated by the spirits communicating through Mrs. Piper, they refrain from giving any information which cannot be verified in a newspaper published the same day or a day or two before. They evidently shrink from giving any reason for suspecting them of being "lying spirits." Everything that they rap out on M. Loze's table has already appeared in print, with the exception of the single word Vadrouille, and a conversation unsuitable for publication. What more can one say than that!

Yet there is one little point more in their favour.

Jeanne Eymery can communicate clearly and unconfusedly three days after she is murdered, and Demi-Siphon also a very short time after her life of degraded excess had come to an end. The promptitude and precision of these communications would seem to show that a table is a more easily manipulated medium than a human organism.

But are we bound to believe that these are really messages from another world? Well, M. Rouillon can find no other explanation that fits the facts; for, says he, "suggestion or transmission of thought (in other words, telepathy), presupposes a knowledge in the mind of one or of several persons present of the facts revealed by the subject." Now we are assured again and again that none of the persons present at these séances were cognisant of the facts revealed by Demi-Siphon and Jeanne Eymery.

Possibly M. Loze could solve the mystery.

J. G. SMITH.

The Making of Religion. By ANDREW LANG, M.A., LL.D. (Longmans, Green and Co., London, New York and Bombay, 1898, pp. 380.)

This volume is full of interest for students of our evidence. It may, in fact, be defined as an essay in applied Psychological Research. Mr. Lang's object throughout the first half of the book is to compare primitive and savage beliefs in various classes of marvels—clairvoyance, possession, poltergeists, the fire-ordeal, etc.—with modern civilised testimony to the same effect, and to suggest a modification of current anthropological theories, in so far as they assume the baselessness of these primitive beliefs. The noble savage, Mr. Lang argues, is not such a fool as the anthropologists make out: he had

at least as good ground for believing in powers transcending the material universe as any modern Spiritualist. The present writer does not feel competent to criticise Mr. Lang's views as to the origin and growth of religious ideas, nor is it necessary here to consider them. Special training is no doubt required to enable one to appreciate the value of the evidence offered. To the layman, indeed, it looks as if in the vast field of anthropology each inquirer is free to select whatever facts best fit his views, and still leave enough for those who come after to confute him withal. But no doubt there are rules of the game to be observed.

In support of his position, however, Mr. Lang quotes a good deal of modern evidence for clairvoyance, "opening the gates of distance" and the like. The most striking are the crystal visions of the lady whom he names Miss Angus, some account of which has already been given in a paper read before the Society last year. Mr. Lang met "Miss Angus" early in 1897, and received from her accounts of some curious hallucinatory experiences. Mr. Lang then induced the lady to look into a glass ball, and she shortly developed an extraordinary faculty of seeing visions of persons and places unknown to her, but known to or connected with those in her company at the time. Here is one account of the kind given in Miss Angus's own words (pp. 97-98).

"II.—One afternoon I was sitting beside a young lady whom I had never seen or heard of before. She asked if she might look into my crystal, and while she did so I happened to look over her shoulder and saw a ship tossing on a very heavy choppy sea, although land was still visible in the dim distance. That vanished, and as suddenly a little house appeared with five or six (I forget now the exact number I then counted) steps leading up to the door. On the second step stood an old man reading a newspaper. In front of the house was a field of thick stubby grass where some *lambs*, I was going to say, but they were more like very small sheep . . . were grazing.

"When the scene vanished the young lady told me I had vividly described a spot in Shetland where she and her mother were soon to spend a few weeks."

[Mr. Lang adds]—"I heard this case from Miss Angus within a day or two of its occurrence, and it was then confirmed to me verbally by the other lady. She again confirms it (December 21st, 1897). Both ladies had hitherto been perfect strangers to each other. The old man was the school-master apparently. In her MS., Miss Angus writes 'Skye,' but at the time both she and the other lady said Shetland (which I have restored). In Shetland the sheep, like the ponies, are small."

The next case to be quoted is more striking, as being strictly experimental. Mr. Lang writes (p. 99):—

"The next case is attested by a civilian, a slight acquaintance of Miss Angus's, who now saw him for the second time only, but better known to her family.

"IV.—On Thursday, March — ? 1897, I was lunching with my friends the Anguses, and during luncheon the conversation turned upon crystal balls. The subject arose owing to Miss Angus having just been presented with a crystal ball by Mr. Andrew Lang. I asked her to let me see it, and

then to try and see if she could conjure up a vision of any person of whom I might think . . . I fixed my mind upon a friend, a young trooper in the [regiment named], as I thought his would be a striking and peculiar personality owing to his uniform and also because I felt sure that Miss Angus could not possibly know of his existence. I fixed my mind steadily upon my friend and presently Miss Angus, who had already seen two cloudy visions of faces and people, called out, 'Now I see a man on a horse most distinctly; he is dressed most queerly and glitters all over—why, it's a soldier! A soldier in uniform, but it's not an *officer*.' My excitement on hearing this was so great that I ceased to concentrate my attention upon the thought of my friend, and the vision faded away and could not afterwards be recalled.—December 2nd, 1897."

"The witness gives the name of the trooper whom he had befriended in a severe illness. Miss Angus's own account follows: she had told me the story in June, 1897.

"Shortly after I became the happy possessor of a 'crystal' I managed to convert several very decided 'sceptics,' and I will here give a short account of my experiences with two or three of them.

"One was with a Mr. —, who was so determined to baffle me, he said he would think of a friend it would not be *possible* for me to describe!

"I had only met Mr. — the day before, and knew almost nothing about him or his personal friends.

"I took up the ball, which immediately became misty, and out of the mist gradually a crowd of people appeared, but too indistinctly for me to recognise anyone, until suddenly a man on horseback came galloping along. I remember saying, 'I can't describe what he is like, but he is dressed in a very queer way—in something so bright that the sun shining on him quite dazzles me, and I cannot make him out!' As he came nearer, I exclaimed, 'Why, it's a *soldier* in shining armour, but it is not an *officer*, only a soldier!' Two friends who were in the room said Mr. —'s excitement was intense, and my attention was drawn from the ball by hearing him call out, 'It's wonderful! It's perfectly true! I was thinking of a young boy, a son of a crofter, in whom I am deeply interested, and who is a trooper in the — in London, which would account for the crowd of people round him in the street.'"

One other case may be referred to, as illustrating the intrusion on a vision of the ordinary kind of an alien impression—which, if due to thought-transference, may have had its origin in the mind of a lady at a considerable distance, through the mediation, it may be conjectured, of her mother, who was present in the room with Miss Angus.

Miss Angus, on February 2nd, 1897, was looking in the crystal and describing what she saw—a man with an agreeable smile, whose appearance and surroundings were described with some detail and recognised by the persons who had set the test. "The vision, which interested Miss Angus, passed away, and was interrupted by that of a hospital nurse, and of a lady in a *peignoir*, lying on a sofa, *with bare feet*." The vision was apparently not recognised at the time; but a few days later, a Mrs. Cockburn, who had been present, struck by the exact fulfilment of a later vision, wrote to her young married daughter, then some fifty miles away and much in her

thoughts, "and asked whether on February 2nd, she had been lying on a sofa in her bedroom, with bare feet. The young lady confessed that it was indeed so; and when she heard how the fact came to be known, expressed herself with some warmth on the abuse of glass balls, which tend to rob life of its privacy." The incident, however, occurred between 4.30 and 7.30 p.m., whilst the crystal vision was seen about 10 p.m.

As an alternative to the hypothesis of thought-transference from the younger lady's mind, it may be suggested that the attitude was perhaps a not unfamiliar one; and that Mrs. Cockburn may have subconsciously pictured her daughter as so placed at that time.

It is curious to note the effect produced on Mr. Lang by Mrs. Piper's trance-revelations. This is how he refers to the most astonishing body of testimony which has ever been published for supernormal faculty; the only testimony of the kind, indeed, in which every evidential flaw has been, as far as possible, eliminated.

"In this treatise, I may have shown the 'will to believe' in an unusual degree; but, for me, the interest of Mrs. Piper is purely anthropological. She exhibits a survival or recrudescence of savage phenomena, real or feigned, of convulsions and of secondary personality, and entertains a survival of the animistic explanation." But this sentence was written, apparently, before the publication of Dr. Hodgson's last Report. Mr. Lang, after that publication, adds the following:—"The published reports do not produce on me any such impression [*i.e.*, as they have produced on Dr. Hodgson]. As a personal matter of opinion, I am convinced that those whom I have honoured in this life would no more avail themselves of Mrs. Piper's 'entranced organism' (if they had the chance) than I would voluntarily find myself in a sitting with the lady." All which means, I take it, that Mr. Lang does not like Mrs. Piper.

Mrs. Piper, no doubt, makes—or used to make—ugly faces in going into the trance: and her "spirits" use various Americanisms, and say "Sir" to each other and the sitter with irritating frequency. But setting aside such æsthetic considerations (which can hardly be supposed to affect a critical estimate of the case) I can suggest but one explanation for what seems a curiously inadequate appreciation on Mr. Lang's part of evidence valuable not less for his purposes than for ours. Mrs. Piper's trance utterances are presented in all their original crudity, with repetitions, incoherencies, loose tags, and irrelevant—or seemingly irrelevant—digressions and interpolations. As such they form a striking contrast to the smooth and finished narratives with which Mr. Lang has so often delighted himself and us—witness, *The Book of Dreams and Ghosts*.

The difference, of course, is the difference between the elaborate antiquities which are the produce of Birmingham or Sobo, and the battered and wormeaten fragments with which the genuine amateur has often to be content. Mr. Lang, if the figure is allowable, likes his psychical bric-a-brac "restored" and decorated.

Mr. Lang is very happy in his treatment of Herr Parish, one of the most recent critics of our evidence. There is no doubt that Parish, though more candid and more painstaking than most persons who have undertaken to demolish the evidence for telepathy, has made several

blunders, has fallen into many singular misconceptions, and has committed himself to some untenable positions. Mr. Lang deftly criticises the critic, and illustrates from Herr Parish's own argument the various sources of error—want of attention, inaccuracy, fallacious memory and all the rest of them—with which Parish charges our witnesses. Here for instance is an example:—

“A lady, facing an old sideboard, saw a friend with no coat on, and in a waistcoat with a back of shiny material. Within an hour she was taken to where her friend lay dying, without a coat, and in a waistcoat with a shiny back. Here is the scientific explanation of Herr Parish:—‘The shimmer of a reflecting surface [the sideboard?] formed the occasion for the hallucinatory emergence of a subconsciously perceived *shiny black waistcoat* [quotation incorrect, of course], and an individual subconsciously associated with that impression.’ I ask any lady whether she consciously or subconsciously associates the men she knows with the backs of their waistcoats. Herr Parish's would be a brilliantly satisfactory explanation if it were only true to the printed words that lay under his eyes when he wrote. There was no ‘shiny black waistcoat,’ but a waistcoat with a shiny *back*. Gentlemen, and especially old gentlemen who go about in bath-chairs (like the man in this story), don't habitually take off their coats and show the backs of their waistcoats to ladies of nineteen in England. And, if Herr Parish had cared to read his case, he would have found it expressly stated that the lady ‘had never seen the man without his coat’ (and so could not associate him with an impression of a shiny back to his waistcoat) till *after* the hallucination, when she saw him coatless on his deathbed. In this instance, Herr Parish had a hallucinatory memory, all wrong, of the page under his eyes. The case is got rid of then by aid of the ‘fanciful addenda’ to which Herr Parish justly objects. He first gives the facts incorrectly, and then explains an occurrence which, as reported by him, did not occur, and was not asserted to occur.

“I confess that, if Herr Parish's version were as correct as it is essentially inaccurate, his explanation would leave me doubtful. For the circumstances were that the old gentleman of the story lunched daily with the young lady's mother. Suppose that she was familiar (which she was not) with the shiny back of his waistcoat, still, she saw him daily; and daily, too, was in the way of seeing the (hypothetically) shiny surface of the sideboard. That being the case, she had, every day, the materials, subjective and objective, of the hallucination. Yet it only occurred *once*, and then it precisely coincided with the death agony of the old gentleman, and with his coatless condition. Why only that once? *C'est là le miracle!* ‘How much for this little veskit?’ as the man asked David Copperfield.”

It is delightful to have the tables turned in this fashion. There is space for but one more quotation. Parish, following Professor Royce, argues that many cases of so-called fulfilled presentiments are really due to hallucination of memory, started by the news of some sudden and painful event; ignoring, or making light of, the fact that many of these “pseudo-presentiments”—as he styles them—are attested by a second witness, to whom the vision was told before the news came.

This is Mr. Lang's humorous presentation of our critic's argument :—

“ Jones tells me that he has just seen his aunt, whom he knows to be in Timbuctoo. News comes that the lady died when Jones beheld her in his smoking-room. ‘Oh, nonsense,’ Herr Parish would argue, ‘you, Jones, saw nothing of the kind, nor did you tell Mr. Lang, who, I am sorry to find, agrees with you. What happened was *this*: When the awful news came to-day of your aunt's death, you were naturally, and even creditably, excited, especially as the poor lady was killed by being pegged down on an ant-heap. This excitement, rather praiseworthy than otherwise, made you *believe* you had seen your aunt, and *believe* you had told Mr. Lang. He also is a most excitable person, though I admit he never saw your dear aunt in his life. He, therefore (by virtue of his excitement), now *believes* you told him about seeing your unhappy kinswoman. This kind of false memory is very common. Two cases are recorded by Kraepelin, among the insane. Sure you understand my reasoning?’

“ I quite understand it, but I don't see how it comes to seem good logic to Herr Parish.

“The other theory is funnier still. Jones never had a hallucination before. ‘The rarity and the degree of interest compelled by it’ made Jones ‘connect it with some other prominent event,’ say the death of his aunt, which really occurred, say, nine months afterwards.”

Naturally, Mr. Lang appears to me less convincing when he turns to criticise my own views on Poltergeists. I submit, indeed, with some diffidence—a diffidence not lessened by the suspicion that to Herr Parish, perhaps, his own case may present itself in a similar light—that Mr. Lang has not fairly represented my argument. Briefly, that argument is as follows: I took all the cases of mysterious stone-throwing, etc., which the Society during its existence, *i.e.*, from 1882 onwards, has investigated at or shortly after the time of their occurrence. Naturally, these eleven cases, selected for investigation out of a much larger number reported in the newspapers during the last fifteen years, were selected because they were *primâ facie* good cases—that is, good for the supernatural interpretation which Mr. Lang here champions.

Now, until I carefully collated these eleven cases, I held it not improbable that there was something inexplicable in these Poltergeist manifestations. I expressed that opinion, indeed, in my report on the first case which I personally investigated in 1883. That I now hold the contrary opinion is not because I find the intervention of a new physical force antecedently incredible. It is solely a question of evidence. I cannot find any evidence that would justify such a supposition, even as a working hypothesis. There are, broadly, two qualities which we demand in evidence for the operation of a new cause. It must be good as evidence, that is, it must be recent, and must proceed from witnesses of good intelligence and character; and it must be demonstrative, that is, the things attested must be of such a kind that no other interpretation is possible. Now, by a careful analysis of these eleven selected cases, and of others gathered from various sources, I endeavoured to show that these two qualities in the evidence are never found together. There is much testimony that is credible, but inconclusive, as where, to quote a case investigated by Colonel Taylor, the new servant's

best hat is reported by her mistress, a respectable householder, to have been found in the water jug, and a pair of stockings were revealed in the kitchen boiler.¹ And again, there is much that would be conclusive, if its remoteness, the defect of education on the part of the witness, or other circumstances, did not lessen its credibility; as where, to take another illustration from a case investigated by Colonel Taylor, a rustic is reported two or three weeks after the event to have seen a shadowy hand upset a tray of potatoes.²

My argument then proceeded to infer that since over so wide a field the qualities of conclusiveness and credibility were never found combined, this characteristic of the evidence was probably not accidental, that the evidence which was credible was for that reason necessarily inconclusive; and that, when the evidence seemed demonstrative, its conclusive quality was due to the circumstances which lessened its credibility.

This argument was strengthened by the demonstration of trickery in some cases, of strong grounds for suspecting trickery in others; and, generally, of errors, discrepancies, and sources of weakness in all the testimony available. Mr. Lang meets my argument by taking one case and analysing it. The case which he selects is the case which I reported upon in 1883—the first case investigated by any representative of the Society. Let me at once admit that Mr. Lang points out one or two flaws—one of considerable importance—in my treatment of this case. The evidence of the Worksop disturbances does not altogether, as erroneously stated, rest on reports made several weeks after the events. Mr. Lang has done what I ought to have done, and has compared the version of the events given to me by one of the principal witnesses on April 10th, with an earlier account by the same witness, published in the local papers on March 9th. These two accounts are in almost complete agreement. And this disposes of my statement as regards this witness, at any rate, that the testimony to the marvels must be discounted in view of the long interval which elapsed between the events and their record. Mr. Lang is also clearly justified in pointing out that in my original report (of 1883) I ought to have quoted the evidence for certain statements made as regards the servant girl concerned and her mother. In fact, they were based, if I remember right, on common—and possibly inaccurate—report.

For the rest, Mr. Lang devotes several pages to shewing that the descriptions given by the various witnesses, and even my own report (of 1883) are inconsistent with the hypothesis of trickery. In all this he does me the honour to reinforce my own argument. The various witnesses are doubtless honest and intelligent; and the things which they say they have seen cannot be accounted for by trickery. That was the impression produced on me by the reports in 1883; it is the impression produced on me still; and it is interesting to note that such is also the impression produced on so acute a critic as Mr. Lang.

But beyond this point our views diverge. To charge Mr. Lang—the champion of our English tongue against all irreverent assailants—with

¹ S.P.R. *Proceedings*, Vol. XII., p. 86.

² S.P.R. *Proceedings*, Vol. XII., p. 71.

misunderstanding an English sentence savours of impertinence, almost of impiety—as if a schoolboy should accuse his headmaster of a false quantity. And yet: I wrote, of the Worksop case, “The phenomena described are quite inexplicable by ordinary mechanical means.” Mr. Lang quotes this sentence (p. 357) and a few lines lower down, twice over, converts it into “Yet a half-witted girl, in Mr. Podmore’s theory, can do what is quite inexplicable by ordinary mechanical means.” But I did not say the *things done* were inexplicable; I said the *phenomena described* were inexplicable; and I meant what I said and no more. It is hardly necessary to point out that the difference is something more than a mere verbal nicety. It is of the very essence of the argument which Mr. Lang does me the honour to criticise. My contention is that things described are by no means the same as things done. Mr. Lang’s view apparently is that they are. And so a page or two further on he introduces a highly decorated piece of bric-à-brac—Mr. Bristow’s account of the Swanland disturbances. The disturbances took place in 1849, when Mr. Bristow was a joiner’s apprentice. In 1854 he made some rough notes of the occurrence; in 1891 he expanded those rough notes into the account printed in our *Proceedings*.¹ This account, as already said, is highly decorated. Mr. Lang introduces it as follows: “The most curious modern case known to me is not of recent date, but it occurred in full daylight, in the presence of many witnesses, and the phenomena continued for weeks.” Mr. Lang finds this case interesting, notwithstanding that unlucky interval of five years; to me it seems probable that the interest which the case possesses for Mr. Lang is due mainly to the interval. If those “moving odds and ends of wood” had been watched by Mr. Westlake, say, instead of by Mr. Bristow: or, if the account had been written down in full the next day, instead of in brief five years afterwards, it may be suggested that the narrative would hardly have possessed sufficient interest to tempt Mr. Lang to quote it.

For myself, I am grieved to think that the Poltergeist should go. He was a more picturesque figure than the naughty little girl who takes his place. There are too many naughty little girls on this planet already.

Mr. Lang next criticises my suggestion—often, of course, made before—that in certain cases the witnesses to alleged “physical phenomena” may have been hallucinated. In two instances, indeed, owing it is likely to a want of clearness of expression on my part, Mr. Lang has misunderstood me. Thus he quotes the account given by the Master of Lindsay of Home’s levitation, which was briefly to this effect: that the witnesses—Lord Lindsay, Lord Adare and Captain Wynne—heard the window in the next room lifted up; that Home was carried out of the window, and in at the window of the room in which they were sitting; that Lord Adare then went into the next room, to look at the window, and “expressed his wonder how Mr. Home had been taken through so narrow an aperture. Home said, still entranced, ‘I will show you,’ and then, with his back to the window, he leaned back, and was shot out of the aperture, head first, with the body rigid, and then returned quite quietly.”

In arguing for the hypothesis of collective hallucination, built up on a nucleus of physical fact supplied by the medium, I suggested, as an

¹ Vol. VII., pp. 384-394.

illustration of my meaning, that Home on this occasion, "at least thrust his head and shoulders out of the window."¹ Mr. Lang quotes this sentence and rejoins, "But if he did, they could not see him do it, for he was in the next room. A brick wall was between him and them." But as they didn't see him do it, and only inferred it from a noise, which again they inferred to come from the opening of the window in the next room, it is clear that the suggested explanation, so far as regards that part of the performance, is superfluous. As a matter of fact, of course, the suggested explanation was intended, not for the first "levitation"—which as having been led up to and introduced by Home at a time and under conditions of his own choosing, can scarcely, I submit, be claimed as a strong case even by those who are convinced that such levitation does occur—but for the second, which was of the nature of a direct experiment.

So again with the fire-test; as performed by Home I should suppose—still on the theory of hallucination—that the manifestation was generally worked by the substitution, aided by a subdued light, of a cold cinder for a hot one; and that he "possibly on some occasions (as when the hot coal produced a blister on the hand of Mr. Lang's friend) held the live coal in his hand, protected by some non-conducting substance."² Mr. Lang, on the authority of Sir W. Crookes, tells me that there is no substance known which can protect the skin from the effects of great heat, and challenges me to produce my non-conducting substance. Well, I was thinking of nothing more recondite than a piece of asbestos cloth.

Mr. Lang does not find the theory of collective hallucination a satisfactory explanation of the alleged physical phenomena commonly called Spiritualistic. Nor do I. But I prefer it, as regards the cases just quoted, to any other hitherto advanced. If I may venture, indeed, to interpret Mr. Lang's position, he is arguing not so much in favour of explanation by supernormal forces, as against an indolent acquiescence in the explanation by hallucination. Of those alleged physical phenomena, the greater part, Mr. Lang would no doubt agree, are due to trickery pure and simple. But there are some recorded cases in which the explanation by trickery pure and simple would amount almost to an outrage on common sense. Mr. Lang and I should possibly differ pretty widely as to the size of this residuum. But we are both agreed that some of the manifestations in Home's presence recorded by Sir W. Crookes and others fall within it. Now, on the assumption that trickery is inadequate in these cases, we have the choice, broadly speaking, of two other explanations: (1) that the things really happened—say that Home really took a live coal out of the fire in his naked hand and was not burnt; (2) that Home did not take up a live coal in his naked hand, but that, partly by exercise of the ordinary conjurer's art, partly by the aid of such verbal suggestion as is employed by the hypnotist, he succeeded in inducing in Sir W. Crookes' mind, at the moment or subsequently, the belief that he had done so. Let us admit that whilst neither explanation is impossible, both go beyond common experience. But the first is without

¹ *Studies in Psychical Research*, p. 121.

² *Op cit.*, p. 121.

parallel or analogy of any kind, and involves the assumption, so far as we can at present see, of an entirely new physical law; the other involves merely the intrusion into a new region of tolerably familiar mental processes. It is perfectly true, of course, that whilst we know of sense-deceptions of many kinds, and occurring under various conditions to various subjects, we know of no other case in which a Fellow of the Royal Society has been hallucinated in the course of a scientific experiment. But then other Fellows of the Royal Society—more's the pity—have not had the courage or the curiosity to place themselves under the same conditions.

Mr. Lang raises two special objections to the theory of collective hallucination; first that all the witnesses, as in the levitation case, are affected in the same way (p. 362). But if the hallucinations are suggested by the word or gesture of the performer, we should expect them, as at a hypnotic séance, to be uniform. We should marvel rather if like causes did not produce like effects. And again, "contagious hallucination cannot affect witnesses ignorant of each other's existence in many lands and ages, nor could they cook their reports to suit reports of which they had never heard" (p. 361). No doubt the uniformity of testimony to these occurrences indicates a uniform cause. But a uniformity of mental constitution in the human race predisposing to similar tricks or similar delusions, will fit the facts just as well as a hypothetical mode of physical energy. And we have some evidence for the one view, and none for the other. In brief, since we have to make some assumption—for merely to admit that these phenomena are of interest is to uphold a theory about them, viz., that they are not yet explained by trickery,—it seems clear that, other things being equal, we should make, as tentatively and undogmatically as we please, the assumption that involves the least departure from the established order.

But other things are by no means equal; there are two special considerations which point very strongly towards hallucination rather than "ectenic" force. The first is the extreme diversity of the effects reported—the levitation of the human body, the handling of red hot substances, the apports of flowers through closed doors, the "materialisation" of objects, the production of musical sounds, the lengthening of the human body, etc. Each new kind of manifestation increases the difficulty of the physical hypothesis. It is apparent, if all—or several—of these alleged phenomena really occurred, that we have to deal either with several new forces, or with one force altogether remote from familiar analogies. An "ectenic" or psychic force wielded by the medium's automatic consciousness can only appear an easier hypothesis than that of the Spiritualists because some of the phenomena are ignored, and some of the difficulties evaded. Unless, indeed, adopting a suggestion made by Mr. Myers, we are willing to credit the automatic consciousness of the medium with the power of dealing, like Clerk Maxwell's demon, with molecular forces. If Home could handle molecules with as much ease as the rest of us can play at marbles, no doubt he could perform all the marvels reported of him. But that is a larger assumption than the champions of "ectenic" force have hitherto shown themselves willing to make. The second set of considerations is, to my

thinking, even more decisive. Sir W. Crookes, at the beginning of his own investigation, indicated very clearly the rules to which proof of the alleged new physical forces should conform. The Spiritualists, he says, tell us of flowers and fruit and Mrs. Guppy being carried through closed doors and brick walls. "The scientific investigator naturally asks that an additional weight (if it be only the 1,000th part of a grain) be deposited on one pan of his balance when the case is locked, and the chemist asks for that 1,000th of a grain of arsenic to be carried through the sides of a glass tube in which pure water is hermetically sealed."

In other words, the phenomena should be performed under strict experimental conditions. The proof of the thing done should depend upon something else than the mere observation of the experimenter, however skilled. There should be a permanent automatic record. Now when Sir W. Crookes wrote the words above quoted—more than 25 years ago—the absence of any evidence of this kind, though a serious defect, was scarcely in itself suspicious. These alleged phenomena had for the most part up to that time been investigated by persons without any scientific training, who were not aware of the kind of proof required. The spirits, or the "ectenic" force, could not have failed to meet tests which had never been demanded. But in the 25 years which have passed since then, not only Sir W. Crookes himself, but many other trained and capable investigators have examined the subject, have witnessed the phenomena, have, on occasion, propounded tests of the exact kind indicated. And yet the evidence stands now exactly where it stood when the words were written; but with a difference. There are plenty of competent persons who have seen things which neither they nor we can explain; but no one can yet point to the fulfilment of the simple test proposed. Many eminent persons can vouch for movements or alterations in the weight of heavy bodies—but the balance in its locked glass case remains unaffected; flowers and fruit and Parian statuettes have continued to make their appearance in closed rooms—but that small particle of arsenic has not yet found its way through the walls of the hermetically sealed tube. Intense cold has been felt at a séance; but has never been recorded by a self-registering thermometer. Strange draperies, delicious scents, solid luminous bodies, even material human forms, have been produced out of the viewless air; and into the viewless air have returned unweighed, unanalysed, and rarely photographed.

Now, whether these tests have been applied and evaded, as we know to have been the case in certain experiments with Eglinton and Slade; or whether the inquirers have simply forgotten to apply them, the result for us is much the same. Twenty-five years ago the hypothesis of fraud *plus* hallucination was at least as probable as the alternative hypothesis of a new physical force. It is now so much more to be preferred, because the tests which alone could distinguish between fact and fantasy have been tried and have failed; or, in the alternative, those who have examined the phenomena have proved themselves unfitted for their task by omitting to apply the tests at all.

FRANK PODMORE.

Hypnotism and Its Application to Practical Medicine, by DR. OTTO GEORGE WETTERSTRAND, M.D. Translated from the German Edition by HENRIK G. PETERSEN, M.D. London and New York. Putnam's Sons. 8vo., pp. 166.

La Revue de Psychologie. Paris.

L'Hypnotisme et L'Orthopédie Mentale, by DR. EDGAR BÉRILLON, Medical Inspector of Public Asylums, and Editor of *La Revue de L'Hypnotisme*. 8vo. pp. 45. Paris, 1898.

Among the medical pioneers of hypnotism no one fills a more prominent place than Dr. Otto George Wetterstrand, of Stockholm, and we welcome a book from his pen, especially so as he is a corresponding member of the S.P.R. and displays this title of honour on the title page.

Though the English translation bears the date 1897, we find the German edition was published in 1891, and we must express regret that the results have not been brought up to a later date, for much has been done in the last six years to systematise and develop the use of hypnotic suggestion. However, we are glad to have the results of Dr. Wetterstrand's early years' experiences, and hope he will soon supplement them by a new edition. We would then suggest the addition of an index.

Dr. Wetterstrand is above all things a practical physician, and his book is written for medical men. He modestly disclaims any pretension to scientific completeness, and he refers the reader who requires a text book to the works of Liébeault, Bernheim, Moll, etc. But scattered throughout the book are many practical observations and reflections which are of great interest to the student of psychology as distinct from the physician. It is more especially this psychological aspect which it is proposed to notice here.

Dr. Wetterstrand at once proclaims himself as a follower of the Nancy School, as opposed to that of Charcot or the Salpêtrière. As is well known, Charcot investigated hypnotism almost entirely on hysterical women at the Salpêtrière, and he regarded the hypnotic state as morbid, and, in fact, as an induced neurosis. Whereas Liébeault and Bernheim working among ordinary men and women at Nancy came to exactly the opposite conclusion, and consider hypnosis closely allied to ordinary sleep and a physiological condition to which almost every healthy person is susceptible. The Nancy theory is that now generally held, and no one with practical experience can fail to endorse it. The Swedes are not supposed to be neurotic, or even highly imaginative, but they are noted for their fine physique and practical common-sense. It is therefore interesting to compare Dr. Wetterstrand's experience with that of other observers in other parts of the world. Some years ago a German medical writer, after visiting Nancy and Paris, expressed his thankfulness that he was not a neurotic Frenchman, but belonged to a nation whose people were insusceptible to hypnotic influence. The experience of Moll, Forel, Schrenck-Notzing and other physicians in all parts of Germany, does not bear out this boast, and it is instructive to find that the results achieved are very similar, whether reported from France or Germany, England or America, Sweden or Italy.

Dr. Wetterstrand finds temperament an important factor in hypnotism, and holds that fidgety, querulous and contentious persons are the most difficult to influence. Age is a consideration. He says that all children from 4 to 15 are hypnotisable, and he admits no exceptions except those of weak intellect. As one gets older, susceptibility, he says, seems to decrease, but he finds that even in old age the majority of people are susceptible to a greater or less degree.

He employs the method of persuasion, and in all cases endeavours to gain the patient's confidence before attempting to hypnotise him. When there is apparent insusceptibility he often uses chloroform as a predisposing agent and endeavours to make the chloroform anæsthesia pass into the hypnotic state. He has been successful in several cases of this kind after repeated attempts without chloroform had failed, and susceptibility once induced the drug can be dispensed with. (p. 4.)

On several occasions he has obtained acceleration and retardation of the pulse ratio by suggestion, and he has raised blisters on the skin and caused local bleeding in several susceptible subjects. (pp. 30, 32.)

As bearing out his idea that hypnotism is closely allied to ordinary sleep, he relates (p. 34) how sleeping persons, especially children, can be made to pass from one condition to the other. He places one hand lightly on the forehead, while with the other he makes some passes over the body, at the same time telling the sleeper not to awake but to answer him. The sleeper soon becomes cataleptic and replies to questions without waking.

He has endeavoured to repeat the phenomena of transference of paralysis from one side to the other by means of magnets observed by Charcot and Binet and Féré, but he has not obtained their results, and believes that suggestion is the cause, as he has seen similar transference occur when a sham magnet made of wood has been substituted for the metal one.

In the therapeutic part of his book, Dr. Wetterstrand classifies diseases and gives his experience with hypnotism in each class. He finds it very useful in the treatment of drunkenness and relates several striking cases. One is that of an engineer who had had *delirium tremens* three times. His wife was on the point of getting a divorce on the ground of incurable drunkenness (this being possible in Sweden). He went to Dr. Wetterstrand for treatment and was hypnotised fourteen times. A complete cure resulted and the couple went to America, where they got on well.

Dr. Wetterstrand notices, as do many other observers, that anæmic persons are particularly good hypnotic subjects, and he has found that many bad cases of anæmia, especially those dependent on shock and depressing emotions, are cured by hypnotic treatment after ordinary remedies have failed.

As he has succeeded in inducing hemorrhage by suggestion it is not surprising to learn that he has seen it rapidly stop severe bleeding from the nose in a highly susceptible phthisical patient (p. 74).

He remarks (p. 79) that he knows no remedy which exerts so soothing an influence over the dying person as hypnotic suggestion. His experience is borne out by other observers. At such times drugs often lose their effect, or become inadmissible, and the patient's last hours are rendered distressing by restlessness, irritability and sleeplessness.

The rapidity with which suggestion acts, even in removing such objective symptoms as the swelling of joints after injury is shown by some of the cases. For instance, a boy of sixteen went to Dr. Wetterstrand on October 13th, 1887. As the result of a blow about a month before on the knee, the joint was swollen, and he could neither bend nor straighten it, but kept it in a semi-flexed position. Strong fluctuation could be felt in it, and there were two places above the patella painful on pressure. After being hypnotised he was able to walk without limping, all pain had disappeared, and he could bend and extend the leg without difficulty. The next day the effusion had almost entirely disappeared, and he could walk quite well (p. 108). Such a case reminds one of many similar instances reported by Braid fifty years ago, and perhaps throws some light on the success sometimes attending the operations of "bone-setters." These are often men of great ignorance, but of unbounded confidence, and they practically hypnotise many of their patients.

Dr. Wetterstrand says he knows of hundreds of people in whom he can produce anæsthesia in any part of the body by a word, and he finds this of great advantage where he has to examine a sensitive part, e.g., the larynx with the laryngoscope. The patient seems perfectly awake and yet the back of the throat may be tickled with a feather without producing any cough or sputter. Dr. Wetterstrand is extremely careful (p. 111) not to claim too much for hypnotism, and states nothing but the results of his own experience and observation. He reports failures as well as successes, which is not the invariable rule with medical men! He confutes with much indignation some of the theoretical objections levelled against hypnotism, and while acknowledging that it might be employed for evil purposes by bad people, asserts that he has never seen anything but good follow its use in medical practice. He finds that the cures are real and permanent, and that there is no reaction to combat as is sometimes the case with other methods of treatment.

In a new edition perhaps Dr. Wetterstrand will tell us of the results he has found to follow prolonged hypnosis, especially in cases of epilepsy. He has in some cases—reported in the *Revue de l'Hypnotisme*—kept patients in the somnambulant state for over five weeks at a time, eating, drinking, and the natural functions being carried on in obedience to suggestion. One can well understand the benefit which should follow such prolonged rest of the highest brain centres in cases of "nerve strain," cerebral exhaustion and recurrent attacks of mania. We should also like to learn the result of the experiments he stated his intention of making, with Professor Tigerstedt, the physiologist, concerning the raising of blisters and the slowing of the heart's action, etc., by suggestion in somnambulists.

The last fifty pages of the book are devoted to four letters by the translator, Dr. Petersen, in which he gives a very clear account of his experience in Professor Bernheim's clinique at the Hôpital Civile, Nancy, and expresses his own views as to the value of hypnotism in disease and especially in the treatment of moral obliquities.

La Revue de Psychologie is the second medical journal devoted especially to hypnotism and allied subjects published monthly in Paris. It is well

edited by Drs. Hartenberg and Valentin, and is now in its second year. In the numbers for March and April Dr. Van Renterghem contributes two articles, in which he summarises his experience of hypnotism during the four years 1893-97. As he, in collaboration with Dr. Van Eeden, has previously related his experience in the two previous similar periods,¹ we now have available the carefully prepared record of this successful doctor's hypnotic practice for twelve years. During this third period he treated 488 patients, 248 women and 240 men. Of this number 55 persons proved insusceptible to hypnotic influence, about 11 per cent. ; 142 persons, or about 29 per cent., were only slightly hypnotisable ; about 56 per cent., or 271 persons, were profoundly hypnotised ; and about 10 per cent. of the whole, or 47 persons, were hypnotic somnambulists, *i.e.*, they had no memory on waking of what had happened while asleep.

These figures are extremely interesting and instructive and will be found to correspond pretty closely with those reported by other observers in different parts of the world when dealing with private as compared with hospital patients. In hospitals, as Bernheim finds at Nancy, the patients are more thoroughly under the physician's influence, and the atmosphere is charged with suggestion ; hence the proportion of insusceptibles is much smaller and that of somnambulists is much larger.

Cure, however, in psycho-therapeutics does not depend upon the depth of the hypnotic state induced, and some of Dr. Van Renterghem's best results have been obtained with patients who were only slightly hypnotisable. With such a treatment as the hypnotic at present it is found that the physician's clientèle is largely made up of patients who have exhausted all other methods of cure, and Dr. Van Renterghem is therefore justified in expressing satisfaction that he has succeeded in curing 158 or 33 per cent. of his patients and in improving the health of about 36 per cent., or 176 patients. In 84 cases, or 17 per cent., there was no effect, and in 70 cases, or about 15 per cent., the result of treatment was unknown.

Dr. Van Renterghem follows his previous classification and divides the diseases he has treated into :—1. Those affecting the nervous system. 2. Those affecting other parts of the body. The classification is rigorous and the results are noted with accuracy. To the medical inquirer, therefore, this record must be of great help, and much credit is due to Dr. Van Renterghem for the care and thoroughness he has shown in the task.

We note that he has treated ten cases of real epilepsy without a single cure or even any marked improvement, and this unfortunate result is the more disappointing since most observers have claimed at least to get some amelioration in the severity of the cases they had treated. But in hysterio-epilepsy, which is often confounded with real epilepsy even by some medical men, he has been very successful, and also in that distressing and intractable condition known as neurasthenia.

As one would expect, his greatest success has been in the treatment of neuralgias of functional origin, headaches, occupation neuroses (writers' and

¹ *Comptes-rendus, etc.*, Bruxelles. A. Naucereau, 1889. *Psychothérapie*, Paris, Soc. d'édit., Scieⁿce, 1894.

telegraphists' cramp) nervous dyspepsia, insomnia, and bad habits in children. But he has also had considerable success in treating old paralysees of organic origin, locomotor ataxy, and the painful symptoms of phthisis, cancer, and Bright's disease.

Among mental diseases he classifies chronic alcoholism, and he has been successful with the two cases he has treated ; he has also cured some cases of melancholia and *folie du doute*.

Dr. Bérillon has made a special study of the application of hypnotism in the education and treatment of children for over twelve years, and his official position has given him many facilities for experimenting in this field on a large scale. The present pamphlet is the twenty-third contribution he has published on the subject. In it he treats of the method of procedure he adopts, the class of cases in which he applies hypnotism, and the results he obtains.

From his personal experience of several thousands of cases, he deduces the following conclusions :--

1. Eight children out of ten are hypnotisable to a profound degree at the first or second sitting.

2. Normal children are more easily influenced than abnormal ones. The more intelligent the child the more open he is to suggestion.

Dr. Bérillon thinks children should only be hypnotised by cautious and experienced medical men for a definite purpose, and with these precautions he considers the treatment absolutely free from risk, physical, moral, or mental.

It is in the treatment of *degenerate* children that he has found hypnotism most valuable. Among the characteristic signs of degeneracy he places the existence of certain habits which in these children tend to become automatic. Holding a first place among these is nail-biting, which is, however, generally associated with other habits. By hypnotic suggestion he endeavours to raise these automatic actions into the domain of consciousness, and then to arouse or create centres of inhibition. For instance, he tells the nail-biting child that in future he will always be conscious of the habit, and will feel a weight and tension in his hand which will make it difficult for him to put it to his mouth, and that this feeling will call up a desire and power to resist the habit. The same idea applies to kleptomania, of which he has cured many children. He observes that in a real kleptomaniac the impulse to steal is automatic, beyond the control of the will, and the act often is only imperfectly remembered.

Dr. Bérillon quotes some remarkable cases of extreme idleness, inattention, and pusillanimity which he has cured by suggestion. A medical man was once heard to say that he should prefer to have his children naturally naughty than hypnotically good. We imagine the question turns upon the degree of naughtiness which may be considered natural. No sensible person would advocate the indiscriminate hypnotising of young children, but for cases where ordinary educational methods have failed hypnotic suggestion is certainly a most valuable auxiliary and may save the child from an adolescence of misery and crime.

CHAS. LLOYD TUCKEY, M.D.

The New Psychology. By E. W. SCRIPTURE, PH. D., Director of the Yale Psychological Laboratory. 8vo. pp. 500. London : Walter Scott. New York : Charles Scribner's Sons. 1897.

This work is a revision of a more popular treatise by the same author. The popular features are still retained to some extent, but are supplemented by matter of more scientific interest. The animus of the book is a strong defence of experimental methods in Psychology, especially the quantitative measurement of mental phenomena, as opposed to the introspective policy of the past. In the course of the work, the author meets psychical research in his path and assumes the office of fool-killer in seven pages! The main body of the work requires no discussion here. This belongs to the ordinary psychologist. It would also be as unnecessary to notice the author's animadversions upon psychical research were it not for the persistent demand all through the work for the utmost thoroughness and accuracy in the investigation of facts.

The criticism which must be directed against the author is that he betrays absolute ignorance of the subject which he attacks and ridicules. And this after such repeated demands for thoroughness. The accusation thus made can be easily substantiated by an examination of the author's remarks. (pp. 62-69.)

Professor Scripture's mode of attack consists in a comparison of what he regards as the slipshod method of psychical research and the more scientific procedure of Hansen and Lehmann in their criticism of the Sidgwick experiments. How much he knows about the Society's work is shown by the single fact that there is not the slightest evidence of his ever having seen the Society's Reports. The first instance of this negligence is found in the reference to some experiments alleged to have been made by Dr. Ochorovitz, and ridiculed here with a persiflage that is wholly unscientific. The character of the experiments I do not defend. They may be anything you please. But we are entitled to know where they were published and how much weight was given them by Ochorovitz himself. Not a reference is given, while it would seem from the very language that Ochorovitz attached no value to them. The record of them is not found in any of the publications of the Society, nor can I find any trace of them in Ochorovitz's book on "Mental Suggestion." A few of his experiments were mentioned in *Phantasms of the Living*, (Vol. II., p. 660 ff.), but not a word in them refers to the instances criticised by Professor Scripture. If psychical research is to be held responsible in this way for matter to which it has never given its imprimatur, what is to be said of the author's boasted scientific method? I do not say a word in defence of either the method or the results of the Society's work. Any man may think what he pleases about this. I only hold up the standard which Professor Scripture had set for himself. The failure in this respect is seen in another incident.

The example of scientific method here recommended for imitation is that of the two Danish students, Hansen and Lehmann. The value and suggestiveness of their experiments I shall not question, but recognise with unstinted praise. But Professor Scripture shows no knowledge of either Professor Sidgwick's original experiments or his reply (*Proceedings S.P.R.*, Vol. XII.) to Hansen and Lehmann. Moreover, there are distinct indications

in the author's statements that he has relied altogether on the article in Wundt's *Studien* for both his material and his judgment of the subject. He says: "If we look through the hundreds of drawings in Richet's work, and in the *Proceedings* of the Society for Psychical Research, we readily see that the resemblance of the two drawings [referring to the example taken from Hansen and Lehmann's paper] is in only extremely few cases more than in the one given." (p. 67.) This is an exact translation from the article in the *Studien*, and is not included in quotation marks,¹ as it should be, unless Professor Scripture intends to imply that he has examined the publications of the S.P.R. But if he had done this he would have found no basis for such a reckless statement. I may refer him to the following places for proof of my allegation:—*Phantasms of the Living*, Vol. I., pp. 39-48; *Proceedings*, Vol. I., pp. 83-98, and 175-216; Vol. II., pp. 33-42, 195-200, and 208-215; Vol. III., pp. 425-452; Vol. IV., pp. 116-126, and 324-337; Vol. V., pp. 58-112, and 174-191; Vol. VI., p. 398, five pages; Vol. VII., p. 22, seven pages, and p. 382, five pages. The statement quoted cannot be honestly made by any man who has examined the diagrams given in these references, and it only shows that the author under review has abandoned his own injunction to be thorough when he merely translates Hansen and Lehmann without sufficient acknowledgment, and without himself consulting the work of the S.P.R.

Further, Professor Scripture says:—"Hysterical or hypnotised persons are the most frequent percipients in such experiments." (Translation again without quotation marks.) But what evidence is produced for this statement? None. Professor Scripture lays great emphasis on quantitative measurements, and surely here is a statement that is capable of statistical proof if anything is. But not an iota of proof is given. This is scientific method! The statement, indeed, is a pure assumption entirely without foundation. But what if it were true? What difference would that fact make in careful experiments of the kind under review, viz., drawings? The percipients could just as well be insane. It matters not who or what the percipient may be, if the precautions are sufficient to prevent fraud. If we should prove telepathy, and assume or prove that the percipients were abnormal, the fact might require us to abandon the materialistic theory of insanity.

Not only, however, is Professor Scripture completely ignorant of the actual work of the Society for Psychical Research, but he is also apparently completely ignorant of the simple doctrines of chance that need to be applied for any exact estimate of the probable results in experiments in thought-transference. He supposes (p. 65) that if a counter be drawn by chance from a total of 90 counters, the probability that a random record out of the 90 figures will agree with it is only 1 in 8100!! His words are: "We might, like the psychical researchers, proceed to calculations of probability, e.g., if a counter be drawn by chance from the total of 90 counters, the probability of drawing any particular one is 1-90, and likewise the probability of recording at random any particular one of 90 possible

¹ The paragraph of which the above extract is part begins on the previous page (66) with the remark: "This case, continue the authors, seems to be quite noteworthy," but no marks of quotation are used.

figures is 1-90. Now, the probability that the two agree by chance is equal to the product of the separate probabilities, or 1-8100. Only once out of 8100 times ought an experiment to succeed." The probability that Professor Scripture describes is the probability that the number drawn and the number recorded will *both agree with a previously designated number*. But this is not the question. For example, to take one of the instances quoted by Professor Scripture, the problem is not the determination of the chance, prior to the experiment, that the agent would draw the particular number 33, and the percipient also guess 33. The problem is the determination of the chance, that after the agent has drawn one of the numbers, no matter which, the percipient should guess the same number. The agent having drawn 33, this number is already settled, and is calculated as a certainty. It is one of 90 numbers, any one of which the percipient may choose, and the chance that he will guess right is obviously 1 in 90. I do not blame Professor Scripture merely for being ignorant of the very simplest application of the laws of probability, but I do blame him for being ignorant and at the same time attempting, in a professedly scientific work, to deal with a subject where some elementary knowledge of these laws is absolutely essential for even a superficial judgment.

In ordinary controversy, among persons not claiming to be "scientific," we rightly condemn any expositor who offers only a gross misrepresentation of the views he attacks, and shows an ignorance of the fundamental principles pertaining to the subject-matter; but what shall we say of the culpability of a professed scientific investigator who in a professed scientific work, actually makes a charge of "unscientific methods of experimentation" against a body of investigators of whose publications he shows absolute ignorance, holding up to ridicule a gross travesty of the experiments upon which their conclusions are founded, and betraying at the same time a sublime ignorance of the quantitative estimate to be applied to their results?

JAMES H. HYSLOP.

L'Année Psychologique. Publiée par ALFRED BINET, quatrième année. Schleicher Frères, Paris, 1898.

The fourth number of the *Année Psychologique* has recently appeared under M. Binet's editorship. It is only fitting that students of every branch of Experimental Psychology should express their gratitude to M. Binet, M. Victor Henri, and their collaborators for this masterly annual summary of work done over a wide and various field. The summary itself is invaluable, — is practically indispensable to serious students; and each volume contains in addition a mass of original work largely due to MM. Binet and Henri, and in this last volume to M. Vaschide. The labour, patience, and skill involved in the production of each successive volume is positively astonishing. The mere amount of accurate précis-writing and reviewing seems enough to occupy nearly the whole year, and when all M. Binet's own experiments are added, one fancies that night and day, without intermission, his hands must have grasped a dynamometer, a chronograph, an ergograph, a pneumograph, a plethysmograph, or a pen. I will not venture to review his work in detail, important though it be to students of "psychical" phenomena to keep

themselves *au courant* of the labours and results of this growing multitude of workers. The relation of their work to ours may be compared without offence to the relation of surface-mining to deep-level mining among the auriferous reefs of the Witwatersrand. The surface-miners are certain of finding payable gold, scattered pretty evenly through a stiff conglomerate. Here and there the reefs trend downwards, but uncertainly; and work at deep levels at first ruined more companies than it enriched. I offer here no prospectus of our assets or profits; and assuredly the modern machinery, the regular dividends of the surface-miners, may well attract the admiration even of those who are themselves impelled to push on for gold towards the unknown heart of the earth.

F. W. H. M.

Hours with the Ghosts. By HENRY RIDGELY EVANS. (Chicago: Laird and Lee. Pp. 302. 1897.)

This book, which is also otherwise entitled by the author as "Nineteenth Century Witchcraft—Illustrated Investigations into the Phenomena of Spiritualism and Theosophy," scarcely pretends to add anything that is new, of a constructive character, in the way of contribution to Psychical Research.

Mr. Evans states in his preface that he "has had sittings with many famous mediums of this country and Europe, but has seen little to convince him of the fact of spirit communication. The slate tests and so-called materialisations have invariably been frauds. Some experiments along the line of automatic writing and psychometry, however, have demonstrated to the writer the truth of telepathy or thought-transference. The theory of telepathy explains many of the marvels ascribed to spirit intervention in things mundane." The author professes his belief that "we bear within us the undying spark of divinity and immortality," but after giving, in the first part of his book, a description of various "mediumistic manifestations," concludes that the majority of these are due to conjuring, and that telepathy will account for the rest without "spirit intervention." He holds, however, that the supernormal phenomena which he does accept seem to indicate that the human personality is a "spiritual entity" which survives death.

The second part of the book is devoted to an account of "Madame Blavatsky and the Theosophists," and includes references to the chief exposures which have thrown light upon the fraudulent methods used by some of the leaders of the Theosophical movement. The author finds it "difficult to place any reliance in the accounts of Mahatmic miracles." He also adopts the conclusion justified by the researches of Mr. W. E. Coleman, that "'Isis Unveiled' and the 'Secret Doctrine,' by Madame Blavatsky . . . are full of plagiarisms and garbled statements." Mr. Evans draws attention to facts concerning the early period of the Theosophic movement which many of Madame Blavatsky's followers to-day either are ignorant of or prefer to forget. One of these facts is that Madame Blavatsky first located her Mahatmas in the ruins of Thebes and not in Thibet. Another is that a lecture given by a certain George H. Felt was the circumstance that primarily led to the founding of the Theosophical Society. Mr. Evans should have given further details on this point, which

may be found in the *New York Herald* for November 10th, 1895. The information given in that article was furnished by Mr. Henry J. Newton, a well-known spiritualist of New York, since deceased, who had furnished me with a similar account some years previously and had shown me the documents in his possession. It appeared that Mr. Felt asserted that phenomena of "materialisation" could be produced "by a combustion of aromatic gum and herbs." Mr. Newton claimed to have taken the first step in forming a society for the investigation of these particular phenomena, and to have himself proposed the name "Theosophical Society." He also stated to me that the term *Theosophy* was chosen expressly on the basis of the first meaning given to that word in *Webster's Dictionary*: "Any system of philosophy or mysticism which proposes to attain intercourse with God and superior spirits, and consequent superhuman knowledge, by physical processes, as by the theurgic operations of some ancient Platonists, or by the chemical processes of the German fire philosophers." And hence the first sentence of the original preamble of the Theosophical Society read:—

"The title of the Theosophical Society explains the objects and desires of its founders; they seek to obtain knowledge of the nature and attributes of the Supreme Power and of the higher spirits by the aid of physical processes."

The date of its organisation was October 30th, 1875, and not, as Mr. Evans states, November 17th. Even as late as the early part of 1879 the first of "the general plans of the Society" was declared to be: "To keep alive in man his belief that he has a soul, and the Universe a God," which was revised, at the end of the same year, into: "To keep alive in man his spiritual intuitions." Indeed, the true inner history of Madame Blavatsky and the Theosophical Society yet remains to be written, though the sketchy account given by Mr. Evans may sufficiently serve the purpose of the outsider who wishes to get a brief popular history of the movement.

In the first part of his book Mr. Evans relates several apparently super-normal experiences coming under his own notice, but it is to be regretted that he gives no corroborative testimony in the cases where this seems to have been obtainable. Thus he describes a railway accident, where an old man was decapitated by a locomotive between Washington and Baltimore. Mr. Evans was on the train and saw the head, with white hair and beard, standing in a pool of blood. Other details are given. On reaching Baltimore, Mr. Evans went to a newspaper office and wrote out an account of the accident, and did other work at the office before going home. His brother, who slept in an adjoining room, had then gone to bed, and in the morning related to the family a dream which he had during the night and which agreed in the main details with his brother's actual experience in connection with the accident. Yet no statement is offered by the brother or any other member of the family, and the date of the incident is not given any nearer than "the fall of 1890." Corroborative details should have been given either in a footnote or an appendix.

Following these telepathic experiences, after a few pages concerning "muscle-reading," Mr. Evans describes some of the trick methods used by fraudulent mediums,—among them, Henry Slade, The Davenport Brothers, Annie Eva Fay, Charles Slade, Pierre L. O. A. Keeler, and F. W. Tabor,—

and has a little to say about "Spirit-Photography" and "Thought-Photography." If Mr. Evans had confined his attention to this part of his subject and taken special pains to produce as far as possible a complete classified exposition of the various trick devices used by mediums, he might have rendered much more efficient service. It is a good work to popularise the knowledge of the trick methods of fraudulent mediums, and we commend Mr. Evans for what he has done in this direction. He offers a very meagre account of Eusapia Paladino, and quotes from a statement in the *New York Herald*, as though it had been communicated to that paper by myself, whereas it is a grossly inaccurate account of a reporter's interview with me.

In describing the method of writing on the under surface of a slate held against the under surface of a table, Mr. Evans appears to think that the "ability to write in reverse" is involved. This is not necessarily the case. The writing as "visualised" from above would appear in reverse, but the motor changes, which are the most important, are largely the same as in ordinary script. On p. 156 Mr. Evans writes :—

"The 'spirit necktie' is one of the best things in the whole range of mediumistic marvels, and has never, to my knowledge, been exposed. A rope is tied about the medium's neck with the knots at the back and the ends are thrust through two holes in one side of the cabinet, and tied in a bow on the outside."

Manifestations follow which indicate that "either the medium gets loose the necktie and impersonates the spirits or the materialisations are genuine." The explanation given by Mr. Evans is "substitution." The medium cuts the cord round his neck, thus releasing himself, and after producing the manifestations takes a second cord from his pocket, ties it round his neck, and calls for the cord to be unfastened outside the cabinet. He then pulls the first cord into the cabinet and conceals it in his pocket. The general method of substitution in rope-tying tricks is not new, nor, indeed, is the general method of "taking up slack," which, in this particular trick at least, I think is superior to the method described by Mr. Evans, and of which an account was published by Col. Bundy in the *Religio-Philosophical Journal* for May 9th, 1891, on information given by myself. The medium was a Mrs. Martin, who was giving sittings in New York. She pulled in "slack" after she was in position in the cabinet, and before the rope was tied on the outside. She was thus able to widen the loop round her neck and slip it off, replacing it in position after producing the manifestations. I suspected the method used, did all the tying and untying myself, marked the rope privately with a piece of crayon which I had "palmed" for the purpose, and examined the rope at leisure afterwards.

Although Mr. Evans devotes more than forty pages of his book to D. D. Home, there is clear indication that he has not made any careful examination of the mass of testimony to Home's phenomena, and in fact it is difficult to understand that, if he had ever even read the bulk of this testimony, he could have offered the "cheap and ready-made" accounts of the music-box tricks and fire-tricks as providing in themselves an adequate explanation of certain notable incidents described by Home's witnesses. I refer to two other instances of the want of care shown by Mr. Evans. He quotes a statement from "Celia Logan, the journalist,"

concerning "one of Home's séances at a nobleman's house in London," in which occurs the charge that the host saw Home place a bottle on the mantelpiece just before leaving the room for the staircase where luminous hands were seen. The host, it is alleged, seized the bottle and found later that it contained "phosphorated olive oil or some similar preparation," and "after the discovery of the phosphorus trick he dropped Home at once." Who is Celia Logan? Where and when did this account originally appear, and who was the host? We protest against any such vague and uncorroborated charge. At least two such charges against Home have come under my own direct notice; in each case the person making the charge was compelled to acknowledge that the charge was completely unfounded, and, oddly enough, one of the charges was based upon a quotation from Home himself, who was describing the tricks of another medium. This question as to the proof of fraud on the part of Home was considered fully in the article in the *Journal S.P.R.* for July, 1889, by Professor Barrett and Mr. F. W. H. Myers, and at that time no proof of fraud was forthcoming. Later, in 1897, Mr. Podmore writes (*Studies in Psychical Research*, p. 111): "I am not aware that clear proof of imposture was ever brought forward against him." Again, Mr. Evans quotes the statement made by Dr. Carpenter in the *Contemporary Review* for January, 1876, concerning Home's alleged levitation, that "a single honest sceptic declares that Mr. Home was sitting in his chair all the time." This was proved to be a gross misstatement, and was so proved by Captain C. Wynne, the supposed "sceptic" himself, who actually corroborated the account of the levitation. (See *D. D. Home: His Life and Mission*, by Madame Dunglas Home, p. 307; also *Journal S.P.R.*, July, 1889, p. 108.) In dealing with Home, Mr. Evans seems to have followed blindly the lead of Dr. W. A. Hammond's inadequate treatment in his book *Spiritualism and Nervous Derangement*, published in 1876. Whether Home's phenomena can be explained away or not—and there is a large mass of testimony to be taken into consideration—they most assuredly have not been satisfactorily accounted for as yet by any ordinary explanations which I have seen offered, and we cannot but condemn such ignorant and superficial treatment as that accorded to them by Mr. Evans. I should, however, regret if I did Mr. Evans an injustice. Possibly he may have intended merely to present a loose and popular view of Home by quoting various opinions for and against him; but this would be hardly consistent with his professed intention in his preface "to give an accurate account of the lives and adventures of celebrated mediums and occultists."

Generally I may say that much of the book consists of a compilation of extracts from various sources, many of which appear to have been selected without sufficient discrimination, and after a very partial and superficial survey of the subjects considered. I regret that I cannot speak more favourably of a book which is evidently written with sincerity and earnestness, and which recognises so sympathetically the value of the work performed by the S.P.R. There may be a class of readers to whose less substantial wants the book may provide some temporary supply, but I should be surprised if our members found anything enlightening in the volume, unless it were, as I have already indicated, some of the descriptions of trick-devices.

R. HODGSON.

PROCEEDINGS
OF THE
SOCIETY FOR PSYCHICAL RESEARCH,
PART XXXV.

PROCEEDINGS OF GENERAL MEETINGS.

The 95th General Meeting of the Society was held at the Westminster Town Hall on Friday, November 4th, 1898, at 8.30 p.m.; DR. A. W. BARRETT in the chair.

MR. F. PODMORE read a paper on "A Predecessor of Mrs. Piper," which was embodied in his article published in *Proceedings*, Part XXXIV., under the title of "Discussion of the Trance Phenomena of Mrs. Piper."

MR. F. W. H. MYERS gave a "Discussion of some Reciprocal and other Cases recently received."

The 96th General Meeting was held in the same place on Friday, December 9th, at 4 p.m.; the President, SIR WILLIAM CROOKES, in the chair.

MR. H. ARTHUR SMITH read "A Note on 'Fisher's Ghost.'"

A paper by PROFESSOR W. ROMAINE NEWBOLD, entitled "A Further Record of Observations of Certain Phenomena of Trance," was read by MR. MYERS.

Both these papers were included in *Proceedings*, Part XXXIV.

The 97th General Meeting was held in the Lower Hall of the same place (a larger room than that generally used) on Friday, January 27th, 1899, at 4 p.m.; the PRESIDENT in the chair.

PROFESSOR RICHET delivered an address in French, "On the Conditions of Certainty," a translation of which is printed below.

The 98th General Meeting was held in the Council Chamber of the same place on Friday, March 10th, at 8.30 p.m.; DR. G. F. ROGERS in the chair.

MISS MARY H. KINGSLEY read a paper on "The Forms of Apparitions in West Africa," printed below.

The 99th General Meeting was held in the same place on Friday, April 28th, at 4 p.m.; the PRESIDENT in the chair.

Some portions of the paper by MISS ALICE JOHNSON on "Coincidences," printed below, were read by MR. MYERS.

A paper by DR. MORTON PRINCE entitled, "A Case of Triple Personality and Crystal Vision," was also read by MR. MYERS, and some extracts from it are printed below.

I.

ON THE CONDITIONS OF CERTAINTY.

ADDRESS DELIVERED

BY PROFESSOR CHARLES RICHEL.

(Translated.)

MR. CHAIRMAN,—LADIES AND GENTLEMEN,—I do not propose to address you upon the Conditions of Certainty from a philosophical point of view. That theme has been often treated by learned metaphysicians; and the present occasion is ill-suited for metaphysics. Whatever my own respect for that science (of which, by the way, I am entirely ignorant), I think that here and now we had better take our stand upon the more solid ground of actual experiment. That, indeed, is a view to which you have all, more or less explicitly, given your adhesion; and your illustrious President has at different times given to the world examples of the value of experiment,—however daring and unforeseen,—too brilliant to leave me any scruple in speaking to you of Experiment as the true and rightful mistress of scientific enquiry.

The problem before us is how it comes to pass that the facts, so numerous and often so decisive, which you have accumulated during the last twenty years have not carried with them any general conviction. The problem, I say, lies in the disaccord which still exists between the state of public belief and the existence of authentic facts whose cogency would under other conditions have been by this time admitted without dispute.

To explain this persistent incredulity, my simplest plan will be to give you a sketch of my own history. I know well that *le moi est haïssable*:—"I is a hateful word,"—as the great Pascal told us. But I use myself only as a concrete example; and a narrative of actual experience may illustrate more convincingly than any theoretical explanation could do, the strength of the resistance which the human spirit can sometimes oppose to proofs which the reason cannot but admit as complete.

Conviction, indeed, cannot be achieved like a geometrical demonstration;—and it often happens that even a geometrical demonstration does not carry conviction to all minds. M. Thiers, it is said, when no longer young, was anxious to get some notion of mathematics. His excellent teacher one day proved to him, with all the rigidity of

geometrical reasoning, that an oblique section of a cone, in whatever direction it was made, was a regular ellipse. But this M. Thiers would not admit. "It is simply impossible," he said, "there cannot be the same ellipse at the cone's base and at its summit." To persuade him his instructor had to send for a sugar loaf to make the oblique section, and to show him the actual ellipse. Experiment convinced the learner whom theoretical demonstration had left incredulous.

All of us, indeed, are recalcitrant in accepting facts which do not seem concordant with the facts of every day. We are incredulous of the extraordinary; and *how* incredulous I could hardly illustrate better than by my own prolonged and almost invincible opposition to the facts called *occult*.

And now, to begin with, we must get rid altogether of this word *occult*;—or rather we must give it the only sense which it ought to bear. Occult means unknown. Alchemy, before it became chemistry, astrology, before it became astronomy, medicine, before it became bacteriology, were nothing more than occult sciences. Nor, indeed, would it be very hard to show that the classic sciences, of which we are so proud, are not yet far removed from the occult stage. We may know certain phenomena, and even the laws which govern their appearance; but we do not adequately understand a single one of them. To say of the stone which falls to earth that it obeys an attraction which varies directly as the mass and inversely as the square of the distance, is not to *understand* the stone's fall. Familiar though that phenomenon is, it is not a phenomenon which is *understood* in all its elements. Not one phenomenon, I repeat, is fully understood. All are linked together, and if we really understood one, we should understand all.

When I first began to occupy myself with the sciences called occult it was to make experiments in somnambulism. At that time,—it was in 1873, very long ago!—sommambulism was still a mysterious, magical science; and in the account which I gave of my experiments I began by saying, "It needs a certain courage to pronounce the word *sommambulism*." I was right, I think, to have this courage; for a few years later,—and possibly my own efforts helped towards this change,—sommambulism had taken its place among facts which no one denied. As you know, the hypnotic trance is now matter of common knowledge; it forms a theme of ordinary medical instruction, and is no more a subject of doubt than is small-pox or cholera. Thus may an occult science become a classic science in twenty years.

In the course of these studies I had here and there observed certain facts of lucidity, of premonition, of telepathy; but since these facts were denied and ridiculed on every side, I had not pushed independence of mind so far as to believe them. I deliberately shut my eyes

to phenomena which lay plain before me, and rather than discuss them I chose the easier course of denying them altogether. Or, I should rather say, instead of pondering on these inexplicable facts I simply put them aside, and set them down to some illusion, or some error of observation.

Nay, in my servile respect for the classic tradition I mocked at what was called *spiritism*; and after reading the astounding statements which Mr. Crookes had published, I allowed myself—and here do I publicly beg his pardon for it!—to laugh at them as heartily as almost every one else was doing. But now I say just what my friend Ochorowicz says in the same matter;—I beat my breast and I cry *Pater, peccavi!* How could I suppose that the *savant* who has discovered thallium and the radiometer, and foreshadowed the Röntgen rays, could commit gross and inexplicable blunders, and allow himself to be duped for years by tricks which a child could have exposed?

A certain experiment in *spiritism* (I keep the word, although it corresponds to no theoretical idea at all) came to shake my disbelief. One of my friends discovered that he possessed the curious faculty of causing a table to go through certain movements—for him involuntary and unconscious—but which were nevertheless *intelligent*. That is to say, one could put questions and get answers of which he had no knowledge, although he remained fully awake, and his own personal consciousness seemed quite intact. Unwilling to look for any cause outside the causes of familiar phenomena, I invented as an explanation of these strange facts a theory which has not survived, and did not deserve to survive,—the theory of *hemi-somnambulism*. This was in 1883.

Several years before this date, one of my relations had experienced in my presence a telepathic hallucination, under circumstances of the most striking kind. But of this I had taken no serious thought. Little by little, however, as you went on accumulating in your *Proceedings* facts of just the same order, this veridical hallucination of which I had been cognisant returned more strongly to my memory, and a kind of suspense and floating uncertainty took possession of my spirit.

It must be remembered, too, in my excuse, that as a professional physiologist I moved habitually along a road quite other than mystical. I had been taught a scrupulous respect for fact, a habit of exact and prosaic observation, controlled by rigorous tests;—by the balance, the myograph, the chemical reaction. I began to feel myself dragged in two directions by contrary currents.

It would have been something if psychical experiments had been susceptible of exact measurements! But you know too well that this is not so. In the best experiments with sensitives there is always a

caput mortuum which escapes analysis,—something loose and approximate which fails to satisfy men who have taken as their motto these words of the Preacher which govern science: “*Omnia in numero et pondere.*”

But on the other hand the history of science showed me into what strange mistakes men fall by ignoring facts plain to see. The wisest of our forerunners was blind to many a conspicuous phenomenon, simply because it was a phenomenon which he could not understand. “And may it not be thus,” I said to myself, “with these psychical phenomena? The unlearned deny them; the learned exclude them from their text-books; but they may exist for all that.”

Then, as my next step, I imagined,—I ask your pardon for this public confession,—that certain psychological facts of lucidity, of telepathy, perhaps of premonition, were true; but that no occult facts actually affected the material universe. Our human intelligence, I said to myself, is perhaps endowed at certain moments with extraordinary powers, with faculties which remain latent in the mass of men; but that is all; it cannot act directly upon matter.

This novel power of insight—I thought—will in no wise alter our fundamental conception of the world; the only truth in spiritism is just this *lucidity*. Nay, the lucidity itself, although it seems possible, even probable, is not as yet established by vigorous proof.

I was at this point when M. Aksakoff came to see me in Paris, and reproached me for not interesting myself more keenly in experiments with mediums. “Well,” said I, “if I were sure that a single true medium existed, I would willingly go to the end of the world to see him!”

Two years later, M. Aksakoff wrote to me: “You need not come to the end of the world; if you come to Milan it will do.” Milan! that was not far to go to find the key of the mystery.

I took part, then, in those celebrated Milan séances with Eusapia Paladino; and while those séances were going on I was fully convinced of the reality of the phenomena. Numerous precautions were taken; the incessant repetition of tests and experiments satisfied the most scrupulous mind. When I left Milan I was fully convinced that all was true;—as also were the eminent *savants* who took part in the sittings;—Brofferio, Gerosa, Finzi, and the great astronomer Schiaparelli.

But at this point a remarkable psychological phenomenon made itself felt,—a phenomenon deserving of all your attention. Observe that we are now dealing with observed facts which are nevertheless *absurd*; which are in contradiction with facts of daily observation; which are denied not by science only, but by the whole of humanity;—facts which are rapid and fugitive, which take place in semi-darkness, and almost by surprise; with no proof except the testimony

of our senses, which we know to be often fallible. After we have witnessed such facts, everything concurs to make us doubt them.

Now, at the moment when these facts take place they seem to us certain, and we are willing to proclaim them openly, but when we return to ourselves, when we feel the irresistible influence of our environment, when our friends all laugh at our credulity;—then we are almost disarmed, and we begin to doubt. May it not all have been an illusion? May I not have been grossly deceived? I *saw*, no doubt; but did I see aright? who can prove to me that I did so?

And then, as the moment of the experiment becomes more remote, that experiment which once seemed so conclusive gets to seem more and more uncertain, and we end by letting ourselves be persuaded that we have been the victims of a trick.

Our own conviction,—the conviction of men who have seen,—ought properly to convince other people;—but, by a curious inversion of rôles, it is *their* conviction, the negative conviction of people who have *not* seen, and who ought not, one would think, to speak on the matter, which weakens and ultimately destroys our own conviction. This phenomenon occurred in my case with such intensity that scarcely a fortnight after witnessing the experiments with Eusapia Paladino, at Milan, I had persuaded myself that there had been nothing beyond fraud and illusion.

Nevertheless, I wished to repeat those experiments; and at Rome, in company with an eminent *savant*, Schrenck-Notzing, and a celebrated painter, H. Siemiradzki, I again made experiments of the most decisive kind. But a second time I found that doubt seized me after a short interval. I was not yet satisfied; and I invited Eusapia to my house for three months. Alone with her and my excellent friend, Ochorowicz, a man of penetrating perspicacity, I renewed the experiments in the best possible conditions of solitude and quiet reflection. We thus acquired a positive proof of the reality of the facts announced at Milan.

Other friends belonging to your society, Messrs. Myers and Lodge especially, came and shared our conviction. It has since undergone serious oscillation,—partly from that psychological process of recurrence to habitual modes of thought already described, partly through the fault of the medium herself;—but my own fourth series of experiments in Paris, brought with it for me, as also for Mr. Myers, a conviction of reality even stronger than before. Nevertheless, before discussing or publishing experiments in detail, a yet further series should be held under the most careful conditions.

In the meantime it is quite possible that my friends and I may lose that vigour of conviction which recent experience gives. We may return to that curious state of mind of which I have already spoken.

The real world which surrounds us, with its prejudices, well or ill-founded, its scheme of habitual opinions, holds us in so strong a grasp that we can scarcely free ourselves completely. *Certainty does not follow on demonstration, it follows on habit.*

But the duty of the *savant* is precisely not to allow himself to follow the routine of unreasoning respect for what Bacon termed *idols*. Our mission is to seek truth, without caring for the opinion of the vulgar. What should we care for popularity? Sarcasm or indifference ought to leave us equally unmoved.

If we have been credulous, our credulity has not been spontaneous and easy; we have made, as you have seen, an obstinate defence. It took me twenty years of patient researches to arrive at my present conviction. Nay,—to make one last confession,—I am not even yet absolutely and irremediably convinced! In spite of the astounding phenomena which I have witnessed during my sixty experiments with Eusapia, I have still a trace of doubt; doubt which is weak, indeed, to-day, but which may perchance be stronger to-morrow. Yet such doubts, if they come, will not be due so much to any defect in the actual experiment, as to the inexorable strength of prepossession which holds me back from adopting a conclusion which contravenes the habitual and almost unanimous opinion of mankind.

NOTE.

[To explain what is here said of “oscillation,” it must be briefly stated that a series of experiments with Eusapia Paladino was made at Cambridge in the summer of 1895; that the chief investigators arrived unanimously at the conclusion that systematic fraud had been used from first to last in these experiments; and that reference to a description given by Professor Richet himself, in February, 1893, of the manner in which Eusapia's hands were held “en général,” showed that her main method of cheating in the Cambridge experiments must have been practised by her systematically for years.

It ought to be added, in justice to Dr. Hodgson, who rendered much important aid in the exposure of her trickery at Cambridge,—since admitted by her own trance-utterance,—that his opinion of Eusapia Paladino's performances has been in no way modified by the brief notes which were all that it was possible for Professor Richet and myself to make of certain experiments in Paris, on December 1st and 3rd, 1898, which appeared to us conclusive.—F. W. H. MYERS.]

II.

COINCIDENCES.

BY ALICE JOHNSON.

Associate of Newnham College, Cambridge.

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INTRODUCTION.

By a "Coincidence" is meant any conjunction of circumstances that would primarily be regarded as accidental, but in which a special aspect is involved, suggesting a causal relation. It is always this special aspect that attracts our attention, not the fact that the conjunction is in itself a particularly unlikely one to occur. It has a certain antecedent probability, which—theoretically—can always be calculated; and the calculation always shows it to be no more unlikely than many other possible conjunctions. For instance, suppose that some one had correctly prophesied what a certain hand at whist would be. We can calculate what is the antecedent probability of getting this hand and it will be found to be very small, but no smaller than the probability of any other specified hand. The only noteworthy point in the matter would be the correspondence between the actual hand and the prophesied hand, and this would suggest some cause, such as a preconcerted arrangement of the pack.

The consideration of the special aspect that attracts our attention in a coincidence is useful, because it may lead to the discovery of a cause hitherto unknown, or at least unrecognised by science. The appearance of a causal relation, however, is often fallacious; the circumstances may seem to come under the operation of a common cause when they are really produced by entirely independent causes, and such conjunctions or coincidences are accidental.

In psychical research we are constantly confronted with the question:—are those coincidences which form the main material of our study,—the apparitions seen at the time of death or of some crisis in the life of the person represented, the cases of detailed apparent foreknowledge of events, the mass of correct information to be found among the utterances of at least one trance medium, the precise resemblances between the thoughts of two persons in successful experiments in thought-transference, the movements of the divining-rod over concealed underground water,—are all these coincidences to be put down to chance, or is there something more in them?

It may be thought that we have already discussed this question *ad nauseam* and decided it. But this is at most true only in regard to some sections of the above-mentioned classes of facts, and there is room for difference of opinion about many of them. Thus, some persons may

think that the evidence for telepathy is conclusive, but not that for clairvoyance. Some may be convinced of the possibility of telepathy between the living, but yet remain dubious as to communications from the dead. In deciding on this latter question, there are of course other points to be taken into consideration besides the possible scope of chance coincidence. But I think the main doubt is whether—after excluding the cases that may be explained by telepathy from living persons—those that remain are sufficiently numerous to be beyond chance. I do not propose to discuss this difficulty at all now,—still less to pronounce any opinion upon it,—I merely wish to point out that in many lines of our work the question of chance coincidence is still to the front and refuses to be shelved.

The chief reason why apparently telepathic phenomena may be thought accidental is that we have no notion how telepathy—assuming that there is such a thing—causes the phenomena. We may some time find traces of the method by which it works; that is, we may find some of the intermediate links between the cause and the effect. But, so far, we not only have no knowledge of any intermediate links, but there is nothing which even affords any clue as to the direction in which they should be looked for. Telepathy is generally defined as the action of one mind on another otherwise than through the recognised channels of sense. The action may be an exclusively mental one. Of course, in the case of telepathy between living persons, the brain of each person must be supposed to be concerned in the action, but the communication between the two minds may be exclusively mental, involving no physical energy of any kind. This would make it much more difficult—if not impossible—to discover the laws of the transmission, on account of the difficulty of direct observation of any mental action outside our own minds. At all events, the evidence for telepathy rests for the present on two main lines of argument. The first is the fact that a large mass of apparently miscellaneous phenomena, for the occurrence of which there is strong evidence, can be reduced to order and coherence by the hypothesis. It does not account for all the phenomena with which the Society for Psychical Research deals; but if we confine ourselves to really well-authenticated cases, we find that it covers a large proportion, if not the great majority of them, just as the hypothesis or theory of gravitation covers the apparently chaotic motions of the stars. It is true that the theory of gravitation not only covers a much larger number of cases than the theory of telepathy, but also involves all sorts of complicated consequences, nothing clearly contradictory of which has ever been observed. Still, it seems to me that the evidence for both theories is practically the same from a qualitative point of view, though so obviously not the same in quantity.

