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PROCEEDINGS

OF THE

SOCIETY FOR PSYCHICAL RESEARCH.

VOLUME II.

(CONTAINING PARTS V-VII.)

1884.

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Errata.

In page 10, lines 14, 15, *for* "experiments performed under this last condition—a condition which precludes any unconscious guidance from the 'agent'"—*read* "experiments performed under conditions which preclude the possibility of unconscious guidance from the 'agent.'" And add the following note to the word "contact," in the eighth line from the bottom of the same page :—

It should be borne in mind that indications may be unconsciously given otherwise than through contact. *Cf.* Proceedings, Part I., pp. 18, 19, &c.

In page 55, line 15, *omit* the figure 1 before 657,000. And in line 25 of the same page, *for* 1,657,000ths × 10, read $\frac{1}{657,000 \times 10}$.

In page 250, line 20, *for the words*, "the probability that in the whole series there will be one such success is $\frac{8}{24}$, or $\frac{1}{3}$," *read* "the most probable result is that there will not be a single success in the whole series."

In the same page, line 25, *for the words*, "the chance of a single success in the series is three times greater than before; that is, it is 1," *read* "the probable number of successes in the whole series is 1."

Note to Page 226.

It should be stated here that besides the anagrams given in the text, Mr. A. obtained also other anagrams; as *wfvs yoitet* (Testify! vow!); *ieb iov ogf wle* (I go, vow belief!); and in reply to the question "How shall I believe?" *neb* 16 *vbliy ev* 86*e earf ee* (Believe by fear even, 1866). How unlikely it is that all this was due to mere accident may be seen by anyone who will take letters, (the vowels and consonants roughly proportioned to the frequency of their actual use,) and try to make up a series of handfuls *completely* into words possessing any grammatical coherence or intelligible meaning. Now in Mr. A.'s case all the *professed* anagrams (when legible) were found to be *real* anagrams, (with one error of i for e); some of the sentences were real answers to the questions; and not even the absurdest sentences were wholly meaningless.

SOCIETY FOR PSYCHICAL RESEARCH.

PROCEEDINGS OF THE GENERAL MEETING ON

Thursday, November 22, 1883.

The fifth General Meeting of the Society was held at 11, Chandos Street, Cavendish Square, on Thursday, November 22, 1883.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

I.

FOURTH REPORT
OF THE
COMMITTEE ON THOUGHT-TRANSFERENCE.

[The Report has been somewhat enlarged since it was read.]

Committee:—EDMUND GURNEY,* M.A.; F. W. H. MYERS,* M.A.; and PROFESSOR W. F. BARRETT,* *Hon. Sec.*

Since the last report was presented to the Society, the Committee on Thought-transference have been steadily pursuing their inquiries, and have not only obtained a considerable amount of additional evidence confirmatory of their previous work, but also evidence of a new and important character. Moreover, we are happy to find that the inquiry is beginning to be pursued by independent groups of investigators throughout the country; some of these have communicated with us and have courteously permitted us to examine their mode of experiment, and gladly availed themselves of the precautions upon which experience has taught us to insist. To Mr. Malcolm Guthrie, J.P., of Liverpool, we owe our warm thanks for his most capable and courteous co-operation, and we are glad to say that he has lately joined our Committee. Since the extensive and careful series of experiments published in Part IV. of our Proceedings, Mr. Guthrie has enlarged his range of experiments and obtained remarkable success in the transference of visual impressions —diagrams and the like—of tastes, and of pains. The only sort of distinct sensation of which the transference yet remains to be obtained, is that of *smell*,—there being, of course, special difficulties in so arranging the experiment that a "subject" shall have no opportunity of detecting

* THE COUNCIL HOLDS ITSELF GENERALLY RESPONSIBLE FOR THE REPORTS OF ITS COMMITTEES. AT THE HEAD *OF EACH REPORT THE* NAMES OF THOSE MEMBERS OF COMMITTEE WHO ARE *SPECIALLY RESPONSIBLE FOR ITS* COMPOSITION ARE MARKED WITH ASTERISKS.

by direct means any strongly odorous substance which the "agent" is smelling close by. In many of Mr. Guthrie's later experiments the *sole* "agent" has been himself, or one or other of the gentlemen engaged with him in the investigation.

We trust that Mr. Guthrie's example will stimulate others among our members to conduct experiments of the same kind. Not all investigators, indeed, may have similar advantages of wide command of "subjects" for experiment; but anyone among us may very possibly find in his own family circle some one "subject" of value; and it is, of course, in the family circle that it is easiest to secure that continual and patient repetition of experiments which seems essential to any marked success.

1. *Transference of Tastes.*

The experiments which we shall first detail have reference to the *transference of tastes.* This particular form of transferred sensation had been often experimented upon, by ourselves and by others, with "subjects" in the hypnotic state; but, so far as we know, Mr. Guthrie was the first person to procure the phenomenon when agent and percipient were both of them in a normal condition. There was no chance of collusion, conscious or unconscious; for the taste to be discerned was known to no one except the actual experimenters; and the sensations experienced were verbally described by the "subjects" (not written down), so that all danger of involuntary muscular guidance was eliminated.

The following series of experiments were made in Liverpool during the first week in September, 1883:—

A selection of about twenty strongly-tasting substances was made. These substances were enclosed in small bottles and small parcels, precisely similar to one another, and kept carefully out of the range of vision of the "subjects," who were, moreover, blindfolded, so that no grimaces made by the tasters could be seen. The "subjects," in fact, had no means whatever of knowing, through the sense of sight, what was the substance tasted.

Smell had to be guarded against with still greater care. When the substance was odoriferous the packet or bottle was opened outside the room, or at such a distance, and so cautiously as to prevent any sensible smell from escaping. The experiments, moreover, were conducted in the close vicinity of a very large kitchen, from whence a strong odour of beefsteak and onions proceeded during almost all the time occupied. The tasters took pains to keep their heads high above the "subjects," and to avoid breathing with open mouth. One substance (coffee) tried was *found to give off* a slight smell, in spite of all precautions, and an *experiment made* with this has been omitted.

The tasters were Mr. Guthrie (M.G.), Mr. Gurney (E.G.), and Mr. Myers (M.). The percipients were two young ladies employed in Mr. Guthrie's establishment, whom we will call R. and E. The tasters lightly placed a hand on one of the shoulders or hands of the percipients. During the first experiments (September 3rd and 4th) there were one or two other persons in the room, who, however, were equally ignorant as to the substance tasted. During the experiments silence was preserved. During the last 15 experiments (September 5th) only M. G., E. G., and M., with the two percipients, were present. On this evening Miss E. was, unfortunately, suffering from sore throat, which seemed to blunt her susceptibility. On this occasion none of the substances were allowed even to enter the room where the percipients were. They were kept in a dark lobby outside, and taken by the investigators at random, so that often one investigator did not even know what the other took. Still less could any spy have discerned what was chosen, had such spy been there, which he certainly was *not*.

A very small portion of each substance used was found to be enough. The difficulty lies in keeping the mean between the *massive* impression of a large quantity of a salt, spice, bitter, or acid, which confounds the specific differences under each general head, and the *fading* impression which is apt to give merely a residual pungency, from which the characteristic flavour has escaped. It is necessary to allow some minutes to elapse between each experiment, as the imaginary taste seems to be fully as persistent as the real one.

September 3, 1883.

EXPT.	TASTER.	PERCIPIENT.	SUBSTANCE.	ANSWERS GIVEN.
1	M.	E.	Vinegar	"A sharp and nasty taste."
2	M.	E.	Mustard	"Mustard."
3	M.	R.	Do.	"Ammonia."
4	M.	E.	Sugar	"I still taste the hot taste of the mustard."

September 4.

5	E. G. & M.	E.	Worcestershire sauce	"Worcestershire sauce."
6	M. G.	R.	Do.	"Vinegar."
7	E. G. & M.	E.	Port wine	"Between eau de Cologne and beer."
8	M. G.	R.	Do.	"Raspberry vinegar."
9	E. G. & M.	E.	Bitter aloes	"Horrible and bitter."
10	M. G.	R.	Alum	"A taste of ink—of iron—of vinegar. I feel it on my lips—it is as if I had been eating alum."
11	M. G.	E.	Alum	(E. perceived that M. G. was *not* tasting bitter aloes, as E. G. and M. supposed, but something different. No distinct perception on account of the persistence of the bitter taste.)

September 4 (*continued*).

EXPT.	TASTER.	PERCIPIENT.	SUBSTANCE	ANSWERS GIVEN.
12	E. G. & M.	E.	Nutmeg	"Peppermint—no—what you put in puddings—nutmeg."
13	M. G.	R.	Do.	"Nutmeg."*
14	E. G. & M.	E.	Sugar	Nothing perceived.
15	M. G.	R.	Do.	Nothing perceived. (Sugar should be tried at an earlier stage in the series, as, after the aloes, we could scarcely taste it ourselves.)
16	E. G. & M.	E.	Cayenne pepper	"Mustard."
17	M. G.	R.	Do.	"Cayenne pepper." (After the cayenne we were unable to taste anything further that evening.)

September 5.

18	E. G. & M.	E.	Carbonate of soda	Nothing perceived.
19	M. G.	R.	Carraway seeds	"It feels like meal—like a seed loaf — carraway seeds." (The *substance* of the seeds seem to be perceived before their *taste*.)
20	E. G. & M.	E.	Cloves	"Cloves."
21	E. G. & M.	E.	Citric acid	Nothing felt.
22	M. G.	R.	Do.	"Salt."
23	E. G. & M.	E.	Liquorice	"Cloves."
24	M. G.	R.	Cloves	"Cinnamon."
25	E. G. & M.	E.	Acid jujube	"Pear drop."
26	M. G.	R.	Do.	"Something hard, which is giving way—acid jujube."
27	E. G. & M.	E.	Candied ginger	"Something sweet and hot."
28	M. G.	R.	Do.	"Almond toffy." (M. G. took this ginger in the dark, and was some time before he realised that it was ginger.)
29	E. G. & M.	E.	Home-made Noyau	"Salt."
30	M. G.	R.	Do.	"Port wine." (This was by far the most strongly smelling of the substances tried; the scent of kernels being hard to conceal. Yet it was named by E. as salt.)
31	E. G. & M.	E.	Bitter aloes	"Bitter."
32	M. G.	R.	Do.	Nothing felt.

* In some cases *two* experiments were carried on simultaneously with the same substance; and when this was done, the first percipient was of course not told whether her answer was right or wrong. But it will perhaps be maintained that, when her answer was right, her agent unconsciously gave her an intimation of the fact by the pressure of his hand; and that she then coughed or made some audible signal to her companion, who followed suit. Whatever the theory may be worth, it will, we think, be seen that the success of the second percipient with the nutmeg was the only occasion, throughout the series, to which it can be applied.

We should have preferred in these experiments to use only substances which were wholly inodorous. But in order to get any description of tastes from the percipients it was necessary that the tastes should be either very decided or very familiar. It would be desirable, before entering on a series of experiments of this kind, to educate the palates of the recipients by accustoming them to a variety of chemical substances, and also by training them to distinguish, with shut eyes, between the more ordinary flavours. It is well known how much taste is helped by sight and determined by expectation; and when it is considered that the recipients in these cases were judging blindfold of the mere shadow of a savour, it will perhaps be thought that even some of their mistakes are not much wider of the mark than they might have been had a trace of the substance been actually placed upon their tongues.

The interest and novelty of the foregoing experiments consist in the fact, already mentioned, that the "subjects" were apparently in their normal waking state. It has long been on record that such transference of impressions may take place between a mesmeriser or hypnotist and a sensitive "subject;" and some instances will be given in the subjoined report of the Mesmeric Committee. But here no preliminary mesmeric passes nor fixation of the eyes had been resorted to; nor indeed have the "subjects" ever been mesmerised.

2. *Transference of Pains.*

The same subjects, Miss R. and Miss E., who proved sensitive to the transference of tastes, were also found sensitive to the transference of pains whilst in their normal waking state. Mr. Guthrie has made a number of experiments in this direction, the record of which he hopes to present to the Society shortly. Here, too, the results are paralleled by those of hypnotic experiments, of which some have been already recorded (*Proceedings*, Part III., p. 225-6) and others will be described in the ensuing Report.

3. *Casual Experiments.*

We have received from correspondents in different parts of the country, records of more or less successful trials made amongst members of their own families. As a rule, however, these experiments have been too few and too fitful to be of much value; at the same time they are of interest as affording information that the faculty of Thought-transference is probably tolerably widely diffused. Here, for example, is the result of a casual trial by one of our members, the Hon. Mrs. Fox Powys, as "percipient," with her husband as "agent."

'*I send the results of a trial* my husband and I had alone. To me it *seemed like magic! We had tried*, I think, three times before this, with

indifferent success. I was the guesser, and he held my left hand with his right, and merely thought of a number. I sat with my eyes closed. The rapidity with which the thing was done astonished us; the number seemed to flash instantaneously into my brain. In fact, so simultaneous was it that I began to think that I, perhaps, had impressed the number upon Colonel Powys' brain first. However, when we reversed the operation, and my husband guessed, he was not at all successful. Here are our experiments; the complete series, including failures as well as successes, is given.

"First we tried single figures, and only one 'guess' was allowed.

NUMBER THOUGHT OF.	RESULT.
3	I perceived nothing.
2	I answered 2.
4	„ 7.
6	I perceived nothing.
5	I answered 5.
9	„ 9.
3	„ 3.
8	„ 8.

"Here out of 8 trials 5 were right. We now tried numbers of two figures, and where a second 'guess' was given it is noted.

NUMBER THOUGHT OF.	RESULT.		
58	24		
36	36		
27	72	On a second guess,	27
69	28	„ „	82
100	100		
42	42		
55	55		
22	79	„ „	97
38	38		
30	42	„ „	78
22	120	„ „	20

"Here 5 were right the first time, and 1 the second time,* out of 11 trials. We tried again the next evening, but out of 20 only got 5 right on the first trial, and 2 right on a second trial. The secret of success the previous evening I believe to lie in the fact that I felt almost absolute confidence in my power to guess correctly, and to this height of confidence I have never since been able to attain."

The coincidences here recorded seem clearly beyond the power of pure chance to account for.

Another correspondent, writing from Brunswick Place, Leeds, sent to us an account of some successful experiments of his own, which, however, seemed probably due to mere muscular interpretation. We explained this to him, pointing out the precautions necessary to guard against the error. In reply he writes to us as follows:—

"I have made a number of experiments since writing to you last; perhaps the best of these was my finding a pin secreted in a purse which was

* *It will be noted that this sixth case was all but a success on the first trial—the digits being correctly given, but inverted.*

in my sister's pocket. I also found the number of a bank note correctly, and correctly discovered 10 figures out of 12. But these experiments were made when contact was permitted, or by my running the 'agent's' hand over a series of figures from 0 to 9. When I tried yesterday to tell the figures *without* contact, I failed completely.

"I then blindfolded my sister, aged 13, and placed a piece of pencil and paper before her. Then I drew, with a piece of chalk on a school slate, a simple diagram (a circle with a cross inside), without her hearing the scratching of the chalk. To my surprise she drew the enclosed,* in about a minute, there being no contact between us, and I giving no indication whatever, merely thinking and staring at the slate. I then made other experiments in a similar way, and enclose you the results."

These, and a few other diagrams subsequently received, made in all 10 trials, with diagrams of various random and irregular shapes. Of the 10 trials six were as good reproductions of the original as could have been drawn had the child seen the original drawing and attempted to draw it blindfolded. The remaining four bore less, but still some, resemblance to the original. The hon. secretary of the Committee has visited this correspondent in Leeds, and seen the way in which the experiments were made. As far as could be judged from such a visit no information could have reached the percipient through the ordinary channels of sense. The "agent" is a highly intelligent young man, and quite alive to the precautions necessary to be taken to avoid obvious errors of experiment.

The following is an extract from a letter written to one of us by Mr. R. Gibson, of Limerick. He has since supplied us with additional details, and tells us that the "subject" (who has a large apothecary's business), and several of the agents, had previously been quite incredulous as to the phenomenon.

March 20th, 1884.

On last Tuesday week we were trying some experiments, at a friend's house, and a Mr. Day who was there told the number thought of in five different cases, by five different people—told them one after the other without a bungle or any hesitation whatever. After these five consecutive trials he got a violent headache, and on trying again could not tell any more. He was successful by picturing to himself (with his eyes shut) a black-board, and the number seemed at once to stand out on it in white.

The headache is an interesting, but (we are happy to say) not a frequent feature of these experiments. The imagining of the black-board is a device worth noting.

* *Some of the drawings made in this series* will be engraved for the next number *of the Proceedings.*

The following account was received, some weeks ago, from Miss Crabbe (Gordon College, Chatham Street, Liverpool), a lady known to two members of the Committee:—

One evening, at a rectory where I have been staying, we were trying pin-finding, when I said I had seen much more wonderful things than that done, and told them of what is done in your Society, such as placing figures and other objects behind people, &c.

They were *very* sceptical about it, said it could *not* be done without trickery, collusion, &c., that it was nonsense, &c., &c., and we had quite a lively argument about it, for I stuck to my point, and vowed it could be, and *was* done. At last the rector said he would try it, for that if anyone was able to do it he would. He acted as agent, and his daughter as percipient. The latter was blindfolded and placed in the usual position; the background was a large piece of white cardboard, and on that I pinned a pink oblong card, and, to my astonishment, in about a minute or two's time Miss —— described it; for I must confess I expected a failure on account of the scepticism of all parties.

Of course this success slightly changed their opinion, and they tried again. The next object I placed up was a round fan or hand-screen, which was most accurately described; the order we proceeded in was 1st, colour, 2nd, shape, and 3rd, object. Object after object was tried, and *every* time was a success. They declared it was wonderful. Night after night we tried it, and the whole time I was there Miss —— never once made the *slightest* error, and often named the object after the lapse of a *second* or so, with no second guess.

We tried everything we could think of, including spoons, door-keys, oranges. In describing the latter Miss —— said: "It is something with a reddish tinge, not quite round but a little flat at the top and bottom. Oh, I see; it is an orange."

After the objects, we tried taste-transference. The effect was marvellous. Salt, sugar, nutmeg, &c., &c., were tasted by Mr. —— and transferred to his daughter.

Next we tried the number-reading, which was also a grand success; in this case we *sometimes* reversed the agent and percipient, Mr. —— acting as the latter (and not as the former, as on the previous occasions), and either Miss —— or I as the agent. Whichever plan we took proved successful, and we tried very many times. The percipient wrote the numbers down while the agent was in contact with either the left hand or the forehead.*

After the number-writing, we tried objects again, *without* contact. This was also done without an error, Mr. —— acting as agent and making a few downward passes while fixing his mind on the object to be described.

Next we tried reading sentences written on the background, *i.e.*, I wrote in large hand on a card "Don't kill dogs"; then, "Thou shalt not kill," both of which were read by Miss ——, with the exception of one word, where I stopped her on account of an interruption. Then Mr. —— acted as percipient

* This is not satisfactory, for the reason given at the bottom of p. 10.

and Miss —— as agent, and I wrote up, "Be quick." Mr. —— said, "B-e—be, q-u-i-e-t—quiet." "No," we said, "not quite right." "No," he said, "the last two letters are c-k, not e-t; it is 'Be quick.'" Miss —— however, never even made that much of a failure.

Of all the experiments I have seen performed, I *never* saw any to equal these, which were all so quickly and accurately made. I can tell you I felt elated at having turned "sceptics" into such clever performers.

In a second letter Miss Crabbe adds:—

When I proposed trying taste-transference, I said to Mr. ——: "It must be something that has no smell," whereupon he replied, "Well, write down the name of anything you wish me to taste on a piece of paper." I did so, writing the word "Ginger." He then put the paper in *his mouth*, and I suppose imagined he tasted the ginger, for in a minute or two Miss —— said what it was. The reason I did not mention this before is that I am not sure whether Miss —— actually *tasted* the ginger, or whether the *word* was impressed on her mind, and she felt bound to say it. This was the first experiment in taste-transference; afterwards I always gave the substance to Mr. —— to taste himself first, and then his daughter evidently did really perceive the taste.

In the *sentence-reading* I wrote up *first*, "Don't kill dogs," but some one entered the room before this was read at all, and an interruption ensued, whereupon I said, "Never mind that one now, don't tell us anything about it, and I will write another." I then wrote, "Thou shalt not kill." This was read correctly, and immediately afterwards Miss —— said, "I may as well tell you what I thought the first sentence was. Was it—Don't kill?" and then she made some remark about the last word being a short one or something of the kind, but I really don't remember what the remark was.

4. *Unconscious Muscular Action.*

The subject of "Thought-reading" is just now in a rather singular position; for it is obtaining immense vogue throughout the country by dint of public exhibitions which, however clever and interesting, have no claim to be considered "Thought-reading" at all. These exhibitions usually produce a perfect deluge of letters in the local journals, in which the "willing-game" and its results are discussed from every possible standpoint and in every possible key, by believers, disbelievers, and doubters. In the more scientific contributions to this correspondence, expression is usually given to three distinct views, each of which deserves serious attention.

Some contributors are certain to give an adequate explanation of the process of "muscle-reading"—an explanation which easily covers the successes of the public performers, as well as 99 out of 100 cases of success in the "willing-game."

[illegible]se is pretty sure to put forward the hypothesis

strongly suggested by the 100th case of the "willing-game"—where what is done is of so subtle or complicated a kind as to raise doubts whether unconscious muscular pressure, or rather *release* of pressure in a certain direction, is adequate to account for it; and one is tempted to look deeper for the springs of action, and to conceive the governance of one organism by another through some sort of nervous induction.

And finally, some prudent correspondent will point out that—as long as the form of experiment adopted is the performance of some *action*—the problem can never be solved as long as *contact* of any sort is allowed between the "willer" and the "willed"; and will perhaps do us the honour to refer to some of our own experiments, in which success in far more delicate operations than pin-finding and number-writing has been attained without contact.

It is clearly only to experiments performed under this last condition —a condition which precludes any unconscious guidance from the "agent" —that the word "Thought-reading" can be safely applied. That name, of course, in no way implies the absence of a physical basis for the phenomena; the theory of brain-waves (which would be only an instance of "nervous induction") has been suggested to supply such a basis. But of such a *physical basis* we know nothing; of the *psychical facts* we know a great deal, all of them being, in various forms, transferences of impression or idea from mind to mind otherwise than by the recognised sensory channels. The difference between these cases and the public exhibitions of muscular and tactile sensibility is, of course, fundamental; and it is unfortunate that the word "Thought-reading" should have become associated with the latter. Even for the genuine cases "*Thought-transference*" is a much better expression—the other term having apparently conveyed to some persons the notion that, if once the reality of the phenomena were established, we should all be able to "read" each other's secrets.

We must emphatically repeat, then, what we laid down in our first report,—that wherever contact is permitted, success in the performance of a desired *action* must be attributed to indications given by the "willer"— that his unconscious and involuntary variations in pressure are unconsciously and involuntarily, or consciously and voluntarily, interpreted by the percipient.* We have thought it desirable to make a series of experiments to ascertain what can be done in this way; and the results have been most striking, but not unexpected. The note on "Muscle-reading" by one of our members,

* The same objection naturally applies to all cases where the subject *writes down* something which is in the agent's mind—the *action*, due to unconscious guidance, being then the movements of the pencil or chalk. The objection does not apply to cases where the "subject" gives his notion of the "transferred impression"—word, number, taste, or whatever it may be—*by word of mouth*.

published in Part IV. of our Proceedings, shows the result of some such trials. The same gentleman has since given a lecture on the subject in the Ulster Hall, Belfast, where he performed the following muscular feat, as described by a local newspaper. "A £5 note was handed to Mr. W. Gray, who fixed the number of it in his mind, the lecturer being blindfolded. The blackboard was brought into requisition, having five sections or colours marked upon it. Mr. Sugden, whose right hand, holding a piece of chalk, was in contact with Mr. Gray's left, then made the figures 5, 5, 3, 4, 0 with great deliberation, and these were acknowledged as the correct number of the bank-note." Further experiments have been made by the hon. secretary of the Committee with another "subject." Numbers thought of have been written with perfect accuracy by the "subject," when the tips of the agent's fingers were allowed to rest on the hand that held the pencil, *provided* that the agent himself followed the movements of the pencil with his eye. When the "agent's" eyes were averted, and there was no more chance of unconscious guidance, failure was at once the result. Diagrams have been accurately reproduced in like manner, while failure has inevitably followed the closing or blindfolding of the agent's eyes. With a lady as agent, two drawings were even reproduced with a considerable degree of accuracy when her fingers lightly touched the "subject's" left hand, he holding the pencil in his right.

We may conclude with a practical suggestion. Public performances, such as those which are exciting so much interest throughout the country, have this advantage—that they invariably set people to work in private houses; and it would clearly be a great thing if this result could be made useful as well as amusing. We would venture, then, to suggest to those who feel drawn to the pursuit that, instead of repeating the old "willing-game," and merely re-proving what has been proved a hundred times before, they should devote themselves to experiments *without contact*, or else adopt some form of experiment where the "subject" has only to *name* an object or sensation—and so aid in the establishment of facts completely new to science. Cards, numbers, names, diagrams, all supply good forms of experiment; and we may remind our friends that convenient blank schedules for recording experiments may be had gratis by anyone willing to forward the work of the S.P.R., on application to the Assistant-Secretary.

II.

THIRD REPORT
OF THE
COMMITTEE ON MESMERISM.

[The Report has been somewhat enlarged since it was read.]

Committee:—W. F. Barrett,* M.R.I.A.; Edmund Gurney,* M.A.; Richard Hodgson, B.A.; A. T. Myers, M.D.; Frederic W. H. Myers, M.A.; Henry N. Ridley, M.A., F.L.S.; W. H. Stone, M.A., M.B.; George Wyld, M.D.; and C. Lockhart Robertson, M.D. and Frank Podmore, M.A., *Hon. Secretaries.*

In previous Reports, attention has been frequently called to the affinity between transference of impressions obtained when the "subject" is in a normal state, and those results which have been held to indicate a special sympathy or "*rapport*" between a hypnotist or mesmerist and a sensitive "subject." The latter class of sympathetic transferences has been far longer observed than the former: we may say, indeed, that the recognition of the special case preceded by about a century the recognition of the more general faculty. This was natural enough. Observations in this particular department of hypnotic or mesmeric effects reaped the benefit of all the other striking phenomena which hypnotism or mesmerism had to show. The "community of sensation" was only one more wonder in a veritable wonderland. Here, too, it often presented itself in a high degree of perfection, and (between suitable persons) with a high degree of certainty—in higher perfection and with higher certainty than we can often hope to realise in experiments where the "subject" is in a normal waking state. The narrowing down (so to speak) of the exercise of the faculty to a single channel—the fact that the *rapport* is confined to a single pair of minds—seems to concentrate and enhance the force of the influence. And in these cases, therefore, results were soon manifest the possibility of which, outside mesmerism, was scarcely dreamt of, and which have only lately been seriously looked for and distinctly obtained in cases where no specific *rapport* had been established.

There remains, however, one sort of transference where the relation of the hypnotic to the normal state is harder to establish than might at first sight appear, and presents a certain peculiarity. It is sometimes loosely called the transference of *volition*; but is, more accurately, an influence which takes effect on the *motor-system* of the "*subject*," either by originating or inl[illegible] motor impulses. This sort of transference seems to be spe[illegible] a the "subject" is in a

normal state. For we must be careful to avoid interpreting in this way those cases in the "willing-game" where a person, without contact with the "willer," does some action which the latter intends him to do; for in such cases it is, as a rule, merely the *idea* of the action that is conveyed to him; and he acts on the idea merely because he knows he is to do *something*. He is only acting in accordance with the spirit of the game if he does that of which the idea comes across him; but he does not feel any special *compulsion* to do it. Nor can the allegations of certain persons that, *e.g.*, they can make strangers in church or in a theatre turn their heads by "willing" that they should do so, be accepted as establishing even a *primâ facie* case; till success and failure in such experiments are accurately noted, *chance* must always be the readiest explanation of the results. We may provisionally say, then, that persons in a normal state seem to be little if at all liable to have their will dominated, or their actions dominated against their will, by the silent determination of another. There are, however, cases where persons who appeared to be in a perfectly normal state, and had not been subjected to any process of fixation or passes, have been impelled to do things by a power which they felt themselves unable to resist; always, however, through the will of some person who had been proved to possess strong mesmeric power. And outside the sphere of mesmeric influence, we have in our possession a few very striking cases of *undesigned* results, where a powerful and emotional desire has influenced the movements of an absent person in a way which it is almost impossible to parallel by a process of deliberate experiment. On the whole, then, the evidence seems to tend to the conclusion that these telepathic affections of the will, or of the motor-system, involve profounder changes in the person influenced than the mere transference of passive sensations; and that, as a rule, they demand in the "willer" either distinct *emotion* or some amount of that specific faculty which we have called "mesmeric."

1. *Transference of Motor or Inhibitory Impulses.*

To proceed now to the experimental work of this Committee:—It will be remembered that in our last report we recounted a series of experiments, in which a hypnotised "subject" had been made to answer, or not to answer, the question "Do you hear?" according to the unexpressed intention of the operator. A further series was carried out last November in his own house, at Dublin, by Professor Barrett; who, for convenience, will now speak in the first person.

The hypnotist was Mr. G. A. Smith. The "subject" was an entire *stranger to Mr. Smith*, a youth named Fearnley, to whom *nothing whatever was said* as to the nature of the experiment about

to be tried, until he was thrown into the hypnotic state in my study. He was then in a light sleep-waking condition—his eyes were closed and the pupils upturned—apparently sound asleep; but he readily answered in response to any questions addressed to him by Mr. Smith or by myself.

I first told him to open the fingers of his closed hand, or not to open them, just as he felt disposed, in response to the question addressed to him. That question, which I always asked in a uniform tone of voice, was in each case, "Now, will you open your hand?" and at the same moment I pointed to the word "Yes" or "No," written on a card, which was held in sight of Mr. Smith, but entirely out of the range of vision of the "subject," even had his eyes been open, which they were not. Without the slightest change of expression or other observable muscular movement, and quite out of contact with the "subject," Mr. Smith then silently willed the subject to open or not to open his hand, in accordance with the "Yes" or "No." Twenty successive experiments were made in this way; 17 of these were quite successful, and three were failures. But these three failures were possibly due to inadvertence on Mr. Smith's part, as he subsequently stated that on those occasions he had not been prompt enough to direct his will in the right direction before the question was asked.

The experiment was now varied as follows:—The word "Yes" was written on one, and the word "No" on the other, of two precisely similar pieces of card. One or other of these cards was handed to Mr. Smith at my arbitrary pleasure, care, of course, being taken that the "subject" had no opportunity of seeing the card, even had he been awake. When "Yes" was handed Mr. Smith was silently to will the "subject" to answer aloud in response to the question asked by me, "Do you hear me?" When "No" was handed Mr. Smith was to will that no response should be made in reply to the same question. The object of this series of experiments was to note the effect of increasing the distance between the willer and the willed,—the agent and the percipient. In the first instance Mr. Smith was placed *three feet* from the "subject," who remained throughout apparently asleep in an arm-chair in one corner of my study.

At three feet apart, 25 trials were successively made, and *in every case* the "subject" responded or did not respond in exact accordance with the silent will of Mr. Smith, as directed by me.

At 6 feet apart six similar trials were made without a single failure.

At 12 feet apart six more trials were made without a single failure.

At 17 feet apart six more trials were made without a single failure.

In this last case Mr. Smith had to be placed outside the study door, which was then closed with the exception of a narrow chink just wide enough to admit of passing a card in or out, whilst I remained in the study observing the "subject." To avoid any possible indication from the tone in which I asked the question, in all cases except the first dozen experiments, I shuffled the cards face downwards, and then handed the unknown "Yes" or "No" to Mr. Smith, who looked at the card and willed accordingly. I noted down the result, and then, and not till then, looked at the card.

A final experiment was made when Mr. Smith was taken across the hall and placed in the dining-room, at a distance of about 30 feet from the "subject," two doors, both quite closed, intervening. Under these conditions, three trials were made with success, the "Yes" response being, however, very faint and hardly audible to me, who returned to the study to ask the usual question after handing the card to the distant operator. At this point, the "subject" fell into a deep sleep, and made no further replies to the questions addressed to him.

Omitting these final experiments, the total number of successive trials at different distances was 43. If the result had been due to accident, there would have been an even chance of failures and of successes,—whereas in fact *there was not a single failure in the entire series.*

I subsequently made a series of a dozen successive trials in an absolutely dark room, conveying my intention to Mr. Smith by silently squeezing his hand, once for "No," twice for "Yes." Every trial was successful. When Mr. Smith was placed outside the darkened room, I handed him the card through a small aperture, which could be closed. Eight trials gave six results quite right, one wrong, and one doubtful. Afterwards 20 trials, made when Mr. Smith was recalled, and the room lighted, were all entirely successful. There was, I need hardly say, no contact between operator and "subject" in any of these experiments.

The difference in the power of the will of the hypnotist and that of any other person was strikingly manifest, and the proof of the existence of a peculiar "*rapport*" between operator and subject was simply overwhelming. I several times exerted my will in opposition to that of Mr. Smith—that is to say, willed that the "subject" should or should not respond, when Mr. Smith willed the opposite, both of us being equally distant from the "subject." In every case his will triumphed. As in the case of Mr. Beard, recorded in our last report, the "subject," on being aroused, stated that he had heard the question each time, but that when he gave no answer he felt unaccountably unable to control his muscles so as to frame the word.

It was noticeable that neither in the normal nor in the hypnotic state was this subject able to tell any word or number or describe any diagram thought of or viewed by the operator. Only his ability to act in a particular way could be controlled, and he was not susceptible to even the most rudimentary form of thought-transference proper.

The following shorter series with another operator, Mr. Kershaw of Southport, and with Mrs. Firth, a nurse, as "subject," though the precautions were less elaborate than in the case just recorded, was to an eye-witness almost equally satisfactory. For the trial was quite suddenly suggested to Mr. Kershaw by the experimenter, Mr. Gurney (who will, for the remainder of the report, speak in the first person). Not only was it planned out of Mrs. Firth's hearing, but Mr. Kershaw himself had some difficulty in understanding what was wanted. A variety of small circumstances combined to show that the form of experiment was entirely new both to operator and "subject."