The second main argument for the existence of telepathy is that the number of cases—which may be spoken of here as “coincidences,” because it is always some kind of coincidence in them that indicates their supposed telepathic nature—are far too numerous to be reasonably attributed to chance. The argument is used for instance by De Morgan (see *A Budget of Paradoxes*, pp. 279-280) with regard to apparitions seen at the time of death. He points out that if there was no causal connection between the apparition and the death, we should expect to find a comparatively large proportion of cases of what he calls “the wrong spectre,”—that is, cases in which it is seen “at the moment of the death of one or another of all the cluster who are closely connected with the original of the spectre,” instead of at the time of death of the original himself. But, says De Morgan, this class of cases is “almost without example.” As a matter of fact, among our own more recent evidence, perhaps as many as half-a-dozen cases are to be found in which a near relative of the dying person was seen at the time of the death; but this as compared with hundreds of cases where the dying person himself was seen.

In the present state of obscurity as to the mode of action of psychical causes, the statistical argument is the strongest one that we can bring to bear in proof of their reality. Though we find its importance recognised now and again by thinkers like De Morgan, and even in much earlier times, and hints thrown out as to the necessity of statistical investigations for settling the question of chance coincidence, the first serious attempts to collect statistics on anything like an adequate scale were made by the founders of the Society for Psychical Research,—pre-eminently by Mr. Edmund Gurney,—and some of the most important work done by the Society since has been on these lines.

I may recapitulate briefly some part of what has been done up to the present. First, as to spontaneous cases of apparitions at the time of death of the person seen. Apparitions of persons known to the seer are not uncommonly seen when nothing particular is happening to the person represented by the apparition. On the other hand, this person sometimes dies at the time. Statistics have been collected by the Society on an extensive scale to test whether the well-authenticated coincidences of this kind are more numerous in proportion to the non-coincidental apparitions than would be the case by chance, and they were found to be very considerably more numerous.

Next, with regard to experiments in thought-transference. In the early days of the Society, some of the most striking results obtained were in experiments in which the percipient attempted to reproduce drawings or diagrams made by the agent,—care being of course taken to prevent the former from gaining knowledge of them through any of the ordinary channels of sense,—and much useful work might be done

now by persons who have opportunities of repeating these experiments. They were criticised on the ground of the familiar fact that the minds of men have a tendency to run in certain grooves,—so that, for instance, if one is asked to think of or to draw objects, or to think of playing-cards or numbers, each person, though he may not be aware of it, has favourites and is more likely to think of some objects, cards, or numbers, than others. These mental “habits,” as they are sometimes called, may be alike in several persons; and when this is so in the case of two experimenters, a certain proportion of the diagrams drawn by the percipient may resemble those drawn by the agent, and thus simulate the phenomenon of thought-transference. Similarly, if cards or numbers are chosen by the agent, his mental habits may lead him to choose a large proportion of those that happen also to be favourites with the percipient, who will therefore have a better chance of guessing right. This is, of course, one reason why it is always best for the agent in experiments with cards or numbers to draw them at random from a batch and not to choose them.

In order to test how far mental habits might have simulated thought-transference in the experiments with diagrams, Colonel Taylor carried out a series of dummy experiments, made in the same manner as the experiments in thought-transference, but with the element of thought-transference eliminated (see *Proceedings of the S.P.R.*, Vol. VI., p. 398). He got eighty persons to draw twenty-five diagrams each, and so obtained 1,000 pairs of diagrams, which could be compared according to a pre-arranged plan. The comparison showed how many resemblances were actually produced by chance, combined with similarity in the mental habits of the persons who drew the diagrams; and the number of resemblances were found to be proportionately far less than those found in the experiments in thought-transference.¹

In experiments with drawings, where the number of possible drawings is unrestricted, it is, of course, impossible to calculate how many successes might be obtained by chance; the question can only be

¹ To this it may be objected that the persons who drew the diagrams being taken at random, there was no reason to expect similarity in their mental habits; whereas—since some experiments in thought-transference fail, while others succeed—it might be argued that only those succeed where the mental habits of agent and percipient happen to be similar. Similarity of mental habits could not, of course, in any case ensure success, because it would be very unlikely that the percipient would think of his favourite forms in the same order as the agent; but it might increase the chance of success. In many of the experiments in thought-transference, however, the diagrams were drawn or selected by some person other than the agent, and not always the same person, so that a general similarity in mental habits—as well as a general tendency to think of the favourites in the same order—would have to be assumed, and this seems to be negated by Colonel Taylor's experiments. The most crucial test of the hypothesis would be to see whether any considerable proportion of the diagrams in successful experiments are those which there is reason to regard as general favourites, and I do not think it is possible to maintain that this is the case.

tested empirically, and a very large number of trials is necessary to ensure a completely satisfactory test.

Taking next the thought-transference of numbers, two Danish psychologists, Messrs. Lehmann and Hansen, have attempted to prove that unconscious whispering—a possible source of error which, I need hardly say, had been present to the minds of the experimenters from the first—accounted for the successes obtained. They found that when they tried similar experiments with one another they had a tendency to whisper the numbers unconsciously, and the one who was playing the part of percipient at a little distance from the agent heard indistinctly what was whispered, and thus often got the number right. But the most important part of their criticism consisted in showing that, when the number was guessed wrong, through being heard too indistinctly, the same mistakes were often made as were made in the experiments in thought-transference. For instance, 2—when it was not guessed right—was guessed as 3 more often than anything else in both sets of experiments. This suggested that the mistake was due to the same cause, viz., indistinct hearing of a whispered sound, in both cases. The argument was founded on *coincidences in mistakes*, which were certainly striking at first sight. Further investigation, however (see *Proceedings S.P.R.*, Vol. XII., p. 298), showed the inconclusiveness of this reasoning. There was a certain series of experiments in thought-transference in which agent and percipient were in different houses, so that the hearing of unconscious whispering was out of the question. In this series, the number of successes was not above what might have been produced by chance; so that all the results that occurred in it—both right and wrong—were to be put down to chance alone. Now it was found that there were quite as many correspondences (as to the mistakes that were made most frequently) between this series and Messrs. Hansen and Lehmann's whispered series as between theirs and the thought-transference series. It follows that the correspondences or coincidences that seemed so remarkable were not beyond what chance could produce, so that no argument as to the cause of the mistakes made could be founded on them.

In the present paper, no attempt is made to carry on further any statistical inquiry of this kind, but rather to compare on more general grounds the curious coincidences sometimes met with in ordinary life to those met with in psychical research. With regard to the examples selected, I have tried to consider as far as possible whether they are due to some cause which is not immediately obvious; and if not, whether there is any reason to suppose that they are not the result of chance. For this end, I endeavour to consider whether the probability of their occurrence is great or small. In choosing cases, I have been guided chiefly by the apparent improbability of

their occurrence,—the common sense impression of their oddness. The degree of improbability is sometimes less than the narrators of the incidents seem to think; but in most of my cases, no numerical calculation of it is possible; only a very rough estimate can be formed, and on general grounds alone. There is, however, a case of two persons guessing independently the numbers drawn in a lottery, where the chances against the double event were calculated by a competent mathematician as about 22 billions to 1 (see p. 249). It is very remarkable that a coincidence such as this should occur,—by which I mean that it must be so exceedingly rare that we are justified in feeling surprised when we meet with it. But if nobody ever met with such a coincidence, we should be still more surprised; as the very reasoning which shows us that it is not likely to occur *more than once* in a certain large number of times, shows us also that it is not likely to occur *less than once*. The improbability in this particular case is enormously in excess of the improbability of any one of the coincidences we meet with in psychical research,—so far as we are able to estimate them numerically. But, as already implied, for the proof that these psychical coincidences are beyond chance, we do not depend on the degree of improbability of any one coincidence, but on the accumulation of many coincidences of certain well-defined types; and in deciding whether other coincidences are accidental, we have to consider whether they too fall into natural classes too numerous to be accounted for by chance.

Since we are dealing with cases which—like apparitions seen at the time of death—obviously *might* be due to chance alone, the question is complicated by an inevitable uncertainty as to which coincidences, if any, are due to chance and which are not. All that the theory of probability can tell us is that out of a certain number of events, the most likely number of coincidences is so-and-so. If we find the actual number to be largely in excess of this, we are justified in thinking it probable that something beyond chance has operated in the whole group of events taken together. But we are not justified in drawing any conclusion about any individual coincidence. The very same reasoning that has led to the conclusion that the whole group of coincidences taken together is not due to chance involves the assumption that *some* of the coincidences *are* due to chance, and affords no criterion by which we can distinguish these coincidences from the others. In course of time, however, we may learn enough about all the circumstances and processes concerned to enable us to distinguish between casual coincidences and those which arise from a cause, and reasoned speculation based on psychical research has, in fact, already reached such a stage that we may often be more or less convinced on rational grounds as to the nature of a given coincidence.

CHAPTER I.

ON THE CLASSIFICATION OF COINCIDENCES.

“To understand what a thing is, one must generally give some attention to appreciating what it is *not*. . . . Applying this plan to the term ‘Chance,’ there will be found to be two sets of terms which are very commonly used to indicate an antithesis. One of these is ‘Causation,’ and its synonymes, and the other ‘Choice’ or ‘Design.’ These two sets of terms mark in strictness . . . a very different kind of opposition; but the controversies to which they give rise will be found to overlap, and in some instances to merge into one another.”—J. VENN. *The Logic of Chance* (2nd Edition, p. 224).

The quotation just given suggests a basis for the provisional classification of Coincidences which I propose to adopt. I divide them accordingly into three classes:—(A) Coincidences suggestive of “Causation”; (B) Coincidences suggestive of “Design”; (C) Coincidences due to “Chance.”

(A) Coincidences suggestive of “Causation.” In this class there seems to be a definite causal connection between the coinciding events. Either one causes the other, or both are due to a common cause, such as thunder and lightning resulting from a certain condition of the atmosphere. The mere fact of two events frequently happening together may lead to the discovery of new causes. Many scientific discoveries have, indeed, resulted from the observation and study of coincidences.

When the things that coincide are due to chains of causes that are up to a certain point independent, the coincidence may yet be by no means accidental. Examples of this may be found in the extraordinarily minute imitation of plants or other objects by some animals, especially insects, the imitation being so close as frequently to deceive both other animals and human beings. For instance, the following account of an incident observed by Mr. Belt in Nicaragua is given in Wallace’s *Darwinism* (2nd Edition, p. 203). Describing the armies of foraging ants in the forests which devour every insect they can catch, Mr. Belt says:—“I was much surprised with the behaviour of a green leaf-like locust. This insect stood immovably among a host of ants, many of which ran over its legs without ever discovering there was food within their reach. So fixed was its instinctive knowledge that its safety depended on its immovability. that it allowed me to pick it up and replace it among the ants without making a single effort to escape. This species closely resembles a green leaf.” Now the ants no doubt took the locust for a green leaf, and if

they had been able to consider how it had been produced, they would probably have argued that, like other green leaves, it had grown on a tree. A very little knowledge of natural history suffices to show that the antecedents of a locust are totally different from those of a green leaf,—that two independent chains of antecedent events have in this case produced two closely similar effects. But a good deal of further investigation is required to prove that the similarity is not accidental, but was brought about by a definite cause,—the gradual selection in one generation of locusts after another of the individuals who escaped destruction through a constantly increasing resemblance to their surroundings.

(B) Coincidences suggestive of "Design." Design is, of course, a cause, but from some aspects of such a different character from other causes that it is convenient to treat it separately. Typical instances of the class are coincidences or combinations of events that make so decidedly for the convenience or inconvenience of particular individuals that—to their minds, at least—the idea of a special intervention on their behalf or against them may be forcibly suggested. Yet these combinations of events may seem to be of exactly the same character as many other combinations which affect nobody, and are therefore considered accidental, their only distinguishing feature being their importance to the individual. Thus, supposing a man misses a train through his watch being slow and the train meets with an accident in which all the passengers are killed; or supposing he misses a train which meets with an accident in which no one is injured. The two coincidences would be of just the same apparently accidental character; yet he would probably be inclined to attach much more significance to the first than to the second. It is, of course, easy to suppose that the man's watch would have been slow that day in any case, quite irrespective of what was going to happen to the train—that, in fact, it was not specially contrived to make him miss the train; just as we assume, speaking generally, that the numbers that turn up in a lottery are determined by chance, without reference to the question whether any one has a stake on them or not. Still, coincidences of this kind may conveniently be treated as a separate class.

(C) Coincidences due to "Chance," that is, those in which the coinciding events are due to independent causes. Mr. Gurney makes the following remark on casual coincidences (see *Phantasms of the Living*, Vol. II., p. 18, foot-note):—"In a general way, coincidences where previous experience affords some ground for suspecting (however faintly) a cause other than chance are distinguished from coincidences where no such ground exists by this fact—

that the latter sort of cases, if *a priori* highly improbable, are not mentioned or described until *after* they have happened. From the mere fact that they do not belong to any known or surmised type, they do not enter into any one's head; no one suggests, without any sort of grounds, that a particular thing will happen to some one at a particular time, or predicts any particular highly improbable coincidence, and then afterwards finds this thing or this coincidence actually occurring." For instance, he says, "The odds are very great against two of the foremost men in a century being born on the same day; yet this happened in the case of Darwin and Lincoln, and no one imagines that the one birth depended on the other."

There are two obvious difficulties in making use of the provisional classification suggested: (1) we often cannot tell to which class a given coincidence belongs; and (2) doubts may arise as to what distinctions really exist between the classes. The second point needs discussion first, the words "Design," "Causation," and "Chance" having been used above in their ordinary popular sense, which perhaps requires definition.

Design and the antithesis between it and Chance.

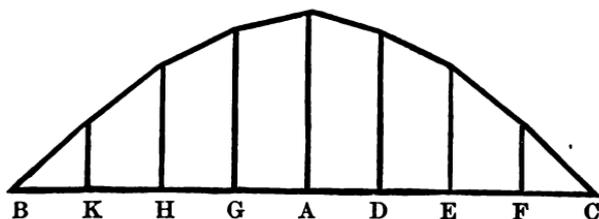
The word "Design" is generally intended to convey the idea that an intelligent Will is manipulating circumstances with some purpose—some end in view. When we speak of the means as being considered more important than the end—if, *e.g.*, a man refuses to gain some advantage for himself by an immoral action—this is rather a loose way of speaking. We do not really mean that the end is disregarded; but that the end ultimately aimed at is not the particular advantage, but morality. Whatever the end may be, and though it may vary from moment to moment, there is always at any given moment in the case of intelligent human action, some end which is aimed at. Effort is concentrated and attention chiefly fixed on one circumstance or condition, all the others being regarded as relatively unimportant. For a finite intelligence with personal interests, it is indeed inevitable that certain things should seem vastly more important than others.

The antithesis between Chance and Design is clear, so long as we confine ourselves to human action. To say that a series of events is a Chance series, and not produced by Design, means that human volition determines that *some* events of certain kinds shall occur, but does not choose exactly *which* event it shall be each time, leaving this to depend on the external physical conditions, which are expressly arranged in such a way as to allow of several alternative events. For instance, in games of pure chance, the player places the object used in

such a position (*e.g.*, up in the air) that one of several things must happen (*e.g.*, if the object has several flat faces, it must fall on one of them); but he expressly abstains from determining which of the things shall happen. If, instead of behaving thus, he deliberately aims at producing one event to the exclusion of the other possible ones, the event is said to be due to Design.

Another typical case is that of a marksman, with some degree of skill, firing at a target, when the particular arrangement of any large number of shots on the target will be the result of Chance. Here the centre of the target may be compared with the particular event in a game of chance on which the player stakes at any given moment. It is true that the two cases are not altogether analogous, because the general distribution of the shots depends to a considerable extent on the skill of the marksman. The greater his skill, the more will they cluster about the centre. Whereas in a game of pure chance, the variations in the events depend merely on the external conditions, and not at all on the actions of the player. But the marksman also deliberately leaves something to be determined by external conditions,—such as gravitation and the wind,—which he does not attempt to control, though he varies his own action to allow for them. If he simply determined to touch the centre of the target with the bullet, he might carry it in his hand up to the target and do so, just as the player might keep his dice in his hand and turn them at will. This would be the work of Design in both cases. But whenever Design, of its own accord, stops short of deciding between several alternatives, the final result is due to Chance.

A Chance series such as the distribution of shots on a target is often represented by a diagram like the following.¹ B A C is a horizontal



straight line, whose central point A represents the centre of the target; other points, D, E, F, G, H, K, in the line to the right and left of A, represent areas in the target at distances to the right and left of its

¹ The same diagram can be used to represent various features in a series of throws of dice, etc., but the method of doing so is a rather more complicated one, and the explanation of it is therefore omitted.

centre,¹ proportionate to the distances of the respective points from A. From all these points vertical lines are drawn upwards, proportionate in length to the number of shots that hit the corresponding areas in the target. If the upper ends of these lines be joined together, the line joining them approaches to the form of a curve, as shown in the figure. The larger the number of shots, the smaller is it feasible to make the subdivisions in the target and the corresponding subdivisions in the line B A C, and the smoother and more symmetrical will the curve representing the distribution of the shots become. The ideal chance distribution in such a case is represented by a perfectly symmetrical curve, and when any curve representing a large number of incidents or events approximates closely to this, there is strong presumption that the whole series was due to Chance and not to Design (or any other cause).

It is, however, often very difficult to find out what has produced a single event. For instance, if we only saw the upshot of a single event—say, a pair of dice lying on a table with certain faces uppermost—it would be impossible to say whether this had been produced by Chance or Design.

In some individual cases, however, there may be grounds for forming a judgment on this point. Suppose, for instance, that in a game of whist all the thirteen trumps are found in one hand. A hand of thirteen trumps is a highly improbable hand, but is just as likely to occur by chance as any other *given* hand. In other words, among all the millions of possible hands, there is no one individual case which is more likely to occur than the case of thirteen trumps. It is, of course, infinitely more probable that *some* other one among the millions should turn up, but not at all more probable that *any particular one* should do so.

It was, then, quite possible that the thirteen trumps came into one hand by Chance. The reason why we may doubt if they did so has nothing to do with our knowledge of the workings of Chance, which practically gives us no help in the case. It is simply that *some one profits* by the hand of thirteen trumps more than he would by any other hand, and there is therefore a *motive* for him to manipulate the pack with a view to this result. Such a case does not, of course, afford conclusive proof of Design, but it certainly affords evidence of it—evidence more or less strong, according to the circumstances.

The Operation of Chance in Nature.

There are many groups of things in Nature, altogether outside human control, which it is possible to represent by diagrams like the

¹ The particular method of subdivision of the target into areas is a matter of detail. Perhaps the best way would be to divide it into concentric ring-shaped areas round the bull's eye, and then draw a vertical line through the centre and take as units the halves of the areas on the right and left sides of the vertical line.

one given above ; for instance, the heights of a large number of men,— if represented in the same way as the shots on the target,— will form a similar curve, the number of men of the average height being indicated by the central vertical line, and the numbers of men of various heights in excess or defect of this by the vertical lines on either side of the centre. These become shorter as they recede from the centre, showing that the more any height differs from the average, the fewer men of that height are to be found. The average in this case may be called the *type*, and the analogy is suggested of Nature aiming at the type, as the marksman aims at the centre of the target. This is, of course, nothing more than an analogy, though in some respects a very close and instructive one. Among the two series, we may find several instances of (a) two men of the same height, and of (b) two shots hitting the same spot on the target. All these are *accidental coincidences*, that is, similar effects produced by independent causes ;—the heights do not influence one another, nor do the shots. The coincidences obviously become more numerous as we approach the average height in one case and the centre of the target in the other ; since in doing so, there are a greater number of heights or shots, as the case may be, per unit of area. This, however, does not make the coincidences any the less accidental ; they are only the more likely to occur.

The same remarks would apply to the heights of a number of hills as to the heights of the men. If any of them coincide, it is not the result of some force directed towards making one the same height as another. Separate causes have acted in each case, and it is only by accident that similar effects have been produced.

Suppose, however, we compare with this the heights of the tide in a number of different places. If the tide is at the same height at a given moment in two places a little way off each other, this coincidence is not accidental, but produced by a common cause,—the combined attraction of the sun and moon on the two places.

A practical criterion of whether these three kinds of coincidences are accidental or not may be afforded by asking the question :— Does knowledge of the height of one man, or hill, or tide, enable us to infer what the height of another specimen of the same class will be ?

After measuring the height of one man, we should expect to find that another will be more nearly of his height than of the height, say, of a giraffe or of an ant ; and the more men we measured, the greater would be our confidence that any other man's height would come within certain limits ; similarly with the heights of hills (though here a certain vagueness would be introduced by having to settle arbitrarily what height must be attained before a rising of the ground can be called a hill at all). A similar method might also be applied to tides ;

we might satisfy ourselves by a large number of measurements that the heights of all terrestrial tides would be almost certain to lie within certain limits.

The distinction between the classes lies in the fact that in the cases of men and hills, we should never reach the point of predicting exactly what the height of any one specimen would be, because the heights are produced in each specimen by causes that are to a large extent independent of the causes that affect the other specimens; whereas exact measurement of the height of one tide (combined with other ascertainable facts) might theoretically give us the heights of all other individual tides, because of the common cause producing all of them. (For a full discussion of a case illustrating this theoretical distinction, see Appendix IV.)

Distinction between Causation and Design.

By "Causation" we mean such a connection between events that the same consequences always follow from the same antecedents. Causation is a more general term than Design—Design being merely a special case of Causation. There is therefore no antithesis between the two terms, and they are sometimes used to imply two different aspects of the same thing. When we speak of Design in an action we are thinking of the subjective aspect,—the point of view of the active Intelligence controlling it. When we speak of Causation we are referring to the objective aspect—the method of action used to produce the result.

Causation and Design in Nature.

Many of the operations of Nature—of inanimate Nature, at least—carry with them no suggestion of their existing for the sake of any purpose or end. They merely present the appearance of a number of events inevitably following by fixed laws the events that have gone before them, and inevitably leading on in a similar way to future events—that is, they seem simply to exemplify the working of Causation. When we come to sentient living organisms, individual Wills and Designs begin to come into existence, and there begins to be scope for the pursuit on their part of the one most obvious and rational purpose of Happiness. We may next imagine a Universal Design, embodying and transcending the designs of all the individuals. But the working of Causation is in no way excluded or superseded by the existence of Design. The conception of Causation seems to imply that each separate item with which it is concerned forms part of one large scheme of which every portion is important as regards the whole,—since the character of the whole would be altered if it were absent, or in any way different from what it is,—but of little or no importance as regards itself taken alone. Supposing that the whole of creation were made

up in this way of parts, past, present and future, all rigidly determining one another, it would not even then follow that Design was absent, for the whole scheme might obviously be the work of Design. Further, this Design might—as a human being would in an analogous case,—regard some parts of the scheme as more important than others, and construct it with some view to the happiness of individual organisms.

To illustrate this point, let us consider the case of eclipses of the sun or moon. These eclipses can only be of extremely slight importance in the solar system, involving nothing more than a brief interception of the rays of light falling on part of the earth or moon, while the effect of gravitation on the sun, earth, and moon at the time is very slightly different from its effects under other circumstances. That is, on the whole the condition of the three bodies at the time of an eclipse is imperceptibly different from their condition an hour before or an hour after the eclipse. For the astronomer, however, there are few events that can compare in importance with these eclipses, from which a great part of his knowledge has been derived. Yet we do not suppose that the solar system was arranged so that eclipses might occur with a special view of instructing astronomers; because we see that it would be difficult,—if not impossible,—to construct a solar system in which eclipses did not occur on anything in the least like its present plan. They are clearly a sort of bye-product of the general scheme, and the fact that their occurrence tends to facilitate scientific discovery does not affect our view of them. Still, it does not seem altogether irrational to suppose that this incidental advantage may have been one of the points taken into consideration in constructing the whole plan.

At the same time, it is difficult to see how there could be any clear proof that the universe was the work of Design and not the result of unintelligent mechanical forces, except through some striking change in the known and more or less familiar course of Causation; while, even if such a change occurred, it would always be possible to put it down to the action of a cause, similar in kind to known ones, but manifest only on rare occasions.

Distinction between Chance and Causation.

The distinction between Chance and Causation is theoretically a perfectly sharp and clear one. Causation is an even and inevitable flow of sequences, unhurried and uninterrupted. If we were able to isolate a simple series of events causally connected (as we can isolate a chance series of events) it would appear that each event depended on the preceding ones, and contributed to the sum of conditions by which all the following ones were determined, so that knowledge of

what had happened up to a certain point would help us to guess what would happen next. There are a great many cases in which we are practically certain about the future, *e.g.*, that the sun will rise to-morrow morning, or that if we throw a stone up in the air it will fall down again. The convenience of this practical certainty is too obvious for remark. There are a great many other cases in which we are only uncertain of the future for want of knowledge of the circumstances. An extension of the same sort of knowledge we now possess would add—it seems that it might go on adding almost indefinitely—to our power of predicting the future. But this would hold only on the assumption that the same consequences would always follow the same antecedents; that is, it is only true when we are dealing with events that are causally connected.

Chance may be regarded either as a constant interruption of a sequence of causally connected events, or—what is the same thing—the breaking up of a long sequence into a number of little short separate sequences, to each one of which the term “event” is applied; for instance, in throwing a pair of dice several times, the whole of what happens from the beginning of one throw to the beginning of the next is called an “event.” In a Chance series each “event” is determined independently of all the others, so that the events exercise no influence on one another. The result is that what has already happened gives us no indication whatever of what is likely to happen next. We know that it will be one out of several things, because we arbitrarily limit the conditions so that only certain things can happen; but we cannot possibly tell which of these things will happen.

As was said above in contrasting Chance with Design, it is comparatively easy to determine whether a long series of events has been produced by Chance or by some cause, but it may often be difficult to judge of the origin of a single conjunction of events, or coincidence. Even here, however, some principles are available for guidance; *e.g.*, an exact and detailed agreement or resemblance between two things will suggest a cause for the resemblance, and it may be so detailed that we are practically certain that it is not accidental.

Theoretically, no degree of complexity and exactness would conclusively prove this. Thus, Dr. Venn remarks, in his *Logic of Chance* (p. 380):—“[Suppose that all the letters of the alphabet are contained in a bag and that one is drawn at a time from the bag and then replaced.] If the letters were written down one after another as they occurred, it would commonly be expected that they would be found to make mere nonsense, and would never arrange themselves into the words of any language known to men. No more they would in general, but . . . if the process were continued long enough,

words making sense would appear; nay more, . . . any book we chose to mention,—Milton's *Paradise Lost* or the plays of Shakespeare, for example,—would be produced in this way at last. It would take almost as many days as we have space in this volume to represent in figures, to make tolerably certain of obtaining the former of these works by thus drawing letters out of a bag, but the desired result would be obtained at length."

Practically, however, if we come across a book with the title, *Paradise Lost*, we are absolutely certain that its close agreement with other books bearing the same title has not been produced accidentally by putting together a number of letters taken at random, but is due to their all being copies of the same original. And a very much lower degree of correspondence in detail may lead to the view that a coincidence of this kind is not accidental. As an illustration, I give the following extract from an article on "Coincidences" by Professor Max Müller in *The Fortnightly Review* for July, 1896 (Vol. IX., p. 48). The article relates to some of the resemblances between Christian and Buddhist writings. Professor Max Müller tells us that the Buddhist Canon was reduced to writing between 88 and 76 B.C., and he gives several instances of stories in it closely resembling some Bible stories, e.g. :—

We read in *Jātaka*, 190 (Vol. II., p. 77):—"One evening, on his way to Jetavana, he, the disciple of Buddha, came to the bank of the river Achiravati, when the ferrymen had pulled up their boats on the shore in order to attend service. As no boat could be seen at the landing-stage and our friend's mind was full of delightful thoughts of the Buddha, he walked into the river. His feet did not sink below the water. He got as far as mid-river, walking as though he were on dry land; but there he noticed the waves. Then his ecstasy subsided and his feet began to sink. Again he strung himself up to high tension and walked on over the water. So he arrived at Jetavana, greeted the Master, and took a seat on one side. The Master entered into conversation with him pleasantly. 'I hope, good layman,' said he, 'you had no mishap on your way.' 'Oh, sir,' he replied, 'on my way I was so absorbed in thoughts of the Buddha that I set foot upon the river; but I walked over it as though it had been dry ground!' 'Ah, friend layman,' said the Master, 'you are not the only one who has kept safe by remembering the virtues of the Buddha.'"

In this case the mere walking on the water would not startle me so much, for among miracles this is not a very uncommon miracle. But walking on the water by faith, and sinking from want of faith, seems a coincidence that can be accounted for by some historical contact and transference only, and in this case we must remember that the date of the Buddhist parable is chronologically anterior to the date of the Gospel of St. Luke.

One more coincidence and I have done. You all know the parable of Christ feeding the five thousand. . . . Well, in the 78th *Jātaka*, as pointed out to me by Professor Estlin Carpenter, we read of Buddha receiving

one cake in his alms-bowl, and after he had fed his five hundred brethren as well as his host and hostess, nay, all the people in the monastery, there were still so many cakes over that they had to be thrown into a cave near the gateway.

Here again, there is, no doubt, some dissimilarity, but the similarity is far stronger, and requires some kind of explanation. We should remember that the Greeks also did not tell their ordinary fables exactly as the Hindus did, nor need the Jâtakas of Buddha be the mere copies of the New Testament parables, or *vice versa*. Yet we could hardly deny that communication and exchange there must have been. The chapter of accidents may be much larger than we imagine, but when we have to deal with fully elaborated stories, with tales composed for a moral purpose, we can hardly fall back on mere chance.

. . . It is not enough that these coincidences should be pointed out; they should be traced to their real source. We have to decide once for all whether we can honestly ascribe them to mere accident, or to our common human nature, or whether we must ascribe them to some real historical intercourse between Buddhism and Christianity. If they can be accounted for by our common human nature, let it be done by pointing out analogous cases. If they can be ascribed to mere accident, again I say let us have similar cases from the chapter of accidents.

Similar evidence of a cause for such coincidences is afforded when the resemblances, although not very close, are repeated several times; for instance, when all the main incidents of a story, though with more or less varying details, are found among several distinct races, like the "far-travelled tale" of Jason and Medea, variants of which Mr. Andrew Lang has pointed out in Zulu, Gaelic, Norse, Malagasy, Russian, Italian and Japanese mythology. He gives this (in *Custom and Myth*) as an instance of a story which "may have been diffused by slow filtration from race to race all over the globe." Speaking of the resemblances between myths in general, he remarks (*op. cit.*, Preface, pp. ix.-x.):—

"As a rule, the incidents in Märchen are common to all races; an artistic combination of many of these in a plot must probably be due to a single imagination, and the plot must have been diffused in the ways described in *Custom and Myth*. Independently evolved myths may closely resemble each other when they account for some natural phenomenon, or are based on some common custom. Wherever a sequence of such incidents is found in a distinct and artistic plot, we may provisionally assign diffusion from an original centre as that cause. Singular as are the coincidences of fancy, it is unlikely that they ever produced *exactly* the same tale in lands which have never been in communication with each other. . . . The suggestion that exactly the same plot, in exactly the same shape, and with exactly the same incidents, can have been invented by several persons independently, seems to me inconceivable. . . . I believe in no

such exact coincidences of imagination, though how far precisely coincidence may go is a delicate question."

It is, of course, possible to attribute a great many of the slighter coincidences met with in folk-lore and other forms of literature to the fact that a large proportion of the current stock of ideas in religion, poetry, and philosophy are the common property of the human race. But whether any given coincidence of this kind is to be put down to this or some other cause, or to chance, is obviously a question which only scholars can settle.

Chance not the Negation of Law. .

Many persons are unwilling to admit the agency of Chance in any matter outside games of chance and the like, because they imagine that this is tantamount to admitting that phenomena may belong to a realm of chaos, and not of law. But more careful investigation shows that it is merely a question of whether they come under the jurisdiction of one set of partially known laws—the laws of Causation, or another set—the laws of Chance. The second set is perhaps even less known, and in a certain sense more mysterious, than the first set. Yet there are clear traces of some definite method in its operation, and it does not even entirely supersede the first. Chance is not the destruction of Causation, but merely a peculiar relationship of Causes to one another. The principle governing it may be described briefly as that of order in the mass, combined with disorder in the constituent units of the mass, and some of the various features and developments of this principle are discussed more fully in the next chapter. Meanwhile, since we sometimes find the same characteristics occurring in nature as in the chance series produced by art and man's device, the question arises whether to apply the same rules of interpretation to them,—the same tests for deciding whether a given coincidence or combination of events is really produced by a discoverable natural cause, or by an intelligent extra-human design, or by chance. When studying an artificial series of events we first try to find out if there is anything in it different from what generally occurs in a chance series. If not, we regard it as due to chance, and this method is generally valid, because it is very difficult to simulate a chance series by design. Yet such an imitation may be so skilful as to escape detection for a long time (see the account of Monte Carlo roulette below, p. 191). If the operation of design has been long overlooked at Monte Carlo, *a fortiori* we may fail to detect evidence of it in the universe.

Again, it has been remarked above that if we only see a single event, or group of events, in an artificial series, we may have reason to think that it was produced by Design, and not by Chance, if there is some one who profits greatly by it. Is it altogether irrational to apply

the same argument to things that are beyond human control, and to maintain that events making for the special advantage of individuals afford some evidence of Design—not conclusive proof, of course, but evidence?

The reply is fairly obvious that we do not know what is really and ultimately for the best advantage of individuals. An optimistic philosophy will hold that everything that happens is for the best; if so, it follows that Design is manifested in everything, not only or specially in certain cases. And this indeed seems the only possible philosophic interpretation of Chance itself—that it is produced by a supreme Design. In our artificial chance series, it is human design that brings the series into existence, while cutting short its own influence at a certain stage in each event of the series. In the universe, we can hardly suppose that Design, if it exists at all, can stop short anywhere; rather we must conceive of it both as producing and using either causally connected groups of events, or chance series of events, at will. All existing entities may be "Pieces of the Game," which may be in reality a game of skill, and not of chance.

CHAPTER II

GENERAL DISCUSSION OF CHANCE.

Though the word "Chance" is one in constant use, there are perhaps few terms about which so much real obscurity of thought exists; it may therefore be worth while to discuss it at some length.

The difficulty of defining Chance consists in its attributes being chiefly negative; for this reason any general assertions about it often fail to convey any precise meaning, unless reference is made to special cases in what is called a "Chance series of events," such as throws of a penny.

In speaking of a Chance series, it must be remembered that what we are considering is an ideal Chance series; that is, one in which (1) the penny or whatever other object is used is so constructed as to be without any bias which would cause one side to be uppermost oftener than the other; and (2) the person tossing it takes care to avoid any procedure which would cause one side to turn up oftener than the other. This ideal may never be perfectly attained in any actual series, but it must be assumed in discussing the subject of Chance; just as in geometry the properties of ideal circles and straight lines are considered, not such circles and straight lines as are seen in diagrams, however accurately drawn.

Probability and Expectation.

If we say that at any point in a series of throws of a penny, the chances of heads or tails turning up are equal, we mean—for one thing—that our expectations of them are equal,—that we do not expect one event at all more than the other. The expectation is then expressed numerically by the fraction $\frac{1}{2}$.

The first explanation that would occur to us of this state of mind would perhaps be that it was founded on our *ignorance*, inasmuch as we know of no reason why one side should turn up rather than the other. Further consideration shows that it depends rather on our *knowledge*, derived from experience, that the two events occur on the whole equally often.

Expectation is equivalent to what may be called the *subjective probability* of the events; the *objective probability* having reference to what actually happens.

The term "objective probability" may be applied either to a single event or to a series of events. The objective probability of a single event, say, a head, is always the same, viz., 1 or 0, since a head must

either turn up or not. The expectation of a single event can never correspond to its objective probability, since we cannot confidently expect either a head or a tail. The expectation is therefore a fraction, —in this case $\frac{1}{2}$.

When we speak of objective probability with reference to a series of events, we mean the *comparative frequency* with which the various events occur. Having found by experience what this has been in a large number of series, we draw the inference that it is likely to be the same in future series of the same kind. In this case, subjective probability (or expectation) may correspond more or less closely with objective probability (or comparative frequency): in throwing a coin, the expectation of a head is $\frac{1}{2}$, which is what its comparative frequency has been in a very large number of previous cases. The expectation, therefore,—unlike the objective probability,—is the same in the single event as in the series, since it is founded solely on experience of the series.

The expectation depends on the assumption that the series under consideration is essentially similar in all respects to the series of previous experience. If we know that it is similar in some respects, but not in all,—without knowing exactly in what the difference consists,—we can only found our expectation on what we know,—namely, the similarity, as far as it goes. Thus, supposing that the penny used in our series has a decided bias, from being made unsymmetrically, or that the person throwing it has a decided tendency to throw in a certain way, one side would be likely to turn up oftener than the other. We might know that there was a decided bias from one or other of these causes; but, *unless we knew which way the bias tended*, we should have no more reason for expecting one side than the other, so that our expectation of heads or tails would still be $\frac{1}{2}$. But the objective probability (or comparative frequency) would of course be different.

Now, in each individual event of any series, *there always must be a bias*, that is, the forces tending to make one side turn up must be stronger than those tending to make the other side turn up, or else neither side would turn up; that is, the objective probability of the single event, as already remarked, is 1 or 0. The ideal chance series may be described as one in which there is a bias, but the bias *varies from moment to moment*, and what we expect is very much the same on the whole as what happens.

The same equality of expectation and objective probability is found in series where there is a *constant bias*, provided we know enough about it to be able to estimate its effects numerically. Thus, suppose we have a bag containing 75 white balls and 25 black ones, and draw out a ball without having seen it a number of times (putting it back after each draw). We should then expect to draw a white ball three times as

often as a black one, and both the expectation and the probability of a white ball being drawn can be expressed by the fraction $\frac{1}{2}$. This is, of course, a mere extension of exactly the same sort of condition as that in which the chances of the two events are equal.

But if there were a constant bias of whose nature, or even of whose existence, we were ignorant, expectation and objective probability would no longer be equal, but might diverge greatly; the results expected might be very different from those that actually occurred. In trying to estimate what those results were likely to be, we could, of course, only go by what we knew; our estimate would represent expectation, not probability; and it would be the divergence found between the expected and the actual results that would reveal the bias to us.

So far, I have spoken of expectation that is founded on purely rational grounds, on inference from known matters of fact—not necessarily what people *do* expect, but what they *should* expect. In the ordinary affairs of life, we are, of course, guided by many other influences besides reason in forming expectations; emotions, such as desire and fear, play a large part in producing them. Thus the divergence between what we expect and what happens is due not only to our ignorance, but to a varied complication of causes. For instance, if a man himself or some one in whom he is greatly interested is in danger,—say, in a battle,—his expectation of a fatal result may be much greater than would be warranted by a calculation of the actual proportion of persons killed out of all those who had taken part in the battle (which would give roughly the rational basis for expectation that any particular person should be killed). Similarly, there is a case in my possession of a veridical dream about a shipwreck (which I have been obliged to omit for want of space, but which will, I hope, be printed soon in the *Journal S.P.R.*) in which the narrator, apparently through his emotional interest in the story, calculates the chance of survival after shipwreck as being almost infinitesimal; whereas if he had considered the actual number of survivors, say, of all the shipwrecks that had occurred during the present century, the chance would, I think, have appeared very much greater.

In judging of the origin of some kinds of coincidences, however, expectation is the more important consideration, because it may have some causal efficacy in producing them. If, *e.g.*, we are discussing whether an apparition seen at the time of death was produced by telepathy or by anxiety about the person seen, it is important to know, not so much whether there was reason for expectation that the person would die, but what degree of emotional interest the seer took in him, and the extent to which he was likely to allow this to influence his expectation.

The Relation of Events in a Chance Series to one another.

If we have two ideal chance series, produced, say, by two persons, A and B, tossing pennies simultaneously, it is obvious that the two series do not affect one another in any way, so that if A's penny turns up a head at the same time as B's, the coincidence is not due to any influence of the throws on one another, but is accidental,—due to Chance. In other words, it was not *caused* by anything. In saying that nothing caused the coincidence, I do not, of course, mean that nothing caused the coinciding events. We do not know exactly what made the penny turn up a head in either case, but we know it was some definite cause in both cases. Given all the conditions,—the resistance of the air, the smoothness of the surface it fell on, the particular way it was thrown, and so on,—the result of the throw was definitely determined from the instant the penny left the thrower's hand, or perhaps a little earlier. The point is that the chain of causation breaks off completely at the end of each throw or event, and begins entirely afresh at the next one, so that what is true of the relation of the events in the *two* series to one another is equally true of the relation of the events in *each* series to one another. The whole of each series is like a number of "broken arcs" to which there is no corresponding "perfect round," since the breaches are absolute.

Since there is no causal connection between the events, it makes no difference whether they occur simultaneously or successively. Further, their order can be changed in any way we like—provided it is done on some arbitrary principle, irrespective of the nature of the events,—and the nature of the series will remain unaltered. Thus, we might take them in the reverse order, or reverse the order of each successive pair of events, or of every alternate pair; we might omit every other event, or every third event, without changing the nature of the series, if it were a sufficiently long one. If, however, we began to change the series with reference to the *nature* of the events, we should at once introduce a fresh element. Thus, if we omitted every fifth head, or changed every tail coming after a succession of three tails into a head, the series would no longer be a Chance series, but one produced, or at least affected, by Design. Conversely, if, in a long series, we find any principle of arrangement recurring much oftener than would be likely by chance alone (the probability of such recurrence can, of course, be estimated precisely) we suspect, with more or less confidence, that the series is not due to Chance only, but to Design. A mere omission of an improbable event may lead to suspicion if continued long enough. For instance, if we went on tossing a penny 20,000 times and never got more than six heads or six tails running, we should know that there was some special cause at work, preventing a longer succession.

Something very much like this has been discovered by Professor Karl Pearson in the case of Monte Carlo roulette (see p. 191).

Another result of the fact that events in a Chance series have no influence on one another is that, if all the events are equally probable, any set of events is as likely to occur as any other set of the same length. For instance, a hand of thirteen trumps (as mentioned above) is as likely as any other specified hand at whist. Or, again, in twelve throws of a penny we are as likely to get twelve heads as to get (1) six heads followed by six tails, or (2) heads and tails alternately. There is, I think, a real practical difficulty in grasping this idea, which seems to me to arise from failing to realise that the whole twelve throws are being considered in each case. We know that six heads running is more likely than twelve heads running, and that one head followed by one tail is more probable still; also that in twelve throws we are much more likely to get some tails than no tails. All these facts seem at first sight inconsistent with the statement that twelve heads running is as likely as the other two cases given. But the twelve heads are to be compared not to six heads, but to six heads followed by six tails; and not to one head followed by one tail, but to six such successive pairs of events.

The probability of any set of events depends merely on the number of possible alternatives (supposing that all are equally probable) and the number of events in the set, and has nothing to do with the nature of the events. It is expressed by the following formula: If a be the number of possible alternatives, the probability of occurrence of any events, n in number, in any order we choose to specify, is $\frac{1}{a^n}$.

Since in questions of pure probability, it does not, as already remarked, make any difference whether we are referring to events occurring simultaneously or successively, or to events past, present, or future—so long as we are equally ignorant of all of them—the formula can be applied equally well in psychical research to cases of prediction, clairvoyance or telepathy, or retrocognition. It shows the degree to which such cases become more valuable as evidence of something beyond chance in proportion to (1) their *complexity*, that is, the number of simple events involved in the perception; and (2) the *rarity* of the events, that is, the number of possible alternative events that might have occurred.¹

So stated, the remark sounds sufficiently obvious. But one fact in connection with it is sometimes overlooked, namely, that it applies only to events that have no causal connection with one another. Most of the events of ordinary life are causally connected, each event being

¹ Assuming that all the alternatives are equally probable. If not, a rather more complicated formula has to be used.

influenced by what has gone before and exercising influence on what follows, and it is to such events that spontaneous cases of apparently supernormal perception generally relate. For instance, crystal visions of facts unknown to the seer are generally of this kind, and it is impossible to say in these cases how large a part is played by inference in elaborating the details of the perception, though of course there must be some basis of perception for inference to work on. On the other hand, in successful experiments dealing with the events in a chance series (*e.g.*, experiments in guessing cards or numbers) it is not necessary to allow anything for the action of inference. For this reason, such experiments afford more satisfactory proof of supernormal power than spontaneous cases. Incidentally, they possess the further advantage that the degree of probability of success in them is not a matter that admits of difference of opinion, even though it may not be easy to determine what degree of success justifies the conclusion that something beyond chance is producing it.

To the above argument it may be objected that we cannot exclude the action of inference from any mental process whatever. In guessing a number of playing cards, say, drawn from a pack at random, and not seen by any one, the person guessing cannot affirm that each guess is uninfluenced by the previous ones. Apart from the "mental habits" leading him more or less unconsciously to prefer certain cards to others, he can hardly help forming some expectations based on previous results, as that a card once drawn will not recur for a little time. Inference probably guides him to a certain extent in making guesses. But the point is that inference is just as likely to guide him wrong as right, because—in a truly chance series—there are no rational grounds for drawing inferences. The action of inference cannot, therefore, in the long run add to the number of his successful guesses, which consequently need not be discounted on this account.

On the other hand, in considering events that are causally connected, there always are, from the nature of the case, rational grounds for inferring from one event something about another one, and inference is then likely to lead us right oftener than wrong on the whole.

The Aggregate Results of Chance Series.

So far we have considered the relation between individual events in a Chance series and the consequences that follow from the absence of all causal connection between them. One of these consequences is the impossibility of predicting what is going to happen in the case of any one event. It would seem that no extension of knowledge about the circumstances and conditions of the events would give us any help in this; an extension of the ordinary human faculties would seem to be required for it.

But the most interesting and important feature in a Chance series,—the point at which the attributes of Chance cease to be exclusively negative—is that absolute uncertainty about individual events is combined with practical certainty as to the general characteristics of the series, if a sufficiently long one be taken. Experience shows that the longer we make the series, the nearer will the results approximate to the theoretically “most probable” results. This fact is so familiar that one may easily fail to see how remarkable it is that out of the not only apparent but real disorder of the single events arise, as if by a sort of spontaneous generation, the order and regularity of the series taken as a whole.

The accompanying diagrams, for which I am indebted to my brother, Mr. W. E. Johnson, may serve to illustrate this point. Figure 1 shows a short series—100 throws of a penny—recorded by a graphic method on paper ruled in small squares.

The throws are represented by the thick zigzag line drawn along the sides of the squares, the length of one side of a square representing one throw. At every change in the series to heads or tails (which, for the remainder of this chapter I will call, for brevity, H or T) the zigzag line turns respectively to the right or left. It starts at the point A, which is one end of a straight line, AB, drawn diagonally through the squares, and the first turn being to the right of this straight line indicates that the series began with an H.

Any point where the zigzag touches the diagonal line indicates an epoch in the series at which the numbers of H's and T's are exactly equal; the diagonal representing, in fact, the most probable result theoretically. The zigzag being on the right of the diagonal at any point indicates an excess of H's at that point, while its being on the left indicates an excess of T's. The exact difference in number of H's and T's at any point can be measured by a straight line drawn from the point in question on the zigzag line to the line AB, the straight line being drawn *either* in continuation of the zigzag line *or* at right angles to it, and the number of squares along which it passes showing the excess of H's or T's over each other. Thus, at the point C in the series, the dotted line drawn from C (either in continuation of or at right angles to the zigzag) passes along four squares before it reaches the diagonal line AB, showing that at the point C, four more H's than T's had been thrown. Further on, at the point D, the T's had amounted to three more than the H's.

The diagram is like a view of an object seen under a microscope, the individual features being brought into great prominence, so that the more general aspects are masked. The irregularity and disorder of the separate throws are strongly marked, while the general tendency to equality of H's and T's is only very faintly and dubiously indicated.

FIGURE 1.

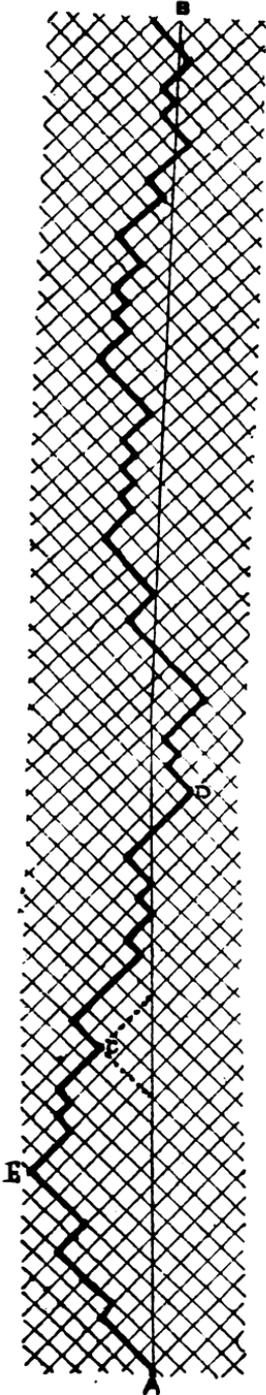


FIGURE 2.

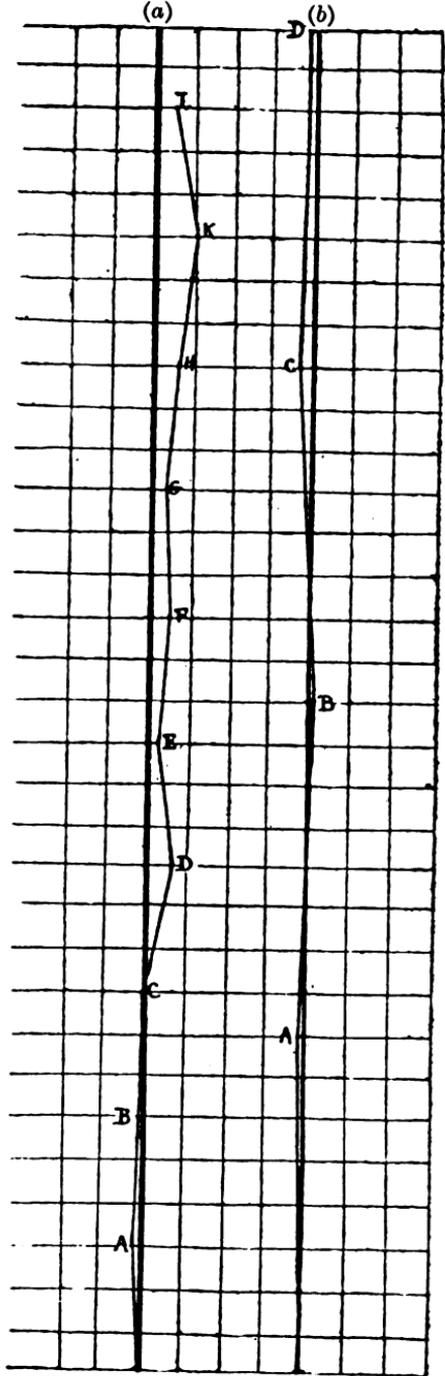


Figure 2 represents in a similar manner the general results of considerably longer series, ignoring altogether the individual results, and the difference in appearance is very striking. The diagrams (a) and (b) record two series of 1,000 and 4,000 throws respectively. The ideal or most probable result—equality in number of H's and T's—is represented in both by the thick vertical straight line, while the zigzag shows the actual results reached at the end of each hundred in the first series, and at the end of each thousand in the second series, these epochs being marked by the letters A, B, C, etc. The position of the zigzag to the right of the straight line shows excess of H's, and the position to its left shows excess of T's—the vertical distances of the various points, A, B, C, etc., from the straight line being roughly proportional to the excess. At the point C in diagram (a) equality happens to have been exactly reached, so that the point C lies on the straight line. The greatest deviation from equality in this series appears at the point K (at the end of the ninth hundred) when 34 more T's than H's had been thrown. At the end of the thousand the excess of T's was 16.

The longer the series, the more chance there is for large deviations from equality; but it is important to note that the deviations, though absolutely larger, become relatively smaller as the series grows in length. Thus, in Fig. 1, the greatest deviation occurs at the point E, where there is an excess of 9 H's, or 60 per cent. of the total number of throws (15). In Fig. 2 (a), at the point D, the excess of T's over H's is 22, or 5.5 per cent. of the total number of throws (400). At K the excess of T's is 34, or 3.8 per cent. of the total number of throws (900). At C, in Fig. 2 (b), the excess of H's is 44, or rather less than 1.5 per cent. of the total (3,000).

These deviations from equality might be represented graphically by the angles which lines drawn from the points in question to the starting point of the series would form with the straight line. Such angles would obviously tend to become smaller as the series proceeds.¹ In other words, the zigzag line (representing the actual results of experiments) tends to approximate more and more closely to the straight line (representing the theoretically most probable results).²

¹ Unless the excess of H's or T's became proportionately larger, in which case we should have strong ground for suspecting a bias—for thinking that the series was not a purely chance one.

² This form of diagram might be used statistically to discover the comparative frequency of two alternatives, by recording them as they occurred. An oblique line joining the initial point to each successive terminal point would represent with continually increasing exactness the required ratio of frequency.

In any case where the *a priori* ratio of frequency is known,—as in the example given in the text,—the oblique line can be drawn beforehand, and the deviations from it show the actual results.

Methods of Analysis of Chance Series.

These diagrams illustrate the relation of actual to theoretical results in shorter or longer series only with regard to the number of times each of the two simple alternative events, H and T, occurs. We may further consider different groups or runs of events in the same series, instead of the simple events.

There are many different ways in which such series can be analysed into groups of events. The usual method is to record the number of times different runs of each alternative have occurred, for comparison with the most probable number of times that they would have occurred by theory. Thus Buffon (quoted in De Morgan's *Budget of Paradoxes*, pp. 169 to 171) analysed his series into "sets" of one alternative, say H. Every time that H is thrown it is called a "set"; if there are several H's following one another, each is a "set" of one throw; if the H is preceded by one or more T's, these form part of the same "set," of two or more throws. The "sets" may thus consist of H, TH, TTH, TTTH, etc., and may be written briefly as H, TH, T_2 H, T_3 H, etc. The following (see Table I.) are the results obtained by Buffon in 2,048 "sets," as given by De Morgan, compared with the most probable results, and with those of a correspondent of De Morgan's whom he calls A.

TABLE I.

"Sets."	Most probable results.	Buffon's results.	A.'s results.
H	1024	1061	1048
TH	512	494	507
T_2 H	256	232	248
T_3 H	128	137	99
T_4 H	64	56	71
T_5 H	32	29	38
T_6 H	16	25	17
T_7 H	8	8	9
T_8 H	4	6	5
T_9 H	2	—	3
T_{10} H	1	—	1
T_{11} H	—	—	—
T_{12} H	—	—	—
T_{13} H	1	—	1
T_{14} H	—	—	—
T_{15} H	—	—	1
Totals	2048	2048	2048

This method shows only the runs of one alternative, T, but of course the same series could be analysed over again, so as to show the runs of H's.

I have adopted a rather more complicated analysis, which seems to me more instructive, showing in one Table: (1) the numbers of runs of different lengths of both alternatives, and (2) in the case of one alternative, H, the runs of each length subdivided to show what length of run of the other alternative they were followed by. This is done by dividing my whole series into consecutive groups of events, each group beginning with an H (which has been preceded by a T) and ending with a T (which is followed by an H). Thus HHHTT, HT, HHT, HTTT, HHT, are 17 consecutive throws divided into groups, shown by the commas. These groups I call "cycles," because they all begin and end in the same way. The plan involves the necessity of regarding the series as a *circular* one; i.e., one in which the end is continuous with the beginning. So that if the series actually ended with one or more H's, these would be brought round to the beginning to form the first part of the first cycle. And if it actually began with one or more T's, they would be carried round to the end to form the last part of the last cycle.

Table II. gives a summary of all the cycles in an actual series of 4,096 ($= 2^{12}$)¹ throws, the number in each square referring to cycles which began with the number of H's stated at the beginning of the line in which the square is, and ended with the number of T's stated at the top of the column in which the square is. Thus, the first line shows that a single H (that is, an H preceded and followed by at least one T) occurred 495 times. (See column headed "Total Cycles"). On 257 occasions it was followed by a single T; on 121 occasions by two T's; on 61 occasions by three T's, and so on. The second line shows that the group of two H's (preceded and followed by at least one T) occurred 262 times, being followed by one T on 137 occasions, by two T's on 59 occasions, and so on.

The column headed "Total Cycles" gives the totals of the different runs of H's, while the bottom line gives the totals of the different runs of T's. The column headed "Total Throws" gives the total numbers of throws corresponding to the total numbers of cycles. This is, of course, a simple matter of calculation. Thus, taking, e.g., the second line from the top, the first square gives a number of cycles consisting of three throws each, the second square a number of cycles of four throws each, and so on.

¹ The series was stopped when this number was reached for convenience in calculating the theoretical or most probable numbers, some of which would be fractional if the total number of events in the series was other than some power of 2.

TABLE II.

	T ₁	T ₂	T ₃	T ₄	T ₅	T ₆	T ₇	T ₈	T ₉	T ₁₀	T ₁₁	T ₁₂	Total Cycles	Total Throws
H ₁	257 <i>256</i>	121 <i>128</i>	61 <i>64</i>	24 <i>32</i>	15 <i>16</i>	8 <i>8</i>	9 <i>4</i>	— <i>2</i>	— <i>1</i>	— 1	— —	— —	495 <i>512</i>	1459 <i>1535</i>
H ₂	137 <i>128</i>	59 <i>64</i>	29 <i>32</i>	9 <i>16</i>	17 <i>8</i>	6 <i>4</i>	— <i>2</i>	2 <i>1</i>	1 1	2 —	— —	— —	262 <i>256</i>	1068 <i>1023</i>
H ₃	63 <i>64</i>	26 <i>32</i>	15 <i>16</i>	8 <i>8</i>	5 <i>4</i>	1 <i>2</i>	1 <i>1</i>	2 1	— —	— —	— —	— —	121 <i>128</i>	609 <i>639</i>
H ₄	33 <i>32</i>	24 <i>16</i>	13 <i>8</i>	2 <i>4</i>	3 <i>2</i>	1 <i>1</i>	1 1	— —	— —	— —	— —	— —	77 <i>64</i>	464 <i>383</i>
H ₅	12 <i>16</i>	6 <i>8</i>	4 <i>4</i>	2 <i>2</i>	1 <i>1</i>	3 1	— —	— —	— —	— —	— —	— —	28 <i>32</i>	207 <i>223</i>
H ₆	5 <i>8</i>	6 <i>4</i>	1 <i>2</i>	1 <i>1</i>	— —	— 1	— —	— —	— —	— —	— —	— —	13 <i>16</i>	102 <i>127</i>
H ₇	6 <i>4</i>	2 <i>2</i>	— <i>1</i>	2 1	— —	— —	— —	— —	— —	— —	— —	— —	10 <i>8</i>	88 <i>71</i>
H ₈	2 <i>2</i>	— <i>1</i>	— 1	— —	— —	— —	— —	1 —	— —	— —	— —	— —	3 <i>4</i>	34 <i>39</i>
H ₉	— <i>1</i>	— 1	1 —	— —	— —	— —	— —	— —	— —	— —	— —	— —	1 <i>2</i>	12 <i>21</i>
H ₁₀	2 1	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	2 <i>1</i>	22 <i>11</i>
H ₁₁	— —	1 —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	1 —	13 —
H ₁₂	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— 1	— <i>1</i>	— <i>24</i>
H ₁₃	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —
H ₁₄	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —	— —
H ₁₅	— —	— —	1 —	— —	— —	— —	— —	— —	— —	— —	— —	— —	1 —	18 —
Totals	517 <i>512</i>	245 <i>256</i>	125 <i>128</i>	48 <i>64</i>	41 <i>32</i>	19 <i>16</i>	11 <i>8</i>	5 <i>4</i>	1 <i>2</i>	2 <i>1</i>	— —	— <i>1</i>	1014 <i>1024</i>	4096 <i>4096</i>

Figures in ordinary type show actual results.

Italic figures show theoretical or most probable results.

Figures in thick type show "residual cycles," see p. 190.

Total H 2057

Total T 2039

Total number of throws 4096

The convenience of this method is that, in a series composed of only two alternative simple events,—the probability of occurrence of each of which is one-half,—we are able to show in a single Table the occurrence of a number of compound events (each made up of two or more simple events) of varying degrees of probability, and to compare actual results in regard to all these events,—both simple and compound,—with the theoretical or most probable results. The latter are given in each square of the Table in italic figures, below the figures in ordinary type, which give the actual results. (For the method of calculation by which the theoretical numbers are obtained, see Appendix I., p. 293.)

This Table is a further illustration of the fact that the longer the series, the smaller in proportion become the deviations from theory. In a series of 997 throws, which formed part of the large total series of 4,096 throws, the cycles of H T were 8 per cent. in excess of their most probable number, whereas in the total series, as seen in the Table, they are about 0.4 per cent. in excess of the same.

But—and this is an equally important point—this closer correspondence with theory is only found in the case of events having a comparatively large probability. Even in the next longer cycles—those containing three events,—viz., $H_2 T$ and $H T_2$,—the deviations from the most probable numbers are 5 per cent. and 7 per cent. respectively (in the shorter series just referred to, they were 13 per cent.). In the cycles of four events, the deviations range from 1.6 per cent. to 7.8 per cent., and in those of five events from 3 per cent. to 25 per cent., and in still longer cycles, the deviations amount often to 100 per cent. or more.

Another feature which is rather surprising at first sight is the exact correspondence with theory of some among the very long cycles, e.g., $H T_6$, $H_3 T_4$, $H_5 T_3$, $H_7 T_2$. It will, however, be seen on consideration that, though the relative deviation from probability in the case of these very long cycles is likely to be often great, the absolute deviation is likely to be almost always small, so that there is really a greater chance of such exact correspondences occurring than in the case of the shorter cycles.

“Residual Cycles.”

There is one further point to which attention may be drawn. On Buffon's method of analysis of 2,048 “sets,” the longest “set” which is likely to occur as often as once is $T_{10} H$. But if we add up the most probable numbers of this and of all the shorter sets, the total comes to one less than 2,048. It is, therefore, necessary for one of the longer sets to occur to make up the total. Now the most probable number of any longer set is a fractional number, less than unity.

But as no case can occur a fractional number of times, it is assumed that one of the longer sets will most probably occur once. The choice in Table I. of $T_{13}H$ as the most probable one is arbitrary, though we should naturally expect one not very much longer than $T_{10}H$.

If a similar series of 4,096 throws be analysed into "cycles," and we add up all the cycles the theoretical number of which amounts to one or more, we find that they fall short of the theoretical total by 11 cycles—corresponding to the single "set" left over on Buffon's plan. These I call "residual cycles." We have, therefore, to fill up the Table by putting in 11 longer cycles (see the figures in thick type), the most probable number of each of which is really a fraction less than unity. Their position in the Table (like the position of Buffon's extra "set") has to be settled in an arbitrary manner, and the particular position chosen in Table II. must be understood to be not more probable than several others. But the "residual cycles" must lie within certain definite and assignable limits, which are explained in Appendix I., with a fuller discussion of the whole subject. It is only necessary to remark here that while, on Buffon's method, there is always *one* set left over,—whatever the length of the whole series may be,—the number of "residual cycles" *varies with the length of the series*, increasing absolutely, but decreasing relatively with it.

By taking into consideration, therefore, more complicated groups of events—"cycles" instead of "sets"—we find that the theoretical distribution involves the necessity of occurrence of a considerable number of groups, each of which, taken alone, should theoretically occur only a fractional number of times. In other words, these are groups which in longer series would be expected to occur only once each.

If, then, we were trying to judge whether there was evidence of any agency beyond chance in an actual series of this kind, we should have to be very cautious in drawing conclusions from these apparently remarkable occurrences, unless they were very numerous. We should be on much safer ground in dealing only with the deviations from the theoretical results shown by the shorter cycles. Similarly, a few extraordinary coincidences, though much more striking to the imagination, afford much less reliable evidence of something beyond chance than a large number of trivial ones all pointing in the same direction.

As an instructive illustration of this point, I may refer to Professor Karl Pearson's investigation of the published results of two months' roulette playing at Monte Carlo (Essay on "The Scientific Aspect of Monte Carlo Roulette," in *The Chances of Death and other Studies in Evolution*, Vol. I., pp. 51 to 57). The total number of trials examined

was over 32,000, with some very curious and anomalous results, which may be briefly stated as follows :—

(1) In the 32,000 and odd trials, the total numbers of *rouge* and *noir* results were about what they should have been by chance.

(2) In a series of 4,052 throws, the runs of odd and even numbers (*i.e.*, among the numbers 0 to 36 which mark the 37 compartments of the roulette table) were also “obedient to the laws of chance.”

(3) In a series of 16,563 throws examined to find how frequently each of the 37 numbers occurred, (all the 37 numbers should, of course, have occurred about the same number of times), the deviations from the most probable results were in some cases enormously greater than would have been expected theoretically.

(4) Even greater divergences from the most probable results were found in the case of runs of the two colours. The omission of the number zero (which is black) from the calculations made the chances of red or black in the remaining 36 numbers equal, so that the chances of runs of red and black were equivalent to those of runs of heads and tails in tossing a coin. Calling a “set” the number of throws of the roulette ball till a change of colour comes, in 4,274 “sets” (resulting from 8,178 throws of the ball in one fortnight), the “sets” of one were found to be enormously more frequent than they should have been, and the “sets” of two, three, and four enormously less so. The writer sums it up by saying (p. 57): “Short runs are deficient, and the colour changes much more frequently than the laws of chance prescribe”; and he observes (p. 55): “If Monte Carlo roulette had gone on since the beginning of geological time on this earth, we should not have expected such an occurrence as this fortnight’s play to have occurred *once* on the supposition that the game is one of chance.” These remarkable results were still further confirmed by independent investigations of the play of two more fortnights taken separately, and Professor Pearson remarks, after describing the last of them (p. 55): “That *one* such fortnight of runs should have occurred in the year 1892 might be looked upon as a veritable miracle; that *three* should have occurred is absolutely conclusive. Roulette as played at Monte Carlo is not a scientific game of chance.”

CHAPTER III.

EXAMPLES.

SECTION I.

Coincidences clearly due to Chance.

An artificial Chance Series, such as a number of throws of a coin, is in many respects so unlike the events of our ordinary existence that it might be supposed impossible that any analogy could exist between coincidences occurring in the former case and those occurring in the latter.

Yet, from a certain superficial point of view, the whole existence of each individual may be compared with a single "event" in a Chance Series, the successive stages of the existence—like the successive positions of a coin during a single throw—being causally connected together, while a sort of definite break occurs at the beginning and end of the life, as at the beginning and end of the throw. Further, the various positions of the coin are produced by the interaction of several forces or causes, and so are the various stages in the man's life. We may, if we choose, assume that all these stages are rigidly determined by what has gone before, and by the conditions surrounding the man at the moment (all of which, in their turn, may be supposed to be determined by previous conditions); just as we assume that what happens to the coin is the inevitable result of the way it is thrown, etc. In any case, whether we take a strictly deterministic view of the individual's actions or not, there is no question but that they are greatly influenced by one another,—that causation plays a large part, if not the whole part, in producing them, as in producing the successive positions of the coin during a single throw.

The analogy is of course an ancient and familiar one, but if we attempt to carry it far, it soon breaks down. A human life is not an isolated "event," cut off from all other such events by the two epochs of birth and death; the causal influence is continued into the next generation and radiates out into the whole of the individual's present environment, as well as into the future, so that the relation between two contemporary lives is something quite different from the relation between two simultaneous chance series.

This being so, it might appear at first sight that there could be no scope for chance coincidences between the events in two human lives. There are, however, many cases in which two events having some kind of agreement with or similarity to one another are brought about by entirely independent causes—just as the causes that make two coins

turn up heads at the same time are entirely independent of one another—and all such coincidences are due to chance.

Thus, if two acquaintances meet in the street, the meeting may be due to chance, or it may not. It is not by chance if they both have a habit, either from choice or necessity, of going there at that time; or if they are drawn to the spot at the same moment by a common attraction, such as a circus passing by. But if they were accustomed to go out at various times in various directions, their meeting would be due to chance. The probability of the meeting might, of course, be fairly large; the times at which they went out and the directions they took would be limited; and the more similar their tastes were, the narrower would these limits become, and they would consequently meet oftener. In the case of each meeting, we should have to know all the circumstances in order to know whether it occurred by chance or not. In any case, each of the persons would have had some reason for being at the place just then,—some *cause* would have brought him there,—but if there was no reason why both should choose the *same* time, the meeting would be accidental.

We should hardly, however, call such a meeting a coincidence, unless there was some special feature in it that attracted our attention,—something that made us ask why it happened; even though consideration of it might show that there was really no cause for it, that is, that it was accidental.

My first example is a case of this kind, the point of which consists in the meeting of two persons of the same name,—both Christian and surname.

CASE I.

The account is taken from *Notes and Queries*, 5th Series, Vol. XII., p. 111, August 9th, 1879, the writer being the Rev. C. W. Bingham. Another coincidence recorded by him is given below on p. 261.

. . . Till within the last few years, the Clergy List contained the name of a highly respected clergyman, in no wise related to me, but bearing and called by the same first Christian name as myself. We had been introduced to each other, but had had no opportunity of cultivating each other's acquaintance. It chanced that in 1867 I was visiting the Paris Exposition, and passing one morning through the building, my attention was arrested by the kind of trophy of Bibles which was raised in the centre of one of the alleys by the British and Foreign Bible Society. On the opposite side stood an English gentleman, who was engaged in a similar survey, and as our eyes fell from the object of our gaze, they met each other and a slight smile of recognition gradually lighted up both our countenances. We advanced towards each other and shook hands, but it was obvious from his manner that we were in precisely the same case—were aware that we had met elsewhere, but had no recollection whatever as to who we respectively were.

"I beg your pardon," was our common exclamation, "but I cannot at this moment remember your name." I forget which was the first to reply, but the answer was identical: "My name is the Rev. Charles Bingham," and "My name is the Rev. Charles Bingham." . . .

C. W. BINGHAM.

In this episode there are several different conjunctions of events whose antecedent probability might be estimated, and I give the story partly because it illustrates the difficulty of disentangling among a mass of incidents the salient ones for the present purpose. For this, I think the only important point, as already indicated, is the meeting of two persons of the same name. As in other analogous cases (*e.g.*, the case of thirteen trumps in a hand at whist) this conjunction of events, as far as its antecedent probability can be calculated, is no more unlikely than any other similar conjunction. It is not more unlikely for two specified persons of the *same* name to meet than for two specified persons of *different* names. The only point that could affect the calculation would be the question of how common the name was. The account implies that there were only two English clergymen then living of the name of Charles Bingham. Crockford's *Clerical Directory* for 1898 gives seven clergymen of the name of Bingham, among whom is one Charles Bingham. If the name were a very common one, it would, of course, increase the probability of the meeting of *some* two persons of that name (though it would not increase the probability of the meeting of *two particular* individuals of that name). Evidently the particular point in the incident that struck the persons concerned was their both giving the same answer to the same question. But the case depends, I think, as to its antecedent probability, on nothing else than the rarity of the combination of the Christian and surnames.

The next is a case of a remarkable resemblance between a real and a fictitious individual.

CASE 2.

The account is a reproduction of an article called "A Remarkable Coincidence;—Letter from Dr. George M. Beard"¹ in *The Popular Science Monthly*, Vol. XV., (May–Oct., 1879), p. 628.

In the April number of your *Journal* for this year (1879) I discussed the subject of coincidences as one of the six sources of error in experimenting with living human beings,² and stated in substance that this department of

¹ Author of "The Psychology of the Salem Witchcraft Excitement of 1692" (New York, 1882), and "Trance and Muscle-reading" (New York, 1882).

² In an article called "Experiments with Living Human Beings" (*Popular Science Monthly*, Vol. XIV., pp. 611 and 751), containing some useful discussion of the sources of error to which such a subject as psychical research is liable.

logic had been most imperfectly studied, and that the mathematical doctrine of chances especially had been abused and misunderstood, to the great detriment of science. The following very remarkable correspondence illustrates my position so forcibly that I beg leave to present it to your readers.

The first letter is a so-called "April-fool" letter, as the date suggests, and is wholly imaginative. It was written for amusement purely, and obtained a very different reply from what was expected.

The author of the communication is a well-known merchant of this city and a friend of mine. The person who replied is also well known in the region where he resides.

This coincidence is certainly one of the most remarkable of any recorded in the history either of logic or delusions.

"202, Columbia Heights, Brooklyn, *April 1st, 1879.*

"MY DEAR SISTER VELINA,—You will no doubt be somewhat surprised to receive a letter from me, but I have a little matter of business, and if you will attend to it, you will place me under obligations to your good self.

"Some time ago a man by the name of John Nasium lived in New York. His father was a Southerner and died last summer of yellow fever. He had two brothers, James and George. The former some years ago went to California, and the latter, I understand, resides somewhere in Kansas.

"This John Nasium seems to have been the black sheep of the family, and when he left New York, he did not leave a very good record behind him. He went from here to Toledo, Ohio, and afterwards, we hear, he went to Tecumseh, Michigan, no doubt thinking that in a quiet country place he would be more secluded than he could be in a city. I and several of my friends would like to get track of him, if it can be done quietly and without exciting any suspicion. He may have changed his name and so I will describe the man as nearly as I can, which may be some help to you. John I never knew very well, but his brother Jem, as they called him here, I knew very well indeed. John is rather tall, weighing about 180 lbs., I should think. He stoops a little and is slightly lame in the left leg. You would not observe his lameness unless you were to pay particular attention to him while walking. His hair is a dark sandy colour, in fact, almost a red, and his side-whiskers are almost the same colour, but a little darker. He is about 38 years of age, but really does not look over 30. His eyes are a very dark brown, and the left eye looks a little peculiar, *i.e.*, unlike the other—looks as if, some time or other, a cataract had been removed by an operation. To look at him you would at once see a difference in his eyes, and yet I cannot describe the difference any better than I have done. While he lived here, he usually wore his hair rather long and carried himself in a style peculiar to the Southerner.

"Now perhaps the best and most prudent way for you to do would be for you to go up and read this letter to Uncle Hiram first. He is a very careful, discreet man, and he can make enquiries and excite less suspicion than you could.

"I am real sorry to make you any trouble and much less Uncle Hiram, but this is a matter, if it can be properly done, which may be of considerable importance to me and several of my friends, and perhaps further the ends of justice.

"There is one other mark which may aid you, which is—this man was in the rebel army, and *his forefinger on his left hand was shot off*. His nose is quite prominent, and he has a very mild and quiet look, and he is the last man you would pick out for the scoundrel that he is.—Yours very truly,

R. T. BUSH.

P.S.—Please attend to it, and oblige."

Shortly after this letter reached its destination, Tecumseh, Mr Bush received a telegram stating that the man had been found, and asking if they should arrest him. The correspondent had not observed the date of the letter, nor suspected that he was reading a novel; and in a few days the following letter was received:—

"Tecumseh, April 18th, 1879.

"MR. R. T. BUSH, —DEAR SIR,—Velina read to me a letter Wednesday evening from you, describing a certain man that was wanted in New York, who had recently left Toledo for this village.

"The next morning, after hearing the description, I informed our marshal of the fact, and requested him to keep a look-out for such a man. In the course of half-an-hour he came to me, saying that he had just seen my man, —with sandy whiskers, rather tall—would weigh 170 or 180 lbs.,—wearing specs., and the front finger of the left hand missing; and was very anxious that he should be immediately arrested, as he was then at the livery stable, for a saddle-horse to ride away. I told him we had better wait and be sure that he was the one we wanted, and also find out if we could whether you wanted him arrested, should he prove to be the right man. I saw the man and he answered the description so well, even to the *finger*, that I thought it best to telegraph you for instructions. The marshal, in the mean time, was to keep his eye on him (as he failed to get a horse). Seeing him walk down to dinner with one of our townsmen, the first opportunity he made some enquiries of this townsman, and found that he was not the man—that he was the cousin of this man that took him to dinner, and was brother to a Mrs. Palmer, whom he was visiting—that he lives in South Cleveland, Ohio, and is a lawyer by profession.

"That he answered the description, both in size and the loss of the finger, as well as the colour of his whiskers, there could be no doubt. Wearing specs. we supposed was to hide the defects of that eye you mentioned, and he looked as though his side-whiskers had recently been cut or shaved; but if, as we are told, his home is in Cleveland and his name is Hick, why of course we were deceived in the matter. And, if his friend has not informed him, he is still ignorant of our suspicions.

"Now, as this is my first experience in the detective business, you will pardon the blunder. Hoping that it has put you to no inconvenience, I remain, yours, etc.

H. RAYMOND."

The one striking feature of this coincidence is, of course, the *loss of the forefinger* in the left hand.

Both the imagined and the real case possessed this very exceptional peculiarity. This is a subject on which statistics cannot be gained; but it is certain that in the whole continent not a small roomful could be found possessing precisely this deformity at the age specified; and it may well be

doubted whether in the whole world there is another person thus mutilated and at the same time possessing all the general physical characteristics of the individual described in the letter.

More striking still is the fact that this individual did not reside in the place where the letter was sent (which is not a large place) and was there by chance only the day that the letter reached there.

Those who believe that the mathematical doctrine of chances can solve the complex problems of coincidences will find in this case material for consideration. I may here quote a single sentence from the second of my series of papers on "Experiments with Living Human Beings," in the April number of the *Monthly*: "In these and all studies of a like character it is to be recognised that coincidences of the most extraordinary character and astonishing nature are liable to occur at any instant, and that they are as likely to occur on the first trial as on the last of a long series."

A second point of great psychological interest in this case is the attempt made by the person to whom the letter was addressed to overlook certain discrepancies between the imaginary and real individual, and to twist and pervert and reason upon the facts of the case, so as to bring them into harmony with what he was *expecting to see*. While the man corresponded to the description in size, in the colour of his whiskers and especially in the loss of his finger, he did not correspond in the fact that he wore spectacles, and had no side-whiskers. The detective reasoned that he wore spectacles to hide the defect in his eye, which defect he did not see; and he assumed, on thought, that the side whiskers had been recently shaved or cut. Nothing is said of his stooping, or of his being lame in the left leg, or of the colour of his hair, or of its length.

The bearings of this whole history on the delusions of clairvoyance, mind-reading, animal magnetism and spiritism are apparent. A successful coincidence of this kind would have made fortune and favour for any clairvoyant or medium or mind-reader.—Truly yours,

GEORGE M. BEARD.

New York, *July*, 1879.

In addition to the comments of Dr. Beard on this case, it may be observed that the greater part of the general description of the supposed individual—apart from the point of the missing finger—might have fitted very fairly, say, one out of every thirty middle-aged men in England or in America. "He is rather tall—stoops a little—his hair is a dark sandy colour, almost a red, and his side-whiskers are almost the same colour, but a little darker—his eyes are a very dark brown and the left eye looks a little unlike the other." Finally, "his nose is quite prominent, and he has a very mild and quiet look and is the last man you would pick out for a scoundrel." The village of Tecumseh probably contained several persons like this. But Mr. Raymond no doubt knew all his fellow townsmen, at least by sight, and it would never occur to him to compare any of them with the description given. He naturally looked out for the first stranger he could find, and it is not very surprising that the first stranger turned

out to have a general resemblance to the fictitious scoundrel. But it is certainly remarkable that in both of them a finger was missing,—and that the same finger,—a deformity which, as Dr. Beard says, is extremely rare.

The next case—of similar incidents occurring in two contemporary lives—presents a curiously close analogy to the coincidences that may occur between events in two simultaneous artificial chance series. The account was obtained by Dr. Hodgson.

CASE 3.

London, *January 21st, 1898.*

Dr. M. and the undersigned H. W. B. are brothers-in-law. Dr. M. resides and practises in London; H. W. B. lives in a suburb, but is engaged in business in the city. In December, 1897, he was temporarily staying with his brother-in-law Dr. M. The undersigned J. T. H., an Australian on a short visit to London, was detained there by illness. He had not previously known Dr. M. or H. W. B., but in November, 1897, he made the acquaintance of Dr. M., who, on account of the ill-health of J. T. H., was kind enough to invite him to stay at his house. In December J. T. H. went to stay with Dr. M., and there for the first time met H. W. B.

It transpired in conversation that H. W. B. and J. T. H.

1. Were both born on May 13th, 1858.
2. Their christening was in each case delayed until the completion of a new church, one church being in England, the other in Australia.
3. At that time (December, 1897) they each occupied a house (one in England, the other in Australia) which house in each case consisted of two small cottages knocked into one by connecting doorways.

[Signed] { HERBERT WILFORD BRETT.
J. T. HACKETT.

Mr. Hackett is an Associate S.P.R. He is an old friend of mine, and gave me the above account orally, and afterwards wrote it out at my request.—R. HODGSON.

CASE 4.

The following is a series of trivial coincidences recorded by Miss C. Shuttleworth, of 10, Cottesmore-gardens, Kensington, W., who writes that the “coincidences are all strictly true.”

1. On one of the coldest days of the bitter frost in the beginning of 1895 I went, at about half-past six in the evening, to pay a visit to a near neighbour in London. Two ladies were sitting with her—an aunt and a niece—and whilst she talked with the aunt, I had to talk to the niece. When the degrees of cold had been disposed of, I could think of nothing else to say to this stranger than to ask if she happened to have read a book which just then had been much interesting me, “A Colony of Mercy,” being an account of the work of Pastor von Bodelschwing in Westphalia. “No,” she replied, “I have not read it, but I know all about Von Bodelschwing. I am going

there to-night." I thought I must have misunderstood her. "*Going to Bielefeld to-night?*" I exclaimed. "Yes," she said, and glancing at the clock she added, "And I must be off, for my train leaves Victoria at eight." It appeared she had a friend living in the "Colony." The lady of the house had never heard of the place or the Pastor, and I knew no more of her friends than if they had dropped from the moon.

* * * * *

2. From a country house, some years ago, we drove over, a large party, to some distant ironworks. During our walk round them we had to stand for a while by a little railway waiting for an engine to get out of the way. Whilst we did so I asked a man of the party who had only arrived the evening before whether he had ever been on an engine. I forget what he said, but I remember expatiating on the fun of driving one—an accomplishment I had lately practised. He seemed confused, and some elderly ladies who were listening looked shocked, which I fear prompted me to enlarge on the topic. On the way back he confided to me that this was the anniversary of his wife's death. She had died that day three years. In the evening I repeated this to our hostess, who then told me the cause of her death. It appeared that once indeed the poor man *had* been on an engine and it was with his wife. It had overturned going round a curve, and his wife was crushed to death beneath it.

3. Many years ago a neighbour was dining with us. He told a story of Mrs. H., à propos of something that had been said, and stated that he had only *once* met her, more than thirty years before. I asked if she was now Lady H., as, if so, she might be identical with a funny old person whom an old governess of ours used to tell us about, but whom we had never seen. She was a neighbour of hers in a remote village nearly 300 miles away, and we had heard her speak of her kindness and her eccentricity. She had told us that when walking home in an evening she would often accost a stranger, usually a labourer, and saying, "I am Lady H., I will take your arm," would proceed to do so, and thus walk home with him, perhaps a mile or two. And with this anecdote I capped our friend's. But he could not tell us whether she was the same person or not, as he had never heard of his Mrs. H. again.

The next morning, whilst we were at breakfast, Lady H. walked in. She had already paid our friend a visit, having arrived at his house at 8 o'clock. She was Mrs. H., and having been told he lived in the neighbourhood, had walked a couple of miles over the fields to breakfast with him. The temptation to tell her how we had discussed her the night before was too strong for him, and, alas! he could not even resist the diabolical suggestion to repeat my bit of gossip about the labourer. So she came on to inquire how it had reached us. We never told her, though we spent the morning under cross-examination. But she was pacified with luncheon, and being afterwards driven back to the house where she was staying. "It was quite true, my dear," she freely admitted; "and only last night, whilst you were talking about me, I was walking home from the station arm-in-arm with the ticket taker."

4. A few years ago my sister asked me as we sat over the fire whether I ever now heard anything of a Mr. M. I was surprised at the question, as

she had known him less than I had done, and I had only once seen him, and had never heard from him in about ten years. I was not aware that he knew my address. But whilst I was answering her question, he came in. He was living in Cambridge and I in London.

5. "Is not 'Seend' an odd name for a place?" a friend once asked me, as we sat writing letters in Tyrol, she at one end of a long room, and I at the other; "Yes," I said, "but how in the world did you know?" "Know *what?*" she inquired. "Why, that I have this instant directed an envelope *there!*" It then transpired that her uncle had just taken a house in the village in which a friend of mine was living, but neither of us knew that the other had ever heard of the place.

C. S.

In reply to a request for corroboration of these incidents, in so far as it could be had, Miss Shuttleworth writes to me:—

June 22nd [1898].

. . . My sister, Mrs. Moore, says her recollection of the No. 4 coincidence is so vague that it would be no use her saying anything in corroboration. That is to say, she remembers it now I have reminded her of it, or thinks she does, but at first could not recall it at all. I find it must have happened in 1886—longer ago than I thought.

No. 3 she remembers perfectly. The "friend" on whom Lady H. first called is dead, and none of his family were at home at the time. That was why he dined with us most evenings.

My friend Mrs. Russell, of 9, St. Albans Road, Kensington, has sent me the enclosed letter from *her* friend, the "aunt" of No. 1. As you will see, she fully corroborates it so far as she goes, though she evidently thinks I was requiring information about Von Bodelschwingh's institutions. Her niece is in America, but I remember she told me that she was taking Bielefeld *on her way to Leipzig*, and was to stay there about a fortnight. I have never seen her or her aunt since. The *whole* of the aunt's letter was not sent me, so there is no signature.

The smaller enclosed note concerns No. 5. That is all I can do.—Yours faithfully,

C. SHUTTLEWORTH.

Miss Shuttleworth's sister, Mrs. Moore, writes:—

I remember Lady H. suddenly making her appearance under the circumstances described in No. 3.

AGNES E. MOORE.

The letter referred to in connection with coincidence No. 1 is as follows:—

May 31st.

I see on referring to my diary that on January 1st, 1895, I and E. C. had tea with you, and I saw E. off for Leipzig by the 8 o'clock train from Victoria. I remember meeting Miss Shuttleworth at your house, but cannot speak as to the date. E. is now in America. . . . E. C. has a brother who has been several years at Bielefeld, and E. has visited him there several times. . . . I have the book "The Colony of Mercy," and would lend it to any one who wanted to see it. . . .

The note referring to coincidence No. 5 is as follows :—

29, Cyril Mansions, Battersea Park, S.W., June 18th.

. . . Yes, certainly I can verify No. 5. I remember it perfectly, and you are quite accurate in what you say. . . .

ETHEL MOFFATT.

CASE 5.

A further coincidence in relation to Miss Shuttleworth's account (No. 2) remains to be mentioned. A portion of the present paper, including her narrative, was read by Dr. Hodgson at a general meeting of the Society for Psychical Research on April 22nd, 1898, and the next day he received the following letter :—

April 22nd [1898].

DEAR SIR,—Having been at the meeting this afternoon, at Westminster Town Hall, I write to tell you about a further coincidence in connection with one of the examples recorded by Miss Shuttleworth. The gentleman with whom Miss Shuttleworth spoke upon the subject of driving engines was, I feel almost sure, my father. The lady who lost her balance while rounding a corner, and who was killed on the spot, was my mother! It was quite a chance my being at the meeting this afternoon, as I am not a member of the Society, and it will probably be the only meeting I shall be able to attend this year, as I live altogether in Ireland. On coming back here this afternoon I said to my father, "Did you ever meet a Miss Shuttleworth?" and on hearing he had done so, I proceeded to tell him of this afternoon's meeting. He says he remembers having spoken to Miss S. about engines. It would be a satisfaction to discover for certain whether the gentleman in question was Captain — — whose wife, Mrs. — —, was accidentally killed from a traction engine at — — in 1869, August 31st.—Yours very truly,

[Signed] — —.

Dr. Hodgson asked if Captain — — would himself furnish a statement of the circumstances, and Miss — — replied :—

April 25th, 1898.

DEAR SIR,—I am very curious to know if it was my father to whom Miss Shuttleworth alluded : Captain — — (now Lord — —).

I hardly like to ask him to write down a statement ; first of all, because he does not like, even now, speaking of the accident, as he is of a highly nervous temperament, and secondly, because his memory is not as good as it used to be, but I gather that it was at the house of Mr. — —, — —, that he met Miss S., to whom he remembers speaking on the subject of my mother's death.

It was either in '71 or '72. My mother was on a small traction engine built by her cousins, the present Lord — — and his brothers, and in turning a corner, she put up her hand to steady her hat, and in so doing, she lost her balance and fell off the engine—being killed on the spot.

I thought it odd the other day that this should be alluded to at the one meeting of the year at which I happened to be present, and that the subject of the meeting should be "Coincidences."

We wrote to Miss Shuttleworth to enquire if it was Captain —, now Lord —, to whom she had referred in this case, and she replied in the affirmative.

CASE 6.

The following case was contributed to *Light* (Vol. X., 1890, p. 371), by Mr. C. C. Massey. He says:—

At the Board meeting of the Brecon and Merthyr Railway¹ last month, a serious and fatal accident to a train of the Company, owing to a “wild run” down the incline at Tallybont, on December 2nd, was reported. At the same meeting also was reported the death, within an hour and a half of the time of the accident, of one — Thomas, who had been in receipt of an allowance of seven shillings a week from the Company ever since 1867, in consequence of injuries sustained by him in that year from an accident to a train at the very same spot and from the very same cause.

In this case the coincidence of the man’s death at the time of the second accident was clearly due to chance; but it was not by chance that the two accidents occurred to the train in the same place, since they were due to the same cause.

On the other hand, when a somewhat rare event occurs several times,—either simultaneously or in rapid succession,—being due to a *different* cause each time, the repetition is an accidental coincidence. The following are instances of this type.

CASE 7.

In *Time*, of September, 1890, (New Series, Vol. II., p. 978), occurs an article by Mr. A. W. Holmes-Forbes on “Contemporary Coincidences,” containing a curious collection of groups of similar events occurring at about the same time in various parts of the world,—mostly in England. These are extracted from newspaper reports, viz., from the *Times*, *Daily News*, *Echo*, *Standard*, *Daily Telegraph*, *Globe*, and *Evening Standard*, and are not confirmed in any way, but one may probably assume that reports of events such as these are accurate in the main.

The following is a summary of the events reported, with their dates:—(1) Fall of buildings, five cases, July 22nd to 24th, 1885; (2) burning of churches, five cases, December, 1886, to February, 1889, two occurring on the same day of February, 1889; (3) fall of bridges, two cases, on September 16th, 1889; (4) theatres burned, three cases, January 1st-5th, 1888; (5) explosions—mostly serious ones—seven cases, October 6th to November 30th, 1888; (6) burying empty coffins, three cases, December 17th to 23rd, 1888; (7) great finds of honey, two cases, September, 1888; (8) marriages interrupted, two cases, February,

¹ I was then a director of that Company and was present.

1886 ; (9) women in men's clothes, three cases, March and April, 1889 ; (10) actions for libel, nineteen cases in the Michaelmas Sittings of 1889—all apparently in London ; (11) similar robbery of lawyers, two cases, on August 16th, 1886 ; (12) death in the legal ranks, four cases, March, 1883 ; (13) constables in trouble, three cases on August 20th, 1887, and three on November 30th, 1889 ; (14) riots, six cases, July 24th-31st, 1885 ; (15) discovery of prehistoric remains, one in the United States and one near Milan, November, 1889 ; (16) asylums burned, two cases (in America), May 8th, 1890.

The writer says that these sixteen series are taken from a list of more than 140 similar groups, in some instances more than twelve serious accidents of the same kind (other than wrecks) having occurred within a few days ; and, in conclusion, he observes :—"Taken all together the catalogue seems to contain some element beyond that of mere coincidence." He explains that his collection is merely the result of cases taken at random in the course of his ordinary newspaper reading, and states, "We are not coincidence-hunters. Not one coincidence has been the result of searching."

There is a certain analogy between incidents of this kind and repetitions of the same event in an artificial chance series ; *e.g.*, suppose, in tossing a coin, the sequence of seven heads followed by three tails occurred twice running. This sequence is likely to occur twice running in a series of 1,048,576 throws. But it is, of course, not more likely to happen at one time than another in the series ; so that it might happen during the first hundred throws as well as at any other point. Regarded as part of a series of 100 throws, it would be highly improbable that it should occur at all ; while, regarded as part of a very long series, it is not improbable.

Now, there is always something arbitrary about the length of a series to which we choose to assign a certain event or sequence of events. Even if we are considering an actual series of a certain finite length, it is always possible to regard it as part of an imaginary longer series ; just as an actual line may be treated geometrically as part of an imaginary longer line. The whole conception of the events as constituting a series is, in fact, an arbitrary one,—which we adopt for convenience, because this is the only way in which it has been found possible to formulate any principles with regard to the events. One of these principles is that, if we wish to be able to judge whether certain events have actually occurred by chance or not, it is necessary to take an actual series so long that the events in question are likely to have occurred by chance a good many times in the course of it. The greater the probability of the events, therefore, the more practically reliable is any evidence for a cause which depends on the frequency of their occurrence. The "residual cycles" in the chance series

described in Chapter II., p. 190, afford an instructive illustration of this point. These were sequences of events having a very small probability, and it was shown that their occurrence in the series in question proved nothing.

Still less is it possible to draw any conclusion from the repetitions of events described by Mr. Holmes-Forbes in the above narrative; first, because it is clear that their antecedent probability is comparatively small; and, secondly, because there are no grounds for judging what length of series they could reasonably be regarded as belonging to.

SECTION II.

Coincidences with a doubtful claim to causal production.

(A) *Cases suggestive of "Design."*

In describing the classification of Coincidences here adopted, I referred to incidents involving an unusual combination of circumstances, whose characteristic is that they are specially convenient for some person or persons,—something that would be called particularly "lucky" for them. The word "lucky" implies that the conjunction of circumstances is thought accidental. Yet, if it implies something of real moment to the persons concerned, it can hardly fail to suggest—to them, at least—that it did not come about by chance, nor through the ordinary course of events causally connected together, but was due to a special intervention on their behalf,—a sort of slight bias given to the circumstances by an overruling Design. There is no doubt that this idea is distinctly suggested by such cases as the last one I give in this section, and by other cases (not given here) which are regarded as answers to prayer.

I do not, of course, put forward the cases cited, or the comments made on them, as any contribution towards settling the question whether such a special intervention ever really occurs. They are intended merely to illustrate the difficulties met with in considering the question. The first of these is that the "lucky" incidents are sometimes so trivial that it seems absurd to attribute any importance to them as evidence of anything; while there is no obvious point at which to draw the line between these and the more momentous ones.

Again, very "lucky" events generally consist in the extrication of somebody from some unpleasant or dangerous situation; and then the evidence for special intervention on his behalf is certainly dubious; since as much seems to have done against his interests in involving him in trouble, as for him in taking him out of it.

Both these considerations would tend to show that what had happened in all the cases supposed was part of the ordinary course of events, and that no special intervention had taken place.

We need not, however, hold that Design operates by any kind of interference at the moment, but might rather hold that all events in the universe were pre-arranged by it from the beginning. The only essential feature in the conception is that things are, in one way or another, arranged with some purpose—some end in view. We can hardly in any case suppose that Design is only, or chiefly, concerned with “lucky” or “unlucky” events. If we believe in it at all, we can hardly help believing that it takes cognisance of everything that occurs. But it might easily happen that only a certain number of all the events that occur were what might be called “evidential” of Design, just as believers in telepathy generally hold that it occurs frequently, but only now and then in a manner susceptible of proof.

It is these apparently “evidential” cases that may be instructively compared with the events that afford evidence of human design. If some one wins by cheating in a game of chance, the fact of the results being specially fortunate for him may, under some circumstances, lead us to guess that he is cheating. Similarly, when circumstances beyond human control concur in an unusual manner to produce some great benefit to a human being, this may suggest to us that a superhuman Design is working for the benefit of that person.

In the case of the fraudulent player, we might find conclusive proof through detecting the agency at work,—a kind of evidence which we can hardly conceive of obtaining in the other case. But we might also find evidence of what the fraudulent player was doing by observing that in a very large number of instances, the results of the game differed from what would be expected to occur by chance. This would only make it more or less *probable* that some cause, not chance, was acting, and a very large number of instances would be necessary for practical certainty. Even then we should have to investigate further to discover what the cause was.

The consideration of games of chance and the like has no very direct bearing on the question of the existence of a superhuman Design. But without some knowledge of what can be, and in fact constantly is, produced by chance in such cases, we should hardly be able to realise what a multitude of incidents,—incidents either specially “lucky” or specially “unlucky” for some individuals—might occur by chance alone, and this ought to make us very cautious in attempting to found arguments on any particular instances.

There are other kinds of Coincidences which, on somewhat different grounds, are occasionally regarded as indicative of Design in the

universe ;—namely, coincidences in which events of a certain order seem susceptible of a special interpretation having relation to other events, apparently unconnected with them and of an entirely distinct order. It has been a favourite idea of mystics in all ages that most events are symbolic of something else, which can be discovered through the use of a sort of hypothetical cipher,—the great object of ambition being to find out the universal cipher that will fit everything, and in the light of which everything would seem, as it were, to make sense.

The cipher must be supposed sometimes to take a very simple form, such as the famous anagram: *Horatio Nelson = Honor est a Nilo*. Here it would have to be supposed that Nelson's parents were guided to give him the name in a spirit of unconscious prophecy. But the practical certainty that the coincidence was accidental is shown by the fact, which any one can verify for himself, that there is little difficulty in making more or less appropriate anagrams out of almost any names, since most of them contain a fair proportion of vowels and the common consonants, while the very large number of incidents in any one life afford unlimited scope for choice of words to compose the anagram.

In many instances some special significance has been attached to certain *numbers* as portents, the numbers having attracted attention for some reason ; for instance (as in the case of Mr. Massey and the number 99, given below), on account of their being associated several times with similar events. Or in some cases the same number is produced repeatedly in several different ways, *e.g.*, in adding up the digits of different dates, chosen for some reason irrespective of the numbers. (See Case 10, below.) The histories of all religions are full of this tendency to attach special significance to all kinds of objects or incidents which have been supposed to give information or supernatural guidance.

Astrology and Palmistry form some of the best known historical instances of this. The ancient astrology of course involved, and was to a great extent based on, the belief that the earth was in all respects the centre of creation, so that there seemed to be nothing unreasonable in the idea that one of the main functions of the stars was to record its history or to guide men in their affairs. Apart from this obsolete belief, the theory depended for its philosophic basis on the notion of a pre-existing (or co-existing) coherence of all phenomena,—the universe being regarded as a continuous structure, every part of which depended on, or was determined by, the rest, and might therefore be supposed to explain the rest. On this assumption, the universe as a whole *might* be empirically recognisable in the relative positions of stars, or the flights of birds, or the lines in a human hand or anything else ; complete

knowledge of any one portion of the universe both requiring and involving complete knowledge of all the rest of it.

This main principle is not only generally accepted in the present day as an axiom of science, but is formulated in a much more precise manner than formerly. But it is difficult to realise exactly what it means when we come to apply it to specific sets of facts. We are accustomed to state it confidently in complicated cases where we are too ignorant of the circumstances to permit of a definite contradiction of the assumption (*e.g.*, in the case of the "flower in the crannied wall.") But there are many simple cases that we know enough about to see that complete knowledge of one part does *not* necessarily involve complete knowledge of the whole. For instance, complete knowledge of what has happened in ten throws of a coin will not tell us what will happen if we go on throwing. Again, the most perfect knowledge of one side and one angle of a triangle will not suffice to tell us the form of the whole triangle. The whole science of Mathematics rests, of course, on the fact that certain things are involved in certain others, so that knowledge of the former is implied in knowledge of the latter; but the science shows exactly what and how much is required to be known in order to give certain further knowledge.

The science of Mathematics owes its certainty to the fact that the objects of which it treats have no real existence. There is thus no possibility of the disturbing influences that may come in in the case of real events that succeed one another in Time—events that have to one other the relation of what we call cause and effect. The absoluteness of Mathematics depends on the assumption of the absence of any disturbing influences; it treats merely of what would happen if nothing interfered with whatever forces are taken into consideration. It is conceivable that all successions of real events might be predetermined from the beginning,—as they are assumed to be in Mathematical problems,—with no disturbing influences ever coming in; also that each portion might involve and be involved in the rest with the same inevitableness that, say, the circumference of a circle involves its radius. But we cannot prove the absence of all disturbing influences; nor are we justified in assuming that any one portion, if sufficiently known, would give us knowledge of all others.

In the first case given in this section, the repeated association of a certain number with trivial incidents, occurring spontaneously, suggested to the person to whom they occurred the action of an external Intelligence.

CASE 8.

The following case was contributed by Mr. C. C. Massey to *Light* of November 14th and 21st, 1885. It must be observed that Mr.

Massey does not represent the case as valuable evidentially, but merely as illustrative of a certain class of experiences.

November 1st.

I am so little of a psychic that I know nothing of my somnambulant consciousness in general, and though once or twice before I have, while falling asleep, detected something like an attempt (quite inarticulate) at a whisper, nothing at all similar to the experience of a few nights ago had ever occurred to me. . . . Certain numbers haunt me; they have very distinct associations for me, but into these I will not enter here. Suffice it to say that one of them especially recurs in a certain month of the year, now just past, and even on certain days of that month. Latterly, to narrow the problem of coincidence as far as possible, I have refused to take note of mere multiples, and have even only given special heed to a certain high number, [99] being the repetition of the root number [9]. This number is presented to me in various objective and seemingly accidental ways, often directly, but more usually by the following mode. My attention will be directed to some slight incident readily to be characterised by one word or two words. Now, though the letters of the modern derivative alphabets are not natural numbers, conventionally, so to speak, for this purpose they may be treated as having the numerical value of their order; thus, a = 1, o = 15, z = 26, etc., in the English. It frequently happens, no doubt, that the same incident may be equally well characterised in brief by *different* words or expressions, and then, of course, the exact summation of the letter values of one of them to the required number is less remarkable, the favourable chances being multiplied by the alternatives. In every case, however, the natural odds remain largely adverse. But as, when attention has once been drawn to this sort of sympathy (or whatever it should be called) others may seek it in their own experience, I should add that any seeking or solicitude is perfectly idle. To be always on the look-out for such coincidences means spending half the day in doing silly addition sums. Attention must be awake to them, of course, for the incidents will be mostly trivial in themselves. But when the *method* has been discovered—and how it occurred to me would take long to tell—it is only necessary to remember that if there is anything in it at all, attention *will be* arrested sufficiently often to make it almost impossible to doubt a significance. The condition seems to be some true and deep feeling concerned. To such, apparently unconscious sympathies respond. Mere idle curiosity, or scientific interest, will not elicit them. And now, having premised an explanation which seemed necessary, I will come to the occasion of my hearing the “voice.”

It was at Eastbourne, October 23rd. I had been rather preoccupied all day, and late in the evening, having finished some writing, I took up a book and lit a pipe. It then occurred to me that the “number” had not that day been given. I felt rather disappointed, when in a few minutes one of the little, commonplace incidents with *the* numerical value occurred in the room where I was sitting with no other companion than a kitten (who, by-the-by, had nothing to do with it). I made the calculation, and soon went on with my book. Later I went to bed, my thoughts occupied with certain abstract speculations unconnected with these experiences. In bed I was still drowsily meditative on the same ideas, when suddenly, to some

internal sense of hearing, *perfectly* articulate, yet with a sort of quick, jerky intonation, suggestive of a spasmodic, but successful effort to overcome obstruction or difficulty—a *breaking in*, as it were—sounded two whispered words. Two more common or simple words are not to be found in the language (one was only a preposition), nor more utterly foreign to any conscious context in my mind. For I was *not* asleep, though drowsy. They roused me instantly, and almost instantly, struck with the absence of any intrinsic sense for me in them, I said to myself: “they will sum out my number.” The calculation was quickly made and *the result was exactly* as anticipated.

[Mr. Massey informs me that the words were “To dinner.”]

Whatever the explanation, the case certainly has a psychological interest, for it can hardly be ascribed to chance coincidence. Allowing that a sub-conscious dream faculty—the “masked” consciousness—could start an audible hallucination of spoken words for the waking consciousness—(the proof that I was not asleep is in the fact that a course of waking, though weak, thought was distinctly *interrupted* by the sound, and I could clearly recall that course of thought)—it is not for a moment to be supposed that the numerical coincidence was accidental. It may be suggested that I had sub-consciously gone in search of words suitable to my purpose; I can only say that *consciously* the process of search would be long and tiresome, as I have tested by actual trial. So that the least the case would prove would be the instinctive superiority of the sub-conscious faculty of calculation, coupled with a power to project the result as a hallucination into a waking consciousness not in the least habituated already to such hallucinations.

I record this case because I can bring it to book for others better than the other modes of my numerical experiences. For myself, those other modes are alone significant, on account of the objective character excluding deceptive agency. In the case in question, even supposing that a spirit (perhaps an “elemental”) impressed my sense of internal hearing with words of the numerical value, that value would have little more significance for me than if some friend—or some one wishing to play upon a weakness—purposely suggested such words to me. The marvel is when strangers, letters, chance occurrences, incidents wholly undesigned, and which, in the nature of things, must have been determined by endless antecedents, co-operate for a result having a particular subjective relation, so that it is the time problem that here presents itself, but with far greater complexity than in the case of prevision. Nor have I anything to urge against those who altogether decline to accept an indefinite statement of such experiences. They are only adverted to here in order to explain the principal incident.

C. C. M.

November 14th.

Here is a case in which a numerical coincidence, I think of a very extraordinary character, was actually predicted by myself, and that not by any occult prevision or impression, but simply by inference from a large experience of similar instances. Those who read my communication in last week's *Light* will remember the reference there to a certain high number, to which the letter values (ascertained by alphabetical order), of words

suggested to me in a particular relation, sum up. For better appreciation of the following case, I will now mention that this high number is 99—the repetition of the root number 9.

Last night, I took up my new, uncut copy of *Notes and Queries*, and glanced at the contents page—having seen no other. One of the topics indexed was “Smoking in Church.” This immediately recalled to my mind the incident, heard long ago, of a young pedestrian on the Welsh hills, being caught in a storm, finding shelter in a little church or chapel, and solace in a cigar there. The recollection belonged to the personal associations for me of the aforesaid number, and assuming that the “Note” in *Notes and Queries* would be of some specific instance, I said to myself (there was no one else to say it to), with a confidence amounting to positive assurance, that the name of either smoker or church would sum out the number 99. I then turned to the place referred to (*N. and Q.* 6th S. XII. 385), and found an account, in a letter of the last century, of how a certain *Archbishop Blackbourn* (so spelt) of *York* had ordered pipes, tobacco, &c., into the vestry of a Nottingham church for his refreshment after a confirmation! Now, if any reader will take the trouble to make the calculation, he will find that “Archbishop” sums to 99, “Blackbourn” sums to 99, and “of York” sums to 90! From experiments I have made with books and directories, I believe the odds against any word or name taken at random giving a particular number to be considerably larger than the odds against naming the particular card to be drawn from a pack of 52. Indeed they are probably not much under 100 to 1.

The first word of the title of the book cited for the account in *Notes and Queries* is “Letters,” also = 99.

I know well that readers will be inclined to suspect that I have overrated or overstated my expectation of this result. No one but myself can know how jealous I am of self-deception, or how used I am to instant analysis of my own feelings and beliefs. But make what abatement you will on this account; there remains the fact, quite independent of the degree of my expectation, that I was led to seek and to find a very improbable numerical result in particular words of a particular passage out of the whole contents of the paper.

C. C. M.

I have referred to the number of *Notes and Queries*. The “Contents” with the item: “Smoking in Church” is on p. 381, so that it would be impossible to see the “Note,” which occurs on p. 385, while looking at the “Contents” page, and Mr Massey informs me that the copy was uncut.

In answer to questions, Mr. Massey has kindly given me the following further information:—

124, Victoria Street, S.W., August 11th, 1898.

I made brief notes of many of the incidents in question at the time of their occurrence. But they were of little evidential value, being merely memoranda of dates and words, to enable me to recall the occasions, but giving no detail, usually, of the latter. And they have long been destroyed. . . . So I can only repeat generally what I said in *Light*, that the number

was given especially at the (for me) significant season of the year, in quick repetition, by "strangers, letters, chance occurrences, incidents wholly undesigned." Sometimes three or four or even more such occasions would occur in the course of a single solitary walk (usually across country from Eastbourne). To give particulars from memory at this distance of time would be useless, even if I could recall a number of them (some, I certainly can), because you could not "control" my statements. At first I took note of 9 and its multiples; but not being able to assign any ratio of probability to the frequent recurrence, I determined to confine my attention to the less likely repetition, 99. The sort of thing went on day after day, more or less, for weeks, and in successive years at the same season, for some four or five years; now, hardly ever. . . .

C. C. MASSEY.

August 19th, 1898.

. . . The brief memoranda—words I jotted down immediately after the occurrences—were merely that I might know accurately at the end of each day how frequent the coincidences had been. Otherwise they would not help you now, even had they been kept, as particulars would still depend on my present recollection. But I am sorry that they are not extant, because it was just the *frequency* of the coincidence in a brief period that in my view excluded any merely subjective explanation. Obviously, the latter is not available, if the frequency of objective facts with the numerical signification is in excess of any reasonable estimate of what might be ordinarily expected daily for, say, a few successive days. My subliminal self would be dependent on external circumstances for the selection, and it was the frequency of circumstances lending themselves to such selection that I say is beyond common probability. We can test this to some extent by experiment, and I did so. Often, moreover, only common attention by a mind *openly* on the alert (as mine was) was requisite, the significant number presenting itself without any calculation, *e.g.*, in ticket numbers, numbers of railway carriages (entered before the number was seen), the number given in the course of a letter from a correspondent wholly unaware of its significance for me, and so on. In other cases, no doubt you would have to be satisfied that the *key words*, so to speak, with the numerical value, were in all cases the simple and obviously descriptive ones, not forced for the purpose. . . .

C. C. MASSEY.

The explanation of this case that first suggests itself is obviously the one referred to by Mr. Massey—that he "had subconsciously gone in search of words suitable to his purpose." The experiences might then be considered akin to the *anagrams* sometimes occurring in automatic writing,—a striking case of which is given in Mr. Myers' paper "On a Telepathic Explanation of some so-called Spiritualistic Phenomena" in the *Proceedings S.P.R.*, Vol. II., pp. 225–231. The particular interest of these anagrams is, as Mr. Myers says, that "the result of the involuntary movement of the pen is altogether puzzling to the writer,—is something which he has to make out with difficulty as if it were the product of another brain." They were "anagrams,

indeed, of the crudest kind, consisting of mere transpositions of letters, but still *puzzles* which the writer had to set himself to decipher *ab extra*. The chances against drawing a group of letters *at random* which will form several definite words and leave no letters over are, of course, very great indeed." An instance was "Tefi Hasl Esble Lies" (Life is the less able).

In cases like this, we may suppose that the real order of the mental processes involved is the reverse of the apparent order. In inventing a riddle in the ordinary way, we first invent the answer and then concoct a suitable question; but in propounding it to another person, we put the question first. In dreams it sometimes occurs that we ourselves have a riddle propounded to us; we then seem to hear the question first, with no consciousness of knowing the answer, which comes to us later as a surprise, although we must really have known it beforehand. Such a dream is "dramatised backwards," like the example given below (see p. 263). The apparent mystery in these anagrams is of the same kind; it consists in the writer seeming to put the question to himself and subsequently finding out the answer, which he must have known subconsciously all the time.

Similarly, in Mr. Massey's case, his subliminal self may have started with the number 99 and made up words, *e.g.*, "To dinner," whose letters summed up to it, presenting the finished result of its labours to his supraliminal self. To the latter, this might appear like a riddle propounded by some external source, the answer to which it worked out and found to be 99. The explanation depends on the assumption that mental processes can be apparently reversed as described, but it seems to me that the large number of cases that can be thus explained justify the assumption.

It is not even necessary to assume the apparent reversal in all the instances referred to by Mr. Massey. In cases where an incident occurred that could be described briefly in several different terms or expressions, the subliminal self might be supposed to select the term suited for its purpose and to reject altogether cases in which it could find no suitable words. This would merely imply that the finished result alone would be presented to the supraliminal self, the mental processes leading up to it being performed unconsciously. The cases where the number was presented directly, as on tickets or railway carriages, might be merely instances of selective attention to it. (The incident of "smoking in church" cannot be thus explained, but a single case like this cannot, of course, be regarded as evidence of anything beyond chance.)

All this implies no further assumption than that Mr. Massey's subliminal self acquired a special readiness in playing a sort of game,—rather more difficult than the making of anagrams, but a person who has

spent a little time over puzzles of this kind soon acquires what seems to one totally inexperienced in them great quickness and skill. With regard to the number 99, it will be seen at once that at least 7 or 8 letters would generally be required to form it, since—if all the letters of the alphabet occurred equally often—13 would be the average value of any letter taken at random. But more than 8 letters would generally be wanted, since the letters in the first half of the alphabet occur much oftener than those in the second half. That this is so with *initials* can be seen in any dictionary, *e.g.*, I see that the first 13 letters—A to M—take up about $\frac{7}{12}$ of the space in an old edition of Johnson's Dictionary. This shows roughly the actual proportion of words beginning with these letters. The greater *frequency* with which such words occur may be shown roughly by the fact that the same initial letters take up 16 out of the 24 volumes of the *Encyclopædia Britannica*. (It is instructive to observe that the letter M—which Napoleon is said to have regarded as having a special significance in his life—takes up a little more than two out of these sixteen volumes). Words clearly too long or too short for the number would be at once rejected, and it is not very difficult to imagine that the subliminal self could memorise all the letters of the alphabet so as to substitute at once for the letter its numerical equivalent, and add up the numbers rapidly whenever it saw that they would come to something like 99. The interest that Mr. Massey had come to attach to the number might furnish a fully sufficient motive for the exercise, which otherwise would have seemed trivial and childish.

In the next case the apparently significant numbers were presented in a simple and direct fashion.

CASE 9.

The account is translated and slightly abridged from the *Annales des Sciences Psychiques* for March-April, 1899.

The narrator, M. E. Desbeaux, writes to Dr. Dariex that these cases are extracted from his private diary, under their respective dates; that he has been to Monte Carlo twenty-three times, and has never had any other premonitions.

His account is as follows:—

Case 1.—On Wednesday, February 6th, 1889, at Monte Carlo, taking a walk in the afternoon, I found a rusty horse-shoe. Being superstitious, like all gamblers, I picked it up, since—as every one knows—a horse-shoe, especially a rusty one, brings luck! On standing up again my eye fell on the number 6, painted red on a mile-stone. I said to myself, "That is the number to bet on to-day," but I had hardly finished making this reflection when I found myself gazing at the number 28 imprinted on the lamp of a carriage that had stopped in front of me. I thought at once that, if I saw the 6 turn up on one of the roulette tables, I ought *next* to stake on the 28.

Two hours after this little incident, which I had almost forgotten, I went into the gambling saloon. I went up to a table, and almost at once saw the 6 turn up. Then, but only then, I remembered that I ought to stake on the 28. As I had been losing for several days, and had but a very limited confidence in my prognostic, I hesitated, and instead of staking a *louis* on the 28 alone, I merely staked 5 *francs* on the six numbers (*transversale*) 28—33. The croupier threw the ball, and the 28 turned up.

Case 2.—On Tuesday, December 26th, 1894 . . . just as I was coming out of [a house] I was obliged to start back out of the way of a carriage, with lighted lamps, driving hurriedly by. The number of this carriage—22—was flashed on my eyes. Then it occurred to me that this was the first number I had seen since [an incident which he had amused himself by regarding as a lucky portent], I entered the Casino, but there was no prior indication which could serve as my cue (*point de départ*) as in the previous case; it was therefore with no confidence that I placed five or six coins on the 22. This number did not turn up, and I retired. I had dinner and then came back, convinced of the necessity of having a cue, and Destiny not having indicated one to me, I decided in my own mind that this should be the number 1. I then walked about among the tables, waiting for the moment when I should see No. 1 turn up. At the end of an hour, having seen nothing, I went towards the door to smoke a cigarette in the anteroom, when passing by a table I heard the croupier call out, "1, red, odd number, *manque*." Here was my cue. I put down a *louis* on the 22. The croupier threw his ball again, and 22 turned up.

The next day we lunched with our friends, the B.'s, at the Hotel du Cap Martin. The first thing I noticed on the table at which we were sitting was a white card, supported on a copper stand, bearing the number 222 (a number referring to the service of the hotel and restaurant). My superstitious mind was struck with the oddity of finding again under my eyes the 22 which had favoured me the evening before. I wondered if this was not another gracious guidance from Fate, and I thought that the first 2 probably meant that 22 would turn up twice for me that day. These reflections were, of course, made rapidly and internally, without conviction.

At nine o'clock in the evening I went into the rooms, and had hardly approached one of the tables when I saw the number 1 turn up. Then, thinking of nothing but the system which had succeeded the evening before, I put a *louis* on the 22. Then the 22 turned up. "Here is one," I thought, "and if my prognostic is correct, there should be a second." Having picked up my winnings, I left on the 22, as my custom was, the *louis* which had just won. The croupier threw the ball, and called out: "2, black, even number, *manque*." This time my *louis* returned to the bank, and I was going to look for another table where I might see the 1 appear, when I suddenly remembered for the first time the card numbered 222 at the Cap Martin, and I thought at the same time that I had the initial indication which had failed me the evening before; it was this 2, preceding a 22, which should be my cue. I hastened to throw a *louis* on the 22. The ball twisted and turned and fell into the 22. This happened under the eyes of two of my

¹ *Manque* indicates any of the numbers from 1 to 18 inclusive, being contrasted with *passé*, which indicates any of those from 19 to 36 inclusive.—A.J.

friends,¹ who, not being aware of the course of my thoughts, were astonished to see me hit off the precise number twice in three turns—both the winning numbers being 22.

Case 3.—On Friday, February 3rd, 1899, at Monte Carlo, I was walking about before lunch, thinking sadly that ever since my arrival in this beautiful country, namely, for three days, the roulette had been treating me unkindly. Suddenly at a street corner my glance fell on the number 11 painted on a wall. "Can this be an indication of Destiny?" I thought, half sceptically, half credulously. But at once I remembered my two previous cases, and I recognised the necessity of having another indication to act as my cue, as before. "Well," I said, "the first figure that I see shall be this." At once I saw coming towards me an electric tram with the figure 4 on its front, large and standing out clearly. I took note of it.

At about four o'clock I entered the Casino. I walked about among the tables, hoping to see my 4 turn up. At the end of an hour I had seen nothing of it. Becoming bored, I began to play, thinking no more of my prognostic. I played for an hour with 5 franc pieces only, and did not win once. At last, at six o'clock, I went up to a table in the large room, and saw the 4 turn up. There was my cue. I at once put a *louis* on the 11, and the 11 came next.

Satisfied. I went towards the door, but, crossing the last room, I glanced at the table on the left, where there were comparatively few people. At that moment the 4 turned up before me. At once I put a *louis* on the 11, and—actually—the 11 came next! . . .

M. Desbeaux discusses with some ingenuity the metaphysical difficulties of any theory of premonition as applied to these cases; but, curiously enough, among the various hypotheses he suggests to explain them, that of chance coincidence is not mentioned! With a view to testing this, it is unfortunate that his diary—like so many other diaries—recorded the number of hits only, and not the number of misses. Still it is made clear that the latter were very numerous, and the narrative, as a whole, is hardly calculated to increase one's confidence in portents.

There is another kind of numerical coincidences in the collection of which much time and ingenuity has been expended by various persons; I mean, coincidences relating to dates. I give an instance which I have seen quoted more than once as an especially noteworthy one, though, so far as I know, no estimate has ever been made of the degree of its improbability.

CASE 10.

Louis Philippe became King of France in 1830, and abdicated in 1848. There are three important dates connected with his life—

¹ M. H. M., who has recently reminded me of the incident, would, if necessary, bear witness of it, and M^{me} S., his sister. [The names of these persons were given in confidence to Dr. Dariex.]

the date of his birth, of his wife's birth, and of their marriage—the four digits of each of which, added to the date of his accession, makes the date of his abdication; in other words, the four digits added together are in each case equal to the number of years that he reigned, namely, 18. Thus:—

Date of accession ...	1830		1830		1830
Date of birth	$\left. \begin{array}{l} 1 \\ 7 \\ 7 \\ 3 \end{array} \right\}$	Date of wife's birth	$\left. \begin{array}{l} 1 \\ 7 \\ 8 \\ 2 \end{array} \right\}$	Date of marriage	$\left. \begin{array}{l} 1 \\ 8 \\ 0 \\ 9 \end{array} \right\}$
	<hr style="width: 50%; margin: 0 auto;"/>		<hr style="width: 50%; margin: 0 auto;"/>		<hr style="width: 50%; margin: 0 auto;"/>
	1848		1848		1848

A somewhat similar,—but, as will be seen, far less remarkable,—series of coincidences is to be found in the life history of Napoleon III., who became Emperor of France in 1852 and abdicated in 1870. The four digits of the corresponding three dates in his life, added to the date of his accession, come in each case to a year short of the date of abdication; that is, they each add up to 17, which is one less than the number of years that he reigned. Thus:—

Date of accession ...	1852		1852		1852
Date of birth	$\left. \begin{array}{l} 1 \\ 8 \\ 0 \\ 8 \end{array} \right\}$	Date of wife's birth	$\left. \begin{array}{l} 1 \\ 8 \\ 2 \\ 6 \end{array} \right\}$	Date of marriage	$\left. \begin{array}{l} 1 \\ 8 \\ 5 \\ 3 \end{array} \right\}$
	<hr style="width: 50%; margin: 0 auto;"/>		<hr style="width: 50%; margin: 0 auto;"/>		<hr style="width: 50%; margin: 0 auto;"/>
	1869		1869		1869

In the case of Louis Philippe, the coincidences consist in the digits of all the three dates summing up to a given number, 18. This number may be said to be "given," because it is obtained independently of the sum of the digits, namely, from the length of the reign. Thus, three numbers, taken more or less at random, are all equal to a given number.

In the case of Napoleon III., the three numbers are all equal to one another, but are not equal to a given number; the length of reign is 18 years and the digits sum up to 17. The antecedent improbability of their summing up to 17 is exactly the same as that of their summing up to 18 (this would not be the case with all possible sums of four digits, but may be found to be so in the case of the sums 17 and 18, when the first digit is 1, as in all the dates now being considered). But 17 only happens to be the number that they sum up to; it is not a number obtained independently of them, as the 18 is obtained in Louis Philippe's dates.

Taking Louis Philippe's dates we may first consider what is the probability that the digits of the date of his birth should sum up to 18. As he came to the throne in 1830 he must have been born either in the 18th or 19th century, therefore the first digit must be 1 and the second 7 or 8. It will be found that a date whose digits sum up to 18 occurs once in every ten years between the dates 1719 and 1890, which are respectively the earliest and the latest dates in the two centuries fulfilling the condition required, and as it is obvious from the circumstances of the case that the birth must have taken place between these limits, the probability that the digits would sum up to 18 was $\frac{1}{10}$. The date of his wife's birth and of their marriage must also have come within these limits, so that the probability in each case that the digits would sum up to 18 is $\frac{1}{10}$, and the probability that they would do so in all three cases is $\frac{1}{1000}$.

Taking Napoleon III.'s dates, we have to ask what is the probability that the digits of the three dates should all add up to the same number; or, in other words, what is the probability that the sum of the digits of two of the dates should both be equal to the sum of the digits of the third—this sum being 17. A date whose digits sum up to 17 occurs once in every ten years between 1709 and 1880, and as, from the circumstances of the case, both dates must be within these limits, the probability is in each case $\frac{1}{10}$, and the probability of its occurring in both cases is $\frac{1}{100}$.

A little consideration will show that the particular dates involved are specially favourable ones for the occurrence of these coincidences. In each series, all the dates must, from the nature of the case, fall within 100 years of one another. Practically then, it is a question of all the possible sums of the last two digits. There are nineteen possible sums (from 0 to 18 inclusive) of two digits, and a hundred different ways of obtaining them; but while 0 and 18 can each be obtained in only one way, so that the probability of the occurrence of each of them is $\frac{1}{100}$, the sums that can be obtained most frequently are 9 (in ten different ways), and 8 and 10 (each in nine different ways). Now it is either 8, 9, or 10—the chance of whose occurrence is most favourable—that is required as the sum of the two last digits in all the dates (since the first two digits are always 1 and 7 or 8) to make the four digits add up to 17 or 18.

If any argument is needed to show the utter irrationality of attaching any significance to coincidences such as these, one might be furnished by observing that there are cases in which they could not possibly occur. Thus, if a man were born in the first year of any century, say, in 1800, the digits of the date of his birth could not sum up to the same as the digits of any other date in his life, but must always fall below them; or if he were born, say, in 1799, a

coincidence would be almost impossible, as it could not occur before 1889, and so on.

Further examples of a similar kind, to which similar reasoning is applicable, are to be found in the following passage, which is taken from a discussion of "fortuitous coincidences" in Jevons' *Principles of Science*. (Third Edition, p. 263.)

CASE 11.

In historical and social matters, coincidences are frequently pointed out which are due to chance, although there is always a strong popular tendency to regard them as the work of design, or as having some hidden meaning. If to 1794, the number of the year in which Robespierre fell, we add the sum of its digits, the result is 1815, the year in which Napoleon fell; the repetition of the process gives 1830, the year in which Charles the Tenth abdicated. Again, the French Chamber of Deputies in 1830 consisted of 402 members, of whom 221 formed the party called "La queue de Robespierre," while the remainder, 181 in number, were named "Les honnêtes gens." If we give to each letter a numerical value corresponding to its place in the alphabet, it will be found that the sum of the values of the letters in each name exactly indicates the number of the party.

The remaining cases I give in this section relate to "lucky" events of greater or less importance to the persons concerned.

CASE 12.

The first is a case of scientific discovery being hastened by a fortunate coincidence,—that of an Egyptian papyrus and a Greek translation of the same papyrus coming into the hands of the investigator, Dr. Thomas Young, through two independent sources, almost at the same time. The narrative is taken from Dr. Young's *Account of some recent discoveries in Hieroglyphical Literature and Egyptian Antiquities* (London, 1823). He was engaged in 1814 in comparing and deciphering the two forms of Egyptian inscriptions on the "Pillar of Rosetta" (commonly known as the Rosetta Stone). He had made some progress with the work, and—

In 1821 he discovered "at Leghorn among a multitude of Egyptian antiquities belonging to Mr. Drovetti, the French Consul at Alexandria, a stone containing an enchorial¹ and a Greek inscription, which was known to have existed formerly at Menouf, but which had been lost and almost forgotten by European travellers in Egypt and I believe by Mr. Drovetti himself." Drovetti refused his request to be allowed to take a copy of this. Soon after occurred (*op. cit.*, p. 38) "the arrival of Mr. Casati at Paris, with a parcel of

¹ Young used the term "enchorial" because this form of writing is called in the Greek *Enchoria Grammata*. Champollion preferred the word "demotic," or writing of the people, and the latter term is used in the description of the Rosetta Stone in the British Museum.

manuscripts, among which Mr. Champollion discovered one that considerably resembled, in its preamble, the enchorial text of the Pillar of Rosetta ;" (p. 43) "Mr. Champollion had ascertained the analogy of one of the manuscripts, purchased of Casati, to the enchorial inscription of Rosetta, and he had obtained from it without difficulty, the mode of writing the name Cleopatra in that character. He did not, however, then mention to me the important consequences which he had derived from this discovery." . . .

Later came (p. 55) "an event which is the most important, considered as a single occurrence, that has taken place since the commencement of my Egyptian researches. It was very soon after my return from France that George Francis Grey, Esq., of University College, Oxford, having been at Naples upon his return from Egypt, . . . had the kindness, on the 22nd of November last, to leave with me a box containing several fine specimens of writing and drawing on papyrus ; they were chiefly in hieroglyphics, and of a mythological nature ; but the two which he had before described to me as particularly deserving attention, and which were brought, through his judicious precautions, in excellent preservation, both contained some Greek characters, written apparently in a pretty legible hand. He had purchased them of an Arab at Thebes, in January, 1820 ; and that which was most intelligible had appeared, at first sight, to contain some words relating to the service of the Christian church. . . .

"Mr. Champollion had done me the favour, while I was at Paris, to copy for me some parts of the very important papyrus, which I have before mentioned as having given him the name of Cleopatra ; and of which the discovery was certainly a great event in Egyptian literature, since it was the first time that any intelligible characters, of the enchorial form, had been discovered among the many manuscripts and inscriptions that had been examined, and since it furnished Mr. Champollion at the same time with a name which materially advanced, if I understood him rightly, the steps that have led him to his very important extension of the hieroglyphical alphabet. He had mentioned to me in conversation the names of *Apollonius*, '*Antiochus*,' and *Antigonus* as occurring among the witnesses ; and I easily recognised the groups which he had deciphered ; although, instead of *Antiochus*, I read *Antimachus*, and I did not recollect at the time that he had omitted the *m*.

"In the evening of the day that Mr. Grey had brought me his manuscripts I proceeded impatiently to examine that which was in Greek only ; and I could scarcely believe that I was awake and in my sober senses, when I observed, among the names of the witnesses, *Antimachus Antigenis* ; and a few lines further back, *Portis Apollonii*, although the last word could not have been very easily deciphered, without the assistance of the conjecture, which immediately occurred to me, that this manuscript might perhaps be a translation of the enchorial manuscript of Casati. I found that its beginning was 'A copy of an Egyptian writing . . . ;' and I proceeded to ascertain that there were the same number of names intervening between the Greek and the Egyptian signatures that I had identified and that the same number followed the last of them ; and the whole number of witnesses appeared to be sixteen in each. The last paragraph in the Greek began with the words, 'Copy of the Registry ;' for such must be the signification of

the word Π Τ Ω Μ Α Τ Ο Σ, employed in this papyrus, though it does not appear to occur anywhere else in a similar signification. I could not, therefore, but conclude that a most extraordinary chance had brought into my possession a document which was not very likely, in the first place, ever to have existed, still less to have been preserved uninjured, for my information, through a period of near two thousand years; but that this very extraordinary translation should have been brought safely to Europe, to England and to me, at the very moment when it was most of all desirable to me to possess it, as the illustration of an original which I was then studying, but without any other reasonable hope of being able fully to comprehend it; this combination would, in other times, have been considered as affording ample evidence of my having become an Egyptian sorcerer.

“Mr. Champollion had not thought it worth while to give me a transcript of the original Greek endorsement; he seemed to consider it as not fully agreeing with the Egyptian text, or at any rate not materially assisting in its interpretation: . . . [and] after an accidental delay of a month, the answer that I received from Paris was only such as to enable me to state that my opinion of the identity of the two endorsements is fully confirmed. I have lost, however, no time in sending to the Conservators of the King's cabinet a copy of my registry; with a request to be favoured with theirs in return . . . in order that the duplicates may stand side by side in the lithographical copy. . . . My application for the copy of the Registry has been received with the liberality which was to be expected from the directors of a great institution, and I have to return my best thanks to Mr. Raoul Rochette for a correct copy of the whole of this highly important manuscript. . . .

“The contents of Mr. Grey's Greek manuscript are of a nature scarcely less remarkable than its preservation and discovery: it relates to the sale, not of a house, or a field, but of a portion of the Collections and Offerings made from time to time on account, or for the benefit, of a certain number of *Mummies*, of persons described at length, in very bad Greek, with their children and all their households. The price is not very clearly expressed; but as the portion sold is only a moiety of a third part of the whole, and as the testimony of sixteen witnesses was thought necessary on the occasion, it is probable that the revenue, thus obtained by the priests, was by no means inconsiderable.

“The result derived at once from this comparison is the identification of more than thirty proper names as they were written in the running hand of the country. . . .

“A second papyrus, [obtained by Mr. Grey] of considerably greater magnitude, contains three Egyptian conveyances in the enchorial character, with separate registries on the margin in very legible Greek. These . . . afford us also many additional examples of enchorial proper names, besides a general idea of the subjects of the respective manuscripts, all of which relate to the sale of lands in the neighbourhood of Thebes.”

On pp. 69-75 of the book Dr. Young gives translations of “the enchorial papyrus of Paris containing the original deed relating to the mummies” and Mr. Grey's “Greek Antigraph,” which begins,—“Copy of an Egyptian writing respecting the dead bodies in Thyn. . . .” On comparing these

two translations, it appears that the Greek is a slightly abridged copy of the Egyptian document, the preamble ("in the reign of So-and-So"—giving a list of kings, priests, and gods) being omitted; after the name of each of the dead persons, the phrase "with his children and his household" found in the Egyptian is mostly omitted in the Greek, as well as a short passage which seems to give a sort of summary of the persons after enumerating them. The Egyptian document, before the names of the witnesses, ends thus:—"Executed and confirmed: written by Horus the son of Phabis, clerk to the chief priests of Amonrasother and the contemplar? Gods, of the Beneficent Gods, of the Father-loving Gods, of the Paternal God and of the Mother-loving Gods, Amen"; while the Greek says merely—"Written by Horus, the son of Phabis, the writer of the [priests] of Amonrasother and the other gods of the Temple." The main parts of the documents, however, judging from the translations, undoubtedly correspond so closely that there can be no question that the Greek is a translation of the Egyptian.

The date of the documents was supposed by Dr. Young to be from 146—135 B.C.

The probability of occurrence of this coincidence may, I think, be taken as equivalent to the probability that an MS. taken at random out of all the MSS. so far discovered in Egypt should turn out to be a Greek translation of a given demotic MS.

Greek being the official language in Egypt at the date when the MS. was written, legal documents would often have been written in it; and if one of the contracting parties did not understand the language of the other, which might often be the case, the document would probably be duplicated in the two languages. Of course only a small proportion of these legal documents would be preserved up to the present century. But it is practically certain that, when they were duplicated, the duplicates would have been originally kept together in the same archives (see Mr. Griffith's letter, below), so that, when first unearthed, they would be unearthed together, and if one was preserved, probably both would be. Therefore Dr. Young's duplicates were almost certainly included originally in the same find and separated later.

Now, if the duplicates had been kept separate to begin with, so that one of them might have been preserved and the other not, the chance that one document found in Egypt should turn out to be the duplicate or translation of a certain other one would depend on the total number of documents of all kinds that had ever existed in Egypt, and would thus be very small. But since, as just remarked, it is pretty certain that both duplicates or neither would be preserved and found, the chance depends practically on the total number of documents of all kinds that had been found in Egypt at that date (about 1820), and is thus comparatively large. By the word "found," I mean merely unearthed and preserved. I do not mean to imply necessarily that

any discovery had been made of the nature of the documents, or even of the language in which they were written, but only that—whether in the hands of investigators or dealers—they formed part of the total material then accessible for study. Probably no one knew at that time that any of the Greek MSS. were translations.

The chance in question may, then, be expressed as follows :—

Let e be the number of documents in the Egyptian language that had been found, of which t had been translated into Greek and *ex hypothesi* found, and g the number of Greek documents—not translations—which had been found. Then the chance that there had ever been a translation of a given Egyptian document is $\frac{t}{e}$ and the chance that a particular document taken at random out of all the documents found should be the translation in question is $\frac{t}{e(e+g+t)}$, that is, it increases with the extent to which translations were made, and decreases with the extent to which documents of all kinds had been found. It cannot possibly be greater than $\frac{1}{e+g+t}$, this being the value on the extreme supposition that all Egyptian documents had been translated, in which case $t = e$, and the chance then depends merely on the total number of documents found. Actually, no doubt, t is considerably less than e and the chance is decreased accordingly.

In considering this problem, it may appear at first sight that the Greek MS. was not taken at random at all, as Grey had specially drawn Young's attention to it. But there was no idea on the part of either of them that it might turn out to be a translation; from the point of view of its being such, it was quite at random that Young examined it first out of all those that Grey had sent him.

From the same point of view, it was, as it seems to me, quite at random that the MS. to which attention was drawn was a *Greek* MS. And for this reason I have treated it simply as an MS. taken at random. But if it be considered that this view is not legitimate, we should then have to regard it as a *Greek* MS. taken at random, which of course increases the probability of its turning out a translation. Using the same notation as before, the chance may be expressed as follows :—

The chance that there had ever been a translation of a given Egyptian document is, as before, $\frac{t}{e}$. The chance that a particular document taken at random out of $g + t$ Greek documents should be the translation is $\frac{t}{e(g+t)}$. If $g = 0$, that is, if there are no Greek

documents except translations, (which, of course, is infinitely far from fact) the chance is simply $\frac{1}{e}$, that is, depends only on the number of Egyptian documents and is independent of the number of translations that were made. Otherwise, it depends on t and increases as t increases. On the extreme supposition that $t = e$, that is, that every Egyptian document was translated (likewise utterly contrary to fact), the chance is simply $\frac{1}{g+t}$, that is, depends merely on the number of Greek documents. These two formulæ, $\frac{1}{e}$ and $\frac{1}{g+t}$, give the greatest possible values of the fraction, $\frac{1}{e}$ being the case in which the denominator is as small as possible and $\frac{1}{g+t}$ that in which the numerator is as large as possible.

It is important to note, further, that the chance of the duplicates coming together again, after having been once separated, depended largely on the number of places in which any exploration had been made. Obviously, the fewer places had been explored, the greater is the chance of the reunion of the duplicates. It seems possible (see below) that only two places had been explored then, so that the chance was much increased by this.

In whatever form the problem be stated, there are, unfortunately, too many unknown quantities in it to admit of an exact solution. However, through the courtesy of Professor Ernest Gardner, I was put into communication with Mr. F. L. Griffith (Editor of the Archaeological Survey of the Egypt Exploration Fund), who is well-known as the leading authority on Egyptian manuscripts, and who has very kindly given me what information is available towards the solution. He writes :—

Riversvale, Ashton-under-Lyne, August 21st, 1898.

The coincidence you treat of, wonderful as it seems, is by no means so extraordinary as it appears at first sight. It is quite true that the copy or "antigraph" in Greek would be kept with the original demotic. The latter was the real instrument, the former was only intended to make it intelligible to the Greek officials, etc., and naturally they would be kept together. The Greek antigraphs, of which several are known, are more or less abbreviated; the example in question is singularly full. Of course these bilinguals only exist for a short period under the Ptolemies and Roman emperors and form only a minute portion of the MS. documents from Egypt.

It was very usual to *endorse* demotic documents in Greek giving a brief indication of the contents; the Greek documents themselves were likewise endorsed in Greek.

I am afraid I could give no estimate of the number of Greek translations, Greek originals, or of the total number of MSS. from Egypt discovered and

preserved down to 1820, unless indeed after long research, which I must not undertake now. The trade in antiquities and papyri was then very active, and while very few lists existed of the collections, numbers of objects were being hawked about or brought home by travellers. . . .

F. LI. GRIFFITH.

P.S.—Unfortunately, “unearthing” documents did not always lead to their preservation. Except in the few places where their value was understood, they were burnt or thrown away.

There was no systematic exploration in Egypt before 1820, but Thebes was the hunting ground and next to it Memphis. I doubt whether documents were preserved from any other sites at that time.

CASE 13.

With the foregoing we may compare another instance of quite a different kind taken from the history of Astronomy, where two independent discoveries, completed almost at the same time, led to results correlated to one another in an important way.

The account is taken from an article on “Coincidences and Superstitions” in the *Cornhill Magazine*, Vol. XXVI. (July to December, 1872), pp. 684 *et seq.* The article in the *Cornhill Magazine* is unsigned, but is reprinted under the same title in *Knowledge*, Vol. V. (1884), the authorship being there disclosed as that of R. A. Proctor. He writes:—

The history of Astronomy has in quite recent times afforded a very remarkable instance of repeated coincidences. We refer to the researches by which the theory has been established that meteors and comets are so far associated that meteor systems travel in the track of comets. It will readily be seen from the following statements that the demonstration of this theory must be regarded as partly due to singular good fortune:—

There are two very remarkable meteor systems—the system which produces the November shooting-stars, or *Leonides*, and that which produces the August shooting-stars, or *Perseides*. It chanced that the year 1866 was the time when a great display of November meteors was expected by astronomers. Hence, in the years 1865 and 1866, considerable attention was directed to the whole subject of shooting-stars. Moreover, so many astronomers watched the display of 1866, that very exact information was for the first time obtained as to the apparent track of these meteors. It is necessary to mention that such information was *essential* to success in the main inquiry. Now it had chanced that in 1862 a fine comet had been seen, whose path approached the earth’s path very closely indeed. This led the Italian astronomer Schiaparelli to inquire whether there might not be some connection between this comet and the August shooting-stars, which cross the earth’s path at the same place. He was able, by comparing the path of the comet and the apparent paths of the meteors, to render this opinion highly probable. Then came inquiries into the real paths of the November meteors, these inquiries being rendered just practicable by several coincidences, as—(1) the exact observations just mentioned; (2) the existence of

certain old accounts of the meteor shower; (3) the wonderful mastery obtained by Professor Adams over all problems of perturbation (for the question depended on the way in which the November meteors had been perturbed); and (4) the existence of a half-forgotten treatise by Gauss, supplying formulæ which reduced Adams' labours by one-half. The path having been determined by Adams the whole question rested on the recognition of a comet travelling in the same path. If such a comet were found, Schiaparelli's case was made out. If not, then, though the evidence might be convincing to mathematicians well grounded in the theory of probabilities, yet it was all but certain that Schiaparelli's theory would presently sink into oblivion. Now there are probably hundreds of comets which have a period of $33\frac{1}{2}$ years, but very few are known—only three certainly—and one of these *had only just been discovered* when Adams' results were announced. The odds were enormous against the required comet being known, and yet greater against its having been so well watched that its true path had been ascertained. Yet the comet which had been discovered in that very year 1866—the comet called Tempel's, or I. 1866—was the very comet required to establish Schiaparelli's theory. *There was the path of the meteors assigned by Adams, and the path of the comet had been already calculated by Tempel before Adams' result had been announced; and these two paths were found to be to all intents and purposes (with an accuracy far exceeding indeed the requirements of the case) identical.*

CASE 14.

The next case, referring to a curiously lucky little incident, was contributed to *Notes and Queries* in 1895 (8th Series, Vol. VIII., p. 270), by Mr. C. J. Fèret, F.R.G.S., F.R. Hist. Soc., author of an exhaustive history of the parish of Fulham, which is about to be published under the title of *Fulham Old and New*.

Mr. Fèret writes:—

49, Edith Road, West Kensington, W.

. . . . Christmas, 1892, I spent at Southsea. One day I wanted to get some envelopes of a somewhat unusual size. I tried some dozen stationers' shops in the town, but not one could supply me. I gave up my quest in despair and was returning home to dinner. I had nearly reached the top of Palmerston Road when I saw a small parcel lying on the pavement. I picked it up, and, as it bore no printed address of a shop, I took it back with me to my apartments. You may judge my astonishment when, on opening it, I found it to contain a packet of twenty-five envelopes of the precise size for which I had fruitlessly enquired! My sister, who was with me at the time, can vouch for the truth of this incident.

CHAS. JAS. FÈRET.

I wrote to Mr. Fèret asking for his sister's recollections of the incident, which she has kindly sent me.

She writes:—

49, Edith Road, West Kensington, *April 19th, 1898.*

MY DEAR MADAM,—I have referred to the paragraph in *Notes and Queries* which my brother wrote. It is perfectly accurate in every detail.

My brother and I, after having failed to get the envelopes we wanted (rather an extra size), were returning home to dine. Just as we were crossing the Palmerston Road to go down Marmion Road I noticed a small parcel on the pavement. I picked it up, but as there was no name on the packet and no one seemed to be looking out for anything lost we took it home and there opened it. As we could hear of no owner, and the packet afforded no clue, we used them, as they were precisely what we wanted. We have often talked about it. A stranger coincidence, except in the realm of fiction, it would be impossible to conceive. I hope these facts may be of use to you.—Yours faithfully,

C. ADÉLE FÉRÉT.

The next case relates to the finding of a lost object, much valued by its owners.

CASE 15.

The account appeared in *The Spectator* of August 27th, 1898, among "Letters to the Editor," under the title of "The Romance of a Ring."

Waldron Rectory, Sussex, August 22nd.

. . . . Many years ago, when at Oxford, my father gave me as a heirloom a ring presented to him by an old friend, and bearing an inscription stating that it contained the hair of the Duke of Wellington. This ring I gave to my wife on our marriage in 1876. In October, 1879, when we were on a visit to Mr. W. Arkwright, of Sutton Scarsdale, my wife felt the ring slip off her finger at the dinner-table, and although careful search was made, nothing more was seen or heard of it for eighteen years, so far as we were concerned.

At the commencement of this year, however, my wife received a letter from her half-sister (Mrs. Hodge) in New Zealand, which stated incidentally that a church in which she was interested out there had received unexpected help some years ago from a curious source. Her sister (Miss White) had sent out from England at her request some gloves purchased at Bide's [a London shop], and on trying on a pair of these gloves she, to her astonishment, found inside one of them a ring containing the hair of the Duke of Wellington, which had evidently been drawn off the finger unconsciously by some one trying on the glove at Bide's. Unable to find the owner of the ring and not liking to keep it, Mrs. Hodge thought it would be a fair thing to sell it and apply the proceeds to the church fund. She did so and the purchaser was a Mr. Frank Arkwright, of Overton, Marston, New Zealand, whose grandmother had given the ring to my father, and who has most kindly replaced it in my possession.

Now, here are a series of coincidences which are only likely to happen once in a life-time. That of all the thousands of people who purchase gloves my wife's half-sister should have lighted upon this particular pair and, unknown to herself and to Bide, should have sent out this ring in them to her sister in the Antipodes, and that there it should have been recovered in the house of a cousin of the Mr. Arkwright in whose house it had been lost eighteen years ago, surely goes to show that sometimes, at any rate, truth is

stranger than fiction. As a minor coincidence I may perhaps mention that the letter which, by the merest chance, happened to mention the finding of the ring, was dated from Wellington, in New Zealand.—I am, sir, etc.,

W. J. HUMBLE-CROFTS.

I wrote to Mr. Humble-Crofts, asking if any corroboration of this account was obtainable, and he replied :—

Waldron Rectory, Sussex, *September 19th, 1898.*

I shall be glad to put you in possession of any information you may require as to the ring, and my wife is, of course, quite willing to corroborate what I say and to copy the extract from Mrs. Hodge's letter which first informed us of the discovery.

But I do not see how this would help you, as Mrs. Hodge had no knowledge of the fact that we had lost any ring, and the reason why she happened to mention it (which she would not have done otherwise) was that my wife had sent out some little help towards the church in which Mrs. Hodge was interested. In acknowledging this, she proceeded to state what a curious addition they had received to their church fund.

W. J. HUMBLE-CROFTS.

Mrs. Humble-Crofts wrote to me later :—

Waldron Rectory, Sussex, *October 11th [1898].*

I beg to enclose copy of extract from letter received by me from my half-sister in New Zealand, and also extract from letter from the other half-sister in London about the lost ring. I also corroborate all the statements made in the account of the loss and recovery of the ring, as given in the *Spectator*.

B. HUMBLE-CROFTS.

The copies enclosed were as follows :—

Copy of letter written by Mrs. Hodge, December 19th, 1897.

“We are hoping to get our church built by the end of February. I am now collecting for the organ, and sold a ring, which I daresay D—— will remember came out by a strange chance in one of the boxes she sent me long ago, and which belonged to the Arkwrights, and contained the Duke of Wellington's hair.

“Mr. Arkwright, who is out here, was very pleased to get it, and sent me £5 for it, which I shall place to the Organ Fund.”

Copy of letter written by Miss White (sister of Mrs. Hodge, and half-sister of Mrs. Humble-Crofts), dated January 31st, 1898.

“The history of that ring I remember *perfectly*, and it was this :—

“B—— and L—— wrote asking me to send out one dozen kid gloves. I was in London, and went to Byde's glove shop, North Audley-street, chose a dozen of gloves, and sent them out to New Zealand. When L—— tried them on, behold in one pair was this ring. Some one buying gloves at Byde's

must have been trying them on, and left the ring in the gloves. I wonder who that could have been?"

The next case relates to a matter of still greater urgency, and, as will be seen, admits of being explained by telepathy.

CASE 16.

The account is an extract from a narrative printed in the *Journal S.P.R.* for July, 1898, under the title of "Three Generations of Psychical Faculty." Professor Lodge, who obtained the account, says:—"The following narrative has been recently written by a gentleman well known to me, an active clergyman of the Church of England. Of his good faith there can be no doubt, and the narrative may, I believe, be regarded as substantially accurate." Several incidents are described, and the writer continues:—

. . . These curious experiences generally occur in groups, and are simultaneous with the close of a period of excessive mental activity. A very singular one occurred last January, when I went to D. with my wife for a few days' quiet. While there, I remembered that E. was in the neighbourhood, where I had a cousin residing, a brother of a distinguished artist, and that he had written to me some time before about a picture of his brother's, which he wanted to sell. I at once resolved to ride over next morning. Upon enquiry from the manager of the hotel, I learnt that the road was bad, the weather unpromising, other directions more interesting. But I stuck to my purpose and went, though the road was atrocious, and, within three or four miles of my goal, I was so dead beat that I almost turned back.

However, I reasoned with myself that I might perhaps never have the chance of seeing my cousin again, and that I might even find him in some great need of my help. I continued my journey, found out his cottage and knocked at the door. He opened it himself and said, without showing any surprise at my appearance, "It's very good of you to come so promptly, but I didn't expect you to-day." "Why did you expect me at all?" I said. "Haven't you got my letter?" he replied, "I wrote to you last night." "To W.?" I said. "Yes," he answered. "But I am staying at D., and have had no letter," I said. He then took me in, and, to make a long story short, I found him in very great distress, and my coming on the scene really proved a sort of lifebuoy to a drowning man.

Through the kindness of Professor Lodge, I was put into communication with the writer, who replied to my enquiries:—

July 24th, 1898.

. . . Fortunately, I made notes at the time of all the incidents referred to. . . . As regards my cousin, I had certainly no idea that he was in trouble. His letter was addressed to me at [the writer's home], and he had no knowledge that I was at [D]. I have his letter somewhere, but cannot lay my hands upon it at the moment. Mrs. — was quite as much struck with this coincidence as I was. . . .

Mr. — was unable to send me his notes, as they were embedded in manuscript relating to other matters ; but Mrs. — kindly sent the following corroborative statement :—

July 25th, 1898.

DEAR MADAM,—I have much pleasure in endorsing my husband's account of the coincidences recorded since our marriage. . . .

The call on his cousin from [D] was made last January, so is quite recent, and clearly in our minds.

[Signed in full] — —.

A case very similar to this, but involving a more strongly marked coincidence, is related by Bt. Major Kobbé, U.S. Army (see *Phantasms of the Living*, Vol. I., p. 288). While at home in New York city he felt a sudden desire one day to visit a cemetery some six or seven miles distant, on Long Island, where his family owned a vault. None of them had ever been accustomed to go there and there was no reason at all to suggest the idea to him ; but on arriving, he found his father standing at an open grave, in which he had just had placed the remains of an infant son who had died before Major Kobbé's birth ; the remains having been removed from the vault of the new grave. It appeared that the father had left a message at home for him to meet him there just at that time, but he had not gone home and consequently had not received the message.

Mr. Gurney prefixes this case by the caution that "it is scarcely too much to say that cases of this character, in whatever number accumulated, could never decisively exclude the hypothesis of accident."

It must be admitted that the same remark applies to the following case, which may also be explained as due to telepathy. But I place it here because, among all those I have come across in this connection, it is the one that seems to me most strongly to suggest a special intervention.

CASE 17.

The account was obtained by Professor Lodge, who writes :—

October 18th, 1896.

The enclosed statement is the record of what was carefully and precisely told me yesterday by the son of the old lady, who calls himself C. B. in the narrative, a personal friend of mine. He is a solicitor (and a recent M.P.) and thoroughly understands about evidence. He is an ideal witness, but is doubtful whether it is worth recording. It may be considered either a coincidence, or a telepathic leading, or an answer to prayer. As a coincidence, the chances are remarkably heavy against it.

C. B.'s account is as follows :—

At the beginning of the present year, 1896, after fifty-two years of ideal married life, A. B. lost her husband, a clergyman then in his eighty-first

year. For some time before her husband's death, she had shown signs of failing memory, but not in any remarkable degree. Immediately upon his death, her memory began to show signs of failure, which increased very rapidly. Soon after the death, A. B. was taken from the provincial town in which she had lived to live in London (Kensington) with her sister. By the 16th May, 1896, the date which is important for the present purpose, A. B. had failed so much that she was not always aware that she had left her old home, often fancied she was there or elsewhere in her old town. She was quite incapable of finding her way from one part of London to another, and unable to state her own address in London. On the morning of 16th May at 10 a.m., she left her house alone, carrying a parcel which she intended to leave at a shop a very short distance off. I believe her sister was unaware of her intention, and it does not seem to have occurred to the servants that, as the distance was so short, there would be any risk of loss of way.

At 9.30 p.m., C. B., one of the three sons of A. B., arrived at his house in London, having travelled up from the country. He there met his sister-in-law, wife of another son of A. B. She informed him that A. B. had left home at 10 a.m. under the circumstances stated and had not been heard of since ; that in the meantime her husband and his brother had communicated with Scotland Yard, and with the district police office, giving a description of A. B. ; that the description had been duly circulated throughout the Metropolitan District ; that communications had been sent to all the London hospitals ; and that in the opinion of the police authorities nothing further could be done.

C. B. (that is, the son who had arrived in London) waited for a few moments until his wife came in. When she came in, the three people (that is, C. B., his wife, and his sister-in-law) drove first to A. B.'s house. They saw there A. B.'s sister and the servants, but learnt nothing more than the facts already stated.

C. B. went to the local police-station with his wife, and found that all the notices had been properly circulated. He asked the Chief Inspector whether any useful purpose could possibly be served by going to Scotland Yard. The Inspector said it would be useless, as the system was such that if news of the missing lady were known at one police office, all the other offices would instantly be made aware of it.

C. B. then went with his wife to the house of his brother, quite close to where A. B. lives. He found that his brother was then receiving telegrams from various hospitals saying that they had seen nothing of A. B.

By this time it was past 10 p.m. C. B. had no idea that anything further could be done. He thought that all that remained was to await news, and never expected to see his mother alive again ; but as under these circumstances no rest is to be had by sitting still, he said to his wife that he intended to take the Underground Railway to Westminster, and to call at Scotland Yard. He thought it possible that he might find some official in high authority who would give him some ideas based upon wide experience. C. B.'s wife offered to accompany him. Accordingly these two people went to Westminster Bridge.

On leaving the station C. B. asked a porter the way to Scotland Yard, for the purpose of making enquiry as to a missing person. The porter directed

him to go on to the Embankment, the Thames Embankment, and to take the second street to the left. C. B. and his wife went along the Embankment and took what they thought was the second street to the left. It was in fact the third turn, so that they were in error. On taking the turn they could see nothing of Scotland Yard and they called at a large block of flats and asked the porter the same question as that put to the railway porter. He told them to go straight on into Parliament Street and then to turn to one side, C. B. forgets whether to the right or left. They followed the direction and found themselves in Parliament Street, Westminster clock then striking eleven. C. B. saw a young man passing along Parliament Street and turned after him a few paces in order to find out whether he should turn to the left or the right. The young man pointed in the right direction. C. B. then turned round to tell his wife, who was a few paces behind, and saw her then talking to A. B.