The trial took place at Southport, on September 7th, 1883. Mrs. Firth, who had been previously thrown into a light stage of trance, was placed in a chair in the middle of a bare room. Mr. Kershaw and I stood about three yards behind her; and sight of us, or of any part of us, on her part was out of the question. The window was in the wall in front of her, but altogether on one side; and there were no reflecting surfaces in the room. I drew up the subjoined list of *yeses* and *noes*, and held it for Mr. Kershaw to see. He made a quiet connecting motion of the hand (not touching me, and being many feet from Mrs. Firth), when there was to be an answer, and an equally quiet transverse or separating pass when there was to be none. I attribute no virtue to the passes, except so far as they were a means of vivifying Mr. Kershaw's silent intention to himself. The passes were almost absolutely noiseless, and the extremely faint sound which they made was, from the very nature of the gentle motion, the same in every case. Complete silence was preserved but for my question, "Do you hear?" repeated time after time, in a perfectly neutral tone; and there did not appear to be the very faintest chance of signalling, even had there been an opportunity for arranging a scheme.

1. Yes..... Right (*i.e.*, Mrs. Firth responded).
2. No ..... Right (*i.e.*, Mrs. Firth did not respond).
3. Yes..... Right.
4. Yes..... Right.
5. No ..... Right.
6. Yes..... Right.
7. No ..... At first no answer, which was right: then I gave a very loud stamp, which provoked a "Yes."
8. *No* ..... Right.
9. *Yes*..... Right.

The next short series took place at my lodgings at Brighton, on September 10th, 1883. The operator was Mr. Smith, the "subject" a very intelligent young cabinet-maker, named Conway. Mr. Smith and I stood behind him, without any contact with him. I held the list, and pointed to the desired answer each time. The silence was absolute. I repeated the question, "What is your name?" in a perfectly neutral and monotonous manner.

1 Yes..... Right (*i.e.*, the "subject," said "Conway").
2 Yes..... Right.
3 No ..... This time the answer "Conway" was given; but when the *next* question was asked, the "subject" seemed unable to answer for some seconds, as though Mr. Smith's intention had taken effect a little too late.

4 Yes..... Right.	7 Yes..... Right	10 Yes..... Right
5 No ..... Right.	8 No ..... Right	11 No ..... Right
6 No ..... Right.	9 Yes..... Right	12 Yes..... Right

2. *Transference of Pains and Tastes.*

The following short series of experiments in transference of pains and tastes was carried out the same evening with the same operator and "subject." Conway sat with his eyes closed, in a tolerably deep trance. Mr. Smith and I stood behind him, without contact, and Mr. Smith preserved absolute silence. I from time to time asked Conway whether he felt anything, but of course gave no guiding hint or indication of whether he was right or wrong.

I pinched Mr. Smith's right upper arm. Conway at once showed signs of pain, rubbed his right hand, then passed his left hand up to his right shoulder, and finally localised the exact spot.

I silently changed to Mr. Smith's *left* arm. In a very few seconds Conway's right hand flew to the corresponding place on his own left arm and he rubbed it, uttering strong complaints.

I nipped the lobe of Mr. Smith's right ear. Conway first rubbed the right side of his neck close to his ear; he then complained of his right leg, and used threats. I then gave a severe nip to his *own* right ear, and he made no sign of any sort. He then rubbed close to the left ear, and finally localised the spot on that ear exactly corresponding to the place touched on Mr. Smith's right ear.

I now pinched the right side of Mr. Smith's right thigh. Conway, without receiving any hint that he was expected to feel anything, immediately began to rub the corresponding part of his *left* leg.

Mr. Smith now put a succession of substances into his mouth, according to my indications, still keeping behind Conway, and preserving total silence. I kept Conway's attention alive by asking him *from time to time what the taste* was like, but gave not the faintest

guidance, except in the single case of cloves, when—to see if Conway would take a hint—I asked if it tasted like spice and he said it did not.

Mustard.—"Something bitter." "It's rather warm."

Cloves.—"Some sort of fruit." "Mixed with spirits of wine." "Not like spice." "Tastes warm."

Bitter Aloes.—"Not nice." "Bitter and hot." "Sort of harshness." "Not sweet." (I had suggested that it was sweet.) "Not nice." "Frightful stuff." "Hurt your throat when you swallow it." "Bitterness and saltness about it."

Sugar.—"It's getting better." "Sweetish taste." "Sweet." "Something approaching sugar."

Powdered Alum.—"Fills your mouth with water." "Precious hot." "Some stuff from a chemist's shop, that they put in medicine." "Leaves a brackish taste." "Makes your mouth water." "Something after the style of alum."

Cayenne Pepper.—Conway showed strong signs of distress. "Oh! you call it good, do you?" "Oh! give us something to rinse that down." "Draws your mouth all manner of shapes." "Bitter and acid, frightful." "You've got some Cayenne down my throat, I know." Renewed signs of pain and entreaties for water.

The "subject" was now waked. He immediately said, "What's this I've got in my mouth?" "Something precious hot." "Something much hotter than ginger." "Pepper and ginger."

In the next series, which took place on September 20th, 1883, at Heckmondwike, in Yorkshire, Mr. and Mrs. Sidgwick were also present. Mr. Kershaw was the operator. The "subject," who had come to the place by the merest accident, was Mrs. King, the wife of a neighbouring clergyman. Mrs. King was in a light hypnotic trance, with closed eyes; and was questioned, as to what she tasted, by Mrs. Sidgwick, without the very slightest guidance—Mrs. Sidgwick not knowing herself what the substances were. Mr. Kershaw put the substances into his mouth, according to my directions, in another room. He then stood three or four yards behind Mrs. King, keeping absolutely still.

Cayenne Pepper.—"Dry." "Choking feeling." "Not so choking." "Sandy and dry; thirsty feeling." (I took Mr. Kershaw out to renew the dose.) "Thirsty, parched feeling." "Just the same"—though Mrs. Sidgwick, who thought it had been changed, had made a remark which would have naturally led her to believe that such was the case. Mrs. Sidgwick said, "Is it cool, hot, sour, acid, dry, or bitter?" "Hot, dry."

Mrs. King was woke, and said that she had a burning feeling in her *throat, and it was* [illegible] *her.* She took some water, and was then *re-hypnotised.*

Bitter Aloes.—"Something sour." "As if I'd been eating acid drops." Mrs. Sidgwick said "Not disagreeable," in a tone of agreement. "No." She was given some water—"Mouth's nasty."

Sugar.—"Slightly sweet, I think." "Mixture; rather hot, but sweet." "Just the same; rather sweet taste."

Alum.—"Something like sour-bitter." "More sour than bitter." "Don't know the taste." "Not particularly nice nor particularly disagreeable." "Gives a watery feeling." "Tongue feels watery." "Watery feeling, as if I had been tasting something sour." She was woke, and was given a little of the stuff; she said that was just the taste she felt in her mouth.

Cloves.—"Like nutmeg." "Spicy taste; nutmeg, or cinnamon, or some other spice."

Ginger.—"Saltpetre sort of taste." "Gone." "Salty taste slightly."

In both these series every experiment has been given, and there has been no selection of results. The results would not be convincing, if taken alone; especially since Mrs. King failed completely in similar trials on two subsequent occasions. But no one who had witnessed more conclusive experiments could doubt that the genuine phenomenon was exhibited here, though the faculty was imperfectly developed.

Two other trials made about the same time with two other "subjects," one with Mr. Smith and the other with Mr. Kershaw as operator, gave results about as satisfactory as the above. In both cases, as in the two series just recorded, Cayenne pepper was described as strongly burning.

In the middle of this latter series with Mr. Kershaw, I suddenly pinched the upper part of his left arm. The "subject," who was sitting in a trance about eight feet off, with her back to us, did not know what sort of experiment I was going to try, and was only asked by me "What do you feel?" She instantly started up, as if in great excitement, rubbed the *exactly* corresponding place on her own person, and complained of violent pain. Being woke, she said her arm was dreadfully painful, as if some one had nipped her. A few more experiments on tastes followed, and then I nipped Mr. Kershaw's left little finger, and asked the "subject" if she felt anything beside the taste. She began at once to complain of pain in her left side and arm, all down to the finger ends.

APPENDIX.

It may be convenient to our readers if we quote or refer to a few cases of transference of tastes and pains to hypnotised "subjects," recorded by previous observers.

Mr. Esdaile, who was for many years Presidency Surgeon in Calcutta, gives the following case of transference of taste between himself and a patient he had mesmerised. The subject was a young Hindoo, Baboo Mohun Mittre, who had been operated upon painlessly whilst in the mesmeric trance.

One day that the Baboo came to the hospital to pay his respects, after getting well, I took him into a side room, and mesmerising him till he could not open his eyes, I went out and desired my assistant surgeon to procure me some salt, a slice of lime, a piece of gentian, and some brandy, and to give them to me in any order he pleased, when I opened my mouth. We returned, and blindfolding Lallee Mohun, I took hold of both his hands : and, opening my mouth, had a slice of half-rotten lime put into it by my assistant. Having chewed it, I asked, "Do you taste anything ?" "Yes, I taste a nasty old lime :" and he made wry faces in correspondence. He was equally correct with all the other substances, calling the gentian by its native name, *cheretta* ; and when I tasted the brandy, he said it was *Shrâb* (the general name for wine and spirits). Being asked what kind, he said, "What I used to drink—brandy." For I am happy to say he is cured of his drunken habits (formerly drinking two bottles of brandy a day) as well as of his disease. (*Practical Mesmerism*, by J. Esdaile, M.D., p. 125.)

Rev. C. H. Townshend, in his *Facts in Mesmerism*, gives several examples. (See especially pp. 68, 72, 76, 122, 150, 151, 184.) The following experiments were made on a servant of his own, in whom he had produced the trance-condition.

Wine, water, and coffee were handed to me successively, in such a way as to prevent the patient from perceiving, by any usual means, what the liquors were. He, however, correctly named them in order. The order was then changed, and the results of the experiments were the same. Flowers were given me to smell. I was holding the patient by one hand at the time, but turning altogether away from him to a table, over which I bent, so as to interpose myself between him and anything that might be handed to me. He, however, when I smelt of the flowers, imitated the action, and on my asking him what he perceived, replied, without hesitation, "Flowers." Upon this, one of the party silently changed the flowers for a bottle of *eau de Cologne*, when he observed, "That is not the same smell; it is *eau de Cologne*." With the manner of conducting this experiment and its results, all who were present declared themselves perfectly satisfied.

"Three of my sleep-wakers," Mr. Townshend says in another place "could in no way distinguish substances when placed in their own mouths, nor discriminate between a piece of apple and a piece of cheese ; but the moment that I was eating, they, seeming to eat also, could tell me what I had in my mouth."

The next case is also one of Mr. Townshend's.

Did any one strike or hurt me in any part of the body when Anna M. was in sleep-waking, she immediately carried her hand to a *corresponding* part of her

own person. Then she would rub her own shoulder when mine was smarting with a blow, manifesting that the actual nerves of that part were, *pro tempora*, restored to their functions. Once an incredulous person came near me unawares, and trod upon my foot, which was quite hidden under a chair. The sleep-waker instantly darted down her hand and rubbed her own foot with an expression of pain. Again, if my hair was pulled from behind, Anna directly raised her hand to the back of her head. A pin thrust into my hand elicited an equal demonstration of sympathy.

Stimulated by Mr. Townshend's experiments, the Rev. A. Gilmour, of Greenock, made some experiments on one of his servants. He described the results in a letter to Professor Gregory (quoted in *Animal Magnetism*, p. 211), in which the following passage occurs :—

I could throw her into the mesmeric sleep in 40 seconds. She is able to tell what I taste, such as soda, salt, sugar, milk, water, &c., though not in the same room with me. When my foot is pricked, or my hair pulled, or any part of my person pinched, she feels it, and describes it unerringly.

Professor Gregory himself says (*Animal Magnetism*, p. 23) :—

I have seen and tested the fact of community of sensation in so many cases that I regard it as firmly established. No one who has had opportunities of observing this beautiful phenomenon can long hesitate as to its entire truth—such is the expression of genuine sensation in the face and gesture, besides the distinct statements made by the sleeper.

The following account is given by Dr. Elliotson in the *Zoist*, Vol. V., pp. 242-245.

I requested my butler to enclose, in five different packets of blotting-paper, salt, sugar, cinnamon, ginger, and pepper. These were wrapped in one common cover when given to me, and I handed them over to Mr. Scarlet, the eldest son of Lord Abinger, who gave me one packet after another, any that he chose, as each was done with by me. The Archbishop of Dublin and several clergyman and other friends were present.

When I put each into my mouth, I was ignorant of its contents, and learnt its nature as the paper became moistened and gave way. The first was salt, and I stood with it in my mouth at Mrs. Snewing's side and rather behind her, saying nothing. Before a minute had elapsed she moved her lips, made a face, and said, "Oh, that's nasty enough." "What do you mean?" "Why you've put salt into my mouth, you needn't have done that." I removed the packet of salt, and took another, which proved to be cinnamon. Presently she said, "Well that is odd; I never heard of such a thing; to put such things together into one's mouth!" "Why what do you mean?" "Why now you've given me something nice and warm, very pleasant, but you've mixed salt with it." The impression of the salt thus still remained. "What is it?" "I don't know the name of it, but it's very nice; it's what we put into puddings; brown, and in sticks." She puzzled a long while and then on my asking if it was cinnamon, "Yes, that's it," she replied, "How odd that I shouldn't recollect the name." I then removed it, and took into my mouth another packet, which proved to be sugar, and I observed that Mr. Scarlet very properly peeped into it before he gave it to me. After a minute or two she began, "Oh, that's very sweet; I like that; it's sugar." I removed it from my mouth and took another packet, which proved to be ginger.

After a minute or two she exclaimed, "Well, this is the funniest thing I ever heard of, to mix salt, and cinnamon, and sugar, and now to give me something else hot." "What is it?" "I don't know; but this is very hot too. It sets all my mouth on fire." In fact, I felt my mouth burning hot. After some difficulty, for she was puzzled between these conflicting impressions, she said it must be ginger, and went on complaining of the heat of the mouth. I took a glass of cold water, and she instantly said, smiling, "That isn't hot, that's nice and cool, it makes my mouth quite comfortable." "What is it?" "Why it's water; what else can it be?" The last packet was now put into my mouth, and proved to be pepper. She cried out, "Why you're putting hot things again into my mouth. It gets down my throat, and up my nose; it's burning me," and she soon declared it was pepper. I could scarcely endure it, and took a draught of water. She was instantly relieved, and said, "How cool and nice that is." She could not have seen what was doing had her eyes been open.

A gentleman now came beside me and pricked one of my fingers with a pin. She took no notice of it at first, but, after a few minutes, slowly began to rub the fingers of her corresponding hand, and at last rubbed one only, that corresponding with my finger which had been pricked, and complained that someone had pricked it. The back of one of my hands was now pricked. She made no remark, but remained in quiet sleep. The pricking was at length repeated at the same spot, and pretty sharply, in silence. Still she made no remark. We gave it up, and my other hand was pricked in silence. After a little time, she began to rub her hand, *corresponding with that of mine which was the first pricked*, and complained of its having been pricked at the very same spot as mine. Gradually she ceased to complain, and was still again. After the lapse of another minute or two, all the party observing silence, she complained that the other hand, corresponding with that of mine last pricked, was pricked, and wondered that any person should do so. This is a most remarkable circumstance; perfectly corresponding to the phenomena of sympathetic movements in the Okeys, which often came out so long after the movement of the operator had been made. Indeed after he, in despair of any effect, had made another motion for them to imitate, and when he was expecting the latter, the first would take place. It shows how easily persons ignorant of the subject and unqualified to make experiments may come to false conclusions, and set themselves up as the discoverers of failures and imposition. In my patients the movement given for sympathy and not productive of apparent effect has often come out again in a *subsequent sleep-waking*, the impression remaining unconsciously in the brain. The heat and taste of the pepper still remained in Mrs. Snewing's mouth, and she went on good-naturedly, as always, complaining of it. While she was complaining, I suddenly awoke her, and asked what she tasted and whether her mouth was hot. She looked surprised, and said she "tasted nothing" and her "mouth was not hot;" and she smiled at the questions.

A few weeks afterwards, I repeated these experiments with all the same precautions, in the presence of Mr. H. S. Thompson and Mr. Chandler, who are very accurate observers, Mrs. Thompson, and a few other friends. I stood quite behind her large high-backed leather chair. Mr. Chandler gave me the packets at his own pleasure, and, on tasting each, I wrote on a slip of paper what I tasted, and held up the slip at a distance behind her, that all might judge of her accuracy and my truth. These were the same articles as in the former experiments; but, as they were on both occasions taken at random, the order, of course, turned out to be different. In addition, Mr. Chandler gave me a piece of dried orange-peel from his pocket; and I tasted water and wine. She named each

article with perfect accuracy, and readily; remarking that it was very strange she once could not recollect the name of cinnamon. Indeed, on the first occasion, she described the taste and the external character and uses of the various articles with perfect accuracy, but hesitated in giving the names of the cinnamon and ginger and pepper; a fact showing that the sleepiness extended a little more over the mental powers than one might imagine. In a note sent me lately by Mr. Thompson are the following remarks:—

The patient's lips moved, and in a very short time after you had detected its nature, she appeared to taste it as well as yourself; and when it was anything disagreeable begged you would not put the nasty stuff into her mouth in this way. She told, without the slightest mistake, everything you tasted: salt, sugar, cinnamon, pepper, ginger, orange-peel, wine, water, and some others. Not a word was spoken by any of the party to each other, and the only question that was asked the patient was, what she had in her mouth that she complained of. After the spices, when you drank water, she seemed to enjoy it much, saying it cooled her mouth; but at other times, as you drank it very freely, she requested that you would not give her any more water, for that so much water was disagreeable to her. There were present, Mr. Chandler, Lord Adare, Baron Osten, a friend of his, whose name I do not know, myself, and my wife. We were all perfectly satisfied with the entire success of the experiments.

I then smelt eau de Cologne, without any noise. She presently said, "How nice; what a nice thing you've given me to smell." But she could not tell what it was; when I mentioned its name she recognised it. I did the same with water. She made no remark. I asked her if she smelt anything. She replied, "No, I don't smell anything; what should I smell?"

I put snuff to my nostrils: she almost immediately complained of snuff being given to her.

III.

AN ACCOUNT OF SOME EXPERIMENTS IN THOUGHT-TRANSFERENCE,

Conducted by MALCOLM GUTHRIE, J.P., and JAMES BIRCHALL, Hon. Sec. of the Literary and Philosophical Society of Liverpool.

By MALCOLM GUTHRIE.

After Mr. Irving Bishop's visit to Liverpool in the spring of this year, it became a favourite amusement to imitate his skill in pin-finding; and some persons also made experiments in reading numbers thought of by others, they themselves meanwhile being blindfolded. Out of experiments made in this casual way arose the systematic study which it is my business to recount in the present paper. A party of young ladies amusing themselves after business hours found that certain of their number, when blindfolded, were able to name very correctly figures selected from an almanack suspended on the wall of the room, when their companions, having hold of their hands, fixed their attention upon some particular day of the month. There, so far as the young ladies were concerned, the matter ended. They had their few evenings' amusement, and other occupations and interests supervened.

It so happened that about this time I read an article by Mr. F. Corder in the February number of *Cassell's Magazine*, which was written with such an air of truthfulness, and spoke of Thought-transference as a matter of such very ordinary experience, that for the first time I began to think that there must be some foundation in fact for the belief so confidently expressed. Up to that time I had been thoroughly sceptical, nor had I read any literature on the subject. I thereupon determined to try the experiments, as described in Mr. Corder's paper, upon my son, a nervous and susceptible fair-haired boy of ten years of age. Much to my astonishment, and his own, he named quickly and without difficulty objects which I placed behind him when blindfolded under the usual conditions. He, however, would not perform more than two or three experiments at a time, saying that it made him "feel queer." Moreover, after the first experiments, when I asked others to witness the curious phenomenon, he seemed disposed to ensure success by taking a sly peep at the object, which indeed made me suspicious of the whole thing. Under these circumstances of the boy's reluctance and the difficulty of imposing stringent conditions, I abandoned him as an unsuitable [illegible] or study, more particularly as *other satisfactory* means sh[illegible]ame under my notice. I,

however, at a subsequent period, tested my son's powers under proper scientific conditions with the assistance of Mr. Birchall; and we were both satisfied as to his possession of the faculty, although we did not consider him a useful subject for study.

It was after the discovery of my son's powers of receiving impressions that I heard of the casual experiments before referred to; and having tested the accuracy of the reports which I had received, I mentioned the matter to the Council of the Literary and Philosophical Society of Liverpool, asking for assistance for the purpose of a scientific study of the phenomena. Mr. James Birchall, the Hon. Secretary, to whom the subject was quite new, having, I suppose, some confidence in my good judgment, agreed to give me the advantage of his assistance, and we thereupon held our first meeting. From the very beginning it was arranged between us that Mr. Birchall should make a full and complete record of every experiment, and I have to tender him my grateful thanks for the exactitude and care with which this record has been kept. He has performed his office as a man free from any prepossessions, and simply as a matter of official duty. His suggestions have all been in the direction of stringency of conditions, and I may state that he very shortly became convinced of the *bona fides* of the experiments, and interested in the scientific prosecution of the study. I very much regret that he is not able to be present at this meeting as we had arranged, in order personally to present the valuable and interesting register of the experiments which he has prepared. I hope, however, that he will be able to attend some future meeting with a report upon experiments which we are continuing week by week.

And now as to the party of young ladies to whom I referred, as having discovered the possession of the power of Thought-transference. You must know that I am a partner in one of the large drapery establishments in the city of Liverpool, and that the young ladies are connected with one of the show-rooms of that establishment. A relative of mine (entered in Mr. Birchall's report as Miss C.), also in the business, had been present at the careless, offhand experiments made in imitation of Irving Bishop, and, recognising their genuine character, informed me of the circumstance, being aware of the experiments I had made with my son.

One of the most important considerations connected with my study has been that I have been made cognisant, through this relative, of the very beginnings of the affair. I have, as it were, been behind the scenes from the first, and through my representative have been informed of almost every experiment which has been made outside our weekly meetings,—although after the first month the young ladies agreed only to practise at these meetings, as I (rightly or wrongly) thought it desirable on considerations of health

to limit their work in this direction. Thus I have had the advantage of studying a series of experiments *ab ovo*. I had not to enter upon an examination of the achievements of people who had been working together for years; but have been acquainted with the origin and whole development of the manifestations and have witnessed the genuine surprise which the operators and the subjects have alike exhibited at their increasing successes, and at the results of our excursions into novel lines of experiment. The affair has not been the discovery of the possession of special powers, first made and then worked up by the parties themselves for gain or glory. The experimenters in this case were disposed to pass the matter over altogether as one of no moment, and only put themselves at my disposal in regard to experiments in order to oblige me. The experiments have all been devised and conducted by myself and Mr. Birchall, without any previous intimation of their nature, and could not possibly have been foreseen. In fact they have been to the young ladies a succession of surprises. No set of experiments of a similar nature has ever been more completely known from its origin, or more completely under the control of the scientific observer.

As to the young ladies concerned, I have known them all for many years, and am able to speak in the highest terms of their probity and intelligence. I know also that they have a high regard for me, and would not willingly lead me into any error.

The two "subjects," Miss R. and Miss E., are about 20 years of age, engaged at business all day from 9 a.m. to 6 p.m., in the same show-room, and they discovered their aptitude with their companions in the same room as operators or "agents." They do not meet elsewhere, as all the assistants live with their friends, and do not board on the establishment. The lady, Miss R——d, who is supposed to have the principal influence in conveying impressions, is the head of the room, and occupies a position of great trust and responsibility.

The experiments were commenced with simple shapes, such as diamonds, circles, triangles, &c., cut out of brightly-coloured ribbons, and exhibited upon a black background; also with cards, and letters of the alphabet printed in a bold type. Afterwards objects were introduced, and short words were formed of the letters. The first series of experiments were of a *visual* kind, and were very successful. They were conducted in the presence of Mr. Birchall, myself, several lady companions of the subjects, and usually one or two members of the Literary and Philosophical Society. In the earlier experiments the "subject" was in contact (that is to say clasped or touched hands) with Miss R——d or some other lady friend. I need not say that the "subject" was always effectually blindfolded, and that the object was placed in such a position that it could not be seen

by her, even if she were not so incapacitated for observation. These conditions apply to all the experiments, and, to save iteration, I state them once for all.

After a short time experiments of this description were performed without any contact whatever. This was a suggestion of my relative, and under her superintendence the first set of experiments with Miss R., under these new conditions, was completely successful, and will be found recorded in Mr. Birchall's report.* At the next meeting after this discovery, the experiments were tried without contact, and were almost uniformly successful. The party sat in a semi-circle before Miss R. or Miss E., the object being placed behind her, and the attention of all being concentrated upon it, the object was very speedily described and sometimes named. It is noteworthy that the idea or name of the object did not come first to the percipient, but the appearance seemed to dawn gradually upon the mind, and sometimes it only presented itself in its general features, so that very often it could only be described and not named. First the colour impression was received, then the general shape, and afterwards any special characteristic, and finally, the name. This was the usual order of the process. As an illustration, take the case of a blue feather. The "subject" said, "It is pale? It looks like a leaf; but it can't be a leaf—looks like a feather curled. Is it a feather?" Again a key was described as "A little tiny thing with a ring at one end and a little flag at the other, like a toy flag." Urged to name it, she said, "It is very like a key."

The foregoing is a summary of our work during the first few weeks. All our regular meetings were successful, but in our desire to exhibit our discovery to our fellow members, we had some extra meetings, which proved total failures, much to our chagrin and much to the disappointment of our subjects. It would appear that any mental disturbance on the part of the operators or on the part of the "subjects," due to anxiety to succeed, or to the novelty of the *entourage* of persons or things, very much interferes with the success of experiments. I may here remark that the result of our experience is that success or failure depends as much (if not more) upon the condition of the agent as upon that of the percipient. It has happened that after a complete failure before strangers, the agent and percipient have been almost immediately able to obtain a successful transference of a number of impressions—the previous failure having been probably due to the mind of the agent occupying itself with the presence of the visitor, instead of

* The first series of experiments was summarised and reported by Mr. Birchall at a meeting of the Literary and Philosophical Society of Liverpool, and the experiments referred to are included in his report. This report is a record of every experiment made—with only two exceptions, when the subjects were hurriedly called upon to exhibit their powers to callers.

being directed exclusively upon the object to be described. As regards the condition of the agent, I may say that although I have been very successful myself in giving impressions to each of the "subjects" without the presence of any other person, still, under precisely similar conditions, when I have not felt equal to the required effort of concentration, I have been unable to repeat the success. We have also found that wandering attention on the part of the agents is misleading; and it would, I think, be fair to explain in this way one or two failures of the following type—that although a certain article was placed for observation, a picture hanging on the wall a little above it was described. On account of the disturbance of the agent's mind as well as that of the "subject," it is judged that experiments in a drawing-room, before a large miscellaneous company, are not likely to be successful.

At the conclusion of the first series of experiments some new departures were made.

First we tried the experiment of producing an article in the absence of the subject from the room and after concealing it re-admitting her, and, after blindfolding and isolating her, asking her to describe the object we had been looking at. This experiment was tried both before and after the summer interregnum and was successful. Thus a lady's purse, in form of a satchel, with a bright metallic frame and steel-bar handle above, was thus described: "Is it something not quite square? Something bright in the middle. Is it a purse? There's something very bright at the top. Has it anything else over it? Don't know what this is—whether it belongs to the purse. I've lost it—is it a bag?" On another occasion a key was correctly named, and Mr. Johnson's gold watch-chain, hanging in a curve from his watch-pocket to a button-hole of his waistcoat, elicited the answer: "Is it a chain, a watch-chain, hanging from a pocket like this?" the patient then describing in the air a curve similar to that formed by the chain we had been looking at but which was now concealed. Then she added, "There is a little pendant at the end of it."

Proceeding a step further we agreed, in the absence of the subject from the room, to imagine some object, and, under similar conditions, to ask her to describe it. This experiment was also successfully performed, on several occasions, by both subjects. A gold cross, pine-apple, and other objects imagined in this way have been correctly named.

We tried also the perception of *motion*, and found that the movements of objects exhibited could be discerned. The idea was suggested by an experiment tried with a card which, in order that all present should see, I moved about and was informed by the percipient, Miss R., that it was a card, but she could not tell which one because it

seemed to be moving about. On a subsequent occasion, in order to test this perception of motion, I bought a toy monkey, which worked up and down on a stick by means of a string drawing the arms and legs together. The answer was: "I see red and yellow, and it is darker at one end than the other. It is like a flag moving about—it is moving. . . . Now it is opening and shutting like a pair of scissors."

We have also tried experiments in the transference of real or imaginary *pains*, which Miss R. is able to receive from Mr. Birchall. This branch of our studies has not as yet been carried very far, for want of time, although I think it more likely to throw light upon the nature of the mode of transmission than any other branch of the inquiry. In particular, it would be desirable to ascertain by observation or even experiment, if the part affected in the subject shows signs of physical change, such as contraction, tension, rush of blood, redness, or any other physical change similar to that produced upon the person of the agent in causing the pain.

At the conclusion of our Spring Session, we tried (without contact) the transference of names, short quotations, &c., all the company thinking of the word, or words. In this we met with but little success, but on one occasion, the proverb, "Time flies," having been thought of by the company, elicited the answer, "Is it two words?—is it 'Time flies'?"

On this occasion, seeing that the subject was so apt, I proposed to the company that we should think of a historical scene, and two experiments were made, which are published in Mr. Birchall's report. They were imperfect, viewed critically, each of them having been done at the second instead of at the first attempt; but if the fact of Thought-transference is accepted as proved on other grounds, they are suggestive of further experiments in the same direction.

We discontinued our experiments at the end of May, on account of press of business and arrangements for holidays; and I am told by the "subjects" that no experiments whatever were made until we resumed our meetings, towards the end of August, in preparation for a visit by Mr. Myers and Mr. Gurney. As I expected, after the interregnum we met with very little success, and I wrote to those gentlemen not to expect much under the circumstances. And, indeed, the "subjects" were able to do very little, and our visitors would have gone away disappointed, had not our inquiries taken the direction of experiments in transference of *tastes*. As to these you have just heard the report prepared by Mr. Myers, and he and his coadjutor appeared fully satisfied as to the genuineness of the transference of impressions.

I may add the results of a few casual experiments, made in the course of the last week in London, which illustrate the partial

transference of somewhat more complex visual impressions than most of those above described.

One evening I called Miss E. and a friend of mine, Mr. Lee, out of the room, and requested them to assist me in imagining the large stained glass rose-window in the transept of Westminster Abbey, opposite to which Miss E., Miss R., and I had been sitting at the service the same afternoon. I then asked Miss R. to say what object we were thinking of. After a while she said, "I cannot tell what you are looking at, but I seem to be sitting in Westminster Abbey, where we were this afternoon." After another interval she said, "I seem to be looking at a window," and again, "I think it is the window in the chancel with the figures." When afterwards told which window it was, she said that she did not see any window distinctly, and certainly not the rose-window thought of.

I next proposed another object, and decided upon something which had struck our attention in a lamp shop in New Bond-street, a lighted lamp with a stuffed monkey clinging to it—the lamp at the same time revolving, and the monkey moving a cocoa-nut, which was suspended from its foot. This experiment took a very long time, and was only partially successful. First Miss R. said she thought of a cat, or it might be a dog. After a while she said it was something long, dark, and hanging—describing the size and shape pretty well with her hands. Then she said that she saw something hanging straight down, and moving up and down. After the removal of the blindfolding, she looked at the gas chandelier, and said, "Was it not that?" and then immediately, "No, it was not that—it was a lamp and it was lighted." Asked if the cat she saw had anything to do with the lamp, she said "No."

The following completely successful results in the simpler forms of Thought-transference were also obtained on the same occasion as the above.

I proposed to Miss R. to tell a name thought of by myself, Mr. Lee, and Miss E., without contact and blindfolded. The name "Polly" was written on a card and passed round in strict silence. In a few minutes Miss R. said, "I can only think of Polly."

The name "Isabella" was then selected by me, and passed round silently. After a longer interval Miss R. said, "I don't know what it can be. Somehow I can only think of my own name." Asked what was her name, she said "Isabel."

Mr. Lee proposed thinking of a number, and as only single numbers had previously been thought of it occurred to me to take a double one —34—which I wrote down and passed round. Miss R. shortly said, "Are there two figures?" I said "Yes." "One is 4 and the other 3." She did not know whether it was 34 or 43.

Our endeavours recently have been towards the ascertainment of individual powers on the part of the agents in regard to each of the two "subjects." The test selected has been the production by the blindfolded "subjects" of copies of drawings placed behind them. An improved method has been to place the drawing on a stand, with a wooden back between the agent and the "subject," and the agent placing himself on the opposite side of a small table, either joins hands with the "subject," or, by preference, does not touch her at all, and gazes at the drawing until the "subject" says she has an impression thereof. The drawing is then taken down and concealed, the blindfolding is removed, and the "subject" being already provided with drawing materials, proceeds to delineate the impression she has received. In most of these cases, no one besides the agent and the "subject" has been present in the room, and the result is held to establish the relative power of each agent in giving off an impression of this kind. In this way it has been found that all the agents have been successful in giving information individually to each of the subjects, although the range of experiments is not yet complete, because some new operators we have introduced have not yet had time to develop or settle down properly to their work. However, it has been found that both Miss R. and Miss E. have been able to receive impressions of drawings singly from myself, Mr. Birchall, Miss R——d, Mr. Steel, President of the Literary and Philosophical Society of Liverpool, and imperfectly from two or three other gentlemen. Since my arrival in London, a very successful producer of impressions has been discovered in the person of one of your members, Mr. F. S. Hughes. Mr. Gurney was also successful at one of our meetings.

Under these circumstances about 90 drawings have been produced in Liverpool, and 60 in London. It is difficult to classify them. A great number of them are decided successes; another large number give part of the drawing; others exhibit the general idea, and others again manifest a kind of composition of form. Others, such as the drawings of flowers, have been described and named, but have been too difficult to draw. A good many are perfect failures. The drawings generally run in lots. A number of successful copies will be produced very quickly, and again a number of failures, indicating, I think, faultiness on the part of the agent, or growing fatigue on the part of the "subject."

The originals of the Liverpool experiments and the corresponding copies have all been mounted, and I shall be pleased to show them at the close of this meeting. Every experiment, whether successful or a failure, is there in the order of trial, with the conditions, name of "subject" and agent, and any remarks made by the "subject" specified at the bottom. Some of the copies exhibit the curious phenomenon of inversion. These drawings must speak for themselves. The principal

facts to be borne in mind regarding them are that they have been executed through the instrumentality, as agents, of persons of unquestioned probity, and that the responsibility for them is spread over a considerable group of such persons; while the conditions to be observed were so simple—for they amounted really to nothing more than taking care that the original should not be seen by the "subject"—that it is extremely difficult to suppose them to have been eluded. The results may thus be considered a solid addition to the evidence for the transference of impressions from one mind to another otherwise than by the ordinary channels of sense.

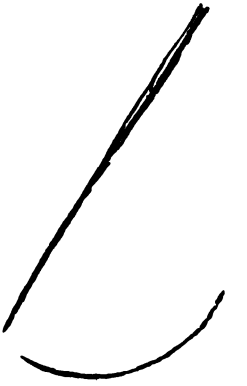
The originals of the following diagrams were for the most part drawn in another room from that in which the "subject" was placed. The few executed in the same room were drawn while the "subject" was blindfolded, at a distance from her, and in such a way that the process would have been wholly invisible to her or anyone else, even had an attempt been made to observe it. During the process of transference, the "agent" looked steadily and in perfect silence at the original drawing, which was placed upon an intervening wooden stand; the "subject" sitting opposite to him, and behind the stand, blindfolded and quite still. The "agent" ceased looking at the drawing, and the blindfolding was removed, only when the "subject" professed herself ready to make the reproduction, which happened usually in times varying from half-a-minute to two or three minutes. Her position rendered it absolutely impossible that she should glimpse at the original. She could not have done so, in fact, without rising from her seat and advancing her head several feet; and as she was almost in the same line of sight as the drawing, and so almost in the centre of the "agent's" field of observation, the slightest approach to such a movement must have been instantly detected. The reproductions were made in perfect silence, and without the "agent" even following the actual process with his eyes, though he was of course able to keep the "subject" under the closest observation.

In the case of all the diagrams, except those numbered 7 and 8, the "agent" and the "subject" were the only two persons in the room during the experiment. In the case of numbers 7 and 8, the "agent" and "subject" were sitting quite apart in a corner of the room, while Mr. Guthrie and Miss E. were talking in another part of it. Numbers 1-6 are specially interesting as being the complete and consecutive series of a single sitting.

No. 1. Original Drawing.



No. 1. Reproduction.

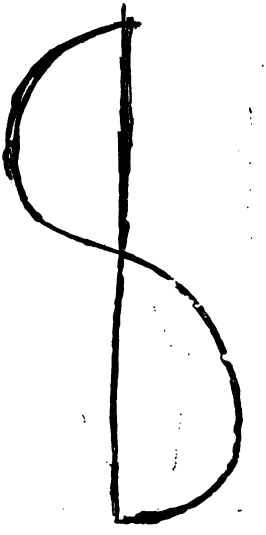


Mr. Guthrie and Miss E. No contact.

No. 2. Original Drawing.

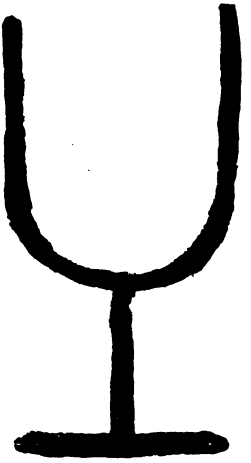


No. 2. Reproduction.

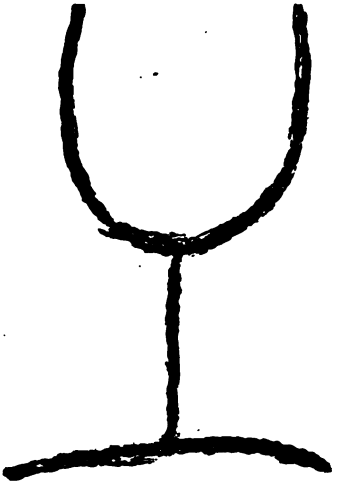


Mr. Guthrie and Miss E. No contact.

No. 3. Original Drawing.



No. 3. Reproduction.

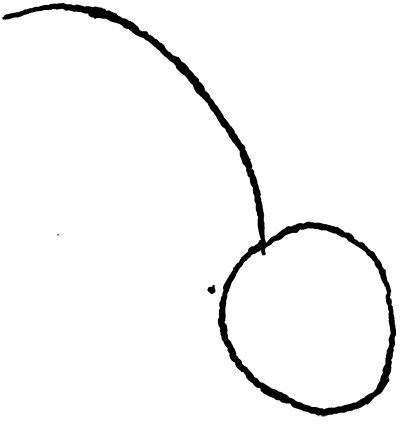


Mr. Guthrie and Miss E. No contact.

No. 4. Original Drawing.



No 4 Reproduction.



Mr. Guthrie and Miss E. No contact.

No. 5. Original Drawing.



Mr. Guthrie and Miss E.
No contact.

No 5. Reproduction.



No. 6. Original Drawing.



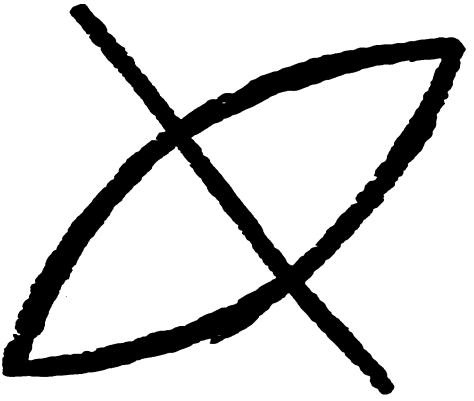
Mr. Guthrie and Miss E. No contact.

No. 6. Reproduction.



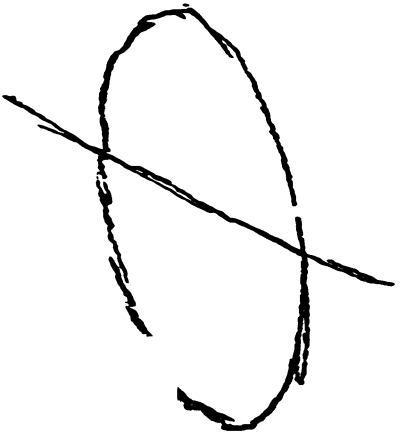
E. almost directly said, "Are you thinking of the bottom of the sea, with shells and fishes;" and then, "Is it a snail or a fish?"—then drew as above.

No. 7. Original Drawing.



Mr Gurney and Miss R. Contact for half-a-minute before the reproduction was drawn.

No. 7. Reproduction.



No. 8. Original Drawing.



No 8. Reproduction.



Mr. Gurney and Miss R. No contact.

No. 9. Original Drawing.



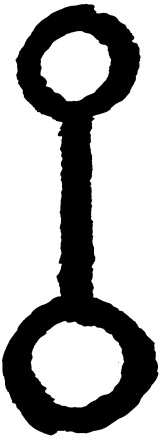
Mr. Birchall and Miss R. No contact.

No. 9. Reproduction.



. said she seemed to see a lot of rings, as if they were moving, and she could not get them steadily before her eyes.

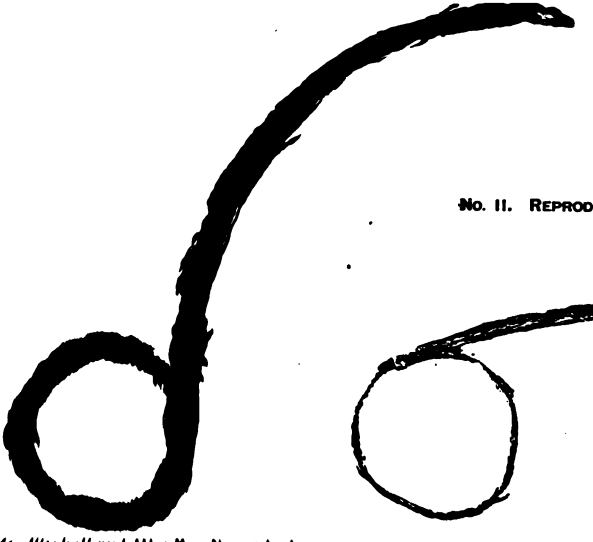
[illegible] Original Drawing. [illegible] Reproduction.





Mr. Marshall and Miss K. No contact.

No. II. Original Drawing.



No. II. Reproduction.

Mr. Marshall and Miss K. No contact.

No. 12. Original Drawing.

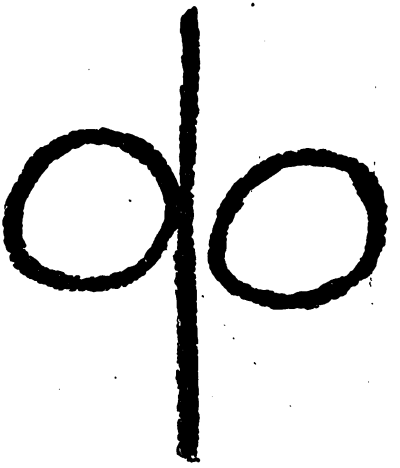


Mr. Steel and Miss R. No contact

No. 12. Reproduction.

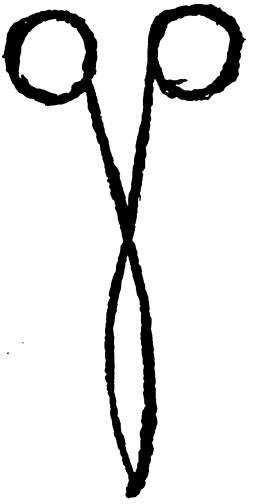


No. 13. Original Drawing.

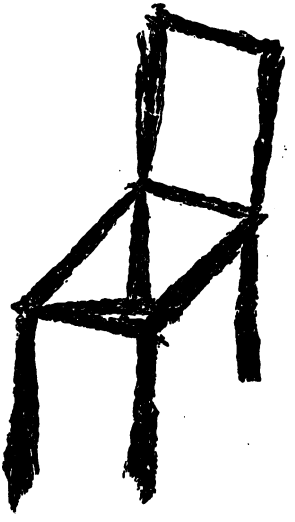


Mr. Steel and Miss E. Contact before the reproduction was made.

No. 13. Reproduction.

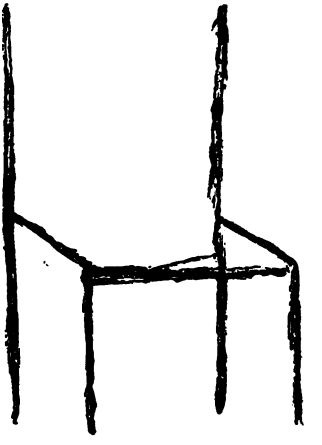


No. 14. Original Drawing.



Mr. Hughes and Miss E. Contact before the reproduction was made.

No. 14. Reproduction.



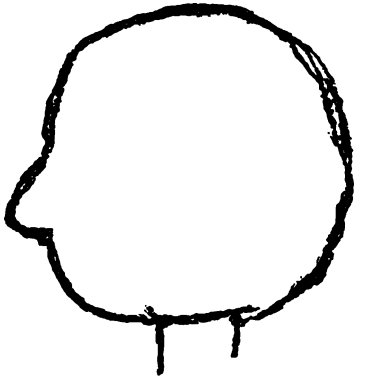
Miss E. said, "A box or chair badly shaped"—then drew as above.

No. 15. ORIGINAL DRAWING.



Mr. Hughes and Miss E. No contact.

No. 15. REPRODUCTION.



Miss E. said, "It is like a mask at a pantomime," and immediately drew as above.

No. 16. Original Drawing.



Mr. Hughes and Miss E. No contact.

No. 16. Reproduction.



PROCEEDINGS OF THE GENERAL MEETING ON

Friday, January 18, 1884.

The sixth General Meeting of the Society was held at 11, Chandos Street, Cavendish Square, on Friday, January 18, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

IV.

SECOND REPORT OF THE LITERARY COMMITTEE.

Committee:—W. F. BARRETT, F.R.S.E.; CHAS. C. MASSEY; REV. W. STAINTON MOSES, M.A.; F. PODMORE, M.A.; and EDMUND GURNEY, M.A., and F. W. H. MYERS,* M.A., *Hon. Secs.*

About a year has now elapsed since the first Report of the Literary Committee was presented to the Society. During that period the Committee has met on nearly 60 days, and its individual members have done much work for it between the meetings. The time seems now to have arrived when we may fitly give some account of the position in which our work at present stands.

First, as to the Society's Library. Many books of value have been presented to the Society, and a lady, who wishes to remain anonymous, has contributed £50 as the beginning of a Library Fund. A Library Sub-Committee has been appointed, and a catalogue of our collection, amounting at present to more than 300 volumes, has been published in Part IV. of our Proceedings.

It has been necessary so far to spend very little of the Society's own funds on books, and the books thus purchased have been almost all of them of a strictly scientific character. Among the books presented to us there is more variety; but on the whole, our collection contains a large proportion of rare and valuable works, which form a good investment, as for the most part they are already rising in price, and the growing interest in our subjects is likely to induce a further increment in their value. It will be seen that we are particularly strong in the older works on Mesmerism and Hypnotism, which contain many important observations, and are now very rare.

Our Library, as we conceive it, is primarily intended for serious

students of psychical topics. We wish to bring it to a point such that no one who is investigating these subjects with any thoroughness may be able to neglect the advantages which it offers. But we desire also to make it conveniently available for the less scientific reader, and we are gradually purchasing duplicates of such books as we find to be most often inquired for.

We shall be happy to receive suggestions as to books (especially foreign books and articles) which might advisably be purchased; and still more glad to receive gifts of such books or donations to the Library Fund.

To pass on to the more specific work of the Committee; the collection and examination of evidence.

A series of meetings of the Literary Committee, occupying the greater part of eight days, was held at the President's house, at Cambridge, August 20th-27th, 1883. The evidence bearing on Phantasms of the Living,—some 400 narratives which had been already printed on slips,—was then considered; and a resolution was passed, (and subsequently approved by the Council,) entrusting to Messrs. Gurney, Myers, and Podmore the composition and publication of a book to be entitled "Phantasms of the Living," to be published with the sanction of the Council, and as one of the publications of the Society.

The proposed scheme includes the apparitions at or about the moment of death, which are one of the most widely attested forms of such phenomena. The persons whose phantoms are discerned at this last hour may still rank among living men. But, even thus, our projected work is intended, as will be seen, to deal with a small part only of the field which we hope in time to traverse. That field is a dark territory, and full of pitfalls, and the safest way of moving across it is not to leap, but to crawl. We wish to make each foot of ground secure as we proceed, and never to lose our line of communication with familiar and established things.

Now our Society claims to have proved the reality of Thought-transference,—of the transmission of thoughts, feelings, and images from one mind to another by no recognised channel of sense. And here we find a starting-point from whence we can begin to consider *all* impressions which seem to be flashed from one human being to another,—*phantasms*, as we call them, in order to include under a term more general than *phantoms* impressions which may be not visual only, but auditory, tactile, or purely mental in character.

While we deal thus with living human beings, we feel that we have behind us a solid and increasing mass of experimental analogies, of which we shall make the fullest use, and which will supply us, as we venture to hope, with at any rate something nearer to a true philosophy of apparitions than it has till now been possible to frame.

Such is the task on which we are engaged; and of which we hope in the course of a few months to present to the Society some considerable instalment. As for the book itself, we shall aim at its publication in the course of this year; but the material grows on our hands, fresh problems present themselves, and fresh inquiries are undertaken, till the work which we once hoped to compress within one duodecimo volume seems now likely to extend itself over two volumes octavo.

In the meantime, the greater part of the narratives which form the substratum of the forthcoming book, are offered in printed slips to the inspection of Members and Associates in the Society's rooms. Those only are excepted which have been communicated under conditions which would make it a breach of confidence thus to submit them to view.

The cost of these slips, which will amount altogether to some hundreds of pounds, is defrayed by a member of the Society, who wishes to remain anonymous. Their convenience is manifest. They enable each member of the Committee to study each narrative, and to record his comment or criticism in the readiest way. It must, of course, be understood that the fact of printing or exhibiting these slips does not imply that any conclusion has been arrived at by the Committee as to the precise value of any one of them. They are at present lying open to discussion, and we should be very glad to receive any comments, additional particulars, parallel cases, &c., which any members of our Society who study them may be disposed to write on the slips, or to send us. Most of the slips already printed refer to phantasms of the living, but we are also collecting and printing similar narratives relating to phantasms of the dead, premonitions, and other subjects, for future consideration.

The labour involved in preparing these slips is very great. We calculate that over 10,000 letters have been written during 1883 in the course of the collection and verification of evidence. This may seem an extravagant number, and we trust that in future years the task will in some respects become an easier one. For the difficulty depends in great measure on the *novelty* of the inquiry, and consists in explaining to correspondents what is wanted, and in inducing them to hunt up dates, obtain corroborative signatures, &c., which in many cases they can easily do when once they have been led to perceive the all-importance of accuracy and precision in narratives of this kind. Much time and trouble have been expended on persuading correspondents to allow the publication of their names. We ourselves have, of course, the names of all our correspondents; but we naturally think it right to place the names before the public whenever we are permitted to do so. Some cases, of course, there are where we should not think of requesting such permission; cases where some painful or intimate circumstance

connected with the narrative makes anonymity plainly desirable. But it is not in these cases that our difficulties lie, but often in cases where the story is one which our correspondent would be willing enough to tell at any dinner-table. For the fact is, that persons unaccustomed to seeing themselves in print suffer still in many cases from a kind of unreasoning timidity which makes them dread unknown consequences if they allow the public eye to rest on their own name printed in a book, or on the name of their grandfather, or their mother-in-law, or the husband of a former cook. Our correspondents' letters, their very prejudices, are sacred in our eyes, or we could give some amusing instances of this coy reluctance in cases where no human being could derive any annoyance if the whole story were in the newspapers at once.

Much good has been done in this direction by the publication in the *Fortnightly Review* of our first Report, containing a number of signed narratives of apparitions. Not one of these narratives has been shaken, and not one of the writers has complained of any ridicule, notoriety, or other inconvenience as following on the publication of his name and address in this widely circulated periodical. But a wide change in this respect can only be induced by gentle and gradual persuasion;—we must not associate Psychical Research in the public mind with brusque interrogation or rash intrusion into the sacred places of memory.

Again, a large part of this correspondence is of a nature which cannot make much show in any book or report. It has consisted in the practice of the somewhat difficult art of courteous and unobtrusive cross-examination;—in endeavours to elucidate points where it seems to us that there is a risk that our correspondents' memory may have been dim or incorrect. We desire, of course, not only that all our narratives shall be the *bonâ-fide* depositions of trustworthy persons—(of that we think we can make sure)—but also that there shall be as few as possible of those little slips and inaccuracies into which the most trustworthy person sometimes falls, and which if subsequently discovered are represented (sometimes very unfairly) as vitiating the whole account.

And if to any of our correspondents we have seemed needlessly pertinacious, irritatingly pedantic in our constant requests to them to look out contemporary documents, to procure original signatures, to get the corroboration of a second person, which often seems so needless to a narrator, conscious that he is telling the exact truth;—in short, to attend to minutiæ on which we sometimes dwell so much that we may perhaps seem to have overlooked the main point of the narrative altogether;—if we have seemed to inflict on our most useful informants in this way trouble which might have been spared, we would ask their pardon, and we would assure them that the result of this minute verifi-

cation is by no means, as a rule, to throw doubt upon the narratives sent us, but much oftener to strengthen them in unexpected ways, and especially to exhibit those *undesigned coincidences* with other accounts of the same kind, which help to show that we have got hold of a plexus of coherent phenomena, and not a mere bundle of isolated and individual fantasies.

It is, however, an unfortunate peculiarity of the human race that they are very unwilling to answer many letters of inquiry on the same subject; and as we have no means except persuasion of getting any answers from anyone at all, it may be supposed that to bring any given narrative up to the required pitch of accuracy and completeness, may sometimes be a very tedious matter. But the very slowness and tediousness of the inquiry is in itself a safeguard, and correspondents who see how attentively we ponder over every expression which they use, feel themselves thus invited to a careful precision of statement which in many cases is really remarkable. A critic of the rough-and-ready school, indeed, will sometimes say to us, "Get your witnesses together, *put them on oath*, and then I will believe them," but until he offers to arm us with the powers of the law, "to swear witnesses and call for documents," we shall permit ourselves to smile at his somewhat crude conception of the way in which his fellow creatures would be likely to behave when approached as he proposes to approach them.

But although we do not present ourselves to our correspondents with a Testament ready for them to kiss, and an oath to administer, we do think it important to secure *personal interviews* with them (at considerable cost of time and trouble to ourselves, and sometimes, we fear, to them), whenever we can arrange such an interview without its being felt as an intrusion. It has been suggested by one or two critics that this personal acquaintance is valueless in those cases where it has merely been acquired for the purpose of the inquiry. We may reply that a juryman's acquaintance with a witness is generally acquired in the process of the inquiry to which that witness's evidence relates; but that nevertheless he would be but an indifferent judge who should consider it unimportant whether the jury saw the witnesses or not. The fact, of course, is, that in judging of men and women an interview of an hour or two, though it is not so good as a lifelong intimacy, is incomparably better than nothing. It increases the sense of responsibility and gives the opportunity for much individual detail, which may have been thought too trivial for a formal correspondence.

On the whole, the trouble which we have taken in calling on our correspondents has resulted in great satisfaction to our own minds. We have found them decidedly not lacking in common-sense and accuracy of mind; by no means ignorant of the possibility of mere morbid

hallucinations;—and, in short, distinguished from the mass of mankind not at all by being more imaginative or more hysterical, but rather by taking somewhat more of interest than other people in the scientific acceptance of phenomena which they happen to have themselves observed.

We are not to be understood as disparaging persons whose nervous organisations are abnormally sensitive, or as expressing distrust of experiences which differ widely from those of ordinary mankind. We believe that from some *exceptional*, even from some *morbid* organisations, psychical science has much to learn. Some such abnormal natures we are very glad to have encountered; and for certain experiments it is much to be desired that we should find more "sensitives" of every kind. But we are merely stating a simple matter of fact when we say that the correspondents who have seen the apparitions which we have so far collected, have, for the most part, been persons of a perfectly healthy and normal type. We make it a rule (for instance) to ask each of them whether they have ever experienced anything in the nature of a hallucination besides the special case which they recount, and in hardly any instances have they ever seen or heard anything else of the kind at all.

But this brings us to a point at which we shall have to ask of all our Members to render us all the assistance that they can. For it is not enough, we think, for us to ask our correspondents who tell us of dreams that "come true," and apparitions that coincided with deaths, how many dreams of the same sort they have had which have *not* "come true," and how many apparitions they have seen which have coincided with nothing at all. This is necessary, but this is not enough. What we should *like*,—it is of course impossible,—would be to get a regular *census* of death-dreams and hallucinations, so as first of all to know how many people in England had ever had hauntingly-vivid dreams of the deaths of relations, and how many people in England had ever seen a human figure in the room when nobody was there. Then, next, we should like to know how many of these hauntingly-vivid dreams had "come true," and how many of these so-called hallucinations had been *truth-telling* impressions, that is, had coincided with the death or other sudden shock of the person whose image was seen. For, when speaking precisely, we must give the name of *hallucinations* to *all* such images seen where no one is standing in bodily presence:—though we may distinguish those hallucinations into the *deceptive* or *morbid* hallucinations which are merely a symptom of diseased eyes or brain, and the *truth-telling*, or, as we may call them, *veridical* hallucinations which do, in fact, coincide with some crisis in the life of the person whose image is seen, and which, therefore, as we contend, though they are in one sense hallucinations, yet have some reality be-

hind them—some origin exterior to the eyes and brain of the person who sees them.*

Well, if we knew how many hallucinations of both kinds were seen by sane persons in England in a year, and how many of those hallucinations were *truth-telling*, then we should be able to calculate in a very definite way whether chance alone would explain all the coincidences,—whether, in fact, it was by mere accident that hallucinations ever told the truth. We cannot, of course, make such a census complete, or such a calculation definite. But, if only enough people will help us, we ought to be able to get at facts enough practically to put the matter beyond controversy. Provisionally, we think ourselves justified in proceeding on the assumption that hallucinations of the kind with which we are mainly concerned—that is, solitary hallucinations occurring only once in a lifetime, and not attributable to any known morbid condition of the percipient's organism—are very rare phenomena: that they do not occur, let us say, to one person in a hundred. This assumption is confirmed by the best estimate of common experience that we were able to make without formal statistical investigation: and it is at least implied in all that has been published on the subject of hallucination by medical writers. Still it is an assumption which may always be disputed, so long as it does not rest on a basis of carefully collected facts. Let us consider how to ask for the statistics required in as precise a manner as possible. First, as regards dreams. Certain dreams are sent to us which have turned out true, and it is argued that the dream, which *coincided* with the event, was in some way *caused* by the event. Well, let us consider what is the kind of dream which has a *primâ facie* look of being something more than mere ordinary nonsense or nightmare?

If I dream that a friend is dead, about whom I am in no way anxious at the time, and if that dream is unusually vivid, and so haunting that it continues to distress me not only while I lie in bed but for an hour or more after I have got up; and if it afterwards turns out that my friend *did* die that night, then there is some *primâ facie* reason for me to say: "I *felt* that this was no *ordinary* dream; and now it is *proved* that it was no ordinary dream, for it has come true." If, however, it can be shown that most other people habitually have dreams just as vivid and haunting as this of mine, which nevertheless do *not* come true at all, then I shall probably have to admit that *my* dream came true by mere chance, and was no proof of any subtle sympathy between my dying friend and myself.

The question about dreams, then, which we want asked of as many people as possible is this: "Can you recall that you have ever, in the course of the last 10 years, had a dream of the death of some person

* On this subject see the interesting "Thoughts on Apparitions," by the Bishop of Carlisle, in the *Contemporary Review* for January, 1884.

known to you (about whom you were not specially anxious at the time), which you marked as an exceptionally vivid one, and of which the distressing impression lasted for as long as an hour after you rose in the morning?" And similarly as regards hallucinations. In order to find out how common mere morbid hallucinations are we have asked the question: "Have you ever, when in good health, and completely awake, had a vivid impression of seeing, or being touched by, a human being, or of hearing a voice or sound which suggested a human presence, when no one was there?"

The reception which these questions have met with illustrates the dangers of what may be called *over-scrupulosity* in controversy. Of course, we should have been pleased to get as many *Noes* as possible in reply to these questions; for if (as we are inclined to think is the case) only a *very small* percentage of people ever do have vivid death-dreams or morbid hallucinations of any kind, then, of course, our cases of coincidence of dream or apparition with a friend's death are all the more valuable. But we were so anxious not to give any indication that we welcomed the answer *No*, that we worded our questions in such a way that (as it turns out) most people thought that the only answer we cared about was *Yes*. Our critics ridiculed us for eagerly collecting every kind of dream or hallucination that we could lay our hands on, and swallowing them all whole. And our friends did not take the trouble to tell us that they had had *no* hallucinations. They gave as an excuse for not answering our questions that they could only have answered No, which was the very answer that would have been most welcome, and they only wrote to us when they *had* some morbid hallucination to tell us of. We are thus in danger of getting results which are of no value as statistics, because the ordinary mass of mankind—who have *not* had hallucinations or death-dreams—think that we do not care for their answers. We must say more plainly, then, that we care for *all* answers,—and for Noes fully as much as for Yeses,—and we must earnestly ask each of our Members to take the trouble to collect 25 or 50 or 100 answers for us. Let them simply send in the number of Noes or Yeses on a postcard, as the circular instructs them; and then if there are any cases in any way interesting amongst the number, let them send also on a separate piece of paper an account of such cases, as full as they are willing to make it.

This is a kind of help which we can hardly ask from casual acquaintances—for it does, no doubt, involve some little trouble—but we think that we are justified in urging the members of our own body to take this amount of trouble for the ends which we all have in view. It is an eminently good opportunity for the Society to justify its own existence; for the results, the importance of which we trust we have made clear,

can only be obtained by the active co-operation of a large body of people, and, but for some such association as ours, might remain for ever unsought and unknown. If all our members, or even a quarter of them, would help as actively as some have already done, we should have good hope of getting our questions answered, as we desire, by 50,000 persons.

And, in truth, the need of a census of this kind (we must repeat) is conspicuous from a scientific point of view. The medical authors who have written on hallucinations are necessarily extremely vague as to their *frequency*, and for the most part content themselves with repeating a certain group of striking cases, without much attempt to determine whether these are truly typical in character. No real attempt at collecting statistics has been made, and for our purpose statistics of hallucination are of the highest importance; and the best chance of obtaining these is to make an effort so decided as to attract for the moment at least the attention of large masses of people, and to induce them to answer the easily-answered questions which are here proposed. We hope that something of the *collector's* spirit may be aroused in many minds for the service of the inquiry. The collection of perfectly futile objects for the mere collection's sake, which amused our grandfathers and grandmothers, has sunk now into the diversion of children. But the instinct remains in their elders, and still prompts to many a collection of butterflies or flowers, useless except as teaching accurate discrimination to the collector. We would ask that some of this energy may be transferred to a field where there is still a harvest more ample than the labourers can gather in; namely, the collection of definite psychological facts.

We want, then, the questions in the circular answered as widely as possible. And we want also, as much as ever, well-attested narratives of all kinds bearing on matters within our Society's scope. We find sometimes that someone who could give us a well-attested account of some death-wraith or similar phenomenon, refrains from doing so because, as he says, "You must have quite enough of such accounts at this stage of the inquiry to establish the fact in question." But in reality we can never have too many of such narratives. And we can never have *enough*, so long as a well-attested fact of this kind is still spoken of as "contradictory to experience"; moreover, what we want to do is not to *establish* facts merely, but to *explain* them; and in order to make even the first approach to explanation in a subject so strange and obscure we want the widest possible range of narratives to collate and compare. Each narrative, besides its *evidential* interest as strengthening the credibility of similar narratives, has its *theoretical* interest in enabling us to trace with greater clearness the links of connection which correlate one such account with another; the significant features which

must be the basis of a true classification. To illustrate our meaning by a single point: Every detail as to the clothing in which any apparition is seen is of value, as indicating whether the phantasmal picture may be more probably referred to the percipient's mind, or to the agent's, or to some further source. We do not say that definite laws can be laid down on such a point; but we do say that this is the way in which the laws of such apparitions are to be sought for,—by the collation of every scrap of evidence which bears on some perfectly definite problem, which must necessarily have a solution expressible in terms of our existing knowledge, if only that solution can be reached.

This is merely one instance of the numberless questions which need the collection and collation of great masses of evidence before any really permanent or scientific treatment of them is possible.

And those who aid us may feel that they are lending a hand to no slight or trivial undertaking. The very violence of the attacks which a certain school of thinkers have made on our work may be fairly taken as unconscious evidence of the importance and novelty of the results towards which our researches point. The school to which we refer is that which insists, in tones louder sometimes andmore combative than the passionless air of Science is willing to echo or convey, that all inquiries into man's psychical nature, all inquiries which regard him as possibly more than a portion of organised matter, are no longer open, but closed, and closed against his aspirations for ever. For ourselves, we are far indeed from retaliating either the contempt or the animosity which this able group of thinkers often express for us. Many of us have thought at one time as they think now; and have learnt to honour the courage which can face the worst, and which scorns to feed with vain hopes the creatures of a day. If we disagree with this school—as we do fundamentally—it is not that we dispute their ability or their patience in dealing with the facts on which they have concentrated their attention. It is simply that we have taken pains to collect and master an immense mass of evidence, which they have not yet included within the scope of their inquiries.

They cannot complain of us for dogmatising, for we do not dogmatise. They cannot complain of us for invoking sentiment in aid of argument; for we have refrained, with a persistency which has perplexed and disappointed some of our supporters, from anything which could possibly be construed into an *ad captandum* appeal to the friends of religion. All that can be complained of is that we are ready to take facts as they come. But facts are facts, whatever view of the Universe they make for. It is no blame to the materialist if the Universe is a soulless interaction of atoms, and life a paltry misery, closed in the grave. But neither is it any blame to *us* if the Universe is the field of spiritual *energies, and if* "high capacious powers" do in truth "lie folded up in man."

APPENDIX.

We give here one of our cases where the percipient is one who has never experienced any other hallucination of the senses, and where the coincidence of time is very close. It is from the Rev. Robert Bee, 1A, Colin Street, Wigan (curate of St. George's, Wigan) :—

On December 18th, 1873, I left my house in Lincolnshire to visit my wife's parents, then and now residing in Lord Street, Southport. Both my parents were, to all appearance, in good health when I started. The next day after my arrival was spent in leisurely observation of the manifold attractions of this fashionable seaside resort. I spent the evening in company with my wife in the bay-windowed drawing-room upstairs, which fronts the main street of the town. I proposed a game at chess, and we got out the board and began to play. Perhaps half-an-hour had been thus occupied by us, during which I had made several foolish mistakes. A deep melancholy was oppressing me. At length I remarked: "It is no use my trying to play ; I cannot for the life think about what I am doing. Shall we shut it up and resume our talk? I feel literally wretched."

"Just as you like," said my wife, and the board was at once put aside.

This was about half-past 7 o'clock ; and after a few minutes' desultory conversation, my wife suddenly remarked : "I feel very dull to-night. I think I will go downstairs to mamma for a few minutes."

Soon after my wife's departure, I rose from my chair, and walked in the direction of the drawing-room door. Here I paused for a moment, and then passed out to the landing of the stairs.

It was then exactly 10 minutes to 8 o'clock. I stood for a moment upon the landing, and a lady, dressed as if she were going on a business errand, came out, apparently, from an adjoining bedroom, and passed close by me. I did not distinctly see her features, nor do I remember what it was that I said to her.

The form passed down the narrow, winding stairs, and at the same instant my wife came up again, so that she must have passed close to the stranger—in fact, to all appearance, brushed against her.

I exclaimed, almost immediately, "Who is the lady, Polly, that you passed just now, coming up?"

Never can I forget, or account for my wife's answer. "I passed nobody," she said.

"Nonsense," I replied. "You met a lady just now, dressed for a walk. She came out of the little bedroom. I spoke to her. She must be a visitor staying with your mother. She has gone out, no doubt, at the front door."