A. B. was then taken back to her own home, a distance of about four miles. She could give no account of herself. The parcel she started with in the morning was still in her hands. She thought she had been twice in church. She thought she had been to the docks, evidently confusing the river at Westminster Bridge with docks in her old town. She was not aware that she was not in her old town until the lamps were lighted. She then realised that she was lost, but I believe she did not know then that she was lost in London. Her purse contained no money, but it did contain two tickets, showing that she had been first to Tottenham Court Road, and afterwards to Charing Cross. On meeting C. B., A. B. said she had never prayed so earnestly in her life, that "one of you," meaning one of her sons, "would come."

Neither C. B. nor his wife felt any impulse drawing them by the way they took, and which resulted in the meeting. On the contrary, they had no expectation of hearing of A. B. except from the Police.

(B) *Cases of "Physical Phenomena" in which the evidence for a supernatural cause depends on a coincidence.*

In cases of "physical phenomena"—effects on matter which are by some attributed to the action of disembodied spirits—we have, of course, first to consider whether the conditions sufficiently exclude the agency of ordinary known causes,—whether there can have been any deception or mistake on the part of the witnesses. Supposing that we have satisfied ourselves on this point, the next question bearing on the spiritistic interpretation is whether any intelligence has been manifested suggesting the agency of spirits.

CASE 18.

In this case the evidence on both the points just mentioned is obviously very weak; the action of known causes is certainly not excluded, and there is merely a slight indication of intelligence, which depends on the incident happening just at the time it did. That is,

it is a rather curious coincidence, but pretty clearly nothing more. This is the view taken of it by the witnesses, who were two members of the American Branch of the Society, Mr. W. E. Ward and Dr. Anna Lukens. Mr. Ward writes :—

(Russell, Burdsall, and Ward, Manufacturers of Bolts and Nuts.)

Port Chester, N. Y., *May 13th, 1897.*

DEAR DR. HODGSON,— . . . A queer thing happened in the Doctor's [Dr. Lukens'] office last Sunday afternoon. We had been talking about our old friend Professor Cope, who died about four weeks before we returned home from our three months' trip through Mexico, California, and the North-West.

I was referring to Cope's interest in psychical matters, and saying how satisfactory it would be if we could get some reliable report from him of his impressions of the *real* life he has so lately entered into. And immediately the Doctor's large musical box *commenced playing*, and continued to play for over *five minutes*, to our great and almost bewildering astonishment, and ceased playing only when I commenced trying to account for such an unexpected entertainment through ordinary natural methods. Just as soon as I remarked how it might be accounted for, it stopped as abruptly as it began.

The instrument *had not been wound up in over three months*, and when last used, if there had remained unused any tension of the spring, it might have been released long before by the jarring it had been subject to through the occasional shifting of the furniture in the room. . . .

W. E. WARD.

Mr. Ward writes later :—

Port Chester, N. Y., *May 26th, 1897.*

. . . The most simple material cause I can attribute the playing to is mainly surplus spring tension, for some unknown reason unexpended during the last use of the instrument and held until favouring conditions of temperature would probably release any frictional restraint in the working parts, together with an imperceptible vibration from unnoticed jar, [which] might have aided in removing the obstruction to the playing.

A curious part of the experience, which I perhaps omitted to mention in my last, was the response I received to a mental question I asked some ten or fifteen minutes after the playing ceased. The question was addressed to Professor Cope in about this form, "Edward, are we indebted to you for the music we have just heard?" and immediately a response of three loud raps came on the floor apparently quite near me, but this was after Dr. Lukens had left the room, so is unsupported by another witness.

Dr. Lukens will probably write and tell you what her opinion is about the experience. I don't think she has any faith in Cope having anything to do with the playing. . . .

W. E. WARD.

Dr. Lukens writes :—

1068, Lexington Ave., *May 30th, 1897.*

DEAR DR. HODGSON,—I have nothing to add to Mr. Ward's report of the musical box experience except to say that I doubt very much that it was

produced by any supernormal cause. It was, indeed, a rather startling circumstance that, as we sat in my office that Sunday afternoon (May 9th) talking of Professor Cope and wishing we might get some report from him, the musical box should at that moment begin to play, and continue for a few minutes. But I am inclined to believe it was merely a coincidence, due probably to the natural causes Mr. Ward suggested in his last letter to you.

ANNA LUKENS [M.D.].

CASE 19.

This case was brought to our notice as one of supposed "spirit photography," the importance of which seemed to be considerably enhanced by its involving a very well-authenticated coincidence. It will be seen, however, that the lady who took the photograph—Miss S. R. Corbet—was from the first fully alive to the inconclusive nature of the evidence for any supernormal agency being concerned in its production. We are indebted in the first instance for information about the case to Lady Fitzgerald, through whose kindness Professor Barrett was put into communication with the persons concerned. From a mass of correspondence on the subject, Professor Barrett put together in June, 1895, the account printed below, which was afterwards corrected and signed by Miss Corbet.

Miss Sybell Corbet, when staying with her sister, Lady S., at D. Hall [assumed name], took a photograph of the library in the afternoon of December 5th, 1891, between 2 and 3 o'clock. The exposure was rather a long one. No one was present with Miss Corbet when the picture was taken, but on developing the negative, the head and body of an elderly gentleman appeared seated on a high-backed, old oak chair, one arm resting on the arm of the chair, the other arm of the figure and the legs being invisible. The figure, in fact, only went down to the waist, and though the face was rather indistinct, it appeared to have a short beard. When the picture was shown to one of the nearest relatives of Lord D., the late owner of D. Hall, she thought it exactly like him; others, however, who knew him, think it too indistinct to be sure of any likeness. Strangely enough, it turned out that the funeral of Lord D. was taking place on the very same day and hour at which the photograph was taken.

Miss Corbet is sure the plate had not been exposed before, and was one of a parcel of dry Ilford plates. Unfortunately, the exposure being somewhat long, she was not in the room the whole time, and did not lock the doors when she left the room; but the only men in the house were her youngest brother, the butler, and two footmen, and all these four were young men *and beardless*. In answer to inquiries, Miss Corbet states that the servants would not be at all likely to have entered the room, and her brother declares he did not. None of them would, she is certain, be likely to play a practical joke, and even if they did, the difficulty would be to explain the production of an older, bearded figure, without legs, and one considered by some of his near relatives to be very like Lord D., and entirely unlike any men living in the house.

The men were all young and clean-shaven, except Miss Corbet's brother, who had a moustache, and who is not in the least like the figure independently of this.

I certify the foregoing account to be correct in every particular.

SYBELL R. CORBET.

The date is fixed by two separate notes made by Miss Corbet,—at a time, when, as explained below, she had not observed the coincidence,—as follows :—

Entry in photographic note-book.—December 5th, 1891 : subject, library, D. ; exposure, 60 minutes ; plate, Ilford Ordinary ; hour, 2 to 3 p.m.
Entry in diary.—December 5th, 1891 : Lord D. buried at W. Church [two miles from D.]

The following extracts from letters written by Miss Corbet give further details.

Kingsland House, Shrewsbury, *May 2nd, 1895.*

There are the most contradictory opinions about the likeness of the figure in my D. photograph to Lord D. My sister thought it so like him that she begged me to look out the date on which I had taken it, when I first noticed that it was taken at the very time of Lord D.'s funeral. Since that, people who knew him well have declared it was not in the least like him ; others who knew him equally well that it was. But to my mind it is not clear enough to form a very definite opinion about, as the features are hidden by a standard lamp.

It is a curious coincidence ;—an unaccountable figure appearing in a photograph taken at that particular date, and that the figure should, in the opinions of some who knew him most intimately, resemble Lord D.

June 12th, 1895.

You ask if there is any possibility of the plate having been exposed before. So far as I am aware, *none*, as I always keep a very careful account of every plate I expose ; or rather I should say, no probability, as I have never exposed a plate twice without becoming aware of my mistake on taking out the dark slides ; and, moreover, I had in those days never taken a human figure—excepting as a minute object in a landscape—so the figure would still be unaccounted for. However, I am told by some photographers and others that the pre-exposure may happen during the sensitising of the dry plate. Of this, of course, I know nothing. The fact remains a curious one, I think, although *some* photographers declare there is nothing odd in it—mere accident and chance. But I quite agree with you that, as it is impossible to prove that no one entered the room, it cannot be regarded “as affording indubitable evidence of a ‘spirit photograph.’”

July 22nd, 1895.

I did not develop the plate until August of the following year, when I first became aware of the presence of an unexpected figure ; but it was not until my eldest sister, Mrs. R., suggested a likeness to the late Lord D., and asked me if it was taken before or after his death, that I referred to my note-book and diary, and found that it was on the day of the funeral (he died in London,

but was buried in the Church near D.). On later inquiry, I also discovered that the time corresponded, the funeral being a little late, in consequence of the lateness of the special train ; in fact, that during part of the exposure, the service was waiting the arrival of some of the principal mourners. My sister, Lady S., was renting D. Hall at this time ; and so far as I can remember, several of us (my sisters) were there when I arranged the camera, after which we all went out, leaving the plate exposed, and the room quite empty ! I had no particular interest in [the] arm-chair, and have never been able to discover that Lord D. was in the habit of using it, as he usually occupied a smaller room, on the ground floor, when alone.

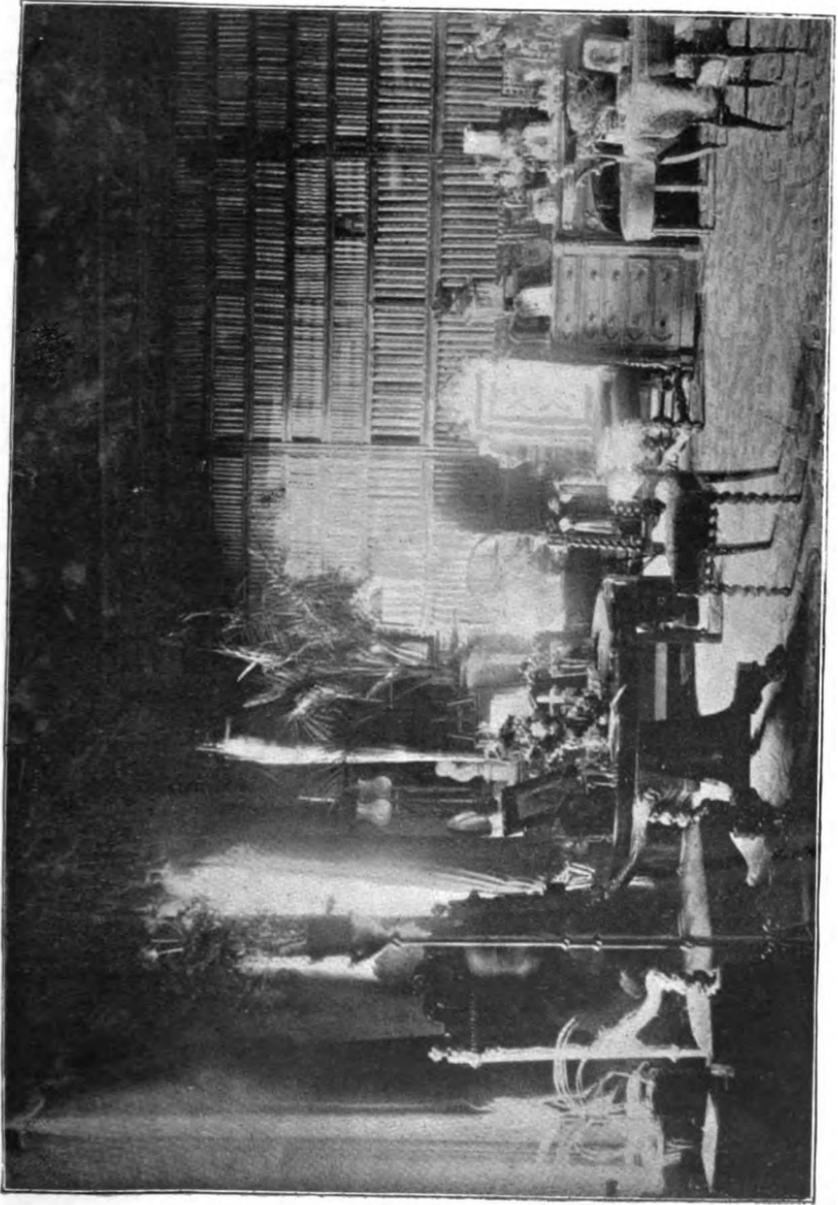
In the photograph, which is here reproduced, half of the upper part of the figure of a man appears seated in the large chair on the left. The high stand, carrying a flower-pot, in front of the chair seems to cut off the other half of the face and body of the figure. The lower part of the body and the legs are entirely wanting ; the head is semi-transparent and the face very indistinct. A vague semblance of an eye-brow, an eye, and a nose is seen on careful examination to be really the carving of the back of the chair showing through.¹ It is impossible to make out with certainty whether the face wears a beard or not, but the head appears to be quite bald, and there is something like a stock round the neck, which adds to the appearance of age. These features were perhaps mainly instrumental in suggesting the likeness.

With regard to the possibility that one of the footmen might have got taken, either accidentally or as a practical joke, Miss Corbet points out that the dress of the figure is apparently that of a gentleman, whereas her sister's footmen always wore livery, although at that time they were in mourning. She thinks it more possible, although very improbable, that some stranger came in for a few minutes ; the butler declared that no one did so, but, unfortunately, enquiry was not made until some time after the event.

The incompleteness of the figure, together with the supposition that the room was empty while the photograph was being taken, having suggested the possibility that it was produced by something other than an ordinary man, the interest of this suggestion was much heightened by the undoubted fact that Lord D.'s funeral was actually occurring at the time.

Professor Barrett, however, with the aid of Mr. Gordon Salt, experimented on the effects that could be produced by the transitory intervention of a figure during a long exposure of a plate. The results thus obtained by a person coming into the room, sitting on a chair

¹ All these points, as well as the semi-transparency of the figure as compared with the solid objects surrounding it, are naturally shown more clearly in the photograph than in the reproduction.



PHOTOGRAPH TAKEN BY MISS CORBET.

and moving his legs, were so very like the reputed ghost picture that Professor Barrett writes:—

September 5th, 1895.

I can see how the "Ghost" picture may have occurred, as I have succeeded in reproducing it almost exactly. I believe that one of the servants came into the room, sat down in the chair, crossed his legs and then uncrossed them, looked down for a moment and then at the camera, saw he was being taken, so got up and went away, having been in the chair about 20 to 30 seconds. This will give the ghost of an apparently older man from a young man, *with no legs*, and a semi-transparent face, &c.

Professor Barrett's photograph is also reproduced here, and will be seen to be very similar in appearance to the other, only half of the upper part of the figure—the half most strongly lighted—being shown, and this being semi-transparent. There are very faint indications of features in the face, and the chin, being abnormally long and with no distinct outline, looks as though there were a short white beard. A similar indefiniteness of outline on the right side of the collar simulates the folds of a stock, and, as in Miss Corbet's photograph, the legs are entirely absent.

Further light was thrown on the possible method of production of Miss Corbet's photograph by Dr. H. D. R. Kingston, of Stormont Lodge, Willesden, N.W., an Associate of the Society, who has had a long and varied experience in the investigation of "spirit photography." He observed a point which, though not at all conspicuous at first sight, is very evident when once attention has been drawn to it,—viz., that almost all the white lines or marks in the photograph are *doubled*, the brightest or best-lighted lines being most so. The doubling is clear, for instance, in some photographs and candles standing on two tables, various parts of some chairs, some of the handles of the drawers of a cabinet, and finally in the books and mouldings of the bookshelves.¹ In each case there is a faint image to the right side of and slightly lower down than the bright image of the same object. This shows that the camera must have been moved slightly *during the exposure*, as no movement of the furniture could have produced such complete uniformity of the double appearance. Since one set of images is much clearer than the other, the camera must have been moved either near the beginning or near the end of the exposure. Miss Corbet, however, informs us that the camera was placed in the open doorway and partly outside it, and thinks it possible that she herself may have shaken it in passing out of

¹ The doubling is not shown very satisfactorily in the reproduction, but is fairly clear in the photograph standing on the small table to the extreme right, also in another photograph in a high stand on the large table and a candle to the right of this, and may be seen more faintly in the books.

the room. This also makes it possible that another person passing by may have moved it slightly without coming into the room at all.

It is important to ascertain whether or not the "ghost"—the figure in the chair—has a double outline as well as the other objects in the room, as, if so, it would prove that the figure had been in the chair during the whole of the exposure, and, therefore, could not be that of the person who moved the camera. Unfortunately, it is altogether so faint, and its outline is so much blurred that it is impossible to make certain of this point. So far, however, as there can be said to be any double outline, it clearly does not correspond with the double outline of the other objects. It is possible to trace more than one outline in the head and stock or collar, but impossible to say that the one on the right is lower down than the one on the left; if anything, it is slightly higher up. Further, there are two distinct images of the elbow, but one is resting on the arm of the chair, while the other projects some way below it. This seems to prove conclusively that the figure—unlike all the other objects in the room—moved during the exposure, and that the multiplication of its outlines was not due to the movement of the camera.

Thus the camera may have been moved by some one who came into the room while the photograph was being taken, and Professor Barrett's experiments prove that the semi-transparency and whole appearance of the figure may have been caused by the person in question sitting in the chair for a short time, and moving about in it while he did so.

In the remaining four cases given under this head an apparently telepathic impression, relating to the death of a distant person, is described, the "physical phenomenon" being stated to occur at the same time. In two of the cases the object affected had some special association with the dying person. But the evidence for the physical effect having been produced supernormally is generally very slight, and I do not think that the association with an apparently telepathic incident can reasonably be held to add any weight to this evidence.

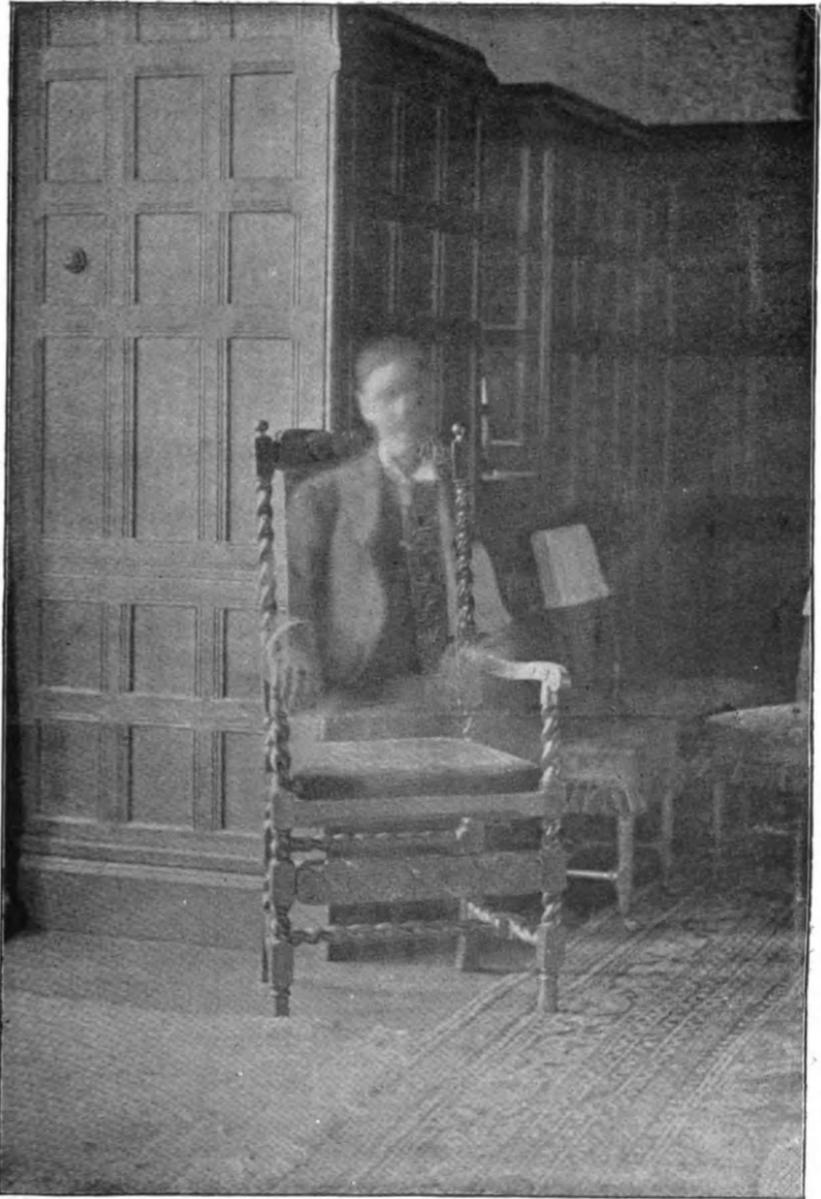
CASE 20.

From the "Appendix to the Report on Phantasms and Presentiments" in the *Proceedings of the American Society for Psychical Research*, p. 433.

[Miss M. O. A. sends the following account.]

February 8th, 1888.

"A strange and curious dream came to me one night last autumn, which, from the events that followed, made a deep impression upon me. I dreamed that my mother's brother died, and I went to Brooklyn to attend the funeral, but was intensely annoyed at arriving too late, as the service was over, and the body being carried out of the house. I saw my aunt and cousins, but could not speak to them. In the midst of this distressing scene I awoke,



**EXPERIMENTAL PHOTOGRAPH TAKEN BY PROFESSOR BARRETT AND
MR. GORDON SALT.**

hearing a loud crash, as of something falling. This was a decided reality, but I could discover nothing in my room that had been disturbed. In the morning I related my dream, and felt as though I should hear some news that day. The noise I heard was accounted for by the falling of a weight in a tall clock which stood in the hall. Two days passed, and on the morning of the third day the paper contained a notice of my uncle's death (my father's brother), stating that he died on the night I had my dream. It was then too late for me to go to the funeral; for some unknown reason I had not been notified by the family, although my dream had informed me, *perhaps*, of the very hour that he died.

“M. O. A.”

[In reply to Dr. Hodgson's inquiries, Miss A. informed him that the date of the dream was September 2nd, 1887, and sent the following statements from two ladies to whom she had related it in the morning before the news of the death came.]

“Philadelphia, March 4th, 1888.

“My sister's dream of our uncle's death, as related by her, occurred at the time she mentions. I was visiting her at the time, and distinctly remember being told the dream, and being asked if I had heard a peculiar sound in the night. The sound I did not hear, but was present when it was discovered that the weight of the hall clock had fallen down. My uncle's death occurred at that time, and the news of it was so late in reaching us that we were unable to attend the funeral.

“ISABELLA A.”

“March, 1888.

“Miss A. related to me the dream which she had relative to her uncle's death the morning afterward, and I also heard the noise made by the fall of the clock-weight the same night.

“ELIZABETH B. H.”

This case is one of the best-evidenced that I have found of this type. It will be observed that the object affected—the clock-weight—had only a very remote connection, if any, with the dying person, and it is hardly possible to regard its fall at the time of his death as anything but a mere coincidence.

The dream may no doubt have been telepathic—though its reference to the wrong person is a reason for doubting this—but even if it were, it does not, I think, add to the significance of the clock-incident; a clock-weight might as easily fall at the moment that its owner was having a telepathic impression as at any other time.

In the next case the great anxiety of the percipient about the dying person is a weak point, as this,—rather than telepathy,—may have caused the hallucination experienced at the time of his death.

CASE 21.

[This case is slightly abridged from an account given in the “Appendix to the Report on Phantasms and Presentiments,” *Proceedings of the American S.P.R.*, p. 429.]

Our attention was first drawn to this case by the following account, which appeared in the *Oil City Derrick* (Oil City, Pa.), December 12th, 1887 :—

“Depôt-master George Fry, whose brother Gideon died at Big Rapids, Mich., Sunday night, December 4th, had a strange presentiment of his death. On Monday morning Mr. Fry received a telegram, announcing the death of his brother, but giving no further information, and in the afternoon, in conversation with a *Derrick* reporter, said, ‘I wrote a letter to Gid last night, and just as I had finished it I glanced up and noticed the clock had stopped. I got up and wound the clock and pulled out my watch to set it by. It was 15 minutes to 10; I set the clock and, just as I started it, I heard the words, ‘I’m gone ! I’m gone !’ It was Gid’s voice, and it seemed to come out of the clock. I heard it as distinctly as I ever heard anything in my life. It startled me and I related the occurrence to my family. I am no Spiritualist, but I believe that must have been the time, and those the last words of brother Gid.’ Yesterday, Mr. Fry showed the *Derrick* a letter from Big Rapids, giving an account of his brother’s last moments. The letter stated that Gideon died at 15 minutes of 10, Sunday night, December 4th, and his last words were, ‘I’m gone ! I’m gone !’ the identical time and words as related to the reporter Monday last. . . .”

“In a later account Mr. Fry is described as saying—

“. . . . I am not a Spiritualist and never had any such experience before. The clock that stopped belonged to brother Gid. . . .”

“In reply to our inquiries Mr. Fry wrote as follows :—

“106, Sycamore-street, Oil City, Pa., July 9th, 1888.

“RICHARD HODGSON, Esq.,—DEAR SIR,—The account of my experience in regard to my brother’s death is true; that is, the way the Oil City or Pittsburgh papers had it. . . .

“G. W. FRY.”

Later some further evidence was received through Mr. R. W. Criswell, Editor of the *Oil City Derrick*, who obtained, in January, 1889, the statements that follow from various witnesses in the case. He remarks that “George Fry has been a resident of Oil City for 25 years. He is now about 39 years of age, a member in good standing of the Second Presbyterian Church and a man who is regarded as a good citizen in every respect. At the time of the above occurrence he was depôt-master, employed by the Allegheny Valley and New York and Western Pennsylvania Railroads.” Mr. Fry states :—

“My brother Gideon and I had been much together and were more intimate in our associations than other members of the family. I was much attached to him. On Friday, December 2nd [1887] I received a telegram from his physician at Big Rapids that he was ill and could not live over 24 hours. I had known of his illness, but he had not been regarded in a dangerous condition. I could not leave my business to go to him and I was greatly troubled on this account. I was thinking of him almost constantly. In church, Sunday, he was the chief subject of my prayers. Sunday evening, after church, I sat down to write him a letter and while writing it, I observed that the clock in the room—his clock, by the way,—had stopped. I got up to start it, and looking at my watch saw that it had been stopped but a few

minutes. I started to wind it up and found that it had not run down. As I moved the hands around, a strange light flooded the face of the clock, and the words issued from it in the voice of brother Gid, 'I'm gone, I'm gone!' The words were distinctly uttered. I was so impressed that brother Gid had died at that moment and that these were his last words, that I hastily sealed the letter, laid it away, and did not mail it. I noted the time as 9.45. . . . Next morning early before going to work, I told my wife of the incident. At about 11.30 that forenoon I was handed a telegram that had been received by my brother, Daniel P. Fry, in these words only, 'Gid is dead. Come to Montague.' This was signed by my sister Lizzie, who was at Big Rapids. That afternoon I told S. W. Turner, a newspaper reporter, of the death of my brother; and also told him . . . of the strange presentiment of the evening before. I had received no word of my brother's death, except the message given above, nor did I receive any more intelligence regarding it until some days later, when a letter came from my sister, giving particulars. This letter added that Gideon had died at 8.45 Sunday evening, December 4th, and his last words were 'I'm gone, I'm gone.' It was at 9.45 that I had heard the voice in the clock; but the difference in time between Big Rapids and Oil City is just enough to cover this discrepancy. My sister, who was with Gideon when he died, is now at home, in Oil City, and she tells me that for some time before he died he was unconscious, and imagined that I was with him. When any one would leave the bedside, he would say 'George, don't leave me.'"

The following statement was obtained from Mrs. Fry :—

"Oil City, *January 13th, 1889.*

"In regard to the presentiment of Gideon P. Fry's death, December 4th, 1887, my husband told me, early Monday morning, December 5th, 1887, of the voice in the clock, which he had heard the preceding evening.

"MRS. KATE J. FRY."

Mr. S. W. Turner, the reporter above referred to, gives a statement, dated January 13th, 1889, substantially the same as the original newspaper account which he had written at the time, but somewhat briefer, and omitting the incident of the clock stopping. On January 18th, 1889, Mr. Turner wrote a more detailed account, in which he mentions that Mr. Fry told him the clock had stopped while he was writing a letter to his brother. He continues :—

[Mr. Fry] "said the exact time was 9.45.

"On the following Sunday, the 11th, Mr. Fry reminded me of our conversation on the 5th, and showed me a letter from his sister, Lizzie Thompson, dated Big Rapids, giving particulars of the death of Gideon. This letter, which I read, said that Gideon had died at 8.45 p.m., Sunday, December 4th, 1887, and his last words were 'I'm gone, I'm gone!'

"With the permission of Mr. Fry, I went to the telegraph offices here Wednesday, December 14th, and . . . searched their files for copies of any messages relating to the death of Gideon Fry. Below is a copy of the only message bearing on the case :—

"'Big Rapids, Mich., *December 5th.*

"'Daniel P. Fry, Oil City, Pa. Gid is dead; come to Montague.

'LIZZIE K. THOMPSON.'

"This message was received in Oil City at 9.40 Monday morning, December 5th. No message was received at the postal telegraph office. . . .

"S. W. TURNER."

With regard to the clock stopping, it may, of course, be supposed that Mr. Fry, in his great anxiety about his brother, forgot to wind up the clock the night before. Against this we have to set his statement that he found on investigation that it had not run down. But this statement does not occur in the newspaper account, presumably given by him at the time, but only in the narrative written rather more than a year later. In the agitation of the moment he may easily have omitted to notice whether the clock had really run down or not, and may have assumed afterwards that he must have taken this elementary precaution. On the other hand, as a "depôt-master," he was probably more careful instinctively to keep his clocks in good order than the average man would be, so that it is comparatively unlikely that he forgot to wind it up. Even if he did, this would not cause it to stop at the moment of his brother's death; it would only give it a better chance of doing so.

Then, again, what is the evidence that the clock really did stop? The detail is omitted from the corroboratory statement of Mrs. Fry, and it is omitted from one of the three statements of the newspaper reporter, being mentioned in the other two. I do not think, however, that much stress can be laid on these omissions, as it is very doubtful whether any of the witnesses realised that the stopping of the clock—*supposing it to be causally connected with the brother's death*—was at all more remarkable than the hallucination—telepathic or subjective—of the voice that seemed to come out of the clock.

The connection of the hallucination with the clock suggests, no doubt, that the same agency produced both the veridical impression on the man and the effect of stopping the clock. On the other hand, the clock face may simply have acted as a *point de repère* for the hallucination because Mr. Fry's attention was directed to it at the moment, and any other object would perhaps have served this purpose equally well.

The next case is not, strictly speaking, at first hand, but depends on the kind of evidence that was called, in *Phantasms of the Living*, "second-hand, as good as first"—evidence, namely, from a witness who knew of the percipient's experience at the time and before it was ascertained whether or not it was in any sense veridical.

CASE 22.

The account was sent by Mr. A. Glardon, whose interesting experiments in thought-transference at a distance are well-known. (See *Journal S.P.R.*, Vol. VI., p. 98, and Vol. VII., p. 325).

The name of the witness was given us in confidence. Mr. Glardon writes :—

Tour de Peilz, Vaud, *August 31st, 1894.*

A friend of mine, Mrs. F., a daughter of the well-known geologist, —, has related to me a rather striking instance of telepathy of which she was a witness.

A few years ago, she was sitting on the rocks above the sea, at Nervi, near Genoa, where she resides habitually, with an American young girl, who has since become her son's wife. The young lady, her gloved hands resting on her knees, was talking with Mrs. F., when all at once she gave a slight scream.

“What is the matter?” asked Mrs. F.

“My finger has been stung.”

She took off her glove, and discovered that a ring of hers had snapped. She looked at it with a scared look and exclaimed :

“Oh, Mrs. F., a dear friend of mine has just died.”

She went on to explain that the ring had been given her by a young man at the time of her leaving the United States and that he had said : “If I were to die, this ring would apprise you of the fact.”

Mrs. F. pooh-pooed the matter, being not herself a believer in psychical matters. But, a few weeks later, came the news of that young man's death. Mrs. F. could not tell me if it was on the very day of the breaking of the ring ; but she has little doubt about it.

I doubt if Mrs. F. reads English, but if you write to her, she can, I dare say, get your letter translated and will doubtless answer you, confirming my report.

Her address is —

AUG. GLARDON.

We wrote to Mrs. F. asking for her recollection of the incident and its date, and also whether she could obtain for us any statement from the lady to whom it actually occurred, and she replied as follows : —

Nervi, le 10 *Octobre, 1894.*

MONSIEUR, — En réponse à votre honorée lettre, je ne puis que confirmer les faits dont Monsieur Aug. Glardon vous a fait mention.

En effet, au mois de Janvier, 1877, j'étais assise avec une jeune demoiselle à Nervi au bord de la mer. Nous causions, elle avait les mains immobiles, lorsque tout à coup elle se sentit fortement piquée au doigt. En étant songant elle vit qu'une bague qu'elle portait toujours, s'était cassée et l'avait ainsi piquée.

Cette jeune personne qui avait habité auparavant l'Amérique reçut au moment de partir pour l'Europe, d'un monsieur cet anneau. Il lui dit que s'il devait lui arriver un malheur, elle en serait avertie. Quelques semaines après ce fait qui l'avait péniblement émotionnée elle reçut la nouvelle de la mort de ce monsieur arrivée à la même date et même heure que celle de la rupture de la bague.

Je ne pense pas que cette dame désire maintenant que son nom soit publié, cause pour laquelle je ne puis vous en faire part. . . .

(Signed in full) E. F.

Further inquiries as to the evidence for the closeness of the coincidence remained unanswered, and we have been unable to obtain any more information about it.

It will be observed that though this case, assuming the complete accuracy of the report, points *prima facie* to telekinetic agency—an effect produced by unknown means on *matter* at a distance—it may easily be explained as merely telepathic—an effect produced supernormally on *mind* at a distance (Mr. Glardon, in fact, calls it an instance of telepathy). It may be supposed that a telepathic impulse from the dying man reached the subliminal consciousness of the percipient and produced a motor instead of a sensory effect, which led to an involuntary muscular spasm in the fingers, resulting in the breaking of the ring. We should expect that a motor impulse originating in this way would attach itself, if possible, to some object associated with the agent, just as we should expect that a sensory impulse telepathically started would produce a hallucination representing the agent, either in form or in voice. We have many instances recorded of motor effects apparently due to telepathy, though they do not seem to be so frequent as sensory impressions from the same cause. But it is obvious that as long as any human being is in contact with the object affected, as in this case of ring breaking, we must put down the effect to the action, conscious or unconscious, of that human being, as in all cases of so-called “physical phenomena” where the objects moved are in contact with the so-called “medium.”

CASE 23.

In the following case, an intelligent message, apparently relating to a death that had occurred a few hours before, was received through raps. I have ascertained that the death took place in 1887. The account, dated November 29th, 1890, was written by Mr. Myers, from details furnished to him by the witnesses, who, he says, were “well-known to me and entirely trustworthy.” We are not allowed to print their names. There can, I think, be no doubt as to what the name given through the raps was intended to be.

A group of persons in a country house were amusing themselves by getting a table to tilt out messages. The name of a deceased friend—of whom they were not consciously thinking—was given (as often happens in such cases) as the communicating intelligence. Suddenly the tilts changed to raps, which gave an unintelligible sentence. (I am obliged to change the name, but I give what is a fair parallel.) “Spære us Maroribnks.” While they puzzled over this message, a ring was heard at the front door. The host went out to greet some guests fresh from London. These guests brought with them the news that Mr. Marjoribanks, a valued friend of all the party, had expired suddenly a few hours before. It should be added that the deceased friend whose name had been given as “control” was also a

friend of Mr. Marjoribanks, and that Mr. Marjoribanks, whose life was a very valuable one, had been supposed to be in perfect health.

With regard to the raps here described, I need hardly say that there is no reason to doubt the good faith of the witnesses; but we require also to exclude the possibility that any of them might have produced the raps automatically and unconsciously. For my own part, I believe it possible that the most honest persons may occasionally act in this way, with entire unconsciousness of what they are doing. But the interest and importance of the incident lies in the fact of the *intelligence* manifested by the raps, however they were caused. Even if we adopt the supposition that one of the party rapped automatically, at least information unknown to any of them was elicited,—as information unknown to any one present is sometimes received through automatic writing.

In connection with this interpretation, it may be instructive to refer to an incident described by Mr. E. Gurney (*Phantasms of the Living*, Vol. I., pp. 76 to 78) to show that apparently genuine instances of thought-transference may sometimes occur even in connection with fraudulent phenomena. The following is Mr. Gurney's account, somewhat abridged:—

On September 2nd, 1885, Mr. F. W. H. Myers, Dr. A. T. Myers, and the present writer paid an impromptu visit to a professional "medium" [a person who has been detected in the production of spurious phenomena] in a foreign town. We had decided beforehand on a name on which to concentrate our thoughts, with a view to getting it reproduced. The medium, her daughter, and the three visitors sat round a table on which their hands were placed, and the present writer pointed to the successive letters of a printed alphabet; at intervals the sound of a rap was heard and the letter thus indicated was written down. Now these conditions could not have been considered adequate, had the result been that the name in our minds was correctly given; for though our two companions were not apparently looking at us and not in contact with us, it might have been supposed that some involuntary and unconscious movement on our part revealed to one of them at what points to make the raps. But as the result turned out, it will be seen, I think, that this objection does not apply. The name that had been selected was John Henry Pratt. The result obtained in the way described was J O N H N Y E S R O S A T. From the N in the fifth place to the end, Dr. Myers and myself regarded the letters that were being given as purely fortuitous and as forming gibberish; and though Mr. F. W. H. Myers detected a method in them, he was as far as we were from expecting the successive letters before they appeared. On inspection, the method becomes apparent. If in three places a contiguous letter be substituted, the complete name will be found to be given thus:—

R P T
J O N H N Y E S R O S A T,

the first word being phonetically spelt and the other two being correct

anagrams. It is highly improbable that such an amount of resemblance was accidental; and it is difficult to suppose that it was due to muscular indications unconsciously given by us in accordance with an *unconscious* arrangement of the letters in our minds in phonetic and anagrammatic order. If these suppositions be excluded, the only alternative will be thought-transference.

But in whatever way the knowledge of the letters or syllables reached the "medium's" mind, I see no reason to think that the expression of it by raps was other than a conscious act. The sounds were such as would be made by gently tapping the foot against the wooden frame of the table; and at a subsequent trial with one of these so-called "mediums"—the daughter—I managed by very gradually advancing my own foot to receive on it first a part and ultimately the whole of the impact. The movement required to make the raps may have become semi-automatic from long habit, but can hardly have been unconscious.

(c) *Premonitions.*

I include Premonitions among the class of Coincidences having a doubtful claim to causal production, partly because of the difficulty of forming any satisfactory theory of what mental action or faculty can be involved in a genuine premonition,—that is, a real foreseeing of a future event which could not by any exercise of the reasoning powers be inferred from a knowledge of present or past conditions. There are, of course, a great many so-called premonitions or presentiments which can be explained by supposing some supernormal knowledge of present facts; and a large number of predictions no doubt bring about their own fulfilment in various ways through self-suggestion or suggestion acting on others. All these explanations have been fully discussed in articles on the subject that have appeared from time to time in our *Proceedings* (see especially "On the Evidence for Premonitions," by Mrs. H. Sidgwick, Vol. V., p. 288; and "The Subliminal Self—The Relation of Supernormal Phenomena to Time—Precognition," by Mr. F. W. H. Myers, Vol. XI., p. 408). I may refer also to a more recent article by Dr. G. B. Ermacora in the *Rivista di Studi Psicici* (of which a translation, under the title, "Sur la possibilité des théories rationnelles de la prémonition," appeared in the *Annales des Sciences Psychiques* for Jan.-Feb., 1899), showing how other cases may be explained by a supposed combination of telæthesia and telepathy, and discussing theories of interpretation of those residual cases that may be called genuinely premonitory.

Apart from all theoretical difficulties, however, the cases I give under this head afford but very inadequate evidence of a real premonitory faculty. Some of them share the weakness so frequently found in such cases in that the prediction does not refer to any stated time of fulfilment, and therefore allows comparatively large scope for

accidental coincidence. Some, *e.g.*, the predictions of the results of races, refer to events in which only a small number of alternatives was possible, which again, of course, increases the chance of accidental fulfilment.

In the first two cases given the fulfilment of the premonition seems clearly accidental, although highly improbable.

CASE 24.

This case is Swift's prediction—if prediction it can be called—of the discovery of the then unknown satellites of Mars. The coincidence is well-known, at least to astronomers, but as I have not been able to find any complete account of the relation of the prediction to the actual facts, it seems worth while putting together all the details relevant to it.

The following passage occurs in *Gulliver's Travels* (published in 1726) Part III., Chapter III:—

[The astronomers of Laputa] “have likewise discovered two lesser stars, or satellites, which revolve about Mars, whereof the innermost is distant from the centre of the primary planet exactly three of his diameters, and the outermost five; the former revolves in the space of ten hours, and the latter in twenty-one and a half; so that the squares of their periodical times are very near in the same proportion with the cubes of their distance from the centre of Mars: which evidently shows them to be governed by the same law of gravitation that influences the other heavenly bodies.”

In order to judge how unlikely it was that any one should make this guess, it is necessary to consider how much was known about the planets and their satellites at the time. The two outermost planets, Uranus and Neptune, had not been discovered; but the other six principal ones, Mercury, Venus, the Earth, Mars, Jupiter and Saturn, had been known for centuries. Four out of the five satellites of Jupiter had been discovered, and five out of the eight now known to belong to Saturn. As Mars comes between the Earth with one satellite and Jupiter with four, it was not unnatural, in making a guess about its satellites, to imagine that their number was two. With regard to the theory of gravitation, this had been established by the publication of Newton's *Principia* in 1687, and the law as to the proportion which the periodic times of satellites bear to their distance from their planet had been shown to be a consequence of it; so that the different facts imagined in Swift's narrative were all scientifically consistent with one another.

But these facts involved a remarkable result, namely, that, on account of the closeness of the satellites to Mars, the time they took to revolve round it was less than the time taken by it to rotate on its own axis,—in other words, that the months were shorter than the

days. This was a state of affairs that no scientific man would have been likely to predict, because there was, and still is, no known parallel to it.

However, the imaginary conditions suggested in 1726 were to an important extent verified by the discovery of the two satellites of Mars by Professor Asaph Hall at the observatory of Washington, in 1877. The facts are given in the following quotation from Sir R. S. Ball's *Story of the Heavens*, p. 190 (Edition of 1893).

"The outer of the satellites revolves around the planet in the period of 30 hours, 17 mins., 54 secs. ; it is the inner satellite which has commanded the more special attention of every astronomer in the world. Mars turns round on his axis in a martial day, which is very nearly the same length as our day of 24 hours. The inner satellite of Mars moves round in 7 hours, 39 mins., 14 secs. The inner satellite, in fact, revolves three times round Mars in the same time that Mars can turn round once. This circumstance is unparalleled in the solar system ; indeed, as far as we know, it is unparalleled in the universe."

The diameter of Mars is 4,200 miles, and the period of his axial rotation is 24 hours, 37 mins., 22·7 secs. It may be convenient to show the exact degree of correspondence between Swift's statement and the actual facts in a tabular form, assuming that he knew what the diameter of Mars was.

	<i>According to Swift.</i>	<i>As now known.</i>
Inner Satellite (Phobos)		
Mean distance from centre of Mars 	12,600 miles.	5,800 miles.
Periodic time 	10 hrs.	7 hrs., 39 mins., 14 secs.
Outer Satellite (Deimos)		
Mean distance from centre of Mars 	21,000 miles.	14,500 miles.
Periodic time 	21 hrs., 30 mins.	30 hrs. 17 mins., 54 secs.

It will be seen that there is no very close resemblance between the two sets of figures. But the unique characteristic of the inner satellite—that its periodic time is less than the period of rotation of its primary—is brought out in both. This would cause the satellite to appear to a dweller on Mars to rise in the *west*, instead of—like all other heavenly bodies—in the *east*. Swift's knowledge of astronomy was probably sufficient to show him this anomalous result of the figures he gives for his imaginary moon ; but as he does not state it explicitly, it is not quite certain that he realised it. It must be observed also that, according to him, the outer satellite would have exhibited the

same anomalous feature, which, as a matter of fact, it does not. The correspondence was, however, sufficiently striking to produce a strong impression on the mind of at least one astronomer, as is shown in another passage which I quote from Sir R. S. Ball (*op. cit.* p. 192).

“A curious circumstance with respect to the satellites of Mars will be familiar to those who are acquainted with *Gulliver's Travels*. The astronomers on board the flying Island of Laputa had, according to Gulliver, keen vision and good telescopes. The traveller says that they had found two satellites to Mars, one of which revolved around him in ten hours. The author has thus not only made a correct guess about the number of the satellites, but he actually stated the periodic time with considerable accuracy! We do not know what can have suggested the latter guess. A few years ago any astronomer reading the voyage to Laputa would have said this was absurd. There might be two satellites to Mars, no doubt; but to say that one of them revolves in ten hours would be to assert what no one could believe. Yet the truth has been even stranger than the fiction.”

The coincidence would, of course, have been more complete if it had not been stranger, but only equally strange!

CASE 25.

The next case is a coincidence whose improbability enormously exceeds that of any other treated of in the present paper in which a numerical calculation of chances is possible. I take the account of it from two sources, De Morgan's *Budget of Paradoxes* (p. 168), and the chapter on “Lotteries” in R. A. Proctor's *Chance and Luck* (pp. 139–148), the facts having been originally recorded in a book called *Almanach Romain sur la Loterie Royale de France, ou les étrennes nécessaires aux Actionnaires et Receveurs de la dite Loterie*, by M. Menut de St. Mesmin (Paris, 1830), which contains all the drawings of the French Lottery (two or three each month) from 1758 to 1830. It was intended for those who thought they could predict the future drawings from the past, and various sets of “sympathetic numbers” were given to help them. In the French Lottery, five numbers out of ninety were drawn at a time. Any person in any part of the country might stake any sum upon any event he pleased, viz., that one or more of the numbers drawn would be a certain number or numbers, which was called a *simple drawing*; or that a certain number or numbers would be drawn in a certain order, which was called a *determinate drawing*. If he won, he received a certain multiple—which, of course, increased with the improbability of success—of the value of his stake.

De Morgan remarks that “the enormous number of those who played is proved to all who have studied chances arithmetically by the number of simple quaternes [successes in four out of the five

numbers] which were gained: in 1822, fourteen; in 1823, six; in 1824, sixteen; in 1825, nine, etc."

The case I refer to occurred in July, 1821, when one gambler selected the five numbers, 8, 13, 16, 46, and 64; and for the same drawing another had selected the four numbers, 8, 16, 46, and 64. The numbers actually drawn were, 8, 46, 16, 64, 13, so that both gamblers won. Their stakes were small, and their actual winnings were only 131,350 francs and 20,852 francs respectively. "The coincidence," observes Proctor, "was so remarkable (the antecedent probability against two gamblers winning on a simple drawing [a] simple *quine* and a simple *quaterne* being about 22 billions to one) that one can understand a suspicion arising that a hint had been given from some one employed at the lottery-office. M. Menut insinuates this, and a recent occurrence at Naples suggests at least the possibility of collusion between gamblers and the drawers of lottery numbers. But in the case above cited, the smallness of the stakes warrants the belief that the result was purely accidental. Certainly the gamblers would have staked more had they known what was to be the actual result of the drawing. The larger winner seems to have staked two sous only, the prize being, I suppose, 1,313,500 times the stake, instead of 1,000,000, as on a similar venture in the Geneva lottery. Possibly the stake was a foreign coin, and hence the actual value of the prize was not a round number of francs. The smaller winner probably staked five sous, or thereabouts, in foreign coin."

Since lotteries and all kinds of sports with which betting is associated are events in which large numbers of persons are interested, we should expect premonitory impressions about them—other than mere guesses, like those in the case just given—to occur fairly often and to be occasionally fulfilled. But I have been surprised to find how rarely such premonitions seem to be recorded, even after the event. One case, dating from 1857, was given in the *Journal* for March, 1890 (Vol. IV., p. 226) and among all the records, printed and unprinted, of the Society, I think there are hardly a dozen of the same kind. The following is a recent premonition of this kind, experienced by Professor F. W. Haslam, of Canterbury College, Christchurch, New Zealand, an Honorary Associate of the Society.

CASE 26.

Mrs. Haslam writes to Mr. Myers as follows:—

St. Barbe, Riccarton, Christchurch, New Zealand,

November 10th, 1897.

DEAR MR. MYERS,—I do not know whether the following little circumstance will be at all interesting to you as, perhaps, an example of the working of the subliminal self. I send it on the chance.

Yesterday, being the Prince of Wales' birthday, was a holiday in the Colony, and Frank meditated going to see the racing for the New Zealand cup. He has been so busy with exams. that he has not had a moment to think about the horses, and was *absolutely ignorant* of their names, qualities, etc.

But as he lay between sleeping and waking he tried to imagine the winner, and a jockey with all scarlet cap and jacket passed before his eyes, pulling his horse in hard and finally arriving at the winning post. He thought "scarlet is a common colour to see," but again the same jockey passed before his vision. He told Clarke, our Riccarton blacksmith, that morning, and before the race was run he also told Dr. Nedwill of Oxford Terrace, and Dr. Moorhouse, in the hearing of other people, also of Christchurch. He then took me all about the saddling paddock before the race to see what jockey was in all scarlet, and what sort of a horse he rode, and finally he laid a little money on the race and won it. The horse was "Waiuku," and was not the favourite, and all round us they were saying he had not a chance; but he seemed to become more popular towards the approach of the race, and was about second in the popular estimation. I will ask Frank to read this and correct if necessary.—Sincerely yours,

ROSE ST. BARBE HASLAM.

The above is a correct statement of what happened.

F. W. HASLAM.

Mrs. Haslam adds:—

P.S.—Waiuku's jockey did two or three times during the race sit back and pull the horse in hard. Just at the end he passed everything and won.

In reply to enquiries, Mrs. Haslam writes:—

St. Barbe, Christchurch, Canterbury, N.Z., *March 22nd, 1898.*

DEAR SIR,—In answer to yours of January 1st, 1898, I am trying to get a list of the horses that ran for the N.Z. Cup in November last, also statements from Dr. Nedwill and Dr. Moorhouse. As to newspaper accounts of the prospects before the race, I am afraid that the points upon which Mr. Haslam's word alone must be taken are these. *He did not know before he reached the course and bought a card of the races anything about the horses to run for the "Cup,"* except "St. Paul," a very well-known horse, who he believed would win. *He had no notion of the colours the jockeys wore, excepting those of the local owner, Mr. Stead, whose jockeys always wear yellow. He did not dream of a horse. He saw an all scarlet jockey riding to win, and on obtaining a card at the races his first thought was to look if there were an all scarlet jockey riding. As it turned out, there was, and he rode "Waiuku" the second favourite—the first being St. Paul. Waiuku was a horse from the north island utterly unknown to him. He is not a race goer, and has generally been busy at lectures during the most important race meetings at Christchurch. He had never seen the jockey before to his knowledge, or the colours, or the horse, and knows no jockeys by sight. After his vision of the jockey he thought to himself that scarlet might be a usual colour for people to see with their eyes shut, so he tried again to imagine the winner, and again saw the all scarlet jockey riding in*

to win, and he immediately apprised me of the fact. He cannot remember any similar experience, and I think that your magazine put the idea of trying to see the winner into his head. . . . I first asked Drs. Nedwill and Moorhouse to tell me if they recollected anything about Mr. Haslam's vision. You see that Dr. Moorhouse says "night," whereas it really was early morning, but I expect he naturally concluded that.

ROSE ST. BARBE HASLAM.

Mrs. Haslam enclosed corroborative notes addressed to herself by the two gentlemen mentioned in her letter, Dr. Nedwill and Dr. Moorhouse, as follows :—

Christchurch, New Zealand, *February 21st, 1898.*

On meeting my friend, Prof. Haslam, at the races at Riccarton in November last, he said to me, "let us look at the card to see if there is a jockey in all scarlet, as I dreamed a horse came in a winner in this race, with these colours."

COURTNEY NEDWILL, M.D.

Christchurch, *March 20th, 1898.*

DEAR MRS. HASLAM,—My recollection of the N.Z. Cup day and Professor Haslam is as follows. But I might first mention that the reason I have for remembering the circumstances especially well are that for a good many race meetings Professor Haslam and I have been in the habit of discussing the horses and agreeing on the one we elected to bet on, and backing the horse together as a sort of partnership. On the Cup day I arrived at the course somewhat late and did not at first see Professor Haslam, and had made my bets on the Cup before I found him ; but shortly before the race he came up to me and suggested that we should continue the partnership bets, at the same time telling me that he had dreamt the previous night that he had seen the race run and that a horse with *scarlet colours* had won. I turned up the race card and saw that the only horse carrying scarlet colours was "Waiuku." I said I was sorry that I had made all the bets I intended to on the race, so did not join him in backing the horse. The Professor left me with the intention of backing "Waiuku," but as I did not see him again for some time that day I don't remember whether he told me he had backed him, and I have had no conversation with him since on the subject. Of course you remember that "Waiuku" did win the race. In future if the Professor tells me he has had any more dreams about winners, I shall not fail to back them.

B M. MOORHOUSE.

Mrs. Haslam sent us also a programme of the meeting, dated Tuesday, November 9th, 1897. From this it appears that 14 horses were entered for the New Zealand Cup, the third on the list being Mr. S. McGuinness' "Waiuku," colours "All scarlet"; and that no other horse in the list had any scarlet in its colours.

Some further cases of dreams about races, narrated by an Australian member of the S.P.R., Mr. Donald Murray, will be found in Appendix III., p. 317.

We have another case, which I think hardly worth printing in full, of two dreams about the result of a boat race between the crews of Columbia College, Cornell University, and the University of Pennsylvania in June, 1895. (1) Dr. L. O. Howard, Entomologist in the U. S. Department of Agriculture, dreamt the night before the race that Columbia would win, which actually happened, and his statement, written in 1896, is confirmed by four persons to whom he had told the dream before its fulfilment. Professor Hyslop gives a careful analysis of the criticisms of the crews made in the New York papers beforehand, which were on the whole favourable to Columbia, though the betting was in favour of the other two boats. Dr. Howard, who was living at Washington, expressly states that being, as a Cornell graduate, very much interested in the race, he had read all the newspaper accounts of the condition of the crews, and on the day before his dream had been made rather anxious on Cornell's account by a somewhat unfavourable review in one of the New York papers. (2) Professor Hyslop happened to mention this dream to one of his own students at Columbia College, who informed him that he also had dreamt on the same occasion that Columbia had won, with other correct details of the race. His account is confirmed by a friend of his, whom he had told of the dream before its fulfilment. He, like Dr. Howard, was keenly interested in the race.

In connection with these premonitory dreams of races, I may refer to a small group of much more impressive predictions of numbers to be drawn in the conscription for the Belgian army; the report of which was sent in 1894 by Professor G. Hulin, of the University of Ghent, to Professor Sidgwick, and is given at length in Mr. Myers' paper on "The Relation of Supernormal Phenomena to Time" (*Proceedings S.P.R.*, Vol. XI., p. 545). Five cases of the right numbers being predicted, during the eight years, 1886-1894, are given on good authority; the numbers in the first four cases being respectively 90, 112, 216, 111. The first case was an especially striking one; a clearly externalised vision of the number 90 appeared to the percipient, and produced a strong impression on his mind, convincing him that he would draw that number. In the fifth case, the man who was to draw first announced that his number would be 116, and on being told that that was already drawn, said it would be 115, which turned out correct. The report only professes to give correct predictions, and we have no means of knowing how often predictions of these numbers are made which turn out wrong. Neither are we told how many numbers there were to draw from, except in one case, where it appears that there were at least 150, the lowest of them being 46 and the highest 223. In this case, the number 216 was the one rightly guessed. We must assume, I think, that these facts were known to the man who was to draw,—

the narrative certainly does not exclude this supposition, and, in fact, rather suggests it,—and, if so, the chance of his making a correct guess was, of course, about 1 in 150. The success of the prediction is thus much more noteworthy than in the dreams of the boat-race mentioned above, where the chance of guessing correctly was at least 1 in 3. Still these conscription cases are hardly numerous enough to exclude the explanation of chance coincidence.

Dr. Ermacora in the article already referred to (see *Annales des Sciences Psychiques*, January–February, 1899, p. 46) suggests that the fulfilment of premonitions about the results of drawing by lot—when the drawing is done by the person who experiences the premonition—might be explained by telæsthesia, as we might suppose that in such a case the drawing of the number is not accomplished blindly, but is guided by a supernormal perception which leads the subject to *select* automatically,—and probably unconsciously,—the particular number.

CASE 27.

The next is a case of a premonition which I print because it is well-authenticated, but the fulfilment of which hardly suggests more than a chance coincidence, since it seems not unlikely that on occasions so momentous presentiments of the kind might occur.

The account was obtained for the Society by Lord Bute from Lord Halifax, and is as follows:—

Powderham, *April 8th*, 1885.

Account of the attempted execution of John Lee for the murder of Miss Keyse, at Babbicombe, February, 1885, with letter to Lord Clinton enclosing statement of prison warders as to his dream on the preceding night.

Copied by me from account lent me by Lord Clinton, who was staying at Powderham when he received them.

(Signed) CHARLES L. WOOD.

H.M. Prison, Exeter, *April 8th*, 1885.

MY LORD,—The following are the particulars of the dream of John Lee, which your lordship has requested me to supply.

After the attempted execution of Lee on February 23rd, 1885, I went to his cell, and spoke to him on the extraordinary event that had happened in his case. He replied by saying that he had dreamed last night that it would so happen.

At my request he then related the nature of his dream.

He said he saw in his dream that he was led from his cell down through the reception basement to the scaffold, which was just outside the door of that basement.

He saw himself placed upon the scaffold, and efforts made to force the drop, but it would not work. He then saw himself led away from the place of execution, as it was decided that a new scaffold must be built before the sentence of the law could be carried out.

He then told me that he had mentioned the dream to the two officers who were in the cell with him, when he awoke at six o'clock in the morning.

The officers were not present when he told me this, nor had they been with him since the attempt was made. But in the meantime the officers had reported the dream to the Governor of the prison, to whom also I had gone to make a similar statement.

I ought to add, my lord, that John Lee did not attach any weight to the dream, but had fully believed up to the time of execution that he would be hanged, nor did he think of it while the attempts were being made. The force of the dream came to him after the state of semi-consciousness, into which he had apparently relapsed during the attempts, had passed away.—I am, your lordship's obedient servant,

(Signed) JOHN PITKIN, Chaplain.

The Right Hon. Lord Clinton.

(Signed) A true copy, CLINTON.

February 23rd, 1885.

A true account of a dream dreamed by John Lee on the night before the execution.

At 6 a.m., when he arose from his bed, he said, "Mr. Bennett, I have dreamed a very singular and strange dream. I thought the time was come, and I was led down through the reception out to the hanging place; but when they placed me on the drop they could not hang me, for there was something wrong about the machinery attached to the drop, and then they led me off from the drop, and took me (instead of the way I came) around the A Wing, and back through the A Ward to my cell."

He told me this in the presence of Mr. Milford, who watched with me through the night.

(Signed) SAMUEL D. BENNETT, Assistant Warder.
JAMES MILFORD, Superior Officer.

(Signed) A true copy. CLINTON.

The letter from the Rev. John Pitkin, Chaplain to the Prison, enclosing a copy of the statement of the warders, was written to Lord Clinton at his request when he was staying at Powderham in the beginning of April, 1885, he being in the Chair at Quarter Sessions at the time.

The drop was contrived by two doors secured underneath by a bolt; the prisoner was made to stand with a foot on each door and on the bolt being withdrawn the doors would naturally fall apart.

The drop had been tried *four* [five.—C.] times on the Saturday previous to the execution and had answered perfectly [twice in the presence of the executioner, who expressed himself satisfied with it.—C.]. It was again tried after the attempted execution, but without any weight on the scaffold, when it also succeeded.

On the occasion of the execution, at each successive attempt, the bolt could not be withdrawn from the socket by hardly the eighth part of an inch.

CASE 28.

The following is a case of retrocognition, not precognition,—an apparently supernormal perception of a past, not a future event,—but it

is convenient to class the two kinds of cases together, as is done in Mr. Myers' article above referred to, and this one is accordingly placed here. The percept takes the form of a communication from a deceased person and there is, no doubt, some evidence for post-mortem knowledge of and interest in trifling circumstances like the one described. But it will be seen that the case may easily be explained by telepathy.

The account was sent to us by Professor A. Alexander, of Rio Janeiro, who in a letter to Mr. Myers, dated July 21st, 1896, writes:—

Together with this letter, I send you the evidence for a veridical borderland hallucination. The people who sign the depositions are quite reliable and the facts no doubt occurred as they are stated. The case, however, does not stand altogether outside the possibility of chance coincidence.

Professor Alexander sent us the original depositions signed by the witnesses, with translations kindly furnished by himself. His own statement is as follows:—

Constante Gardonne Ramos, the principal informant in the following case, is employed as a foreman in the bookbinding department of the "Benjamin Constant Institute for the Blind," one of the public establishments of Rio de Janeiro. He has himself been totally blind from his first year, and he is therefore entirely destitute of visual memories. This will account for the fact that the experience related by him was auditory and tactile, and not visual. From his description, it would appear to have been a hallucination of the borderland type rather than a mere dream.

I called on him for the first time on May 30th of this year, and then took the notes from which his written declaration was drawn up. After the correction of some minor points, he agreed that the account was a faithful statement of his memory of the occurrence, and requested his wife, Donna Julieta Ramos, to sign for him.

17, Rua de Todosos Santos, Rio de Janeiro,

June 13th, 1896.

On the 17th of January of this year I went to the house of my mother, Donna Emilia Ramos, at No. 55, Rua São João Baptista. She was out, and while I stayed there, I talked to a parrot, which belongs to me, and which has a value for us because of family associations. My father, who died on the 4th of February, 1895, was, indeed, very fond of this bird.

On the following day in the very early morning, while I was yet lying in bed in that half-conscious state of somnolence that precedes waking, I felt twice the pressure of a hand placed on my body, and I then heard the familiar tones of my father's voice saying, "Nenem, send for your parrot. I have already given orders to deliver it up. It is in the house . . . and street . . . ¹ with two doors closed on the side of the street." The voice sounded as if it were external and came from a person standing near me.

¹ In a later letter, Professor Alexander explains that neither the number of the house nor the name of the street was given in the message about the parrot; the dots are intended to indicate their omission.

Now, as above stated, I had been with the parrot on the preceding day and had observed nothing which might lead me to suppose that there was a possibility of its escaping. A very heavy rain was falling, and no one came on the evening of that day to tell me what had happened.

About 9 o'clock on the same morning of the 18th, as I was just finishing the narration of my experience to my wife, the black woman came from the other house to give us the news that my parrot had in fact escaped. It had got away in the afternoon, after I had left the house in the Rua São João Baptista, and it had been caught by persons at the grocery store [venda] at No. 100, Rua General Polydoro.

As the grocer refused at first to restore the bird to us, we were obliged, in order to regain possession of it, to have recourse to the intervention of the police.

(Signed at the request of my husband, Constante Gardonne Ramos),

JULIETA FIGUEIREDO CARVALHO RAMOS.

As the result of my interrogation, Donna Julieta Ramos confirms the references to her part in the above incidents as follows :—

17, Rua de Todosos Santos, Rio de Janeiro,

June 13th, 1896.

I perfectly recollect that my husband related to me his having heard the voice of his father telling him of the escape of the parrot. The particulars of the deposition bearing his signature are substantially the same as those he gave me on that occasion. I recollect how the black woman arrived with her news just as he was finishing the narration of his experience, and I am quite sure that, up to that moment, none of the persons that were in the house knew of that escape by any normal means [of communication].

(Signed) JULIETA FIGUEIREDO CARVALHO RAMOS.

Donna Emilia Ramos, from whose house the parrot made its escape, assured me that there was nobody at home on the occasion, and that, therefore, it was impossible that the occurrence should have been communicated to her son on that day. The black woman, when questioned by me, seemed to confirm her mistress's statements. The positiveness of the witnesses that no communication prior to the hallucination passed between the two houses is important, for it is hardly two minutes' walk from one place to the other.

Donna Emilia Ramos declares :—

55, Rua São João Baptista, Rio de Janeiro,

June 14th, 1896.

The parrot escaped from my house on the day preceding the dream of my son Constante. It appears that on the afternoon of that day it broke the chain that secured it and escaped out of one of the windows. I arrived home in the evening, and it was only on the following morning that I sent the cook to tell my son what had happened.

In his lifetime my husband made a great pet of this bird, and every day on coming home fed it with ears of Indian corn or other things.

(Signed) EMILIA RAMOS.

Senor Constante Ramos has two brothers in Rio, one of whom has a business in Botafogo, the other being a public employé. It was through their instrumentality that I first heard of the case, and to their perusal I finally submitted their brother's deposition. They were quite sure that the narrative thus written out was identical with that which they had heard from his mouth at the very beginning, and at my request they very willingly made the following formal statement to that effect :—

77, Rua da Passagem, Botafogo, Rio de Janeiro,

June 16th, 1896.

We have just read the account given by our brother Constante, and we are sure that it is in every respect a true one.

It is exactly in this manner that he has told us, from the very first, of the dream in which he was informed of the escape of the parrot.

(Signed) CARLOS G. RAMOS.
FERNANDO GARDONNE RAMOS.

The grocery store in the Rua General Polydoro has three doors on the side of the street. The grocer assured me that none of these have remained closed in the daytime. The parrot had been caught in the street and brought to him by some labourers, and he had been unwilling to give it up without a reward.

CASE 29.

The following is an instance of a fulfilled prediction of death and probably belongs to the class of premonitions that bring about their own fulfilment through self-suggestion. The account of it was obtained and sent to Dr. Hodgson by Mr. Marshall Wait, who writes that the main facts of the case were told to him within two days of Mrs. X.'s death, and the story in full a week after. Her name and the names of the witnesses are withheld by their request.

Mr. Wait writes :—

5,144 Madison Ave., Chicago, September 25th, 1896.

DR. RICHARD HODGSON,—DEAR SIR,—I enclose you herewith a statement of Mrs. X.'s premonitory dream signed by several members of her family. The first account which I received was from Mr. J. S., a brother of Mr. A. S., and was somewhat inaccurate, as you will see. I then called upon Mr. A. S. and after conversation with him wrote out an account, at his request, in the name of his wife. When I presented the letter to him for signature, he told me that in talking over the subject with his wife, he found that his statement was inaccurate in one detail. Mrs. X. was anxious to procure the services of a certain nurse for her confinement and heard in the dream her father say to her mother "You had better engage that German girl now, as she is not at work." He then walked to the wall and put his finger on the calendar. Mr. S. promised to rewrite the letter with this correction, but he has returned it to me as I wrote it, saying that it was otherwise accurate and the family preferred to sign it as it was.

I have seen Dr. T., who attended Mrs. X. He expressed great interest in the case and signified his willingness to give me any information he could about Mrs. X.'s condition. But he is an extremely busy man and lives at a great distance from me, and thus far I have been unable to have more than a few minutes' talk with him. I will try again to see him.—Yours respectfully,

MARSHALL WAIT.

The account enclosed, which is signed by Mrs. X.'s mother, her three sisters, and Mr. A. S., her brother-in-law, is as follows :—

Chicago, *September 18th, 1896.*

DR. R. HODGSON,—DEAR SIR,—In March of this year, my sister, Mrs. X., was expecting her confinement. She was very apprehensive about the result and expressed her belief that she was going to die. On March 5th she dreamed that she was in the kitchen of her house, but at the same time could see her father (who has been dead eleven years) in the room above, talking with her mother, but did not hear what he said. She saw him hold in his hand a large printed calendar and put his finger on the date of March 22nd. She told her dream the next morning and interpreted it to mean that the baby would be born on that date. The accouchement occurred, however, on March 12th. Much fun was poked at Mrs. X. about the mistaken warning in her dream. Whether she put any other interpretation on it we do not know, as she never said anything to indicate that she did so. Her condition was in every way satisfactory to the attending physician until the afternoon of March 21st, when she suddenly became unconscious and died on March 22nd, having never entirely regained her consciousness. The cause of her death was a throat trouble, not connected with her confinement.

My husband, my sisters, and my mother, who were all familiar with the facts and knew of the dream before the death, sign this with me in confirmation, but in case of publication we request you to withhold names.

No memoranda were made of the dream, but the dates were fixed in our minds by the death occurring while our remembrance of them was fresh.

(Signed) — —

Mr. Wait writes further :—

October 19th, 1896.

I called this morning upon Mr. A. S. He requests me to say that Mrs. X. told her dream the next morning to [her mother and two sisters], who were all members of one household. Mr. and Mrs. S. heard of the dream later, but before the birth of the child. . . . The relations between the family and Mr. X., the widower, are strained, and I did not ask for his signature. I enclose a certificate from the Health Office, which I thought would be the best independent evidence of the death and its date.

The certificate states that the death occurred, on March 22nd, 1896, of Mrs. X., aged 27 years; "cause of death, Tubercular Meningitis; duration of disease, three days."

Attempts to obtain further evidence from Mr. X. failed, but Mr. Wait succeeded later in obtaining a statement of the case from the physician in attendance, Dr. T. [assumed initial]. He writes :—

December 4th, 1897.

At last I am able to send you a statement by Dr. T. in the X. case. Dr. T. did not answer my appeal for a special appointment, so as a last resort I wrote out a statement from my memory of our conversation, and last evening I got him to sign it. I would have preferred to have him make out his own statement, but as that seemed impossible, I did the next best thing. He said that my statement was perfectly correct, and that I might have made it even stronger—that Mrs. X.'s condition throughout her pregnancy and up to the moment when she complained of sudden severe pain in her head was “absolutely normal.”

MARSHALL WAIT.

Chicago, December 3rd, 1897.

I was the physician in attendance upon the late Mrs. X. at the time of her death. I was well acquainted with Mrs. X., having been a friend of the family as well as their physician for many years. She was of the tubercular diathesis, but had had no trouble of the kind for some years. Her condition after her accouchement was apparently satisfactory. I called upon her the morning of the day before her death,—more as a friendly matter than as a professional duty,—and found her apparently in so good condition that I jokingly told her that I would not have wasted my time if I had known how well she was. I believe that in the processes of elimination and repair which follow child-birth, the glands in her throat became overtaxed and discharged tuberculous matter into the base of the brain, causing the meningitis which resulted in her death. But at the date of her dream (of which I was told before her death) there was no more reason why she should have anticipated death than at any time for ten years before.

In case of publication I would prefer to have my name withheld.

(Signed in full) — — M.D.

It is an obvious weakness in this case, as Mr. Wait remarked in sending it, that Mrs. X.'s condition at the time of her dream might have predisposed her to presentiments of the kind. This does not, indeed, account for the exactness of the coincidence—in the absence of which the case would, of course, be entirely worthless as evidence of anything supernormal—but it adds considerably to the likelihood that, if not the result of self-suggestion, it was merely due to chance.

Further instances of premonitions, the accounts of which would have made this section too long, are to be found in Appendices II., III., and IV.

CHAPTER IV.

EXAMPLES OF COINCIDENCES WITH A STRONGER CLAIM TO
CAUSAL PRODUCTION.

SECTION I.

Coincidences apparently due to Subliminal Mental Action.

CASE 30.

The following case was recorded by the Rev. C. W. Bingham, apparently within three weeks after it occurred, in *Notes and Queries* (5th Series, Vol. XII., p. 256; September 27th, 1879). He writes:—

On Sunday evening, Sept. 7th, as I was reading the 37th Psalm in church, my attention was suddenly drawn away, and there happened to me what I never remember to have happened to me before in the course of my long ministry, viz., an utter inability to recover my lost place. After an awful pause, the clerk proceeded with the next verse, and a neighbour kindly directed me where to go on. On returning home, my wife pointed out the remarkable coincidence that the clause I had omitted was the latter part of verse 37: "*His place could nowhere be found!*"¹

C. W. BINGHAM.

I should be inclined to apply to this case a suggestion made by Mr. Myers in his discussion of what he calls *promnesia*,—the feeling, often experienced by some persons, that what is going on at the moment is a repetition of what has happened to them before, so that they feel able to predict the next step. (See "The Subliminal Self. The Relation of Supernormal Phenomena to Time: Retro-cognition and Precognition," in the *Proceedings S.P.R.*, Vol. XI., pp. 341-344.)

Mr. Myers imagines that the subliminal perception of what is going on may sometimes be slightly in advance—say, by a small fraction of a second—of the supraliminal perception. Then, when the subliminal percept emerges into the supraliminal or ordinary consciousness, there may be a sort of double consciousness of the percept—or a consciousness, together with an apparent memory of the same consciousness. The memory might appear to relate to a period some time back, whereas it would really relate to a past only just past. The effect would be that what is happening appears to the observer to

¹ The whole verse (in the Prayer-Book version) is:—"I went by, and lo, he was gone: I sought him, but his place could nowhere be found."

be a repetition of what has happened some time ago,—the repetition being really in his own perception of the incidents, not in the incidents themselves.

According to this view, we may suppose that Mr. Bingham's subliminal self was reading on a little ahead, and that the phrase, "His place could nowhere be found," shot up into his supraliminal consciousness a little too soon, namely, at the moment when it was occupied with the first part of the verse. This may have produced a dream-like confusion, in the midst of which, as in a dream, the phrase was totally misapprehended and applied by the reader to himself, instead of to "the ungodly," to whom it really refers. Thus, it may actually have caused him to lose his place, and so—possibly—the coincidence was produced.¹

In further illustration of this view, I may refer to three cases given in Mr. Myers' paper just referred to (*op. cit.* pp. 411-415). In the first, Mr. Newnham, walking in a field, hears a voice telling him that he will find a certain rare moth—*Chaonia*—on a certain oak. Here, as Mr. Myers suggests, he may subconsciously have seen the moth settle on the oak, and the fact may have been conveyed to his ordinary consciousness in this hallucinatory form. In the second case, M. Adrien Guebard, on a geological excursion, is suddenly reminded of a rare variety of fern with bifurcated fronds (in which he had once been much interested, but had never been able to find a specimen of it), and immediately afterwards sees a plant of the variety in question on the ground close to him. The same experience with regard to this abnormality in ferns occurred to him twice more in the course of the same year. We may suppose that on the two latter occasions he was subconsciously on the look-out for further specimens, which might otherwise have escaped his notice altogether. The third case was an experience that occurred to Dr. Hodgson. Walking in a garden and engaged in a train of thought that made him oblivious to his surroundings, he "regained his consciousness of them suddenly to find himself brought to a stand, in a stooping position, gazing intently at a five-leaved clover." Several years earlier he had been interested in getting extra-leaved clovers, but had not for years made any active search for them.

The same superior alertness of the subliminal self seems to be shown in the familiar cases of dreams dramatised backwards, when the dreamer first becomes consciously aware on waking of the noise or other stimulus which has apparently started the dream, and which, therefore, must have been perceived earlier by his subliminal self.

¹ Even if his attention was drawn away by something external, such as a sudden noise, it may have been a subliminal perception of the content of the verse that prevented him from finding his place again.

The following is an instance experienced by Dr. C. Theodore Green, an Associate of the Society, who writes :—

Birkenhead, *June 28th*, 1896.

While reading Mr. W. R. Newbold's "Sub-conscious Reasoning," *Proceedings S.P.R.*, Part XXX., p. 19, I was reminded of a dream I had last winter which may have been "dramatised . . . from its apparent conclusion to its apparent initiation." I thought I was invited by some friends to an Elizabethan mansion in the north of England, as they knew that I was desirous of seeing a ghost. I was told that I should be sure to see some, as they were as "common as blackberries" nearly every day in that house, and that even the children and servants had got used to them. So I went there in my dream, thinking that I should at last have something veridical for the S.P.R. So I wandered through the quaint galleries of the rambling old house, and slept in it for four nights without hearing or seeing anything in the least supernormal.

But during the fifth night—I was to return home next day—a ghost put its long white cold arm slowly up from under the bed and touched me on the right cheek. I instantly sprang up and seized the arm, feeling sure that I had caught a ghost this time. Alas, I awoke, and found myself sitting half up, grasping the cold iron arm of the bedstead, which had evidently touched my cheek as I rolled over in bed.

I have since then been half awaked several times by touching this same iron bar with hand or face, but have not had any dream connected with it.

Assuming that dreams may be "dramatised backwards" as well as forwards in point of time, it follows that the mental process involved was extremely rapid, for I must have awaked to ordinary consciousness within a very few seconds of grasping that cold iron bar in midwinter.

(DR.) C. THEODORE GREEN.

CASE 31.

Perhaps few experiences are more familiar than to meet with a name or a fact in reading or conversation almost immediately after hearing of it, as we think, for the first time. This familiar experience generally strikes us, even though we are quite aware that the coincidence is due to nothing more than selective attention to a point which—for some reason—interests us, and selective disregard of other points. The same explanation obviously applies to such cases as the three following, the first of which was sent to me by Dr. Leaf. He writes :—

Beechwood, Pembury Road, Tunbridge Wells, *May 5th*, 1898.