"It is impossible," said my wife. "There is not any company in the house. They all left nearly a week ago. There is no one, in fact, at all indoors but ourselves and mamma."

"Strange," I said ; "I am certain that I saw and spoke to a lady, just before you came upstairs, and I saw her distinctly pass you, so that it seems incredible that you did not perceive her."

My wife positively asserted that the thing was impossible. We went downstairs together, and I related the story to my wife's mother, who was busy with her household duties. She confirmed her daughter's previous statement ; there was no one in the house but ourselves.

The next morning, early, a telegram reached me from Lincolnshire ; it was from my elder sister, and announced the afflicting intelligence that "our dear mother had passed suddenly away the night before ; and that we (*i.e.*, myself and wife) were to return home to Gainsborough by the next train.'

I could scarcely realise the fact for a moment, and I forbear to attempt any description of my subsequent feelings. I have the consolation of knowing that my mother was prepared for her end, a satisfaction to us all in our bereavement. The doctor said it was heart disease, which in a few minutes had caused her death.

After giving some details of his arrival at home, and of the kindness of friends, Mr. Bee continues :—

When all was over, and Christmas Day had arrived, I ventured to ask my brother the exact moment of our mother's death.

"Well, father was out," he said, "at the school-room, and I did not see her alive. Julia was just in time to see her breathe her last. It was, as nearly as I can recollect, 10 *minutes to* 8 *o'clock*."

I looked to my wife for a moment, and then said : "Then I saw her in Southport, and can now account, unaccountably, for my impressions."

Before the said 19th of December I was utterly careless of these things ; I had given little or no attention to spiritual apparitions or impressions. I was utterly careless of these things.

In answer to inquiries Mr. Bee adds :—

My mother died in her dress and boots : she was taken ill in the street, and had to be taken to a neighbour's house in Gainsborough a few paces from her own house. The figure resembled my mother exactly as to size, dress, and appearance but it did not recall her to my mind at the time. The light was not so dim that if my mother had actually passed me in flesh and blood, I should not have recognised her.

In answer to the question whether this is the only case of hallucination that he has experienced. Mr. Bee answers "Yes."

He further adds :—

The gas light over the head of the stairway shone within a frosted globe and was probably not turned on *fully*.

The fact is, that there was ample light to see the figure in, but just as the face might have been turned to me, or was turned to me, I could not, or did not, clearly discern it. Many, many times, my regret and disappointment when I recall this fact have been deeply felt.

Mrs. Bee writes to us as follows :—

If anything I can say to you will be of any use. I will willingly give my testimony to all my husband has said. I remember perfectly 10 years ago my visit to my mother's, and my husband's unaccountable restlessness on the particular evening mentioned, also Mr. Bee asking me, after I had been downstairs, if I had met a lady on the stairs. I said "No. I do not think there is any one in the house but us." Mr. Bee then said, "Well, a lady has passed me just now on the landing : she came out of the small bedroom and went downstairs ; she was dressed in a black bonnet and shawl." I said, "Nonsense, you must be mistaken." He said, "I am certain I am not, and I can assure you I feel very queer." I then went to ask mamma if there was anyone in the house, and she said no, only ourselves ; still Mr. Bee insisted someone had passed him on the landing, although we tried to reason him out of it.

In the morning, while we were in bed, we received a telegram stating that Mrs. Bee had died suddenly the night before. I said at once, "Robert, that was your mother you saw last night." He said it was. When we got to Gainsborough we asked what time she died : we were told about 10 minutes to 8, which was the exact time : also that she was taken suddenly ill in the street (wearing at the time a black bonnet and shawl) and died in 10 minutes.—MARY ANN BEE.

Now when the apparition is, like Mr. Bee's, a quite unique experience in the percipient's life, and where its coincidence with a death is as close as this, the improbability of a mere chance coincidence is so enormous that it is hard to express it with anything like accuracy in actual figures.

We will, however, roughly show the lines on which such a problem would need to be worked.

Let us say that the percipient has had only 30 years of intelligent life, and that during those 30 years he has slept for nine hours a day. He has then had 15 × 30 × 365=164,250 hours of waking intelligent life. Now, in this space of time he has had *one* apparition, which resembled his mother, and was within a few minutes of her death. Now if we say only that it was within the quarter of an hour in which death occurred, there are 164,250 hours=657,000 quarter-hours of life in which that apparition might have come; so that the chance of its coming in the right quarter-hour was $\frac{1}{657,000}$ (keeping to round numbers). But this is on the supposition that the only apparition which could possibly appear to him would be his mother's, or at least one which resembled his mother in general aspect as nearly as the phantom which he actually saw.

Now visual hallucinations known to be morbid are by no means limited to the human shape; they are grotesquely varied in ways too numerous to mention. But, for the sake of argument, we are willing to assume (it is a monstrous assumption) that *one-tenth* of all phantoms accidentally caused are likely to resemble a lady in a black bonnet and shawl. The chance, then, that an apparition of the right class (the old lady class) would show itself in the right quarter-hour (the quarter-hour of actual decease) will be 1,657,000ths × 10; or the chances *against* this happening to any given person will be over 6½ millions to one. That is to say, in order to explain satisfactorily Mr. Bee's experience on the supposition of accidental coincidence, it ought to be shown that there are something like 6½ million adult Englishmen now living who have had one single visual hallucination of a very distinct sort in the course of their lives.

Even if Mr. Bee's case stood alone, this result would be somewhat surprising. Of course it does *not* stand alone; and if we take from our store, say only nine more cases of equally close coincidence attending an apparition seen by an English adult now living, and not otherwise subject to hallucinations,—say, for instance, the cases on slips 55, 92, 229, 352, 382, 602, 627, 628, 685,—we increase the improbability tenfold, and require that 65,000,000 English adults now living should have had a single distinct visual hallucination, to make it probable that in these ten cases the coincidence would have accidentally occurred as it did.

V.

NOTE ON THE EXISTENCE OF A "MAGNETIC SENSE."

By Professor W. F. Barrett.

The experience of those who have worked with large magnets and of men of science generally, is entirely opposed to the notion that magnetism can be felt or can exert any good or ill effect on the human body or other organism. Nevertheless, the statements of Reichenbach, Gregory, Ashburner, and others seem to indicate that upon certain sensitive "subjects" a magnet can produce a distinct and powerful influence. The investigation of this question properly belongs to the "Reichenbach" Committee, and has been referred to in their first report; the object of the present note is merely to detail a few independent experiments which I have recently made, whilst Mr. G. A. Smith was in Dublin.

When a powerful electro-magnet was employed, Mr. Smith stated that he distinctly felt an unpleasant sensation when he approached the magnetic poles. The effect seemed to be most powerful when his temple was nearly touching one pole; except on one day, when suffering from facial neuralgia, and then he appeared much more sensitive in the face than in the forehead. The peculiar and unpleasant sensation produced by the magnet he describes as slowly rising to a maximum in 15 or 20 seconds after the current has been sent round the coils of the electro-magnet. In like manner the effect seemed to die down slowly after the contact was broken. Unknown to Mr. Smith, the circuit was closed and opened several times, and the magnetism correspondingly evolved or dissipated, the result being that there was a fairly accurate correspondence between the physical and the psychical effect. The faint molecular crepitation which accompanies the magnetisation of iron, and can be heard when the ear is very near the magnet, is, however, very apt to mislead the imagination. To avoid this, Mr. Smith was placed at a distance where this faint sound could not be heard, and he was then requested to walk up to the electro-magnet, and, judging only from his sensations, to state if the current were "on" or "off." The experiment was made 12 times successively, and he was correct in 10 out of the 12 trials. Assuming the fact of Thought-transference, the imagination might, it is true, have been misled; but as I did not find Mr. Smith able at other times to respond to my unexpressed will, it is hardly probable that my knowledge as to when the current was on or off would suffice to explain his

success. Of course it would be possible for a trickster, using a concealed compass-needle, to be able to impose on a careless experimenter; but care was taken, and I have not the least reason to doubt the *bona fides* of Mr. Smith. Obviously the foregoing observation, which I have thought it better to put on record, is of little value unless corroborated by a far more extensive series of experiments, conducted with the most stringent precautions to avoid the creation of illusory effects.

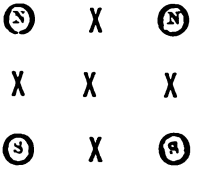
APPENDIX, *added March 12th.*

Sir William Thomson has published in *Nature* for March 6th, 1884, the report of an address on the "Six Gateways of Knowledge," delivered by him in Birmingham. In this address Sir William remarks, "If there is not a distinct magnetic sense, I say it is a very great wonder that there is not," and he then refers to an experiment made to test this point in the following words:—

"This experiment was carried out in a most powerful manner by Lord Lindsay (now Lord Crawford), assisted by Mr. Cromwell F. Varley. Both of those eminent men desired to investigate the phenomena of mesmerism, which had been called animal magnetism; and they very earnestly set to work to make a real physical experiment. They asked themselves, Is it conceivable that, if a piece of copper can scarcely move through the air between the poles of an electro-magnet, a human being or other living creature placed there would experience no effect? Lord Lindsay got an enormous electro-magnet made, so large that the head of any person wishing to try the experiment could get well between the poles, in a region of excessively powerful magnetic force. What was the result of the experiment? If I were to say *nothing!* I should do it scant justice. The result was marvellous, and the marvel is that nothing was perceived. Your head, in a space through which a piece of copper falls as if through mud, perceives nothing. I say this is a very great wonder; but I do not admit, I do not feel, that the investigation of the subject is completed. I cannot think that the quality of matter in space which produces such a prodigious effect upon a piece of metal can be absolutely without any—it is certainly not without any—effect whatever on the matter of a living body; and that it can be absolutely without any *perceptible* effect whatever on the matter of a living body placed there seems to me not proved even yet, although nothing has been found. It is so marvellous that there should be no effect at all, that I do believe and feel that the experiment is worth repeating; and that it is worth examining, whether or not an exceedingly powerful magnetic force has any perceptible effect upon a living vegetable or animal body. I spoke then of a seventh sense. I think it just possible that there may be a magnetic sense. I think it possible that an exceedingly powerful magnetic effect may produce a sensation that we cannot compare with heat or force or any other sensation."*

* Upon this point a medical authority, Dr. W. H. Stone, published the following remarks four years ago:—"It appears that a similar question was long ago asked of Faraday, when he was engaged on his diamagnetic discoveries by means of the

For some time prior to Sir William's lecture experiments have been in progress in my laboratory, the object of which is to ascertain if any special and perceptible effect is produced upon any living organism by a powerful magnetic field. Large helices have been made to encircle the head and limbs, and through them a strong electric current can be sent; so far nothing has been felt in my own case from the intense field of magnetic force thus generated. I have also had the opportunity of trying the effect of the enormously powerful magnetism generated in one of the larger sized "Brush" dynamo-electric machines. The machine was dismantled, its revolving armature being removed, and the massive pole pieces attached to the field magnets of the machine adjusted so that they clasped the head on either side. A current of great strength, generated by a large Siemens dynamo, was then sent through the magnets, but neither in my own case, nor in several others who tried was anything felt that could be attributed to a "magnetic sense." The circles indicate the poles of the pair of magnets and the crosses the successive positions in which the head was placed.



large magnet belonging to the Royal Institution. He stated that he had never been able to trace the slightest physiological effect, whether sensorial, motor, or hypnotic, on himself when using the strongest magnetic fields then attainable. It is true that the discovery of dynamo-magnetic machines has placed at our disposal currents of vastly greater electro-motive force and quantity than were then available. With these the subject should, if at all, be patiently investigated. One or two rather trivial facts, such as the inability of many persons of nervous temperament, of whom the writer is one, to sleep soundly in the north and south position, and the singular vitality of magnetic treatment from very early times down to the present, are hardly strong enough to countervail the negative evidence here adduced. Still the subject, not being in direct contravention of any known scientific facts, deserves further investigation in a fair and dispassionate spirit. It seems *primâ facie* improbable that so important a factor in cosmogony should be otiose and inefficacious in its highest development—human life." Baron Reichenbach, as is well known, made numerous experiments on the effect of the position of the bed on persons of sensitive temperament. He found that invariably the most pleasant position and the most refreshing sleep was obtained when the head was to the north and the feet to the south. These results were obtained with persons he terms "sick sensitives," but a much larger number of carefully conducted observations on these and others are needful before any conclusions of the least weight can safely be drawn, as it is most important, but most difficult, to exclude the effect of the imagination.

But whilst the results were negative in this case, and doubtless would generally be found so, yet Charcot has shown that in certain cases of hysterical patients a transference of sensation from one side of the body to the other is produced by magnetism. An apparent confirmation of these results was obtained by Dr. Stone in the case of a patient of his at St. Thomas' Hospital. The case is recorded in the Reports of St. Thomas' Hospital, Vol. X., 1880, and the following is the part relating to the specific effect of magnetism:—

JULY 1ST.—Dr. Stone, thinking it probable that repeated examination by this time might have put the patient on her guard, and roused an inclination to simulate, described minutely the effects obtained by M. Charcot with magnets in her presence, intimating his expectation that the anæsthesia would be transferred. This was the first hint she had received, as all clinical observations had hitherto been made out of the ward, and the students had been enjoined strict silence in her presence. A magnet of eight large steel plates was placed in the bed beside the sensitive leg, and there left. It was, however, *non-magnetised*, and really only an inert piece of steel. On the following morning she answered clearly and with great simplicity "that it had done nothing at all; she did not know it was there." It was then taken away on some pretext, strongly magnetised by means of a powerful electro-magnet, and returned to the ward apparently unchanged on the 3rd.

7TH.—On its being similarly applied, after magnetisation, the anæsthesia disappeared from the affected leg, but no alteration occurred in the other.

The chief points of interest of the case are, first, that the age, character, and simplicity of the patient precluded any suspicion of imposture; secondly, that singular precautions were from the first adopted to preclude the possibility of unconscious deception from the effect of "expectant attention;" thirdly, the marked and unequivocal effect of magnetism.

This case offers the most trustworthy evidence which has hitherto come under my personal observation of the influence of the magnetic field on ordinary sensation. It is materially strengthened, moreover, by an interesting communication by Dr. Julius Dreschfield, in the *British Medical Journal* of August 14th, in which he shows the efficacy of the same great physical power in non-hysterical cases. Another valuable contribution to the physiology of the subject is contained in the *Journal of Anatomy and Physiology* for January, 1879, from the pen of Professor M'Kendrick, in which he demonstrates the fact that "a portion of nerve stretched between the poles of an electro-magnet, so as to touch each, will not excite contractions in a muscle when touched by a copper wire during the passage of the current through the wire of the electro-magnet."

It is to be regretted that neither of these last writers affords any quantitative clue to the intensity of the magnetic field employed. Judging from the proved phenomena of diamagnetism and those of the action on light, the field should be of considerable strength, but whether it acts most when diffused or when concentrated requires further investigation. The unilaterality of nervous functional conditions certainly points to the hypothesis already named of equilibrium more or less stable between the two halves

of the nervous system, like that of a duplexed cable. This is also c
in a physical point of view by the slow oscillations of transferred se
which closely resemble those of a metronome pendulum, in which t
mass is large and the unequilibrated moving force proportionately s
far less magnetic movement would also be competent to distur
system than one in more stable surroundings.

For farther experiments I have constructed a large electro-magn
with a helix of iron instead of copper wire. It developes a singularly
and regular series of curves even with a battery of moderate inte
propose also to excite it by means of a fine gramme machine w
recently come into my possession. Whether the results ultimat
positive or negative, it is clear that a sufficient *primâ facie* case is
for further investigation.*

Although the magnet did appear to exert "a mar
unequivocal effect" on Dr. Stone's hystero-epileptic patient,
obvious that a larger number of experiments, conducted with s
precautions, are necessary before any general induction can
drawn.

* Dr. Stone informs me that the trials he made with this magnet
self and other healthy individuals yielded no evidence of any sen
peculiar to magnetism. It is chiefly with this electro-magnet, th
Stone's kindness, that the experiments of the Reichenbach Committee
made.

VI

THE STAGES OF HYPNOTISM.

By Edmund Gurney.

One of the first things which strikes the student of what is often loosely called the "hypnotic" or "mesmeric" state is this—that even the simplest manifestations of hypnotism are wont to present not *one* state but *two*, distinguished from one another by very marked characteristics. But the distinction, as usually drawn, has been of a very rough kind. As a rule, it has been noticed that good "subjects" first get into a state in which many of them show great acuteness of sensibility, and in which all of them can be made to do or imagine very odd things; and that from this they gradually merge into a state of profound sleep or even coma. Such were the stages as originally observed by Braid, and subsequent investigation has done little to define them further. I am going to point out what appear to me grave deficiencies in this distinction of states, and to attempt to draw a much more precise one. But, before doing so, it is necessary to refer to another mode of distinction, which, if not cleared out of the way, might greatly confuse us. In discussions on hypnotism we continually find three *types* of condition recognised—the *cataleptic*, the *lethargic*, and the *somnambulic*: the cataleptic being the condition where the limbs will remain in any position in which they may be placed, without effort on the "subject's" part; the lethargic being the condition where the muscles are relaxed, but abnormally liable to contractions and spasms under gentle stimulation; the somnambulic being the condition in which the "subject" exhibits the singular eccentricities of conduct associated with public entertainments. But though these are all real and important conditions, they do not the least represent distinct *states of the individual*, or distinct *stages of hypnotism*. For the peculiarities of muscular condition may quite well *co-exist* with that peculiar mental state which is described as somnambulic. The cataleptic state, moreover, does not belong to normal hypnotism at all, but is a decidedly exceptional phenomenon, and can only be considered otherwise by confusing it with the mere ordinary *rigidity* of the limbs; and again the effects of muscular irritability may be locally produced, by hypnotic processes, in various parts of the body, while the "subject" remains in his normal waking state. On the whole, then, this mixing up of physical and mental and of constant and occasional characteristics, in a list which professes to sum up the fundamental forms of hypnotism, seems extremely misleading; and in what

follows I shall speak only of cases where the *mind* of the "subject" is to some extent affected, and shall base my distinctions primarily on the constant features which that mental affection displays.

If, then, the "subject"—the conscious individual and not merely a part of his body—has succumbed to the hypnotic influence, if he has passed out of his normal waking state and crossed the threshold of trance, then, before he reaches the profound sleep in which his mental condition is a mere blank, there lie before him two and only two markedly distinct states or stages, each of which however may present within itself a very large amount of variety. We may conveniently designate them as the *alert* state or stage and the *deep* state or stage. These states are, I believe, produced or producible in the case of every "subject" who is sufficiently susceptible to hypnotism to be able finally to be put to sleep by it; but the question will very naturally occur how, if that is so, there can ever have been a doubt about it. How is it that the character, and even the very existence, of the two states has escaped general recognition? The answer is broadly this,—that, in the first place, each state admits of many degrees, and the characteristics of either of them may be only very slightly or only very transiently presented; and in the second place, unless special means are adopted, it is very easy to mistake the alert state for normal waking, and the deep state for sleep. This will become clearer when the states themselves have been further described.

To begin, then, with the *alert* state. This is the state in which a "subject" is when, after the usual preliminary period of gazing fixedly at some object held near the eye, or of having passes made over the upper part of his person, and after the usual involuntary closure of his eyes, the strain on his eyelids is released by a few touches and words, and he is restored to what may look quite like his natural waking condition. Sometimes, it is true, the difference is very marked, and he will sit with a vacant air, irresponsive to every voice except that of the operator, and clearly not in possession of his ordinary faculties. He may be made to perform imitative actions and to obey commands in a mechanical way: but his consciousness may be at a very low ebb, or (as some have held) may have lapsed altogether. But even these cases will exhibit two characteristics of the alert stage which are also characteristics of normal waking:—the "subject's" eyes will be open and capable of seeing: and he will (almost invariably) prove sensitive to pain if he be pinched or pricked. Very often, however, the resemblance to normal wakefulness is far closer than this: for the "subject" will be found to converse with perfect comprehension, memory, and even humour. Where then does the essential distinction of his state lie?

The main point which can be observed at the moment is that, though perfectly capable of sustaining a conversation, he does not *originate*

remarks. If not spoken to he will sit quiet, and, if simply asked what he is thinking about, he will almost always answer "Nothing." Perhaps it may be said that even this condition of passive vacancy is after all not so very different from that in which a large number of our fellow-creatures spend a large portion of their waking hours. If this be allowed, then we shall have to seek the essential difference of the hypnotic condition, not in any feature which it immediately presents, but wholly in two *possibilities* attaching to it, either of which demands appropriate treatment to become a reality. In the first place, if the "subject" be left completely to himself, he will rapidly sink into the *deeper* state, and thence into hypnotic sleep, in either of which he will prove insensitive to any moderate amount of torture. Clearly the condition which leads rapidly and naturally on to further conditions of this sort is not a *normal* one: it can never be said of a person in a *normal* state, however sleepy he may be, that in two or three minutes he will be in a condition when pins may be run into him, or the severest pinches applied, without awaking him or evoking any signs of distress. The passage into these deeper conditions, it should be observed, is often so rapid that the fact of their being reached *through the alert stage* may be wholly unnoticed. The hypnotising process may carry a sensitive "subject" in a minute or less from a condition of normal waking into hypnotic sleep; and in such a case the "alert" period has been represented only by the few seconds before his eyes closed. If he had been taken in hand during those few seconds, and had been talked to or kept employed, this passage into the deeper state would have been prevented; but if he is allowed to follow the natural course without interference, he will simply be seen to go to sleep, and he must be awakened by the operator before any phenomena can be exhibited. This *liability to lapse*, then, is one distinguishing characteristic of the alert state.

It is characterised, in the second place, by the possibility of obtaining, while it lasts, certain special phenomena of an active sort. The "subject" can be made to do, and to continue doing, any action which the operator commands, although he may be perfectly conscious of making a fool of himself, and may strongly desire to resist the command. He can also be put under the influence of delusions—can have his senses deceived, so that he mistakes salt for sugar, ammonia for eau de Cologne; or can even be made to believe that he is in some distant place, or that his identity is changed.* These are the common platform-phenomena;

* It should be remarked that even here the necessity remains of stimulating the "subject" from time to time, to keep him going. When he is under a delusion, he will sometimes give long connected accounts, in great part often fictitious, as if he were following the course of a dream; but though very slight questions and comments will be enough to make him proceed, he will not do so if left to sustain a complete monologue.

and as the very object of hypnotising a "subject" at all is usually to procure some of them, and the possibility of procuring them is thus practically certain to be tested, it might seem that the recognition of the abnormality of his state was in this case, at any rate, assured. But though this may be so during the *course* of the manifestations, it is not unfrequently otherwise at their *close;* and a "subject" who has been allowed, perhaps, to lapse into sleep, while others are being operated on, is often roused and even sent away, without its being observed that he has been roused not into a state of normal wakefulness, but only into the alert stage of hypnotism. To all appearance he is quite himself; and, the performance being over, it does not occur to anyone to try whether he is still at the mercy of commands and delusions; while the general stir and commotion prevent him from lapsing again into sleep. So he goes off home, acting and answering in a quite natural manner, till the effect wears gradually off; or, as more often happens, he continues to feel drowsy and headachy, goes to bed, and wakes up in his usual condition next morning.

Passing now to the *deep* stage, we find that this in turn is liable to be confounded with a contiguous condition, namely, the genuine hypnotic sleep into which it tends to merge. It resembles that condition in the fact that the eyelids are closed; that, if one of them be forcibly raised, the eyeball is found to be rolled upwards; in the general insensibility to pain and to ordinary modes of stimulation. And there exists here precisely the same chance as we noted in the former case, that the particular stage will escape detection. If the "subject" be left to himself, he will have no opportunity to manifest its characteristics, but, passing rapidly through the period during which these might be evoked, will soon lose consciousness and individuality in profound slumber. With some "subjects," moreover, the invasion of mental torpor is so rapid that it might be hard to fix and retain them in the genuine deep stage, even if the proper means were adopted. But many others, if taken in time, after their eyes are closed and they have become insensible to pain, but before sleep has intervened, will prove quite capable of rational conversation; they are mentally awake, even when their bodies are almost past movement, and when even a simple command is obeyed in the most languid and imperfect manner. The state is, however, harder to sustain at an even level than the alert one, owing to a stronger and more continuous tendency to lapse into a deeper condition. In the alert state the "subject" can usually be kept going for an indefinite time: in the deep state he usually shows an increasing dislike to being questioned or meddled with.

Enough, perhaps, has been said to show how the two stages of hypnotism may be distinguished from normal waking on the one hand and from blank slumber on the other. But the marks which have been

so far given as distinguishing the two stages *from one another* are by no means equally constant and precise. The closure of the eyes, the insensibility to pain, the disinclination, amounting sometimes almost to inability, to move, are all in a general way characteristic of the deep stage; and to them may be added a diminution of the irritability of the conjunctiva and of the susceptibility of the pupil to light, with irresponsiveness to any voice but that of the operator.* But of these characteristics the only one which is invariable is the bodily torpidity. Closure of the eyes is nearly invariable; but I myself have seen two well-marked instances where the eyes remained wide open throughout the period which, as judged by all other indications, was certainly the deep stage. The irritability of the eye is sometimes only slightly affected. Insensibility to pain, though usual, is liable to still more frequent exceptions, as also is the irresponsiveness to the address of persons other than the operator. Nor are these normal features of the deep stage, even when present, entirely distinctive; as more than one of them are often also present in the alert. The irritability of the eye may suffer marked diminution at the very beginning of the hypnotising process.† Again, not only is insensibility of a *local* sort in the alert stage one of the commonest phenomena of public exhibitions, when an arm or a leg is stiffened, and submitted to the audience to stick pins into or otherwise maltreat; but even *general* insensibility may supervene while the "subject" is still open-eyed and capable of actively responding to suggestions and commands. This is especially the case if his attention is strongly directed in some particular channel; for instance, a "subject" who normally is sensitive in the alert stage, if while in the deep stage he be ordered to do some particular thing when he wakes, and be then roused into the alert stage, will often prove insensitive *till the thing is done.* Similarly I have known a "subject," who was quite sensitive before, become insensitive after, the communication of some strong impression. Being told that his sweetheart was drowned, he expressed the resolution not to survive her; and, while in this heroic mood, he bore the severest and most continuous

* How little even the most elementary distinctions between the two states have been realised by high authorities, a couple of specimens from Heidenhain may show. "The pupil of a hypnotised person contracts energetically when light falls upon the eye." It is impossible that this sweeping assertion could have been made, had Heidenhain examined eyes in any but the alert state. "Hypnotised persons never fall down." This statement, it is true, he qualifies in a note by saying that he has seen *one* person fall down. The fact, of course, is that in an enormous majority of cases (I myself have never seen an exception) the "subject," if standing, falls downs a very few seconds after closing his eyes and lapsing into the deep state.

† See (in addition to the observations of Braid, Tamburini, and Seppelli), the case recorded by Mr. Stanley Hall in *Mind*, XXX., p. 177.

pinching without a sign of sensation. In such cases the nervous energy is all, so to speak, concentrated into a single channel, in the performance of the task or the contemplation of the catastrophe, and there is none left to feel with—just as it has happened to soldiers in the excitement of battle to be temporarily unconscious of wounds. Insensibility to pain may also be present in the alert state, if the "subject" is put under the influence of such a delusion as would naturally involve it, *e.g.*, if he is made to believe he is a statue; and I have known insensibility, which originated in this way, *continue* for some little time after the delusion was withdrawn, and another substituted—the phenomenon being the more remarkable in that the "subject," who had been receiving the most savage nipping with total indifference, actually shed tears at the memory of a *fictitious* nipping, which he was told had been administered to him on the previous day. Closure of the eyes, again, may be present in a state which in respect of every other symptom is the alert one: indeed, unless the spasm which brings down the eyelid during hypnotisation be relieved by some distinct local process, the eye might naturally remain closed as much during the alert stage as during the deep. Nor can we even account the bodily activity which may be evoked during the alert stage, in contrast with the torpidity of the deep one, as a really sharp or essential distinction. The activity, it will be remembered, always has to be evoked *ab extra*, and often much against the "subject's" inclination. If left to himself, he will sit as passive in the former state as in the latter, and the one will insensibly merge into the other. The difference here, then, might seem to be one rather of degree than of kind; and the two states to be merely the less and the more advanced stages on the path to complete torpor. I may add that the advance of *mental* decline, where it can be marked, seems to be of the same graduated kind. Thus a "subject" who, first in the alert and then in the deep state, is assured that he is going to be hanged next morning, will succumb to the idea with about equal readiness in both cases; and the only observable difference will be that in the deep state he does not seem equally to realise its gravity. A "subject," for instance, with whom I tried this particular experiment and who was rendered decidely grave when in the alert state, was chiefly occupied, in the deep state, with the half-jocular invention of dodges to avoid pain. But, where the whole mental condition is so abnormal it is hard to regard such slight differences in the power of judgement as important or distinctive, or to regard them as other than steps in a single process of mental unhinging.

The question then presents itself: Is there any distinction of *kind* between the two states any single test by which we can make sure in which of them the "subject" is—any sort of phenomena, capable of constant reproduction, which will draw a clear line between them, and

not merely represent a gradual and continuous decline of hypnotic waking into hypnotic sleep? I believe that there *is* such a distinction; and that the phenomena needed to establish it are to be found in the domain of *memory*. And as memory will afford the means not only for distinguishing the one stage of hypnotism from the other, but also further for distinguishing the "hypnotic state" as a whole from the normal one, I may attempt to make my rapid sketch embrace all its various conditions, so far as I have been able to observe them.

First, then, as to the "subject's" memory, when completely awakened from the alert state, of what has taken place during that state. The degree of it varies with the number of times that he has been under the hypnotic influence.

(1) A "subject" who is quite fresh to hypnotism has frequently some remembrance, on waking, of *all* that he has gone through. Of such actions as are usually exhibited on platforms—imitative movements, sneezing, laughing, jumping and the like—his remembrance is distinct; and he perfectly recalls not only the actions but the feelings of passive acquiescence, or of surprise, or of repugnance, with which he performed them. A not uncommon description is that he felt as if he had *two selves*, one of which was looking on at the involuntary performances of the other, without thinking it worth while to interfere. He also perfectly remembers such simple mental operations as the effort to recall his name. Of performances which have involved more complex *mental ideas*, and where his mind has been at the mercy of some concrete form of delusion, his remembrance is dimmer. But still he will give some account of parts played by him in imaginary scenes, or even when under the impression that he was some one or something other than himself. It is probable that the delusive impression in such cases has not been quite complete. For instance, a "subject" who, when awakened to his normal state, remembered the fact of having been put to flight by a white ghost, described himself as having in a sort of way known that it was only a handkerchief which the operator was flourishing, and yet as unable to resist the ghostly terror.

(2) After a very short course of hypnotisation, these illusory changes of scene or of identity, and even the simple mental operation of trying to recall some familiar fact, are found to have left no trace on waking; but the "subject" can still perfectly recollect the imitative and other actions which he has performed *in propriâ personâ*, and the sort of feelings which accompanied them. It will be observed how curiously fatal this fact is to Professor Heidenhain's theory that the actions of imitation, and of what he calls automatic "obedience," which a hypnotised person performs, are purely reflex and unconscious. For it is just of these actions that the clearest remembrance is retained; and in fact, if the "subject" has been made to perform a few of them after being

thrown into a light trance-condition, and is then brought back to the normal state, there has been absolutely no breach whatever in the continuous stream of his consciousness. He has passed through a strange experience, and that is all.

The memory of the events of the alert hypnotic state finds its precise parallel in certain cases of natural somnambulism—a condition in which all actions are of course performed *in propriâ personâ*, and without any externally-induced illusion. Somnambulism is, as a rule, a decidely deeper state than the lighter stage of hypnotism; and memory, on waking, of what has occurred in it is exceptional. I have, however, lately met with well-marked cases of it in two of my own acquaintance, who gave descriptions of their somnambulic experiences very similar to those given by hypnotic "subjects." Though exceptional such cases are probably not absolutely rare; and it is the more curious that Despine, Heidenhain, and others should have so hastily and sweepingly assumed that the function of true psychic memory is suspended in hypnotism.

(3) In my experience no true memory has ever been exhibited, on complete waking, of things which have been done or suffered in the *deep* state. Nor am I aware of any published record of such an event.* The performance, at the appointed time, of *commands* impressed on the "subject" when in the deep state hours or even days before, is not a case in point. For though he feels impelled to do what he has been told to do, he has no recollection of the fact of having been so told, and is at a loss to comprehend his own impulse. This is merely an instance of the well-known phenomenon of cerebral (or as it has been called "organic") memory; but is interesting as taking place when the person is otherwise in a completely normal state.

So much, then, for the conditions of the memory on complete waking; next as to its conditions in the hypnotic states themselves.

(1) Facts of the "subject's" general knowledge, his address, business, recent employments, and so on, are remembered even in the deep state, if that state is sufficiently marked and prolonged for some amount of conversation to be sustained in it.

(2) With a favourable "subject," something that has happened during one of the hypnotic states will often recur to the memory on the next occasion when that state is produced, though in the interval of normality—amounting, it may be, to several days and nights—which has intervened between the two occasions, it has been completely forgotten. (The thing, however, must not be an action performed *in propriâ personâ*, for this—as we have seen above—would *not be forgotten* during the

*Dr. Wyld, however, assures me that he has occasionally witnessed the phenomenon.

normal state; nor connected with a delusive impression, for this—as we shall see below—would *not be remembered* on the recurrence of the *abnormal* state.) This recurrence may be made a test of the extreme rapidity with which a "subject" will, in exceptional cases, pass from the alert hypnotic to the normal state—his attention being first arrested by a suitable incident, a few passes of the waking and "clearing" sort obliterating all knowledge of it, and then a few more of the opposite sort bringing it back again to his mind, all within the space of a minute.