[The following is] a sufficiently absurd coincidence which happened to myself.

When I was an undergraduate at Cambridge in 1871, I had an accident at a practice-wicket in cricket, by which two of my teeth were broken out. This was on July 9. I had a good deal of pain, which gave me such a restless night that I was glad, contrary to my usual habit, to get up early next

morning and go to early chapel. In the Psalms for the morning (lviii., 6) came the verse : "Break their teeth, O God, in their mouths."

WALTER LEAF.

CASE 32.

The other two cases were recorded by De Morgan in *A Budget of Paradoxes* (reprinted from the *Athenæum*), 1872, pp. 280-282. Such coincidences must, of course, be met with much more frequently by omnivorous readers like De Morgan than by persons of more limited intellectual interests.

The purely casual coincidence, from which there is no escape except the actual doctrine of special providences, carried down to a very low point of special intention, requires a junction of the things the like of each of which is always happening. I will give [two] instances which have occurred to myself within the last few years ; I solemnly vouch for the literal truth of every part of [both].

In August, 1861, M. Senarmon, of the French Institute, wrote to me to the effect that Fresnel had sent to England, in or shortly after 1824, a paper for translation and insertion in the *European Review*, which shortly afterwards expired. The question was, what had become of that paper. I examined the *Review* at the Museum, found no trace of the paper, and wrote back to that effect at the Museum, adding that everything now depended on ascertaining the name of the editor, and tracing his papers ; of this I thought there was no chance. I posted this letter on my way home, at a post office in the Hampstead-road at the junction with Edward-street, on the opposite side of which is a bookstall. Lounging for a moment over the exposed books, *sicut meus est mos*, I saw, within a few minutes of the posting of the letter, a little catch-penny book of anecdotes of Macaulay, which I bought, and ran over for a minute. My eye was soon caught by this sentence : "One of the young fellows immediately wrote to the editor (Mr. Walker) of the *European Review*." I thus got the clue by which I ascertained that there was no chance of recovering Fresnel's paper. Of the mention of current reviews, not one in a thousand names the editor.

In the summer of 1865, I made my first acquaintance with the tales of Nathaniel Hawthorne, and the first I read was about the siege of Boston in the War of Independence. I could not make it out : everybody seemed to have got into somebody else's place. I was beginning the second tale when a parcel arrived ; it was a lot of old pamphlets and other rubbish, as he called it, sent by a friend who had lately sold his books, had not thought it worth while to send these things for sale, but thought I might like to look at them and possibly keep some. The first thing I looked at was a sheet which, being opened, displayed "A plan of Boston and its environs, showing the true situation of his Majesty's army and also that of the rebels, drawn by an engineer, at Boston, Oct., 1775." Such detailed plans of current sieges being then uncommon, it is explained that "the principal part of this plan was surveyed by Richard Williams, Lieutenant at Boston ; and sent over by the son of a nobleman to his father in town, by whose permission it was published." I immediately saw that my confusion arose from my supposing that the king's troops were besieging the rebels, when it was just the other way.

Another instance of a coincidence produced in great part by selective attention to a subject in which the narrator was specially interested is the following.

CASE 33.

The account of this case was sent to Mr. Myers on the day of its occurrence by Mr. Edward D. Blyth, of 12, Belgrave Crescent, Edinburgh. The account was written by Mrs. Blyth, and corroborative notes by her daughter, Mrs. Henry Anderson, and Mr. Blyth are appended.

12, Belgrave Crescent, Edinburgh, *January 5th, 1893, 8.55 p.m.*

I am at present engaged in tracing the pedigree of an old Irish family of which I am a scion. I expressed a wish to my daughter, Mrs. Henry Anderson, that I could find a book which would give me a full account of the Irish Brigade (time of James II.), two members of which, Colonel Shee and Count D'Alton Shee, were my kinsmen. I expressed this wish several times. Later in the day, accompanied by Mrs. H. A., I went for the *first time* to the Advocates' Library in the Parliament House in the High-street of Edinburgh. Not knowing the way, by mistake we entered a room reserved for members only. Finding we were in the wrong place, Mrs. H. A. said, "Wait here, mamma, while I go and find out where we ought to go," and left me standing in the middle of the large room, surrounded by thousands of books. I crossed the room with no particular end in view, and stopped in front of a division filled with books, and the very title my eyes fell upon was the Army List of the Irish Regiments of King James II., in two volumes, cavalry and infantry, by *J. Dalton*, published in Dublin, in 1860. I have not yet recovered from the curious shock I then received and am immensely impressed. I told my husband of the curious incident when I saw him after returning home.

KATE M. BLYTH.

Mrs. Henry Anderson writes:—

January 5th, 1893, 9.25 p.m.

I can testify to the above occurrence. In the morning my mother repeatedly expressed the wish to obtain a book on the subject of the Irish Brigade, in order to find some kinsmen's names. I left her, as stated, in the large private room, and coming back after an absence of four or five minutes, met her coming out through the swing doors with such an agitated expression that I thought she must have had a start of some kind. She said immediately, "Kate, such a very extraordinary thing happened just now. I went towards one of the divisions of books, and the first title my eyes fell on was ——," and she gave the name of the book. She was quite startled and nervous all the afternoon and evening.

KATE HENRY ANDERSON.

Mr. Blyth adds:—

January 5th, 1893, 9.40 p.m.

The above accounts are in perfect harmony with what my wife and daughter related to me some hours ago.

EDWARD D. BLYTH.

This case is clearly not an accidental coincidence. It must, indeed, have been merely by chance that the particular part of the book-case Mrs. Blyth first approached happened to contain a book with the information she wished for ; that is, it was not her wish that caused the book to be in that part of the room, or, indeed, in the room at all. But given that there was such a book in that part, it was not chance that led her at once to pick it out, but her interest in the subject, making her on the alert for anything relating to it.

CASE 34.

The following case is extracted and somewhat abridged from an article on "Psychical Research and Coincidences," by Professor J. H. Hyslop, in *The Psychological Review* for July, 1898. It relates to some of the experiences of Mrs. D., a lady with whom Professor Hyslop has been long acquainted (an earlier report of her crystal visions was given by him in the *Proceedings S.P.R.*, Vol. XII., p. 259). The central incident in the present account is a premonition of Mrs. D.'s of the death of one of her children. The case therefore properly belongs to the preceding section, but I place it here on account of the very careful and complete analysis of all the evidence for its possibly subliminal causation given by Professor Hyslop, which—for want of space—I do not reprint in full, but for which I may refer the reader to his original article.

Some time in July, 1897, Mrs. D. had a strong impression that some unusual "burden" was going to fall upon the family. She could describe the feeling in no other way, and it will be noticed that the expression is a common one with religious minds, which often employ the term to denote a providential affliction. This meaning Mrs. D. gave to the term herself. But the feeling was too vague to identify with any past cause or any incident to be forecasted in the future. Mrs. D. was in good health ; there was nothing in her physical condition that would suggest a clear physical cause of such a feeling. I am not implying that there were no such causes, for there may have been conditions that a skilled physician would detect. But to the consciousness of the subject there was no indication of indisposition of any kind. In fact, she has answered all my inquiries on this point to the effect that her peculiar experiences always occur most frequently when her health is at its best, so far as her own judgment can determine. Throughout the whole period over which the present narrative extends her health was good. In the month of August this premonitory feeling repeated itself very frequently, and became so annoying that Mrs. D. mentioned it to her husband, who confirms her statement in regard to both facts, and hence supports the supposition that the location of the experience previous to its real or supposed fulfilment is not due to an illusion of memory. Finally, the feeling became so intense and persistent that Mrs. D., as is often the case with religious minds as deeply imbued with piety as is her own, sought relief in prayer. But though this resource had in her estimation

been effective in other cases where it had been instigated, as might well be in a mind so sensitive to automatisms as is her own, yet the feeling could not be dismissed.

To make the matter clearer it is necessary to anticipate the sequel of the story, to which the incidents of the narrative are supposed to refer. This is that the little daughter, whom I shall call Lettie, and who was just one year and nine months old, died on December 2nd, 1897, from the burning of its cradle.

At odd times between August and December Mrs. D., in her thoughts about the child's future and while planning some little thing for her, would hear a voice saying, "She'll never need it." One of these occasions was the following: The family live in a house with few accommodations for a clergyman who requires a study, and Mrs. D. planned to give Lettie a certain room for a bedroom when she grew older, and was running over how she would furnish it, and this voice came as described. It was not exactly what one could describe as an external voice, nor, again, a mere thought impression or product of the memory and imagination, as we usually characterise such things, but one of those internal voices with which psychical researchers have become familiar and which Mrs. D. herself distinguishes as neither a real voice nor a memory reproduction, but an impression with all the characters of a real voice except the sense of external reality.

There were many repetitions of this voice in about the same language. One of them occurred about two weeks before the child's death. Mrs. D. had resolved to write a little diary which she could give to the child when it became older. She wrote down two separate accounts on different days of certain events having an interest to the little girl. But, while writing them, this voice came as before: "She'll never need them." The day before the child died the same voice appeared, and on the morning of its death the child was running about the house in a rather dilapidated pair of shoes, when Mrs. D. remarked to the child that her feet must be cold and thought she must have a new pair of shoes. In the midst of her thoughts came the voice again, "She'll never need them." It must be added also that, previous to the impression of a coming "burden" above described, this voice had been heard several times.

About a week before the child's death Mrs. D. thought she smelled fire at night, and feeling afraid of it went to the cellar to look after the matches and to see that there was no danger. She found no traces of fire and nothing to explain her impression. But from that time she began to be careful about matches, seeing that they were in safe places and out of reach. She even went so far as to look over the house for the matches, and felt a strong impulse to burn all parlour matches which were of that kind that is easily lighted. Once the impulse to do this was attended with something like a voice warning her to the same end, and about the danger of fire. Nothing definite enough having been suggested by the voice to guide her actions directly, Mrs. D. could only imagine the necessary precautions, and finally thought to hang a dripping pan in front of the range fire, a thing Mrs. D. had never done before, to prevent coals from falling out during the night. Nor had any apprehensions of this kind ever been felt before, within

her recollection, and there were no special reasons to suppose that any danger of fire in this way existed.

On the morning of the child's death, and during family worship, another incident of some interest occurred. In the midst of the petition for individual members of the family, when she came to the phrase with which she besought divine care for each one, and attempted to apply it in behalf of Lettie, though no difficulty was encountered in the case of the other children, in this case something seemed to stop Mrs. D.'s voice, and she could not repeat the usual language. She recalls no similar previous experience.

On the same morning, about an hour before the fatal disaster, the propulsion to destroy the matches that were dangerous became stronger and stronger, until Mrs. D. turned and reached for the box to destroy it. But as she picked it up she thought, No ; L. (the elder boy) is gone, and she thought that she might need the matches to light the gas stove. She then said aloud to herself, "I'll destroy it as soon as he comes back." She then went on with her work in the kitchen. When the time came, about ten o'clock, Lettie was taken up to her crib for the morning sleep, and as Mrs. D. was putting her into the cradle a voice, such as has been described above, said : "Turn the mattress." This Mrs. D. was accustomed to do, though she had never experienced any voice before in connection with it. But, being in a great hurry, she simply said in a motherly way to the child that she would turn the mattress after the child had taken her nap. She then went downstairs to her work. After a while she heard the child cry, and hurrying up to the room, found the crib and its bedding on fire, and the child so badly burned that it died in three hours.

The only possible way to account for the accident was to suppose that the child had found a match, possibly in the crib or on the mantelpiece, which she could reach, and, lighting it, had set its bedclothes on fire. The other two children were not present. L. had gone down town on an errand and E., the younger boy, was at school. No fire was on this floor of the house, but in the kitchen and the dining-room, both below.

Now, another incident of much interest had occurred many times during the two or three years' residence of the family in this house. Mrs. D. had often had a visual apparition of this very crib on fire, but, as her apparitions or visual automatisms are very frequent, she had not thought to assign it any meaning or possible coincidental value until after the accident.

These were the experiences of Mrs. D. previous to the event, but there were two other incidents by other persons than Mrs. D., that lend themselves to a construction of coincidence in connection with the accident. The first is exactly like the one narrated as occurring at family devotions. Mrs. D. has a sister living in Connecticut, some seventy-five miles from B., the home of Mrs. D. No correspondence has passed recently between them, and the sister was not given to as devotional a life as Mrs. D. It must also be remembered that the sister had ridiculed Mrs. D.'s stories of her experiences, and even went so far as to half jestingly criticise Mrs. D. for her extravagant piety. She discouraged Mrs. D.'s tolerance of possible significance in many of the coincidences which I have recorded in the

Proceedings of the Society for Psychical Research (Vol. XII., p. 259 *seq.*), when they were the subject of conversation. But on hearing of the child's death she came to B. and narrated an experience of her own. It was to the effect that about a week before the death of the child she had had such an experience as she had never had before. An overwhelming impression of some great calamity to occur in the "family" (the incidents show that the term included the whole family connections), and the impulse arose in her to pray for each one, which she did, feeling, as she expressed it in her narrative, that this was an unusual procedure for her. She went over each person among parents and relatives, until she came to the child, Lettie, when her voice suddenly stopped and she could not pray for her as for the others. She finally managed, however, to utter with struggling voice a petition for "our little blossom," the name which she was accustomed to apply to Lettie when speaking of her.

The second incident was an experience of the next door neighbour to the D.'s. I shall call the lady who had it Mrs. G. On the afternoon of the child's death Mrs. G. came in about three o'clock and, à propos of the accident, remarked that on the night before, I believe it was, she had been wakened by the fear of fire and had gone down to the cellar to search for it, and exclaimed while making the search: "Oh! if our little baby would burn up!" Her own child was about the age of Lettie. I ascertained the facts of Mrs. G.'s experiences from her own statements, and found that they were exactly as told me by Mrs. D.

The night after the burial of the child Mrs. D. could picture to herself nothing but the little coffin and the grave. To remove the unpleasant feeling Mrs. D. prayed to have a realising sense and the power to know that the child was a spirit and did not lie in the grave. At this time she was at the home of her sister, whither the family had gone to seek a burial place. One morning, soon after this prayer, she awakened and lay for an hour thinking over family affairs. The sun was shining brightly in the room, and while thinking, suddenly she saw a form by the bedside, and, turning, saw an apparition of little Lettie with her hands on the bedside and smiling at Mrs. D. By her side was the form of a woman, holding her hands about the child, as if to assist it. Mrs. D. sprang up in bed and unconsciously exclaimed, "Good morning, Lettie," and both figures immediately vanished. The forms were transparent and objects could be seen through them. The grown form was not recognisable as any one that Mrs. D. knew. Mrs. D. retained a strong sense of satisfaction from the vision. She is disposed to interpret it as a providential comfort for her sorrow.

At the end of December another incident took place that will have some interest. This time it was the experience of the little boy E. It was first told me by Mr. D. The following letter from Mrs. D. narrates the details of the occurrence:—

"January 5th, 1898.

"You requested a note of E.'s recent experience. It occurred on Thursday eve, December 30th (1897).

"I lay down on the sofa to rest in the evening, and, as he often does, he climbed back of me to rest with me. I do not remember what my thoughts were, but feel quite confident I was not thinking of my experience at

S—, Conn., when E. said: 'Mamma, is little Lettie air now? Is she like smoke?' Why, darling? 'Cause I just saw her and put my arms around her and she was like air.' I will endeavour to keep account of anything further.

"E— D—."

On inquiry about the incident I could find no trace of any story to the child that might lead to a belief on its part in such a reality as its experience might be taken to describe. The child was only four years old. The incident impressed both parents as very striking, and they were evidently puzzled by it, having a strong aversion to the apparent meaning of such occurrences.

The apparition of the burning cradle is one of the most striking coincidental features of the whole narrative. The fact is that the crib stood within a few feet of a fire grate. But as there had been no fire in this grate for a year or more the accident could not have been caused by this. Mrs. D. herself had all along explained the vision of the burning crib by this very proximity to the fire grate. Almost every one would have such a possibility suggested to the mind by this situation of the crib. But not every one is subject to automatisms, and such thoughts are easily referred to their proper source in association. Mrs. D., however, as we have found, is liable to these occurrences. Now it is the unusual occurrence and character of automatisms that call special attention to them. They are easily remembered as interesting and significant if any coincidence with them is remarked. If the accident of the child's death had occurred only in connection with an *association* of a burning crib, every one would have dismissed it as a coincidence not worth taking seriously, and no significance would be given it. But when an accident of this sort occurs in coincidence with an *apparition* apparently premonitory in character, we forget association and are tempted by the unusual nature of the phenomenon to ascribe it a value that it may not deserve. We may concede that such an experience might have some significance if not connected with automatisms as frequent or habitual occurrences. But here we have in this very subject the existence of automatisms which can be traced directly to emotional influences of various sorts. There is a frequent connection between past thoughts and associations and certain sensory automatisms, and we have only to suppose this case one of them in order to explain it in a natural way.

SECTION II.

Coincidences possibly or probably due to Telepathy.

Among all those apparently supernormal faculties which form the subject of the investigations of the Society for Psychical Research, Telepathy is probably the one whose existence most persons interested in psychical research regard as the most firmly established. I therefore place in my last section coincidences suggestive of Telepathy. I cannot claim that the cases given here add in any at all material

degree to the evidence for telepathy already published, and several of my most evidential ones have already been printed in the *Journal*. No one denies that merely accidental coincidences between the thoughts and expressions of two different persons often occur, and we cannot, of course, treat any coincidences as telepathic unless we have strong reason for thinking that they are not accidental. But since, as already observed, we have no means at present of discovering for certain whether telepathy has operated or not in any individual case, because of our ignorance of its method of operation, the main object of this section is to illustrate the difficulty of drawing the line between accidental and telepathic coincidences. For this purpose I arrange my cases in a sort of rough order of merit, beginning with those that seem to me evidentially weakest and ending with the stronger ones.

The weakness of the first case consists in the absence of any sort of connection between the persons whose thoughts coincided.

CASE 35.

This case, which was recorded a few days after it occurred, was sent to Mr. Myers by Mr. L. C. Powles, an Associate of the Society. He writes:—

9, Queen's Gate Place, London, S.W., *March 1st, 1898.*

DEAR SIR,—I hardly know whether the dream given on the other side of this is worth sending you. I should be more inclined to set it down as a mere coincidence if it were not that I am somewhat easily impressed by the thoughts of others. I ought to add Mrs. Wellesley is known to me.—
Yours truly,

L. C. POWLES.

March 1st, 1898.

On the night of the 26th ult. I had a vivid dream to this effect; that I had determined to commit suicide and had made arrangements to do so, even to the extent of ordering my coffin of the village carpenter; but I was profoundly disinclined to carry out these arrangements when the time had come and confided to a friend that *I should put off the deed till Thursday next*. My friend in the dream replied grudgingly, "Well, I suppose it can be managed." This dream, which I attributed to a reminiscence of a book I had been reading the day before, *The Early Life of Wordsworth*, I related to Lady Eden and Mr. Wellesley on the 27th.

[These persons add]

We certify this { F. EDEN.
VICTOR WELLESLEY.

On the 28th came a letter from the Hon. Mrs. Wellesley to Mr. Wellesley, dated 26th February, Wiesbaden, of which I send you part. The coincidence is very curious.

The passage in the letter in which the coincidence occurs is as follows:—

Homan's establishment is in a great state of excitement and scared, as a chap there contemplates committing suicide and goes about with a revolver. *He has now put it off till next Thursday.*

CASE 36.

The following is a coincidence whose triviality throws doubt on its telepathic character. It was obtained by Professor Harlow S. Gale, of the University of Minnesota, Minneapolis, from Mrs. Charles T. Jerome, who writes to Professor Gale:—

Minneapolis, *February 20th, 1896.*

. . . On the afternoon of October 29th, 1895, at ten minutes past four, I stepped upon a chest in my closet to reach my muff which was lying on a high shelf. As I turned, my muff brushed across the top of the door casing, and a key fell to the floor. I picked it up and recognised it as the key to a cash box belonging to my son, Albert C. Jerome, which he had supposedly lost about a year previous.

When he returned home in the evening I said, "Albert, I found the key to your cash box to-day." "Did you?" he replied, "I was thinking about that key *like everything* this afternoon." "Have you thought of it frequently?" I asked. "No," he replied, "I haven't thought of it before in six months that I remember. I had given up finding it."

I inquired at what time he was thinking of it, to which he answered, "A little after four o'clock, about ten minutes after. I know the time because I was thinking about it just as I reached the Great Western freight depôt, and looked at the clock as I entered, not with reference to that, but on account of my errand there I took note of the time." I had not then mentioned at what time I found the key.

I had also noted the time, simply because I was just then going out to drive, and taking my muff down passed immediately out.

Incidents of a similar nature have several times occurred between Albert and myself, but I have never regarded them of sufficient importance to interest any one but ourselves. . . .

MRS. CHAS. T. JEROME.

As far as I am concerned this is O.K.

ALBERT C. JEROME.

On the afternoon of October 29th, 1895, very shortly before ten minutes past four, Mrs. Charles T. Jerome was standing on a chair-seat before an open chamber door. To keep her balance she put forth her hand, laying her fingers upon the ledge above the doorway, and at the same time, by merest accident, upon the key of her son Albert's cash box—the key having, no doubt, been laid there to hide it; but the place and circumstance of its hiding had been quite forgotten. Indeed, it had been missing for about a year, and its loss had been nearly dropped from memory.

Upon her son's return she told him of her finding his lost key. He asked when it was found; and learning that, exclaimed in surprise that at

just about that time he had been thinking very intensely of the key, and said furthermore that he did not remember having thought of the key before in six months.

I knew the time when the key was found, because it was just before going to drive with my mother, and at starting looked at the clock.

Albert knew approximately the time when he was thinking of the key, because he was on his way from McMillan and Co.'s warehouse (cor. 2 St. and 1 Ave. N.) to the Chicago Great Western freight depôt (cor. 10 Aves. and Wash.), and noted the hour and minute of leaving for the depôt.

CHARLES W. JEROME.

The next case, again, may possibly have been telepathic, though there is no proof that Miss M. was thinking of her friends just at the time when they began to talk about her. The coincidence seems to relate to the place rather than the time of the conversation.

CASE 37. [L. 1099.]

The account was received from one of our Associates who wishes that the names and address should not be given.

Cheltenham, *December 11th, 1897.*

. . . My wife was walking with her married daughter in Cheltenham to-day, December 11th, and when they arrived at Montpellier Walk, about the centre of the town, my wife remarked to her daughter that she would like to receive some information about a lady whom she had met several years ago at her daughter's bungalow in Ceylon. They continued talking of this lady for some minutes, and were much surprised to find on their return home that the lady in question had called only half-an-hour previously, and expressed great regret at not finding them at home, as she had to leave Cheltenham by the next train. The coincidence is rendered the more remarkable by the fact that my son-in-law had met the lady, at the exact spot in the town where, about half-an-hour afterwards, my wife had the impression that she would like to receive some news about her. I may add that the lady does not live at, or near, Cheltenham, and beyond knowing that she had left Ceylon, and was residing in England, neither my wife nor daughter knew where she was living, or had the remotest idea that she was in Cheltenham, and would call upon them.

(Signed) [MR. A.]

In reply to inquiries we received the three following letters:—

Cheltenham, *December 17th, 1897.*

DEAR SIR,—On the morning of the 11th inst. I met Miss M. on Montpellier Walk. She had been to call on my wife, and, failing to find her at home, was walking down town in hopes of meeting her. I turned and walked with Miss M. for some time, looking for my wife until it was time to return to where she was staying, as she had to catch the train to Malvern. At lunch my mother-in-law remarked to my wife that it was a curious coincidence they should have been talking of Miss M. on their way home. I

asked if they remembered whereabouts they began speaking of her, and it turned out that it was on the exact spot to a yard where I had met Miss M.—Yours faithfully,

(Signed) [MR. B.]

December 17th, 1897.

DEAR SIR,—I was walking home from town, and when at the Montpellier Walk I said to my daughter (Mrs. "B.") that I wondered if — had arrived from Ceylon and where she would pass the winter. We continued talking about her almost the whole way home. At the Christ Church crossing we met my son-in-law (Mr. "B.") on his bike and he called out something about — which we did not understand. On reaching home we found a letter from this lady saying how vexed she was to miss us, as she was leaving by next train. At lunch my son-in-law remarked how curious we should have talked of — during the morning, and asked where we were when the conversation began. We said, "At the Montpellier Walk," which proved to be exactly where he had met — about half-an-hour before we returned that way.

(Signed) [MRS. A.]

December 17th, 1897.

DEAR SIR,—I was walking home with my mother when she asked me if I had had any news of Miss M. and where she was, and wondered where she was going to live. We began talking of her on the Montpellier Walk, and continued chatting about her nearly the entire way home. At the Christ Church crossing we met my husband on his bike, who shouted out something about Miss M., which we failed to understand. But when we arrived at the house we found that Miss M. had called and was so sorry to miss us, as she was leaving Cheltenham by the next train. At luncheon we were telling my father and husband how curious it was that we were talking of Miss M. so much and that she should be in Cheltenham, and my husband asked us where we began to speak of her, and we told him at the Montpellier Walk, which proved to be exactly where he had met our friend before we returned that way.

(Signed) [MRS. B.]

CASE 38.

In the following the detailed nature of the agreement suggests more strongly, though still by no means conclusively, that the coincidence was due to telepathy. I received it from Mr. A. Berry, Fellow and Lecturer in Mathematics of King's College, Cambridge, who writes to me:—

Fairseat, Wrotham, April 3rd, 1898.

DEAR MISS JOHNSON,—The circumstances relating to the coincidence of which I was speaking are as nearly as I can recollect as follows:—

I was taking part in an Intercollegiate Examination in Mathematics in 1890. In addition to papers on specific subjects, there is also a paper of problems, which may be chosen from any of the subjects of examination. The other papers contain "bookwork" questions and problems, or "riders," connected with them. Shortly before the examination it appeared that Mr. A. Larmor, of Clare, and I proposed to set closely similar problems—one in the "problem paper" and one in the paper on a specific subject, Statics.

I find that Mr. Larmor's version was actually printed and set in the Statics paper. I infer that I was setting mine in the problem paper, but I have no independent recollection of this. The problem paper is made up by contributions from each of four Colleges which join in the examination ; it is edited by one examiner and circulated in proof to all. I imagine that Mr. Larmor saw my question in proof and then noticed that it was substantially his own. My question was then withdrawn.

I was not able to find the question among the old examination papers which I possessed, but satisfied myself by a process of exclusion that the year must have been 1890, and after leaving Cambridge asked Mr. Richmond, giving him this date, to send me a copy of the question (Mr. Larmor's version), which I enclose.

I have no copy of my own version, but my recollection is that the *data* of the problem—as contained in the first sentence—were identical, but that I only asked for a certain result which expressed the stresses (tensions) along the four strings, whereas Mr. Larmor asked *also* for the stresses along the rods, and expressed the whole result in a singularly elegant form, which I had missed. I do not think that the result asked for is nearly as characteristic of a problem of this class as the *data*, so that the resemblance between our two versions is greater than might perhaps at first sight appear.

A new text-book on Statics (by J. Greaves of Christ's) had recently come out, and it is possible that something in that may have suggested the problem to both of us, but I have no recollection of anything of the sort.

I have written to Mr. Larmor for his recollection and enclose a copy of the letter, so that you may be able to judge how far his reply is suggested by my letter. When I hear from him I will send the letter on.—Yours sincerely,

ARTHUR BERRY.

The copy of the question as finally printed, furnished by Mr. Richmond to Mr. Berry, was as follows :—

A parallelepiped formed of twelve weightless rods freely jointed together at their extremities, is in equilibrium under the action of four stretched elastic strings, connecting the four pairs of opposite vertices. Show that the figure is its own stress-diagram.

The following is Mr. Berry's letter to Mr. Larmor, asking for his recollection of the incident :—

April 3rd, 1898.

DEAR LARMOR,—Do you happen to remember any details of the circumstances under which we both set, or wanted to set, a problem about a parallelepiped in the *Mays*, about 1890? A friend of mine is interested in coincidences, and has asked me to get, if possible, your statement of the matter as well as my own. If you could give me your recollections I should be much obliged.—Yours sincerely,

ARTHUR BERRY.

Mr. Larmor replied :—

April 6th, 1898.

DEAR BERRY,—My recollection of the curious coincidence to which you refer is as follows :—Clare, Caius, King's and Trinity Hall were grouped for

examination purposes ; you were one of the examiners representing King's, and I one of those representing Clare. I believe Statics was one of the subjects assigned to me, and we were each requested to supply three or four problems for the problem paper, representing more or less the subjects assigned to us for the ordinary papers. The procedure was as follows. An editor was appointed who should collect the problems from the various examiners, arrange them, and see to the printing of the problem paper, its circulation among the examiners, and the insertion of corrections suggested by them. This was, I believe, Stearn (or Bell?). The problem was set by me as an example of the principle of Virtual Work. On receiving the proof of the problem paper, I saw that it had been printed twice and drew the editor's attention to it. It turned out, however, that the explanation was that you had also sent it in among your problems. While the problems were practically identical, I recollect that my version was a trifle more general than yours, *inasmuch as it had reference to the diagonals as well as the edges of the parallelepiped.* [The italics are mine. A.J.] This version was retained in the paper and you sent a substitute for the other. We were both struck at the time by the curious coincidence and discussed the matter, but were unable to come to the conclusion that we were led to set the problem by any train of ideas we had in common.

The problem was quite new, I had not discussed it with anybody, and the editor of the problem paper was the only person to whom I had communicated it.—Very sincerely yours,

ALEX. LARMOR.

Mr. Berry wrote, enclosing Mr. Larmor's letter :—

King's College, Cambridge, *April 8th, 1898.*

DEAR MISS JOHNSON,—Here is Mr. Larmor's "narrative." The only discrepancy which I notice between his account and mine is in the sentence [italicised]. It is quite irrelevant to the coincidence and merely affects the question of our "credibility." I think that my problem referred to the diagonals (strings) only, and he thinks that it referred to the edges (rods) only, omitting the strings, so that virtually the results which he thinks that I asked for and those which I think that I asked for are exactly complementary, and together make up practically what he (as we both agree) asked for.—Yours sincerely,

ARTHUR BERRY.

I first heard of this case orally from Mr. Berry, when he drew a diagram of a parallelepiped with dotted lines connecting the four pairs of opposite vertices, to explain to me what the problem was. This diagram shows that his recollection of the general nature of the problem at that time was correct, as afterwards verified by reference to the printed question.

CASE 39.

The next is a series of three coincidences,—all apparently purely accidental with the possible exception of the first,—taken from *Over the Teacups*, by Oliver Wendell Holmes (3rd Edition, 1891, p. 12, *et*

seq.) We are told in the Introduction that the part of the book containing these cases was written in March, 1888.

(p. 12) . . . I relate a singular coincidence which very lately occurred in my experience. . . . I will first copy the memorandum made at the time :—

“Remarkable coincidence. On Monday, April 18th, being at table from 6.30 p.m. to 7.30, with — and — [the two ladies of my household], I told them of the case of ‘trial by battel’ offered by Abraham Thornton in 1817. I mentioned his throwing down his glove, which was not taken up by the brother of his victim, and so he had to be let off, for the old law was still in force. I mentioned that Abraham Thornton was said to have come to this country, ‘and [I added] he may be living near us for aught that I know.’ I rose from the table and found an English letter waiting for me, left while I sat at dinner. I copy the first portion of this letter :—

‘20, Alfred Place West (near Museum), South Kensington,
London, S.W., April 7th, 1887.

‘DR. O. W. HOLMES,—DEAR SIR,—In travelling the other day I met with a reprint of the very interesting case of Thornton for murder, 1817. The prisoner pleaded successfully the old Wager of Battel. I thought you would like to read the account, and send it with this . . . —Yours faithfully,

‘FRED. RATHBONE.’”

Mr. Rathbone is a well-known dealer in old Wedgwood and eighteenth century art. As a friend of my hospitable entertainer, Mr. Willett, he had shown me many attentions in England, but I was not expecting any communication from him ; and when, fresh from my conversation, I found this letter just arrived by mail and left while I was at table, and on breaking the seal read what I had a few moments before been telling, I was greatly surprised, and immediately made a note of the occurrence, as given above.

I had long been familiar with all the details of this celebrated case, but had not referred to it, so far as I can remember, for months or years. I know of no train of thought which led me to speak of it on that particular day. I had never alluded to it before in that company, nor had I ever spoken of it with Mr. Rathbone. . . .

The case I have given is, I am confident, absolutely free from every source of error. I do not remember that Mr. Rathbone had communicated with me since he sent me a plentiful supply of mistletoe a year ago last Christmas. The account I received from him was cut out of *The Sporting Times* of March 5th, 1887. My own knowledge of the case came from *Kirby's Wonderful Museum*, a work presented to me at least thirty years ago. I had not looked at the account, spoken of it, nor thought of it for a long time, when it came to me by a kind of spontaneous generation, as it seemed, having no connection with any previous train of thought that I was aware of. I consider the evidence of entire independence, apart from possible “telepathic” causation, completely waterproof, airtight, incombustible, and unassailable.

(p. 18). I referred, when first reporting this curious case of coincidence, with suggestive circumstances, to two others, one of which I said was the

most picturesque and the other the most unlikely, as it would seem, to happen. This is the first of those two cases :—

Grenville Tudor Phillips was a younger brother of George Phillips, my college classmate, and of Wendell Phillips, the great orator. He lived in Europe a large part of his life, but at last returned, and, in the year 1863, died at the house of his brother George. I read his death in the paper; but, having seen and heard very little of him during his life, should not have been much impressed by the fact but for the following occurrence : between the time of Grenville Phillips's death and his burial I was looking in upon my brother, then living in the house in which we were both born. Some books which had been my father's were stored in shelves in the room I used to occupy when at Cambridge. Passing my eye over them, an old dark quarto attracted my attention. It must be a Bible, I said to myself,—perhaps a rare one,—the “Breeches” Bible or some other interesting specimen. I took it from the shelves, and as I did so, an old slip of paper fell out and fluttered to the floor. On lifting it I read these words : *The name is Grenville Tudor.* What was the meaning of this slip of paper coming to light at this time, after reposing undisturbed so long? There was only one way of explaining its presence in my father's old Bible,—a copy of the Scriptures which I did not remember ever having handled or looked into before. In christening a child the minister is liable to forget the name just at the moment when he ought to remember it. My father preached occasionally at the Brattle Street Church. I take this for granted, for I remember going with him on one occasion when he did so. Nothing was more likely than that he should be asked to officiate at the baptism of the younger son of his wife's first cousin, Judge Phillips. This slip was handed him to remind him of the name. He brought it home, put it in that old Bible, and there it lay quietly for nearly half a century, when, as if it had just heard of Mr. Phillips's decease, it flew from its hiding place and startled the eyes of those who had just read his name in the daily column of deaths. It would be hard to find anything more than a mere coincidence here; but it seems curious enough to be worth telling.

The second of these two last stories must be told in prosaic detail to show its whole value as a coincidence.

One evening while I was living in Charles Street, I received a call from Dr. S., a well-known and highly respected Boston physician, a particular friend of the late Alexander H. Stephens, vice-president of the Southern Confederacy. It was with reference to a work which Mr. Stephens was about to publish that Dr. S. called upon me. After talking that matter over we got conversing on other subjects, among the rest a family relationship existing between us,—not a very near one, but one which I think I had seen mentioned in genealogical accounts. Mary S. (the last name being the same as that of my visitant), it appeared, was the great-great-grandmother of Mrs. H. and myself. After cordially recognising our forgotten relationship, now for the first time called to mind, we parted, my guest leaving me for his own home. We had been sitting in my library on the lower floor. On going upstairs where Mrs. H. was sitting alone, just as I entered the room she pushed a paper across the table towards me, saying that perhaps it might interest me. It was one of a number of old family papers which she had brought from the house of her mother, recently deceased.

I opened the paper, which was an old-looking document, and found that it was a copy, perhaps made in this century, of the will of that same Mary S., about whom we had been talking downstairs.

If there is such a thing as a purely accidental coincidence, this must be considered an instance of it. All that one can say about it is that it seems very unlikely that such a coincidence should occur, *but it did*.

Dreams about letters, coinciding with their actual arrival and correctly representing some part of their contents, are comparable to Dr. Holmes' first case, but belong to a class which is stronger evidentially, since dreams about any subjects must be on the whole less frequent than mere impulses to talk about them. I give two cases of such coincidental dreams.¹

CASE 40. [L. 955.]

The first comes from Sir Lawrence Jones, Bart.²; the account has the advantage of having been written on the day of the incident.

Ventnor, March 23rd, 1891.

I was called at eight this morning, and my letters left outside the door. I fell asleep again, and had what seemed a long and troublesome dream about a cheque which I had to fill up and sign. At nine I awoke, with a vivid recollection of my dream, got up, opened a packet of letters forwarded from home, and found among them a registered letter containing a cheque for a large sum, which I had to sign as trustee.

L. J. J.

N.B.—I had no reason to expect the receipt of the cheque. The dream was not in any way concerned with the real cheque, but was rather my ineffectual attempts to draw a cheque properly on a blank sheet of paper. But the coincidence was very remarkable.

Lady Jones writes :—

April 1st, 1893.

I can entirely corroborate from my own memory the story of my husband's dream about the cheque.

EVELYN M. JONES.

¹ For other instances of this kind, see the *Journal S.P.R.*, Vol. VI., pp. 103 and 105, and Vol. VII., p. 257. In connection with these cases, I may refer to a curious non-coincidental case published in the "Report on the Census of Hallucinations" (*Proceedings*, Vol. X., p. 91) of an extremely vivid, though apparently not fully externalised, vision of a number of letters lying on a (real) table in a room adjoining that in which the percipient was lying in bed. The door was shut, so that he could not really see the table; but along with the vision came the conviction that the letters contained news of the death of all his relations. On going into the room, he found no letters there and nothing occurred later to confirm the presentiment. The percipient (who is personally known to some members of the Committee who drew up the "Report on the Census") declared that he was certainly awake and with his eyes open when the vision occurred, and he seems to have had no sense of awakening from it.

² For another experience of Sir Lawrence Jones', see below, p. 290.

Sir Lawrence Jones writes later :—

Cranmer Hall, Fakenham, *April 1st, 1893.*

I related my dream to Lady Jones immediately after opening my letter. This dream was unusually vivid, and the impression of it remained with me much longer than usual. I dream a good deal, but rarely remember anything except in the case of morning dreams, when I have woken early and gone to sleep again.

CASE 41. [L. 998.]

The next case rests on unusually good evidence, since it was noted before verification, while the full account of it was written from notes made immediately after by Professor A. Alexander, of Rio Janeiro. He says, in sending the account :—

February 4th, 1896.

My informant, Senhor Nascimento, is a life member of the "Society of Arts," and received his technical education in London.

Rio, February 3rd, 1896.

A recent case of apparent clairvoyance has been communicated to me by a Brazilian engineer, called José Custodio Fernandes do Nascimento, who is himself the percipient. I have been acquainted with this gentleman for several years and know him to be a careful and trustworthy witness. It will be seen that he has enabled me also to give direct testimony to the care with which he has provided for proper evidence.

In thus proceeding, he endeavours to atone for former laxness, inasmuch as some seven or eight years ago he failed to take adequate note of a probably veridical dream, in which he seemed to be trying to escape with his family from the deck of a burning vessel, and to witness the jumping overboard of a man whose clothes had caught fire. A telegram from a northern Brazilian port subsequently gave the news that about that time fire had broken out on board a certain vessel, and that on the occasion some individual had in reality jumped into the sea, more or less in the manner perceived in the dream.

Shortly after three p.m. on Saturday, January 11th, 1896, I met Senhor Nascimento in the Rua do Ouvidor in this city, and he at once gave me verbal particulars of a second experience of the kind, which he had had on that very date.

He stated that, as the result of slackness in his business, he had lately been straitened for want of means, and had felt this impecuniosity all the more that his eldest daughter is shortly to be married to the son of a Don J., a merchant resident in Montevideo.

On the preceding evening of the 10th, the young man J. came to visit his *fiancée* at the usual hour, and mentioned that a letter from his father was waiting for Senhor Nascimento in the Rua da Alfandega at the firm of Jorge Dias Brothers, the correspondents in Rio of Don J. He had not brought it himself, as it was to be delivered to Senhor Nascimento personally.

My informant awoke on the following morning at an early hour, and fell again into a state of slumber between 5 and 6 o'clock. He then dreamt

that he had called at Dias Brothers and that they handed him a present from Don J. of one conto of reis (about £40), which he was so glad to receive that he embraced the members of that firm with an effusion of tears. In the dream he seemed to count the money.

He rose with the conviction that his vision would be realised, although no ordinary reasons concurred to make him suppose that such would be the case. This belief led him to write down on a slip of paper (which is herewith enclosed) the following note :—

“Sonhei que ao ir receber a carta dos Senhores Jorge Dias estes me entregaram a somma de 1 : 000 \$ 000 de reis, e que eu comovido abraçei-os chorando.”

11-1-96.

(“ I dreamt that on going to receive the letter from Senhores Jorge Dias, the latter delivered to me the sum of 1 : 000 \$ 000 of reis, and that I, being moved, embraced them with tears.”

11-1-96.)

Senhor Nascimento said nothing to his wife or children about the dream. He merely put the above note under other papers in a pigeon-hole of his bureau, which he then locked. He went into town ; called at half-past ten at the house of Jorge Dias Brothers, and received the letter, which he afterwards opened in the street. This letter he showed to me when we met. In it Don J. makes a present of one conto of reis to his future daughter-in-law, and instructs Senhor Nascimento to draw the money at the house of John Moore and Co. of this city. This sum Senhor Nascimento had duly received about 1 o'clock on that day, and he invited me to accompany him home to verify what he had stated regarding the note taken in the morning. The conto of reis was shown to me ; the bureau was opened in my presence, and the slip of paper was taken out of the pigeon-hole and immediately delivered into my keeping.

On Monday the 13th, I returned for further information. By direct questioning, Senhor Nascimento had learnt that his friends, the Dias Brothers, were not aware of the contents of the letter at the time of its receipt. A similar declaration was made in my hearing by the young man J. who added, however, that he had afterwards (*i. e.*, at an hour later than that of the dream) been informed by a brother of his what their father had done.

John Moore and Co. are not personally known to my informant. J.'s brother has no other connection with him than that established by the coming union between the families, and yet the dream coincided with the arrival of the letter at Rio and not with its despatch from Montevideo. The circumstances of the case, then, seem to render the explanation by clairvoyance more plausible than that by telepathy.

Senhor Nascimento states that, although he sometimes has waking presentiments, the two dreams above narrated are the only vivid ones of the kind he recollects having had in his experience. He does not remember ever receiving similarly positive indications in a vision, which have remained unfulfilled.

(The above is written out from notes taken by me on the date of the occurrence.)

A. ALEXANDER.

U 2

Professor Alexander's account is confirmed as follows by the percipient :—

Rio, *February 3rd, 1896.*

I can testify to the fulness and exactness of all the details above given.

I still have a vivid remembrance of the dream of the burning vessel, the confirmation of which came on the same day. A man was reported to have jumped overboard with his clothes on fire, just as I saw him in my dream.

I reside at No. 33, Travessa de São Salvador, Haddock Lobo, half-an-hour's journey from town in the tramcars, and I never come home during the course of the day.

JOSÉ C. FERNANDES DO NASCIMENTO.

The original note, made by Senhor Nascimento on the morning after his dream and before its verification, was sent with the narrative by Professor Alexander.

Cases referring to the arrival of persons may be compared with those relating to the arrival of letters. Here is one in which the impression about the arrival seems to have coincided with the supposed agent's planning of his arrival. It is in one respect somewhat stronger evidentially than the cases just given, since the impression was a waking one—though possibly an illusion rather than a hallucination. On the other hand, the incident to which the impression referred was of a less unusual kind.

CASE 42. [L. 995.]

This case was received from Mrs. C. R. Griffing, an Associate of the American Branch of the S.P.R., who is well-known to Dr. Hodgson. She made the following note of what she had heard from the percipient, her daughter-in-law, before they knew that it had any correspondence with the actual facts :—

Box 14, White Plains, New York, *June 13th, 1891.*

Just now I am feeling much worried about my son Horace, who is away. My sister, who has many times made correct predictions, wrote me that she saw Horace very ill, and last evening my daughter-in-law, after she went to her room, heard Horace, as she supposed, enter the basement kitchen ; she heard him rock in his favourite large chair and move about. This morning, when I went down to breakfast, she said, "Horace came, didn't he ?" She was so sure that he came that she had more coffee made for him. Just now I am the only one who drinks coffee. She thought she heard me early this morning ask him how he was feeling. All this may not be premonitory, but it worries me.

JANE R. GRIFFING.

Her son actually returned home the next day, and Mrs. Griffing wrote shortly after :—

[*June 28th, 1891.*]

The morning of June 13th, when I went down to breakfast, my daughter-in-law remarked carelessly, "Horace came last night, didn't he ?" "No,"

I said, "he did not come." "But I heard him come," she exclaimed, in a surprised tone. "Are you sure he isn't here?" I answered that I was sure. "But," she persisted, "did you look into the room? I am sure I heard him." It was difficult to convince her that he was not somewhere about. He had been away only a few days, was well, as far as we knew, and we had no reason to be anxious about him. The next day he returned and told me that he came very near coming the evening before, but after talking it over with Charley he decided to wait until the next day. They both told me of their discussing it about eight o'clock.

JANE R. GRIFFING.

The precipient describes her impression as follows:—

During the evening of June 12th, about eight o'clock, I went to my room. Some time before nine, I heard my brother-in-law, Horace, come to the basement-kitchen door; finding it locked he went to a window, raised it and stepped in. I heard him strike a match, move about the room, and sit down in a large rocking chair. I was so absolutely sure that it was Horace that I did not go down to see who it was, or speak to my mother-in-law about it. I supposed she knew that Horace was there. The reason I was so sure that it was Horace instead of any one else was that my other brother-in-law was in the house, my husband would not come until the next day, and all the movements I heard were exactly like Horace's habits when he had been out and came in by the basement, especially his sitting and rocking in that particular chair.

PHEBE L. GRIFFING.

Mr. C. L. Griffing and his brother give the following account of what they were doing at the time:—

The evening of June 12th, my brother Horace and I were together for an hour or so before nine o'clock. It must have been about eight when we were discussing the question of his returning to White Plains that night, or waiting until the next day and [going] up with me. He was quite inclined at first to go by a late train that evening, but decided to wait until the next day.

CHAS. L. GRIFFING.

My brother's account of the conversation between us in regard to my returning home during the evening of June 12th is correct. It could not have been far from eight o'clock, as we were not together until after seven, and separated before nine.

H. M. GRIFFING.

In answer to Dr. Hodgson's further enquiries, Mrs. Griffing writes:—

Box 14, White Plains, New York, April 1st. [1892.]

In reply to your enquiry, at the time of the telepathic incident of June 12th, it was entirely uncertain when Horace would return; he might come at any time and might not for weeks. He was in the City for a special purpose and did not know what the result would be. I was not expecting him at that time, as it was too soon for him to know what his plans

would be, and he returned only for two or three days. I was not in the least anxious about him, as he was well and in no trouble of any kind.

JANE R. GRIFFING.

CASE 43.

The following case comes from Dr. A. S. Wiltse, whose name will be familiar to readers of the *Proceedings* and *Journal*. His success as agent in many experiments in thought-transference adds considerably to the probability that the apparition was veridical. On the other hand, the absence of anything at all remarkable or strikingly unusual in his condition at the time is, of course, a weak point. The percipient, Dr. Wiltse's son, writes to Dr. Hodgson:—

Lancing, Tenn., *February 15th, 1896.*

DEAR SIR,—On the night of the 15th of January last my father was away from home.

As the night was somewhat cold and there was fire in his room, I occupied his bed. During the night I awoke and saw, as I supposed, my father lying in bed on the front side. He seemed to be sleeping quietly, and his left arm was lying at full length on the top of the coverlet.

I got carefully out of bed, thinking not to awaken him, covered up the fire to keep for morning, and stood close to him looking at him, and cogitating whether to go back to my own bed; but, preferring the warm room, I said to myself, "He sneaked in with me, so I'll sneak in with him." I got in bed, and fell to sleep, but afterwards, waking up, discovered that he was gone. I supposed he had been called away, but learned in the morning that he had not been at home.—Yours truly,

JASON WILTSE.

Dr. Wiltse adds:—

Lancing, Morgan Co., Tenn., *February 14th, 1896.*

MY DEAR DR. HODGSON,— . . . I send an account of an apparition of myself.

My share in the apparition is this:—

On the night in question I was staying at the house of Mr. W. T. Howard, two miles from home. I was tired out, and stopped because I really could go no farther.

I had to sleep with two boys who had bad colds, and was so annoyed with them that I laid awake wishing I was at home in my own room, and picturing to myself the comfortable fire, bed, &c., that I was missing, until I was near at one time rising and going home, but concluded I was feeling too weak to make the journey.

On my arrival home my son told me of seeing me, as he supposed, in my bed before I had mentioned the matter, or, in fact, thought of the possibility of my having produced any telepathic impression.—Yours truly,

A. S. WILTSE.

I give next two cases of apparent thought-transference relating to animals. There is of course no *prima facie* reason why people should not have veridical impressions about animals as much as about any

other beings in whom they are strongly interested. It has often been pointed out that such cases do not involve, of necessity, an animal "agent," since—at least in all that I have ever met with—there is always a human being concerned from whose mind the percipient might have gathered the intelligence, apart from the possibility of independent clairvoyance. It is clear that the idea of a dog might be conveyed from one mind to another as easily as the idea of any other object—neither the dog nor the object taking any part in the process—and a real dog just as well as the imaginary dog, who figures in one of my cases.

It is, however, altogether irrelevant to the main purpose of this paper to discuss how this or that supernormal or apparently supernormal incident may have been caused. I am not concerned to argue that the mind of an animal *cannot* communicate directly—by thought-transference—with the mind of a human being. I maintain only that, however probable such a hypothesis may be, the evidence in our possession so far hardly even suggests it; because the well-authenticated cases of supposed telepathy from animals are so few that they can easily be put down to chance. Regarded, however, as cases of clairvoyance, or of telepathy from one human being to another, they go to swell the bulk of cases of these classes, and consequently to reduce the possibility of attributing them to chance.

CASE 44. [L. 1075.]

This case was received by Mr. Myers in December, 1890,¹ from Mrs. Bagot, writing from The Palace, Hampton Court. Both Mrs. Bagot and her daughter, who confirms the account, are known to Mr. Myers.

In the year 1883 we were staying at the Hotel des Anglais, at Mentone. I had left at home (in Norfolk) in the care of our gardener a very favourite little dog, a black and tan terrier, named Judy. I was sitting at table d'hôte and suddenly saw my dog run across the room, and unthinkingly exclaimed, "Why, there's Judy!" There was no dog in the hotel, and when I went upstairs I told my daughter, who was ill, what I had seen. A few days after I got a letter saying that Judy had gone out with the gardener as usual in the morning quite well, but when he returned at breakfast time she was suddenly taken ill, and died in half-an-hour. At this distance of time I cannot distinctly remember whether the dates agreed, but my impression is that she had died the day I saw her.

MARY BAGOT.

Mrs. Bagot's daughter, Mrs. Wodehouse, sent to Mr. Myers on February 9th, 1896, the following corroboration, stating that the

¹ In February, 1896, Mrs. Bagot wrote a second account of the same incident, which was printed in the *Journal S.P.R.* for April, 1896, with her daughter's confirmation, then obtained for the first time. The earlier account, here given, is practically identical with the later one, so that Mrs. Bagot's recollection of the circumstances does not seem to have varied.

quotations were an exact copy of the references to the dog in her diary for March 24th and 28th, 1883. It will be observed that there is no proof that the dog was seen on the day of its death, but it is clear that the death was not heard of till afterwards.

56, Chester Square, S. W.

(*Copy of Diary.*) *March 24th, 1883. Easter Eve* (Mentone).—"Drove with A. and picked anemones. Lovely bright day. But my head ached too much to enjoy it. Went to bed after tea and read Hettner's 'Renaissance.' Mamma saw Judy's ghost at table d'hôte!

March 28th, Wednesday (Monte Carlo).—"Mamma and A. came over for the day. Judy dead, poor old dear."

NOTE.—I distinctly remember my father and mother and sister (Mrs. Algernon Law) and my cousin (Miss Dawnay) coming into my bedroom all laughing and telling me how my mother had seen Judy (black and tan terrier) running across the room whilst they were at table d'hôte. My mother was so positive about it, that one of the others (I think my father) had asked the waiter if there were any dogs in the hotel, and he had answered in the negative. I can find no further mention of the time or day of the dog's death in my diary.

I may also be mistaken in the day on which my mother saw Judy, for although I usually write my diary every evening, I sometimes leave it for two or three days and then write it in as best I can remember. But I distinctly remember lying in my bed at Mentone when they told me the story, and equally clearly I remember receiving the news of Judy's death at Monte Carlo.

ADELA H. WODEHOUSE.

CASE 45. [L. 1000.]

The following case was obtained through Dr. Bramwell. The percipient, Mr. de Solla, is a gentleman well known in the musical world. He writes to Dr. Bramwell:—

February 5th, 1896.

DEAR DR. BRAMWELL,—As promised I now send an account of my little thought-transference experience. 'Twas thus. I sat opposite my eldest daughter, who was reading a book by the fireside. Presently I exclaimed, "Good gracious!" My daughter saying, "What is it?" I replied, "I could have sworn I saw a dog enter the room." I described the dog minutely. My daughter in great surprise told me that she had that moment read a description of just such a dog. I do not even now know the title of the book. We kept no dog at the time, nor had we conversed about one.—Faithfully yours,

ISIDORE DE SOLLA.

In reply to our enquiries, Mr. de Solla writes:—

5, Harrington-square, London, N. W., *March 8th, 1896.*

DEAR SIR,—In reply to yours of 4th inst., the incident *re* thought-transference 'twixt my daughter and myself took place on a Sunday about a year ago. My daughter would be willing to give an account of the matter. She does not remember my giving a detailed description of the dog, but simply that I exclaimed, "I just saw such a big dog rush into the room."

My daughter tells me that immediately before my exclamation she had read the following words from a book (*Lewis Arundel*):—"As he spoke, he uttered a low peculiar whistle; in obedience to his signal a magnificent Livonian wolf-hound, etc., etc., sprang into the room."

It is not a common experience of mine to imagine I see anything anywhere which is not tangibly present, and I am very sceptical about other folks' reports *re* such things.

ISIDORE DE SOLLA.

CASE 46. [L. 1100.]

This case, like the one just given, is an instance of close correspondence between two persons' unspoken thoughts.

Sir William Crookes writes on December 13th, 1897:—

"I enclose a letter from Mrs. Fisher (Arabella Buckley) which is of interest, not so much on account of the instance of telepathy, which is slight, and possibly a coincidence, but as coming from a lady well known for having written good books on science and knowing how to observe."

The account of Mrs. Fisher (Associate S.P.R.) is as follows:—

Elmcroft, Newton Abbot, S. Devon, *December 9th, 1897.*

DEAR SIR WILLIAM CROOKES,— . . . An instance of telepathy has just occurred, which I think, *on account* of its triviality, may be worth putting on record with names and date.

This evening my step-daughter (Mrs. Heriot) and I were sitting together in the drawing-room, no one else being present. She was working and I was reading to-day's *Times*. We had not spoken for some time. I had read through the correspondent's article on the "Engineering Dispute" and the leading article on the same subject.

As I finished, my eye fell on a small print paragraph telling of a gas explosion caused by looking for an escape with a light. Without reading it I exclaimed aloud "Oh! *What idiots some people are!*" Mrs. Heriot replied instantly "Yes, gas."

So far the bare fact. Much astonished I said, "How on earth did you know I meant gas?" "I do not know," she replied, "but as you said 'Oh!' I saw a man looking for a gas escape with a light."

My step-daughter and I have been close friends for more than thirty years, and have often noticed how we speak of the same things simultaneously. But nothing so striking as this has occurred before. . .

—Yours very sincerely,

ARABELLA B. FISHER.

I should perhaps say that Mrs. Heriot was sitting on the opposite side of the fire, that she had not read the paper, and had no clue as to what part was reading.

In reply to inquiries Mrs. Heriot writes:—

Elmcroft, Newton Abbot, S. Devon, *December 17th, 1897.*

DEAR SIR,— . . . Mrs. Fisher has asked me to write to you about what occurred the other evening. I was sitting on one side of the fireplace, working, she on the other, reading the *Times*. She looked up, saying, "Oh! what fools some people are!" As she spoke, it flashed through my mind,

as if she had read aloud, that some person had taken a light to look for an escape of gas, and thereby caused an explosion—so I at once answered “Yes, gas.”—I am, yours faithfully,

ROSE MACKAY HERIOT.

CASE 47.

The next—a case in which a dying mother had an apparently telepathic vision of an absent son who happened to be dying at the same time—is one of those cases which seem to indicate a heightening of certain faculties at the approach of death.

From Colonel C. F. Hicks.

46, Valplaisant, St. Heliers, Jersey, *December 23rd, 1889.*

Agreeably to my promise I now give you a statement of my late wife's last moments. Some days beforehand I was informed she would not last long; and it was in the evening about 5 or 6 o'clock p.m., on 3rd October, 1887, I went into her bedroom. There was the nurse, my second and third daughters in the room with me. The door was a little ajar. She was looking at it very earnestly when she said to my second daughter, Flo, “There is some one outside, let *him* in.” Flo answered and said, “Oh, no, mamma; there is no one—look,” and she opened the door wider. We then talked to her gently for some little time. After a pause she said, “Poor Eddie (my second son, who had gone out to Australia); oh, he is looking very ill—he has had a fall—broken his leg—poor Eddie.” When we all assured her such was not the case—that the last news we had heard from him was that he was quite well—she became more pacified, although restless and doubtful, as she continued to say now and then, “Poor Eddie!” She died at about 20 minutes to 2 a.m., early on the 4th October. We little thought that her words would be verified, with the exception of the broken leg.

Some time afterwards I received a letter from Mr. Thomas Williams announcing my poor son's death. For he left a place called Wyndham on the Cambridge Gulf, N.W. of Australia, on the 4th [evidently meaning 3rd. see below] October, 1887, with a young man of the name of Russell. He suddenly felt ill and called for some water. The latter went off to a spring to get it, but coming back he found that he had fallen from his horse and was lying quite dead. So his poor mother's vision turned out to be quite true, excepting his leg being broken.

Now, the only question is about the time. Did the son die before the mother or after the mother? as, taking the longitude of Wyndham N.W. of Australia, so far to the east of us, there must be a good eight or ten hours difference, and a ship going round the world making east all the way would gain a day, and by westing would lose one.

I give you a few extracts from letters I have received. The one from Mr. Thomas Williams, with whom my son left a letter to be sent on to me. Mr. T. W.'s letter is dated the 5th October, 1887: “Your son left Wyndham to go to Durack station on the 3rd October, in company with Louis Smith and John Russell. They had to go over a very rough country, and your poor boy succumbed to the pangs of thirst, suffering at the same time with fever. I am glad to inform you that his sufferings were short, and that the great God was pleased to take him away quickly. He spoke very affectionately of his

mother, and what he would do if he could only get back to Jersey, for he was heartsick when he was here."

I give you another extract from his employer, a Mr. Durack, a gentleman who dealt largely in horses, and had a great number of horse stations in Australia :

"When I left your son at Wyndham on 27th September last, 1887, he was to start back to the station, as he had a horse, bridle, and saddle to ride."

In conclusion, I have now given you as succinctly as I can the death of the mother and son, the one having taken place here and the other at our Antipodes, both on the same day and date, and as far as I know about the same time. It is more than a coincidence—it is very mysterious.

(Signed) C. F. HICKS.

Colonel Hicks writes later :—

February 22nd, 1890.

. . . The witnesses in my wife's late case are none of them present here. My second daughter, whom I was expecting from Bombay when I received your letter, has arrived here. Her statement I enclose. My third daughter, another witness, is at present at Brisbane, in Australia. It will take some time before I can receive an answer from her. As to the nurse who was attending my late wife, she is residing in Dublin . . . I must write to her, as her address is Miss E. Fenn, 16, Adelaide-road, Dublin, to give her statement of the case.

Discrepancy in dates : my late wife died at about 40 minutes to 2 a.m. on 4th October, 1887—that is taking the time from 12 a.m. on the 3rd to 12 p.m., after which it becomes the 4th. So all the conversation that took place with the above-named witnesses, viz., the nurse, Miss E. Fenn, two daughters and self, took place in the evening of the 3rd, about 5 or 6 p.m., as she died the same night, or more correctly speaking, being after 12 p.m., it was early in the morning of the 4th.

Now for my son's death. Mr. Thomas Williams' letter is dated the 5th October, 1887. He says my son left Wyndham, on the Gulf of Cambridge, on the 4th [the date given in Mr. Williams' letter is 3rd, see below] October, 1887, but he does not mention at what time. But being within the tropics, where people generally travel as early as they can to escape the heat of the sun, it is presumed that he and his friend, Mr. Russell, must have started early, and it is certain that they could not have gone far before he met his end, and most probably Mr. T. Williams must have heard of it the same day, as his letter is dated the 5th October, 1887. . . .

(Signed) C. F. HICKS.

The following letter from Miss Hicks was enclosed :—

February 27th, 1890.

I was in my late mother's bedroom between the hours of 5 and 6 in the evening, on the 3rd of October, 1887, when she asked me to open the door, as some one was outside and wanted to come in. I answered and said, "Oh, mother, the door is open, and there is no one outside," and then I opened the door wider. Then I shut the door. She then said, "Poor Eddie, he looks very ill ; he has had a fall." I said to her, "Oh, mother, how you go on ; he is all right the last time we heard." She said, "Oh, he is looking

very ill." The next morning, at about 40 minutes to 2 a.m., she died. I heard from letters received that my poor brother Eddie died in Australia on the same day and about the same time.

F. HICKS.

Colonel Hicks also sent us the letter from Mr. Williams, giving an account of his son's death. The exact time is, as Colonel Hicks says, not stated, but the letter is dated October 5th, 1887, and states that Mr. E. Hicks started on his journey on October 3rd. It seems probable that the death took place on the same day.

The following case affords perhaps stronger evidence for telepathy than any others in this section, on account of the collective nature of the experience.

CASE 48. [L. 953.]

Mr. Myers writes:—

The following case was sent to me by Sir Lawrence Jones, Bart., a member of the S.P.R. In conversation he described to me the singular and persistent distress accompanying the nocturnal alarm.—F. W. H. M.

Cranmer Hall, Fakenham, Norfolk, *April 26th, 1893.*

On August 20th, 1884, I was staying at my father-in-law's house at Bury St. Edmunds. I had left my father in perfectly good health about a fortnight before. He was at home at this address. About August 18th I had had a letter from my mother saying that my father was not quite well, and that the doctor had seen him and made very light of the matter, attributing his indisposition to the extreme heat of the weather.

I was not in any way anxious on my father's account, as he was rather subject to slight bilious attacks.

I should add, though, that I had been spending that day, August 20th, at Cambridge, and should have stayed the night there had not a sort of vague presentiment haunted me that possibly there would be a letter from home the next morning. My wife, too, had a similar strong feeling that if I stayed the night at Cambridge I might regret it. In consequence of this feeling I returned to Bury, and that night woke up suddenly to find myself streaming with perspiration and calling out: "Something dreadful is happening; I don't know what." The impression of horror remained some time, but at last I fell asleep till the morning.

My father, Sir Willoughby Jones, died very suddenly of heart disease about 1 a.m. on August 21st. He was not in his room at the moment, but was carried back to his room and restoratives applied, but in vain.

My brother Herbert and I were the only two of the family absent from home at the time. The thoughts of those present (my mother, brother, and three sisters) no doubt turned most anxiously towards us, and it is to a telepathic impression from them in their anxiety and sorrow that I attribute the intimations we received.

LAWRENCE J. JONES.

Lady Jones writes:—

I have a vivid remembrance of the occurrence related above by my husband. I was sound asleep when he awoke, and seizing me by [the]

wrist exclaimed : "Such a dreadful thing is happening," and I had much difficulty in persuading him that there was nothing wrong.

He went to sleep again, but was much relieved in the morning by finding a long letter from Sir Willoughby, posted the day before, and written in good spirits. Having read this and gone to his dressing-room, however, he soon returned with the telegram summoning him home at once, and said as he came in : "My impression in the night was only too true."

EVELYN M. JONES.

Mr. Herbert Jones, the other percipient, describes his experience as follows :—

Knebworth Rectory, Stevenage, *April 4th, 1893.*

Recollections of August 20th, 1884.

I had spent the day at Harpenden, and returned home about 8 p.m., and went to bed about 10.30.

I woke at 12 o'clock, hearing my name called twice, as I fancied. I lit my candle, and, seeing nothing, concluded it was a dream—looked at my watch, and went to sleep again.

I woke again and heard people carrying something downstairs from the upper storey, just outside my room. I lit my candle, got out of bed, and waited till the men were outside my door. They seemed to be carrying something heavy, and came down step by step.

I opened my door, and it was pitch dark. I was puzzled and dumb-founded. I went to my sitting-room and into the hall, but everything was dark and quiet. I went back to bed convinced I had been the sport of another nightmare. It was about 2 a.m. by my watch. At breakfast next morning on my plate was a telegram telling me to come home.

This whole story may be nothing, but it was odd that I should have twice got up in one night, and that during that night and those hours my father was dying.

H. E. JONES.

Sir Lawrence Jones adds :—

My brother was then a curate in London, living at 32, Palace Street, Westminster, where the above experience took place.—L. J. J.

CONCLUSION.

In conclusion, some justification may appear necessary for discussing at so great a length and with so much detail the possible scope of accidental coincidences in various departments of *Psychical Research*.

Any empirical treatment of a subject partakes to some extent of the nature of statistics, and to persons who are not actively engaged in collecting and studying them, statistics generally appear not only intolerably tedious, but essentially unreliable. Statistics—it is commonly said—can be made to prove anything. And it is quite true

that they *can* be made to prove anything, so long as arguments are drawn—as is too often done—from mere collections of percentages, while the possible agency of Chance in producing many of the percentages is ignored.

The general necessity of allowing for Chance in the ordinary empirical sciences, indeed, has always been recognised. If a bacteriologist is trying to discover what will destroy certain germs, he is aware that the fact of their dying the first time he introduces a certain substance into their environment does not prove that that substance caused their death; their death at that time may have been an accidental coincidence. This does not discourage him from further search, as he knows that something caused the death and it is his business to find out what the cause was. But he will never be able to prove what it was if he ignores the possible agency of Chance.

Similarly, we cannot substantiate the claims of telepathy or any other supposed supernormal faculty by ignoring what Chance may bring about. At the same time, there is no necessity for concluding that if supernormal faculties are not at work in *all* incidents that suggest them, they do not exist at all.

But in dealing with the most difficult and obscure of all sciences—that of Psychical Research—the necessary process of eliminating one possible agency after another must be even more lengthy and tedious than in any other subject. The present article is nothing more than an attempt to clear some of the ground in a preliminary fashion, and is intended to suggest problems rather than to solve them,—their solution being, as it seems to me, impossible for the present.

Some points with regard to the construction and characteristics of artificial Chance Series are put into Appendix I., so as to avoid too much interruption of the text; while the later Appendices contain detailed discussions of individual cases, too lengthy to be included in the article itself.

APPENDIX I.

ON THE CONSTRUCTION OF ARTIFICIAL CHANCE SERIES.

[Written jointly with W. E. JOHNSON.]

This section contains a further discussion of the method of analysis of Chance Series of events into "cycles," of which an example was given in Chapter II., Table II., p. 189. The series there analysed consisted of 4096 ($= 2^{12}$) throws of a penny, which were divided into consecutive groups of events, to which the name "cycles" was given, each group beginning with a head (which had been preceded by a tail) and ending with a tail (which was followed by a head). Henceforth throughout this section, we write "head" and "tail" respectively as H and T for brevity. Thus, 17 consecutive throws, HHHTT, HT, HHT, HTTT, HHT, would be divided as shown by the commas into five groups or "cycles." The series has to be regarded as a *circular* one, that is, one in which the end is continuous with the beginning. So that if the series actually ended with one or more H's, these would be brought round to the beginning to form the first part of the first cycle; and if it actually began with one or more T's, they would be carried round to the end to form the last part of the last cycle.

The theoretic number of every different cycle, etc., included in the Table was given in each department in italic figures, and it must now be explained how these theoretic numbers, which for the future we will speak of as "*norm numbers*," were obtained.

Each cycle involves by definition the occurrence of a group of events two more in number than itself, viz., a preceding T (belonging to the end of the previous cycle) and a following H (belonging to the beginning of the next cycle) *e.g.*, the cycle HT involves the occurrence of the group of events THTH, and so on. Therefore, since there are two alternative events, the probability of any cycle of events r in number is $\frac{1}{2^{r+2}}$ (that is, the product of the probabilities of all the single events); and the norm number of such cycles in the series of 4096 ($= 2^{12}$) events is $\frac{2^{12}}{2^{r+2}}$. For instance, the norm number of cycles of HT in that series is $\frac{2^{12}}{2^4} = 256$.

The same method gives the norm numbers of the different runs of each alternative. Thus, all the single H's in the top line of the Table involve the group THT; all the runs of two H's involve the group

THHT, and so on; the probability of the former is $\frac{1}{2^3}$ and of the latter $\frac{1}{2^4}$; and the norm numbers of them in the series are respectively $\frac{4096}{8}$ and $\frac{4096}{16}$. Similarly with the different runs of T's, shown in the bottom line of the Table.

Again, each cycle—whatever its length may be—involves a single occurrence of the group TH at its beginning;—the occurrence of this serving, as it were, to inaugurate each cycle. For instance, taking seven throws as follows:—

. T) H₃ T₂ (H

one complete cycle is included in the middle of this,—between the two brackets,—preceded by a T, which belongs to the previous cycle, and followed by an H, which belongs to the next cycle. The remainders of these two cycles are indicated by the dots. The seven throws include two occurrences of TH, but it is clear that there is only one such occurrence for each cycle; so that the actual number of cycles must always be the same as the actual number of times TH occurs.

The norm number of occurrences of TH in our series is $\frac{2^{12}}{2^2} = 1024$.

This must therefore be the norm number of cycles in the same series.

Residual Cycles.

It was shown in Chapter II., p. 191, that if we made up the Table out of all the cycles the norm number of which is one or more, we get only 1013 cycles, instead of the theoretic total of 1024. The 11 remaining cycles were called “residual cycles.” In every series analysed in this way, some residual cycles are found, the number of which increases absolutely, but decreases relatively, with the length of the series; this number being $(n-1)$ in a series of length 2^n . It was remarked in Chapter II. that their exact position in the Table had to be settled in an arbitrary manner, but that there were definite and assignable limits within which they must lie. We now proceed to explain how these limits are ascertained.

Norm Series.

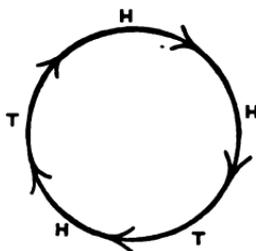
To find the limits of the norm distribution of the residual cycles in any series, we have recourse to a second method of analysis of the series.

The analysis into cycles is a division into mutually exclusive sets or “sequences” of events; every event in the series is included in one sequence, and in one sequence only. These may, therefore, be called “closed sequences.”

Another method is to divide the series into *overlapping sequences*, of any length we choose to adopt,—so that every event belongs to several sequences. These may be called “*unclosed sequences*” to distinguish them from the cycles, or “*closed sequences*.”

Sequences are successions of events, arranged in a defined order ; e.g., HHT, HTH, and THH are various sequences of three events, or what may be called sequences of length 3. A series of events of length m may always be regarded as containing m sequences of length r , where r is any number not greater than m .

A complete analysis of a brief series on this plan is perhaps required for clearness, and we will take as an example 5 throws of a penny, HHTHT. This must—like the long series already discussed—be conceived of as a *circular series* ; i.e., we may suppose it to be repeated, so that its initial terms would follow after its final terms. It could be represented in a circular diagram thus :



where the final T is followed by HH, etc., as if the actual series were repeated.

Now, this circular series of length 5 contains (a) 5 sequences of length 5, viz., HHTHT, HTHTH, THTHH, HTHHT, THHTH ; (b) 5 sequences of length 4, viz., HHTH, HTHT, THTH, HTHH, THHT ; (c) 5 sequences of length 3, viz., HHT, HTH, THT, HTH, THH, (d) 5 sequences of length 2, viz., HH, HT, TH, HT, TH ; (e) 5 sequences of length 1, viz., H, H, T, H, T. In all these cases the several sequences *begin* at the several successive throws in the series. Hence the *number* of sequences is equal to the number of throws.

It will, of course, be noticed that all the m sequences in a series of length m are not necessarily *different in formation* from one another. We count them as m by distinguishing, not their kind or formation, but their *position* in the series.

We may define an ideal or Norm Chance Series as follows :—

Definition : If t be the number of alternative events in a series, a *Norm Series* of length t^n is a series which contains *all* the possible different sequences of length n which can be made with t alternatives.

It has just been shown above that a series of t^n events contains t^n sequences of any length that we may choose (up to t^n). Hence the series of length t^n contains, in particular, t^n sequences of length n . But with t alternatives we can (by the rules of Permutations) construct t^n *differently constituted* sequences of length n . Hence, according to the definition of the Norm Series, each of these differently constituted sequences occurs once, and *once only*, in it. The occurrence of each *once* corresponds to its probability, which is $\frac{1}{t^n}$; that is, in any series, t^n in length, the norm number for each sequence of length n is *one*.

Definitions. In a series of length t^n any sequence of length n may be called a *unit sequence*; sequences of lengths smaller or greater than n may be called *minor* or *major* sequences respectively.

It will be obvious that in a Norm Series of length t^n , every "minor sequence" (as well as every "unit sequence") will occur as often as its probability requires. For the probability of a specific sequence of length r is $\frac{1}{t^r}$; it ought, therefore, to occur in t^n events $t^n \div \frac{1}{t^r} = t^{n-r}$ times. But t^{n-r} is the number of ways in which a given sequence of length r could be extended into a sequence of length n . Hence the proposition follows.

But in the Norm Series of length t^n the "major sequences" cannot occur according to their probability number. For the probability number of each would be fractional. Thus the probability of a given sequence of length $n + 1$ is $\frac{1}{t^{n+1}}$; hence, in t^n events, this should occur $t^n \div \frac{1}{t^{n+1}} = \frac{1}{t}$ times, which is impossible.

Hence *no finite series can be regarded as norm with reference to its "major sequences."* It will however, of course, contain major sequences, since any sequence of length greater than n is a major sequence. These have, therefore, to be introduced arbitrarily.

To sum up, the conditions of the Norm Series may be stated in the form of the rule that in a series of length t^n , all the "unit sequences" of length n must occur each once, so that all the "minor sequences" of length r will occur each t^{n-r} times.

For instance, out of three alternative events, a , b , and c , 3 sequences of length 1, 3^2 sequences of length 2, and 3^3 sequences of length 3, can be made. So that a Norm Series of length 3^2 would contain three of each sequence of 1 and one of each sequence of 2; and a Norm Series of length 3^3 would contain nine of each sequence of 1, three of each

sequence of 2, and one of each sequence of 3. The following are such Norm Series :—

(1) Series of length 3^2

$a a b a c b b c c.$

(2) Series of length 3^3

$a a a c a c b c c a b a c c c b b c b a a b b b a b c.$

The following are examples of Norm Series with two alternative events, viz., throws of a penny, H and T.

(1) Series of length 2^1 : $H_1 T_1.$

(2) Series of length 2^2 : $H_2 T_2.$

(3) Series of length 2^3 : $H_3 T_3 H_1 T_1.$

(4) Series of length 2^4 : $H_1 T_2 H_2 T_1 H_4 T_4 H_1 T_1.$

(5) Series of length 2^5 : $H_3 T_1 H_1 T_1 H_2 T_1 H_5 T_5 H_1 T_3 H_2 T_2$
 $H_1 T_1 H_1 T_2.$

(6) Series of length 2^6 : $H_1 T_3 H_1 T_1 H_3 T_1 H_2 T_3 H_3 T_2 H_1 T_1 H_1$
 $T_4 H_2 T_2 H_2 T_1 H_1 T_1 H_1 T_1 H_2 T_1 H_6 T_6 H_1 T_2 H_4 T_1 H_1 T_2.$

(7) Series of length 2^7 : $H_4 T_2 H_5 T_1 H_4 T_1 H_1 T_1 H_3 T_2 H_1 T_1$
 $H_2 T_3 H_1 T_1 H_1 T_3 H_2 T_1 H_1 T_2 H_3 T_1 H_2 T_1 H_2 T_2 H_2 T_2$
 $H_1 T_2 H_1 T_1 H_1 T_1 H_1 T_1 H_2 T_1 H_1 T_1 H_1 T_2 H_1 T_3 H_1 T_2$
 $H_2 T_1 H_3 T_1 H_1 T_4 H_1 T_1 H_7 T_7 H_1 T_5 H_2 T_4 H_3 T_3.$

(8) Series of length 2^8 : $H_1 T_2 H_4 T_1 H_2 T_1 H_3 T_2 H_2 T_1 H_2 T_3$
 $H_2 T_2 H_1 T_3 H_1 T_2 H_2 T_2 H_3 T_1 H_2 T_2 H_1 T_1 H_1 T_1 H_2 T_1$
 $H_2 T_1 H_1 T_1 H_1 T_1 H_1 T_3 H_1 T_1 H_1 T_1 H_1 T_1 H_3 T_1 H_3 T_1$
 $H_1 T_1 H_1 T_2 H_1 T_4 H_1 T_2 H_1 T_2 H_3 T_2 H_1 T_2 H_1 T_1 H_2 T_1$
 $H_1 T_2 H_1 T_1 H_1 T_2 H_2 T_1 H_1 T_1 H_2 T_2 H_2 T_3 H_1 T_3 H_2 T_1$
 $H_4 T_1 H_1 T_5 H_1 T_1 H_8 T_8 H_1 T_6 H_2 T_5 H_3 T_4 H_4 T_3 H_5 T_2$
 $H_6 T_1 H_5 T_1 H_1 T_1 H_4 T_2 H_1 T_1 H_3 T_3 H_1 T_1 H_2 T_4 H_1 T_1$
 $H_1 T_4 H_2 T_1 H_1 T_3 H_3 T_1.$

The reader will find that, as the length of the series is increased, it becomes increasingly difficult to solve the problem of constructing the Norm Series, although, of course, the mere *number* of cycles of each order is a matter of simple arithmetical calculation.