But the chief interest of this *induced* phenomenon of alternating memory lies in its resemblance to what occurs in *spontaneous* conditions. Even ordinary *dreaming* occasionally presents this feature in an embryonic form—a dream-scene or dream-incident being often more apt to recur in dream than in waking moments. Not that the mere recurrence could be taken necessarily to imply memory; but experiences are recorded in which the scene or incident, though one not associated with the waking life, is distinctly *recognised* as familiar when it recurs, which of course does imply a sort of memory. *Natural somnambulism* may seem to present more distinct resemblances; and certainly, if one judged from expressions used by the somnambulist, particular ideas which have made no part of the waking life are apt to recrudesce in the sleep-waking state. As a rule, however, it would be hard to represent this phenomenon as involving more than a mere re-awakening into activity of certain nervous tracts, which naturally manifest the fact of their activity by the same external results as on previous occasions. Even in the rarer cases, which more strongly suggest the recurrence of the past events as such, the presence of true psychic memory is more doubtful than in hypnotism, or at any rate is harder to substantiate; for the very tests which might substantiate it naturally tend to wake the somnambulist, and so to put an end to the condition. Perhaps, therefore, the clearest interest of the hypnotic alternations of memory is rather as illustrating the spontaneous alternations in cases of "*double consciousness*," where a single individual lives in turn two (or more) separate existences. There, as here, the transition may be almost instantaneous; and there, as here, while the memory of the *normal* state is continuous (its events being remembered even in the abnormal condition, just as we have seen that the events of ordinary life are remembered in either of the hypnotic states), the memory of the past events of any *abnormal* state lapses and recurs with the disappearance and reappearance of that state.

(3) If the phenomena mentioned under the last head are somewhat uncertain, it is otherwise when the condition intervening between two hypnotic states of the *same* kind is not normal wakefulness, but an hypnotic state of the *other* kind—*i.e.*, when a deep state intervenes between two alert, or an alert between two deep states. I have then found that (with certain well-marked exceptions to be mentioned here-

after) the ideas impressed in the one sort of state are invariably forgotten in the other, and are as invariably again remembered when the former state recurs. Thus the "subject," when in the alert state, is told something—some anecdote or piece of ordinary information—which we will call A. He is then thrown, or allowed to fall, into the deep state with closed eyes, and is asked, "What were you told just now?" He is quite unaware what is meant, nor will the broadest hints recall the missing idea. He is now told something else, which we will call B; and is then re-awakened into the alert state. Being asked the same question as before, he at once repeats not B but A, and it is impossible to evoke in him any memory of B. Thrown again into the deep state, he in a similar way recalls B, and A has once more vanished. Finally he is completely awakened, informed that two things have been told within the last five minutes, and offered £10 to say what either of them was—with a result entirely satisfactory to the experimenter. Occasionally I have succeeded in hitting a transitional moment at which *both* things were remembered; but it was a sort of knife-edge, and the slightest manipulation or pause tending to deepen the condition brought about the customary separation and oblivion of the thing told in the alert state.

The phenomena seem singularly constant. I have obtained them with a large number of "subjects," and with three operators in three different parts of England, two of whom certainly had no idea what result I was expecting. They represent, of course, that clear distinction of the two hypnotic stages—as something more than mere continuous degrees of a single trance-condition—to which I have been leading up; and the great rapidity of the transition, together with the sharpness of the results, seems to make them as satisfactory indications of that distinction as could well be imagined.

(4) To the rule thus established, there are certain definite exceptions. If the idea impressed in the alert state is a *delusion*, involving either a change of *scene* or a change of *identity*, it is not remembered in the usual way. A "subject" who has been made to believe himself elsewhere than in the room where he actually is, or to assume the part of another person or of an animal, and who while under this delusion has been carried into the deep state, on returning from that state proves almost always to be the natural self of the alert state, and refuses to believe that any idea of any sort has been impressed upon him. In the few cases within my experience where this has not happened, and where the delusory part was resumed when the alert state returned, it was noticeable that the bare *idea* seemed to revive first—very likely as the memory of the remark which had preceded and produced the illusion—and that then the illusion followed in the wake of the idea. Thus a boy who had been enacting the part of a fish on the floor, and who had *been* then thrown into the deep state and placed in a chair, was

brought back to the alert state by gentle upward passes. He sat for some seconds staring at the floor in a puzzled way, and then flung himself down and recommenced the fish-like movements. Waking to the alert state by any *sudden* means always ensured forgetfulness, carrying the "subject" at once over that low degree of the alert stage where recurrence of the delusion was possible. It may be added that though delusive ideas are thus forgotten, yet if the same delusion be again suggested in a general way, the *details* of the former one will be remembered. Thus a youth who had been impressed with the idea that he was a schoolboy attending Brighton College, and that his name was "Gerald Hamilton," completely forgot this change of identity when he returned to the alert state in which he had undergone it; but on being then again told that he was attending Brighton College, and asked his name, he gave it as Gerald Hamilton.

(5) The next case is stranger still, though quite as definite. If in the alert state any physical effect is produced by suggestion—*e.g.*, if the "subject" is made unable to flex his arm by being told that he cannot do so—a further very marked effect is produced on his mental powers and memory, although there is no special sign that his mind is preoccupied with attending to his bodily symptoms. A boy's arm was thus extended; he became unable to talk rationally; and being set to read aloud, he did so in a stupid and mechanical way, and could not recollect what he had read. He was now passed into the deep state, during which his arm dropped; and on being recalled from this state, was asked what he had been doing just before he went to sleep. He replied that he was holding his arm out; but both forgot and utterly denied the fact of the reading. Similarly the operations of opening a piano and picking out a tune on it, carried out by a "subject" while under an impression that he could not unclench one of his fists, were clean forgotten after an interval of the deep state.

(6) Any sort of argument or bothering has a singular effect in causing the "subject's" mind to drift into a deeper dream-like state. Thus, at the close of the experiment just mentioned, the "subject" was pressed for some time as to how his arm, which he remembered to have been stiff and extended, had dropped. While he was in a state of puzzle and worry, a sudden clap and call brought him instantly to a point at which the whole circumstance as to his arm was completely forgotten; but being allowed to lapse quietly, he again recalled it. There are thus sub-divisions of recollective power within the alert stage itself.

(7) We now come to an apparent exception of another sort. If the thing impressed on the "subject's" mind in the deep state is a *command*, which he is to execute "on waking," he will execute it as soon as he returns to the alert state; or, if allowed to work off his trance in natural sleep, he will usually perform the act on

normal waking; but if the act has been performed in the alert state, he will have no recollection of it when brought to his normal state.* Such obedience is, however, no true exception to the rule that psychic memory of ideas does not extend from one state to another, any more than in the case above noticed where a "subject" obeys a command fixed for some distant hour: he feels an impulse but does not remember its source. A singular point in connection with this obedience is that it seems apt to fail in cases where a vivid and interesting idea is suggested at the same time as the command. Thus several "subjects" who were told in the deep state that a fire had broken out at home, and that they must go and help to put it out, on being recalled to the alert state sat without moving, and denied any impulse to do anything. The idea probably produced a strong mental picture, which, in disappearing with the change of state according to the rule above given, involved the further disappearance of the sense of obligation.

(8) Obedience also fails in the following case. If a command has been imposed in the deep state, and the "subject" is woke into the alert, but then, before he has time to perform it, is put under a delusion—this will *suspend* the performance of the act. Thus, a youth who had been told that he was to put on his hat and begin reading the newspaper, and had then been roused, was on the point of carrying out the command, when he was suddenly told he was a chicken. He instantly went down on the floor and began to cluck. He was then allowed to lapse into the deep state, and again brought out of it: he now at once performed the order. In this particular instance the order was not *remembered* in the second deep state, though carried out on emergence from it; the delusion had altogether obliterated it, as far as psychic memory was concerned. But this feature seems unsymmetrical, and was found not to be constant—the delusion as a rule having no effect beyond the particular sort of state in which it is induced.

Such in briefest outline is a sketch of the conditions of memory connected with hypnotism, so far as my own observation has gone. Brief as it is, it may perhaps suggest matter of reflection as to theories which assume hypnotism to be a state of mere unconscious automatism, on the ground that no true memory ever exists of what happens in it.

[All the experiments here mentioned were carried out as part of the work of the Mesmeric Committee of the S.P.R.]

* After what has been said above, it will be readily understood that performance in the alert state of commands given in the deep, and remembrance of impressions from one alert state to another with a deep state between, are liable to be re- as performance or remembrance *on waking—i.e.*, on *complete* waki -al state—owing to the ease with which the alert state may be -normal.

VII.

REPORT ON WELLS SUNK AT LOCKING, SOMERSET, TO TEST THE ALLEGED POWER OF THE DIVINING ROD.

By W. J. Sollas, M.A., D.Sc., Fellow of St. John's College, Cambridge, Professor of Geology in the University of Dublin.

In response to an invitation I made one of a party of investigators, who on the 14th of May, 1883, proceeded to the little village of Locking, to conduct an inquiry into the alleged phenomena of "dowsing," "rhabdomancy," or divining for water. Our party consisted of Dr. Burder, the esteemed President of the Bristol Naturalist's Society, a lady who was believed to possess the power of divining, Mr. Pope, Mr. Pease, and others. I accompanied them as a professional geologist to determine the geological conditions under which the experiments were to be made.

At Locking we were introduced to the "dowser," Mr. Thomas Young, an aged and experienced man, who for 50 years had been engaged in finding underground water, and with admitted success. He showed us the whole *modus operandi* of his art; and we were told to particularly notice that, at the critical moment, the dowsing rod turned of itself, without any pressure being exerted by the fingers of the dowser; on this point, however, it was the general opinion that the dowser was mistaken. Dr. Burder and myself are positive that every time the rod turned we plainly discovered a corresponding movement in the fingers of the dowser. Not only Mr. Young, but Mr. Lawrence, a dowser of great local reputation, lay great stress on the fact, as they assert, that the movements of the rod are not governed by the movements of the fingers, and also that it turns independently of their volition, in fact, even in spite of their willing to the contrary. As to their volition of course I am unable to speak, but I am confident from what I observed, that the sole immediate cause for the turning of the rod is to be found in the muscular contraction of the hand of the operator.

We found Mr. Young very frank and communicative; one of his statements was to the effect that when the dowser walks backwards over the water he is in search of the rod turns downwards, while on walking forwards, it always turns upwards! A psychical fact so extraordinary that it seems worth while to put it on record. Mr. Young showed a close acquaintance with the ways and habits of underground

water. He told us that in the district about Locking it always flowed from east to west; moreover he actually showed us the natural spring from which the underground water issued which he had previously sunk upon at a point nearer its source, to obtain a supply for an adjacent farmhouse. With so much real knowledge the use of the "rod" seemed superfluous, so I ventured to inquire whether he would regard it as morally wrong to make use of his evidently practical knowledge to assist him in the discovery of springs. He replied, certainly not, but that "he would be nowhere without his rod."

We were now to put the dowser's skill to a practical test. A field on the alluvial plain, which extends from Locking to the Bristol Channel, was selected for the experiment, and on this the "dowser" was to indicate two spots, one beneath which water should be found, and another where it should not. Through these spots shafts were then to be sunk, and the prediction verified or not. In order to simplify the conditions of the experiment, Mr. Pease requested the dowser to operate blindfolded, but to this he absolutely refused to consent. This was very unfortunate, as the trial became one rather of the skill of the dowser than of the truth of dowsing: and I noticed during the trial that the dowser made very good use of his eyes. Looking about, he walked direct to one side of the field, without using his rod;—not wandering as one might have expected, rod in hand, hither and thither, in blind search for an indication. Arrived at the place he had chosen he used the rod over a line of march some 20 or 30 paces long, and as he did so the rod plunged vertically upwards at a spot where he then asserted water would be found. He repeated the traverse further down the field, in the same manner and with the same result. This he did again and again, and so produced the impression that a stream of water was flowing towards the sea beneath our feet, and along a line indicated by the successive points which he indicated by the rod. We therefore asked him to dowse along the length of the stream instead of finding points over it by traverses, and this he very willingly did, walking in an undulating manner over what he gave us to understand was the course of the stream. Finally, we requested him to make a fresh traverse and to indicate the spots where digging was to be commenced. He did so, and at our suggestion selected a place about 20 paces to the north of a ditch or "rhyne" which flows through the flat, as that where water would be found, and another 15ft. north of this, where he stated it would not.

We determined the position of these places in a way that left no possibility of changing them, and made arrangements for pits to be sunk by the dowser and his assistants. The supervision of the work as it proceeded was deputed to me.

Before proceeding further with an account of the experiment we

may consider the nature of the case we are about to try. It is the prediction of a practical man, skilled by experience, opposed to that drawn from the generalisations of science.

The dowser predicts that in one of the pits now to be sunk we shall find water, let us call that the + well; in the other we shall not, we will call that the — well. As a geologist I predict that we shall find water in both or neither. The dowser does not give reasons for his prediction; for mine they are the following.

The field we have selected for the experiments is part of the great alluvial flat which borders the eastern side of the Severn and Bristol Channel. Sinkings have frequently been made in various places through this flat, and I can therefore state beforehand that sand will certainly be met with in both wells, and will certainly furnish some water to both. Gravel may possibly be reached, and if in one, probably in both wells, and then both will yield water freely.

I now proceed to give an account of the observations made on the wells, as I visited them from time to time during their sinking.

May 22nd. Verified the positions of the wells. The + well had been sunk 10ft., and contained 6ft. of water; the — well had been sunk 5ft. and contained 1ft. of water.

June 1st. The + well was now 16ft. deep, and contained a good deal of water, eight or nine feet; water was still trickling in from the sides. As work had been suspended in this well, water had had time to accumulate. The — well was 20ft. deep, and contained 4½ft. of water. Water was trickling into it, but not so fast as into the other. The well-sinkers said that this 4½ft. was merely surface water. A bed of peat 14ft. from the surface had been struck in both wells.

June 15th. Visited the wells, but found that the work of sinking was suspended. The + well indeed had been abandonded owing to the caving in of the sides. The — well had been timbered down to 20ft.; it was 22ft. deep, and water was slowly dripping into it.

June 18th. Received a letter from Mr. Hellier, the bailiff of the estate, stating that the real depth of the + well was 17ft. 6in., and the depth of water in it 9ft. The — well 20ft. with 4ft. 6in. of water (these numbers are for Friday, June 15th).

June 24th. Commissioned my friend and pupil, Mr. F. P. Evans, to visit Locking. He reported that the water stood in both wells at precisely the same height, viz., 5ft. 4in. below the level of the field. So that the + well contained 12ft. 2in. of water; the — well 14ft. 8in. of water.

The reports from the 16th June to the 24th require some comment. On Thursday, the 14th, the men were sinking, consequently on that day the — well was dry; on the 15th, Friday, work having ceased for 24 hours, it contained 4ft. 6in. of water; on Saturday, the 24th (and

may be several days earlier), it contained 14ft. 8in. Thus the well which should not have furnished any water could not by any perversion of language be called "dry."

June 28th. Mr. Hellier writes, saying that the men have sunk through the clay and found black sand, in which they have sunk 8ft. On making a subsequent visit I found that this black sand was a fine arenaceous clay.

July 9th. Paid an unexpected visit to Locking; I was expected the previous Saturday; work had been suspended, but one of the sinkers, George Gale, was still there. He told me that they had been sinking on Saturday, the 7th, and had "kept the water down, for me to see everything!" On the 8th, Sunday, water, so he said, had accumulated to the extent of about 4ft.; on Monday afternoon, when I took measurements myself, there were 12ft. of water in it. So that 12ft. had accumulated in 48 hours. I made a thorough examination of the shaft, descending as far as the water, and examining the nature of the sides. The deposits passed through were as follows:—

Blue Silt	14ft.
Peat	1ft., or 1ft. 6in.
Blue Silt, becoming sandy lower down...	8ft.
Red Clay	5ft. or 6ft.
? Trias	

The sinker told me they had sunk to 29ft., in which case the red clay would have been penetrated to the depth of 6ft. If this red clay is Trias, then Trias must be beneath both wells, and the bottom rock being the same in each, the dowser must have calculated on the presence of gravel in the + well, and its absence in the — well, if he made his prediction on scientific grounds.

Water was streaming from the sandy sides of the — well just as I had seen it issuing from the sides of the + well on June 1st.

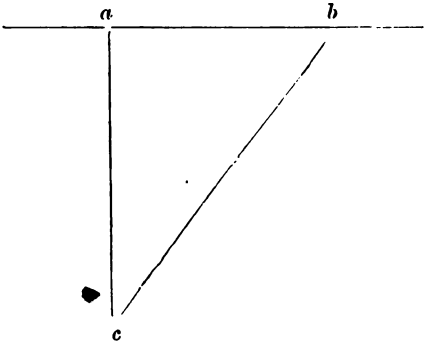
August. Mr. Metcalfe visited the wells for me and found the water standing at the same level in each.

This record of observations seems to me to completely verify the prediction made on the scientific side that the behaviour of the two pits with regard to furnishing water would in all probability be the same for each. The dowser attributed the water in the — well to an outflow from the +, but this is of course an excuse made after the trial had been completed, under conditions to which he had himself assented; and an excuse which is without any real basis. For if the rock between the — and + wells is so [illegible] that water can stream through it from one to the other (and the [illegible] so implies this) then the rock between the two, and necessarily

surrounding the two, will be water-bearing, and would furnish water to both, and thus would have supplied the — well if the + one had not existed.

The tenant who farms the estate said he had *seen* the water flowing from the + to — well! The dowser and his assistants confined themselves to asserting that it went into the — well from the side nearest the + well, as indeed, from my own independent examination, I can confirm it did; at the same time it trickled in from the other sides, that farthest off included, quite as freely.

To me, therefore, it appears that the case of the dowser has conspicuously failed, and I attribute it to the fact that the dowser's experience has throughout been gained and employed in the finding of *water*, not in seeking for dry places. As to his ability to discover underground water, these experiments simply confirm general testimony which is unanimously in favour of his powers. That these powers are totally independent of the use of the rod is, I think, proved by the following considerations. For the sake of argument, let us assume that the dowser is physically affected by the presence of water beneath his feet. The action between him and the water will take place in a straight line, drawn from him to it. On the analogy afforded by physical forces, the force of the action will vary directly as the mass of the acting bodies and inversely as the square of the distance. Let *a* be the + spot marked by the dowser, and *b* the — spot, distant from each other 15ft. Let *c* be the position of a stream of water, 20ft. vertically below *a*



Join *c*, *b*.

Then the action at *a* will be $\frac{m}{a\,c^2} = \frac{m}{400,}$ and at *b* $\frac{m}{b\,c^2} = \frac{m}{576.}$

So that if the action at *a* be represented by 1, at *b* it will be $1-\frac{11}{36}$, or about $\frac{2}{3}$.

Now, if a force of 1 causes the rod violently to assume the upright

position, surely one of $\frac{2}{3}$ should have some appreciable effect, whereas the rod shows no sign of agitation, remaining quite unconcerned, till *a* is reached.

Again, we have stated the case here far too favourably for the dowser, for the underground water is not all concentrated at *c*; it is diffused throughout the alluvium, the underlying deposits are all moist, and the diffused water will act upon the dowser as well as that concentrated in one place. The action at *a*, therefore, is:—

1 + influence of the diffused water = X, and at *b*: $\frac{2}{3}$ + influence of the diffused water = Y, and the difference between X and Y is much less than $\frac{1}{3}$.

Moreover, on many occasions, the underground spring exists at a much greater depth than 20ft., and yet affects the dowser when he stands vertically over it. Its influence in these cases must be often *less* than the $\frac{2}{3}$ in our experiment, yet it makes itself decidedly felt.

The dowser is naturally possessed of much mother wit, and he has a large experience of the behaviour of underground water; but his wonderful rod has no more magical power than the gold-headed cane of the medical practitioner of the last century.

NOTE BY EDWARD R. PEASE.

Whilst agreeing on other grounds with most of the conclusions which Professor Sollas sets forth in his very able report, I cannot feel his complete confidence in the conclusive nature of this particular experiment. In September I went again to Locking, and had both the wells emptied of water. The + well I found now to be a mere hole some 10ft. deep, whilst the — well was a carefully-timbered shaft 24ft. in depth. I made minute observations of the rate at which both wells filled with water, but during the course of them the sides of the + well caved in and filled it to the depth of 2ft. This almost destroyed the value of the measurements, and I was not able to repeat them. But in so far as they have any value, they seem to show that the + well filled more rapidly than the — well, although its depth was so much less. The tenant of the farm told me that the water in the + well was of better quality than thatin the — well.

VIII.

THE DIVINING ROD.*

By Edward R. Pease.

The Art of Divining has long been considered by men of science and by the general public as one of the Black Arts, which alone has survived in remote villages and amongst uneducated mining populations. Unlike the others, it is considered a harmless superstition that preys only upon men's pockets, and does not imperil their liberty or their lives. Therefore the strong arm of the law has neglected to extirpate it, and we are able to examine the only living example of a class of superstitions that flourished many centuries ago.

This is the commonly-held view of the matter. But a little investigation discloses the fact that belief in the power of the divining rod is by no means confined to remote villages and to ignorant persons. The evidence of its value is strong, and it comes from unexpected quarters.

The Divining or "Dowsing" Rod is a V shaped twig, commonly of hazel, from 1 to 3ft. in length, and from a quarter to half an inch in diameter. It is firmly grasped by the two ends, one in each hand, and the "dowser" walks carefully over the ground to be tried, holding the rod before him. When he comes upon a spring of water the rod moves as if of its own accord.

In England the rod is used for seeking for water chiefly in the south-western counties. It is employed also in the United States, and on the Riviera. In Cornwall and California it is used to discover veins of metallic ore, and in Pennsylvania to find mineral oil.

A rod is the most natural symbol of authority, and hence at all times and places, those who lay claim to occult powers are found to use some sort of magic wand.

But the divining rod which we are now considering, is a *forked* rod, and the fork is its essential feature. We must, therefore, disregard all

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"Nature." Paper by E. B. Tylor, May 17th, 1883.
"The Divining Rod." By Rossiter W. Raymond, Ph.D. Read at Boston Meeting of the American Institute of Mining Engineers, February, 1883.

evidence of the use of a simple rod, whether by ancient peoples or by savages, and confine our attention to the comparatively modern custom of using a forked divining rod. A work on the art and science of water-finding, published in Orleans in 1569, contains no allusion to it, although in some places it must have been known at that date. Melancthon, who died in 1560, is one of the earliest writers on the divining rod, and he explains its power by the then common theory of affinities. Agricola, the great German mineralogist of the first half of the 16th century, also wrote on it at considerable length, and was by no means a believer in its efficacy.

From this time till the end of the 17th century, the rod was commonly employed for many purposes throughout the West of Europe, and the writings of philosophers and enthusiasts are numerous and bulky. Mr. Raymond, of New York, in his pamphlet, to which I shall refer more particularly hereafter, enumerates some 46 works, by nearly as many different writers, all published in the century and a-half preceding the year 1701.

During this period, especially towards the end of it, the foremost philosophers of the age, princes and noblemen, and Church dignitaries, debated, and experimented, and quarrelled over the existence or non-existence of this mysterious power, over the various explanations propounded to account for it, over the honesty or trickeries of celebrated diviners.

The Baron and Baroness Beausoleil were amongst the earliest devotees to its cause. The Baron was "undoubtedly, one of the foremost mine-engineers of his time." He was made comptroller-general of the mines of France, Hungary, the Papal States, and other countries, and he travelled all over Europe studying the subject, whilst the Baroness published books on the use of the rod. They obtained wealth as well as fame, but were accused of sorcery by their enemies, were imprisoned by Richelieu, and died in poverty about the year 1645.

Amongst other writers of the period Mr. Raymond mentions Robert Boyle, who in 1666 brought the subject before the Royal Society.

In 1692 comes the celebrated case of the murderers tracked and discovered by Jacques Aymar. The story is so well known that it is not necessary to dwell upon it here. It may be found in the *Cornhill Magazine* for January, 1883, and is fully discussed by Mr. Raymond in his pamphlet. Aymar attained great fame by his success, but he conspicuously failed when tested by Prince Condé, and was obliged to retire into private life.

In 1701 the use of the divining rod was condemned by the Inquisition, and for some time we hear but little about it. In 1780 a Dr. Thouvenel experimented with a diviner of Dauphiny, named Bleton,

and published a memoir in its favour, which created much sensation. Bleton was tested by other scientific men, but without success. The experiment of blindfolding him was repeatedly tried, and the indications of his rod were, under this condition, proved to be fallacious. Penet, who lived at the beginning of this century, was another celebrated diviner, who professed to discover coal and hidden metals. A commission of savans tested him for three days at Padua, but failed to obtain good evidence of his alleged powers, and afterwards he was detected whilst examining at night the enclosure where an experiment was to be made.

It would seem, therefore, that the claims of diviners were carefully investigated by competent men of science at intervals during the 17th and 18th centuries, and that they uniformly failed to elicit satisfactory results. The domain of science was then mapped out far more imperfectly than it is at present, and therefore alleged facts of an extraordinary nature were examined with an absence of prejudice which it is impossible now to obtain. We must therefore in my opinion, attach considerable weight to the negative results of these investigations.

During the last few years the art of divining has attracted fresh attention, and a number of articles and letters about it have appeared in various newspapers and periodicals.

A member of the Society for Psychical Research, Mr. E. Vaughan Jenkins, of Cheltenham, has made a most valuable collection of contemporary evidence, which is an unique and most important contribution to the literature of the subject, and which, with great generosity, he has placed at the disposal of the Society.

He has communicated with various professional diviners, and has obtained reports of their operations from andowners, architects, builders, and other persons, who are entitled to speak with authority. Concerning one of the best known of the professional diviners, John Mullins, of Collerne, Wilts, he has obtained twenty-two records of the successful location of wells, nearly all from persons of standing, chiefly owners of property in Gloucestershire and Lincolnshire. Twelve records refer to Mr. W. S. Lawrence, of Bristol, of whom I shall have occasion to speak later on; seven cases are recorded in which Stokes, a carpenter of Newbury, Berkshire, was the diviner; and seven others refer to Pavey of Cheddar, and one or two other persons. All these, it must be recollected, have been written and sent to Mr. Jenkins within the last 18 months, all are from persons actually cognizant of the facts, and generally present at the divining operation, and many of them are very careful and detailed statements of what took place. It must be remembered too that these were all collected by one gentleman, in the course of a few months, and are probably but samples of a vast mass of

testimony which a more prolonged and wider search would reveal. Several of them also refer to more than one case of successful dowsing.

In an appendix to this paper a complete list of these cases is given, and a few of them are printed in full.

Notwithstanding the general consensus of scientific opinion against dowsing up to this time, there seems to be a *prima facie* case for the necessity of further examination of the subject from the evidence collected by Mr. Vaughan Jenkins.

This evidence tends to show.

1. That the power of certain persons to find water when experts fail is widely believed in to this day, and is utilised by practical men to whom the finding of water is a business matter. This will be seen by a glance at the cases given in the Appendix. Many of these persons doubt the dowser's power to begin with, but are convinced of it by his success.

2. That this power does not depend on superior knowledge of the locality. See, especially, case 5, where Captain Smith says that "Mullins had not been in our neighbourhood before"; case 2, where Mr. Finch Hatton says, "We established on good authority that Mullins had no previous acquaintance with Haverholme"; case 4, where an old well was to be re-opened "but no one on the estate knew where it was, not having been opened for a number of years. But Stokes, with his divining rod, discovered the well (although a perfect stranger to the place), and it proved to be where he predicted, under the paving in the centre of the pathway." See also cases 1, 26, 29, 31, and 46.

3. That the power does not depend on geological or quasi-geological knowledge or instinct. See especially case 13, where Sir W. E. Welby-Gregory says, "To save time I took him (Mullins) to a spot from which he could see the contour of the hills (about a mile distant) from which most of our water comes, showed him what springs we knew of, and told him to choose for himself the best part to try for more. He unhesitatingly selected the upper part of a certain hill (which was afterwards pronounced by an eminent civil engineer to be unquestionably the most likely spot within sight to contain water), and thither we went. He tried the whole hillside over without finding a drop." Also in the same case 13, "The civil engineer before referred to assured me that from his knowledge of the geological formation of the country, he could say confidently that there was no chance of finding a water supply under my new house at a depth of less than 120 or 130 feet; and his opinion was confirmed by another geological authority who was then in the neighbourhood." Mullins had previously, on the site of the new house, "indicated two lines, about 30 yards apart, along which he said water was flowing. This was all he could find there." Ultimately Sir

W. E. Welby-Gregory "determined to disregard the geologist and have a well sunk on one of these lines," where water was found in due course.

4. That the power is not only shown in finding spots where water is, which might be explained by supposing that there was water everywhere in that region at that depth—but spots where it is not. See again case 13 where, as previously mentioned, a well was sunk to a depth of 25ft. on one of Mullins' lines. Subsequently a shaft for a lift was sunk "between the two lines . . . . some 12 yards or 15 yards from either, and to a depth considerably greater (10ft. or 12ft.) than the well. As no water came into this, though the formation was precisely similar, and the well has not been affected by it, I am satisfied," Sir W. E. Welby-Gregory continues, "that, had I not employed Mullins, I might have sunk wells in any number to no purpose." See also case 31 where a well is sunk so that its side is below the point indicated and water comes in from that side. And case 41 where Mr. Adey says, "Stokes . . . . told my foreman that it was of no use going on with that well . . . . and told them where the spring was, and that it was near the surface. . . . . I told him not to pay attention to such rubbish, and to continue sinking the well. . . . . At the depth of 40ft. there was not the slightest appearance of coming to water. My men then threw out a hole where Stokes indicated, about 30ft. or 35ft. from the well, and at a depth of only 5ft. from the surface came upon a spring."

5. That dowsers believe themselves to possess some sort of occult power, although they can offer no explanation of it. It would appear, moreover, that they rarely make mistakes in locating wells. But some allowance must be made for the fact that most of our evidence has reached us from correspondents obtained through the diviners themselves. See, however, cases Nos. 3, 5, 7, 38, 39, 40, and 41.

We have next to consider the experiments recently made with diviners by myself and other members of the Society.

Mr. W. S. Lawrence, of Elton House, Bishopston, Bristol, a retired stone merchant, was known by my father for many years as a Poor Law Guardian, and on December 26th, 1882, he came over to try some experiments with me in my father's grounds. He is an elderly man, of much intelligence, and is widely known for his power of dowsing, which he still occasionally practises. He uses a piece of watch-spring, about a foot long, in preference to the hazel-rod.

He first walked about the garden and fields and located numerous springs. We then blindfolded him and took him over the same ground, but the springs were so plentiful that it was not easy to ascertain whether he found them again. Once or twice, however, we clearly observed that the rod did *not* move with the dowser blindfold in spots where, just previously, he had located springs.

We next tested him with metal, hidden under plates. Mr. Lawrence had not tried this for some years, and was doubtful of success. But at the first attempt, he found, amongst seven plates, the two under which metal objects were placed. On this occasion some of the numerous spectators knew which were the right plates. In a second attempt, when none of those present knew which were the right plates, he succeeded in finding one object, but was at fault in regard to the other. In a third attempt he wholly failed.

A lady present was thought to possess some power, and a metal object was hidden for her under one amongst eight plates. The rod moved over two plates, and both being taken up, metal objects were found under both of them, the second having been added by one of those present, unknown to the dowser and all others. This circumstance appeared to be very remarkable, but as the lady in question has totally failed in her later experiments, this success must be regarded as accidental, or possibly it may be explained by Thought-transference.

The result of these experiments as a whole was, therefore, by no means conclusive, as the existence of the supposed springs was not tested by sinking wells, either on this occasion or after the experiments about to be recorded.

In March, 1883, I received from Mr. Jenkins an introduction to Mrs. B., of Clifton, a lady who is known to be an amateur dowser.

Mr. Podmore and I called upon her, and we then learned that several years ago she found out that she possessed the power of divining for water, in connection with the well-known case of discovery of water by Mr. Lawrence, at Lawrence Weston. She also uses a steel watch-spring.

The following report was drawn up by us at the time:—At our request Mrs. B. tried to discover money hidden under plates, by means of her steel spring. She stated that she had not attempted this before, and that she "*did not believe in it.*" In the first trial coins under two plates out of five were correctly discovered. In this case all present but Mrs. B. knew which were the right plates. In two subsequent attempts in another room, when no one present knew the right plates, she failed partially in one case and completely in the other.

We drove to Cote Bank, and in the first place tried over the same ground (a gravel path) that Mr. Lawrence had tried in experiments previously recorded. Mrs. B. located a spring on a spot which Lawrence passed over, and several yards further on Mrs. B. passed over a spot where Mr. L. thought he detected a very strong spring. A dozen yards further on Mrs. B. found a spring in about the same position as a continuation of the last mentioned spring was located by Mr. L. We told Mrs. B. of the strong spring detected by Mr. L. and passed by her, and on her return she confirmed Mr. L.'s discovery, though locating the

spring a yard or two away. We then blindfolded Mrs. B. and led her over the same ground, when the results were decidedly different, the steel watch-spring moving in several cases where previously it had been still, and *vice versâ*. But, owing to the limited space and peculiar varieties of level, the result was not satisfactory in either way.

We then asked Mrs. B. to test carefully a long level gravel walk, and we marked several springs which she detected upon it. She went over the ground a second time, and did *not* very certainly confirm previous discoveries. We then blindfolded her, and led her over the same ground. The watch-spring now moved in four or five spots, only one of which approximately coincided with a previously marked spring. Mrs. B. stated that the springs were very slight, the rod movements vague and unsatisfactory.

We next tried over a place where we told Mrs. B. that an underground current of water existed. This she asserted to flow about a yard from the spot where both the appearance of the land and other circumstances led us to believe the drain actually was. She maintained her opinion after several trials with closed eyes. She traced the drain (which carries the purest spring water) for 100 yards, but the conformation of the ground left little choice of directions.

We then tried coins under plates. We selected a spot in the drawing-room clear of springs, and placed four plates on a wooden table, which we moved round whilst Mrs. B. sat in a chair. One of the party only hid the metal (a large heap of silver, and a brass ½lb. letter weight), and then left the room. The conditions were therefore carefully arranged. Mrs. B. selected two out of four plates, because the watch-spring moved freely over them, whilst it remained still over the others. These plates over which the rod moved were the two covering *nothing*. The other two covered the metals.

Mrs. B. then dowsed over the house, and detected correctly a very strong spring in a passage, which we told her led over a well. We went into the cellar, and here, over the same spring, close to the pump, the rod did not move the first time she passed over it. She detected springs in several places where Lawrence had not, and *vice versâ*.

This concluded our experiments with Mrs. B.

The next experiment was on a larger scale.