Residual Cycles in the Norm Series.

The constitution of the Norm Series shows us the limits within which the residual cycles must lie. Table III. (A) and (B) gives an ideal series of 1024 ($=2^{10}$) throws of a penny, analysed twice over, into cycles beginning with heads and tails, called respectively Head-Cycles and Tail-Cycles, to illustrate this. From the formula given

TABLE III. (A)

	T ₁	T ₂	T ₃	T ₄	T ₅	T ₆	T ₇	T ₈	T ₉	T ₁₀	Total Cycles.	Total Throws.
H ₁	64	32	16	8	4	2	1	1	—	—	128	383
H ₂	32	16	8	4	2	1	1	—	—	—	64	255
H ₃	16	8	4	2	1	1	—	—	—	—	32	159
H ₄	8	4	2	1	1	—	—	—	—	—	16	95
H ₅	4	2	1	1	—	—	—	—	—	—	8	55
H ₆	2	1	1	—	—	—	—	—	—	—	4	31
H ₇	1	1	—	—	—	—	—	—	—	—	2	17
H ₈	1	—	—	—	—	—	—	—	—	—	1	9
H ₉	—	—	—	—	—	—	—	—	—	—	—	—
H ₁₀	—	—	—	—	—	—	—	—	—	1	1	20
	128	64	32	16	8	4	2	1	—	1	256	1024

SERIES OF 2¹⁰ EVENTS.—HEAD-CYCLES.
 Figures in thick type show residual cycles.

TABLE III. (B)

	H ₁	H ₂	H ₃	H ₄	H ₅	H ₆	H ₇	H ₈	H ₉	H ₁₀	Total Cycles.	Total Throws.
T ₁	64	32	16	8	4	2	1	—	—	1	128	385
T ₂	32	16	8	4	2	1	—	1	—	—	64	256
T ₃	16	8	4	2	1	—	1	—	—	—	32	160
T ₄	8	4	2	1	—	1	—	—	—	—	16	96
T ₅	4	2	1	—	1	—	—	—	—	—	8	56
T ₆	2	1	—	1	—	—	—	—	—	—	4	32
T ₇	1	—	1	—	—	—	—	—	—	—	2	18
T ₈	—	1	—	—	—	—	—	—	—	—	1	10
T ₉	—	—	—	—	—	—	—	—	—	—	—	—
T ₁₀	1	—	—	—	—	—	—	—	—	—	1	11
	128	64	32	16	8	4	2	1	—	1	256	1024

SERIES OF 2¹⁰ EVENTS.—TAIL-CYCLES.
 Figures in thick type show residual cycles.

above (p. 293) of the probabilities of cycles, the probability of each cycle of length 8 is $\frac{1}{2^{10}}$. There will, therefore, be one of each of the cycles of length 8, which are seen along what may be called the "diagonal" from H_7 to T_7 . But these are the longest cycles whose norm number amounts to one in this series, and their norm numbers, together with those of all the shorter cycles, amount only to 247, whereas the total number of cycles should be $\frac{2^{10}}{2^2} = 256$. There are, therefore, 9 residual cycles to distribute.

Since there are 2^{10} different sequences of length 10, each of these must occur once, and once only, in a Norm Series of length 2^{10} . Similarly, since there are 2^9 different sequences of length 9, each of them should occur twice; and since there are 2^8 different sequences of length 8, each of them should occur four times, and so on. We cannot here enter into a complete explanation of how all these different sequences occur in the series which is summarised in Table III.; but it is obvious that certain sequences of 8, 9, and 10, which are absent from the part of the Table up to and including the "diagonal" from H_7 to T_7 ,—viz., $H_8, T_8, H_9, T_9, H_{10}, T_{10}$,—have to be introduced. This can be done by putting one cycle into each square along the next "diagonal" (from H_8 to T_8) and adding a single cycle, $H_{10} T_{10}$ (see figures in thick type in Table III. A). The cycle $H_{10} T_{10}$ contains (a) one each of the sequences H_{10}, T_{10} ;¹ (b) two each of the sequences H_9, T_9 (since the sequences, by definition, overlap one another); and (c) three each of the sequences H_8, T_8 . We require, then, one more of the sequences H_8 and T_8 , which are to be found respectively in the cycles $H_8 T, H T_8$. Similarly, there are 2^7 different sequences of length 7, which should, therefore, occur eight times each in the series; hence H_7 and T_7 should occur eight times each. They occur four times each in the cycle $H_{10} T_{10}$, and twice each in the cycles $H_8 T, H T_8$. Two more occurrences each are required and are to be found in the cycles $H_7 T, H_7 T_2, H T_7, H_2 T_7$. Similarly for all the shorter sequences.

It must be observed that the distribution shown in Table III. is not the only possible arrangement satisfying the conditions of a Norm Series of this length. Any arrangement will do by which, after first distributing all the cycles whose norm number amounts to one or more, *one cycle, and one only, is added to each line and to each column of the Table, with the exception of the line beginning H_9 and the column*

¹ It also contains one of all the sequences of length 10 which can be made out of combinations of single runs of both alternatives, viz., $H_9 T, H_8 T_2, H_7 T_3, H_6 T_4, H_5 T_5, H_4 T_6, H_3 T_7, H_2 T_8, H T_9$.

headed T_9 . (This exception has to be made, because, as already explained, the necessary introduction of the sequences H_{10} and T_{10} involves the introduction of all the required sequences of H_9 and T_9 .) It is not difficult for the reader to assure himself that this is the only distribution which would ensure an equal number of H's and T's in the whole series. As soon as the position of the cycle in each line is fixed, it will be found that there are two alternative positions for the cycle in the line below, until the last line is reached, when only one position is possible. This gives in general 2^{n-2} varieties of the Norm Series 2^n in distributing the $(n-1)$ residual cycles which occur. There are two symmetrical ways of filling up the Table according to this rule, which are shown in Table III. (A) and (B).

All these results can be immediately generalised.

Series with more than two Alternative Events.

A series with any number of alternative events may be analysed into cycles on the same method as that used when there are only two alternatives, by taking one of the alternatives as the invariable first term of the cycle and beginning a new cycle whenever this alternative occurs *after* any one of the other alternatives. Thus, supposing there are three alternatives, a, b, c , and we choose to begin the cycles with a ; the series $a c, a a b b, a b b c, a c c c b b, a c c, a b c$, would be divided into the cycles shown by the commas. These cycles, like those in a series with two alternatives (or, for that matter, with any number of alternatives) may consist of two events, or of any number of events more than two. But the cycles composed of one event following the a or a 's are of two different kinds, as this event may be b or c ; those composed of two events following the a 's are of four different kinds, as the events may be $b_2, b c, c_2$, or $c b$; those composed of three events following the a 's are of eight different kinds, and so on.

The probability of each cycle depends, of course, on the number of events in it, being the same for all cycles of the same length, no matter what the events are. As before, any given cycle involves the occurrence of two more events than are comprised in itself, viz., a b or a c preceding it, and an a following it. Thus the cycle $a b$ implies either $b a b a$ or $c a b a$. The probability of the cycle $a b$ is therefore the sum of the probabilities of the sequences $b a b a$ and $c a b a$. Since there are three alternative events, the probability of each of these sequences of four events is $\frac{1}{3^4}$; therefore the probability of the cycle $a b$ is $\frac{2}{3^4}$. The probability

of the cycle ac is of course the same.¹ Similarly the cycle abc implies either $babca$ or $cabca$, the probability of each of which is $\frac{1}{3^3}$; therefore the probability of the cycle abc (or any other cycle of three events) is $\frac{2}{3^3}$. It is obvious that the same method of reasoning applies to the cycles of all the different lengths; so that the probability of a cycle of r events (in a series with three alternatives) is $\frac{2}{3^{r+2}}$. In a series with t alternatives, the probability of a specific cycle of length r is, then, $\frac{t-1}{t^{r+2}}$.

Again, every cycle, whatever its length may be, involves at its beginning the occurrence of either ba or ca , the probability of each of which is $\frac{1}{3^2}$, so that the norm number of cycles in a series of x events is $\frac{2}{3^2}$ of x .

TABLE IV.

	b	c	b_2	bc	c_2	cb	b_3	b_2c	bc_2	$cbcb$	c_3	c_2b	cb_2	cbc	Total Cycles.	Total Events
a	18	18	6	6	6	6	2	2	2	2	2	2	2	2	76	208
a_2	6	6	2	2	2	2	—	—	—	—	—	—	—	—	20	68
a_3	2	2	—	—	—	—	—	—	—	—	—	—	—	—	4	16
															100	292

SERIES OF 729 ($=3^6$) EVENTS.

The Table shows only the cycles whose norm number amounts to one or more, omitting the "residual cycles" and corresponding "residual events."

If we take a series of a definite number of events (choosing some power of 3 as the number, for convenience of calculation) and summarise the cycles in a Table (see Table IV.), we find as before that there are some "residual cycles," (not shown in this Table) the number of which always increases absolutely, but decreases relatively, with the length of the series. The formula for this number is given below (see p. 307). These residual cycles can be distributed according to the rules of the Norm Series, the conditions to be fulfilled being that the sequences of the various lengths must occur in the series according to

¹ These being the shortest possible cycles, it follows that 3^1 is the shortest series in which the norm number of any cycle will amount to one.

TABLE

	b	c	b_2	bc	c_2	cb	b_2c	bc_2	$cbcb$	c_2b	cb_2	$cbcb$	b_4	bc_3	c_4	c_2b_2	cb_3	$bcbcb$	$cbcb$
a	6	6	2	2	2	2	1	1	1	1	1	1	1	—	—	1	—	1	1
a_2	2	2	—	—	—	—	—	—	—	—	—	—	—	1	1	—	1	—	1
a_3	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
a_4	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
a_5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

SERIES OF 243

Showing a possible distribution of the

their respective probabilities. In this calculation we must remember that, as before, a certain number of the shorter sequences are included in the longer ones. Thus, there are :

3 different sequences of length 1,

3^2 different sequences of length 2,

3^3 different sequences of length 3,

3^4 different sequences of length 4,

3^5 different sequences of length 5,

and so on. Therefore in a Norm Series of 3^5 ($= 243$) events, there should be

1 of each "unit sequence" of 5

3 of each "minor sequence" of 4 (viz., 1 in addition to those in the longer sequences)

9 " " " 3 (" 4 " ")

27 " " " 2 (" 12 " ")

81 " " " 1 (" 36 " ")

e.g., a_1 must occur 36 times in the series, a_2 12 times, a_3 4 times, and so on. This gives the total number of cycles in each line of a complete Table.

In Table V. is given a solution of the series, fulfilling these conditions. It will be observed that the total number of cycles (see column headed "Total Cycles") correspond to the sequences (or runs) of a of the different lengths, and inspection of the Table will show that the different runs of b and c also occur in the required proportions. The Table has to be extended as far as a_5 , b_5 , and c_5 , in order to include all the different sequences of 5, so that the longest cycle that

V.

b_5	b_3c_2	b_2c_3	b_2c_2b	b_2cb_2	b_3cb	bc_3b	bc_bcb	c_5	c_2bc_2	c_2b_2c	cb_3c	cbc_3	$cbcb_c$	Total Cycles	Total Events
	—	1	—	1	—	—	1	—	—	—	1	1	1	36	128
	1	—	1	—	—	—	—	—	1	1	—	—	—	12	64
	—	—	1	—	1	1	—	—	—	1	—	—	—	4	32
	—	—	—	—	—	—	—	—	—	—	—	—	—	1	9
	—	—	—	—	—	—	—	1	—	—	—	—	—	1	10
														54	243

(= 3^5) EVENTS

“residual cycles” (figures in thick type).

might have to be included would be $a_5 b_5 c_5$. For simplicity all the columns containing no cycles are omitted.

General Solutions.

The conditions which have to be fulfilled in the case of three alternative events may be expressed in a more general form (see Table VI., A and B). These Tables give a general solution of a series of 729 (= 3^6) events, the numbers and formulæ given being applicable to all the possible different arrangements of the series. They show in the case of every cycle, etc. : (1) the sequence of events involved ; (2) the calculation used in consequence, and (3) the resultant number.

Taking R to represent any of the alternatives except a (that is, in this case, either b or c) all the cycles involve at their beginning the occurrence of the sequence Ra ; there must, therefore, be as many cycles as there are occurrences of Ra . The probability of R is $\frac{2}{3}$, and of a $\frac{1}{3}$, so that the probability of Ra is $\frac{2}{3^2}$. Therefore, the total number of cycles will be $\frac{2}{3^2}$ of the total number of events. All the cycles in the first line of the Table (see “Total Cycles” in Table VI., A) involve the sequence RaR , the probability of which is $\frac{2^2}{3^3}$. Similarly, all the cycles in the second line involve Ra_2R , the probability of which is $\frac{2^2}{3^4}$, and so on. Again, all the cycles in the first column (see “Totals”

TABLE VI. A.

	R_1	R_2	R_3	R_4	R_5	$R_6 \dots$	Total Cycles
a_1	$Ra Ra$ $\frac{2^2}{3^4}$ of 3^6 = 36	$Ra R_2 a$ $\frac{2^3}{3^5}$ of 3^6 = 24	$Ra R_3 a$ $\frac{2^4}{3^6}$ of 3^6 = 16	$Ra R_4 \dots$ $\frac{2^5}{3^6}$ of 3^6 = 32			$Ra R$ $\frac{2^2}{3^3}$ of 3^6 = 108
a_2	$Ra_2 Ra$ $\frac{2^2}{3^5}$ of 3^6 = 12	$Ra_2 R_2 a$ $\frac{2^3}{3^6}$ of 3^6 = 8	$Ra_2 R_3 \dots$ $\frac{2^4}{3^6}$ of 3^6 = 16				$Ra_2 R$ $\frac{2^2}{3^4}$ of 3^6 = 36
a_3	$Ra_3 Ra$ $\frac{2^2}{3^6}$ of 3^6 = 4	$Ra_3 R_2 \dots$ $\frac{2^3}{3^6}$ of 3^6 = 8					$Ra_3 R$ $\frac{2^2}{3^5}$ of 3^6 = 12
a_4	$Ra_4 R_1 \dots$ $\frac{2^2}{3^6}$ of 3^6 = 4						$Ra_4 R$ $\frac{2^2}{3^6}$ of 3^6 = 4
a_5	$Ra_5 \dots$ $\frac{2}{3^6}$ of 3^6 = 2						$Ra_5 \dots$ $\frac{2}{3^6}$ of 3^6 = 2
a_6							
	Ra $\frac{2}{3^2}$ of 3^6 = 162						

SERIES OF 3^6 EVENTS

Showing limits of distribution of residual cycles (figures in thick type) in lines.

TABLE VI. B.

	R_1	R_2	R_3	R_4	R_5	$R_6 \dots$	Total Cycles	
a_1	$Ra Ra$ $\frac{2^2}{3^4}$ of 3^6 = 36	$Ra R_2 a$ $\frac{2^3}{3^5}$ of 3^6 = 24	$Ra R_3 a$ $\frac{2^4}{3^6}$ of 3^6 = 16	$\left. \begin{array}{l} \dots a R_4 a \\ \frac{2^4}{3^6} \text{ of } 3^6 \\ \frac{2^5}{3^6} \text{ of } 3^6 \\ \dots a R_5 \dots \\ \frac{2^5}{3^6} \text{ of } 3^6 \\ \dots a R_6 \dots \end{array} \right\} = 32$	$\left. \begin{array}{l} \dots a R_6 \dots \\ \frac{2^6}{3^6} \text{ of } 3^6 \\ \dots a R_7 \dots \end{array} \right\} = 32$			
a_2	$Ra_2 Ra$ $\frac{2^2}{3^5}$ of 3^6 = 12	$Ra_2 R_2 a$ $\frac{2^3}{3^6}$ of 3^6 = 8	$\left. \begin{array}{l} \dots a_2 R_3 a \\ \frac{2^3}{3^6} \text{ of } 3^6 \\ \frac{2^4}{3^6} \text{ of } 3^6 \\ \dots a_3 R_4 a \\ \frac{2^4}{3^6} \text{ of } 3^6 \\ \dots a_4 R_5 a \\ \frac{2^5}{3^6} \text{ of } 3^6 \\ \dots a_5 R_6 a \\ \frac{2^6}{3^6} \text{ of } 3^6 \\ \dots a_6 R_7 a \\ \frac{2^7}{3^6} \text{ of } 3^6 \end{array} \right\} = 8$					
a_3	$Ra_3 Ra$ $\frac{2^2}{3^6}$ of 3^6 = 4	$\left. \begin{array}{l} \dots a_3 R_4 a \\ \frac{2^4}{3^6} \text{ of } 3^6 \\ \dots a_4 R_5 a \\ \frac{2^5}{3^6} \text{ of } 3^6 \\ \dots a_5 R_6 a \\ \frac{2^6}{3^6} \text{ of } 3^6 \\ \dots a_6 R_7 a \\ \frac{2^7}{3^6} \text{ of } 3^6 \end{array} \right\} = 4$						
a_4	$\left. \begin{array}{l} \dots a_4 R_5 a \\ \frac{2^5}{3^6} \text{ of } 3^6 \\ \dots a_5 R_6 a \\ \frac{2^6}{3^6} \text{ of } 3^6 \\ \dots a_6 R_7 a \\ \frac{2^7}{3^6} \text{ of } 3^6 \end{array} \right\} = 2$							
a_5								
a_6								
Totals				$a Ra$ $\frac{2}{3^3}$ of 3^6 = 54	$a R_2 a$ $\frac{2^2}{3^4}$ of 3^6 = 36	$a R_3 a$ $\frac{2^3}{3^5}$ of 3^6 = 24	$a R_4 a$ $\frac{2^4}{3^6}$ of 3^6 = 16	$a R_5 \dots$ $\frac{2^5}{3^6}$ of 3^6 = 32

SERIES OF 3^6 EVENTS

Showing limits of distribution of residual cycles (figures in thick type) in columns.

in bottom line of Table VI., B) involve the sequence $a R a$, the probability of which is $\frac{2}{3^3}$. All the cycles in the second column involve the sequence $a R_2 a$, the probability of which is $\frac{2^2}{3^4}$, and so on.

The calculation used for the "residual cycles" (figures in thick type in the Tables) differs in one respect from that which applies to the other cycles, because we have solved it in the general form, by taking cycles of all the possible lengths that could occur in a Norm Series. All the residual cycles that are contained in the first line (see Table VI., A) are cycles of a followed by *at least* 4 R 's; those in the second line are cycles of a_2 followed by *at least* 3 R 's, and so on. The respective sequences involved are $R a R_4 \dots$ and $R a_2 R_3 \dots$.

Taking the residual cycles contained in each column, (see Table VI., B) the number of a 's is similarly indeterminate; the residual cycles in the first column involving *at least* 4 a 's, followed by one R ; those in the second column *at least* 3 a 's, followed by 2 R 's and so on. The respective sequences involved are $\dots a_4 R a, \dots a_3 R_2 a$.

The brackets indicate respectively the lines or columns in which the numbers of cycles included under them must occur, leaving the distribution of the cycles among the squares undetermined.

It must be observed that the limits in regard to residual cycles shown in the two Tables VI. A and VI. B have to be *jointly* satisfied.

In attempting to make any particular solution of such a series, it has to be remembered that the various sequences of a and R must

TABLE VII.

	R_1	R_2	R_3	R_4	R_5	R_6	R_7	Total Cycles	Total Events
a_1	36	24	16	—	—	—	32	108	464
a_2	12	8	—	16	—	—	—	36	164
a_3	4	—	8	—	—	—	—	12	64
a_4	—	4	—	—	—	—	—	4	24
a_5	1	—	—	—	—	—	—	1	6
a_6	1	—	—	—	—	—	—	1	7
Totals	54	36	24	16	—	—	32	162	729

SERIES OF 3^6 EVENTS.

Residual cycles in thick type.

occur in their due proportions; and also that the cycles must be of such a length as to include between them all the events of the series. The latter condition, taken alone, is of course comparatively easy to fulfil, and Table VII. gives an example of it, which it may be instructive to compare with Table II. (See Chapter II., p. 189.)

General Formulae for Series with any number of Alternative Events.

The method of reasoning and calculation used with regard to series with three alternative events can without difficulty be extended to series with any number of alternatives, $a, b, c, d \dots$, if we take R to stand for any of the alternatives except a , say, which is chosen as the invariable first term of the cycles, and calculate the fractional probabilities of the various cycles in the same way as shown above.

Thus, in a series with t alternatives, the probability of the sequence Ra is

$$\frac{t-1}{t} \times \frac{1}{t} = \frac{t-1}{t^2}$$

Therefore in a series of length t^{n+2} the norm number of cycles is

$$\frac{t-1}{t^2} \times t^{n+2} = (t-1)t^n$$

out of which total it will be found that the number of residual cycles is

$$\frac{t-1}{t-2} \left\{ (t-1)^{n+1} - 1 \right\}.$$

All the cycles containing s a 's involve the sequence

$$\dots Ra_s R. \dots$$

and the norm number of these cycles in the same series is

$$\frac{(t-1)^s}{t^{s+2}} \times t^{n+2} = (t-1)^s t^{n-s}$$

This formula gives the norm number of cycles in any *line* of the Table.

All the cycles containing s R 's involve the sequence

$$\dots a R_s a \dots$$

and the norm number of these cycles is

$$\frac{(t-1)^s}{t^{s+2}} \times t^{n+2} = (t-1)^s t^{n-s}$$

This formula gives the norm number of cycles in any *column* of the Table.

Any cycle containing s a 's and m R 's involves the sequence

$$\dots R) a_s R_m (a \dots$$

and the norm number of such cycles is

$$\frac{(t-1)^{m+1}}{t^{s+m+2}} \times t^{n+2} = (t-1)^{m+1} \times t^{n-s-m}$$

Series in which all the Alternatives are not equally probable.

It may be worth while to note further that the same method may be used for series in which one alternative is more likely to occur than the other or others.

One of the simplest cases would be that of a series with two alternatives, *a* and *b*, in which the chance of *b*'s occurrence was twice as great as the chance of *a*'s occurrence. Table VII. just given would show the proportionate numbers of the various kinds of cycles in a series of this kind of 3^6 events, since the fractional probabilities are exactly the same whether *R* is taken to represent two out of three equally probable alternative events, or an alternative whose occurrence is twice as likely as that of the other alternative.

Similarly, if there are two alternatives one of which is three times as likely to occur as the other, this would be equivalent to a series in which *R* represented three out of four equally probable alternatives. For instance, in a series of guesses of the *suits* of playing-cards, when each card to be guessed is drawn at random from a full pack, the results may be treated as two alternatives: (a) right guesses, and (b) wrong guesses; and of these two, (b) is three times as probable as (a).

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APPENDIX II.

EXAMINATION OF A PREMONITORY CASE.

Mr. Lane's dream of the death of Mr. Terriss.

P. 174.

This is a case which has attained some notoriety on account of its association with the well-known actor, William Terriss. Mr. Frederick Lane, his "under-study," dreamt the night before the murder that he saw Mr. Terriss lying unconscious in a certain part of the Adelphi Theatre where he actually saw him the following evening. The narrative shows that Mr. Lane had apparently no reason for expecting the event. But it is also clear that the premonition might be explained as a clairvoyant or telepathic perception of the result of the intentions of the unfortunate man who committed the murder.

The preliminary statements were obtained by Mr. Podmore, who writes on January 4th, 1898 :—

I enclose accounts of a dream of Terriss's death.

- (1.) By Mr. Lane, the dreamer.
- (2.) By Miss Haygate, the first person to whom the dream was told.
- (3.) By Mr. Carter Bligh, one of several to whom the dream was told at the theatre, in the early afternoon of the 16th : Terriss was stabbed at 7.20 on the 16th.

Mr. Lane and Miss Haygate were understudies for Terriss and Miss Millward respectively.

Miss Haygate is a connection by marriage of a friend of mine, Mr. Ronald Hepburn. Mrs. Hepburn was dining with the D——'s on the evening of the 16th when Miss Haygate came in and told them of the murder and of the dream which she had heard a few hours before.

Mrs. Hepburn told me this the next day, and arranged a meeting for me on the 18th.

The accounts are as follow :

1. *From Mr. Frederick Lane.*

Adelphi Theatre, December 20th, 1897.

In the early morning of the 16th December, 1897, I dreamt that I saw the late Mr. Terriss lying in a state of delirium or unconsciousness on the stairs leading to the dressing rooms in the Adelphi Theatre. He was surrounded by people engaged at the theatre, amongst whom were Miss Millward and one of the footmen who attend the curtain, both of whom I actually saw a few hours later at the death scene. His chest was bare and clothes torn aside. Everybody who was around him was trying to do something for his good. This dream was in the shape of a picture. I saw it like a tableau on

which the curtain would rise and fall. I immediately after dreamt that we did not open at the Adelphi Theatre that evening. I was in my dressing room in the dream, but this latter part was somewhat incoherent. The next morning on going down to the theatre for rehearsal, the first member of the Company I met was Miss Olive Haygate, to whom I mentioned this dream. On arriving at the theatre I also mentioned it to several other members of the Company, including Messrs. Creagh Henry, Buxton, Carter Bligh, etc. This dream, though it made such an impression upon me as to cause me to relate it to my fellow artists, did not give me the idea of any coming disaster. I may state that I have dreamt formerly of deaths of relatives, and other matters which have impressed me, but the dreams have never impressed me sufficiently to make me repeat them the following morning, and have never been verified. My dream of the present occasion was the most vivid I have ever experienced; in fact, life-like, and exactly represented the scene as I saw it at night.

FREDERICK LANE.

Mr. Podmore appends the following note :—

January 4th, 1898.

At a meeting on the 20th December Mr. Lane gave me first a *visà voce* account of his experience, and then wrote it down, as above. He explained that he was in the neighbourhood of the theatre when Mr. Terriss was stabbed on the evening of Thursday the 16th December, 1897, and ran to the Charing Cross Hospital for a doctor. On his return he looked in at the private entrance, and saw Mr. Terriss lying on the stairs as in the dream.

F. P.

2. *From Miss Haygate.*

Adelphi Theatre, *December 18th, 1897.*

On Thursday morning about twelve o'clock I went into Rule's, Maiden Lane, and there found Mr. Lane with Mr. Wade. In the course of conversation after Mr. Wade had left, Mr. Lane said that he had had a curious dream the night before, the effects of which he still felt. It was to this effect: he had seen Terriss on the stairs, inside the Maiden Lane door (the spot where Terriss died), and that he was surrounded by a crowd of people, and that he was raving, but he (Mr. Lane) couldn't exactly tell what was the matter. I remember laughing about this, and then we went to rehearsal.

OLIVE HAYGATE.

3. *From Mr. Carter Bligh.*

Adelphi Theatre, W.C., *January 4th, 1898.*

DEAR SIR,— . . . I have much pleasure in being able to state that Mr. Fred Lane, on the morning of the 16th ult., at rehearsal at the Adelphi Theatre, told me among others in a jocular and chaffing way (*not believing in it for an instant*), how he probably would be called upon to play Captain Thomas, that night, as he had dreamt that something serious had happened to Terriss. I forget now, and therefore do not attempt to repeat, the exact words Mr. Lane used as to the reason (in the dream) why Mr. Terriss would not appear that night, but I have a distinct recollection of him saying that he (Terriss) could not do so, because of his having dreamt that something had

happened. It was all passed over very lightly in the same spirit in which it was given, i.e., in the spirit of unbelieving banter. . . .

H. CARTER BLIGH.

In reply to further inquiries, Mr. Podmore received the following letter from Mr. S. Creagh Henry :—

5, Milborne Grove, The Boltons, S.W., *January 20th.*

DEAR SIR,—With reference to your letter concerning Mr. Lane's dream, he mentioned it to me at rehearsal during the morning of the day which proved fatal to poor Terriss. The description he gave me was that he saw Mr. Terriss on the staircase (upon the landing where he died) surrounded by several people who were supporting him in what appeared to be a fit.

Something serious seemed to have happened, and no performance took place that evening,—another fact which was verified. As far as I recollect this was all Mr. Lane mentioned.—I remain, yours faithfully,

S. CREAGH HENRY.

Mr. Terriss, whose real name was William Charles James Lewin, was murdered by a man named Richard Archer Prince (Archer being apparently his family name and Prince his professional name), an actor out of employment, who was afterwards proved to be insane at the time.

The value of this case as evidence of supernormal perception of a future event of course depends primarily on whether Mr. Lane had any reason for anxiety on Mr. Terriss's behalf. The account of the murder given in the *Times* of the next day shows that Archer was known by some persons to have a grudge against Mr. Terriss, but it seems clear that very little attention was paid to this and that no one had any suspicion of the length to which he was prepared to go. And the evidence given later seems to show that this account exaggerated the suspicious incidents, in accordance with the common and natural tendency to imagine, after an event, that one has noticed some indications of it, if one did not actually expect the thing itself. The evidence further shows that, though Archer had expressed hostility to Terriss, he seems to have avoided doing so to persons in any way connected with the Adelphi Theatre; also that there were several other persons besides Terriss whom he had a grudge against, on account of their fancied ill-usage of him. Mr. Lane did not know him, except perhaps by sight, and had only heard of him as previously connected with the theatre and that he had the night before asked for Terriss. To this very common incident he could hardly be supposed to attach any importance.

I do not think, therefore, that anxiety about Terriss could have had any share in causing the dream; but it seems desirable to give the

evidence for and against this view, and I therefore quote the account in the *Times* of Friday, December 17th, and give an abstract of all the evidence given in the *Times* reports of the inquest, the examination before the magistrate, and the trial, which seems to me to have any bearing on the point. (See *Times* of December 18th, 20th, 21st, and 30th, 1897, and January 14th, 1898).

The evidence shows further the close correspondence of the events of the death with Mr. Lane's dream.

From the *Times* of Friday, December 17th, 1897.

Last evening, Mr. William Terriss, one of the most popular actors on the London stage, was assassinated at the private entrance to the Adelphi Theatre in Maiden Lane, Covent Garden. He had spent the afternoon with some friends, and had gone home to dinner at about 5 o'clock. Subsequently, he proceeded as usual to the theatre, where he was taking the chief part in *Secret Service*, and on reaching the private entrance he was suddenly attacked by a man between 30 and 40 years of age, who stabbed him in the region of the heart and again in the back. The weapon employed is described as a long, thin-bladed knife. Mr. Terriss at once fell to the ground, exclaiming: "He has stabbed me, arrest him." The assassin, after a struggle, was captured, and straightway conveyed to Bow Street Police-station. Mr. Terriss, meanwhile, was carried inside the theatre and medical aid was at once summoned from Charing Cross Hospital and obtained. It was not possible, however, to convey him further than the foot of the stairs leading to his dressing-room, and here, after lying in a state of semi-consciousness for about a quarter of an hour or twenty minutes, he died.

The assassin is known by the name of William Archer, or William Archer Flint. Some years ago he was employed as a supernumerary at the Adelphi, one of the pieces in which he appeared being *In the Ranks*. It is understood that he had frequently applied to Mr. Terriss and other members of the company for help, and a great deal had been done for him by them. Recently, however, he had become so importunate in his demands that Mr. Terriss refused to do anything more for him, but referred him to the Actors' Benevolent Fund, from which association he received a grant. For several nights past, Archer had been noticed hanging about the private entrance to the theatre, and on Wednesday night, it is reported, he made an enquiry of the stage-door keeper as to Mr. Terriss's whereabouts. He is said to have been known in the theatre as "Mad Archer," but the stage-door keeper and others last night expressed a doubt as to whether he is insane. . . . In reply to the charge of murder, he is reported to have said: "He has done me out of the Benevolent Fund this morning, and I am out of it for life."

Mr. Frederick Lane, who "understudies" Mr. Terriss in the part of Captain Thorne, had a peculiar story to tell. He said:—

"I dreamt about this very thing last night, and when I came to the theatre this morning for the rehearsal, I told all the 'boys' about it. I dreamt I saw Mr. Terriss lying in the landing, surrounded by a crowd, and that he was raving. I seemed to see it all and then it all seemed to fade

away. It was a horrible dream, and I could not tell what it meant. I tried to forget it during the day, but to-night again, when I came to the theatre, I was going down Bedford Street, when something seemed to say, 'Do not go there.' I then went round to Maiden Lane, and there I saw this villain. I had heard of him as being an old super, and I knew he was asking for Mr. Terriss last night. His appearance struck me as peculiar. He wore a big cloak and a slouch hat. I, however, do not know him, and he said nothing to me. I walked on, and then a few minutes afterwards I heard a great noise and found that he had stabbed Mr. Terriss. I rushed back and saw Mr. Terriss taken indoors. If it had not been for the police, I believe the man would have been lynched. He was a fellow of average height, had a dark moustache and a somewhat foreign appearance. I can suggest no motive whatever for the crime. The man may possibly have been refused money. I cannot tell, though; and it is at any rate certain he had no reason to go to such an awful extreme. Mr. Terriss was the kindest of men, and we loved him both on and off the stage. He was, indeed, 'one of the best.'"

Another member of the Adelphi company further corroborated the statements previously made in reference to the personality of Mr. Terriss's assailant, mentioning that at the theatre he was generally known as "Mad Archer." When seen by Mr. Nicholls on the previous evening and told that he could not see Mr. Terriss, he is stated to have gone away murmuring "Not yet." . . . Archer is alleged to have nursed a grievance against Mr. Terriss even before he left the employ of the Adelphi, and is reported to have more than once stood in the wings and made sarcastic remarks at the expense of the deceased actor. One of his remarks is quoted as an instance of this: "Fools often succeed in life where men of genius fail."

ABSTRACT OF EVIDENCE GIVEN IN "TIMES'" REPORTS.

MR. J. H. GRAVES said that he spent Thursday afternoon with Mr. Terriss and at about 7 o'clock drove with him to Adelphi Theatre—driving to corner of Maiden Lane, Strand, where they both alighted and walked to the private entrance a few yards up the lane. This entrance was only used by Mr. Terriss and one or two others. As he was putting his key into the lock, the prisoner rushed forward from across the lane and stabbed him. Mr. Terriss fell and Mr. Graves seized the prisoner and gave him in charge to a constable, whom he accompanied to Bow Street. He went back to the theatre and found Mr. Terriss lying at the foot of the stairs a few paces from the door, attended by a doctor and several others. He died a few minutes later.

The prisoner said to him that Mr. Terriss had kept him out of employment for ten years, and he had either to die in the streets or take his revenge on him. The truth was that Mr. Terriss had often helped him and had recommended him to the Actors' Benevolent Fund, from which he had got help many times. He had called at the office of the fund on that day, and had been told that the Committee could not meet then to reconsider his case. He had for some time been almost a weekly applicant there.

Sub-Divisional-Inspector WILLIAM FRENCH was at Bow Street when the prisoner was brought in and returned with Mr. Graves to Adelphi Theatre,

entered private door in Maiden Lane, and found Mr. Terriss lying in the passage. He was unconscious and remained so until his death. Three doctors were present, and Miss Millward was supporting his head. She asked him if he knew her. He made no reply to her, but shouted out several times: "Get away, get away."

MR. BRAGG, Police-Constable, who arrested the prisoner, said the latter had been asked by Graves on the way to Bow Street why he did it, and he answered: "In revenge. He has blackmailed me for ten years. I gave him due warning. I should either have had to die in the streets or have my revenge."

INSPECTOR WOOD, before whom he was brought at Bow Street, said he gave same answer to him. To INSPECTOR CROXTON he said that his sister was in league with Terriss in blackmailing him.

At the trial, MR. GILL, the Counsel for the Crown, said there could be no question but that the prisoner had some years ago conceived a violent hatred for Mr. Terriss, and would appear to be under the impression that Mr. Terriss was in some way preventing him from getting on in his profession. That was shown by statements made at different times to persons to whom he complained that Mr. Terriss was preventing him from getting employment and was blackmailing him. As far as could be ascertained that was a complete delusion on his part; Mr. Terriss had, on the contrary, assisted him to obtain charitable relief and employment. The point at which the evidence commenced was in October last, when the prisoner came into contact with Mr. Croydon, a theatrical manager at Newcastle. About the end of October he came to London and purchased a knife (probably the one with which the murder was committed). It was during the period between this time and the murder that he was getting assistance from the Actors' Benevolent Fund. On November 9th he obtained a letter of recommendation to them from Terriss, on the strength of which he received various small sums from the fund.

After being arrested, prisoner said to the Inspector about the knife, "That is what I stabbed him with. He had due warning, and if he is dead, he knows what he has to expect."

It was clear that he was acting with the greatest deliberation and that, cherishing a feeling of hatred against Mr. Terriss, he was uttering threats as to what he proposed to do. During the time he was in London at the end of October, he was obviously contemplating the crime, because they found him purchasing the knife and uttering the threats against Mr. Terriss.

MR. R. CROYDON, theatrical manager, had engaged the prisoner in October last to take a part in his company at Newcastle. The prisoner told him he had only left the Adelphi through one man, and might have starved but for the Actors' Benevolent Fund, and that he would be even with this man some day. He did not learn his part and behaved very strangely, and consequently was discharged next day. He said he had then two enemies, and on being asked who the other was, said it was Terriss. Mrs. Croydon said "You are mad;" and he replied, "Yes, and the world will ring with my madness." He then left and they had not seen him again.

MR. DENTON, theatrical manager, of Maiden Lane, said he had known the prisoner for some time, and had then lost sight of him till October or

November last, when he called nearly every day at his office for employment. He tried to get him employment and offered him a week's engagement, which he would not accept. On the afternoon of December 16th, prisoner called to ask if he had got him any employment and he said he had not. Cross-examined, said the prisoner did not appear to have any particular animosity against Mr. Terriss; was peculiar, and could easily be put into a temper by chaff—he seemed to think he ought to play more important parts than were given him.

MR. COLSON, Secretary to the Actors' Benevolent Fund, said that help had been given to the prisoner on the recommendation of Terriss in his letter of November 9th. He read seven letters from the prisoner to the managers of the Fund asking for help, saying that he was in great trouble, on account of having lost an engagement "through no fault of my own," but not complaining of any one otherwise than in that phrase, and expressing great gratitude for what they had done for him.

MRS. DARBY, his landlady, had seen the knife in his room upon more than one occasion before the murder, but she never saw him use it for anything. She had seen marks on it as if it had been used for cutting bread. She knew he was in trouble for want of money, as he could not pay his rent.

G. LORBERG, cutler, Brompton Road, said in October he had knives for sale similar to one produced, price 9d., and one evening in October, a tall, shabbily-dressed man bought one. (Evidence at police-court made it almost certain that this was the knife with which the murder was committed.)

MR. THOMAS TERRISS, son of the deceased, said he never knew him to be threatened at all, did not know he had an enemy in the world. "He had not to his knowledge ever seen the man who was charged with his murder until he saw him at Bow Street on Friday morning, when he found he was not a man whom he had previously known as 'Mad Archie.'"

HENRY SPRATT, stage door keeper to the Adelphi, said he first saw the prisoner about two months ago, when he brought a letter for Mr. Terriss to the stage door and waited for an answer. Half an hour later a message came down, "All right." A few nights after this Mr. Terriss spoke to witness about messages. Witness saw the prisoner about half a dozen times after this. He would wait outside the door for half an hour or so and then go away without saying anything, except on December 15th, which was the last time witness saw him, when he asked if Mr. Terriss came out that way, meaning by the stage door. Witness replied that he did, which was not the fact; but he said it in consequence of what Mr. Terriss had said to him, in order to keep the man away from the private entrance in Maiden Lane. Cross-examined, said it was not an uncommon thing for persons who had been employed at the theatre to wait about the stage door, nor was it an uncommon thing for persons to come there and enquire in regard to others.

W. ALGER, dresser to Mr. Terriss, said that he did not know the prisoner. On the night of December 15th he saw the prisoner at about 8.30 watching the people coming out of the stage door, but did not speak to him.

THE DEFENCE was directed towards proving insanity. Evidence was given that the prisoner had had many delusions of persecution, thought his

mother and others had put poison into his food ; that Mr. Arthur, the "theatre-master" at Dundee, "blackmailed" him. He had also worked as a labourer and thought his fellow workmen and others had tried to keep him out of work, had violent fits of passion, thought his brother was in league with Mr. Arthur in blackmailing him and had attacked his brother several times, once with a knife and poker. Two of his brothers had been insane.

R. BEVERIDGE, attendant at the Dundee Theatre, gave evidence of his thinking himself a great actor and badly used in general. Once he wanted to "go for" an actor named Stewart, and when Beveridge turned him out, prisoner pulled out a revolver from his pocket and threatened him.

A. HUSBAND, foreman at Wallace Foundry, in Dundee, said prisoner worked there in 1896 (in the intervals of his being employed as an actor), and said Mr. Terriss blackmailed him. He showed him a letter from Terriss saying that he should be glad to hear of his getting an engagement, and that he might give his name as a reference. The prisoner had asked Mr. Terriss for a character, and he thought that letter not sufficient, and called it blackmailing. He said the same thing about Mr. Elliston, who, he said, was in league with Mr. Terriss.

MR. ELLISTON, theatrical manager, who had employed prisoner, said since prisoner had left him, he had received a number of letters and post-cards from him stating that he had not given him a reference, that he had blackmailed him and tried to prevent his getting employment, which was not true.

DR. BASTIAN gave opinion that he was insane, and his mind saturated with delusions of persecution. He did not attach any importance to purchase of knife ; did not think prisoner contemplated doing the murder a month before he did it. If he had premeditated it, he might have been insane all the same, but he did not believe he had. Did not believe prisoner went to theatre with intention of committing the act, but that he wanted to speak to Mr. Terriss, and as Mr. Terriss did not speak to him, he struck him, having the knife in his hand.

Two other medical experts gave very decided opinions as to his insanity, but said nothing as to whether the act was premeditated or not.

The verdict given was to the effect that the prisoner was guilty of wilful murder—that he knew what he was doing and to whom he was doing it, but, on the medical evidence, that he was not responsible for his actions.

APPENDIX III.

SOME PREMONITORY DREAMS OF RACES.

The following narrative, which is of interest in connection with Professor Haslam's dream of the winner of the New Zealand Cup (see above, p. 250), was compiled by Mr. Donald Murray, Editor of the *Sydney Morning Herald*, a Member of the Society.

MELBOURNE CUP DREAMS.

The race for the Melbourne Cup has acquired such celebrity in Australia that it has come to be regarded as a national event. It attracts visitors from all parts of Australia, and for weeks beforehand the Melbourne Cup is one of the chief subjects of conversation over a continent as large as Europe. Cup Day is the culminating point of a saturnalia of gambling, the results of which penetrate to every corner of Australia. Under these circumstances it is not surprising that there is always a large crop of dreams of the Winner of the Cup. It is also not surprising that, as there are never more than about 150 entries and seldom more than about 20 or 30 starters for the Melbourne Cup, and as the dreams must be numbered by thousands, one or two of the dreams occasionally come true. The odds are heavily in favour of such a contingency. As an illustration of the fact that racing dreams are fairly numerous in Australia, I enclose an extract from the *Sunday Times* of November 1st, 1896, a Sydney paper, in which mention is made of half-a-dozen Cup dreams. The Cup was run two days after the publication of these dreams, and it is notable that not in a single instance was either the first, second, or third horse correctly indicated. The extract, however, serves to show how widespread is the tendency amongst sporting people to dream of the Cup, and consequently the extreme probability of one or two of the dreams occasionally coming true by chance coincidence.

Another, and a very droll aspect of the matter was revealed to me by the sporting editor of the *Sydney Morning Herald*. I asked him if he thought there was much betting on dreams of the Cup. "Of course there is," he replied, "why, some people make a practice of taking a heavy dinner of pork in order to dream of the winning horse." The same authority informed me that entries for the Melbourne Cup took place about the second of June each year, and that the Cup is run in November, or about six months afterwards. Bets used to be made as much as a year beforehand, but long range speculation of this kind is not now practised to any extent. The entries number from 100 to 150, and of these usually not more than from 30 to 40 horses actually take part in the race. The odds against some of the individual horses, rank outsiders, may be as long as 1,000 to 1, while the chances against other horses may be as short as 5 to 1. On the average, however, the odds against any horse selected at random six months beforehand would not be more than 150 to 1, and a few days before the race not

more than about 50 to 1. The names of the horses entered for the race are published all over Australia for months previously, and there is consequently plenty of opportunity for the dreamers. If there were only 1,000 dreams (possibly there are 10,000) then the odds would be about 10 to 1 in favour of at least one dream coming true. For this reason—because racing dreams come so completely within the range of explanation by ordinary coincidence—but little importance can be attached to such occurrences.

On at least two occasions, however, Cup dreams have occurred of such a remarkable character that they have received notice in the public press. One of these I shall describe as the "Auraria dream," and the other is the so-called "Nimblefoot Mystery." The latter seems to have impressed the public mind to such an extent that twenty years afterwards it was remembered and used as a heading for a Cup dream paragraph. A passing acquaintance in a boarding-house was also able to tell me about it. I will deal first with the "Auraria dream."

On November 6th, 1895, the *Evening News*, a Sydney evening paper, published [a] paragraph [about it, which, however] gave no clue for further enquiries, but the following paragraph from the *Australian Star*, another Sydney evening paper, on November 9th, 1895, dealing with the same subject, mentioned the name of Mr. J. Dalveen, from whom I obtained some corroboration of the story:—

"With the majority of sensible folk, dreams of the Melbourne Cup winner are regarded in the light of a time-honoured joke. One instance, however, has come under our notice which it is impossible to accept otherwise than as a truthful and remarkable occurrence. Some four months ago a working man resident in Redfern had a remarkable dream in which he saw Auraria win the Melbourne Cup; he also dreamt that before the Cup was run he (the dreamer) died. So vivid was the impression that he backed Auraria to win £200—the odds at that time were very long. He gave the tickets to his wife. At the time of the dream he was in his usual health, but he took ill and died some weeks before the race was run. One part of the dream coming true, the story was circulated among the neighbours, several of whom backed the horse. Our informant was among these. He put £1 on the filly, with the result that he won £50. He laid the wager with Mr. J. Dalveen, who at the time laughed at the idea. But the laugh was on the other side when settling time came, and the young man who backed the dream fancy has now the bank deposit slip to show for his faith. Numbers of his friends also chaffed him about his superstitious fancy—before the race. Now he is engaged in the pleasant pastime of asking them whether their opinion is shaken. And the widow of the man who dreamt the winner is the richer by £200. Truth is still stranger than fiction."

I wrote to Mr. Dalveen, a bookmaker, asking him whether the paragraph was true, and if so, whether he could supply me with the name of the gentleman who laid the wager with him, or better still the address of the widow of the man who had had the dream. After some delay my letter was returned with the following note written upon it by a Mr. J. W. M.—

"Mr. Dalveen has handed me this letter of yours, as I took the bet of £50 to 1 about Auraria from him. I spoke to the widow and she does not wish to have her name published in any paper about the sad affair. The

dream was, sad to state, quite true in every detail.—Yours faithfully, [signed in full] J. W. M.— Don't publish my name."

I wrote to Mr. M. explaining that I simply wanted more precise details. I got no reply. Finally I called on Mr. Dalveen and obtained Mr. M.'s address. He was an employé of a clothing company in George-street, Sydney. He assured me that the paragraph was perfectly true, and he added:—"When I heard of the death, I said to myself, 'Here's a leg in' and I laid the wager." Mr. M. is a respectable young man, and the details of the dream may, I think, be relied on as far as they go. He told me he must decline to give me the address of the widow, as she had been worried so much by people calling to see her about the dream that he did not care to subject her to any further annoyance. I made some more attempts to find out the address, but without success, and being very busy at the time I had to abandon the quest. It will be seen that there are certain features about this dream which would have rendered more precise details very desirable. For instance, the odds, as already explained, against the dreaming of the winner of the Cup were not more than about 100 to 1. In the *Star* account the date of the dream is given as "some four months" before the day of the race, and the paragraph says, "At the time of the dream he was in his usual health, but he became ill and died some weeks before the race was run." Much would depend on the nature of the illness. At the time of the dream it is quite possible he may have known he had not much longer to live—or he may at least have had a subliminal consciousness of the fact, arising from unnoticed inroads of the malady, whatever it was. In that case the additional coincidences of dreaming of his death before the Cup and of his widow drawing the money, would not increase the probabilities against chance coincidence. These incidents would be merely deductions from facts known to the subliminal mind. Unfortunately, as I was unable to obtain the address of the widow, I was also unable to obtain the address of the doctor who attended her husband.

The mention of the "Nimblefoot Mystery" in the heading to the *Evening News* paragraph led me to make enquiries about this case also. A boarding-house acquaintance told me it was most remarkable, that it had involved a "bet on a double" of £20,000, and that there had been a big lawsuit about the payment of the bet after the death of the owner of Nimblefoot. He also mentioned that it had been fully reported in the *Australasian*, a well-known Melbourne weekly paper. I wrote to the editor asking him for the date, and the reply under the head of answers to correspondents was as follows:—

"S.P.R. (Sydney). We do not remember any lawsuit about Nimblefoot. J. Day, the lad who rode him in the Melbourne Cup, and W. Lang were parties in some action in which the question of Day's engagement to Lang was concerned."

The boarding-house acquaintance then recommended me to write to the proprietor of Craig's Royal Hotel at Ballarat, and ask for the date of the *Australasian* containing particulars of the Nimblefoot dream. I did so, and a reply came from Mr. W. L. Bentley, giving the date as October 31st, 1891. On enquiry it turned out that there were no copies of the *Australasian* of this date for sale, but the following is a copy of a

paragraph it contains referring to the Nimblefoot dream. It occurs in the *Australasian* of October 31st, 1891, page 865, in a sporting article by "Augur," entitled "A Cup Chronicle.—Past and Present."

"While in 1870 Nimblefoot was victorious, and the alleged dream of his dead owner, Mr. Walter Craig, was said to have been realised. It will be remembered that by Nimblefoot's triumph the veteran bookmaker, Slack, had been struck for a double which he had laid to Mr. Walter Craig; but if I mistake not, the bet was in reality taken by Mr. Stephen Holgate in conjunction with Mr. Craig. It was £1,000 to several drinks and cigars against Croydon winning the A. J. C. Metropolitan and Nimblefoot the Melbourne Cup. The double came off, and it was generally believed that Slack paid the £1,000 to Mrs. Craig, but he gave her only half. It was a splendid advertisement for the rough old penciller, and ever afterwards he did a roaring business as a layer of double events."

. . . In 1870 there were 29 competitors for the Cup. The *Argus* tip for the Cup on the morning of November 10th was:

1. Trump Card, 4 to 1 against.
2. Croydon, 100 to 14 against.
3. Nimblefoot or Strop, 100 to 7 against.

The *Argus* of Friday, November 11th, the day after the Cup, has a aragraph as follows:—

"A remarkable circumstance in connection with the victory of Nimblefoot formed matter for a good deal of comment and wonder yesterday; that was the fact that prior to his death Mr. Walter Craig had dreamed of a horse carrying his colours, but with the jockey wearing crape, winning the Cup this year. Differing from the great majority of prophetic dreams, this one was on record prior to the event, and at the beginning of the week appeared in the following form in the *Bendigo Independent*:—

"It is said that the late Mr. Walter Craig, of Ballarat, told some of his friends a short time before his death that he had dreamed that he saw a horse, ridden by a jockey wearing his well-known colours, but with crape on his left sleeve, coming in first for the Melbourne Cup. Now Nimblefoot, Mr. Craig's horse, won the Hotham Stakes on Saturday, and his jockey, who wore crape on his sleeve in memory of the late Mr. Walter Craig, was loudly cheered on coming to scale. . . . Whether Nimblefoot will win the Melbourne Cup is another matter, but should that be the case, it will be somewhat startling."

It will be noticed that nothing is said in this paragraph about the double event bet, but apparently there was such a transaction, and that it was widely known may be fairly inferred from the paragraph in the *Australasian* of 1891, or 11 years afterwards. It is, of course, impossible after so many years to obtain any further corroboration of this case, beyond perhaps looking up the files of the *Bendigo Independent* to verify the foregoing paragraph. That could be done, but it seems hardly necessary, as the *Argus* is recognised as a careful and accurate paper.

Details such as those about the jockey wearing crape on his sleeve, the death of the owner of Nimblefoot before the Cup, the betting on the double

event (if the accounts about the latter can be relied upon) seem to me to bring this case out of the ordinary ruck of Cup dreams. . . .

As a further illustration of the likelihood of chance coincidence being the true explanation of ordinary Cup dreams, it may be mentioned that a few years ago, when the last wave of spiritism, culminating in the exposure of Mrs. Annie Mellon, passed over Sydney, several members of the New South Wales Parliament held a table-tilting séance, and, taking a published list of the entries for the Cup, tilted through the list for the winner. The table persistently bumped at one name, and this horse was backed by about a dozen prominent members of Parliament. The horse won the Cup, and as the odds were fairly long, the members who laid the bets won sums ranging from £10 to £100. The result was that for a time table-tilting as an adjunct to horse-racing became very popular in Sydney; but the method was found to be unreliable, and racing speculators have been compelled to fall back on pork suppers as a stimulant of supernormal faculty.

DONALD MURRAY.

APPENDIX IV.

A GUESSING COMPETITION CASE.

The following account refers to a success in a guessing competition of one of the kinds now constantly being advertised in weekly papers. It happens that a good deal of information is available about it, as it was the subject of two trials, the proprietor of the paper having refused to pay the prize money to the successful competitor, who therefore brought an action against him. The jury gave a verdict for the plaintiff, but this was disallowed by the Judge, on the ground that the competition was a "lottery" (that is, a distribution of prizes determined by lot or chance, and involving no skill) and therefore illegal. The judgment was appealed against, and the Court of Appeal determined that the competition was not a lottery. The question is important theoretically, as well as legally, and as my own view is that it was a lottery, I reprint all the arguments used on both sides.

The case was first tried at the West Riding Assizes at Leeds, on March 22nd, 1898, and I give the report published in *The Yorkshire Post* of the following day in full, in order to show what evidence there is that the correct guess was really made.

NISI PRIUS COURT.

(Before Mr. Justice LAWRENCE and a Special Jury.)

A CURIOUS GUESSING COMPETITION.

A SHEFFIELD BUTCHER CLAIMS £1,000.

John Henry Hall, butcher, near Sheffield, brought an action for breach of contract against William Henry Bingham Cox, printer and publisher of a paper called the *Rocket*. The claim was for £1,000, alleged to have been won by the plaintiff in a guessing competition in December last. Mr. Walter Beverley appeared for the plaintiff, and the defendant was represented by Mr. Tindal Atkinson, Q.C., and Mr. Longstaffe.

Mr. Beverley, in opening the case, stated that Mr. Bingham Cox, in copies of the paper of November last, had offered a prize of £1,000 to any one who should predict the exact number of male and female births, together with the number of deaths in London for the week ending December 11, 1897. There was also a number of consolation prizes offered for those who might not predict the exact figures, but get very near to them. Hoping to be the £1,000 prize-winner, the plaintiff bought 252 copies. From these copies he cut out the coupons, filled them up in the required manner, and forwarded them to the offices of the *Rocket*. In one of these coupons,

according to counsel, he predicted that the number of male births for the week ending December 11 in London would be :—Males, 1,244 ; females, 1,245 ; and deaths, 1,866. Plaintiff afterwards obtained from the Registrar-General or from the Queen's Printers (Messrs. Eyre & Spottiswoode) the returns of the male and female births and of deaths for the week in question, and it was alleged that the figures were precisely the same as those that had been forecast by the plaintiff. On the 16th December plaintiff wrote to the *Rocket* office calling attention to the fact that he had been right in his forecast, and congratulating himself on his luck. To this letter there was no reply. He again wrote on the 29th December but got no reply. He wrote again on the 3rd January, and received as reply that when the result had been arrived at, it would be made known in the *Rocket*. The plaintiff put the matter into the hands of his solicitor. In the course of further correspondence it was contended on behalf of the defendant that the competition had been declared to be an illegal lottery, that he had been fined £20, and that the coupon on which the plaintiff contended he had written the exact figures had got gummed or fastened to another coupon, and had consequently been put to one side, and not deciphered. Even if these statements were true, the defendant, counsel contended, had made them too late to allow him to break away from his contract. The plaintiff, concluded Mr. Beverley, had taken extraordinary pains to win the £1,000, and having, as he thought, become entitled to it, he ought to succeed in this action.

The plaintiff gave evidence bearing out counsel's statement. During his evidence a kettle was brought into court with the object of separating two coupons, one of which the plaintiff contended contained the figures entitling him to the £1,000 prize.

The Associate (Mr. Wade) held the papers over the kettle, and whilst the process lasted, the business of the Court was brought to a standstill.

His Lordship : I think this process might be carried on outside by two solicitors. The first who sees anything favourable to his side can come running back. (Laughter.)

The kettle and documents were here taken out of court by the solicitors.

After the supposed winning coupon was brought into court, it was passed up to the Judge, and the jury afterwards examined it.

Mr. Atkinson contended that it was impossible to make out what figures had been on the coupon, and that in the space for the number of deaths there never had been any figures filled in at all.

Plaintiff, in cross-examination by Mr. Atkinson, said he had been obliged to file his petition through speculating on a stand on the occasion of the Queen's visit to Sheffield.

Mrs. Hall, wife of plaintiff, and a letter carrier also gave evidence for the plaintiff.

For the defence, Mr. Atkinson submitted that the coupon in question did not contain the necessary figures. The question, however, was one entirely for the jury. The defendant had carefully refrained from touching the coupon that was glued to another, and which, according to the plaintiff, contained the exact numbers. It could not be suggested that the defendant had caused the coupons to be glued together. There was every indication of these coupons and others sent by the plaintiff having become glued

before they left plaintiff's possession. Again, was it possible that any one would be able to guess the exact number of male births, of female births, and of deaths in London in one week? With regard to the letters of the plaintiff not being replied to, it was to be borne in mind that there had been thousands of coupons sent in for this competition, and that much time was required to go through them.

Mr. Kennedy, assistant manager to Mr. Cox, gave evidence to show that the coupons as produced in court were the same as had been forwarded to the office. He had not glued them, nor had any one else in the office, nor had any one tampered with the coupons in question or with any of them.

Mr. A. Allison, also in the employment of the defendant, spoke to the care with which the coupons had been examined.

Mr. Bingham Cox, proprietor of the paper, gave evidence. The coupon on which it was alleged the correct figures had been written was glued to another when he first saw it. It was afterwards placed in the hands of his solicitor, and had not left that gentleman's hands since. It was quite untrue that either himself or any one in his employment had tampered with the coupons in any way whatever. There would be from 80,000 to 100,000 coupons.

The Judge, in his address to the jury, laid stress on the fact that it was for the plaintiff to make out his case.

The Jury retired to consider their verdict. On returning into court after half-an-hour's absence they gave a verdict for the plaintiff.

Mr. Beverley : I formally ask for judgment.

Mr. Atkinson, Q.C. : Before judgment is given, I shall have to argue the point as to whether this competition was a lottery or not, and whether the £1,000 can be legally paid over.

The Judge : Very well ; that is clearly a point that must be argued. It may be argued at the next sitting of the Court.

Judgment was accordingly deferred until to-day (Wednesday), in order that this point of law might be discussed.

On the following day the point was argued whether the competition was a lottery or not. As this question is important, not only as a point of law, but, as I have said, from a theoretical point of view, I again give the report in *The Yorkshire Post* (of March 24th, 1898) in full.

Mr. Atkinson, Q.C., who appeared for the defendant, addressing the Judge, now contended that although the jury had given a verdict for the plaintiff, judgment ought clearly to be given for the defendant, and that the plaintiff was not in a position to recover. That the competition was a lottery had already been decided, inasmuch as Mr. Cox had been fined £20 and costs in respect of the competition on which the present action was based. A lottery had been described to be "a distribution of prizes determined by lot or chance." This had been well illustrated in what had come to be known as the "Missing Word Competition"—*Barclay v. Pearson*. In that case Mr. Justice Stirling, before whom it was tried, ruled that if the competition had been one that involved the skill of finding out

the most appropriate word in a particular sentence, that would not have been a lottery, because there would have been an element of knowledge or skill. The word in question was not necessarily the most appropriate one, but merely a word which the publisher of the paper in question had himself decided to be the word which was the most appropriate word. Mr. Justice Stirling had held that under these circumstances it was necessarily a matter of chance that the word could be selected, and consequently no skill whatever entered into the competition. There was, said Mr. Atkinson, just as much of the element of chance in the present case as in the one known as "the Missing Word Competition." The competitors in the present case were to guess the number of male and female births, and the number of deaths in a certain week. In such a calculation there could be no element of skill in arriving at the particular results. There were he did not know how many billions to one against any one arriving at the accurate figures. In roulette, in spite of all its elements of chance, there was a certain margin of skill, although it might be almost infinitesimal. The case of horse-racing tipping might be drawn into the question before the Court. That case, however, as had been shown in *Sagar v. Stoddart*, was entirely different. There was some knowledge on which the first, second and third horses in a race might be prophesied. There the matter was to some extent a matter of experience and knowledge. There were the performances of the horses and other matters tending to raise the prediction from the domain of mere chance. As he had said, however, the case of the *Rocket* was one of pure chance, and therefore came under the Lotteries Act. Being a lottery the plaintiff was not entitled to recover, and judgment should be entered for the defendant.

Mr. Walter Beverley, who was counsel for the plaintiff, proceeded to argue in support of the verdict of the jury, and in refutation of the plea that the competition was a lottery. In a lottery there was, he said, no opportunity given for the employment of any skill or judgment whatever. It depended purely on chance, and no exercise of skill or knowledge entered into it. The present case was entirely different, for the plaintiff had clearly shown that he had taken a considerable amount of trouble to obtain information to enable him to form a correct judgment.

The Judge: He could scarcely inquire at a house when a baby was likely to be born. (Laughter.)

Mr. Beverley repeated that he had done much to obtain information to form a correct judgment, and if with the aid of such knowledge the plaintiff had guessed the correct figures, he was entitled to win in the present action.

The Judge said the case before the Court was one full of difficulties. He had not had an opportunity of looking into all the cases bearing on the subject, but so far as he had formed an opinion, he was inclined to the belief that there was more of the element of chance than of skill in the matter. The case, however, was a very important one, and one on which there ought to be a definite pronouncement. He was going to give the judgment he intended in order that there might be another lottery, and that the plaintiff might go to the lottery of the Court of Appeal if he wished, and try his luck there. (Laughter.)

Mr. Atkinson : I take it there will be judgment for the defendant with costs, with stay of execution.

The Judge : That is so ; and with a further stay of execution if necessary for the appeal.

The judgment was appealed against, and the case was tried again before the Court of Appeal of the Supreme Court of Judicature, Lord Justices A. L. Smith, Rigby and Collins on the bench, on December 2nd, 1898.

The following report of the trial appeared in *The Times* of December 3rd.

HALL V. COX.

This was an appeal by the plaintiff from the judgment of Mr. Justice Lawrance at the trial of the action, with a jury, at Leeds. The defendant advertised in a paper called the *Rocket* that a prize of £1,000 was offered for a correct prediction of the numbers of male and female births and the number of deaths in London during the week ended December 11, 1897 ; the competitors to fill in and sign the accompanying voucher, paste it on a sheet of paper, and send it, with a coupon cut from the front page of the *Rocket*, in accordance with the instructions given. The voucher contained the following words :—" I say that the number of births and the number of deaths in London during the week ending December 11, 1897, as disclosed by the Registrar-General's returns, will be ——" Then followed spaces for the number of births (male), births (female), and deaths. The answers were to be sent in by December 10 ; and if more than one correct prediction was received, the £1,000 would be divided. The advertisement also stated that, "according to the Registrar General's returns, the number of births and the number of deaths in London during the week ending December 12, 1896, were as follows :—Births (males), 1,342 ; ditto (females), 1,213 ; deaths, 1,539." The plaintiff, having sent in a correct prediction, brought this action to recover the £1,000. The action was ordered to be tried without pleadings under order 14. At the trial before Mr. Justice Lawrance, at Leeds, the defence was that the answer had not been received ; but the jury found that a correct answer had been sent in by the plaintiff, and gave a verdict for him for £1,000. The learned Judge then, upon the application of the defendant, entered judgment for him on the ground that the competition was a lottery. The learned Judge, in giving judgment, said that the case was full of difficulty, and he had not an opportunity of looking at all the cases bearing upon it, and he did not think he would be very much wiser if he did look at them. The foundation of a lottery was chance, and he should have thought the foundation of guessing how many children would be born next week or next month was a matter more of chance than of skill. Of course, with a verdict of £1,000 in question, neither party would rest at that Court ; and the case seemed to be eminently one on which the Court of Appeal should give their opinion. For the purpose of sending it there he would not take time to consider his judgment, but would hold that the competition was a lottery, and that the plaintiff might once more be put into a lottery which might be as much a matter of chance as of skill—go to the Court above. He accordingly entered judgment for the defendant. Since

the trial the defendant had died. His personal representatives did not appear upon the appeal.

Mr. Brooke Little, for the plaintiff, contended that this was not a competition of mere chance, but some skill came into it—namely, calculating from previous returns of the Registrar-General how many births and deaths would be likely to take place in London in a particular week. It was not, therefore, a lottery. He referred to “*Caminada v. Hulton*” (60 L.J., M.C., 116), “*Morris v. Blackman*” (2 H. and C., 912), “*Taylor v. Smetton*” (11 Q.B.D., 207), “*Barclay v. Pearson*” (1893, 2 Ch., 154), “*Stoddart v. Sagar*” (1895, 2 Q.B., 474).

The Court allowed the appeal.

Lord Justice A. L. Smith said that, in his opinion, this was not a lottery. The solution of the question did not depend upon mere chance. It depended very largely upon chance, but there was an element of statistical inquiry brought into it. The advertisement which offered the prize set out the number of births and deaths for the corresponding week of the preceding year. A statistical calculation was imported into it, and it was not all chance. In “*Caminada v. Hulton*,” a Divisional Court, consisting of Mr. Justice Day and Mr. Justice Lawrance, held that an offer of prizes to any purchaser of a book who should fill up a coupon with the names of the winning horses in certain races about to come off was not a lottery. How the learned Judge reconciled his judgment in the present case with his decision in that case he (the Lord Justice) did not quite understand.

Lord Justice Rigby and Lord Justice Collins concurred.

In reply to an inquiry made later in the same month, I was informed that “the *Rocket* and its late proprietor are both dead,” so that it seems very doubtful whether the prize money was ever paid.

I have ascertained that the numbers of births and deaths quoted are the same as those given in the Registrar-General’s *Annual Summary of Births, etc., in London*, for 1896 and 1897.

The question whether the competition was one of pure chance or whether any skill entered into it is very complicated, and I venture to think that the arguments used by the Counsel and Judges betray some obscurity of mind on the subject. There are, however, certain statistics which it might be supposed would afford help in the guessing, and I give a few figures taken from the *Annual Reports* and *Annual Summaries* of the Registrar-General about those facts which seem to have the most direct bearing on the question, and then proceed to consider what inferences may be drawn from them.

(1) *The Marriage Rate* (i.e., number of persons married per thousand of the population).—This has been steadily rising in the whole of England and Wales during the last ten years, the rate of 1896 being higher than it had been any year since 1876. In 1896 it was 15·8, the average of the ten previous years being 14·9. In London the same rise was observable; and the rate was also higher, being 18·0 in 1896, while the average of the ten previous years was 17·2.

It would not of course necessarily follow that the births were more numerous, as the birth rate might be decreasing at the same time, which in fact has been the case. Still, if the marriage rate rises in any year, we should rather expect to find the birth rate rising in the following year. Investigation of the rates of each year from 1847 to 1896 bears this out to some slight extent; in 24 cases the birth rate of one year increases or diminishes according as the marriage rate of the preceding year increases or diminishes; while in 23 cases no such influence is discernible. Taking the rates, however, in periods of 5 years, from 1841 to 1895, there are 7 cases in which the birth rate increases or diminishes in accordance with the marriage rate of the preceding period, and only 3 cases in which it does not.

There would therefore be some slight presumption that the births in 1897 would be rather more numerous than they were in 1896. (As a matter of fact, they were in London 215 less in 1897 than in 1896.)

(2) *The Birth Rate* (number of births per thousand of the population).—The birth rate has been steadily decreasing since 1876, the rate of 1896 being 29·7 in England and Wales and 30·2 in London, while the average of the ten preceding years was respectively 31·0 and 31·6. In 1897 the rate in London was the lowest on record, viz., 30·0.

The proportion of male to female births (counted as births of males to 1,000 females) has been decreasing since 1840 in the whole country. Taking periods of five years, it was at its highest, viz., 1,052, in 1841–5, and decreased steadily to 1,036 in 1886–90, remaining the same in 1891–5. The average of all the periods recorded is 1,042. In London, however, the proportion was 1,048 in 1896, whereas the average of the ten preceding years was the same as that in the whole country, viz., 1,036. But the proportion in London fell again in 1897.

(3) *The Death Rate* (number of deaths per 1,000 of the population).—The death rate has also been steadily decreasing on the whole since the period 1846–50, when it was 23·3. In 1896 it was 17·1 in England and Wales and 18·2 in London, while during the preceding 10 years it was respectively 18·8 and 19·9.

There is another general consideration with regard to all three rates, viz., their variation at different times of the year. The Registrar-General's Table giving the rates during the different quarters of the year since 1840 in periods of ten years shows that

(a) The Marriage Rate was always highest during the last quarter of the year and lowest in the first quarter.

(b) The Birth Rate was nearly always highest during the first quarter, though in 1891–6 it was slightly higher in the second quarter than in the first. It was always lowest in the last quarter.

(c) The Death Rate was always highest in the first quarter and lowest in the third.

Now, speaking generally, all these facts help to limit somewhat the ranges within which the numbers required to be guessed will probably lie. But they are really of very little practical use, because, in such

short periods as weeks, the numbers vary very much more in proportion than they do for longer periods.

The only practical plan, then, is to find the actual range of the variations that have occurred during a large number of weeks, and make enough guesses to cover all the possible combinations of numbers within the range,—or, better still, a little beyond it.

The following Table gives under each heading the highest and lowest numbers recorded in any week of the two years 1896 and 1897.

		Births.			Deaths.
		Total.	Male.	Female.	
In 1896.	Maximum	3041	1563	1478	2146
	Minimum	1829	904	925	1238
	Difference	1212	659	553	908
In 1897.	Maximum	2928	1519	1427	2360
	Minimum	2106	1059	1027	1158
	Difference	822	460	400	1202

It will be seen that in 1896 there is a greater range of variation in all the numbers of births, while the range of variation in deaths is greater in 1897.

With regard to the proportion of male to female births, the extreme limits in any week were as follows :—

In 1896, greatest excess of male over female births, 183, greatest excess of female over male, 52; total range of variation, 235. In 1897, greatest excess of male over female births, 131, greatest excess of female over male, 65; total range of variation, 196.

During the weeks of 1896, the female births exceeded the male in 6 cases; during 1897, in 7 cases.

These variations, however, are much more than covered by the differences between the maxima and minima in the Table. Supposing that the latter represent the greatest ranges that can occur, the chance of getting all the three numbers (male and female births and deaths) right in a single guess is one in 438,770,108 (the product of the three greatest differences, 659, 553 and 1204).

Since the successful competitor made only 252 guesses, it was extremely unlikely that he should have got one of them right; while the chance against some one of the total 100,000 guesses said to have been sent in being correct (assuming that the guesses were all different, which they probably were not) was about 5,000 to one.

These considerations seem to me to show that no more real skill can enter into such a competition than into the guessing of numbers in a lottery. If the ranges of the numbers in the lottery were known—

as they probably are in some cases—it would be easy to calculate how many guesses must be made to ensure one being right, and the most ignorant person would know that the more guesses he made, the greater would be his chance of getting one right. Similarly the previous statistics of births and deaths can only tell us what the general trend of things has been so far ; that is, they afford rational ground for expectation that the numbers will lie within certain limits, but no rational ground for expecting *any particular numbers* within these limits.

On the other hand, in the case cited by the Judge as a parallel one—that of guessing the name of the horse that will win in a race—the kind of knowledge from which *some particular inference* may reasonably be drawn is available, and there is consequently scope for skill. It appears to me, therefore, that the two cases are not really analogous.

SUPPLEMENT.

I.

THE FORMS OF APPARITIONS IN WEST AFRICA.

BY MARY H. KINGSLEY.

GENTLEMEN AND LADIES,—I have accepted with much hesitation the honour of addressing the Society for Psychical Research, for I know the object of this society is the determination of the truth regarding apparitions ; whether they have a separate existence apart from their human seers and so on. With this branch of the study of apparitions I have no concern ;—all I am interested in is the discovery of the ideas that the natives of West Africa have concerning apparitions ; what position they have in West African native religion ; and what the West African holds to be their significance and their characteristics.

Briefly, I entirely believe the West African sees apparitions, but I do not think this any evidence that the apparition has a separate existence. I have had an extensive and sad experience with people,—white and black, strong-minded and weak-minded, educated and uneducated,—suffering from delirium, which has taught me that men, under certain physical conditions, can and do see things that men in a normal state cannot and do not see. I also know that the African, in spite of all his hard-headed common sense, is endowed with a super-sensitive nervous organisation—he is always a step nearer delirium, in a medical sense, than an Englishman ; a disease that will by a rise of bodily temperature merely give an Englishman a headache will give an African delirium and its visions. Whether this indicates a difference in physical make between Africans and Englishmen, I do not know. It may be that the African, by his long process of acclimatisation to malarial districts, has a chronic state of malarial delirium ; or, on the other hand, it may be that his undoubtedly more sensitive nervous system enables him to see things the duller-nerved Englishman does not ; or, as one might say, the African mind may be a more perfect photographic plate on which the spirit world can print itself.

I do not know one way or the other ; all I am concerned in is that I should give you as clear and unbiassed an idea as lies in my power of the spirit world as an African sees it.

First, therefore, I would like to state what the spirit world is that the African sees and believes in. All the world is a spirit world to him—he has not, as many Europeans have, a feeling that there are two distinct worlds, a world of spirit and a world of matter ; neither has he the idea that there is certainly a world of matter, and there may be, or there may not be, a world of spirit ; the thing is one to him from the highest to the lowest. From the supreme Godhead to the pebble on the seashore, it is merely a matter of grade.

I beg you will allow me to detain you a few minutes with the statement of what any one man, woman, or child is to an African who has not been influenced by a Semitic Religion—Christianity or Mohammedanism. To the pure pagan African what we call a man is not an individual one soul,—he is a syndicate of souls, usually four, details of which I have given elsewhere. Well, this syndicate wear flesh and blood, and they wear clothes, require and like ornaments and food, exercise functions, and use tools, and so on, presided over by the soul syndicate. But the soul syndicate has other business to deal with besides that of these things it lives in and directly lives by ; it also deals with other soul syndicates called men, with other non-human souls of equal power to its own, with other souls of lower power to its own, and with other souls of greater power than its own, in grades of power right away up from dust to the Godhead who is above all things that are. You may notice I am dodging crediting the African with holding the idea of this over-god being the Creator. This interesting point is not yet worked out. I myself at present regard creation-myths, which abound in Africa, as being the resultants of European and Mahomedan teaching. The native African, as far as I know, has not the same feeling about time that we have ; he does not see why things should come to an end, or have a beginning ; he recognises that they change, but I do not think he would commit himself further. I cannot, however, detain you with these, to me, interesting points in African philosophy, but return to the position of the human soul—the syndicate of dream soul, shadow soul, bush soul, and surviving soul. This syndicate deals most intimately with those souls most nearly connected with its interests, but it knows of the souls beyond, upper and lower, in ultimate power. I do not think I should be going too far if I were to say you will find in West Africa no people who have not the conception of the idea of the supreme Godhead at the apex of things, and matter, the very lowest form of soul, at the other end of the scale ; or to put it in other words, you will find no people who have

the European conception of matter at all. There is to them no conception of the difference between these things, but an infinitely intricate conception of degree, and herein is the secret of the terror of Africans for what are to Europeans material objects. The thing is perfectly clear in itself, but it is complicated by the fact that an African can conceive a powerful spirit wearing a silk cotton tree, a rock, a fierce river rapid, or the everlasting, booming, man-killing surf itself, as a garment. The African sees the spirit inside that garment; he no more worships, or fears the garment than you would fear, or worship a great man's chimney-pot hat, or pocket handkerchief, or pen, and when he chooses to think the spirit that he fears or worships has cast these things off, has done with them, the African also has done with them, and if it is necessary to him, tries to find out the spirit he wants to get on with in its new clothes; with this difference, mind you, and it is an important one,—the African, regarding such things as clothes as being in a measure possessed of life and responsibility, is liable to think that the thing was cast off because it was bad and unsatisfactory to the spirit who once used it as a garment, and this explains a certain quantity—but not all,—of the cases in which an African smashes and vilifies his idol; the other cases in which he does this arise from his feeling the spirit itself has behaved unfairly to him, a fine instance of which has been given in Mr. Dennett's *Folk-Lore of the Fjortes*, published by the Folk-Lore Society—you will find it in the chapter on the Burial of the Fjorte. The far more common thing, however, is the absolute carelessness as to what becomes of the garment of the spirit, the thing that was the idol or the charm. The spirit has thrown it off, it can itself be thrown away, or left to rot, it does not matter; though it is not matter as you conceive it, it is in itself merely too low a grade of spirit to be worth troubling about. The African thinks no more of it than you think of a harmless insect, or any other inferior living thing. I apologise for detaining you on this point, but until you have grasped this position you cannot understand the African's mind, and its attitude towards apparitions; it is a position you cannot avoid grasping when you live among Africans who are unadulterated. The result of this point of view is intricate, complex, and so on, but it is never for one single moment confused.