About this time Mr. Pope, of Clifton, had a well successfully located for him at a farm near Locking, in Somerset, by a dowser named Thomas Young. He sent us a report of the case and of some experiments made with the rod, and offered us permission to dig a trial well at the same place. Some members of the Society generously provided funds, and in May a party of us went down, including Professor W. J. Sollas, then of Bristol, now Professor of Geology at

the University of Dublin. He has presented us with the report which precedes this paper.

I must now proceed to the second part of the inquiry, by examining more particularly the methods and claims of diviners. My object is to ascertain on what points the theory and practice of the majority of diviners is agreed, and whether any physical or psychical cause or causes can be suggested which might furnish an explanation of the phenomenon.

As previously stated, the orthodox rod is a hazel twig shaped like a V, and from 1 to 3ft. in length. But many other materials are used; a piece of steel watch-spring is employed in Somersetshire; and in other places a whale-bone instrument with a terminal chamber full of mercury, or full of a mysterious unknown compound; or ordinary forked rods made of any sort of wood, green or dry, of iron or brass wire, or, as one authority says, "of any other supple and solid matters." Occasionally the genuine dowsing-rod seems to be unforked, and we hear of cases in which no rod at all is used. When a piece of watch-spring is used, it is held bent into a bow, which acts in the same manner as a forked twig. The rod is grasped by both hands, in various ways, which however have this in common, that the dowser holds it in a position of extreme tension, and at the same time of unstable equilibrium. Consequently a very slight variation in the pressure produces forcible and even violent motion, apparently quite unconnected with any action of the dowser. The dowser, thus holding his rod, walks slowly across the ground to be tried, and when he comes over the spring of water, or vein of metal, the rod begins to move. How it moves seems to depend upon the locality and the practice of the dowser. Sometimes it is held horizontally, and jerks violently up, as if repelled by the water spring; sometimes the water appears to attract the rod, and it moves downwards; sometimes it moves in a complete circle, approaching and then receding from the water.

The powers of the rod are a question in much dispute. It has been used to discover many things, viz.:—

Water in general (as in buried tubs); spring water, as opposed to surface water, when both are in buckets; water springs [even beneath the sea]; running water, as distinguished from all other water. Any metals, metallic ores, or compounds, and pure metals as opposed to alloys; coal, mineral oil, ochre, gypsum, red chalk, sulphur, &c.; lost boundaries of estates; Protestants; murderers, thieves, and other human criminals; and in fact, to quote the author of "Jacob's Rod," it can discover "many hidden things about which one is often much troubled, but few persons know the way to find them."

The popular theory is that there is some unknown force acting directly between the hidden thing and the rod. Such a notion need not, however, be seriously discussed here. It is impossible to imagine any

direct interaction between rods (which may be composed of wood, metal, or whalebone), and hidden things, which may be water, metals, metallic compounds, mineral oils, &c.

Moreover, this alleged occult force in some places attracts the rod to the object sought, whilst in other localities it would seem to have a repulsive effect, and again, in other cases, it produces a rotatory movement. This variation occurs even in the same locality, and with one diviner. It seems obviously highly improbable that any specific force can exert such varied movements.

The next theory to be examined is that the rod is moved by the diviner's muscles, and is merely an index of the effect of some subtle force which emanates from the water or metal, and acts on the diviner himself.

It is a moot point whether the diviner feels any sensations when the rod is working. Most witnesses assert that they have none whatever, whilst Mullins, and one or two others, state that they experience a thrill, or vague sensation, when they come upon the water.

Diviners always tell us that they are certain they are not moving the rod, and even that they attempt to restrain its action. No doubt, they are perfectly honest. But anyone who has had the smallest experience in psychical research is aware that such statements have no value whatever. We know by experience what care and study are required to discover whether our own hands are conspiring to deceive our intellects. We are not, therefore, surprised to find that honest dowsers are easily deceived by the unconscious actions of their own muscles.

It is commonly asserted that the rod cannot be made to turn, except in the hands of a diviner. This is simply a mistake. Anybody can work a divining rod, and can produce all the usual motions by means of muscular contractions, so slight as to be scarcely apparent to a careless observer.

Moreover, how can believers in the occult rod-moving force explain the curious experiment previously recorded, in which the steel spring moved strongly when held over two plates covering nothing, and was motionless over two others covering masses of metal? In this case it seems clear that the rod movements must have been caused by the unconscious volition of the diviner; and in this case above all others, the honesty of the diviner is beyond dispute.

The theory that a peculiar physical constitution is an important factor in the diviner's art receives much support from the fact that only a few persons are able successfully to use the rod. Mr. Baring Gould, however, tells us that on one occasion he saw watching the diviner a

crowd of villagers who each in turn tried the rod over the hidden spring; no one of them could restrain its movements, and they were somewhat surprised that Mr. Gould, when his turn came, succeeded in keeping it motionless. This would seem to be an exceptional case, and we shall now proceed to examine the phenomenon in its simplest form. We find that spring water and metals are alleged to have a certain obscure effect on certain exceptional persons, and that this physical disturbance of their tissues causes slight muscular contractions, resulting in movements of a rod held in their hands.

As regards pure metals the evidence is entirely experimental. Somebody hides a heap of coins, and the dowser proceeds to discover them as a proof of his power.

Unconscious indications given by the spectators would afford a simple explanation of this class of phenomenon; and in cases where these indications are excluded, the diviner may be assisted by the newly discovered faculty of Thought-transference. In harmony with this suggestion, we found in our own experiments that when those present knew the chosen plate, the diviner was often successful, whilst, when no one present knew it, the diviner generally failed. Messrs. Pass and Tawney tried experiments of this sort in Somersetshire, an account of which they read to the Bristol Naturalists' Society in 1874. In five trials, in each case amongst seven or eight objects, the diviner was successful three times in all. Messrs. Pass and Tawney attempt to explain away this decidedly remarkable result; but their success in so doing seems to me by no means so remarkable.

In a similar manner we may explain how it is that Mr. Stokes, of Newbury, can detect between two buckets of water, which of them is surface and which spring water, although older diviners assert that when the rod moves they "do not know if it is a spring or stagnant water." ("Jacob's Rod," p. 24.)

Next turning to metallic ores, I have been unable to obtain evidence beyond the general statement that the rod is commonly used in Cornwall for mining purposes. Mr. Raymond, in his paper, gives many instances of its complete failure in America, and of money lost by mining companies through their belief in its efficacy.

Finally we come to the use of the rod for the discovery of water springs. The evidence for this, as shown in the accompanying abstract, is exceedingly abundant and strong. There are plenty of diviners of good repute now living who without doubt use the rod, and find water. But the value of this evidence wholly depends on the abundance of water springs below the surface of the earth. If water is commonly to be found wherever one digs, but little of the evidence before us is of much value. This question can only be answered decisively by sinking a number of experimental wells such as those at Locking. This, how-

ever, is a very costly operation, and I fear the experiment is not very likely to be made.

Mr. Raymond's summary of the evidence is as follows: After stating that the application of the rod to the discovery of metals, coal, buried treasure, oil, &c., is shown to be chimerical, he proceeds in a passage of which the following is the substance :—

"The case is somewhat different with the discovery of springs and (since ore-deposits always have been and often still are the channels of springs) of ore-deposits.

Here we have much stronger and more abundant evidence in favour of the rod, and here, in my judgment, there is a residuum of scientific value after making all necessary deductions for exaggeration, self-deception, and fraud. There is undoubtedly a practical science of discovering metal-deposits and springs. Everybody knows that Indians in our desert West can find water where most white men cannot, and the experienced frontierman has learned the art from the savages. The superficial signs of water springs are often subtle, and in settled districts they are little studied by the inhabitants. It is not necessary that a farmer who will want to locate a well once in his lifetime should know the signs of water as a ranchman must know them. Hence men who are keen observers get impressions amounting to a local science of the "lay" of the rocks, the difference in surface vegetation, &c., &c. The unconscious judgment of such an expert may decide upon a given spot, as he walks over it.

This brings me to the final inquiry whether there may not be, apart from unconscious skill or judgment, a purely physical effect produced by a subterranean spring upon a person walking over it. The effects of moisture and temperature upon the nerves, are very striking, and here, I think, is a matter which writers on the Divining Rod have generally overlooked."

With this suggested explanation of the possible residuum of fact, Mr. Raymond closes his most valuable paper, and a summary of the evidence here presented would seem to point to a similar conclusion.

If we looked only at the history of Divining, we should dismiss it at once as a superstition. And, again, if we confine our attention to test experiments, we find nothing that merits serious discussion.

But the evidence for the success of dowsing as a practical art is very strong, and there seems to be an unexplained residuum when all possible deductions are made for accident, for local knowledge, and for inaccurate observations.

Mr. Raymond's hypothesis of the effects of moisture and temperature is not a very satisfactory one, but no better has yet been suggested.

APPENDIX I.

ABSTRACT OF EVIDENCE

Collected by Mr. E. Vaughan Jenkins, of Cheltenham.

Diviner—John Mullins, of Collerne, Wilts.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
1	H. D. Shrines, Claverton Manor, Bath	Somerset	1852 and since	Mullins, a stranger in the place, located several wells successfully, and pointed out a spot where a spring was known to be; details of other cases. Archdeacon Earle was a witness.
2	W. J. Brown, Hazlebury House, Box	Somerset	1872	Details of two good cases; only well near, 180ft. deep; found a disused well which was covered over; found sovereigns hidden under stones.
3	Col. Wilson, Sleaford	Lincolnshire		Employed M. "a good deal;" sunk six or seven wells by his advice, all successfully. Wells found in dry country where large sums had been spent in boring for water without success.
4	J. H. Vessey, Welton Manor, Louth	Lincolnshire	1876	Well located correctly, and depth fairly given.
5	Capt. H. Smith, J.P., Folkingham	Lincolnshire		Given in full in Appendix II.
6	R. H. C. Nevile, Wellingore Hall, Grantham	Lincolnshire		Water found near trial well 10ft. deep. Spring at a higher level and only 5ft. down; other striking details.
7	A. Taylor, Hay Hill Dairy, Bath	Somerset	17 years ago up till now	Has known him 17 years, and has employed him often, with most satisfactory results.
8	G. W. Johnson, Steward to Lord Lindsay, Stamford	Lincolnshire	1883, &c.	Details of several cases of success; says that local knowledge is needed to judge depth of spring. An emphatic testimony.
9	Geo. Hancock, Corsham, Wilts	Wilts		Given in full in Appendix II.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
10	Geo. Cooling and Sons, Seed Warehouse, Bath	Somerset		Given in full in Appendix II.
11	E. Issott, Ardwick Brewery	Manchester		Abundant water at depth predicted. Ground near had not been tried for water.
12	Hon. E. A. Pelham, Grantham	Lincolnshire	1877	Strong spring found at depth predicted.
13	Sir W.E.Welby-Gregory, Bart., M.P., Grantham	Lincolnshire	1877	Given in full in Appendix II.
14	F. T. Mott, F.R.G.S., Birstal Hill, Leicester	Leicester	1882	Given in full in Appendix II.
15	J. Thompson, Steward to the Duke of Beaufort, Badminton	Glo'ster	1877	Water scarce. Strong spring found deeper than predicted. Mr. T. was then not a believer in the Divining Rod.
16	G. Aust, Agent to Taylor Trustees, Bath		1878	Well found at about predicted depth, "where it is difficult to obtain good water."
17	P. Penchin, The Brewery Box	Wiltshire		Two cases described.
18	J. Copley, Melton Mowbray	Lincolnshire		Found a small spring at 7ft. in a locality where water is scarce; had previously sunk 75ft. in vain.
19	J. R. West, Bath	Somerset	1877	Found spring for a brewery
20	J. H. Vesey, Malvern		1883	Spring found at right depth (letter addressed to Mullins)
21	The Hon. M. E. G. Finch Hatton, M.P., Sleaford	Lincolnshire		Given in full in Appendix II.
22	Benj. Perry, Bristol	Glo'ster	1873	Springs found in two cases by watch-spring bending *down*. Detailed account.

Diviner—W. S. Lawrence, of Bristol.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
23	Samuel Lang, 3, Bath Parade, Bristol	Glo'stershire		Well dug in vain at cost of £100 ; spring located 20ft. away; tapped by "culvert." Details given. A remarkable case.
24	J. Parsons, Accountant, Bristol	Glo'stershire	1882	Water found for Great Western Electric Light Company.
25	J. G. Davey, M.D., and Ass. S.P.R.	Somerset	1865	Old well at Northwood Asylum insufficient; new spring found and culvert driven to it. Careful and detailed account.
26	W. H. Cowlin, Building Superintendent, Eagle Land Co.	Somerset-shire		Detailed account of discovery contrary to local expectation. Clear and good.
27	Bristol Port and Channel Dock Company	Glo'stershire	1883	Letter from Dockmaster and Hydraulic Foreman; accurate details ; water discovered at 70ft.
28	T. Stone, Bristol	Glo'stershire		Well dug with moderate success.
29	Mrs. Hare, Newton Abbot	Devon		Spring found, where local men considered sinking hopeless ; Lawrence a stranger. Good case.
30	R. Harvey, Ashton Gate Brewery	Somerset	1873	Spring found for the Company. (Mereweather also present as dowser.)
31	Stephens and Bastow, Bristol; S. Martell, their Foreman; H. J. Shaw, Architect, London	Sussex	1879	Given in full in Appendix II.
32	Col. Blount, Dorchester	Dorset		Strong spring found.
33	J. H. Lockley, Lewins Meade Brewery, Bristol	Somerset		Valuable spring found for the Company ; also a well pointed out which was known only to writer.
34	H. Crisp, Architect ; R. Butterworth ; Lawrence ; Mereweather ; Mrs. Bengough, &c.,&c.	Glo'stershire	A few years ago	Given in full in Appendix II.

Diviner—W. Stokes, of Newbury, Berks.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
35	Canon Portal, Newbury	Berks		Strong spring found.
36	Capt. Ward, Newbury	Berks		Spring found.
37	H. Taylor, Oare Hermitage, Newbury	Berks		Detailed account of experiments.
38	W. Chatters, Sanderford Priory, Newbury	Berks		Eight or nine times and no failure.
39	W. Church, Builder, Newbury	Berks		Never known failure. Gives case of discovery 2ft. from vain boring, 35ft. deep. Has seen Stokes distinguish between a bucket of spring water and one of stagnant water.
40	Charles Adey, Marlborough	Wiltshire	1880	Successful find. Diviner not in his own locality.
41	W. G. Adey, Newbury	Berks		Given in full in Appendix II.

Diviner—Charles Cross, coachman, of Hallatrow, near Bristol.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
42	T. Killen, Farrington Gurney	Somerset		Spring found at 30ft.
43	M. J. Williams, Paulton	Somerset		Spring found at 37ft.
44	G. Thresher, Frome	Somerset		Spring found.

Diviner—Thos. Pavey, of Cheddar, Somerset.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
45	T. K. Taplin, F. J. Nalder, Westbury-sub-Mendip	Somerset		Disputed case, in newspaper correspondence. Mr. Nalder had a well found by three independent dowsers and considers it clear and valuable. A former owner pooh-poohs the matter.
46	Alfred Smith, Pershore	Worcestershire	1883	A good case; Pavey went, on Mr. Vaughan Jenkins' recommendation, to a new district; water found in an unlikely place.

Diviner—George Lookyer.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
47	E. V. Jenkins, Cheltenham	Monmouthshire	1852	Given in full in Appendix II.

Diviner not known.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
48	Angus and Co., Contractors, Australia	Dorset	1874	Given in full in Appendix II.

APPENDIX II.

SELECTED CASES

From Mr. E. Vaughan Jenkins' Collection.

No. 5.

From CAPTAIN HENRY SMITH, Horbling, Folkingham, Lincolnshire.

December 26th, 1882.

It will perhaps be the best answer to your letter respecting John Mullins (the water-finder as he is called here) if I tell you as well as I can what took place when he was at my place. My object in sending for him was to obtain his advice as to the best place to bore for water on some of my land, and when he came he said he must get a twig. I told him I did not believe in his conjuring tricks, I only wanted his advice, and he then said he was no conjurer, but was not the slightest use without his tools, so I stood by whilst he cut a couple of white thorn twigs. My nephew went with me, and we each carried a thistle spud. In crossing the first field I said "I have water here, so need not trouble you," but Mullins said he might as well carry the twig in his fingers, and whilst we were talking about the stock in the field he stopped suddenly and said we were crossing water. This I knew was the case, although there was then no appearance of any. The next field was the one I especially sent for him for, as I had dug a pond and bored about 40ft., but found no water. I took him to the side of the field above the pond, and after casting about for some time he said decidedly there was no water there, and proposed trying the other side, where he said there was plenty. We marked the place, and I have since bored there and found an abundant supply of water. I then took him across two other fields, and in each he found what he called a weak spring not worth boring for. We were at that time walking a little distance from him on each side, and marked the ground where he said he was over water, I believe unknown to him. On our return I said, "Now Mullins, I shall blindfold you and see if you can find those places again." He agreed to hold his head back and keep his eyes firmly closed, and as far as we could see he did so, and in each field stopped at exactly the same places we had marked, and the marks on the turf were so slight and from 50 to 100 feet from him on each side that I do not believe he could have seen them if he had opened his eyes.

We then came home to my lawn, and I showed him a beautiful spring, and also informed him what quarter we supposed the water came from (north); he tried all over the north side, and said most positively there was no water there, and then proceeded to the south side, and seemed almost convulsed. He said it was the strongest spring he had ever felt. I and my nephew took hold of the twig on each side below his fingers, and still it twisted up. I have bored there and found abundance of water. I may say here that Mullins had not been in our neighbourhood before, and distinctly asserted the water bearing strata lay very different from that we had always before supposed. We are on clay between the fen and limestone, and the land rises steadily from us until we reach the highest

point on the Great Northern Railway (Great Ponton), and my theory was that the rainfall there percolated the limestone and under the clay strata which is continued from thence to us. This Mullins quite denied, and our borings have proved the correctness of his statement so far as this, that there is abundance of water in the direction pointed out by him, but there may also be water where he says there is none; this has not been tested.

I did not look upon Mullins as a charlatan, but suspected he did a good deal by guess, and must confess to being unable to prove this.

That he could find the same indications of water blindfold quite puzzled me and my nephew, too, who is a thoroughly practical man.

That the twig was acted upon when Mullins had no control over its movements, we were also both satisfied. Mullins was very confident both in his assertion there was water or otherwise, and certainly did not adopt the usual *rôle* of a charlatan of falling in with the views of those who paid him.

I have recommended him to some first-class business men, and all tell me they are satisfied he is no impostor. I may add that, as a magistrate, I have frequently been told that I ought not to countenance such a man, but I generally take my own line, and try to arrive at some conclusion. In this matter I can only say I do not understand it at all, but give you the facts as near as I can, and shall be glad to hear of any further steps taken to explain this mystery.

No. 9.

From GEO. HANCOCK, "Fair View," Corsham, Wilts.

January 2nd, 1883.

Our testing of John Mullins' water finding powers has been of a negative rather than of a positive character. We have lately sunk a number of shafts with a view to mining operations, and our desire has been to select spots where there was no water to hinder us in our sinkings.

Mr. Mullins has gone over the ground with me and pointed out such spots. He has always been careful to explain "that we may, in a wet time, be troubled with surface water, but that we should find no springs." In every case he has been right. But in a shaft which we are now sinking we have come to a fissure about 40 ft. from the surface, and through this fissure there comes so much water that I have to-day been obliged to arrange a pumping apparatus in order that the men may be able to proceed with the work.

The ground in the neighbourhood of this shaft Mr. Mullins reported to be full of springs, and sinking our shaft below the level of the springs and breaking into the fissure may have diverted the water from its natural course. In that case there may not have been, when Mr. Mullins went over the ground, any water where now it is coming into our shaft.

Mullins generally forms an opinion—and a roughly correct one—as to the depth of the water below the surface, but he lays no claim to accuracy on this point. He says that a weak spring near the surface exerts the same degree of force upon the twig as a strong spring far down.

I have observed that after he has been walking about for some hours he gets physically exhausted, and, if I mistake not, somewhat irritable. He has told me that when he passes over a strong spring and the twig turns quickly, a thrill runs through his entire system, and that if he follows the water finding for several days consecutively, he is unable to sleep at night.

From what I have seen and heard of Mullins, I believe him to be a perfectly honest and reliable man. It is certain that he finds hidden springs and streams of water by the means above described, but he himself is ignorant of the natural law—and I make no doubt it is a natural law—by virtue of whose operation there is an affinity between him and the stick, and between the stick and running water, when the stick is in his hands.

No. 10.

From GEO. COOLING AND SON, The Seed Warehouse, Broad Street, Bath.

January 6th, 1883.

We had very strong proof of the efficacy of the divining rod through Mullins, of Colerne. We had gone to very considerable expense in excavating to secure a supply of water for our greenhouses at the nursery, and were about giving up the matter in despair when we were recommended to send for Mullins, which we did, although with a great deal of incredulity. He, however, pointed out a spot where he said at a distance of 10 or 12 feet we should get a good spring, and within that depth we found water, and have had a constant supply running through all our houses since.

No. 13.

From SIR W. E. WELBY-GREGORY, Bart., M.P., Denton Manor, Grantham.

January 28th, 1883.

In the spring of 1877 I was preparing to build a large country house on a new site, which I had selected on account, among other reasons, of its dryness. This site was on a large plateau of red loamy soil, resting on a bed of solid rock* several feet thick, in which there was no indication of water; and it was a problem where the water supply was to come from, as there was none apparent which could be brought to the house without great expense. While puzzling over this I heard that John Mullins, of Colerne, had been employed to find water at various places in the neighbourhood with very remarkable success; that he had been tested in almost every conceivable way, and that not only country gentlemen and farmers, but plumbers in the neighbouring towns had frequently called in his aid, purely as a matter of

* The rock I believe is marlstone, and the thickness of the upper bed has been since ascertained to be between seven and eight feet.

business, and I was at last induced to send for him. When he came, I asked him whether he required a twig of any particular wood, and he replied that almost any wood would do, except lance-wood and box-wood, which were too stiff. He then cut for himself from the nearest convenient tree, which I think was a sycamore, a forked twig in the shape of a Y of a foot or 18 inches long. I set him to work for a few minutes at first on my lawn, sending him over parts where I knew there was a current of water at a depth of a few feet, though none was visible or audible on the surface. He slowly quartered his ground like a pointer, bending forward and holding the twig about the level of his knees, point downwards, and tightly grasped an inch or two from the extremities of the prongs of the fork, with, if I recollect rightly, the thumb and fourth and little fingers of each hand below, and the fore and middle fingers above the twig. Whenever he crossed the water, of which I have spoken (a drain), the twig turned upwards in his hands, though he apparently resisted its doing so to the utmost; and the same thing happened at other parts of the lawn, where, though we did not know of any runlet, it was very likely that one existed. I then took him to the kitchen garden, where we had always been much in want of water; and presently the twig stopped him at a spot apparently as dry as the rest, and with nothing distinctive about it. I asked him what amount he supposed there was, whether running or stagnant, and at what depth. He answered that he was certain there was water, and that it was running, for no amount of stagnant water had the smallest effect upon him; but that he could only guess at the amount and the depth from the force with which the twig turned up. From his experience he should say that it was a stream not thicker than a walking stick, and at a depth of from 20 to 30 feet. He traversed the rest of the kitchen garden without finding any more, and I next took him to the site of the new house. Here he soon indicated two lines, about 30 yards apart, along which he said water was flowing in somewhat greater volume than the rill he had found in the kitchen garden, and at a depth, he thought, of from 30 to 40 feet. This was all he could find there, and as this seemed hardly likely to be sufficient for my requirements, I felt that it must be looked for at a greater distance. To save time, I took him to a spot from which he could see the contour of the hills (about a mile distant from the house) from which most of our water comes, showed him what spring we knew of, and told him to choose for himself the best part to try for more. He unhesitatingly selected the upper part of a certain hill (which was afterwards pronounced by an eminent civil engineer to be unquestionably the most likely spot within sight to contain water), and thither we went. He tried the whole of that hillside over, without finding a drop, and we afterwards took another hillside with no better success. It was now late, and Mullins was evidently becoming exhausted. I felt that I had seen enough to convince me that he was no impostor, and that whatever discoveries he made were due to some force over which he had no control, not to any power he had acquired by experience or observation of making good guesses at where water was likely to be; but I was not satisfied that he had found anything for me which promised to be of any pr[illegible]: so I dismissed him to tea in the housekeeper's room. After [illegible]he servants whose curiosity was excited got him to exhibit his

art to them, and tried whether they themselves had any power with the twig. My gardener, Joseph Towers, found that it worked in his hands nearly as strongly as in those of Mullins himself. I took no further action then in the matter; but described what I had seen to the civil engineer before referred to, who, though by no means altogether pooh-poohing it, assured me that from his knowledge of the geological formation of the country, he could say confidently there was no chance of finding a water supply under my new house at a depth of less than 120 or 130 feet; and his opinion was confirmed by another geological authority who was then in the neighbourhood. So I virtually gave up all hope of deriving any benefit from Mullins' assertions. Some weeks later, however, my gardener came to me and said that he was in great want of water in the kitchen garden; that he had tried repeatedly with the twig over the place where Mullins indicated a rivulet; that it invariably turned up at the spot, and that he was quite convinced there was water there. If I would allow him, he could sink a well with the garden labourers, so as not to involve any additional expense. I consented to this; the well was sunk to a depth of nearly 20ft., when water poured into it freely, and it has supplied a long range of hothouses ever since.

I had previously decided upon bringing down the main supply of water to the new house in pipes, by gravitation, from a considerable distance; but as this was a heavy work, necessarily taking much time, the question of a supply during building had become urgent, and on the strength of what had occurred in the kitchen garden, I set Towers to traverse the new site, where the lines of water indicated by Mullins had been marked by pegs 60 yards or 70 yards apart, and just visible above the grass. These lines Towers and his twig emphatically confirmed, and I proceeded to test him. I had the projecting extremities of the prongs of the twig held tight by pincers, so that there could be no voluntary action on Towers' part when crossing the marked lines. Despite of this, the point of the twig twisted itself upwards, till the bark was wrinkled and almost split, while the strain and pressure upon the muscles of the man's hands were most apparent. I then blind-folded him, and turned him loose. The result was precisely the same. Whenever he crossed Mullins' lines, up went the twig. The presumption now appeared to be so strong in favour of the twig that I determined to disregard the geologists, and have a well sunk on one of the lines. This was done; at the depth of about 28ft. the water rushed in, and rose till it stood about 8ft. deep, at which it now remains, having, in the meantime, fully supplied all the requirements during building the house, which were probably not less than 1,000 gallons a day for three years or more, and since that having acted as an ample reserve to the house for all purposes whenever the distant supply for any reason has had to be shut off. I may add that I have since had occasion to sink a shaft for a lift between the two lines indicated by Mullins, some 12 yards or 15 yards from either, and to a depth considerably greater (10ft. or 12ft.) than the well. As no water came into this, though the formation was precisely similar, and the well has not been affected by it, I am satisfied that, had I not employed Mullins, I might have sunk wells in any number to no purpose under my house, unless I had happened to hit upon the rills indicated by him with such perfect precision.

No. 14.

From F. T. Mott, F.R.G.S., Birstal Hill, Leicester.

Written for the British Association Meeting at Southampton, 1882.

I happen to be the owner of two acres of land on the slope of a hill in Charnwood Forest, Leicestershire, 600ft. above sea level. The hill consists of some ancient metamorphic slate, of which the geological horizon has not been definitely determined. On this land there is no spring water. Springs exist in several directions at distances of from half-a-mile to a mile, but on the other hand several wells have been sunk at similar distances in other directions but no water found. In one case the boring was carried to about 100ft., in a rich valley, but the search for water was unsuccessful. About a month ago I heard accidentally, but from what appeared to be a disinterested source, that a man was living near Chippenham, in Wiltshire, who had a peculiar method of discovering springs. As a good water supply would double the value of my land, I made some inquiries and received from a brewer at Manchester, and from a large agriculturist at Grantham, such strong testimony as to the ability of the person referred to that I thought it worth while to send for him and to let him try his skill. He came accordingly, a fine, sturdy, intelligent artisan, with no scientific knowledge, but a frank, manly address, and not the slightest appearance of a charlatan. He said, "If there is a spring on your land I think I can find it, but my method is a mystery to myself; I am always in doubt whether it will be successful, but hitherto it has never failed, and I have performed the operation hundreds of times." I took him to the land, and on the way he stopped at a hawthorn hedge and cut out a branch. This he trimmed up until he had got two twigs about the thickness of a cedar pencil, united at one end so as to take the form of the letter V with something like a right angle at the base, and each of the arms rather more than a foot long. On reaching the ground he took one of the arms in each hand, holding his extemporised implement before him with the angle downwards, and he walked across the field in a direction at right angles to the slope of the land. When he came to a certain spot I saw the angle of the stick turn upwards suddenly and forcibly. He stopped at this point and desired me to drive in a stake. Then he went about 20 yards further down the field and crossed it again in the same manner. This time, however, I accompanied him step by step, holding his hands firmly grasped in mine and watching carefully for any movement of his muscles. Again the angle of the stick leaped up, twisting round within the grasp of his fingers, and another stake was driven in. A third time he crossed the field, still lower down, while I held the two arms which projected beyond his closed hands, nipping them with my full strength and watching his hands at the same time. Yet again the stick turned up as before, twisting round within my fingers as well as his, and a third stake was put down. This process was gone through several times more, with like results, and we then found that the stakes stood in a diagonal line across the middle of the field. "Now," said he, "if you sink a well anywhere on that line you will find water, and judging from the force with which the stick moved, and my previous experiences, I should say that you will have to sink at least 30ft., and possibly as much as 40ft. or 45ft." As the line thus marked

out was not conveniently situated I desired him to try the upper part of the land, which he did, but without result. Another short diagonal line was, however, found at the bottom corner.

Not placing much reliance upon what looked like a sort of necromancy, I yet resolved that it should be tested, and just before I left home for Southampton I sent a gang of well-sinkers to the spot. Yesterday morning I received the following telegram: "Well, 28ft., sandy bottom, water 2ft. deep." I may say that to the depth of 20ft. the soil was dry, and I had received notice two days previously that on reaching 21ft. it became moist for the first time.

These are the facts, what is the interpretation of them? It may be said that the finding of water was purely accidental, and that a well sunk in any part of the field would have been equally successful. But there is no proof of this, and the experience of other persons at Manchester and Grantham, and the extraordinary behaviour of the stick have still to be accounted for.

Of course, my own unsupported testimony as to the behaviour of the stick will not, and *ought* not to be taken as absolute proof of my statement. There were in fact three other witnesses, but I cannot produce them, at Southampton. Can any physical cause be suggested which could in any way be supposed to produce such behaviour in a forked stick? It had much the appearance of the deflection of the index in a galvanometer. If this man had a particularly sensitive constitution, and if an underground water-stream running for centuries in one direction could set up some kind of electric current, might he not actually become a galvanometer giving visible indications of that current?

Note.—The well was sunk to the depth of 31ft. and contained always 6ft. of good water.

No. 21.

From the Hon. M. E. G. Finch Hatton, M.P., 23, Ennismore Gardens, S.W.

February 29th, 1884.

Hearing that J. Mullins possessed the power of finding springs of water, by means of a forked twig held between his hands, I desired him to come over to Haverholme Priory, near Sleaford, where I live.

This is what happened. First he cut a forked twig from a living tree, and held it between his hands, the centre point downwards and the two ends protruding between the fingers of each hand.

He then stooped forward and walked over the ground to be tried. Suddenly he would stop and the central point would revolve in a half circle until it pointed the reverse way. This he stated to be owing to the presence of a subterranean spring, and further that by the strength of the movement he could gauge the approximate depth.

My brother, Hon. Harold Finch Hatton, and I each took hold of one of the ends, protruding as stated above, and held them fast while the phenomenon took place, to make sure that it was not caused by a movement, voluntary or otherwise, of the man's own hand or fingers. The tendency to

twist itself, on the twig's part, was so great that, on our holding firmly on to the ends, the twig split and finally broke off.

The same thing occurred when standing on a bridge over a running stream.

Stagnant water, he states, has no effect on the twig.

Though now convinced ourselves of the man's good faith, and of the automatic—or rather independent—action of the twig, we resolved to test Mullins in various ways, of which the following is an account.

First.—We established it on good authority that Mullins had no previous acquaintance with Haverholme.

1. I took him on to the grass in front of the house, across which the water-supply pipe passed. There was no indication of its presence on the surface, nor did I previously mention its existence to Mullins; on crossing it, the twig moved in the manner described, and he could trace the water to right and left by its means, along the path actually taken by the pipe.

2. On our way to the kitchen garden Mullins discovered a spring on the open lawn, whose existence was unknown to me, it had been closed in so long, but was subsequently attested by an old labourer on the place who remembered it as a well, and had seen it bricked in many years before.

3. On reaching the kitchen garden I knew that a lead pipe, leading water to a tap outside the wall, crossed the gravel path at a certain spot. On crossing it the twig made no sign. I was astonished at first, till I remembered what Mullins had said about stagnant water, and that the tap was *not running*.

I sent to have it turned on, re-conducted Mullins over the ground, when the twig immediately indicated the spot.

When Mullins had passed on, I carefully marked the exact spot indicated by the twig. When he had left the garden, I said, "Now, Mullins, may we blindfold you and let you try?" He said, "Oh yes, if you don't lead me into a pond or anything of that sort." We promised.

Several sceptical persons were present who took care the blindfolding was thoroughly done.

I then re-conducted him, blindfold, to the marked spot by a different route, leaving the tap running, with the result that the stick indicated with mathematic exactness the same spot. At first he slightly overran it a foot or so, and then felt round, as it were, and seemed to be led back into the exact centre of influence by the twig.

All present considered the trial entirely conclusive of two things.

First, of the man's perfect good faith.

Secondly, that the effect produced on the twig emanated from an agency outside of himself, and appeared due to the presence of running water.

My brother, Mr. Harold Finch Hatton, is present as I write, and confirms what I say.

It is true that one of the Misses Wordsworth tried the twig, and was surprised to find that an influence of a similar nature, though not so strong, was imparted to it.

I merely give facts, without attempting to explain them.

No doubt it will be long before they are generally admitted—probably not until scientific (?) men cease to gratify their vanity by denying the existence of everything they cannot explain or account for fully.

It appears to me to be due to some occult form of magnetism, requiring a high conductive power in the operator. Mullins says that he feels something akin to an electric shock each time, and that his arms ache all night after many experiments.

No. 31.

From STEPHENS AND BARSTOW, Bristol Steam Joinery Works.

25th March, 1883.

In the year 1879 we were building a house at Horsham, in Sussex, for Mr. J. Renton, of Guildford.

There were one or two wells on the property, but little or no water could be obtained from either (in fact water was very scarce indeed in the neighbourhood generally), and it was thought desirable to sink another well.

As we had known Mr. Lawrence for many years, and knew that he professed to be able to discover water, we communicated with and arranged with him to go to Horsham, hoping that if he should point out a place where we might sink, that the well would be a greater success than those already there.

Mr. Lawrence proceeded to the place, but we cannot, from personal observation, give you an account of what took place, as no member of the firm was able to accompany him (we may here state that neither of us have ever seen him use the "rod"), but we enclose you a letter from the man who was foreman upon the works at the time of Mr. Lawrence's visit (he is still in our employ, and we wrote to him upon receiving your letter), in which he fully describes how Mr. L. proceeded.

You will see by Mr. Martell's letter that he was in every way successful.

From S. MARTELL, Foreman.

March 26th, 1883.

He selected one, a hazel twig, and walked about with it. On coming to a certain place he said, "Here is the spring of water," and the twig was violently jumping up and down, and he marked the place which was to be the centre of the well. He then tried the steel spring with the same result. On commencing to dig the well I was obliged to move a little on one side, as one of the new drains was in the way, and the centre of the well, as marked by him, became the *side*. On sinking down about 40ft. the spring burst out as large as a hammer handle in the *very spot* that he marked for the centre.

From HENRY SHAW, Architect, 52, New Broad Street, E.C., and 39, Clapton Square, E.

March 28th, 1883.

With regard to the "Divining Rod," in 1878 I erected a house at Rudgwick for J. T. Renton, Esq. ; the site was on high ground, and the subsoil of stiff clay. During the progress of the works the contractor proceeded to

form a well close to the new building, and *had bored a very considerable depth without coming to water.* Messrs. Stephens and Bastow, of Bristol, were the contractors, and they advised that Mr. Lawrence should be employed *to detect a spring.* I had heard of him before, and was greatly interested as to his mode of procedure. He was shown where we had already bored. [Then follows a description of the process.] He said we *should find water, the position being only some few yards from the original boring.* We proceeded to sink at the place indicated by Mr. Lawrence, and within some 14 yards from the surface we came *upon a good spring of water,* and which has, I believe, continued to supply the house from that time to this. I may state that there was an old well some 100 yards from the new building, but Mr. Lawrence was not informed of this to my knowledge.

No. 34.

From Mrs. Bengough, 5, Apsley Road, Clifton, Bristol.

March 7th, 1883.

I discovered I could use the "Divining Rod" in this way. A professional water-finder in this neighbourhood called on me on some business, and happened to say he was going that afternoon to find water at my cousin's new house at Henbury. A large well had been dug to a considerable depth without any sign of water, and as the house was nearly completed the matter was very important. I asked how he was going to find water, and he took from his pocket a watch-spring, and, going on to the lawn, showed me how he used it. I then tried it, and found it worked quite as much in my hands as in his. After he left I determined to go over to my cousin's house and try if I could find the water before he came over in the afternoon, so I bought a watch-spring and drove over, and tried it (before many witnesses) close to the well. I found it was most agitated at a spot about two yards from the well, and from this spot I traced a line across the garden to a field beyond, by the agitation of the watch-spring. My friends marked the place, and then we waited for the water-finder. He came and fixed exactly on my spot.

From H. J. Crisp, Architect, Bristol.

March 24th, 1883.

Prior to sinking the well, we consulted a geologist as to the probability of finding water, and at what depth. He informed us that it was not likely that water would be found until we had sunk through the bed of the mountain limestone existing there, and which was about 150ft. deep, when we should come on a bed of clay and find an abundant supply of water. This applied to the district where this stone exists, and therefore the well could be sunk in any part with the same chances of finding water. We accordingly sunk a shaft to a depth of 150ft. through the rock, and then bored 10ft., but found no water. This may perhaps be accounted for to some extent, as by ill luck we came in contact with some faults (or saddle-backs as they are

sometimes termed) in the rock. We then called in Mr. Lawrence with his rod ; he held it over the mouth of the well and it was motionless, and Mr. Lawrence stated it was no use sinking any deeper there ; he then walked in a spiral line round the well, and when at a distance of about 20ft., the rod moved vigorously, but nowhere else near the well. With his advice we drove a level heading from the shaft at a depth of about 100ft. from the surface, towards the spot indicated by the rod, and after proceeding about 30ft. the water suddenly flowed in at the end of the heading, and the men had to leave the heading at once and get to the surface. Since this there has been a good and regular supply of water.

[These accounts are confirmed by Mr. Butterworth, the owner of the house, Mr. Lawrence and others. There is some slight discrepancy in the measurements, which, however, does not materially affect the case.]

No. 41.

From W. G. ADEY, Builder and Timber Merchant, West Mills, Newbury.
April 19th, 1883.

William Stokes has been in my employ as a carpenter and wheelwright from the year 1865, in which year I built some stables and chaise houses for the Rev. M. J. Ridley, of East Woodhay, and Stokes was on the works as a carpenter ; and, while it was in hand, Mr. Ridley wished the well that supplied the house to be opened and cleared out, but no one on the estate knew where it was, not having been opened for a number of years ; but Stokes, with his divining rod, discovered the well (although a perfect stranger to the place), and it proved to be where he predicted, under the paving in the centre of the pathway.

Altogether Stokes has been employed by me in that capacity as "water-finder" or "prophet," as he is called, in, probably, 18 or 20 different places, and I cannot say that he has failed on any one occasion, and I must confess that no one made greater ridicule of his abilities in that direction than I did, but was quite converted and made a true believer by the following circumstance.

In the year 1872, I was employed to build a mansion in this neighbourhood, and was naturally desirous to have the well as near to the scullery as possible, and directed my men to sink the well accordingly at the N.W. angle of the building ; but after they had sunk the well a few feet, Stokes went up, unknowing to me, and told my foreman that it was of no use going on with that well as we should not get water, and told them where the spring was, viz., in the N.E. corner, and that it was near the surface. My foreman asked me what he should do in the matter, and I told him not to pay any attention to such rubbish, and continue sinking the well. We did so, and at a depth of nearly 40ft. there was not the slightest appearance of coming to water. My men then threw out a hole where Stokes indicated, about 30 or 35ft. from the well, and at a depth of only 5ft, from the surface came upon a spring which kept the bricklayers and plasterers supplied all through the job, and has been used for the supply of the house to this day.

On another occasion I *deviated very slightly from the course of the spring as indicated by Stokes and had to sink another well where he directed.*

I could give you a list of several wells sunk under his direction, but I believe you have written to, and had replies from, several of my employers.

One bucket filled with spring water and another with rain water, placed side by side, and he will tell you, when blindfolded, which is the spring water and which rain water.

He is an abstainer, and a highly nervous, sensitive man, and I am now as great a believer in his powers as I was formerly a disbeliever.

He is going in the country to-morrow to advise in the sinking of a well on the hill for a new house I am commencing.

No. 47.

From E. VAUGHAN JENKINS.

October 7th, 1882.

About 30 years ago I purchased a plot of land on a hill slope two acres in extent whereon to erect a residence of considerable value. It formed part of an estate laid out for building purposes in a suburb to Newport, Monmouthshire. The absence of waterworks necessitated the holder of each plot who intended building thereon to sink a well for his water supply. Having chosen the site for my residence, the architect fixed upon the most convenient spot for the first requisite—the well.

After the well-sinkers had reached a depth of 51ft. they decided, from the nature of the strata, &c., that it would be perfectly useless to proceed further with the sinking, as the search for water in that direction would be sure to end in failure.

A consultation of all the "knowing ones" in the matter was therefore held, with the result that, owing to the peculiar dip of the land and for various other reasons, "they did not consider there was the least possible chance of water being obtained on the plot of land anywhere." In this dilemma the foreman of the masons, a native of Devon or Cornwall—I forget which—exclaimed, "Why don't you try the divining rod? In the part of the country I come from no one would think of sinking a well without the guidance of the rod." Although quite incredulous, I replied that I should only be but too glad to have it tried if he knew any one who could use it. Upon which he said his little boy, 11 years old, possessed the power in a remarkable degree, and that if water was to be obtained on the plot he would pledge his character that his boy would find it. The lad, an honest, innocent, and nice-looking little fellow, being sent for and informed what was required of him, immediately repaired to a neighbouring hedge, and returned with a rod of blackthorn or hazel—I think the former—about 2ft. 3in. in length and of the thickness of telegraph wire. Then placing the ends of the rod between the thumb and forefinger of each hand, bending it slightly and holding it before him at a short distance from the ground, he started on his expedition, I and others following him and watching every movement closely. After going up and down, crossing and re-crossing the ground several times, but never on the same lines, the lad stopped, and, to our great surprise, we saw

the rod exhibit signs of motion, the fingers and thumbs being perfectly motionless. The motion or trembling of the rod increasing it slowly began to revolve, then at an accelerated pace, fairly twisting itself to such an extent that the lad, although he tried his best to retain it, was obliged to let it go, and it fled to some distance. The experiment being thus far successful, coupled with the respectability of the parents, the lad's transparent innocence, and the father's positive assurance that the operation might be immediately commenced with the certainty of success, the next day saw the well-sinkers in full swing on the spot indicated, and on reaching the depth of 48ft. they had the gratification of striking on a strong spring of pure and beautiful water coming in so fast as to cause them to make a hurried exit, and in a few hours the well contained a depth of 10ft. of water, rising since occasionally to 15ft., and so it now continues. The father stated that when he was a boy he possessed the same power, but entirely lost it at 16 years of age. I send you this incident for what it may be worth. To myself personally its results were most important, as it changed the position of my residence and secured me an exhaustless supply of beautiful water. I was then, and I am now, fully convinced of the total absence of any deceit or collusion and of the full integrity of the whole transaction, no fee or reward being asked for or expected, and I therefore cannot avoid entertaining the opinion that there must be "something in it," that something being dependent upon some peculiar magnetic or other condition of the human agent employed, and it may yet form one of the grand discoveries of this or some future age.

No. 48.

From Topham Angus and Co., Contractors.

December 28th, 1882.

In the summer of 1874, we had instructions from the Somerset and Dorset Railway Company to sink a well at Shepton Mallet Station. We sunk to a depth of 103ft., and got no traces of water. Our chief agent sent over from Bath a man who could find water with the "Divining Rod." On arrival he went down the well, and after sundry manœuvring with the rod, he said "You will never come upon water here." He came up. After walking round the well a few feet, he came upon water which he said was boiling 40ft. below the surface. He recommended a small heading to be driven, about 40ft. from where he was standing. And he said, "I do not think the miners will have time to finish timbering before the rush of water will be upon them." Naturally the Cornwall miners, who had sunk the well, and myself thought it all humbug, but we found his statement true, for in less than 24 hours we had 63ft. of water, and we got a 10-horse power engine brought on the ground, and we could not reduce the water more than two feet in the 24 hours.

ANNUAL BUSINESS MEETING.

The annual business meeting of the members of the Society was held at 11, Chandos Street, Cavendish Square, London, on the 18th of January. The President, Professor H. Sidgwick, was in the chair.

The balance-sheet for 1883 (which has been circulated among all the Members of the Society) was placed before the meeting. It showed that although the receipts from subscriptions had greatly increased since the preceding year, they had, even with the amount received from the sale of the Proceedings, fallen short of the expenditure by over £300. The President said that this deficiency had been fully met by the donations which had been given in aid of the Society's work. Looking at the fact of the rapid growth of the Society, which had increased its numbers from 150 at the commencement of last year to over 300 at the present time, and was still steadily increasing, he thought that this discrepancy between the income proper of the Society and its expenditure need not cause the Members any concern. He thought that, at this early stage in the development of the Society, it was far more important to make the work effective than to balance subscriptions and expenditure. Accordingly he contemplated with perfect equanimity the prospect that, although there would be a further large increase in the amount received from subscriptions, the Society would again this year have partially to subsist upon "charity." Indeed, he had the pleasure of announcing three or four additions to the Research Fund since the beginning of the year.

The following alterations in the constitution and rules, which had been agreed to by the Council during the past year, were submitted to the meeting and adopted :—

That in Rule 3 the words—"Provided that no President shall hold the office for more than three years consecutively"—be omitted.

That a new Rule, to follow Rule 8, be adopted as follows :—"The Council shall have power to elect as Corresponding Members, who shall be on the same footing as Honorary Members, persons able and willing to forward the objects of the Society."

That the following clause be introduced as the second sentence in Rule 17 :—"The names of persons for the first time proposed to be co-opted on the Council shall be brought forward at one meeting of the Council, and shall be sent round to all Members of Council previous to its next meeting, when the voting shall be by ballot, and a unanimous vote of those present shall be requisite to carry the election."

That in Rule 22 the third clause stand thus :—"In all meetings of the Council four shall be a quorum"—instead of as at present"—"In all meetings of the Council five shall be a quorum."

The following gentlemen had been nominated by the Council to fill the vacancies caused by the resignation of two Members during the year, and by the retirement of four others under Rule 18 :—

Edmund Gurney	Frank Podmore	Henry A. Smith
Edward R. Pease	E. Dawson Rogers	Prof. Balfour Stewart

No notice having been given of any other nominations, these gentlemen were declared elected.

On the motion of the Hon. Percy Wyndham, M.P., Mr. Morell Theobald, F.C.A., was elected, in accordance with Rule 28, as auditor for 1884, on behalf of the Members.

PROCEEDINGS OF THE GENERAL MEETING ON

March 28, 1884.

The seventh General Meeting of the Society was held at the Garden Mansion, Queen Anne's Mansions, S.W., on March 28, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

I.

THIRD REPORT OF THE LITERARY COMMITTEE.

Committee:—W. F. BARRETT, F.R.S.E. : CHAS. C. MASSEY ; REV. W. STAINTON MOSES, M.A.; F. PODMORE, M.A.; and EDMUND GURNEY,* M.A., and F. W. H. MYERS,* M.A., *Hon. Secs.*

A THEORY OF APPARITIONS.

PART I.

This is a question which, after five thousand years, is still undecided ; a question, whether in theology or philosophy, one of the most important that can come before the human understanding.—DR. JOHNSON.

BY a curious combination of circumstances the question of which Dr. Johnson speaks—namely, what Apparitions really are—which has in a kind of fashion smouldered on since the days of the cave-men, or at least of Job and Homer, is now entering on an acute phase. It is coming at the same time to seem more *important*, and to seem more *soluble*. In this respect it resembles many other time-honoured questions, which men have been for many generations content to debate backwards and forwards without result, partly from a pre-scientific indifference to accurate tests, and partly from despair as to the possibility of applying them. The attitude of our generation towards such questions is very different. There is an inclination now to get to the bottom of subjects, or at any rate definitely to recognise them as bottomless. If any subject has actuality enough to retain any place at all in general conversation, it is felt that somebody or other may be expected to make it his business to analyse and expound it. The

* THE COUNCIL HOLDS ITSELF GENERALLY RESPONSIBLE FOR THE REPORTS OF ITS COMMITTEES. AT THE HEAD OF EACH REPORT THE NAMES OF THOSE MEMBERS OF COMMITTEE WHO ARE SPECIALLY RESPONSIBLE FOR ITS COMPOSITION ARE MARKED WITH ASTERISKS.

change fairly indicates the extent to which the modern scientific spirit has permeated the world of intelligence. Working unseen among multitudes who belong to no learned bodies and have no scientific pretensions, it helps to bear along on its imperceptible current the craft of the discoverer, which used to have to be ever wrestling with the opposing tides of ignorance and bigotry. The set of this current gradually broadens. At first it speeds the barks only of the older and more established sciences; but little by little the explorer finds himself supported into more devious channels, through which hitherto the impetus of popular curiosity had never swept. Within the last generation, for instance, anthropology, from being rejected year after year by the British Association as a vain cobweb spun from travellers' tales, has taken its place as one of the most constant and popular elements in their proceedings. And later still, the ingenious speculations of Mr. F. Galton in the delicate domain of "eugenics," and in the idiosyncrasies of mental imagery—received at first with something as near derision as the eminence of their author permitted—are now recognised as a necessary development of the method into which Darwin has cast the thought of the age.

It is natural that the tone and the claims of science should change with this gradual popularisation of the scientific instinct. She can now demand, without fear, to subject, as it were, to her police regulations the Broad Sanctuary which was once governed by tradition and sentiment alone. Everything which claims to be *known* is expected to show its credentials; and views about the seen and unseen worlds are alike conceived as amenable to objective tests. This process has been applied, as we all know, to every element in ancient creeds and institutions. It would be absurd to say that any kind of general agreement has been in this way attained. But if we had to submit two resolutions, as a kind of compromise, to be voted on by the readers of the innumerable tractates, "symposiums," &c., which have dealt with these high matters, we should select the following as prudent generalities, likely to gain more assent and provoke less strenuous opposition than any others which we could think of:—

1. The thesis that the universe is governed by unchanging laws, as opposed to arbitrary interferences, has gained in probability.

2. The crudely materialistic account of things, which refuses to allow us even to seek the key of any of the phenomena of life and mind outside the admitted scope of physiological and psychological laws, has failed to commend itself as a complete or ultimate solution of the problems without and within us.

Now if we wish to see what real guidance lies in these two somewhat vague resolutions, taken together, our practical corollary, as it would seem, must be something of this kind:—that while accepting as

perfectly valid every law which recognised science can establish, we may fairly suppose that further laws, of a different kind it may be, but perhaps none the less susceptible of rigorous investigation, are actually in operation in the domain of human life; and certainly no reason exists for contentedly ignoring any hint of such laws which experience may offer.

We select, then, a test-instance. We propose to deal with a class of phenomena at once ancient, widespread, and notorious—the standing jest and the standing mystery of age after age. *Apparitions*, of course, in one form or another, are an element in nearly every religion known among men; and the discussion of their reality has been a perpetual feature in religious controversy. But the apparitions which have been most associated with religious ideas have been those of the *dead;* and we shall do our best to avoid controversial ground, and also keep our subject within manageable limits, by altogether excluding this class. Let us take only the alleged apparitions of living persons, the commonest of which are death-wraiths, or apparitions of persons near the moment of death. How does opinion stand at present with regard to death-wraiths?

We think we may say that the subject holds a position absolutely unique. The main question, it must be remembered, is simply as to the reality of certain contemporary events. Differences of taste, of temperament, of skill in historical interpretation, of religious or philosophical proclivities, have nothing to do with it. Yet on this bare question as to whether or not a particular sort of phenomenon, alleged to be observed a good many times every year, is a reality or a figment, intelligent opinion is found to be utterly, it might almost seem hopelessly, divided. In what other department of real and pretended knowledge can we find a parallel to such facts as the following? Within the space of half a year two papers appear in two of the leading monthly Reviews. In the first, a rising physician, acting as spokesman to a party of vigorous and enthusiastic scientific workers, dismisses the phenomenon in question as a baseless absurdity. In the second, one of the ablest bishops on the bench, whose scientific aptitudes were at any rate sufficient to obtain for him the highest mathematical honours at Cambridge, represents the same phenomena as attested in a way which makes doubt of them almost impossible to a fair mind. This instance is a typical one; and without hazarding a guess as to the relative strength of the two parties, we feel assured that, if every casual assembly of educated Englishmen could be polled, each view would almost invariably claim a certain number of adherents. One may not infrequently find the very antipodes of opinion located at the two ends of a friendly dinner-table, and two groups, till then harmonious, each pursuing the theme under a fire of contemptuous

glances from the other. In a professedly scientific age, this division of belief on a point of contemporary testimony is surely an anomaly amounting almost to a scandal; and the more so that the alleged events, though not to be commanded at will, are not, like the sea-serpent, remote and inaccessible; nor, like him, are they described by any particular class of the community professionally addicted to yarns or to marvels. They occur, if they occur at all, in our very midst; and are testified to by no single class, but by individuals drawn from every class, and by representatives of every profession and pursuit.

A question of fact which is thus in suspense clearly cannot long escape the widening current that sets towards minute and exhaustive inquiry. For though there has been but little attempt at accurate treatment, it certainly cannot be said that the general interest in the subject has in any way flagged. With all their difference of view, the two parties at any rate agree in their inability to leave Apparitions alone. There is, no doubt, a growing distaste for the fictitious tales of "the supernatural" which have had in their day a considerable vogue; and it is still safe and easy to treat any thing which can possibly be called "a ghost-story" as on a par with such figments as these. But, for all that, such records as bear marks of genuineness are, as a rule, received with a much more thorough-going curiosity, and refuted (it may be) with a much more thorough-going zeal, than seems to have been the case in times generally accounted far more credulous and far more intolerant. If, therefore, little serious hope has been felt of ascertaining the truth by subjecting the alleged phenomena to a really scientific analysis, it must be owing to an indistinct idea that the necessary evidence exists in fragments too sporadic and uncertain to form the basis of any accurate inductions.

It is against this idea that our first stand is made. We deem it premature to despair of the success of an enterprise which has never once been fairly attempted. We spoke just now of the invigorating effect of the infusion of the scientific spirit into popular thought: we must now even more strongly insist on another aspect of the democratic tendency of modern science. The public are for the first time being made *participators* in scientific work: for the first time they appear as the sources of the evidence, as the actual material (so to speak) of the experiments, which the novel nature of recent inquiries demands. Here, again, we have the example of Mr. Galton to point to.* The

* Mr. Galton has felt himself at liberty to offer pecuniary rewards for information of the sort that he desires. We have held ourselves absolutely precluded from taking this course. We do not, indeed, feel that there is any fear of intentional fictions being palmed off on us; as our system of cross-examination (which our informants may sometimes, we fear, have thought pedantic) has by this time set us quite at rest on that point. But it is plainly necessary that our witnesses should not be induced, by any means that can be avoided, to cast their evidence in a striking or attractive form.

fact is, as he has pointed out, that the very spread of the scientific spirit has immensely increased the number of men and women who are able to give straightforward and accurate answers to questions bearing on their personal experience. And as science gradually extends her empire over the subtler problems of life, the importance of this new departure may be expected to be more and more felt. Many a problem which a Reid or a Hamilton settled down to in a mood of conscientious self-examination, and in the philosophic cave of his study, may hereafter be solved in the market-place, by the Method of Averages and by tables of statistics.

But to consider more precisely the present state of the argument as regards the genuineness of Apparitions.

For them there is a considerable mass of evidence (which, as we say, has never been carefully collected or measured), and a good deal of popular belief—mainly held, however, by people who believe also that they are more or less *miraculous*, more or less special interferences with the ordinary laws of events.

Against them there is a large mass both of scientific and of unscientific opinion. The unscientific opposition need not detain us long. It embodies itself in such assertions as that no one ever heard a ghost-story first-hand; or that seers of apparitions almost always end by believing in the Claimant, the Israelitish origin of the English, and the flatness of the earth; or that no one who was not of a nervous or hysterical temperament was ever troubled with such experiences; and it rests probably on tacit reasonings of this kind, "*I* am a strong-minded and superior person, and *I* never saw anything of the kind;" or on a comfortable and diffused sense of the progress of the age, and a piquant recollection of Sir Matthew Hale's belief in witches. Supplementary arguments can readily be improvised to suit the occasion. If a believer in apparitions is a Catholic, it is said that of course he is one who will swallow anything: if he is a Freethinker, it is said that he is setting up grotesque images to replace his lost gods. These people give us no anxiety. Their assertions are mere phrases; and one phrase is as good as another. We entertain no doubt whatever that those who are now asserting that the world has become too instructed for this sort of thing, and that nobody ever heard a ghost-story first-hand, will—after the evidence has been brought under their notice—be equally loud in asserting that of course they always knew that any amount of people could be found to vouch for personal experiences of this sort, and that they are only too painfully aware what a thin crust of science overlies the chaos of primitive superstitions.

But the *scientific* opposition has, of course, a more logical basis. It supports itself on two arguments; one of which is an *explanation* of some at least of the alleged phenomena; the other *presumption* against

the antecedent possibility of the phenomena, except so far as that explanation will cover them.

(1) The *explanation* is that apparitions are merely cases of morbid hallucination; and that the time-coincidences involved (as when the figure of a friend is seen at the precise moment of his death) are due to chance alone.

(2) The *presumption* is that however hard it may be to explain away the coincidence as due to chance, nevertheless that is the only line to take; and we must not suppose that the apparition has really anything to do with the dying person, since this would involve an interference with the order of Nature.

Now the *explanation* here suggested really resolves itself into a pure question of facts and figures. No one doubts that there are numerous hallucinations which do not correspond with any objective fact whatever outside the organism of the percipient—which merely reflect and externalise some morbid condition of his brain. And few would care to deny that *some* such hallucinations or apparitions may have been observed in close coincidence with deaths, &c., and may thus have given rise to the belief in wraiths. It is obvious that until the evidence as to wraiths has been collected, sifted, tested in every way with thorough care, and until also some conclusion is arrived at as to the *frequency* of mere morbid hallucinations, it is impossible to say on grounds of fact whether the coincidences of death and wraith are due to chance or not. If it be at once asserted that *of course* all death-wraiths are mere chance-coincidences, this positive and wholesale assertion must be based on the *presumption* which we shall discuss presently, not on the ground of recorded *fact* which we are discussing now.

Here, then, we have come clearly in view of the *two* lines along which facts must be collected for the purpose of our introduction. We must collect information not only of death-wraiths, but also of morbid hallucinations—of purely subjective visions—having no claim to be anything else. Up to this time no one has adopted the method of systematic collection of facts along both these lines of inquiry, which alone can be expected to bring the controversy to a final close. It is not too much to affirm that the student of the subject may read every word that has ever been published on both sides of the argument, without encountering a sign—we do not say of the necessary statistics, but of even an idea that statistics can be wanted. Confident assertion is a far handier weapon. Thus, to quote a typical instance from a leading daily paper, "The number of well-attested coincidences" between the apparition of a person and his death "does not exceed, as Mr. Lang has suggested, the limits that the laws of chance allow." The idea that *a point of this kind* can be settled off-hand by anybody's suggestion—an

idea which Mr. Lang himself would be the first to repudiate—clearly belongs to the pre-scientific era. It is much as if some one should take a stroll through Hyde Park, observe the children he met, and then "suggest" the proportion of males to females in the population of England. Again, on the other side of the controversy, it is no doubt a more grateful task to relate or to print an interesting narrative than to ascertain, for instance, from twenty of one's acquaintance the dull fact that they have never experienced a distinct visual hallucination. Just in the same way a scientific lecturer will win more regard at the moment by a sensational experiment with pretty colours and loud explosions than by laborious quantitative work in his laboratory. But we must persistently impress on the friends of "psychical research" that the laborious quantitative work has to be done; and it is some satisfaction to think that the facts themselves may stand as material for others to deal with, even if the conclusions that we ourselves draw from them are incorrect.

But while we much prefer facts to the most intelligent "suggestions," we must point out that we have no wish wholly to discard the common-sense view of intelligent men, merely because it is not based on careful numerical estimates. On the contrary, we ourselves are in a position to appeal to that view with confidence, in respect of one, at any rate, of the two lines of inquiry which we have indicated. The question whether or not a very large proportion of the population have had experience of morbid or purely subjective hallucinations is one, we submit, where we might fairly have taken our stand on the ordinary observation of educated persons, and have thrown on our opponents the *onus* of proving it wrong. On this point a broad common-sense view does exist; and according to it, "spectral illusions"—distinct hallucinations of the sense of vision—are very far from the everyday occurrences which they would have to be if we are to suppose that, whenever they coincide in time with the death of the person seen, they do so by accident. Nay, if we take even one of our critics, and bring him fairly face to face with the question, "If *you* all at once saw in your room a brother whom you had believed to be a hundred miles away; if he disappeared without the door opening; and if an hour later you received a telegram announcing his sudden death—how should you explain the occurrence?" he does not as a rule reply, "*His* day and hour for dying happened also to be *my* day and hour for a spectral illusion, which is natural enough, considering how common the latter experience is;" he usually says, "The supposition is absurd; there are no really authentic cases of that sort." Under the immediate pressure of the supposed facts, he instinctively feels that the argument of chance-coincidence would not seem effective.

But to return to our census—it would perhaps have been too much to expect our opponents to aid *us* in a task which it was rather for

them to have undertaken. But we were scarcely prepared for the reception which our proposal for a numerical estimate encountered. It was introduced in a circular letter, every word of which might have been penned by a zealous sceptic, anxious above all things to prove that, in cases where a distinct apparition or vision of a distant person has been simultaneous with the person's death, the coincidence has been an accidental one. If apparitions are extremely common things, then—it was pointed out—it might naturally happen, according to the law of chances, that one of them here and there would fall on the same day or night as the real event which it suggested. Not a syllable was used implying that the authors of the letter had themselves any opinion as to whether apparitions to which no real event corresponds *are* or are *not* common things; it was simply argued that it is necessary to have some idea *how* common they are, before deciding whether apparitions to which real events *do* correspond are or are not to be fairly accounted for by chance. And since sensory hallucinations, whatever their frequency, are at any rate phenomena as universally admitted as measles or colour-blindness, it did not occur to us that the following question could possibly be misunderstood:—Have you ever, when in good health and completely awake, had a vivid impression of seeing or being touched by a human being, or of hearing a voice or sound which suggested a human presence, when no one was there? Yes or no?

Clearly, the more *yeses* are received to this question— *i.e.* the *commoner* the purely subjective hallucinations prove to be—the stronger is the argument for *chance* as an adequate explanation of the instances of coincidence; the more *noes*—the *rarer* the purely subjective hallucinations prove to be—the stronger the argument that the death which *coincides* with the apparition is in some way the *cause* of the apparition. We should have expected, if any injustice was to be done us, that it would have taken the form of attributing to us an inordinate desire for *noes*. To our amazement we found that we were supposed to be aiming exclusively at *yeses*—and not only at *yeses*, but at *yeses* expanded into orthodox "ghost-stories"—to be anxious in fact that every one in and out of Bedlam who had ever imagined something that was not there, or mistaken one object for another, should tell us his experience, with a view that we might immediately interpret it as due to the intervention of a bogey. A more singular instance of the power of *expectancy*, of the power of gathering from words any meaning that the critic comes predisposed to find there, can hardly be conceived. A statistical question on a perfectly well recognised point in the natural history of the senses was treated, in scientific and unscientific quarters alike, as a manifesto of faith in "supernatural" agencies; and we found ourselves solemnly rebuked for ignoring the morbid and subjective nature of many hallucinations—that is to say, for ignoring the fact

which we had set forth as the very basis of our appeal, and from which its whole and sole point was derived.

So much for the statistical aspect of the controversy.* We have next to consider the *presumption* against Apparitions as being interferences with Nature, exceptions to ordinary law-abiding phenomena, and so *a priori* impossible. Now let us try to fix the meaning of this expression. Arago's dictum pronounces "impossible" to be a meaningless word, outside the region of pure mathematics; but for present purposes we are content to take it in a much looser sense, and to count as impossible anything which can fairly be called too improbable to be worth inquiring into. Let us try a definition then, and say "An alleged phenomenon is impossible if it runs counter to a sufficient previous mass of experience; or if (without definitely contradicting any established generalisations) it postulates a force whose existence, did it exist, must long ago have been inevitably observed." This perhaps is as fair a working definition as can be got; it is at any rate one that all may agree to. But definitions are inanimate finger-posts, not living guides. And as soon as a really difficult case meets us, it is seen that the whole question has still been left unavoidably open in the words *sufficient* and *inevitably*, whose meaning it must be left in each case to common sense and the dicta of experts to determine. And even if we suppose a decision come to, it will be difficult to regard it as final, so long as common sense insists,—and it certainly *will* insist—that all experience shall be counted for what it is worth. How long will our definition bind us to go on dismissing, as "running counter to experience," what is widely and strongly testified to as experienced? Facts which have ended by settling down quite comfortably with other facts, have before now had the door of science slammed in their face; and phenomena which are now the very starting-points of research were once distorted or ignored as intractable and intrusive. Plenty of good solid substances have in their day been jeered at as bitterly as if they were the airiest of spectres. Pre-Adamite fossils, which flew in the face of established chronology, were interpreted as marks elaborately imprinted in the rocks by some uncertain Power, with obscure reference to man's ultimate belief or disbelief in the book of Genesis. Aerolites were scouted as a kind of fetish *in excelsis*—a transcendental bætyl—"the image which fell down from Jupiter." "There are no stones in the sky," said Lavoisier, "therefore none can fall upon the earth." Again, forces now used in a wholesale

* **The collection of the statistics themselves must be the work of many months; and we earnestly appeal for help in it. To obtain an adequate number of answers, a very large number of collectors is needed. Any person willing to aid in the work should communicate with the Hon. Sec. of the S.P.R., 14, Dean's Yard, S.W.**

commercial manner, and which existed as widely in the days of Archimedes and in the days of Bacon as in our own, remained almost latent, because the right conditions were not applied to draw them forth or measure them. When we consider the place which electricity now holds in the world, and yet how trivial were the forms under which men for ages had been wont to recognise it, and how little they thought of lightning in connection with it, we must surely be cautious of our assertions as to *when* men *ought* to have observed a force, and what particular phenomena they *ought* to have referred to it.

This last illustration is specially apposite. For, as it happens, our present theory comprises elements which are connected in somewhat the same way as the electrical phenomena of the firmament and of the cat's coat. Under our term *Telepathy* * we connect very small experimental, and very striking spontaneous phenomena. Apparitions may stand for the lightning; while the ancient observations on the attraction of amber for straw may be paralleled by the modest experiments in Thought-transference, to which the Society for Psychical Research has been for some time inviting attention. Apparitions, on the one hand, have been observed in every age, but observed with mere terror and bewilderment. And on the other hand, candid friends have expressed surprise at our taking a serious interest in getting a rude diagram from one person's mind into another, or proving that ginger may be hot in the mouth by the effect of unconscious sympathy alone. Yet we hope to be able to show that these trivial cases of community of sensation are the germinal indications of a far-reaching force, whose higher manifestations may outshine these as the lightning outshines the sparks on Puss's back. We hope to show that the lowest telepathic manifestations may be used to explain and corroborate the highest. The experimental work from which, mainly at any rate, the lower manifestations have been drawn, is as yet indeed only in its infancy. But the infancy is a vigorous one; and the results which, when the subject was broached in our Proceedings two years ago, rested on the assumption of the sanity and probity of a few observers, have now been varied and repeated so as to divide the responsibility for their genuineness over a group of persons too large, at any rate, to be summarily discredited.