I will now turn to the forms of apparitions of the various grades of spirits, and we will commence with those of the great over-god you find among all the West African peoples under varying names. This god has no organised worship given to him, he is regarded as an unmanageable being, who has no interest in human affairs in detail, but who now and then manifests an interest in, what one might call,

the higher politics ; he can interfere and save, or scourge a people, but it is no use for any gentleman or lady who has lost property, or got a disease, to appeal to him. As far as I know the Krumen call on this over-god of gods more frequently than other West Africans. When the surf on the shore of their country is very bad, and the men cannot get out fishing, they will go in numbers to the beach and talk to the great god about it ; most carefully explaining to him the whole of the circumstances of the case,—how the women and children want food, and food depends on fishing, and so on, just as if they were addressing a great judge who had no personal knowledge of human affairs, but who could be depended on to be just if you could only get him to be interested and to understand. Sometimes it takes a week to get the great god to understand, but he always does in the end, and the surf becomes passable, for a time at any rate. Also among the noble tribes of the Bight of Panavia you find a regular talking to the great god, and among them you find his apparition in some way connected with the moon ; at each new moon the chief of a village goes out and stands alone in the open and talks to Anyambie. He does not praise Anyambie ; he does not request him to interfere in human affairs ; he, the chief, feels competent to deal with them, but he does want Anyambie to attend to those spirits which he, the god, can control better than a man, and he always opens the address to the great god with a catalogue of his, the chief's, virtues, saying : " I am the father of my people ; I am a just man ; I deal well with all men," etc. The chief's account of his own virtues is naturally too long to detain you with here, and after this he winds up with a request that all spirits may be kept away from his village. At first hearing these catalogues of the chief's virtues used to strike me as comic, and I once said : " Why don't you get some one else to say that for you ; praising yourself in that barefaced way must be very trying to you." " Oh, no," said the chief, " and, besides, no man knows how good he is except himself," which is a common West Coast proverb. But by-and-bye—when I had been the silent spectator of several of these talks with the great god—the thing struck me as really very grand. There was the great man standing up alone, conscious of the weight of responsibility on him of the lives and happiness of his people, talking calmly, proudly, respectfully to the great god who he knew ruled the spirit world. It was like a great diplomat talking to another great diplomat of a foreign power, saying, " Let us keep our people from interfering with each other ;" there was no whining or begging in it, and as I have said, the grandeur of the thing charmed me. But you must not think I am one of the modern school who believe that great over-gods are always moral gods in a human sense, because the African over-god is not. I have often asked carefully, " Is he good ?" and I never found an African who had

not been in touch with mission influence say Nzambi, Nyankupong, or whatever other name he was called by, was good. He is a thing apart from the human moral code. No! they say firmly, he is not what you would call good; he lets things go too much, he cares about himself only, and I have heard him called "lazy too much, bad person for business," and a dozen things of that kind, and very rarely do you come across any one who speaks of an apparition of him. You will find in the *Folk-Lore of the Fjortes* an account of a man who went aloft and had a chat with him, but this is not a common tale, and, I think, shows mission influence. The most frequent form of apparition spoken of as appertaining to this great god is some tremendous elemental catastrophe: he rides upon the whirlwind and the pestilence.

I think I may say all the various names given to the over-god can bear the translation of the Awakener, the Lord of the sky. I think this great over-god is just merely a realisation of the fact that there is something outside and beyond the earth we know and the sky we see to the African mind. It seems that this thing can also include terrestrial things. I myself, therefore, completely accept Dr. Sneider's statements that the African is a monotheist; but a dissertation by me, the diffuse, on a German philosopher, also diffuse, is not a thing suited for an evening's address; it would be more of the nature of a Chinese play that takes a week or so to get acted, so I turn to the apparition of the gods below the over-god, but above the grade of power of man.

The apparitions of these gods are very frequent; you will hardly find a man or woman who does not know a man or woman who has seen them, while the priests belonging to their various cults are always in ready touch with them,—on terms of easy familiarity. Sometimes the god takes possession of the priest or priestess, talking through him or her, but always with a strange voice. This occurs when the priest has to arrange an affair between the god and the laity when the laity are present, but at other times the priest when he is out alone meets the god in a visible form, and you often hear a priest say, "I met—whatever the name of his god may be—this afternoon," in the most casual way, and when asked after the god's state of temper, the important thing about a deity to the laity, the priest will say, "Oh, pretty fair," or, "rather bad," or "much as usual," and will advise any one who wants to do business with him to seize the opportunity of the state of amiability, or to leave any little affair until a more hopeful time. Down below the Negro districts you very rarely find these gods with a regular priesthood; there they are dealt with by the headmen of the villages, and at Emfetta when one night there was a row on, a headman, one of the parties involved in the discussion,

roaring like a bull with rage, spun round and charged at a steep dwarf cliff to make a short cut to the residence of his village god above; when he was half way up, the bit of cliff came away with him, and flump on his back our friend landed on the beach below. There was a general grunt, and the man picked himself up and returned to the discussion in a more reasonable frame of mind, recognising it was no good expecting spiritual support that evening, and that he had only got himself to depend on. You will find in Ellis's works many descriptions of the form of these gods when they appear; you will find their apparitions are almost all of human shape, that their appearance is almost always gigantic and terrifying, and that some of them are males, some females; frequently they are married, and strangely enough, many of the worse gods are white.

There is a certain amount of reason in some of them being white. Fohsu is monstrous in size and whitish in colour, but that may be because of her association with salt making. That most horrible Aynfwa who lives at Moree, whose name signifies "not to be looked upon," and whose priests say she is covered with white hair like a goat, being the protectress of Albinos, also has a reason for being white, though why she should be hairy I do not know. But why Ihturi, the god of the rivers, Sirwi and Kakum, on the Gold Coast, should be white there seems no reason; any more than there is for Bons'ahm, the swallower of men and a marine god, being exceedingly black like Fuan Fuan-Fo, driver of men and war-god of Moree; while Aupah, the good person, who appears always with a casting net in his hands, being attached to the fishing industry, is brown in colour; while—to make this question of colour more difficult—Bobowissi, blower of clouds, chief god of the Southern Tshi, is as black as night, and Tando, chief god of the Northern Tshi—the Hater—the god to whom driver ants are sacred, is a sort of golden brown. There is, I am convinced, some strange stuff involved in this question of colour. I do not believe whiteness has anything to do with white men, for people who have no direct intercourse or experience of white men have white gods, and if you enquire if these white gods are like white men, you will be told, no, they are white like chalk, not like white flesh. You will also find a feeling of veneration for chalk, it is used in ceremonial decorations; a gentleman engaged in some particularly important fetish affair paints a ring with white chalk round one eye: when you have had a stroke of luck, people say, God has thrown chalk to you, and so forth. I should incline to the belief that this whiteness had a connection with the rarity of the colour in West Africa as a colour at large, and also with its occurring in connection with those two important things—so valuable and so dangerous respectively—namely, salt and the surf that unceasingly breaks on West Africa's seashore.

I will now leave this class of gods, whom Ellis has so admirably described in his works on the Ewe Tshi and Yoruba-speaking peoples of West Africa, merely remarking that they have a characteristic in common with their representative forms further south down the coast, and that is that it is a dreadful thing for any one but a priest belonging to them to see an apparition of any one of them. To the layman seeing one of these gods means death in the Bight of Benin, and in the Bight of Biafra it means disaster to the tribe to which the layman seer belongs.

I need not detain you here with details of the apparition form of that god and goddess to whom I have given most especial attention, Sasabonsum and his wife Srahmantin, for I have published observations on them already. These two deities have a great fascination for me, they are so widely diffused, so powerful, so unlike those local deities I have named, and they are equally unlike the great over-god and the lower range of spirits equal to and inferior to man. They are the rulers of the witchcraft world; hated by priests and respectable house fathers from one end of West Africa to the other, and feared exceedingly. No man dare openly brag of having had dealings with Sasabonsum; to see him is to die; to be touched by him is to rot; but if you are very brave and bad, and want a power from him, you risk this and go into the forest, try and gain from him a power, a *suhman*; if he grants your request for one, you do not see him, nor feel him, you hear him. If he refuses, there is a vacancy in your family circle, and it may be fragments of your remains are some day come across in the forest in a very scattered state; or merely a few rags of your garments are found sticking to the trunk of one of Sasabonsum's silk-cotton trees; and then your family and friends say that while you were going to see a sick friend, the awful thing grabbed hold of you and took you to his under-world home to suck your blood at his leisure (Sasabonsum never eats flesh) and your enemies say you were in that forest for no good, you were making witch, and there is a row, and actions for libel and trials by ordeal break up the peace of your town for months after.

I fear I have detained you too long already in my attempt to place before you the thing that in the African mind is behind the mere thing seen, this great, very-real-to-the-African spirit world. When people up here see a ghost, I find it is usually a ghost of a departed human being, or the vision of a Saint or Angel. Out there the majority of ghosts seen are not human at all; a very high percentage do not take human forms. One of the most important of the Fjorte gods is Nkala the Crab, who comes now and then from his kingdom under the sea; and lower in power, but very terrible, are the ghosts that are flying spears, ghosts that are paths leading men to destruction, ghosts that are blood; in fact, there is nothing we know that

cannot be to an African an apparition, the cloth of a spirit, and just as a learned African man can put on to his spirit the form of a leopard or a crocodile, so can a great spirit put on the form of man, beast, storm, pestilence or thing.

Tando the Hater comes among people he has a grudge against in the form of a weeping miserable orphan child, and woe to those who take that child in, for he gives them a disease that they give their neighbours, and all die of. Nzambi, of the Fjorte country, comes among people as a woman carrying a child, and begs for food or drink for it, and woe to those who refuse her. The contradictory conduct of these two gods is not isolated—many gods do that sort of thing—and it makes the human being very careful how he deals with strangers. There is an immense amount of very curious stuff connected with what one might call the natural history of African apparitions. I should much like to know whether any other apparitions are divided—like the West African ones—into those whose disappearances leave ash, or leave a common object like a fallen bough, or a pool of water, and those who leave no sign behind them at all. These apparitions that leave no ash are the most dreaded.

I fear I have neglected too much to-night the legitimate ghost, the human soul out of a body, but I have said all I can say on *Ensis* and *Aszahmanfo*; but tiresome as they are and on occasion dangerous, they are not to be compared in danger with the gods—the Imburi or local deities, or Sasabonsum. Human souls, if properly buried and looked after, are manageable beings, commonly called the well-disposed ones, but no one would dream of regarding a local deity as a person as easily managed as an ancestor; while a Sasabonsum with his temper well up is the most awful and unmanageable thing West Africa knows. But, besides apparitions of dead human beings, there are apparitions of live ones. You will hear many stories of people who have been belated at night on a journey, coming to a village, and after dark finding heaps of people in the village merry-making; and seeing that none of them wore clothes, knew they were witches, and, hiding themselves, have spent an awful night, for if the witches had found them out, they would have torn them to shreds. These stories are very cheery when you hear them at a bush fire in the heart of a forest, whose local geography you know little of in detail, and they are told in most dramatic convincing fashion, and the witches in the tale always keep on leaving off dancing and saying “I smell live flesh,” and go hunting round for that live flesh, which is you. It recalls one's early terrors of Fee-fo-fum vividly, and you wonder if you may not yourself some night stray into a witch town. Then you will hear story after story of people who have been alone on the path, women and children chiefly, who have been seized and carried away by the

bibendi and kept by them in their town, may be for years, or until they have had sense and opportunity to get a handful of red pepper and seizing a favourable opportunity, throw it in the faces of the guardians of the gate, and flee for it hard all back to real life. I came across a very pathetic affair connected with this at Adenolo'ongo: a couple had lost a child, it had strayed away and been no more seen, but they heard it sometimes crying at night, and they said "the bibendi have got it," and they used to go out and search for it, calling it by its pet names. There are, in fact, endless tales of the bibendi and their ways and their towns and their markets, whereunto by day live people sometimes unintentionally go, and buy at the market-place goods that vanish before they reach their own homes, or goods that turn into horrible useless things. I may remark for the benefit of any one who is going to the land where such things are, that if you get among spirits, never touch the food they offer you, or you will have to stay with them for a long time, and never give them your clothes in exchange for finer raiment, for if you do you will alarm your relations by coming in without a rag on you, and, as a man said to me, "One's wife never believes when one says one's cloth has been given to a bibendi man. She always thinks it is living women who have got it." And if you deliberately go to a spirit town to find out something from them, never drink water, or anything else, until you get home; if you do you will forget what the spirits told you. These bibendi are the souls of dead human beings, and Mr. Du Chaillu got himself more discredit up here than he need have by repeating the tales he had heard about the bibendi and their towns, and crediting them to gorillas,—I expect from a confusion in the native words.

Although throughout all West Africa there is, as Merolla says, "an exceeding plentie of Devils," there are some places where they are so numerous as to make the place almost uninhabitable to the human being, used as he is to their goings on and manners and customs. Of course I, being out there for the purpose of studying Fetish, have always had, when I heard of such a region on good authority, to go and potter quietly into it. I am not a person who would violate any man's temple or sanctuary, but miscellaneous devils do not count in that category; the greatest difficulty you experience in getting into these devil haunts is getting respectable people to go with you. This, however, you can do without, provided there is even a small resident human population therein. Your friends among the surrounding tribes will, it is true, tell you the men who live in such places are little better than the devils, but that may be just prejudice; at any rate my personal experience among the human residents in the notorious devil regions of the Sierra Del

Crystal and the Ourounougou swamp has been exceedingly pleasant and friendly, and although I, according to local opinion, have suffered grave inconvenience from the devil population upsetting my canoe, making me lose my way, and on one awful occasion grabbing a box of most valuable fishes, that by natural laws ought to have floated on the water after an upset in a swamp but did not,—these are not things to discourage an enthusiastic observer of the ways of devils, but rather stimulating than otherwise. Well, the strange thing about these particular devil haunts is that the people who live in them hold the same opinion about their spirit co-residents that the outside world does, and they simply glory in the richness in horrors of their homes ; nothing, indeed, pleases a Nkama or Ourounougou man more than when on one of his visits to a town on the border of his region, he succeeds in making the flesh crawl of every man and woman in it. Round among the villages by Lamberenie, a little experience soon taught one that a night of excursions, and alarms, and shrieks, meant that one of those swamp men had been up telling tales and disorganising the nervous systems of the respectable Ajumba and Igalwa population, and when I was down in the swamp myself staying with the swamp men, the local conversation was just as alarming and lurid, but being used to that sort of thing it did not upset people so much. I well remember, the first night I spent at one of their little islands, asking why there were no window holes in their houses like there were in the Ajumba and Igalwa houses ; and being told quietly that the fewer holes a house had down there the better, because things looked in through them. Then followed a catalogue of things that had been seen through holes : one large eye occupying a hole all night, a horrid but complete face, a red-hot plate of metal (a neptune, as those plates are called), a hand hanging out the sill that, when you cut at it with a knife, bled and bled but did not go away, and had no body belonging to it, and so on ; and I gathered that, when my friends came down to occupy these houses on the islands in the swamp for the fishing season, they had for the first week, until things were tidied up, a very lively time of it, and I am bound to say that when things were tidied up and quite all right, from my host's point of view, the time, to one not brought up to that sort of thing, remained lively. I do not think I have ever seen a weirder scene anywhere than I saw nightly at full moon from the door of my perched up little pigeon-house on one of those low clay islands named Njuki ; there was clear water right round it, some 200 yards across, and then rose up the black forest wall a hundred feet high, so dense that neither sun nor moon could illuminate it. The black shadow of the forest wall would cloak the water and be withdrawn off it as the moon rays struck it, like drawing a velvet

cloth off a bright silver table ; and in moonlight and in darkness shone the light of the fireflies ; while the beach below and up under the house was in the possession of crocodiles, squatting about as gracefully as swans upon a gravel walk, quite different things to the sleepy log-like things crocodiles are by day, save that they stank just as bad. But they were now in their clumsy way active and noisy, giving their quaint bark that ends in a moaning whine, and evidently generally enjoying themselves, and making you understand why the natives went to bed early, and built their houses on the tops of poles, but poles not quite high enough or strong enough, to my way of thinking. Now and again the great black mass of a hippo would raise itself out of the water, and come ashore and have a look round, or just give a great sougning grunt of satisfaction and sink again, and during the early hours of the night there would be strange wandering wefts of silver and white mist that trod the water solemnly, gracefully, and now and then waltzed together into one mass that would sink into the lake, or stroll away and stand showing white in the darkness, or glide away among the tall columns of the forest wall ; it was a scene to make one believe in ghosts, and see them, if you could see a ghost at all. I cannot,—never have,—but my African friends can and do to any extent, in tamer surroundings than the Ourounougou country, which I have never dared attempt to describe, which haunts me and calls me more than any other thing I have ever seen. Then there is another region I love well, but where all spirits are not bad,—though most of them are,—the region of the rapids of the river Ogowe ; there from Boue and Lope down to Otaia Amagonga are the fearful spirits of the rapids and the Okanda gorge, ever quarrelling with each other, and with the rocks and with the whirlpools in between ; ever ready to kill the man who comes near them ; but there are other spirits there who are kinder to him and who call him off from danger, and these are the singing sands of Okanda. There are, as I have elsewhere described, beautiful sand-banks of white sand, that owing to the fall of temperature at night, emit a long faint musical note, which you can hear through the noisy squabble and roar of the rapids, if you keep your head bowed low near the surface of the water as you toil up or flash down the Okanda in a canoe at night, and you know that there is slack water where the sound comes from, and keep into it. Naturally the Adooma canoe men who work the regions of the rapids feel a gratitude to these spirits and give them things, and I confess I got used to giving them things when with the native canoe men, and use grows on one there, and I had to pull myself up one night and say to myself, “Look here, you don’t believe in these things,” for coming down alone that night I found myself flinging tobacco to the spirits of the singing sands. This passive growing into a belief in fetish is a thing liable to

happen to one when one lives among such scenes and surrounded by people who so simply and so thoroughly do believe in fetish. and you will hardly find a white man who has lived long alone in West Africa who does not think there may be something in fetish. I fell more rapidly into the fetish line of thought than a trader or official would. because my mind was carefully kept swept of preconceived notions, and I was profoundly interested in the thing and anxious to follow it through all its varied twinings and turnings. A little practice, a great deal of patience and sympathy with the African, which he completely won from me by his kindness to me, soon taught me to think fetish, and when you once think fetish, it is so easy,—the outside world and circumstance are so readily explained by it,—that you find it difficult—it requires an effort—to think in any other way. You all know up here people who have a great knowledge of French? Well, they always, when there is anything particular to be said, use a French phrase. I use a fetish phrase—that is all.

You will often hear this religion of fetish called a religion of terror and painted black with crimson patches. Well, facts are facts; find me a more cheerful set of human beings in this wide world than the West Africans who believe in fetish; find me a region where crime for private greed is so rare as in West Africa, and then, and not till then, will I say fetish is a horrible thing. I will grant you there is human sacrifice under it from Sierra Leone to the Niger; I will grant you there is a sending down with the dead of their wives, slaves, and friends; I will grant you it kills witches, that it produces cannibalism in this region; but before you write down the men who do these things as fiends, I ask you to read any respectable book on European history, to face the Inquisition and the fires of Smithfield, and then to go and read your London Sunday newspapers. West Africa could not keep a Sunday newspaper going in crimes between man and man; its crimes are those arising from a simple direct absolute belief in a religion. From no region that I know can so truly go up the sad cry to God, *Doch, alles was dazu mich trieb, Gott, war so gut! ach! war so lieb!* as from West Africa. Down below the Niger you strike two schools of fetish who do not care so much for religion in a religious sense. I do not care so much for the Bantu negro, and the true Bantu you meet below Cameroons, as for the true Negro. I like them, but the Negro is an infinitely finer man, physically and morally, than the Bantu; so the mere fact of the Bantu doing little or nothing in the way of human sacrifice or sending-down killing, does not make me prefer him to the Negro. But to both alike everything is an apparition, and what matters about an apparition is its grade of power.

II.

A FEW NOTES ON OCCULTISM IN WEST AFRICA.

BY J. SHEPLEY PART, M.D.

Late Assistant Colonial to the Gold Coast Colony.

When I first went to Africa, few men probably were more sceptical on such subjects as clairvoyance, apparitions and the so-called supernatural generally; I had of course heard the popular smatterings of these things and when I did, had, in the usual style, put all down to fiction, over-excited brain, suggestion or auto-suggestion and the like. I hope to advance in what follows some evidence that phenomena do at times occur that are not explainable by ordinary scientific methods and that certain men can avail themselves of forces which are beyond the ken of the ordinary individual. It is certain that when I was first brought into contact with these things I was incredulous, and, as a consequence, put them on one side for a considerable time as not worth investigation—much to my subsequent regret.

I propose to roughly divide the paper into divisions, as stories I have heard and facts observed, but under the former heading I only propose to include those stories which, unless otherwise stated, I believe to be worthy of credence from personal knowledge of the men who told them.

I do not intend always to mention my authorities by name, as in some cases I am under obligations not to do so, and in others it would possibly be distasteful to them did I do so, and, owing to the short time at my disposal, I am unable to ask for their permission.

There are many tales told by natives of the haunting of roads by the spirits of the deceased, but of these I do not purpose to treat, not having had means of investigating; but when travelling in the interior I have repeatedly been told of what the English-speaking native of the uneducated classes calls, for want probably of a better word, ghosts, *i.e.*, of messengers having been seen on the approaches to villages and so on, and we are told that these "ghosts" are men who go out to keep a look out on the road to give timely warning of the approach of strangers. This matter will be dealt with later on in more detail.

In cemeteries the presence of apparitions is looked upon by the native as the usual thing, and these are said to be the spirits of the departed buried there. They make their appearance as usual at night and take various forms, sometimes appearing as replicas of their once living selves, either material or in ghostly form—more often in an undefined (more or less) ghostly or cloudy condition, luminous or otherwise, as the case may be.

These forms are looked upon with fear by the ordinary people and it is considered dangerous to come into personal conflict with them at times—this idea leading occasionally to blackmailing by personators; it is also

thought to be very wrong to grapple with a genuine ghost, as it may probably injure him (or it) greatly, though, as to why, I have not been able to gain any enlightenment. These stories of graveyards I have quoted in outline here as showing the general drift of ideas on the subject, and certainly I have had luminous appearances in the form of an apparent nebulous mass of light pointed out to me in these situations as being what are described as ghosts of the dead.

Many of the old castles and ports teem with accounts of ghosts of old officials haunting them at times; of these I propose to give one or two in outline as given to me by men whom I believe to be absolutely trustworthy, Englishmen holding administrative and judicial appointments.

At one port is a very fine old castle, built, I believe, by the Portuguese about the seventeenth century, still used as temporary quarters for officials travelling through or visiting the place. On one occasion a district commissioner went to the place on official business and arrived after dark. He proceeded to change and during this time his boys prepared the table for dinner in the hall, and on his going out of his room to have a walk round, his boy met him and said, "a white man come for chop, sar, I tell cook get chop for two." My informant said this was nonsense—where was the white man? and went into the hall, on which the boys said "he sit for table" in the commissioner's place, and pointed to the head of the table, apparently astonished that my friend did not see him, but they stuck to it he was there, and, on being asked what he was like, described a man with a pointed grey beard and moustache, dressed in Spanish or Portuguese style of the last century. My friend said he could see nothing whatever, but he was quite satisfied that several of his boys did see the man and they were unanimous in their descriptions of him. Furthermore they refused to stop in the castle at night, and preferred to camp out where they could.

The gentleman who told me this I knew well, and am aware that he was a thoroughly hard-headed reliable man, who was not given to romancing, and as entirely free from superstition as any one I ever met. Of the facts I am satisfied—explanations I must leave to others.

While passing through one of the outposts, I was informed of an extraordinary case of murder. The crime was seen to be committed by competent and credible witnesses at a place some 80 miles from the coast—some five days' marching—but equally credible witnesses, including white evidence, vouched for the man's presence on the coast on the same day and at the time of the murder. The interest in this case lies in the fact that the accused was a known witch doctor (not fetich man) of no mean ability.

With regard to the transmission of intelligence by occult means, the matter is treated by the better classes of natives as everyday knowledge, the medicine men occasionally being asked to obtain or transmit information for various purposes.

I have repeatedly been told by well-educated and broad-minded natives (and such do exist) that it is possible for certain trained individuals to "project their consciousness" to a distance irrespective of time or distance, and to do so while retaining a continuity of consciousness with that in their ordinary condition. We also hear the same fact stated in this way: That the individual has the power to go to any place without regard to time or

distance, etc., and it is not an uncommon remark to hear that so-and-so had been to such-and-such a place "during the night," or "yesterday afternoon," or "this morning," such journey being out of all possibility by ordinary means.

In respect to the transmission of articles, such as letters, from place to place, the accounts are fewer, but more precise, if anything, than those of the mere transmission of intelligence or the obtaining of information by personal observation of the seer; but very few seem to be possessed of this power, and only a few seemed to me to be acquainted with its existence, and it was always spoken of as being rare; but on one occasion a witch doctor undertook to do this for me, but, owing to peremptory orders to change my station at short notice, I was unable to put this to the test.

I feel that I must depart from my general rule and mention one name among my informants, that of the late Mr. Ferguson, who was murdered by Samory's (or Samoli's) men at Wa, in Dagati, whilst engaged as a member of the expedition under Lieut. Henderson, R.N., in 1897, to which party I had also the honour to be attached. I do this in consequence of Miss Kingsley's having already quoted him at the meeting of the Society on the 10th inst., when I had the pleasure to be a guest. Miss Kingsley stated then that Mr. Ferguson *claimed* to have the power of projecting his intelligence (I use this as a convenient and non-committing phrase) to a distance and making personal observations. Now I know that he did make this claim, and I propose to relate an incident here which goes far in my mind to substantiate it. When we had "sat down" at Wa for some time, and were anxiously waiting for reinforcements and renewed supplies, we were exercised much in our mind as to what had become of the relief column, as it was considerably overdue, and Mr. Ferguson undertook to try and find out its whereabouts. He did so, and reported that he had discovered an armed party travelling from East to West through a certain district which had better be nameless. He said that this party had but one white man with it, whereas we knew that the one we expected should have had three. He also said that he did not know them, although he was acquainted with two of the expected officers, neither could he recognise the uniforms of the men. Now the point here is that the place where this party was reported was many miles east from the route to be followed by our relief party, and their direction of march was at right angles to what our party would have followed. Further, there was no column of which we had been advised anywhere near this place. The sequel appeared upwards of two months after this, subsequent to the evacuation of Wa, when we had joined a second British column under Captain Donald Stuart, then in command of the Hinterland forces. On our leaving him to proceed to the coast, he gave instructions to us to enquire at a certain town into the truth of a native *rumour* which had *just* reached him of a party belonging to another nationality having passed from East to West through that town about two and a-half months previously. It can easily be imagined how interested I was thereat.

When we reached the town named in our orders we found that undoubtedly their party had passed as described, and answered in every particular to Mr. Ferguson's description given three months before, and at a distance of some 130 miles.

Now this incident occurred under circumstances, as it happened, that precluded any possibility of personal communication with Mr. Ferguson by any one without my knowledge, as the whole time the whole of Mr. Ferguson's tent was in my view, backed by a high swish wall. There was no transmission by water, as he had no stream there, and drums are not used by the Soudanese tribes in the West to anything like the extent they are by the pure negro tribes on the coast belt, and, in addition, Mr. Ferguson was not a drummer or a king's linguist. They are hereditary posts, and indeed his native rank was superior to them, high as they stand.

On the way up to the Hinterland, shortly before leaving the forest country, we were benighted one night, and our guide did not know how far we were from the town to which we were bound. We had been marching for some hours in the dark along one of the forest roads when I and others saw what we took to be a lantern in the thick bush. It was peculiar, as it moved as fast as we did. It presently came out on the track, and an attempt was made to capture the bearer, which failed, and Mr. Ferguson said we had better leave it alone. The impression that I retain of it is as a focus of light throwing a circle of light round it, much as an ordinary stable lantern would, but afterwards I could not say that I actually saw the wires of the lantern, but that was the general impression.

As we approached, the light eluded us, and then followed the path ahead of us for some few miles, and then disappeared, just outside the town we were approaching; it moved exactly as if carried by a man. The explanation given to me was that this was the "double" (or "Ka" of the ancient Egyptians) sent out along the route to act as our guide.

Towards the end of the last Ashantee expedition I happened to be stationed on the coast at the termination of the *shortest* route from Kumassi to the coast. A day *before* the expected entry of the expedition into Kumassi, I was informed by my boy that the Governor had entered the town at noon (this was about 1.30). About an hour later I was told the same thing in the town by an old chief, an educated man, who, when I laughed at it, remarked that native means of communication were much more rapid than ours.

I may mention that the upper portion of the line was in charge of the Royal Engineers, and the coast lines under reserve for Government wires only. The news was confirmed the following evening by official wire. The head of the military wire was some 30 to 36 hours' distance from Kumassi, so this excludes irresponsible chattering by the operators. As to runners, we were five days at the *shortest* for special runners from the front. The route is entirely through forest country, and, in regard to water transit of sound from drums, there is no direct access to the coast from Kumassi by water.

With regard to the means by which these phenomena are obtained—these are secret, and their professors are members of a secret society; but I was informed on good authority that the process gone through to obtain the power of clairvoyance as exemplified above is purely physical, and requires—when the means have been taught—but constant practice to bring the several stages to perfection, but that special means are used in each stage.

initiation being necessary to each stage of development and then only on approval of the chiefs of the order, which is very stringently guarded. Only a very few are initiated into the highest development.

The stages may be divided as under :

1. Simple Clairvoyance.
2. The paying of "Astral visits" or projection of the consciousness only.
3. The same as 2, with power to materialise the entity projected or (which I am not certain) to transport the body itself and to affect material objects.

Mr. Ferguson's name having been mentioned in public in connection with this subject, I should like in justice to him to say something as to his character as I knew it.

I found him to be a most modest and retiring man with regard to those subjects, and *not* inclined to talk about even the possession of the power of clairvoyance, which he treated as a great honour to have, and as a sacred trust not to be reposed in any one who came along. He was a well educated man and a most competent surveyor and mathematician, as can be verified by the honours obtained for work done in the field from the R.G.S. I may add that I have myself the utmost confidence in what he said to me on the subject of this paper, and I have not trespassed on any confidence shown me, as I have confined myself to those points which are common knowledge among many on the coast, and which Mr. Ferguson was willing to confirm.

In conclusion I can only reiterate that, as to ways and means in detail, I am in the dark, and should be pleased to correspond with any one in the position to enlighten me.

As to any criticism not dealt with in the paper, I shall be glad to give any further information in my power to any one addressing me through the Editor, who has my address.

III.

PSYCHOLOGY AND PSYCHICAL RESEARCH :

*A Reply to Professor Münsterberg.*¹

BY F. C. S. SCHILLER.

Boss locutus est: Professor Hugo Münsterberg, of Harvard University, the lord of I don't know how many thousand dollars' worth of psychological machinery, has planted the banner of the 'only genuine' psychology amid the pulverised fragments of 'Mysticism,' and sent back 'the Cinderella of the sciences,' Psychical Research, to supervise what alone she is fit for, viz., the culinary operations of the witches' caldron. And yet, perhaps, in a critical age, such *ex cathedra* pronouncements, even of the greatest scientific dignitaries, are not as safe nor as effective as the lofty and scornful silence affected by most of his *confrères* on the General Staff of the Army of Science. The Goliath of Authority cannot stalk forth into the field of debate without a risk that a little pellet of reason may pierce through the thickness of his skull and put an end to his pretensions. For too often the effect of authority is impaired by argument, and the impressiveness of a judgment is destroyed by divulging the reasons on which it was based. Professor Münsterberg should have remembered Lord Mansfield's advice to the man who undertook the job of judging what he did not understand, or, as one must say in Professor Münsterberg's case, what he *would* not understand. For Professor Münsterberg's inability to grasp the nature of the case for Psychical Research is manifestly of an emotional rather than of an intellectual character, and affords as fine an example of the effect on the mind of a passionate 'will to disbelieve' as I have had the pleasure of meeting. I am accordingly confident of expressing only the sentiment of every psychical researcher when I thank Professor Münsterberg for the interesting light he has (however inadvertently) thrown on the psychology of psychologists, and the nature of the fixed ideas by which they seem to be obsessed.

And, personally, I owe Professor Münsterberg a debt of gratitude also on account of the undesigned, exquisite, and almost ideal illustration which his remarks yield of the aptness of a comparison I ventured to institute in reviewing Mr. Podmore's book in the pages of *Mind*,² when I called 'psychical' phenomena "the Dreyfus Case of Science." I then pointed out that they had never received a fair and open trial, *coram populo*, that

¹ Cf. his article on "Psychology and Mysticism" in the *Atlantic Monthly*, January, 1899.

² For January, 1899. No. 29, N.S.

the evidence on which they had been condemned had never been published, and could not be produced, that their banishment from the society of scientific fact and their relegation to the company of the Devil had been effected by a secret and nameless court-martial, which made no serious pretence of examining the evidence, and that for nearly two centuries the authorities who professed to speak in the name of Science had, when questioned, done nothing but invoke the sanctity of the *chose jugée*, and intimidate inquirers with solemn prophecies of the absolute ruin that would overtake the whole scientific order if any investigation or revision of the matter were to be attempted.¹

I also drew attention to the fact that, in spite of all discouragements and threats, a demand for 'revision' had grown up, which was supported by an increasing number of 'intellectuals,' who were not afraid of being maligned as the hirelings of "a conspiring syndicate of all the superstitions." But I could not anticipate that Professor Münsterberg would simultaneously have been goaded into divulging the contents of the secret *dossier*, of which we have heard so much and seen so little, that he would have exhibited to an astonished *urbi et orbi* the ridiculous documents on which he relies to substantiate his case. In short, it was *εὐχάϊς ὁμοίων* that among the Anti-Dreyfusards of Science there should so soon be found a champion to emulate the career of M. Cavaignac! Once more, therefore, let me express my gratitude for the service he has done to the cause of Psychical Research.

Nevertheless, gratitude must not prevent us from exposing the extraordinary nature of the misrepresentations and misconceptions to which he has attempted to give currency, from drawing attention to the weird character of the arguments whereby he seeks to appease his intellectual conscience, in his hurried return to the shelter of the ancient prejudices from which he ought never to have emerged.

I.

Professor Münsterberg does well to open his article with a definition of his subject: but, unfortunately, this definition would be scouted as ridiculous by every psychical researcher. He defines 'mysticism'² as "the belief in supernatural connections in the physical and psychical worlds." Assuredly he did not discover this, or anything like it, in the *Proceedings* of the Society for Psychical Research, which he professes to have included among the 100 volumes he claims to have read—(p. 78)—with more speed, apparently, than care. In fact, if he had paid the most ordinary attention

¹ Professor Münsterberg, of course, trots out this old bugaboo also. Courageous confidence in the ability of science to deal with every order of fact—with "the psychology of spirits, angels, and demons, if such things there be, as well as with that of men and beasts" (as I said in *Mind*), he declares to be "*wrong and dangerous from beginning to end*" (p. 76). Can it be that he has a lurking fear that if he attempted to investigate demons, they might fly away with him?

² Which term, by the way, he uses in a wholly popular and unphilosophic fashion, in order to include under a common name widely divergent attitudes towards these matters.

to the continually reiterated 'declared objects of the Society,' he could not but have observed that no more grotesquely inappropriate travesty of its object and method could possibly have been devised. The notion that any phenomenon in God's world could be meant to be *supernatural*, i.e., put there to provoke and to baffle inquiry, is one of the very worst of the superstitions which the Society for Psychical Research set out to slay. Its fundamental postulate was that no *fact* could possibly be supernatural, that however anomalous it might appear, it must yet admit of investigation by the appropriate methods, and that to declare it supernatural was simply an obscure and offensive way of declaring one's *unwillingness* to have it investigated. From the psychical researcher's point of view, therefore, Professor Münsterberg is himself a supernaturalist of the worst type, little better than the advocate of the Satanical explanation. The very term "supernatural" is eschewed by the Society for Psychical Research, and ever since its foundation it has striven to supplant the 'supernatural' by the 'supernormal,' aiming at the momentous consequence of substituting something which courts and challenges, for something which discourages and defies, inquiry. Either, therefore, Professor Münsterberg has wholly mistaken the purpose of the psychical researchers *ab initio*, or he has grossly libelled them in the rest of his article by representing them as 'mystics' and advocates of a 'supernatural' view of the alleged phenomena. Among the horns of this dilemma I must leave him to choose the one on which to impale himself, and would ask him only to look up S.P.R. *Proceedings*, No. 1, p. 4.¹

It is because of this initial misrepresentation of the aims of the Society that he seems throughout incapable of conceiving any other motive for taking an interest in anomalous phenomena than a morbid love of the marvellous *qua* unintelligible, or, as he would call it, 'supernatural.' That it is possible to contemplate such matters in the spirit of an explorer, of a C.O.S. worker, or of a *detective* (those who are familiar with the practical working of the Society and have suffered at its hands, indeed, have often complained of the excess to which it had carried this last spirit), that it is possible to be animated by a desire to extend the sway of scientific method over unconquered ground, and to rejoice rather than repine when the new facts have been connected with the old principles (which, however, they often transform), that it is possible to take up the subject from a keen sense of the scientific scandal and social dangers perpetuated by abandoning it to the vagaries of superstition, in short, from a self-sacrificing willingness to "take up the white man's burden"—all these are alternative motives which Professor Münsterberg is sedulous to ignore. Yet he might have found one

¹ Perhaps, however, in view of the difficulty he seems to find in grasping the point of the literature he reads with such rapidity, I had better quote for him an extract from the inaugural circular: "The aim of the Society will be to approach these various problems without prejudice or prepossession of any kind, and in the same spirit of exact and unimpassioned inquiry which has enabled Science to solve so many problems once not less obscure nor less hotly debated. The founders of the Society fully recognise the exceptional difficulties which surround this branch of research; but they nevertheless hope that, by patient and systematic effort, some results of permanent value may be attained."

or other of them expressed on almost every page of every presidential exhortation ever delivered unto the members of the S.P.R. !

But not only is Professor Münsterberg's definition of mysticism inapplicable to Psychological Research, it is also liable to grave exception in itself. Is it possible, *e.g.*, to put an intelligible interpretation on the phrase 'supernatural connection'? *Prima facie* it seems a contradiction in terms; for to Professor Münsterberg the 'supernatural' is that which is *not* connected with nature. If it is connected in any stable and calculable fashion, it *ipso facto* becomes a fit subject for scientific investigation, *i.e.*, becomes *natural*. If, therefore, Professor Münsterberg had thought his definition sound, he might have contented himself (and his readers) by pointing out that the conception of 'mysticism' was self-contradictory, by inferring '*ergo cadit questio*' and stopping 'right there.'

Again, a dangerous ambiguity seems to lurk in the phrase about the connections being '*in*' the physical and psychical worlds. As a philosopher who ventures to soar to the metaphysical heights he reaches later, Professor Münsterberg must of course know quite well that these worlds are not separate universes, but *aspects* of the world (of experience), or rather, the *results of special methods* of handling our experience. Hence, in one sense, any fact will be 'out of,' *e.g.*, the physical world, when it does not lend itself to treatment by the established methods of physics, and so is ignored by them. But of course it goes on existing just the same, and may receive recognition in another science or by other methods. That is, it will remain '*in*' the physical world in a wider sense, and so long as it coheres with other facts (or aspects of facts) it must be capable of scientific exploration, and the only question will be as to the proper method of investigating. The only sin against the Holy Ghost of Science or, less metaphorically, against the fundamental assumption of all science, which it is possible to commit, is the assertion that *any* fact can be incapable of being connected with others. This offence is far from the thought of the psychical researcher, but by implication Professor Münsterberg commits it whenever he argues against the recognition of any fact on the ground that it is 'supernatural,' and allows his fears to dictate his arguments.

Having started from a vicious definition, Professor Münsterberg is forced to justify it by ascribing to his 'mystics' an utterly anti-scientific attitude. A lay observer is not allowed to use the teleological language of ordinary life in expressing the connection of phenomena as they appeared to him, without having his words twisted into a denial of the possibility of a scientific connection of events. Now, of course, Professor Münsterberg may nourish whatever metaphysical prejudices he pleases¹ against the teleological explanation of things, but there are two things which they do not entitle him to do: (1) They do not entitle him to treat a teleological ordering of events as no order at all—that is merely begging the question, and brings him into conflict with all the religions as well as with 'mysticism'; and (2) they do not entitle him to overlook the fact that in many sciences the

¹ His argument on p. 83 amounts to an assertion that a final cause is no cause—a doctrine open to much objection philosophically, and only apparently borne out by the present practice of the natural sciences.

teleological explanation often yields the clue which leads on to the discovery of the mechanism whereby the effect is mediated. Thus, in biology, the purpose of an organ is often far more easily apparent than the mechanism by which it functions. Similarly, if departed spirits did communicate, we might detect *why* long before we discovered *how* they did so. To declare, then, one's belief that certain events are purposive does not in the least imply that they are not also mechanical, according as we contemplate them in one way or in another.

II.

I do not know whether the true mystic, if such there be, would resent Professor Münsterberg's account of his mental attitude as a distortion, or, as is more probable, would remain profoundly indifferent to anything that might be urged by so unsympathetic a critic. But I do know that Professor Münsterberg's distortions of his opponents' views must not be acquiesced in when they affect people who do aim at precision in the use of their technical words. His caricatures are not even correctly drawn, and are hardly recognisable. Thus he does not scruple freely to use the technical term which the S.P.R. has introduced in its infancy, and has clung to ever since, viz., *Telepathy*. But this is how he defines it (p. 68): "To perceive in an *incomprehensible* way the ideas and thoughts of others." On the other hand, the Society has unvaryingly defined its meaning as "the transmission of thoughts and feelings from one mind to another by no *recognised* channel of sense."¹ It seems impossible to ascribe to Professor Münsterberg either such linguistic ignorance or such logical incapacity as would lead to the substitution, without a suspicion of the scope of the change, of "incomprehensible" for "uncomprehended." By what psychological processes, then, did he extract his interpretation from the literature which he claims to have examined?

At any rate, a candid critic could not but have observed that the official definition of telepathy in no wise excluded transmission either through as yet unrecognised sensory channels or through a supernormal intensifying of the recognised channels; *i.e.*, telepathy includes both hyperæsthesia and physical hypotheses of the 'brain wave' type, and it is simply one of the many delusions with which the subject seems to suppose that the recently alleged extensions of hyperæsthesia constitute a refutation of the telepathic hypothesis. They simply show how completely right the S.P.R. was 15 years ago in holding that the bounds of science had to be enlarged to take in the new facts. On the other hand, the suggestion that telepathic transmission is incomprehensible is a request that the S.P.R. should gratuitously commit suicide.

Not but what a strong criticism might be directed against the telepathic explanation on grounds other than those Professor Münsterberg saw fit

¹ "All impressions received at a distance without the *normal* operation of the *recognised* sense organs" are telepathic. S.P.R. *Proceedings*, I., p. 147, *cf.* II. pp. 44, 117, *et passim*. Mr. Podmore, whom Professor Münsterberg holds up to scientific execration, explicitly states that, "though there are grounds sufficient to justify telepathy as a *working hypothesis*, the proof of its transcendental nature is still wanting." *Studies in Psychological Research*, 1897, p. 8.

to adopt. For telepathy is, after all, not a positive explanation but a negation, arrayed in the garb of an affirmation, and crying aloud to gods and men for a more positive definition. Hence the effect that it seems to triumph to its own loss, that in proportion as the facts for which it obtained recognition find positive grounds of explanation and enrich the sphere of science, its own foundations seem to be withdrawn. But this would be distressing only if the psychical researcher were, as Professor Münsterberg insinuates, interested in the growth, not of knowledge, but of mystery.

And if the S.P.R. be accused of having been somewhat slow in developing the positive content of 'telepathy,' it may fairly urge in defence that its workers were few, and that other needs were more pressing. The main use of the conception of telepathy was to assist in the analysis of the far more anomalous phenomena of spiritism and ghost-seeing. Of the consequence that the proof of spirit-life, spirit identity and spirit agency was thereby rendered indefinitely more difficult, Professor Münsterberg betrays not the least suspicion. He treats telepathy throughout as but the most modern avenue to the Hall of Illusion. And yet, if he had only seen it, telepathy would have served his purpose far better if he had not abused it, but used it, like, *e.g.*, Mr. Podmore, to arrest the premature flights of superstition. Hence the ingenious author of the *Studies in Psychical Research* figures only as "the most ardent believer in telepathy" (p. 77), anxious to credit needless marvels. Just as if his perhaps somewhat hypercritical studies did not embody incomparably the most successful attempt to take a sceptical view of the phenomena as a whole, as if they did not strike a far more telling blow at superstition than the collective efforts of all the *a priori* scoffers from the days of Hume onwards!

It is clear, then, that Professor Münsterberg has grossly misrepresented both the aims and methods of the S.P.R., and the character of the explanations which it has, quite provisionally, suggested. In other words, the secret *dossier* which he reveals does not refer to the S.P.R. "*Ce canaille de D.*" is not Dreyfus, and whoever reads it thus reads it wrong, and knows it. Professor Münsterberg owes the Society an apology, and in view of the extent of his obligations to its critical work (from which his explanations derive all the real force they possess), poetic justice would seem to demand that he should at least contribute to its funds the ill-gotten gains of his *Atlantic Monthly* article!

III.

Strictly speaking, I might end here—after vindicating the integrity of the only approximation to a Cour de Cassation which exists in this analogue of the Dreyfus Case against the irresponsible attack of a would-be Quesnay de Beaurepaire. But I cannot resist the temptation of commenting still further on some of the many delectable passages in Professor Münsterberg's article.

Professor Münsterberg has a pretty knack of enunciating sonorous maxims which, upon examination, turn out either not to be relevant or not to be intelligible. Let us take, for instance, the impressive dictum which tells us (p. 69) that "the psychologist insists that every perception

of occurrences outside of one's own body, and every influence beyond one's own organism, must be intermediated by an uninterrupted chain of physical processes." Surely Professor Münsterberg need feel no anxiety lest this should seem "an apparently arbitrary decision"—for no requirement can be more easily complied with. Does not the continuity of Space mediate between all processes that occur in the perceptual universe and would not the weirdest miracle that a theological imagination ever concocted be accompanied by "an uninterrupted chain of physical processes"? And, contrariwise, would not the laxness of the maxim open wide the door to the wildest claims of hyperæsthesia that might be advanced by any pseudo-scientific 'crank'? Professor Münsterberg's admissions arouse the gravest apprehensions. A blind man, he tells us, may perceive distant obstacles by touch (p. 70), "but that does not conflict with the propositions of psychology, and is not mystical," and "even if a man . . . had a sense-organ for electric currents more sensitive than the finest galvanometer, the psychologist would have no reason for skepticism, so long as the physical nature of the transmission from the outer object to the brain is admitted." Such portentous credulity would render him the victim of any swindler who claimed to perform his tricks by 'physical' means, while to the honest investigator, who observed the facts but did not come prepared with figments to 'explain' them, Science would say: "Avaunt, vile mystic, if thou canst not show thy physical mediation, thy physical substrata"! Something must somehow have gone wrong with critical canons which lead to such results!

The truth is, of course, that Professor Münsterberg is once more juggling with the double meaning of 'physical' mentioned above (p. 351), and trying to infer from the (assumed) impossibility of treating an alleged fact by the present methods of the special science of physics that it can have no existence in a world which (among its many other aspects) is also physical. But, to my limited intelligence, it appears almost inconceivable to suppose that any fact should *not* have a physical aspect, however subordinate, while it is obvious that the principles of physics are not rigid and immutable, like the famous 'laws of the Medes and Persians.' Hence I cannot but think that the attempt to proscribe any fact *a priori*, on the ground of the *lèse majesté* it commits against the laws of physics, necessarily fails. No independent jury would convict a fact of the humblest and most suspicious character on such grounds—whatever verdict might be extracted from a packed and hoodwinked court-martial of Professor Münsterberg's laboratory assistants.

And the same objection disposes also of Professor Münsterberg's laboured contentions that (for some reason which he never succeeds in making clear) 'mystical' phenomena are incompatible with the causal connection and mechanical treatment postulated by science. Professor Münsterberg, I make bold to say, is perfectly aware that this is all moonshine, that the mechanical explanation, just because it is a method of science, is universally applicable; that the causal connection as an '*a priori*' postulate is the same; and that if, for the purposes of special sciences or for the sake of excluding distasteful facts, its scope has been unduly narrowed, nothing is easier or more imperative than to expand it to any requisite degree. I

should also like to credit him with the knowledge¹ that a mechanical and (in his sense) causal explanation of a fact in no wise excludes, prejudices, or dispenses with, its explanation by higher categories, and that all the High Priests of all the Sciences in solemn conclave assembled could not effectively taboo the 'romantic' and teleological interpretation of the facts of life.

IV.

I may pass next to what is perhaps the cleverest thing in Professor Münsterberg's article, viz., the splendid audacity with which he interprets away the rout to which scientific dogmatism has so often been put by the victorious intrusion of new facts. "It is absurd," he says (p. 83), "for the mystics to claim the backing of history, because it shows that many things are acknowledged as true to-day which were not believed in earlier times. The teaching of history, on the contrary, annihilates almost cruelly every claim of mysticism, as, far from a later approval of mystical wisdom, history has in every case remoulded the facts until they have become causal ones. If the scientists of earlier times disbelieved in phenomena as products of witchcraft, and believe to-day in the same phenomena as products of hypnotic suggestion and hysteria, the mystics are not victorious but defeated. As long as the ethical (*sic*) category of Satanic influence was applied to the appearances they were not true; as soon as they were brought under the causal categories they were accepted as true, but they were then no longer mystical—it was not witchcraft any more. This process of transformation goes on steadily; millions of propositions which life suggests remain untrue till they are adjusted."

If Professor Münsterberg means what he says by his last remark, it would follow that for lack of a little 'adjustment' he was losing his chance of becoming the greatest discoverer of truth the world has yet seen, a veritable millionaire of verities, in fact! If, again, he means what he says in his last sentence but one, it would follow that he should swallow any yarn so soon as any one has taken the trouble to coat it over with an illusory varnish of 'causal' phrases.

But I am loth to think that he should really believe that the truth or falsehood of reported facts can depend on the atmosphere of worthless *a priori* theories with which we surround them before investigation. If that were all, it would be contemptibly easy to make the extremest assertions look respectable, and to put Professor Münsterberg in a hole whence he could extricate himself only by conversion to the principles of psychical research; I myself should not need that provocation to undertake to show that, with unlimited license of conjectural extension, the orthodox principles of suggestion, hallucination and the 'threshold' of consciousness would be quite capable of 'accounting for' far more marvels than have ever been

¹ He has read his colleague, Professor James', *Will to Believe*, though he misrepresents it. And though his metaphysics, as will be shown (p. 357), result in an impossible separation of the world of phenomena from that of reality, they do contain a sort of admission that both the mechanical and the teleological explanations are (in different senses) true.

alleged,¹ whereas the real difficulty of the subject is to *authenticate the facts*, and as soon as a sufficient supply of authentic facts has been accumulated, no difficulty has ever been encountered in fitting them on to previous facts. To Professor Münsterberg the important aspect of this process is that the facts should have fitted; to the psychical researcher, that the facts should have been admitted—the one is anxious to recover from the shock to his prejudices, the other to obtain recognition for unappreciated truth. It is obvious which of these is the more scientific attitude. And the triumph of science over ‘mysticism’ which Professor Münsterberg here discovers is a triumph only over his own absurd conception of mysticism, while the new facts triumph, not over science, but over the prepossessions of the scientists who arrogated to themselves the monopoly of truth. If any historical fact is certain, it is surely this, that if those who had faith in the unknown possibilities of existence had allowed themselves to be deterred by the *a priori* denunciations of those who thought like Professor Münsterberg, the existence of, *e.g.*, hypnotism would never have been brought to light. And if Professor Münsterberg regards the hundred years’ struggle before the facts were grudgingly admitted a creditable chapter in the history of science, I can compare his audacity only to that of a theologian who should regard the history of witch-burning as a credit to theology, or of a member of a certain historic Sanhedrim who should take credit for his part in promoting the establishment of the Christian religion by stimulating Pontius Pilate’s somewhat torpid zeal for the discharge of his official duties!

V.

At intervals in the course of his article Professor Münsterberg betrays symptoms of an uneasiness about the progress of his crusade against ‘mysticism.’ Thus, when about half-way through, he confesses (p. 75) that so far “we have given decisions, but not arguments.” One cordially agrees, and after this expects a revolution in the logical texture of the article. But, alas, it goes on very much as before. Then Professor Münsterberg makes another effort and invokes the suprasensible truths of metaphysics to supply him with reasons for the utter rejection of mysticism *a priori*. The appeal is successful, and for the rest of the article the lay reader struggles gallantly with Professor Münsterberg’s attempts to express thoughts that lie too deep for words and intelligible statement, and to bring them to bear on a very simple dispute about facts. At first one is inclined to hold that this recourse to metaphysics is merely due to Professor Münsterberg’s inability to find any more conclusive reasons, and forms his version of the old story: “No case—abuse the plaintiff’s attorney,” *viz.*, “No case; drag in metaphysics!” But attentive reflection soon discovers that the real reason is somewhat different.

Professor Münsterberg is a victim of the Germanic spirit. So he cannot content himself with simply telling the ‘mystics’ that their facts are in point of fact illusory. He cannot be happy until he has convinced himself

¹ As, indeed, I pointed out five years ago in *Philosophical Review*, Vol. III, No. 4, pp. 488-9.

that they are *a priori* impossible. Before he can be got to admit a fact as a fact, he must be provided with a proof that it is possible, and see that it has a passport duly *visé*d and officially stamped, entitling it to find a home in the world of science. He can no more help doing this than the British spirit can help ferreting out the facts first of all and postponing to subsequent leisure the task of devising an explanation for them.

Now personally I have much sympathy with metaphysical prejudices, and am reluctant to sacrifice their brilliance to the laborious pertinacity of the sounder British method. I do not therefore propose to be hard on Professor Münsterberg's metaphysics. They are a national infirmity rather than a personal eccentricity or an attempt to confuse the issue, and I will be merciful to them (and to my readers). Hence I shall say as little as possible about them, and, lest I should spoil their æsthetic effect, shall confine myself to quotations.

"Our real inner life is a system of attitudes of will which we do not perceive as objects of consciousness," and the "causal view has not the slightest meaning for this inner reality." "The real inner life in its teleological reality is spaceless and timeless" and is "in short not a psychological fact at all." "Life as seen from a psychological point of view . . . is utterly worthless." A future life, therefore, as understood hitherto, is "a violation of the ethical belief in immortality" which "means that we as subjects of will are immortal; that is, we are not reached by death." Nay, (in this sense) "immortality is certain; for him, the denial of immortality would be even quite meaningless." This, then, is the real idealism and the true immortality, which cannot become a vulgar object of desire to any one, while with the exception of a few professorial 'immortals' 'every one escapes his own notice possessing it.' No wonder Professor Münsterberg gets angry with those who are trying to profane his transcendental ideals by dragging them down to a world in which they themselves live, and change, and suffer, and speak intelligibly!

When such monsters of metaphysical mountains labour to crush Mrs. Piper, the unsympathetic comment of bystanders is not wanted. I shall remark, therefore, only that Professor Münsterberg's metaphysics is Schopenhauerism, that it has inherited Kant's insoluble difficulty of the double causation of every event, phenomenally and really, and that in any case it is not really relevant to the point under dispute.¹

The only thing that really matters about Professor Münsterberg's metaphysics is that they constitute him a mystic of the most pronounced type—in his own sense of the word. For he denies all connections between his two worlds, that of psychology and that of reality: hence each is 'supernatural,' and neither is intelligible, to the other. Our experience as a whole also becomes unintelligible, because it is cleft in twain irremediably by the two worlds Münsterberg the Professor has set up. And this is sad, but it is so; though it need not have caused any surprise to those who have observed

¹ If I were disposed to make trouble I might say a good deal more. I should urge, *e.g.*, that acts and purposes in a world which is out of time would seem to be sheer nonsense; that ethical valuations are facts in time as much as any other, and have nothing to do with Professor Münsterberg's "ethical belief in immortality"; that he is inconsistent in a variety of ways, etc., etc.

how often extremes meet and how truly Aristotle held that excellence lay in a mean. Once more *a priori* scepticism has played into the hands of superstition and puts obstacles into the intermediate path of Psychological Research!

VI.

Minora canamus. It is a relief to turn from such excursions into 'the vast inane' to the pleasant side-lights Professor Münsterberg sometimes throws on his personal characteristics. He is a very Galahad among psychology professors. He has never, he assures us, "taken part in a telepathic experiment or in a spiritualistic séance." But he once had a thrilling adventure "with two famous telepathists in Europe" (p. 77), who had discovered "a medium of extraordinary powers" at a distance, unfortunately, which would have involved him in "15 hours' travelling"—no slight journey even for a well-girt man. For a moment the professor wavered in his decision, and his mighty mind was nigh overthrown. But, before he had started, the telepathists had discovered the fraud. A most providential escape, surely; for if the professor had come and seen, he might have been conquered, and then a flood of superstition might have swept us all back into the Middle Ages! But, after all, his (and our) escape was not, as he confesses, so much due to his scientific caution as to his dilatoriness; the virgin purity of his scientific character was preserved unsullied only by his professorial slowness in packing his trunks!¹ Perhaps his readers would have been more interested to hear the reason why he has never expended a couple of hours and of 5-cent car fares and, fortified only with, say, a couple of bags and a choice selection of psychological instruments, tried his luck with Mrs. Piper! After such heroic readiness to pack his trunks for a 15-hour journey, that seems a little strange.

The reason, doubtless, lies in his sense of the surpassing innocency of his character. He is in very deed "the Israelite in whom there is no guile" we read of in the Scriptures. "Why do I avoid these séances?" he asks pathetically. "It is not because I am afraid that they would shake my theoretical views and convince me of mysticism, but because I consider it *undignified* to visit such performances . . . and because I know I should be the last man to see through the scheme and discover the trick" (p. 78).

And, after an appeal to the effete old *ignava ratio* that a conjuror and not a scientist is the proper person to detect trickery,² he proceeds to

¹ Note the *plural*.

² This seems a truism *a priori*, but is not borne out by experience. For such experts have often most unreservedly admitted the supernatural character of many of the disputed facts—even where the scientists subsequently discovered trickery. The explanation, of course, is that the expert is quite as liable (or in some cases, because of his conceit of knowledge, even more liable) to be deceived by trickery on lines which are unfamiliar to him. And, as Messrs. Hodgson and Davey have shown, the subtler sort of spiritistic fraud really rests on a higher plane than ordinary conjuring. It rests not so much on the deception of the senses by apparatus and prestidigitation, but on the fact that the spectator is induced to *deceive himself* by lapses of attention and errors of interpretation. Hence his mental processes present far subtler, more complicated and interesting psychological problems than those of the conjuror's audience.

commend himself, and his science, for his lack of detective insight. As an experimental psychologist he is by his whole training "absolutely spoilt for the business of a detective." He does not know "another profession in which the suspicion of constant fraud becomes so systematically inhibited as it does in that of the scientist." Daily work in a scientific laboratory he regards "as a continuous training of an instinctive confidence in the honesty of one's co-operators." Hence, he implies, Zöllner, Richet, Crookes, etc., were deceived, and he was saved from a like fate only by his resisting the temptation to investigate—or, more precisely (as we saw above) by the fortunate delay in packing his trunks!

Now, of course, we must accept Professor Münsterberg's description of his own idiosyncrasy. I am quite ready to believe that he is as easy to deceive as he is difficult to convince. But on the other points of this argument I take leave to differ.

I think he exaggerates the incompetence of other scientific men in psychical research when he judges them by himself. They are, of course, not born experts in psychical research, but become such by a pretty severe training, in the course of which they may often fall into error. For they are no more infallible in their observations than in their *a priori* convictions. An instinctive insight into the possibilities of fraud comes to them, as to the detective, only as the fruit of long experience. (That is just why I am not impressed by the authority of scientists whose qualifications resemble Professor Münsterberg's.) But they can make themselves very fair judges of trickery, though perhaps they would do well, both before and after investigating, to consult with a real expert with the long and varied experience of, *e.g.*, Dr. Hodgson. And it is just because isolated investigation is so hazardous, because experience and special study are so valuable, that it is so desirable that the S.P.R. should have the means to employ dozens of trained investigators, who 'know the ropes,' and are fully alive to all the difficulties of the subject, instead of one. For our science here stands shivering on the shore of an 'unharvested sea' of unknown dimensions.

Again, I am a little reluctant to accept Professor Münsterberg's account of the experimental psychologists' superhuman guilelessness. This may in part be due to the unfortunate outcome of my only attempt to enlist an experimental psychologist's co-operation in a 'psychical' experiment. He took advantage of the opportunity to secure the failure of the experiment. No doubt his scientific conscience permitted, nay, persuaded, him to protect 'science' against the possible inroads of 'superstition' by such means, but after this I naturally incline to guard myself against the possibilities of deception on both sides. For it is decidedly humiliating to have escaped the wiles of the professional mediums only to fall a victim to the excessive zeal of a professorial psychologist, whose good faith one had taken for granted!

Further, and this is a contention which has a wider scope, I would maintain that, whatsoever may be the natural and acquired guilelessness of psychologists *de facto*, they have no business to pride themselves on it and to cultivate it *de jure*, and that if Professor Münsterberg's account of the mental attitude of himself and his 'experimental' *confrères* is correct, it *pro*

tanto unfits him, and them, for the prosecution of delicate psychological inquiries. For the obstacles which impede the attainment of truth in Psychical Research and in 'experimental' psychology are in many important respects identical. And if the psychical researcher has to recognise the superior position of experimental psychology in many respects,¹ and may learn not a little from its ingenious use of mechanical contrivances for recording and controlling observations, yet on the other hand the psychical researcher has acquired an openness of mind and a practical experience of the sources of psychologic error, which might be applied to psychological experiments in a most fruitful and revolutionary manner.

Very possibly such assertions will strike Professor Münsterberg as novel and preposterous, and he will feel inclined to retort that he puts his trust in instruments which cannot lie, and apparatus which cannot be hallucinated. And in a limited sense this is doubtless true: instruments are a mighty defence against fraud, though, like all fortifications, they require to be manned by the right sort of garrison and to be properly looked after, in the absence of which care it is, *e.g.*, perfectly easy to produce bogus 'spirit' photographs from the most trustworthy cameras. So that the use of instruments of the most marvellous exactness alone will not prevent deception if the experimenters cannot be trusted.

But the charge is that in psychical researches they cannot be trusted. whereas in 'experimental' psychology they can, and it is here that I find the home of the delusion which puffs up the psychologist with so overweening a conceit of his own superiority, with so overbearing a contempt for the poor psychical researcher struggling amid the pitfalls of deception. But this view of the situation may be shown to be utterly erroneous.

In the first place, it is not true that exposure to deliberate deception is the only or the chief difficulty which besets psychical research. No one who has had any considerable practical familiarity with the evidence and the process of collecting it, believes that conscious deception is the source of any considerable part of it, or that an appreciable percentage of the narrators of marvellous stories are liars, pure and simple. There are such cases, and they throw much valuable light on the study of human mendacity (even though it may not be possible to determine by their means the coefficient of human mendacity with mathematical accuracy), even as one has heard of bogus statistics and bogus surveys in other sciences. Occasionally a well-constructed and well-sustained lie may impose even on an expert, though, fortunately, this danger is rapidly diminishing as the methods of criticism of 'psychical' evidence are becoming better understood, and as the social atmosphere grows more favourable to the truthful reporting of such experiences. Even the possibilities of fraudulent 'mediumship' have become pretty well understood, with the result that the area of the phenomena calling for investigation has been correspondingly restricted. On the whole, therefore, the danger of deception from deliberate and conscious fraud and mendacity is not serious, and it would be as absurd to

¹ Especially as regards its financial resources and its command of the well-paid services of hundreds of workers with a high academic status.

explain the mass of phenomena in this way as to hold that the constancy of the experimental results obtained by psychologists was due only to a tacit conspiracy to foist a profitable pseudo-science upon the universities of the world. The real danger lies elsewhere. It is, as Plato says, a 'lie in the soul' and not 'in words.' It lies in the as yet unexplored possibilities of unconscious and self-deception, of hallucination and suggestion. Of the subtle sophistications which lurk in these agencies we know as yet so little that one sometimes thinks that a new era in psychology might spring from an attempt to answer the question—How are collective hallucinations possible? It must be admitted that the psychical researcher is here largely groping in the dark, sustained only by the consciousness that every moment he stumbles upon something new and unexpected, and that his best security lies in his acute appreciation of the peril of his position.

But what about the 'experimental' psychologist? *His position is precisely analogous, only he does not appear to know it!*

(1) Like the psychical researcher with his 'sensitives,' he has to confine most of his experiments to a very limited section of the human race, viz., those who are not only educated up to the point of understanding them, but are also constitutionally endowed with a rare capacity of resisting the tedium of performing them.

(2) Like him, he puts his subjects under *abnormal* conditions and assumes that what is found to hold of mental processes under 'experimental' conditions will apply also to them under the normal conditions of natural life. This he does, although he knows quite well that the *limen* of consciousness and the capacity of attention vary most surprisingly according to the interest which the subject inspires.

(3) Like the psychical researcher he is dealing with conditions which give the widest possible scope to *suggestion*. But unlike the former he seems to have been (at all events until recently) blissfully unconscious of the fact and to have taken no appreciable precautions against the vitiation of his results from this source.¹ Yet the conditions of the ordinary psychological laboratory are almost the ideal conditions for inducing a maximum of suggestibility. First of all the subject's mind is prepared by impressive and dogmatic statements of his professor to expect certain results. Then he is steeped in a literature admirably calculated to lower his vitality, to stupefy his critical capacity, to abase his imagination, until no thought can enter it of any but a certain sort of result. Meanwhile he is subjected to a thorough course of 'ward training' in the laboratory, receiving all the time frequent 'hints' from his director and breathing an atmosphere which simply reeks with suggestion. When finally he produces his research, is it a wonder that in 99 cases out of 100 its results should be found faithfully to reflect the assumptions of 'experimental' psychology in general and to confirm the views of the director of his laboratory in particular? And do not the training and the ideas instilled in the Leipzig laboratory still

¹ It will suffice to refer to an exception which proves the rule, viz., Professor G. A. Tawney (*justissimus unus qui fuit in Teucris*), who, in an article in the *Psychological Review* (Nov., 1897), shows that the 'double-point illusion' is mainly due to auto-suggestion.

play much the same part in 'experimental' psychology as the 'three-stages' view of the Salpêtrière School once did in the theory of hypnotism? And is it not generally admitted that the wonderfully symmetrical results originating from the Salpêtrière were mainly the result of suggestion, though the fact could not be proved until independent centres of experimentation imbued with different notions had come into prominence? We have then positive proofs that the effects of suggestion can vitiate a whole science; and suggestion can be brought to bear on the youthful minds that frequent psychological laboratories with far greater force than even in the palmiest days of the Salpêtrière theory. Would it not then border on a miracle if such strong suggestions had failed to produce their effect, would it not be still more marvellous if such a 'failure' should ever be thought worth printing?

(4) Like the psychical researcher, the psychological experimenter deals with mental processes whose delicacy renders them peculiarly liable to *hallucination*. And when we bear in mind the powerful suggestions that are operative throughout the process, it will hardly seem surprising that not only, *e.g.*, the 'perception' of infinitesimal differences in sensory stimuli and the reactions of an expectant attention upon an imagined signal, but even the readings of a micrometer or the chronoscopic estimates in thousandths of a second which are needed to produce the required conformity in the results should often be explicable as sheer hallucinations fostered by the peculiar conditions of the experiment. Indeed, we even find some such results recorded by the psychologists, though without any consciousness, apparently, of their methodological significance. At all events I feel that I personally could match Professor Münsterberg's avowal of his limitless capacity to be deceived in 'psychical' experiments by an equally frank avowal of my capacity to deceive myself in psychological experiments. If I did not succeed in getting myself turned out as an unsuitable subject by failing to see anything at all. I am sure I should delude myself into fancying that I saw whatever was expected of me. For in such experiments one cannot trust oneself: there is often just the same difficulty about distinguishing a real from a hallucinatory sense-perception, as there is in some 'borderland' experiences about saying whether one is asleep or awake.

(5) Like the psychical researcher, the psychologist cherishes a weakness for figures—in the one case in the shape of statistics; in the other in shape of 'exact quantitative measurements.' And though, of course, a method of inquiry which can use figures is far more provocative of confidence than one which cannot, it may be questioned whether in both cases the Pythagorean reverence for Number is not often carried to a superstitious intensity. To swamp the inquirer with a flood of figures is not necessarily to solve a problem, especially while one of the chief doubts is as to the relevance of the figures. And, on the other hand, it is well known that with skilful manipulation, statistics and 'exact quantitative determinations' may be made to prove many things, while they serve as opiates to the critical faculty and induce a comfortable glow of scientific rectitude. Hence the pride which the psychologist takes in his figures may often prove a snare, and a prelude to his fall from his fancied eminence of uncontested truth.

In addition to the above five points, in which the problems of the psychical researcher and the 'experimental' psychologist are analogous, I may briefly allude to two others in which there is a divergence, significant as bearing on the attitude which the latter so often takes up towards the former.

(6) The psychologist deals with partial processes of human mentality, isolated as far as possible from their context; the psychical researcher generally experiments with the psychical organism as a whole. The former procedure is more in accordance with the analogy of the other sciences (though, to be sure, it is here a question how far the analogy holds), but it renders him liable to the charge of false abstraction in ignoring the context and the connection of the phenomena studied with the whole mental life. The latter runs the risk of treating as a simple fact what is really the complex resultant of many factors. But, being more concrete, this method is more interesting and more human, as the psychologist cannot but himself feel. And, being irritated both by the popular preference for the wider sweep of the more concrete method and by his consciousness of the responsive note it strikes in his own bosom, he reacts upon the temptation by pouring out the vials of his wrath from time to time in indiscriminate abuse of *Psychical Research* and all its works and ways. Which, though very human, is foolish, and not science.

(7) The psychical researcher is aware that he has to deal with what appears a more aberrant type of fact, and to exercise the greatest care to take nothing for granted. He must state all the conditions of his experiments with the utmost fulness and the greatest attainable accuracy, regarding nothing as too trivial for mention, and sedulously eschewing anything like 'cooking' of the evidence. And I think that no candid reader can deny that such has been the aim of the S.P.R., and that the evidence it publishes conforms to the highest standards in an ever-increasing degree as time goes on, and the knowledge of its methods becomes more widely diffused.

The facts of the 'experimental' psychologist, on the other hand, are intrinsically of an entirely commonplace character. They are so closely connected with those of the recognised science of physiology, that their dependence on it has often seemed mere parasitism. Hence the psychologist feels freer from the restrictions which are imposed on psychical research, and his accounts show that he enjoys his freedom to the full. He appears to recognise no limits to the suppression of unsuitable records and to the selection of the 'facts' he propounds. And in any case his statement of the conditions is hardly ever full enough to disarm the suspicion that the first four sources of error I have mentioned have not been adequately guarded against. Hence the reports of psychological experiments are hardly ever such as to carry conviction to any one disposed to dispute their results; they are accepted because, as a rule, they shock no antecedent prejudice. But this uncritical *naïveté* can hardly be esteemed a recommendation by an impartial student of the methodology of the sciences, and hence Professor Münsterberg's complacent delight in the absence of suspiciousness fostered by his psychological methods vividly reminds me of General Mercier's innocent surprise at being informed of the illegality and impropriety of his use of the documents whereby the condemnation of Dreyfus

was secured. It would be far better if, instead of propagating misrepresentations of the S.P.R., Professor Münsterberg imitated its methods somewhat more closely, and got, *e.g.*, his fellow-townsmen, Dr. Hodgson, to open his guileless eyes to some of the sources of illusion which infect his most favourite assumptions.

VII.

Now why, it may be wondered, have I done Professor Münsterberg's incoherent and ridiculous reasonings ("*Raisonniren*" as the Germans aptly say) the honour of so detailed a refutation? It is not that so elaborate a counterblast is needed against sophistries which can hardly impose on their author. It is not for the sake of gaining a dialectical victory and humbling his professorial pride. It is not in the hope of effecting his conversion. For if any man can remain deaf to the charming of his colleague, Professor James, no power in heaven or earth can move him in his prejudices. It is simply because I would put it to *M.M. les Généraux de l'Etat-major* that, even from their own point of view, they are making a mistake in their conduct of this awkward little affair. Their methods of suppressing the 'revisionists' are too crude and inefficient. We want something fresher than a re-hash of the old *a priori* prejudices in lieu of conclusive documents. We want something more unlike the belated forgeries of Colonel Henri than Professor Münsterberg's perversions of the S.P.R.'s attitude. He is simply adding blunders to the ancient crimes, and, from the point of view of humanity, adding crimes to the ancient blunders.

For, socially speaking, the policy pursued in this Dreyfus Case of Science has been criminal as well as stupid, criminally stupid and stupidly criminal. The success of the S.P.R., to say nothing of more fanatically 'Dreyfusard' associations, must make it obvious even to the obtusest professor that the policy of the *chose jugée* has failed. It has not achieved what was expected of it—it has not burked inquiry. And it is quite plain that it cannot do so in the future, however great becomes the growth of Science. For, as the sciences grow more specialised and their language more technical, the radius of their influence contracts rapidly, their ideas affect the minds of men less and less. Already in Professor Münsterberg's article (unless its obscurity was intentional) the real ground of his technical objection to 'mysticism' can hardly have been intelligible to one out of a thousand of educated readers. And experimental psychology is still in its infancy, crying aloud to every *alma mater* to notice and nourish it. To what depths of technicality and obscurity may we not then expect it to descend by the time it is full grown?

Now the impotence of science is the opportunity of superstition. It flourishes unchecked all around the scientist, who will not compromise his dignity by recognising its existence. He cannot check its growth by arguments so technical that they are understood only by the very few who have been able to spend the best years of their youth in his laboratories. His attitude of *a priori* disgust is too unsympathetic to have the slightest effect upon the convictions of his adversaries who, as Professor James has well pointed out, "are indifferent to Science, because Science is so callously

indifferent to their experiences." ¹ In fact, it stands to reason that one cannot expect to convince any adherent of mistaken views by either ignoring him or saying to him: "Now, my good man, you are either a liar or a lunatic."

And for the scientists to hold utterly aloof from the beliefs of the vulgar is just as stupid and dangerous a practice in the end as was that of the Roman philosophers to discourse about humanity, while leaving untouched institutions like that of the gladiatorial games. Similarly the psychologist consumes his energies in researches which, from a social point of view, can only appear ingenious modes of academic trifling, while leaving unexplored subjects which *prima facie* raise the most stupendous issues, have the most direct practical bearing on society, and most naturally fall into his province.

No wonder, then, that the policy of the *chose jugée*, as practised for the last two centuries, has not eradicated superstition: it has been a blunder. By the same token, it has also been a crime; for a different policy *would* have eradicated superstition. And thus the real responsibility for the persistence of superstition lies not with the ignorant masses, who interpreted their experience according to their lights, but with the educated Pharisees, whose delicate nostrils were offended at the very idea of associating with publicans and sinners. It is these latter who are the true obscurantists, who by keeping aloof have kept alive the belief in witchcraft, Satanism, and the offensive aspects of supernaturalism generally, whose consciences ought to be burdened with the unspeakable evils these beliefs have brought upon mankind. And if they knew not what would be the effect of their attitude, they were foolish; if they knew it, they are, to put it mildly, disingenuous. Does it not behove, then, every good scientist and every good citizen to work for the 'revision' of the Dreyfus Case of Science?

¹ *Will to Believe*, p 323. Professor Münsterberg has apparently read this excellent essay, and that after doing so he should write as he does gives the measure of what one would be disposed to call either his moral bias or his mental obtuseness, if one did not know to what lengths the pathological obfuscation of the psychological intellect may be carried in the discussion of these matters.

IV.

DR. MORTON PRINCE'S "EXPERIMENTAL STUDY
OF VISIONS."

BY F. W. H. MYERS.

The paper which Dr. Morton Prince allowed us to read at the last meeting of the S.P.R. has now appeared in *Brain* under the above title. It describes one of the most curious cases of split personality yet recorded;—a case, however, which is somewhat tantalising, since it seems plain that, had Dr. Prince chosen, he might have pushed his experiments in yet more interesting directions. Let us hope that he may in fact have done so, and may ultimately give his observations to the world. Meantime some quotations, with occasional comment, may be acceptable to our readers.

I have had, says Dr. Prince, an opportunity to make some experiments of this kind on a particularly favourable subject. In most of the observations reported hitherto, it has not been possible to thoroughly investigate the relation of the vision to antecedent events in the subject's life, beyond the evidence of the waking memory of that person. But in this instance, the fact that it was possible to hypnotise her and obtain *two additional and distinct personalities, three in all*, each with distinct memories, gave an opportunity to search in the hidden depths of consciousness, and obtain information about facts long forgotten by the normal personality. Further, it was possible to experimentally study the relation of the sub-conscious personality to, and the influence of this upon, the production of visions.

To understand this, it is necessary to state a few facts regarding the subject. Miss X.¹ suffers from what is ordinarily called neurasthenia, or hystero-neurasthenia, but what I think is more correctly called hysterical neurasthenia. But she has no physical stigmata, excepting a possible contraction of the field of vision.² She is easily hypnotised; first passing into a state which resembles that of the ordinary classical deep stage. After waking there is complete loss of memory of this state. During hypnosis she is very susceptible to suggestion; but on the whole there is nothing very remarkable about the mental condition, which does not materially differ from one of the deeper stages of hypnosis as ordinarily observed. The only points to which I would call attention are that Miss X. in this stage of hypnosis knows all about Miss X. awake, and apparently remembers all that

¹ Dr. Morton Prince's subject, "Miss X.," is, of course, not to be confused with the former pseudonym of Miss A. Goodrich-Freer.

² At the time these observations were made this seemed to have disappeared, or, at least, could not be recognised by the finger test.

Miss X. remembers and some other things besides, which she has forgotten when awake ; but Miss X. when awake knows nothing of what has occurred in this hypnotic state. For convenience' sake I have called Miss X., in this stage of hypnosis, X. 2, as distinguished from Miss X. or X. 1.

Now, on commanding X. 2 to sleep more deeply, there appears a third personality, whom I have called X. 3. This person knows all about both Miss X. and X. 2, everything they do and think, but is unknown to both. Her memory, also, in some respects, is much fuller than that of either Miss X. or X. 2, so it comes about that X. 3 can tell much in the past life of Miss X. that that person has forgotten, and can explain much that the waking personality is at a loss to account for. X. 3 even knows all about many of the little absent-minded and half-voluntary doings of Miss X., nor does she hesitate to voluntarily tell of them, although Miss X. is morbidly and unnecessarily reserved about her whole life.

Thus far this case seems quite in accordance with analogy. An unusual point, however, now follows ;—in the relation of the different *characters* of the personalities to each other. In cases already observed we have generally found that the *deepest* attainable personality, besides possessing the completed memory, possessed also, on the whole, the most intelligence. In this case, on the other hand, the deepest phase of personality at which we seem to get is the least satisfactory of all.

It may only be added that the characters of the three personalities were very different and distinct. Miss X. is reserved, morbidly conscientious, self-contained, serious, deferential, and dignified. X. 2 is sad, serious, and gives the impression of weariness and suffering. X. 3 is flippant, jovial, free from all physical infirmities, full of fun, reckless, and contemptuous of Miss X., whom she stigmatises as silly, stupid, and dull, and in an apparently heartless way enjoys every trouble that comes to her. X. 3 always speaks of Miss X. as "She,"¹ and insists that they are different persons—that they don't think the same things or know the same things. She speaks of X. 2 as "Miss X. asleep."

This tripartite personage looked in a crystal,—or rather in "an electric-light globe or lamp," of course not connected with a circuit, but held free in the hand. The mode of vision was just such as our readers are accustomed to hear of.

The visions were not seen like small objects reflected in or on the glass bulb, but Miss X. stated that the bulb disappeared, and she saw before her the scene she described, which appeared real, the characters being life-size and like living persons. I should infer that she saw them, much as one sees the characters on the stage of a theatre, but she saw them, nevertheless, where the bulb was. Perhaps the illustration of looking through an opera-glass at the stage is apposite, for she several times at first broke off from her

¹ In the following account, She, with a capital S, always refers to Miss X. (X. 1).

inspection and examined the bulb to see if there was any explanation of the vision to be found in the glass, much as one who had never seen an opera-glass or a kaleidoscope might examine it for this purpose. To me as I observed her she appeared like one who, at a theatre, was completely absorbed by the play, and in that sense was unconscious of surroundings, but not at all in a trance state. Her absorption and the exceeding mobility and expression of her face when describing a vision gave the impression that she was entirely oblivious of myself and surroundings, until spoken to, but not as one hypnotised, but rather as one who is intensely absorbed in the scene of a theatre and has forgotten where she is. Every feeling, timidity, surprise, interest, seemed to be expressed by the play of her features, and at times, especially at first, she seemed rather frightened by the uncanniness of what she saw. Most of her descriptions were given in answer to my questions, which were continuous but never suggestive. They were simply expressions like "What else?" "Go on," "Is there anything more?" etc. It may be noted, then, as psychologically of interest, that the visions appeared like ordinary hallucinations or vivid dreams, the scenes real, of life size, but *dissociated* from her surroundings, and not as part of them.

The substance of the visions consisted largely of memories of past visual experiences, but partly also of scenes which the percipient had at any rate never discovered with ordinary consciousness. I give first an instance of a scene on which Miss X.'s eyes had no doubt looked, but without conscious attention.

Experiment 4.—X. 3 now (after explaining the last vision) volunteered the following remarkable story, telling it with great gusto and as a joke on Miss X., and speaking with great rapidity, so that it was difficult to follow the sequence of events. The language was substantially that of X. 3, but condensed. "She" yesterday received a letter from a photographer. She had it in her hand while walking down Washington Street, and then put it into her pocket (side pocket of jacket) where She kept her watch and money (bank notes). Then as She walked along, She took out the money and tore it to pieces, thinking it was the letter from the photographer. She threw the money into the street. As she tore up the money she said to herself:—"I wish they would not write on this bank-note paper." At my request X. 3 repeated the words of the photographer's note, which was to say some photographs were ready. As to the money, there were twenty dollars—two ten-dollar notes; this, at my demand, X. 3 counted mentally, with some difficulty and concentration of thought. X. 3 manifested considerable unwillingness to show me the letter, which she said was in "her" pocket still, and which She still thought to be money. Finally, after some insistence, she did so. It was folded up into a small square, just as one often folds bank-notes. The language of the photographer's note was identically the same as quoted by X. 3. X. 3 said that "She" was absent-minded, and thinking of something else, when "She" tore up the money. I then gave X. 3 the note to put back in her pocket preparatory to waking her up. This impish, hypnotic personality remarked upon what a joke it would be when She found it there instead of the bank-notes. I will say here the

heartless, cold-blooded delight which this almost Iago-like personality found in the loss of the money, which was a very great one to Miss X., was appalling. To X. 3 it was a splendid practical joke.

Miss X. was now awakened. I asked her whether she did not have some money and had not received a letter from a photographer. She said "Yes," but seemed to think it rather queer my asking these questions, but by this time she had become accustomed to being astonished. In reply to a series of questions she said she had not the letter with her, but had torn it up and thrown it into the street (Washington Street) The bank bills she had in her pocket. They were two ten-dollar bills. I asked her to show them to me. She put her hand in her pocket to take them out and brought out the photographer's letter. She evidently received a shock, although she tried not to show it. I asked her where the bills were, and after searching her pocket, she insisted that she must have left them at home. I remarked that she must have destroyed them by mistake instead of the letter; she refused to admit it. I pointed out the circumstantial evidence; she recognised that it was suspicious, but could and would not believe it. The loss meant a great deal to her, and she evidently encouraged herself with a forlorn hope. I then said, taking the glass globe:—"We will see whether it is not true. Look in and you will see what you have done." At first she saw only indifferent things; then I said:—"Think of bank-notes and the feeling of tearing them up." Now, to her astonishment, she saw herself walking along Washington Street and putting the letter in her pocket, then taking out what looked like bank-notes, that is, green pieces of paper, and tearing them into pieces and throwing them into the street. The vision in all its details corresponded to the account given by the hypnotic personality.

This curious story shows us a secondary personality in (so to say) its most alien and even hostile form;—such a form as might well suggest actual possession by a malign sprite. Yet the truer analogy is probably the madman who beats one side of his own body under the impression that it is an enemy.

I add another quotation which shows the third personality in a striking light;—as remaining alert and unconfused throughout the perilous delirium of the primary personality.

(B) Experiments in which the hallucination was not a revival of a past visual experience, but was largely a newly-created visual representation of a past experience other than visual.

Experiment 5.—I will give this in the brief language of my notes: Miss X. looked again into the globe; she saw a room with a bed in it. There was a figure in the bed; the figure threw off the bed-clothes and got up. Miss X. exclaimed:—"Why, it is I." (Appeared rather frightened at what she saw, but went on to describe it, largely in answer to my promptings, such as, "Go on," "What do you see?" etc.) She saw herself walking to and fro, up and down the room. Then she climbed on to the window sill which is the deep embrasure of a mansard roof. Then she climbed outside the

window and from the sill looked down into the street. It was night—the street lamps were lighted, there was also the gas light in the room. As she looked down she felt dizzy. Here Miss X. turned away frightened, saying she felt the same dizziness as if she were standing there. She soon continued. She saw herself throw an inkstand into the street below, which she had seen herself take before climbing on to the window. Miss X. was again obliged to stop looking because of dizziness. After a time she returned to the globe. She saw herself go back into the room and walk up and down; the door opened and she jumped into bed and lay quiet. Miss L. (a friend) entered, went out and returned several times; brought a poultice which she put on Miss X.'s chest; Miss X. herself remaining quiet. Then Miss L. went out and Miss X. got up and took the poultice, rolled it up into a little bunch and hid it in a corner, putting a towel over it. Here the experiment ended.

Miss X. stated, on being questioned, that she could not remember any incident like the vision, excepting that she recognised the room as the first one she occupied when she came to Boston four or five years ago. It was in the top storey of a house on Columbus Avenue; she was ill there, and Miss L. took care of her. But she did not remember ever having climbed on to the window, or having thrown an inkstand, or any of the incidents of the vision. She could throw no light on the affair. She was now lightly hypnotised and X. 2 was present. X. 2 could add very little to Miss X.'s statement. (My notes of X. 2's memory are somewhat confused; it is not plain from them whether or not she remembered ever having done any of the things seen in the vision, although it is stated that she had an imperfect memory of the incident. It is possible X. 2's statement as noted was mixed up with that of X. 3, but X. 2 remembered being ill in the room described, and that ink was found in Miss X.'s shoes; X. 2 did not know how it came there, but Miss L. had said that Miss X. had poured ink in her shoe. At any rate, X. 2 could not explain the incident as X. 3 did later.)

Deep hypnosis; X. 3 appeared with great vivacity and amusement. X. 3 explained the whole scene. (As was customary with her, X. 3 spoke of Miss X. (X.) as "She," as if it were an entirely distinct person and not herself.) "She" had pneumonia and was delirious, and She imagined She was on the sea-shore and was walking up and down the sand. This was why she walked up and down the room, and She stuck her toes in the carpet, thinking it was the sand. There were rocks there, and the window sill was one of them and when She climbed out upon the window sill She thought She was climbing upon a rock, and she took up a stone, as She thought, and threw it into the sea. This was the inkstand that she threw into the street. Then when She took the poultice and hid it in the corner, She thought She had buried it in the sand. She had not poured ink into her shoes, but her hand shook and She had spilled it into her shoes. Miss L., seeing the inkstains, had inferred that Miss X. had poured the ink into the shoes, and had told Miss X. so. Hence the statements of X. and X. 2. X. 3 was highly amused at all the mistakes of Miss X.'s delirium.

Finally, one more quotation,—the last which can be here admitted,—indicates the line of inquiry which one must hope that Dr. Morton

Prince has followed, or will yet follow ;—however cautious he may be as to printing the results to which it leads.

Experiment 8.—Miss X. had lost a scarf pin she valued. I suggested that she should try to find it by looking in the globe. After looking a few minutes, she saw a bed-room with a brass bedstead, and on the dressing-table was a pin-cushion (described in detail), and in the pin-cushion were several pins. Amongst them she recognised her own, and exclaimed somewhat excitedly “Why, that is my pin !” Involuntarily she reached out to seize it. During this her face was very expressive, and showed great astonishment. She did not recognise the room, and was certain she had never seen it before, but thought it a city room, and not a country one, from its general appearance and furnishing.

In hypnosis both personalities were also ignorant of this room ; but X. 3, being asked to tell the last time that Miss X. had the scarf pin, recollected perfectly. “She” (Miss X.) came into her room (in the country, not in Boston) one day with a very severe headache, took the pin out of her dress and stuck it into the head of the couch, then doubled up the pillow and placed it over it, and lay down on the couch with her head on the pillow and rested. “She” forgot all about the pin, and had no recollection of putting the pin where She did, and, in fact, did it rather absent-mindedly. This was several months ago, and X. 3 presumed the pin was gone. On Miss X. being awakened, I told her what she had done with the pin, but still she had no recollection of the affair.

Was that pin actually in that pincushion ? Perhaps it *was* ;—and if it was, that fact has no inconsiderable bearing upon the way in which the world is made.

Meantime there is one practical lesson—often urged already in our *Proceedings*,—which these experiments should plainly reinforce. Crystal-vision should become one of the habitual and recognised occupations of the psychological laboratory. I know of no real reason whatever for supposing that the power of seeing these visions is commoner in hysterical than in normal patients, or is a token of any kind of “disaggregation.” This has, indeed, been assumed on *a priori* grounds by some writers. And the fact that a good many hysterical patients, being under their doctors’ control, have been induced to look in the crystal, has inevitably tended to foster the assumption. But the relative proportion of normal to hysterical seers is as yet undetermined ;—and cannot be determined until a far greater number of normal persons have seriously made the attempt. The instances of normal seers quoted in our *Proceedings* and in Mr. Andrew Lang’s *Making of Religion* represent but the very first hasty ingathering from a vast and unworked field. And I believe that that field will never be adequately worked until the scientific world shall frankly adopt the view which I have often suggested :—namely,

that these crystal-pictures are experiments in the extension and externalisation of inward or central vision ; and that the inward or central vision which we thus externalise by empirical artifice may ultimately become for us even fuller of instruction than that outward or peripheral vision on which mankind are accustomed exclusively to rely. The highest use, perhaps, of this optical vision of the material world is to teach us an alphabet,—a scheme of visual presentations,—whereby we may behold, “as in a mirror, darkly,” truths not only material but spiritual which lie outside and beyond our optical scope and horizon.

V.

EXTRACT FROM J^e-E DE MIRVILLE'S

"DES ESPRITS ET DE LEURS MANIFESTATIONS FLUIDIQUES."

(Paris, 1854). *Third Edition*, Vol. I., Ch : I : pp. 18-32.

INTRODUCTORY NOTE BY ALFRED R. WALLACE.

In the last Part of the *Proceedings* (December, 1898) Mr. Podmore, in the preliminary remarks to his "Discussion of Trance-Phenomena of Mrs. Piper," devotes more than five pages to an examination of the evidence for clairvoyance in the case of Alexis Didier, which evidence he depreciates throughout, and arrives at the conclusion that Alexis might have been, and probably was, a clever impostor. He urges that "bandaging the eyes," as described, was not "satisfactory;" that many indications showed "that the power exercised by Alexis was perfectly normal"; that the reading a book several pages in advance of any page opened at random was "the most strongly suggestive of trickery"; and that the most probable explanation of his card-playing performances "is that of deliberate fraud." He urges that his manager, Marcillet, might have been a confederate, and that the reports "are mostly at second-hand or insufficiently detailed." He quotes lengthy reports of some of the special instances of clairvoyance which were of such a nature as to be explained by thought-reading, but says nothing of those in which facts were correctly given which were not known to any one present; and, finally, he omits all reference to the most remarkable and convincing evidence of Robert Houdin, whose testimony has been quoted by Dr. Lee, by myself, and by many other well-known writers, while a detailed report of it is to be found in the great work of the Marquis de Mirville to be seen in the Society's library.

When preparing my reply to Mr. Podmore (which appeared in the *Journal* of February) I borrowed De Mirville's work, and for the first time read his detailed account of Houdin's experiences certified as correct by Houdin himself. This account seemed to me to be so important, as well as so intrinsically interesting, that I suggested the printing of a translation of it in our *Proceedings*. To this the Editor has assented, and the following very close translation has been kindly made by Mr. J. G. Smith. So far as I am aware, it now appears for the first time in English, although it has been known

to a few students for nearly half a century; and, if it is admitted that the question is one of evidence, it must be held to prove the reality of the clairvoyance of Alexis, both that kind due to thought-reading and that termed "true clairvoyance" in which the object described is not known to any one present or, as in the case of the cards dealt by Houdin and the book brought and opened by himself, to any living person.

In his rejoinder (in the *March Journal*) Mr. Podmore admits that "Houdin's testimony is, no doubt, very striking"; but he urges that it is not conclusive as against the theory that subjects in trance may possess "preternormal acuteness of vision." To this I would reply that any such preternatural acuteness of vision as is here required has never been *proved* to exist, but has been *suggested* as the only means of explaining phenomena deemed too incredible for acceptance on any testimony; and, further, that if trance patients can see through cards, and tables, and eight pages of a printed book, to admit such "acuteness of vision" is only to admit "clairvoyance" under another name.

I would here earnestly call the attention of our members to a very important elementary principle of sound reasoning too often neglected in discussions of these questions—that, as tersely stated by J. S. Mill, "an argument is not answered till it is answered at its best," and that no amount of negative or indirect evidence is of any weight as against good, positive, and direct evidence on the other side. I ask them to compare carefully this evidence of De Mirville and Houdin with that adduced by Mr. Podmore, and they will find that while the former consists of the very best *direct* evidence of *facts*, the latter is wholly negative, consisting of doubts, suspicions, and possibilities, every one of which is excluded in the direct evidence here given.

This fundamental defect applies, in my opinion, to all Mr. Podmore's writings on this subject.

Translation.

You all know *Robert Houdin*, and you will deny to this Prince of Conjurers neither the pre-eminence to which his skill entitles him, nor, consequently, his entire competency as a judge of the skill of others.

Evidently, to put an end to this everlasting and unsound argument,¹ the only thing to do was to go and find *Robert Houdin* himself.

No sooner said than done. I am shown into his drawing-room, and our interview begins.

¹ [i.e., that as Robert Houdin could, by trickery, do everything that clairvoyants did, and even more, there was no reason to suppose supernormal power in the case of clairvoyants.—Translator's note.]

I am going now to give a careful word-for-word report, with all the accuracy of the *Moniteur*, of everything that subsequently took place.

M. Houdin's signature will guarantee the truth of the record.

"Monsieur Robert Houdin, I am an admirer of your second sight, but will you kindly tell me if you have any knowledge of magnetism? Have you seen any somnambulists?"

"A slight knowledge, Monsieur; I have seen only two somnambulists."

"What did you think of them?"

"Their tricks were so badly done, so contemptible, that I could, then and there, have given them a lesson."

"So you regard a somnambulist as a *brother conjuror*, and often as a clumsy one."

"But how else would you regard him? After all, as I said before, I have seen only these two poor specimens. All I can say is that in the course of a journey that I made in Belgium, to Brussels, Liège, and Aix-la-Chapelle, I constantly followed M. Laurent and M^{lle}. Prudence, two of your 'magnetic' celebrities, and I can assure you that the day after their séances I invariably dissipated, as if by enchantment, their triumph of the previous night. Whereupon, to my great regret (for I have always a genuine dislike to causing any one the smallest injury), the kind of stupor of admiration that they had caused was suddenly turned into sarcasm, insults, and even coarse abuse, the outcome of absolute incredulity. However, to do them justice, I must add that, a few days later, and with a courage that I can only describe as heroic, they returned to the charge, and succeeded in the very same towns in reversing public opinion, and in conquering afresh what I had just deprived them of. I have often since reflected on this fact, and have found no explanation of it."

"Do you want an explanation, and would you be curious to see a *genuine* magnetic, or rather somnambulant phenomenon?"

"That has long been my wish."

"Would you consent to accompany me and spare me a few moments of your time?"

"Although I am very busy just now, nothing would give me greater pleasure."

"Very well, I will not ask you, in the unlikely event of your being convinced, if you would have the honesty to acknowledge it and further to subscribe to your convictions; I will not ask you to do so, for I already read in your face the entire frankness of your reply."

"Rest assured, Monsieur; in that case you will have no cause to be dissatisfied with me."

"It will be delightful then to show the savants, whom we were speaking of just now, that love of truth has flown to you for refuge. But don't forget to bring with you some cards (orthodox ones, mind, not *yours*), a book, some hairs, etc., in fact anything which could best contribute to settle your convictions."

"Have no fear. I know what I'm about. Can Madame Houdin come with us?"

"Most certainly."

"All right, I will come back at one o'clock to fetch you."

It was then 12 o'clock, and when I got into my carriage, R. Houdin heard me give the address of 42, Rue de la Victoire *for the first time*. I have underlined the words "*for the first time*," because there are plenty of magnetisers in Paris, and nothing having intimated beforehand our choice of one more than another, there could have been no time to foresee or to forestall that choice.

On the way the future neophyte employed all the resources of his dialectic (no difficult task in such cases) to prove to me what he considered as self-evident, namely, that it was all a question of *dodges* more or less elaborate, and of a *répertoire* better "staged" than the average. . . . At the moment of our arrival the oracle is performing in the adjoining room for the benefit of several people. Presently one of them (M. Prosper G—t) comes out, greatly impressed by having just been given a description of his country house, situated at the opposite extremity of France, down to the series of pictures hanging in his bedroom. Further than this, after a description of all the out-houses, stables, and finally of the dog kennel, M. G—t had added :

"Could you tell me the name of the powerful animal that sleeps in the kennel?"

"His name is—wait—his name's Es . . . Esterl—and it's the name of the guide who got him for you."

Here we, too, found ourselves on familiar ground; for who does not know Esterl, the smartest and wiliest of all the Eaux Bonnes guides?

I have often passed several hours together in Marcillet's salon, amusing myself with the kind of stupefaction (there is no other word for it) stamped on the faces of the sitters, very different from the expression which they wore on entering.

Evidently, it had required very personal and private revelations to upset them to such a degree. But I was forgetting that the persons who attended there one after another, from morning to night, were, one and all, primed accomplices.

However, to return to the investigation of M. Houdin, who is now face to face with Alexis.

Alexis exhibits on waking the drawn features, the peculiar look, and neurotic stamp so characteristic of somnambulists, which alone would be amply convincing to a doctor's eye. Then gradually the face regains its composure, the colour returns, until on being put to sleep again by his magnetiser by means of a simple pressure of the arm, a slight and barely perceptible convulsion once more shakes his whole system, and plunges him into a somnambulant state.

Robert Houdin, who is an adept in such matters, asks leave to bandage Alexis' eyes himself. After a careful examination of the wadding and the enormous silk handkerchiefs that were handed to him, he covers the whole of the subject's face with the wadding. But after folding two of the handkerchiefs over the rolls of cotton wool, which enveloped the face as though it were a most precious statuette, and which from the top of the forehead to below the lips did not allow of the very tiniest opening, he declined to use the third, and did not require, as certain doctors do, a complete mask. The reason being that R. Houdin knows his business and the Prince of Conjurers

does not bother about such trifles. These two most suspicious eyes once well encased in wadding and covered with bandages, with all the chinks stopped up, so to speak, R. Houdin drew from his pocket two packs of cards still in their wrappers with the Government stamp intact, opened them, shuffled and asked Alexis to cut. He does so, and, I must admit, does so in some special way, which *I* do not remark, but which brings a passing smile to the lips of the expert observer. It is clear that Robert Houdin has noticed something, and thinks he recognises the trick, and any one but myself would have trembled for the success of the experiment. Nevertheless he lays down five cards before his opponent, who takes good care not to touch them, gives himself five and is just going to pick them up, when Alexis stops him with the words: "You needn't do that. I take every trick," and names the ten cards which are still lying on the table face downwards.

"Let us begin again," says R. Houdin coldly, stunned though he was, as if by a heavy blow.

"With pleasure."

Ten fresh cards are substituted for the first lot, and this time there was no smiling.

"I discard," says R. Houdin.

"Why do you keep these two cards, and this very weak trump too?"

"Never mind, give me three more."

"Here they are."

"What are they?" says Houdin, covering them with both hands.

"Queen of Diamonds, Queen of Clubs, and eight of Clubs."

"Go on, a third round."

Same accuracy and infallibility.

It is now my turn to watch, and what do I see?

R. Houdin fixes Alexis with those remarkable eyes of his; he changes colour, his face grows livid, and a kind of nervous quiver passes over his features, and then, with the passionate excitement of an artist who suddenly recognises his master, cries,

"What is this? What's happening? IT IS SPLENDID."

Then, as used to happen in the Chamber after a fine speech, the sitting is perforce suspended for a time. A fresh start is made, and R. Houdin, after taking off the somnambulist's useless bandages, draws a book of his own from his pocket, and asks him to read eight pages further on, starting from a given place. Alexis pricks the page two thirds of the way down with a pin, and reads, *Après cette triste cérémonie.*"

"Stop," says R. Houdin, "that's enough. I will look."

Nothing like it on the eighth page, but on the next page at the same height are these words, "*Après cette triste cérémonie.*"

"That's enough," says Houdin. "What a marvel! Can you tell me who wrote me this letter?" Alexis feels it, puts it on the top of his head, and against his stomach, and describes the writer fairly accurately, but he makes what a doctor would call *mistakes*. Trifling mistakes, however; for instance, he is mistaken as to the shade of the hair, and as to his profession, calling him a librarian, because he sees him surrounded by books. Briefly, errors of detail, such as somnambulists are constantly being forced to

commit when too closely pressed. . . . R. Houdin does not allow these errors of detail to stop him, and, returning to the subject of the letter, asks : "Where does it come from?"

"From ——"

"Ah," says R. Houdin, "and the postmark. I never thought of that. But since you see this house, can you tell me in what street it stands?"

"Wait. Give me a pencil." After five minutes' reflection, he wrote rapidly, "Rue d'A——, Number ——." "This is too much, says R. Houdin. "It's beyond me. I don't want any more. One word more, though. What is the writer of the letter doing at this moment?"

"What is he doing? Take care. Be on your guard. He is betraying your confidence at this very moment."

"Oh! that is an utter mistake," says Houdin, "for the writer is one of my best and most trusted friends."

"Take care," repeats Alexis, this time in an oracular tone, "he is deceiving you shamefully."

"Nonsense,"¹ replies Houdin.

Madame Houdin now takes a turn at asking questions.

"Could you tell me, Monsieur, what I am thinking of at this moment?"

"Give me your hand. What are you thinking of? Wait. You are thinking of a child, of a very young child. Ah! poor mother, how sorry I am for you!"

Madame Houdin, who, so far, had endeavoured to keep a smiling countenance in order to mislead him, sheds a few tears.

"But, Monsieur, you can see him, then!"

"Yes. He died on the 15th of last July."

"At what o'clock?"

"Four o'clock in the morning."

"At Paris?"

"No. Three leagues from Paris. Wait. Ah! It was too late."

"What do you mean?"

"I mean that you changed the wet nurse too late. You know that well; the child was poisoned by the first nurse's milk. The poor woman was very ill."

"Oh! that's quite true, quite accurate. And could you tell me what I am thinking of now?"

"Alas! You're thinking of a still younger child, for it is not yet alive."

This was as a matter of fact the subject of Madame Houdin's thoughts, whose maternal hopes were anticipating the future. Thereupon Alexis, seeing me writing in a pocket-book, snatches it from my hands, places it quickly on his head and reads from it two or three lines written in pencil, which we find on the page named.

¹ We hasten to add that last year, on my paying another visit to Robert Houdin, in company with my friend, M. Lacordaire, Manager of the Gobelins Manufactory his first words were: "You remember, Monsieur, the famous letter of my friend, living at ——, and my repeated denials to Alexis." "Yes." "Well, this wretched friend was robbing me of 10,000 francs at the very time of the sitting." It will be admitted that this development has given a more serious turn to the matter.

A curious thing then occurred, which we submit to the consideration of persons interested in this inexplicable agency. In the pocket-book was a loose object.

“What is it, Alexis?”

“A piece of cardboard.”

“Yes, but what exactly is the cardboard?”

“I don't know; it is surrounded by little short engraved lines, but I don't know what they are.”

“Try hard. It isn't difficult; a piece of cardboard in a pocket-book.”

“Wait. It's a large visiting card—a card on which steel pens are displayed for sale—a tradesman's address.”

Nothing of the kind, and the capricious faculty of the somnambulist could not succeed in guessing an *almanac*. A certain doctor of my acquaintance would have been triumphant and have quickly closed the sitting as usual. We continued.

“And the paper next to it?”

“The one folded in four?”

“Yes.”

“Oh, that's very different and not difficult. ‘Receipt of MM. Saquier and Bray, booksellers, 64, Rue des Saints-Pères, for 15 francs, 20 cents.’”

R. Houdin opens the paper and verifies the correctness of this statement. Astonishment greater than ever. On second thoughts, however, he says:—

“As far as I'm concerned this is not evidence, Monsieur, for I have not the honour of your acquaintance, and though I am, personally, convinced that you are not in league with the somnambulist, yet I must act as though you were so on this occasion. Allow me then to confine myself to matters within my own personal knowledge, and to make one last experiment.

“To whom does this hair belong?”

“To a young man.”

“Who may that be?”

“Your son.”

“What age?”

“Three years less than you allow him to be.”

“True. What is he suffering from? He is ill.”

“Yes. He suffers much pain in the right side. But wait. You have just touched these hairs, and I have made a confusion in the fluids. It's you who are suffering in the right side, and at this very moment.”

“That's quite true; but how about my son?”

“Your son? He has nothing the matter with him.”

“Yes, he has. Try again; there's something wrong with him. Don't you see anything?”

Alexis feels himself all over, moving his hand over his legs, stomach, heart, chest, and head, but perceives nothing.

“Try again.”

“Ah! I've got it. But do you mean to say you worry about that? About this faint little speck that I can just see on the right extremity of the right eye? You think that it's the beginning of amaurosis, and he is miserable about it. It's true the doctors . . . but make your mind quite easy.

Don't do anything. Your son, I tell you, is in perfect health. He's now sixteen years and three months old ; at eighteen it will have disappeared."

"It's astounding," exclaims R. Houdin. "That's enough. Let us go away. Wake him up."

Marcillet breathes upon the somnambulist's face, the nervous symptoms which accompanied the entrancement reappear, but in reversed order, the vital functions gradually resume their ordinary course, and finally the medium returns completely to his ordinary every-day existence.

The two investigators withdrew in silent astonishment, and on reaching the street, I said :—

"Well, what about jugglery?"

"Monsieur, if the world contained a juggler capable of performing such miracles, he would, quâ juggler, astound me a thousand times more than the mysterious agent whom you have just shown me."

"If you like I will take you straight away to see ten others, and you will witness more or less the same phenomena."

"I assure you that there is no necessity."

"I can rely then on your keeping your promise."

"I am a man of my word, Monsieur, and no base promptings of self-interest or of vanity will have any effect on me."

"Quite so, and the moment I saw you I never doubted your good faith. But explain to me then why you smiled when the cards were cut before the first hand of *écarté*."

"I merely thought I noticed a coincidence between the cards cut and the number of cards required."

"But I always hear it said that your games of *écarté* and those of Alexis are as like as two peas."

"Ah! Monsieur, that may seem so to a man of no experience in these matters, to the ordinary person, (though even then such a mistake is hardly admissible), but to the expert!—Just consider, Monsieur, that all my cards are faked, marked, often of unequal sizes, or at least artistically arranged. Again, I have my signals and telegraphs. But in this case a fresh pack was used, which I had just taken out of its wrapper, and which the somnambulist cannot have studied. There is another point, where deception is impossible, namely, in the handling of the cards: in the one case, the entire artlessness of the performance, in the other, that tell-tale air of effort which nothing can entirely disguise. Add to that his total blindness, for need I insist on the impossibility—the absolute impossibility—of his having seen. Besides, even supposing he could see, how can we account for the other phenomena? With regard to my own 'second-sight' performances, without being able to divulge my secret to you now, bear in mind what I am careful to tell you every evening, that I only promise a *second sight*! consequently in my case a *first sight* is indispensable."

The following day R. Houdin gave me the following signed statement :—

"While I am by no means inclined to accept the compliments which M—— is kind enough to pay me, and while I am particularly anxious that my signature should not be held to prejudice in any way my opinion, either for or against magnetism, still I cannot refrain from affirming that the incidents recorded above are ABSOLUTELY CORRECT, and that the more

I think about them the more impossible I find it to class them with those which form the subject of my profession and of my performances.

“ May 4th, 1847.”

“ ROBERT HOUDIN.”

A fortnight later, I received another letter, as follows :—

“ Monsieur,—As I informed you, I was anxious to have a second sitting. This sitting, which was held at Marcillet’s house yesterday, proved even more extraordinary than the first, and has left me without a shadow of doubt as to the clairvoyance of Alexis. I went to this séance fully determined to keep a careful watch on the game of *écarté*, which had astounded me so much before. This time I took much greater precautions than at the first séance; for distrusting myself I took with me a friend, whose natural imperturbability enabled him to form a cool judgment and helped to steady mine. I append an account of what took place, and you will see that trickery could never have produced such results as those which I am about to recount. I undo a pack of cards, which I had brought with me in a marked wrapper to guard against another pack being substituted for it. I shuffle, and it’s my deal. I deal with every precaution known to a man well up in all the dodges of his profession. It’s all of no use, Alexis stops me, and pointing to one of the cards that I had just placed in front of him on the table, says :

“ ‘ I’ve got the king.’

“ ‘ But you can’t possibly know yet; the trump card has not been turned up.’

“ ‘ You’ll see,’ he replies. ‘ Go on.’

“ As a matter of fact I turn up the eight of Diamonds, and his was the King of Diamonds. The game was continued in an odd enough manner, for he told me the cards that I had to play, though my cards were hidden under the table and held close together in my hands. To each lead of mine he played one of his own cards without turning it up, and it was always the right card to have played against mine. I left this séance then in the greatest possible state of amazement, and convinced of the utter impossibility of chance or conjuring having been responsible for such marvellous results.—Yours, etc.,

“(Signed)

ROBERT HOUDIN.”

“ 16 May, 1847.”

VI.

REVIEWS.

“*Some Peculiarities of the Secondary Personality.*” PROFESSOR G. T. W. PATRICK, University of Iowa. (*The Psychological Review*, Nov. 1898.)

This article by Professor Patrick, which is to some extent a Review of Dr. Hodgson's Report on Mrs. Piper's trance-phenomena, (S.P.R. *Proceedings*, Part XXXIII.), although not throwing much new light on the problems at issue, is, nevertheless, a very welcome utterance. For we have here, in one of the foremost psychological journals of the United States, a strong appeal to experimental psychologists to occupy themselves with the phenomena of automatism. The fact is, as Professor Patrick delicately hints, that there are a great many experimental psychologists in America,—solidly established in a great many “psycho-physical laboratories,”—and that they are not all of them precisely overwhelmed with important jobs to do. “It has been felt,” says Professor Patrick, “that to maintain the dignity of experimental psychology, this subject and certain related ones must be ignored—they have been almost uniformly kept out of American psychological laboratories, where infinite labour has been spent upon other probably less fruitful problems. But experimental psychology has now long passed its probationary period, and may quite freely choose its subjects for research; and at present there is perhaps no other subject promising to throw more light upon certain dark chapters in mental science than that of automatism.” Some of my readers may remember my often expressed hope that the generation of young American savants, whom psychology attracts, would some day let some of those “probably less fruitful problems” wait a little, and make a methodical attack upon the problems which a very few very busy persons have been gradually eliciting out of the mixedness of things for organised Science to deal with as best she may. That is what we pioneers desire;—the repetition of our experiments, the discussion of our provisional conclusions, with diligence and competence;—no matter how much or how little credit may be allowed to our own work. What we do *not* want, on the other hand (to speak at least for myself), is the hasty adherence, the facile eulogy which, indolently assuming that all we have even conjectured is positive truth, uses our *Proceedings* as a foundation for all kinds of whimsical speculations, without a particle of fresh work or fresh experiment to support them. For heaven's sake, I would say, do not accept our conclusions too easily; revise our methods and improve them if you can; re-travel again and again the early stages of our road! All this is needful, if a number of able young men (in whom is our hope!) are to feel that the subject is, indeed, *their own*;—a subject which they have pursued from its beginning, and in which they mean to make their mark.

Just of the required type, as it seems to me, is this paper of Professor Patrick's. He is not, of course, convinced of the agency of spirits of the dead (and I for one desire no easy convictions!); but he puts the questions which suggest themselves to all of us in a clear way, and he has himself made experiments of a kind to us familiar, in automatic writing. I quote his concluding passage, which may serve as the text for some brief comment:—

“As regards the various traits of the secondary personality, some of which have been referred to in this paper, it has been suggested by Mr. Podmore and others that they are instances of survival or reversion. One cannot indeed fail to be impressed by the similarity of these traits to what we know or conjecture about the primitive mind. The general low moral and intellectual tone of the communications, the vulgarity and mild profanity, the frequent impersonation of the medicine-man, Quaker doctor, Indian doctor, etc., the keen memory and dull reason, the vivid constructive imagination, the deception and prevarication, the unwavering belief in spiritism, and the superstitious devotion to amulets, trinkets, and petty articles of ornament or apparel, all point to an early stage in the evolution of mind. Even the peculiar intuitive power sometimes exhibited by the secondary personality may be compared to the superior intuition of woman, whose mental peculiarities are in general representative of the more stable, basal and abiding phenomena of mind. Both may point to some nearly extinct faculty no longer serviceable. Still other peculiarities suggest the same theory, such as the extreme suggestibility and motor force of ideas, marks of automatism and of the hypnotic state, and at the same time characteristic of the child and savage mind. In close relation to this is the peculiar intimate connection between ideas and organic, nutritive and circulatory processes, best shown in hypnosis, and common to this group of phenomena. In view of such facts as these, certain of the more simple physiological theories of double personality gain considerable plausibility, such, for instance, as the revival of disused and outgrown brain tracts, particularly perhaps those of the less specialised hemisphere. The frequent appearance in automatic writing of *Spiegelschrift*, which occurs also among children, lends some support to this view.”

I will endeavour to disentangle from this *résumé* some elements, as it seems to me, both of truth and of error:—

(1) “General low moral and intellectual tone.” I think that this very frequent criticism of automatic utterances depends largely on a misapprehension, which mistakes baffled effort and incoherence for lowness or savagery. The so-called Phinuit, for instance, who has been much attacked on this line, did undoubtedly fish for information and pretend to possess information, (though how far with definite intent to deceive, I cannot say), and did undoubtedly talk in the slangy, tiresome style of a man who has a very poor vocabulary at his disposal. But so far as I heard him, or heard of him, he was neither immoral nor stupid. On the contrary, he gave much advice on private and delicate matters; and that advice, so far as I heard it, was always both shrewd and kindly. I feel bound to say this: since from having often acted as amanuensis in Phinuit's days, I have heard many criticisms and admonitions of his, which naturally did not appear in printed

reports. Professor W. James and Professor Lodge, who also heard much of this intimate talk of Phinuit's, will, I think, agree in this view.

(2) The "mild profanity" of which Professor Patrick speaks is, in my experience, so mild that I confess that it has never succeeded in shocking me. The pencil, or planchette, will sometimes, as though in fatigued impatience, write *devil*, *devil*—just as it will write *no*, *no*, or *yes*, *yes*, or *Mary*, *Mary*. This means, I think, a mere inability to use the instrument (whatever the real instrument may be!) in any but rudimentary fashion. But if the word *devil* is received by bystanders as mysterious and interesting, the incoherent intelligence (whatever the real incoherent intelligence may be!) may even pluck up spirit to add the word *d—n*. Ignore the rubbish and it ceases to appear. But, indeed, in my view, the sources of automatic writings (confining ourselves to cases of genuine automatism) are far from being one. They may, e.g., originate in the subliminal self of the medium, or in the minds of still embodied other human beings present or absent, acting telepathically on the subliminal self of the medium, or from spirits, and there may be, and, in my opinion, in most cases usually is, an admixture of these various influences.

(3) "The unwavering belief in spiritism." Are we not in some danger of question-begging if we treat this belief as an indication of reversion to savage conceptions, when the actual problem is really whether that belief be true or no?

(4) "Superstitions, devotion to amulets," etc. I have nothing to say for amulets!—but I think that what Professor Patrick really alludes to here is Phinuit's custom of asking for "hair cut close to the head, or old hat-linings," as a way of getting "into the atmosphere" of the former wearers of hat or hair. Now, one might well wish to be summoned from the realms of day by some appeal more attractive than the posthumous traces of one's having worn a particular cricket-cap on a hot afternoon. But if the immaterial and the material worlds are really continuous, it is conceivable that what has been closest to the living brain may retain the most perceptible earthly indication of that special personality. We are dealing here with an empirical suggestion rather than with a traditional superstition.

(5) "In peculiar intuitive power," etc. This sentence seems to be a mere play on the word intuitive. A woman, say, with delicate half-conscious observation, and without formal reasoning, might have discovered (although Emma did not) that Frank Churchill was somehow secretly linked with Jane Fairfax. This would be called feminine intuition; but such a guess is absolutely different from the bits of knowledge which really need explaining in these trance utterances;—the utterance, say, of the names of dead strangers, to which nothing has led up. A woman is no better able than a man to tell you straight out the Christian name of your grandfather. There is nothing "stable, basal, and abiding" about such intuitions as *that*.

(6) And here I may touch on the general question as to whether these supernormal powers,—telepathy and telesthesia,—are instances of "survival or reversion";—reversion to primitive ancestors, survival of "some nearly extinct faculty no longer serviceable." I cannot understand the view of protoplasmic evolution here implied. Protoplasm (as the text-books tell us)

from its primary inexplicable irritability gradually develops specific sensibilities, in response to specific stimuli, and by the survival of individuals best fitted to secure food and to escape enemies. Thus faculty rises from touch and smell, which act near at hand, to sight, which acts at great distances. At what point would the struggle for existence develop, say, the power of symbolically cognising the death of a kindred individual beyond the range of sight? Such a faculty would be profoundly indifferent to a hedgehog or a crab. How could a savage acquire it? and of what use would it be to him? I am not aware that, except by a few Psychological Researchers, the gift is particularly valued even now.

Of course this line of argument takes us further still. I frankly find it inconceivable that at any period of man's history, this faculty of far-feeling, this cognisance of distant minds, should have been developed by the actual need and stress of earthly existence. I can imagine no gift which, for the ordinary purposes of living and propagating, would be either more unpractical or more unattainable. "Conscience," says Huck Finn, "takes up more room than all the rest of a person's insides, and yet ain't no good, nohow." If the moral sense acquired by our race after long and painful tribal training, thus often appears worse than useless to its possessor, what should we say of the policy of acquiring a faculty incomparably more elaborate and expensive than sight itself, whose upshot is to give its owner an occasional peep at a ghost? The case might be changed, could we show that savage races habitually acted with advantage on messages given through medicine men. I doubt, however, whether those messages are more often of practical use to them than trance utterances are to us.

But, in truth, to a deeper gaze, the problem of telepathy shows itself as something quite apart from questions of earthly use or advantage. Telepathy, in my own view, involves an opening out of inner vision, bearing a relation different from that borne by any human sense, certainly to space, and perhaps to time. It involves a *communicatio idiomatum*,—a contact and converse of minds as minds, different altogether from any known contact or converse of organisms. It belongs primarily,—as I needs must think,—to an environment quite other than that we know. It may be a vestige, a survival; but, if so, it is the vestige of prenatal faculty, the survival of something which existed in the universe before the individualisation of incarnate man.

(7) And now a few concluding words as to the possible preferential use of the right cerebral hemisphere in automatic messages. This suggestion was, I believe, first made by our valued correspondent, the late Rev. P. H. Newnham, who was struck with peculiarities, very unlike his wife's character, in certain messages which both he and Mrs. Newnham believed to proceed ultimately from some stratum of her mind. I developed this hypothesis in a long paper in *Proceedings*, Vol. III., p. 1 (January, 1885), dwelling especially on the rather frequent occurrence of *mirror-writing* (*Spiegelschrift*) in automatic script, as a clear indication of dextro-cerebral preponderance where it occurred. The suggestion was coldly received at the time, but in some recent papers in *Brain* and elsewhere I observe that the idea of an oscillation between the two hemispheres, as regards preponderance in verbalisation, is again renewed. And to me it seems that many automatic messages received since I first wrote—and those of Mrs.

Piper's especially—do increasingly suggest a difficult or *makeshift* usage of a human brain. The brain may conceivably be thus used in a makeshift way either by a subliminal stream of consciousness or by an external intelligence. On the assumption that there is an invading intelligence, that intelligence (as Dr. Hodgson somewhere says) is employing a kind of imperfect typewriter;—which, one may add, is also an erratic calculating machine—you drop your thoughts into it, and some of them come out somehow. But the machine has *holes* in it;—places where the thoughts which the spirit puts in drop through ineffectively;—and it has also strains or cramps in it;—places where it has got set or stuck in a way which the spirit cannot alter. This last fact resembles the well-known difficulty in the way of making therapeutic suggestions to a hysteric. She has already given *herself* such strong suggestions on certain points that no counter-suggestions of ours can shake her.

Now, I think it possible that our left hemispheres, having been more constantly used than our right hemispheres, may be more crowded and blocked (so to say) with our own already fixed ideas. An external intelligence, wishing to use my brain, might find it convenient to leave alone those more educated but also more preoccupied tracts, and to use the less elaborated, but less engrossed, mechanism of my right hemisphere.

If this be thus, we should have here a striking instance of light thrown upon our own being by the procedure of beings almost incredibly remote. But this would not surprise me. As I have elsewhere remarked, such an incident would resemble the light thrown upon the elements which constitute our familiar earth by spectroscopic analysis of the radiance of distant suns. It would be another step in the reconstruction of what once seemed so solid and self-contained—what now seems so diffused and unseizable—the personality of man.

“Humpty Dumpty”—the Self, *totus teres atque rotundus*—“sat on the wall” of the old psychology. “Humpty Dumpty has had a great fall.” “Not all the King’s horses nor all the King’s men”—nay, not the allied battalions of the Great Republic—“can set Humpty Dumpty up again.” Yet they may find, perhaps, that only his shell is broken, and that something has somehow hatched out of him which, instead of sitting on his wall, will fly over it.

F. W. H. MYERS.

The Psychology of Suggestion: A Research into the Subconscious Nature of Man and Society. By BORIS SIDIS, M.A., Ph.D., Associate in Psychology at the Pathological Institute of the New York State Hospitals. With an introduction by PROFESSOR WILLIAM JAMES, of Harvard University. pp. X. +386. New York: D. Appleton and Co., 1898.

The book before us is a type of work which should be encouraged. It is an attempt to establish on a basis of observation and experiment facts which have too long suffered from the failure to apply to them exact methods. The general subject of subconsciousness, with which the author deals, is naturally elusive, and capable of varied interpretations. It is for this reason that it demands our most unprejudiced and exacting research, in the

light of recent advances in the anatomy and physiology of the nervous system. This is Sidis's attitude towards the problem he has undertaken to solve ; he is critical throughout his discussion, excepting where demands for explanation of psychical facts lead him too definitely to the assumption of underlying physical causes, as, for example, in his free adoption of the retraction theory.

The book is divided into three parts : Suggestibility ; the Self ; Society. Each of these demands consideration.

After a brief discussion of the careless way in which the term "suggestion" has come to be used, Sidis commits himself to the following definition (page 15) : "By suggestion is meant the intrusion into the mind of an idea ; met with more or less opposition by the person ; accepted uncritically at last ; and realised unreflectively, almost automatically." It is evident that this definition will not pass wholly unchallenged. The fact of opposition is certainly not essential to suggestion ; not infrequently the most powerful suggestion is that which is accepted uncritically at first, and to which not the slightest opposition is made.

An interesting series of experiments into the evidence of normal suggestibility is given, from which the conclusion is drawn that normal suggestibility exists as an undoubted fact, and that "*Man is a suggestible animal, par excellence.*" Following this is a critical analysis of both normal and abnormal suggestibility, with a discussion of the hypnotic state, and illustrative cases, all of which constitutes an excellent, brief presentation of matters of vital concern both to the neurologist and psychologist. The result of the analysis is that "suggestibility is a disaggregation of consciousness," in which the subconscious self enters into direct communication with the external world, hence the general law :—"*Suggestibility varies as the amount of disaggregation, and inversely as the unification of consciousness.*"

The next section of the book is concerned with the "Self." The argument already begun is carried to its conclusion, and the "subconscious" is given a place of dignity in the realm of mind as a consciousness of relative independence ; "it is a secondary consciousness, a secondary self." This conception Sidis reaches not by *à priori* methods, but by experiment with numerous subjects both in and out of the hypnotic state. He places his two personalities, as it were, side by side, and studies their character, their methods of inter-communication, and their general inter-relations. This is extremely well done, and while constantly feeling that the problems are being stated in too diagrammatic a way, the reader cannot fail to see the force of the argument, and the suggestiveness of its treatment. "The Problem of Personality" is the title of a chapter which is largely taken up with an adverse criticism of Professor James's theory of personality, and is of interest to those whose concern is with the more metaphysical side of the question. We cannot pass over the chapter on "The Physiology and Pathology of Sub-consciousness," without a word of comment. Sidis here sketches very briefly the new anatomical theory of the nervous system, and commits himself unhesitatingly to the so-called retraction theory. This chapter is superficial and dogmatic, and brings no adequate evidence in support of its position. A postulate so vague and so incapable of demonstration as the retraction of neuron terminations should certainly be approached

tentatively and not dogmatically. Such allegiance to an unproved, and to many minds fanciful theory, is not in accord with the spirit of scientific exactness, and repels the unprejudiced reader.

The author is on much safer ground when he discusses in the following chapter the case of a Rev. Thomas C. Hanna, in whom a complete amnesia followed an accident. The history of this case is worked out in admirable detail, though given merely in outline. By means of a method which the writer calls "hypnoidization," he was able to prove that the patient had all his lost memories stored in his subconscious mind, which could temporarily be brought to the surface of consciousness. By degrees the two personalities, which had developed since the accident, were fused into one, and the patient completely cured. This case must be regarded as one of the most valuable contributions which the book contains. Its practical bearing needs only to be mentioned to be recognised. Much more regarding the subconscious self is of great interest, as, for example, its characteristics, as distinguished from the waking self, and its relations to insanity. These are, however, matters for which we must refer the interested reader to the text for further detail.

The volume concludes with a section on "Society," in reference particularly to social suggestibility. This is by all means the most entertaining part of the entire book, and is most cordially to be recommended to the student of social movements. The psychology of the mob and the crowd, and mental epidemics, are described in a highly original and graphic manner. Some of the author's statements are extreme, and must be condemned by a sober judgment, as, for example, (page 362): "American society oscillates between acute financial mania and attacks of religious insanity"; or (page 363), "Woman cannot leave long the routine of her life, the beaten track of mediocrity; she can rarely rise above the trite: she is a Philistine by nature." These and many other statements are unnecessarily sweeping, but the faults of these chapters are so inherently a part of their excellence that we refrain from further criticism. Were Sidis not an extremist the chapters might better have been left unwritten. As it is, they are full of suggestiveness and undoubted truth, in spite of the occasional extravagance of expression.

In general, we have no doubt the book will fill a place and supply a want. It has in a rare degree the qualities of originality and forcible expression, tempered by a guarded scientific spirit, except in a certain tendency toward a too free acceptance of unproved theories of physiological nervous action. The author has done a good service, however, in stating clearly a doctrine of consciousness, and substantiating his claims by an appeal to experiment, which is likely to throw light on various morbid mental states, notably hysteria, and so indirectly on the nature of mind itself.

An introduction by Professor William James adds to the interest of the book. It is noticeable that James is careful to avoid committing himself to Sidis's views, while clearly recognising the intrinsic merit of his work.

The typography and general appearance of the book are excellent. It is embellished by a few illustrations and charts, which tend to make clear the text, and has a good index.

E. W. TAYLOR.

Naturwissenschaftliche Seelenforschung. I. Das Veränderungsgesetz. Von RUDOLF MÜLLER. Leipzig. [1897.] pp. VIII., 168.

Das Hypnotische Hellseh-Experiment im Dienste der Naturwissenschaftlichen Seelenforschung. Band II. Das Normale Bewusstsein. Von RUDOLF MÜLLER. Leipzig. [1898.] pp. 173-322.

In this first instalment of his planned work the author seeks to sketch the foundations of a scientific method of the investigation of mind, and of the points of view from which it proceeds. (Vorrede VII.) The small progress of psychology beyond description to the real end of a science in explanation is ascribed, in spite of the late great activity in psychological study and laboratories, to the want of courage in breaking from old prejudices and speculative theories. (V. and VI.) As against this the author's method needs no hypothetical presuppositions, and will give to psychological phenomena a real explanation in harmony with the other results of exact investigation. (VII.)

The special interest of the book for our Society lies in the importance the author attaches to hypnotism and telepathy as psychological methods of investigation. "Even more than the successes of objective science, the hypnotic and occult phenomena have lately called forth a mental movement embracing ever wider circles, and which seeks to solve the riddle of the mind by an experimental investigation of these phenomena. Several learned societies, foremost the 'Society for Psychical Research' in London and the 'American Psychical Research' in Boston, and numerous clubs of all civilised lands, are concerned with this kind of experimental study of mind, which direction has already found numerous zealous literary champions. But here again the old fault has not been avoided, since the temptation has been too little resisted of seeking the goal on the wings of uncertain speculation, instead of trusting alone to the gradual and slow, but so much the surer, progress of experimental study. No wonder that the official representatives of psychology do not follow this flight. On the other hand, all honour is due to those few earnest psychological students who have the courage to extend their investigations to the hypnotic and—even though in the beginning with caution and sceptical predisposition—also to the occult phenomena, and to use, not speculation, but observation and verification of facts and experimental methods." (VI. and VII.)

Three divisions of psychology are made :—(1) Psychical physiology ; (2) Psychology in the narrow sense ; (3) Hypnology. (pp. 110-118.) It is the methodology of hypnology with which the author is chiefly concerned, and which he believes will be of such great service to psychology in the narrow sense. Stimulated, apparently, by the suggestion of Münsterberg,¹ and by the hypnotic experimental method of Dr. O. Vogt,² the author extends this introspection in hypnotism to the condition of clairvoyance in hypnotism.

¹ *Aufgaben und Methoden der Psychologie*, s. 120.

² *Die directe Psychologische Experimental-methode in Hypnotischen Bewusstseinszuständen*, Leipzig, 1897 ; abstract in *Proceedings of the Dritter Internationaler Congress für Psychologie in München*, s. 250-259.

But under clairvoyance seems to be included also thought-transference (pp. 152-3). The knowledge so reported is not to be taken with uncritical belief, but is to be proved and verified by the usual objective methods (pp. 156-7). Thus three methods of hypnotic clairvoyant introspection are made:—(1) Introspecting self-observation; (2) Introspection of some third person put into rapport by the hypnotiser; (3) Introspection of the hypnotiser. These three simple methods are also to be used in combination (pp. 158-163.) The author has used these methods some years (p. 154), and reserves for his second volume of this work a detailed account of his clairvoyant experiments in hypnotism and their foundation principles (pp. 139 and 158).

Yet from this introductory volume one has some glimpses of the author's own results and his real methods as compared with the above excellent plan. For "the fact of the existence of a subject, which is denied by pure psychology, the *Naturwissenschaftliche Seelenforschung* will undertake to prove" (p. 14).¹ Then specially, "it is the truly astonishing effects which come to light in the phenomena of hypnotism and clairvoyance which force us to recognise the real and actual existence of this something, called soul, within the human organism. . . . The psyche is nothing else, and can be nothing else, than a force" (p. 102). "It must here be expressly emphasised that under the idea of soul is not meant any spiritual substratum of life- and thought-force, but the force itself, i.e., that effective activity, evidencing itself pre-eminently in the matter of the nervous substance, which produces the process of life and thought" (pp. 102 and 103). The *Naturwissenschaftliche Seelenforschung* has no other problem than to investigate the laws of the changes effected by the soul-force" (p. 108).

Indeed, the discussion about this mental subject fills the greater part of this present introductory book, and it does not seem possible to reconcile the author's different standpoints and reasonings. For though the study of the brain mechanism as making possible and conditioning the mind is counted as one of the most important problems (p. 132), and though hypnotic introspection reports "without brain function, no consciousness" (pp. 154 and 156), yet the aim of physiological psychology in seeking to know the causal connection of motions in the nerves, in the hope of ultimately inferring therefrom the causal connection of mental processes, is considered a dream; and were it even reached, it "would neither satisfy our demands of knowledge nor afford an explanation for psychical phenomena" (p. 29; see also pp. 99, 112, 146-7).

The fact appears to be that the author's "subject" has been anticipated in this methodology volume by being carried back from the subsequent metaphysics to which he thinks all science leads. "Force, and therewith also matter, is physical; the substratum of force, the subject of activity, is metaphysical. . . . Since the subject of activity is metaphysical, so all branches of science lead to a natural metaphysics as the real basis of all natural reality" (p. 52; see also pp. 12, 33, 109, 168). This attitude is shown in his vehement protest that Du Bois-Reymond and Helmholtz have

¹ For a dramatic figure of the death of "psychology without a soul," see p. 4.

poured out the child (life-force) with the bath,¹ as compared with his highest admiration for the work of Du Prel (pp. 106 and 120). However different judgments may vary as to the possibility or value of real knowledge leading ultimately to any metaphysics, it is at any rate discouraging to find another author straying so soon from his scientific method.²

In the second instalment of this work the experimental results with a female clairvoyant medium are given; yet only scattered direct quotations of the medium's descriptions, and no attempt is made at giving a full record of even one sitting. When asked to look into her own or the hypnotiser's brain, the hypnotised medium, who was ignorant of such matters, gave the following characteristic descriptions:—"There is so very much together, and all in movement and alive; there and there and here something springs up, in many places at once, and then immediately again at other places, and they are interwoven through and over one another,—that is by no means easy to describe! . . . There are many and many little points like poppies, and thousands and thousands of little threads in which currents run forwards, backwards, from within upwards and downwards. This looks like the foaming on pouring out soda-water or beer" (p. 188). The author wondered if by these "little poppy points" was meant the pyramid and other nerve cells of the cortex, and considered these first descriptions so correct and exact, in spite of the primitive modes of expression, that his beginning doubts gave way to confidence in the medium (p. 189). Asked what that springing up at so many places occasioned, she replied slowly, "That makes thinking, speaking, hearing; for when I or you speak there arise such—how shall I name them?—little bits of balls or knots, I might almost say bubbles, but very small, now like the finest needle points, some larger, some smaller, variously formed" (pp. 189 and 190). Such popularly vague and figurative descriptions can be translated into almost any technical brain language.

Her scheme for the optic tract and chiasma is considered much more probably right from its simplicity and naturalness (pp. 209 and 210). Sensation is made out to be due to a double-sided fluctuation, resulting in a tension of the fibrillated nerve endings (p. 234), and consciousness from the union in the cortex of the biochemical hunger from the cerebellum with the positive fluctuations from the sense organs (p. 235). The cerebellum is the stomach of the nervous system, in that it secretes the nutritive fluid which is taken into the nerves through the fibrillated endings. (pp. 212, 195.) With her "accustomed precision" (p. 221) the heart is the seat of the feelings (p. 220), being dark blue in bitterness and light red in joy (pp. 288 and 291). Such fantastic anatomy and physiology is to be taken as true because it is not self-contradictory (p. 186). But in addition to this deductive logical test of metaphysics, the author awaits experimental verification for the clairvoyant descriptions which he could not bring into harmony with the already acquired scientific knowledge (p. 286).

¹ Cf. Hermann: *Lehrbuch der Physiologie*, 9te Auf. S. 4-8, for a historical sketch of the final elimination of "Lebenskraft" as an explanation in physiology.

² Aa, *e.g.*, in Hudson's *Scientific Demonstration of the Future Life*. See *Proceedings S.P.R.*, Vol. XII., p. 330.

With the difficulty of separating the medium's descriptions from the author's technical interpretation, and with the slight evidence for the existence of strict clairvoyance, anyway as beyond telepathy, these results can hardly be considered of any value. The value of the book is in its advocacy of more use of hypnotism in experimental psychology, and in many just criticisms of much modern psychology. Though the book is difficult reading owing to its metaphysical mixture, yet it is written for the laity, and it is hoped it will stimulate—especially in the author's country—to more knowledge and results in this important work.

HARLOW GALE.

La Telepatia. By DR. G. B. ERMACORA. Padova, 1898, pp. 150.

This "summary exposition of real studies on the transmission of thought" is a collection of the author's articles which have been appearing irregularly in the *Rivista di Studi Psichici*, edited by himself and Dr. G. Finzi. The sudden death¹ of Dr. Ermacora a year ago has left these articles somewhat incomplete as a review of the subject. Yet the book as it stands is a most hopeful and valuable work. It is hopeful as an evidence itself of the progress of psychical research which the author sketches in his Introduction. In this general progress it may be an encouragement to the workers in the S.P.R. to find that nearly 60 per cent. of all the citations and references are from English sources.

The work deals only with the experimental evidence for telepathy. An excellent introduction discusses the methods and the cautions against illusion and fraud. The evidence begins with the production of hypnotism at a distance and of telepathic perception in hypnosis. Before going on to the experiments in the normal condition there is an interpolation of some theoretical considerations on the various effects in telepathy. These are fortunately not concerned with speculations as to the means of telepathic transmission, but with the forms in which the telepathic influence manifests itself in the percipient. This analysis and classification gives the basis for the subdivisions of the experimental evidence which follows. These remaining sections are:—Alterations of function, perception of ideas, perception through internal sensory images, hallucinations hypnotic and post-hypnotic, dreams, waking hallucinations (with subdivision on hallucinations in general), emotions and alterations of personality, and motor perceptions. The author left the titles only of two planned chapters on post-hypnotic suggestions, and the phenomenon of so-called magnetic attraction.

The work then, as a complete review of the evidence for telepathy, is wanting in the spontaneous cases of veridical hallucinations, and in more than a passing notice of the Census of Hallucinations. Strangely enough, too, there is no reference to the mass of evidence in the case of Mrs. Piper.

¹ Noticed in the *Journal S.P.R.* for May, 1898, Vol. VIII., p. 224. See also the *Rivista di Studi Psichici*, Anno IV., pp. 103-108, for his portrait, and for a most interesting sketch of his specialised education in physics, with his subsequent attraction and complete devotion of his means and leisure to psychical research.

This oversight can hardly be intentional, because of her phenomena being classed as "clairvoyance," or because of their spiritistic form. For of his own very original and highly valuable study on dreams¹ produced through the "Elvira" personality of Signorina Marie M., he said: "It seems important to me to remark once for all that supposed spiritistic intervention is certainly able to become an important factor in explanations of telepathy, but one ought not now to make a distinction *a priori* (and founded on simple difference of form in phenomena) between messages transmitted by telepathy and messages transmitted by spirit intervention. We ought to remain bound to the definition adopted, according to which all communication between mind and mind that does not come by known sensory means is called telepathy." (*Lu Telepatia*, p. 74, note.) And as, against these omissions, the book offers only four cases which are not included in *Phantasms of the Living* or Mr. Podmore's *Apparitions and Thought-Transference*, the work cannot be regarded as so complete and thorough a review of telepathy as *Apparitions and Thought-Transference*. The high admiration for this work which Dr. Ermacora expressed in his thirteen-page review of it in the first number of his *Rivista*² in January, 1895, and his desire for its translation, had apparently been preceded by his beginning this series of articles which made up *La Telepatia*. And probably his form of popularisation and critical education in this difficult subject may be more fitted to his own country and readers.

Thus, in spite of its more or less necessitated incompleteness, *La Telepatia* is a work of most timely and even permanent value, a worthy monument—together with the three volumes of his *Rivista*—to its open-minded, untiring, and scholarly author. It is surely to be greatly hoped that the work in its collected form may hasten the renewed formation of an Italian Society for Psychical Research, to which the family of the author carry out his spirit in dedicating the income from this work.

HARLOW GALE.

Essays in Psychical Research. By MISS X. (A. GOODRICH-FREER) (London: George Redway. 1899. pp. XV., 330.)

Miss Freer, who, under the name of "Miss X.," has made several contributions to our *Proceedings*, tells us in the preface to this volume of essays that "the papers which are here collected from various periodicals in which they have appeared, are in every sense essays in Inquiry," and she states that she has "no claim to the attention of others, but such as they may grant to a lifelong experience of the phenomena under consideration." Miss Freer is indeed most at home in describing her own experiences and commenting upon them, and is, I think, seen at her best in the article entitled "How it came into my head. The Machinery of Intuitions"—which is substantially, as she reminds us, the reproduction of an article

¹ "Telepathic Dreams Experimentally Induced." *Proceedings S.P.R.*, Vol. XI., pp. 235-308.

² See Dr. Leaf's notice of the *Rivista* in *Proceedings S.P.R.*, Vol. XI., pp. 171 and 172.

published in these *Proceedings* (Part XXVII.) under the title of "On the Apparent Sources of Subliminal Messages." The other essays in the book are all reproduced—with some slight changes by omission or addition or re-arrangement, chiefly verbal in character—from articles by Miss X. which appeared in the magazine *Borderland*, published quarterly by Mr. W. T. Stead, the first number of which appeared in July, 1893, and the last, and expiring, number in October, 1897.

The book, as a whole, is rather of the literary than the scientific order, and deals with the subjects under consideration in the somewhat popular manner suitable to *Borderland*. This fact will doubtless make the present volume more rather than less interesting to many readers. Besides the article referred to above as republished from our *Proceedings*, the book contains, in addition to the Introductory sketch of "Psychical Research in the Victorian Era," papers under the following headings: "Haunted Houses," "Another Theory of Hauntings" (really about "The Land of Faëry," as it was originally called in *Borderland*), "On the Faculty of Crystal Gazing," "The Divining Rod, or the Faculty of Dowsing," "Hypnotism," "Obsession: or the Imperative Idea" (about *fads* and *idées fixes*), "Holywell—Psychic Healing: The Welsh Lourdes," "Saint Columba, the Father of Second Sight."

Of these articles, that on Crystal Gazing has special value owing to the practical experience of Miss Freer in this subject. On the Divining Rod, however, the reader will do well to refer to Professor Barrett's report published in Part XXXII. *Proceedings* S.P.R. It is odd that Miss Freer makes no reference to this in her book, although there are indications which suggest that in revising her article from its original form in *Borderland*, she was indebted for some of her new statements to that report. Of excellent reading is the account of Saint Columba, who, according to Adamnan (whom Miss Freer summarises), united various kinds of psychical gifts, from "thought-transference and clairvoyance" up to "premonitions" of things both small and great. Many instances are quoted by Miss Freer, from which I take the following:—

"The saint called two of the brethren, Lugbe and Silnan, and gave them this charge: 'Sail over now to the Malean Island, and on the open ground, near the seashore, look for Erc, a robber, who came alone last night in secret from the Island Coloso. He strives to hide himself among the sandhills during the daytime under his boat, which he covers with hay, that he may sail across at night to the little island where our young seals are brought forth and nurtured. When this furious robber hath stealthily killed as many as he can, he then fills his boat and goes back to his hiding-place.' They proceeded at once, in compliance with their orders, and found the robber lying hid in the very spot that was indicated."

"On another day a shout was given on the other side of the Sound. The saint . . . said: 'The man who is shouting beyond the Sound is not of very sharp wits, for when he is here to-day he will upset my inkhorn and spill the ink.' Diormit, his minister, hearing this, stood a little in front of the door, and waited for the arrival of this troublesome guest, in order to save the inkhorn. But for some cause or other he had soon to leave his place, and after his departure the unwelcome guest arrived. In his eager

haste to kiss the saint, he upset the inkhorn with the hem of his garment and spilled the ink."

"One day S. Columba was sitting by a well near the fortress of Cethirn with Abbot Comgell. Water was brought to them to wash their hands, and the saint said to the abbot: 'A day shall come, O Comgell, when the well whence this water now poured out for us was drawn will be no longer fit for man's use.' 'How shall the water of this be defiled?' said the abbot. 'From this,' answered the saint, 'that it shall be filled with human blood; for thy relatives and mine—that is, the people of the Cruithni and the race of Niall—shall be at war in the neighbouring fortress of Cethirn. At this well an unhappy relative of mine shall be slain, and his blood, mingling with that of many others, shall fill it up.' An old man present at the battle, many years after, referred to this prophecy, and pointed out its literal fulfilment in the dead bodies in the well and the death of S. Columba's kinsman."

With so much that is of interest in this collection of *Borderland* articles, it is to be regretted that Miss Freer has repeated an important misrepresentation of fact concerning Mrs. Piper, my correction of which had been previously brought to her attention. She gives her readers to understand (pp. X. and 21) that Mrs. Piper's trances involve "the extreme cost of personal suffering," and she speaks of "the convulsed countenance, the gnashing teeth, the writhing body, the clenched hands." Miss Freer gave utterance to similar remarks at a meeting in London, reported in the magazine *Light* for December 17th, 1898. In a communication to the same magazine for February 4th, 1899, I pointed out that her assertions on this matter were entirely baseless. I drew attention to the fact that "the convulsive movements which usually in past years marked Mrs. Piper's going into and coming out of trance" had ceased two years previously, and I referred to the statements which I had already made to this effect in the *Journal S. P. R.* for January, 1898 (p. 167) and in the *Proceedings S. P. R.*, Part XXXIII., February, 1898 (p. 409). I also emphasised the fact that Mrs. Piper's trances did not involve any personal suffering by quoting a statement from Mrs. Piper herself that she had never suffered any physical pain in connection with her trances, and that during the past two years she had experienced better health than before since she was thirteen years old. It is therefore surprising to find that Miss Freer in the volume before us repeats her misrepresentation, and to find further that the latest words which I have quoted above (from the paper on "Psychical Research in the Victorian Era," p. 21), do not occur in the original paper in *Borderland* (July, 1897). Miss Freer is evidently labouring under an *idée fixe*, which perhaps began between two and three years ago, and has become more strongly established since. That this is the explanation of her curious misstatements may appear to the reader who will compare what she says about Mrs. Piper in the book before us (as well as in her remarks reported in *Light* for December 17th, 1898) with what she wrote in *Borderland* for January, 1894,—“Character Sketch. Mrs. Piper”—and for July, 1896,—“The Progress of Psychical Research. Professor James's Address.” (Miss Freer's sittings with Mrs. Piper, it should be noted, were as long ago as December 7th to 9th, 1889.) Not only, in these articles, is there not the

slightest reference of any kind whatsoever to any personal suffering of Mrs. Piper, but it will be very plain to the reader that no thought of any such suffering had entered Miss Freer's mind ; and it seems that as late as July, 1896, Miss Freer wrote as follows, after quoting Professor James's now famous remark that his own white crow was Mrs. Piper : " It is well known that Mrs. Piper is Professor James's own particular show, as they say over in his country. We have learnt many things from Mrs. Piper, and are glad to feel that she is in hands so thoroughly capable of making the most of what she has to teach, as are those of Professor James and Dr. Hodgson." (*Borderland*, July, 1896, p. 310.)

That Miss Freer's recent utterances concerning Mrs. Piper must be regarded as unreliable may be illustrated by another instance. She recently stated that the few prophecies upon which Mrs. Piper had " ventured in regard to her had none of them ' come off ' " (*Light*, December 17th, 1898, p. 613), but several years previously and when her recollections were presumably more trustworthy, she stated that " four important prophecies " were made to her, that two of these had been fulfilled, and that the remaining two " were only prevented by my own will, and both were prominently brought before me as suggestions." (*Borderland*, January, 1894, p. 230.)

RICHARD HODGSON.

Mollie Fancher : The Brooklyn Enigma. An Authentic Statement of Facts in the Life of Mary J. Fancher, the Psychological Marvel of the Nineteenth Century. Unimpeachable Testimony of Many Witnesses. By ABRAM H. DAILEY, Brooklyn, N.Y. pp. XIII., 262.

This book consists of a rather disconnected narrative by Judge Dailey, abstracts of a diary kept by Miss Fancher's aunt, a series of signed statements made by friends, and a number of reprints of articles which had originally appeared in the daily papers. No attempt has been made to reduce this mass to a coherent whole. It abounds in repetitions and irrelevancies to such a degree that it is far from easy to extract from it a clear history of the very interesting case which it is designed to put on record. The main facts may be outlined as follows :—

Miss Fancher was born August 16th, 1848. As a child her health was good, but in March of 1864 it began to fail. She had " nervous indigestion," inability to retain food, " fainting spells," " weakness in the chest," and she " wasted away." May 10th, 1864, she was thrown from her horse and severely injured. In the course of the following summer the lower part of her body suddenly became paralysed. She seemed to recover from the effects of this accident during the autumn, but on June 8th, 1865, as she was stepping from a street car, the car started too soon, she was thrown to the ground, her skirt caught upon a projecting hook, and she was dragged some yards, again receiving serious injuries. This was followed by weakness, transient paralyzes, pain, cough, and hæmorrhages from the lungs. In February, 1866, convulsions appeared. Soon afterwards she lost, in rapid succession, sight, speech, and hearing. From that date to 1894, when the book was written, her history was that of a hysteric of the worst type—

anæsthesias, paralyses, contractures, and convulsions involving now this, now that organ of sense or group of muscles, and succeeding one another in bewildering variety. Her sight, however, was never restored, although in recent years she seems to be recovering some portion of it. Throughout this period normal sleep seemed to be replaced by "trance," in which the whole body became rigid.

In 1875, after a trance lasting a month, it was found that her memory of the nine years immediately preceding had been totally obliterated, the contractures which had marked those years had disappeared, and all the skill in embroidery, etc., which she had acquired during them was lost.

From 1875 to the date of the book Miss Fancher's memories were approximately continuous, the only exceptions being found in the lives of her secondary selves. She also remembers the events of her early life up to the beginning of the nine-year period. About 1878 a new memory-synthesis appeared, but soon vanished, and was not again observed until 1887. It was then named by her friends "Idol." Others of later date were named "Rosebud," "Pearl," and "Ruby." The "normal" Miss Fancher was named "Sunbeam," to distinguish her from these. Each of these memory-systems or personalities calls itself Mollie Fancher, possesses a portion of Miss Fancher's memories, and remembers its own previous occurrences. No one has any knowledge of any other save in so far as informed by other persons. "Rosebud" seems to be identical with Mollie Fancher as she was at six or seven years. "Idol" and "Pearl" are young girls of about sixteen, but neither recalls the first accident, while "Ruby," who seems to be about the same age, recalls the first, but not the second accident. "Idol" and "Pearl" are quiet, and not markedly unlike one another, while "Ruby" is vivacious, cheerful, and talkative. None of them possesses "Sunbeam's" acquired skill, and all are extremely unstable, appearing only during the night, and lasting but a few minutes.

During her years of blindness Miss Fancher has convinced her friends that she possessed supernormal powers of vision. It is claimed that she has repeatedly read sealed letters, described events at a distance, and found lost articles. She also believes that she sees the world of spirits, but is extremely reticent upon that topic.

One is disappointed to find that Judge Dailey adduces little evidence of value in support of these claims. He has, indeed, recorded the narratives of many witnesses whose truthfulness no one would question, but in not one of these narratives are the facts given with that attention to details and that care to avoid misdescription which the nature of the case demands. Many are vague in the extreme, and very few tell us how much time elapsed between the event and its committal to writing. To glance at only the best of these: Prof. Parkhurst submitted to Miss Fancher a sealed envelope containing a slip of printed paper, the contents of which he did not himself know. She told him it contained the words "court," "jurisdiction," and the numerals 6, 2, 3, 4. These he wrote in his notebook, took the envelope away still sealed, read Miss Fancher's statement to two friends, and in their presence opened the envelope. Miss Fancher's statements were found to be correct. But we are not told how large the type was, how many thicknesses of paper covered it, how Miss Fancher handled the

envelope, how long she had it in her possession, or whether she had it at any time when Prof. Parkhurst was not present.

Dr. Speir states that Miss Fancher once wrote for him upon a slate the contents of a letter which had just been brought to her by the postman, and was as yet unopened. We are not given copies of the two, nor are we told how much time elapsed between the event and the record, nor who wrote the letter, nor whether Miss Fancher could have known that Dr. Speir would be present when the postman came.

Miss Fancher once told Judge Dailey that she had seen him upon a given evening with a gentleman whom she described. After some difficulty Judge Dailey recollected that upon that evening he had been with a friend named Sisson. We are not told how he identified the evening. Some months later Judge Dailey, Mr. Sisson, and another person called upon Miss Fancher. She at first said she had not before seen either of the two gentlemen, but after a moment's reflection said that one, pointing to Mr. Sisson, was the man she had seen with the Judge. This is one of the best cases, as it is corroborated by Mr. Sisson, and it appears that his account was written only six months or so after the event. We would like to know, however, whether any further questions were asked Miss Fancher after she had first stated that she did not recognise Mr. Sisson.

In brief, the evidence which Judge Dailey has collected will seem satisfactory only to those who are already satisfied of the possibility of clairvoyance. It will do little towards establishing that possibility. And as Miss Fancher's clairvoyant powers are said to be much less keen now than they were some years ago, it is not probable that her case will contribute much of value to the evidence for the supernatural. One can readily appreciate the repugnance which Miss Fancher felt to submitting herself to the commission of experts suggested by the New York Medico-legal Society, but it is much to be regretted that her friends should have allowed the value of her case to be lost through mere negligence.

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OF THE

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PART XXXIV. VOL. XIV,

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