In thus insisting on Telepathy as a whole—as a conception of far-reaching application—we are but following out a hypothesis which to some of the highest intellects of the past appeared not only a rational,

* We began by restricting this word to cases where the distance through which transference of impressions took place far exceeded the scope of the recognised senses. But there is great convenience in extending the term to *all* cases of *impressions conveyed* without any affection of the percipient's recognised senses, *whatever may be* his actual distance from the agent.

but a probable one. The idea is no mere popular instinct. It comes across our path in works of established fame, from Bacon's stately proposals for "experiments in consort touching the emission of immaterial Virtues from the Minds and Spirits of man, either by Affections, or by Imaginations, or by other impressions," to Goethe's genial view that this power in the case of lovers "is particularly strong, and acts even at a distance." Nor in referring to such names as Bacon, Cuvier, Goethe, in support of a broad theory of supersensuous influence, are we in any way bolstering up by antiquated authority a position which modern discovery has undermined. We most strenuously insist that whatever of rationality and probability that position *ever* possessed it possesses *still*. There is nothing in the advance of science since the days of Goethe, or even of Bacon, which could possibly make telepathy seem impossible to them *now*, if it seemed possible to them *then*. Physiology and psychology have, of course, advanced; the limits of the known senses have been more exactly tested; the invariable co-existence of a physical with every psychical event has been more irresistibly suggested. But the question of sympathy at a distance is left all the time exactly where it was before. Analogy might lead us to suppose that such sympathy, if it exists, has its physical basis, like so many other phenomena of action between distant points. But such sympathy if it exists, being *ex hypothesi* independent of the known organs of sense, is also independent of our enlarged scientific knowledge of them. We have here, in fact, a specially good illustration of the difference between the *negative dogmatism* and the *positive discoveries* of modern science, and of the dangers involved in treating the two with equal respect. It is not the assertion of the possibility but the assertion of the *im*possibility, that the necessary evidence to a widely surmised fact may one day be forthcoming, which is hasty and hazardous; and such attempts at finality lack wisdom in proportion as the prospect excluded is vast, and the instinct which prompts its exclusion a mere moment (so far) in the history of thought. *If* the instinct of the vulgar and the intuition of sages should turn out to have been right, and the fact of communication apart from the recognised sensory channels should once be established, then the scope of the laws (possibly even of the physical laws) which concern our deepest being would turn out to have been quite arbitrarily limited; and the half-century which has surpassed all others in the number of scientific paths which it has opened, would be the very one which had shut out a legitimate curiosity from the most inviting path of all. For in this direction may lie our clearest proofs that we are not mere isolated drops in an aimless storm of atoms, but interacting centres of force, and "every one members one of another." And so far from the scientific study of man being a region whose boundaries are pretty well mapped out, and which only requires to be

filled in with further detail by physiologists and psychologists, we should then perceive that we are standing only on the threshold of a vast *terra incognita*, which must be humbly explored before we can even guess at its true extent, or appreciate its relation to the more familiar realms of knowledge.

We present the ordinary forms of Thought-transference, then, as a first step in this exploring process. And the experimental work, which we thus place in the forefront of our argument, at any rate supplies a new and direct answer to the objection that the implied force—if it exists—*ought* to have manifested itself sooner. For in fact it *did* manifest itself as soon as it was really looked for. Before exact experiments were made, the faculty which we now call Thought-transference was vaguely observed and obscurely asserted; as soon as exact experiments were made, it was definitely observed and explicitly asserted. The mode of experiment may reassure those who look on the faculty as something dangerous or uncanny; we are still quite as far as ever from any likelihood of reading each other's secrets. The results have to be tried for either by a very distinct and irksome process of concentration on the part of the person whose "thought" is to be "read;" or through the establishment by particular means of some sort of hypnotic condition in the "reader." It was when the discovery was made that the somnambulic state could be artificially induced, that experimental community of sensation may be said to have first presented itself. We could fill pages with recorded experiments of physicians and others in the early days of mesmerism in France, where almost precisely the same phenomena as ours were obtained with "subjects" in the so-called "magnetic state." And wherever this induced somnambulism has been largely practised, such phenomena have continued to appear. The novelty of recent years—that with which our Society is specially identified—is the systematic repetition of the same experiments with non-hypnotised subjects. And the experiments, when thus tried, have presented the same results which mesmerists had already obtained in a somewhat different form. No previous experiments were contradicted by these results, since no experiments had ever been performed from which the non-existence of the telepathic force could be concluded: all that happened was that what had always been *suspected* to exist, as soon as exact trial was made was *found* to exist.

We proceed now to illustrate the way in which all sorts and grades of "telepathy" do in fact hang together. And with this view we may conveniently defer our actual evidence for *Apparitions* until we have given some simpler and less startling instances—instances which may form a transition between one end of the scale and the other—of the *distant* and *spontaneous* action of the self-same force which

has been evoked within the four walls of a room by deliberate experiment.

Now experiment has given indications of communication of very various kinds, which follow in a somewhat indeterminate way the well-known fourfold division of mental faculties into the emotions, the will, the senses, and the intellect. And we shall find that the spontaneous cases may be similarly grouped. We do not mean to assert that this division corresponds with the various ways in which telepathic impressions really reach the percipient. The telepathic impact, as we are disposed to conjecture, falls usually upon the *sub-conscious* region of the mind. It emerges into consciousness by whatever channel happens in each case to be the easiest; but we cannot say what it is which determines whether the impression shall be felt emotionally as a diffused gladness or gloom; or fall on the motor nerves as an impulse to some special act; or be interpreted by the sensory centres as a visible figure, or a sound, touch, taste, or smell; or, finally, shall be conceived as a phrase or idea. But adopting the fourfold division for convenience' sake and beginning with the *emotional* class, as the least clearly defined, we may observe that the very vagueness of the emotions renders them a field ill adapted either for precise experiments or precise records. Strong emotion cannot be summoned up at will by an experimenter even in his own mind; while, if it exists, it probably betrays itself in ways beyond his control. And, even when these purely emotional impressions occur on a large scale, and spontaneously, they are still so far defective for purposes of evidence inasmuch as they depend on the percipient's account of his own necessarily indefinite sensations. In the domain of experiment we can, however, refer to the occasional cases where a secret grief or anxiety on a mesmeriser's part is reflected in the demeanour of his "subject." And to illustrate the spontaneous effect, we will select a few accounts from witnesses not likely to be accused of sentimental exaggeration.

We begin with two closely allied narratives from gentlemen of acknowledged scientific position. And we may remark in passing that men of science—who are not, of course, a large class—contribute, we think, quite their proportional quota to our collection of evidence throughout. The following case was sent to Professor Sidgwick by the Rev. J. M. Wilson, head-master of Clifton College, a senior wrangler and well-known mathematician*:—

* Most of the informants quoted in this and the subsequent Report are privately known to us. But since it happens that many of them bear well-known names, we have thought it better to omit in all cases the statement of our acquaintanceship, rather than to insert it in cases where our personal attestation of confidence would have looked highly superfluous. The narratives here given are, of course, mere samples from a very large collection, which we hope soon to lay before the public in its entirety.

CLIFTON COLLEGE,
January 5th, 1884.

The facts were these, as clearly as I can remember.

I was at Cambridge at the end of my second term, in full health, boating, football-playing and the like, and by no means subject to hallucinations or morbid fancies. One evening I felt extremely ill, trembling, with no apparent cause whatever; nor did it seem to me at the time to be a physical illness, a chill of any kind. I was frightened. I was totally unable to overcome it. I remember a sort of struggle with myself, resolving that I *would* go on with my mathematics, but it was in vain: I became convinced that I was dying.

I went down to the rooms of a friend, who was on the same staircase, and I remember that he exclaimed at me before I spoke. He put away his books, pulled out a whisky bottle and a backgammon board, but I could not face it. We sat over the fire for a bit, and then he fetched some one else to have a look at me. I was in a strange discomfort, but with no symptoms I can recall, except mental discomfort, and the conviction that I should die that night.

Towards 11, after some three hours of this, I got better, and went upstairs and got to bed, and after a time to sleep, and next morning was quite well.

In the afternoon came a letter to say that my twin brother* had died the evening before in Lincolnshire. I am quite clear of the fact that I never once thought of him, nor was his presence with me even dimly imagined. He had been long ill of consumption; but I had not heard of him for some days, and there was nothing to make me think that his death was near. It took me altogether by surprise.

JAMES M. WILSON.

Our next case is also from a scientific witness, who can hardly have been tempted to exaggerate, since the experience which he thus records greatly impairs the force of the main thesis of his book, which is directed *against* the transmission of obscure influences (mesmeric and the like) from one person to another.

[*Translation of pp.* 71-73 *of* "*Der sogenannte Lebens-Magnetismus oder Hypnotismus*" *by Dr. E. L. Fischer, of Würzburg* (1883).]

When I was a student at the University, I experienced, on waking one morning, a quite extraordinary feeling of sadness. I was not in the slightest degree unwell, and was aware of no reason for distress, and my state of depression consequently made a great impression on me—the more so that I normally enjoy the best spirits. I asked myself what could be the meaning of it, and whether some serious illness must not be impending. I made every effort to banish this deep melancholy, and especially to assume a gay demeanour in the presence of my friends; but all my efforts were unavailing.

* One or two of the cases quoted by Mr. Galton, of consentaneous action or thought on the part of twins, seem to us to be probably referable to telepathic impressions.

Before lecture two of them asked me what was the matter; they said I must have something heavy on my heart. During the whole forenoon I remained in this state of dismal wretchedness. All at once a telegram arrived from home, informing me that my grandmother was taken very ill, and that she was earnestly longing for me. There I had the solution of the riddle. Nevertheless from that hour my melancholy gradually decreased, and in spite of the telegram it completely disappeared in the course of the afternoon. In the evening I received a second message, to the effect that the danger was over. In this way the second phenomenon, the rapid decrease of my wretchedness—a circumstance which in itself was surprising, inasmuch as the melancholy should naturally rather have *increased* after the receipt of the first news—received its explanation. For the afternoon was just the time when the change in the patient's condition for the better took place; and the danger to her life once over, her yearning for my presence had decreased; while simultaneously my own anxiety was dispelled.

We have space for but one more instance, which is, at any rate, sufficiently terse and business-like:—

20, RANKEILLOR STREET, EDINBURGH.
December 27th, 1883.

In January, 1871, I was living in the West Indies. On the 7th of that month I got up with a strange feeling that there was something happening at my old home in Scotland. At 7 a.m. I mentioned to my sister-in-law my strange dread, and said that even at that hour what I dreaded was taking place.

By the next mail I got word that at 11 a.m. on the 7th of January my sister died. The island I lived in was St. Kitts, and the death took place in Edinburgh. Please note the hours and allow for difference in time, and you will notice at least a remarkable coincidence. I may add I never knew of her illness.

A. C——N.

In answer to inquiries Mr. C——n adds:—

I never at any other times had a feeling in any way resembling the particular time I wrote about. At the time I wrote about I was in perfect health, and in every way in comfortable circumstances.

If further proof be needed that we have not to go to weak or hysterical sources for evidence of these vaguer and more emotional sorts of telepathic impression, we may add that our collection includes under this head accounts from two informants who, in very different ways, have obtained the highest reputation as acute and accurate observers—Mr. Henry James and Mr. J. N. Maskelyne.

We come next to that branch of telepathy which manifests itself as an influence exercised on the percipient's *will*; or on the motor centres where will is transformed into action. And here the connection between the *experimental* cases (both in the normal and in the

hypnotic states) and the *spontaneous* cases becomes at once very marked.

Having space for only a single experimental case, we choose one which is of interest as showing a "subject" midway between the normal and the mesmerised condition. It is interesting, too, as the first publication, on first-hand authority, of an after-dinner incident which made much sensation in Yorkshire society when it occurred, and which even twenty years afterwards was still alluded to with bated breath as a manifest proof of the alliance of mesmerists with the devil. The modern inquirer will rather regret that this diabolical assistance was so frequently perverted to mere works of charity and mercy; for Mr. H. S. Thompson (formerly of Fairfield, now of Moorlands, near York) has devoted his almost unique mesmeric power mainly to the cure and comfort of his tenantry and poorer neighbours, and has only incidentally made, and rarely recorded, those experiments on "the silent power of the will" which few men, we fear, in a generation are able to repeat.

MOORLANDS, YORK.

November, 1883.

DEAR SIR,—I will give you a sketch of some of the experiments I have tried, and which lead me to the conclusion that the will is sufficient to influence some people either far or near.

In 1837, I first became acquainted with mesmerism through Baron Dupotet. The first experiment I tried was upon a Mrs. Thornton, who was staying with some friends of mine, Mr. and Mrs. Charles Harland, of Sutton. She told me that no one had ever succeeded in mesmerising her, though she soon submitted to being mesmerised by me. She went to sleep at once, and was very strongly influenced by my will. One night when I was dining with Mr. Harland, after the ladies had left the room, some gentleman proposed that I should will her to come back again, which I did. She came directly, and after this I could not go to the house without her going to sleep, even if she did not know that I was there.

I have met with many cases of thought-reading, but none so distinct as in a little girl named Crowther. She had had brain fever which had caused a protrusion of the eyes. Of this ill effect I soon relieved her, and found that she was naturally a thought-reader. I practised on her a good deal, and at length there was no need for me to utter what I wished to say, as she always knew my thoughts. I was showing some experiments to a Dr. Simpson, and he asked me to will her to go and pick a piece of white heather out of a large vase full of flowers there was in the room, and bring it to me. She did this as quickly as if I had spoken to her. All these experiments were performed when the girl was awake, and not in a mesmeric sleep.

Believe me, dear Sir, yours truly,

HENRY STAFFORD THOMPSON.

The follo[illegible]r from the last in that the desire became

operative at a distance without any expectation of such a result on the part of the person who exercised it.

[*Extract from a Letter to Professor Sidgwick.*]

CATHEDRAL YARD, WINCHESTER.
January 31st, 1884.

SIR,—As a constant reader of the *Times*, I have noticed the "Proceedings" of the Psychical Society, and as your Society has invited communications, I respectfully beg to offer you a short statement of my experience on a subject which I do not understand. Let me premise that I am not a scholar, as I left school when 12 years of age in 1827, and I therefore hope you will forgive all sins against composition and grammar. I am a working foreman of masons at Winchester Cathedral, and have been for the last nine years a resident in this city. I am a native of Edinburgh.

It is now more than 30 years ago that I was living in London, very near where the Great Western Railway now stands, but which was not then built. I was working in the Regent's Park for Messrs. Mowlem, Burt, and Freeman, who at that time had the Government contract for three years for the masons' work of the capital, and who yet carry on a mighty business at Millbank, Westminster. I think it was Gloucester Gate, if I mistake not. At all events it was that gate of Regent's Park to the eastward of the Zoological Gardens, at the north-east corner of the Park. The distance from my home was too great for me to get home to meals, so I carried my food with me, and therefore had no call to leave the work all day. On a certain day, however, I suddenly felt an intense desire to go home, but as I had no business there I tried to suppress it,—but it was not possible to do so. Every minute the desire to go home increased. It was 10 in the morning, and I could not think of anything to call me away from the work at such a time. I got fidgety and uneasy, and felt as if I must go, even at the risk of being ridiculed by my wife, as I could give no reason why I should leave my work and lose 6d. an hour for nonsense. However, I could not stay, and I set off for home under an impulse which I could not resist.

When I reached my own door and knocked, the door was opened by my wife's sister, a married woman, who lived a few streets off. She looked surprised and said, "Why, Skirving, how did you know?" "Know what?" I said. "Why, about Mary Ann." I said, "I don't know anything about Mary Ann" (my wife). "Then what brought you home at present?" I said, "I can hardly tell you. I seemed to want to come home. But what is wrong?" I asked. She told me that my wife had been run over by a cab, and been most seriously injured about an hour ago, and she had called for me ever since, but was now in fits, and had several in succession. I went upstairs, and though very ill she recognised me, and stretched forth her arms and took me round the neck and pulled my head down into her bosom. The fits passed away directly, and my presence seemed to tranquillise her, so that she got into sleep, and did well. Her sister told me that she had uttered the most piteous cries for me to come to her, although there was not the least likelihood of my coming. This short narrative has only one merit; it is strictly true.

ALEXANDER SKIRVING.

Dr. Fischer, whom we quoted, describes how he was himself once driven forth from the midst of a jubilee-dinner, by the urgent desire (as it turned out) of a person whose need of his attendance was at the time quite unknown to him; and we have reason to believe that the experience is by no means unique in medical practice. We received the following very similar case from Mrs. C——, 11, Upper Hamilton Terrace, N.W. :—

December 17th, 1883.

On the 2nd of December, 1877, I was at church. My children wished to remain to a christening. I said, "I cannot, somebody seems calling me; something is the matter." I returned home to find nothing; but next morning two telegrams summoned me to the deathbed of my husband, from whom I had had a cheerful letter on the Saturday, and who left me in excellent spirits on the Thursday before. All Sunday he was dying, and my friends could not telegraph, and there was no train. I only arrived in time to see him die. As soon as I read your letter, my sons both said they remembered the circumstance quite well, and signed the enclosed. George was 10 years old, John 12 years.

ELLEN C——.

We remember, perfectly, our mother leaving the church, saying she felt she was wanted, some one was calling her. The next day our father died, the 3rd of December, 1877.

GEORGE C——.
JOHN A. C——.

Here we have instances of an impression powerful enough to produce a distinct and unusual course of action—for Mrs. C—— assures us that under ordinary circumstances she would certainly have remained where she was—yet so obscurely seated in the mind that its own source remains unrealised and unknown. Somewhat similar is the process of rising by candlelight in pursuance of an overnight resolution to catch an early train, while, though sufficiently awake to dress quickly and carefully, one does not yet realise *why* one is in such a hurry to be up and out. A still closer parallel is offered by a hypnotised "subject," who has been made in the sleep-waking state to promise to go on some absurd errand when he wakes. He fulfils his mission in just this mood of blank obedience to an inward impulse whose origin he cannot trace, and which affects him, we may suppose, like the migratory instinct that carries the lemming into the deep sea. We none of us grasp our whole identity; the bark of our conscious being may float on currents which neutralise each other in unguessed confusion, till one of them bears us for a moment with it, and shows how much stronger than our rowing is the set of that hidden sea.

Our next class, a wide and important one, includes transferences of actual *sensation—sensory* telepathy as distinguished from transference of emotion, motor impulses, or abstract ideas. And here we naturally find the line between *sensation* and *ideation* very hard to draw. This is not the place to discuss the derivation of ideas from sensations; but among our published experiments may be seen some curious instances of a sensation transferred, but modified in the transference by some erroneous *belief* on the part either of agent or percipient as to the precise nature of the sensation felt or transferred. Perhaps the transmission of *localised pains* is as purely sensory an instance of telepathy as can be well selected. We have occasionally obtained this phenomenon in the normal state; but it is in the hypnotic state that, though still rare, it is most markedly induced. Take a mesmerised "subject" who is sufficiently *en rapport* with his mesmeriser; talk to him on some question which engrosses his attention; and, in the middle of your talk, suddenly pinch (for instance) the mesmeriser's right ear behind the subject's back. The sleep-waker will continue to listen and reply, but his hand will fly to his own right ear, which he will rub with manifest discomfort. Now here is a transferred impression which is as purely *sensory* as we can well obtain, which prompts to an action nearly or quite reflex, and is scarcely present in any conscious manner to the sleep-waker's beclouded intellect.

Now, according to our theory of a close parallelism between the *induced telepathy* of our experiments and the *spontaneous telepathy* which nature offers on a much larger scale to our examination, we might fairly expect to find some cases where a localised pain has been transferred from one person to another at a distance, unaccompanied by any definite *idea* of the cause or source of the pain thus felt. To give force to an account of this kind, it is plainly important that the pain should be sudden, distinctly localised, and not easily referable to some mere ordinary cause. If Brown were to tell us that he got into such *rapport* with Smith at a friendly Greenwich dinner that, when Smith's head ached the next morning, Brown's ached also out of sympathy, we should hand over both headaches alike to a branch of science better established than our own. But when Louis Blanc feels a shock through one of his arms, as if it had been pierced through with a rapier, at the moment that Charles Blanc's arm is pierced in a duel, * we feel that any ordinary sort of common cause for the two events is excluded. The incident which we shall now quote (occurring to Mr. Arthur Severn, the distinguished landscape-painter, and his wife, and the account of which has been obtained for us through the kindness of Professor Ruskin) presents the requisite

* See "Memoirs of C. M. Young" (1871), pp. 341, 342.

characteristics of suddenness, localisation, and unusualness of the pain in a very high degree.

BRANTWOOD, CONISTON,
October 27th, 1883.

I woke up with a start, feeling I had had a hard blow on my mouth, and a distinct sense that I had been cut, and was bleeding under my upper lip, and seized my pocket handkerchief, and held it (in a little pushed lump) to the part, as I sat up in bed, and after a few seconds, when I removed it, I was astonished not to see any blood, and only then realised it was impossible anything could have struck me there, as I lay fast asleep in bed, and so I thought it was only a dream !—but I looked at my watch, and saw it was 7, and finding Arthur (my husband) was not in the room, I concluded (rightly) that he must have gone out on the lake for an early sail, as it was so fine.

I then fell asleep. At breakfast (half-past 9), Arthur came in rather late, and I noticed he rather purposely sat farther away from me than usual, and every now and then put his pocket handkerchief furtively up to his lip, in the very way I had done. I said, "Arthur, why are you doing that?" and added a little anxiously, "I know you've hurt yourself; but I'll tell you why afterwards." He said, "Well when I was sailing, a sudden squall came, throwing the tiller suddenly round, and it struck me a bad blow in the mouth, under the upper lip, and it has been bleeding a good deal and won't stop." I then said, "Have you any idea what o'clock it was when it happened?" and he answered, "It must have been about 7."

I then told what had happened to *me*, much to *his* surprise, and all who were with us at breakfast.

It happened here about three years ago at Brantwood.

JOAN R. SEVERN.

BRANTWOOD, CONISTON,
November 15th, 1883.

Early one summer morning, I got up, intending to go and sail on the lake. Whether my wife heard me going out of the room I don't know; she probably did, and in a half-dreamy state knew where I was going.

I was left becalmed for half-an-hour or so, when, on looking up to the head of the lake, I saw a dark blue line on the water. At first I couldn't make it out, but soon saw that it must be small waves caused by a strong wind coming. I got my boat as ready as I could, in the short time, to receive this gust, but somehow or other she was taken aback, and seemed to spin round when the wind struck her, and in getting out of the way of the boom I got my head in the way of the tiller, which also swung round and gave me a nasty blow in the mouth, cutting my lip rather badly, and having become loose in the rudder it came out and went over-board. With my mouth bleeding, the mainsheet more or less round my neck, and the tiller gone, and the boat in confusion, I could not help smiling to think how suddenly I had been *humbled* almost to a wreck, just when I thought I was going to be so clever! However, I soon managed to get my tiller, and, with plenty of wind, tacked back to Brantwood, and, making my boat snug in the harbour, walked up to

the house, anxious of course to hide as much as possible what had happened to my mouth, and, getting another handkerchief, walked into the breakfast-room, and managed to say something about having been out early. In an instant my wife said, "You don't mean to say you have hurt your mouth?" or words to that effect. I then explained what had happened, and was surprised to see some extra interest on her face, and still more surprised when she told me she had started out of her sleep thinking she had received a blow in the mouth; and that it was a few minutes past 7 o'clock, and wondered if my accident had happened at the same time; but as I had no watch with me I couldn't tell, though, on comparing notes, it certainly looked as if it had been about the same time.

ARTHUR SEVERN.

It is fortunate that in this case the incident was bizarre enough to stamp itself at once on the memory. For one main difficulty in collecting cases of this sort is that, even if they do occur, they are not likely to be observed or remembered. Their theoretical importance is (very naturally) not discerned; they are thought trivial and purposeless —merely incredible, without either pathos or dignity. In reality, no narratives are more significant, or cast a more searching ray on the obscure pervasive co-sentiency of man and man.

We will next give a sample of an auditory impression. And here we could easily give cases in precise parallelism with our recorded experiments—cases, that is, where the actual words used by a person dying or in strong distress are represented to the consciousness of a friend at a distance. But the following, a somewhat more complex instance, is perhaps of still greater interest. We received it from the Rev. Andrew Jukes, Upper Eglinton Road, Woolwich.

On Monday, the 31st of July, 1854, I was at Worksop, staying in the house of Mr. Heming, the then agent there to the Duke of Newcastle. Just as I woke that morning—some would say I was dreaming—I heard the voice of an old schoolfellow (C. C.), who had been dead at least a year or two, saying, "Your brother Mark and Harriet are both gone." These words were echoing in my ears as I woke. I seemed to hear them. My brother then was in America; and both were well when I had last heard of them, but the words respecting him and his wife were so vividly impressed upon my mind that before I left my bedroom I wrote them down, then and there, on a scrap of an old newspaper, having no other paper in the bedroom. Could they have been the end of a dream, haunting me in the moment of waking? They seemed to me like a voice from the unseen. That same day I returned to Hull, and mentioned the circumstance to my wife, and entered the incident, which had made a deep impression on me, in my diary, which I still have. On the 18th of August (it was before the Atlantic telegraph), I received a line from my brother's wife, Harriet, dated the 1st of August, saying that Mark had just breathed his last, of cholera; after preaching on Sunday, he had been taken ill with cholera on Monday, and had died on Tuesday morning; that she herself *was ill, and that* in the event of her death she wished their

children should be brought to England. She died the second day after her husband, on the 3rd of August. I immediately started for America, and brought the children home. The voice I seemed to hear, and which at first I thought must have been a kind of dream, had such an effect on me, that, though the bell rang for breakfast, I did not go down for some time. And all that day, and for days after, I could not shake it off. I had the strongest impression, and indeed conviction, that my brother was gone. But you should notice that at the moment when I seemed to hear this voice my brother was *not* dead. He died early next morning, on the 1st of August, and his wife nearly two days later, namely, on the 3rd of August. I do not profess to explain it—I simply state the facts. I ought perhaps to add that we had no knowledge of the cholera being in the neighbourhood of my brother's parish. My impression was that both he and his wife must, if the voice was true, have been taken away by some railway or steamboat accident.

Here we may conjecture that the first shock of alarm in the mind of Mrs. Mark Jukes—at the idea of cholera in her household—flashed itself upon the mind of Mr. Andrew Jukes at the impressible moment of waking, but clad itself in his brain, for some untraceable reason, with a dreamlike reminiscence of the old schoolboy friend.

We are excluding from this Report all cases where the impression received suggests a presence wholly external to the percipient's organism, and thus seems to differ not only in *degree* but in *kind* from the more ordinary forms of experiment. In the case of *sound*, however, it is admittedly difficult to draw the line between what is *inner* and what is *outer* sense; and we have therefore included Mr. Jukes' narrative. If we were to pass from the ear to the eye, we should be able to cite instances of *incipient visualisation* of a very similar kind; but the subject of visual phantasms will be more conveniently treated as a whole in our next Report.

From these cases of transferred *sensory* impression the transition to transference of more abstract conceptions is gradual and imperceptible. Ideas are derived from sensations, and intimately interfused with surviving elements of sense; and in the spontaneous, as in the experimental cases, the percipient who receives an *idea* from another mind can hardly tell in what shape or investiture it sprang up in his own—clothed upon by mental picture, or mental word, or in the state described by one great Irish orator when he said of another, "He brings a brood of lusty thoughts into the world, without a rag to cover them." Let us quote first a case (originally cited by Mr. Knowles, in a letter to the *Spectator* of January 30th, 1869) which closely resembles some of our published experiments:—

Mr. Robert Browning tells me that when he was in Florence some years since, an Italian nobleman (a Count Giunasi, of Ravenna), visiting at Florence,

was brought to his house without previous introduction, by an intimate friend. The Count professed to have great mesmeric or clairvoyant faculties, and declared, in reply to Mr. Browning's avowed scepticism, that he would undertake to convince him, somehow or other, of his powers. He then asked Mr. Browning whether he had anything about him then and there which he could hand to him, and which was in any way a relic or memento. This, Mr. Browning thought, was, perhaps, because he habitually wore no sort of trinket or ornament, not even a watch-guard, and might therefore turn out to be a safe challenge. But it so happened that, by a curious accident, he was then wearing under his coat-sleeves some gold wrist-studs to his shirt, which he had quite recently taken into wear, in the absence (by mistake of a sempstress) of his ordinary wrist-buttons. He had never before worn them in Florence or elsewhere, and had found them in some old drawer, where they had lain forgotten for years. One of these studs he took out and handed to the Count, who held it in his hand awhile, looking earnestly in Mr. Browning's face, and then he said, as if much impressed, "C'è qualche cosa che mi grida nell' orecchio, 'Uccisione, uccisione!'" (There is something here which cries out in my ear, "Murder, murder!")

"And truly," says Mr. Browning, "those very studs were taken from the dead body of a great-uncle of mine, who was violently killed on his estate in St. Kitts, nearly 80 years ago. These, with a gold watch and other personal objects of value, were produced in a court of justice, as proofs that robbery had not been the purpose of the slaughter, which was effected by his own slaves. They were then transmitted to my grandfather, who had his initials engraved on them, and wore them all his life. They were taken out of the nightgown in which he died and given to me, not my father. I may add that I tried to get Count Giunasi to use his clairvoyance on this termination of ownership, also; and that he nearly hit upon something like the fact, mentioning a bed in a room, but he failed in attempting to describe the room —situation of the bed with respect to windows and door. The occurrence of my great-uncle's murder was known only to myself, of all men in Florence, as certainly was also my possession of the studs."

Mr. Browning, in a letter to us, dated the 21st of July, 1883, affirms that the account is "correct in every particular"—adding, "My own explanation of the matter has been that the shrewd Italian felt his way by the involuntary help of my own eyes and face. The guess, however attained to, was a good one." We think that in this conjectural explanation the illustrious author of *Sordello* has done imperfect justice to his own power of concealing his thoughts; and we fancy that his involuntary transparency of expression would not have enabled the wily Italian to "feel his way" to murder. But of course such cases are more complete when agent and percipient are at a distance which excludes involuntary hints.

Such instances are the following. The first is from a lady who requests us not to publish her name. We learn from the friends with

whom she was staying that they "remember the circumstances perfectly well."

On the 5th of November, 1855, when I was staying at a country house with several friends, one wet November day we amused ourselves by reading aloud, of which I did a large share, but was so overcome by the impression that a very dear brother was drowning, that ice had broken, and that he was drawn under it by the current, that I could not at all follow the purport of the book, and when alone dressing for dinner, could only control my distress by arguing that there could be no fear of ice accidents, as the weather was exceptionally mild at that time. We afterwards learned my brother had been in very actual peril, having jumped into a canal dock to rescue a companion, who, being short-sighted, had fallen in in the dusk of the evening. My brother was then an undergraduate at Cambridge, and I was in Wales. He received a medal from the "Humane Society," and a watch, &c., from the members of his college, in recognition of the act. I have never had any other similar impression of death or danger to any one.

The next case is from Mr. Robert Castle, estate-agent to many of the Oxford colleges:—

OXFORD, *October 13th*, 1883.

In the years 1851 and 1852, when I was from 15 to 17 years of age, I was left in charge of a considerable extent of building and other estate work at Didcot, Berks, at which some 50 or 60 men were employed; and for so young a person a good deal of responsibility was put upon me, as I was only visited occasionally, about once a fortnight on an average, by one of the seniors responsible for the work.

Occasionally this senior was my brother Joseph, about eight years older than myself, and who had always taken, even for a brother, a very great deal of interest in my welfare, and between whom and myself a very strong sympathy existed.

I was very rarely apprised by letter of these visits, but almost invariably before my brother came (sometimes the day before, at other times at some previous hour on the same day) it would suddenly come into my mind as a quite clear and certain thing, how I cannot say, that my brother was coming to see me and would arrive about a certain hour, sometimes in the morning and sometimes in the afternoon, and I cannot remember a single occasion, on which I had received one of these vivid impressions, on which he did not arrive as expected.

I had, without thinking particularly about it, got to act upon the faith of these impressions as much as if I had received a letter, and the singularity of the occurrence was not brought very forcibly to my own mind until one day when the foreman asked me to give him instructions as to how a portion of the work should be carried out, when I answered him quite naturally, "Oh, leave it to-day, Joe will be here about 4 o'clock this afternoon, and I would rather wait and ask his advice about it." The foreman, who had access to my office, and usually knew what letters I received, said, "Perhaps it *would be as* well, but I didn't know that you had received a letter from *Oxford.*" *I had* to explain to him that I had not received a letter, and

that it was merely by an impression I knew my brother was coming, and upon this I got a hearty laugh for my credulity.

As my brother turned up all right at the time named the foreman would not be convinced that I had not been playing a trick upon him, and that I had not received a letter and put it away, so that he might not know of it.

The strangeness of the matter then induced me to arrange with the foreman always to let him know, as soon as I might have the opportunity, on the occurrence of these impressions, so that he might check them as well as myself, and he, although he gave up all attempts to explain the singularity of the thing, came afterwards to trust the certainty of their being right as much as I did myself.

I told my brother of them, who was very much puzzled and could not account for so strange an occurrence; but on comparing my statements as to the time when the impressions occurred to me, in a number of cases, he said that, so far as he could check the time, it would seem to have been always at or about the time when he first received his instructions, or knew of the arrangement having been made for him to come.

As both the foreman and my brother have been dead for some years past, I have no means of comparing their recollections of these matters with my own.

Perhaps I should add that my brother was living at Oxford at the time, 10 miles or so from Didcot, and that although I was visited from time to time by other gentlemen beside my brother, I cannot remember having had these previous impressions in any case except his. ROBERT CASTLE.

The next case may also fairly be classed as the transference rather of an idea than of an emotion. We received it from Mrs. Herbert Davy, Burdon Place, Newcastle-upon-Tyne.

December 20th, 1883.

A very old gentleman, living at Hurworth, a friend of my husband's, and with whom I was but slightly acquainted, had been ill many months. My sister-in-law, who resides also at H., often mentioned him in her letters, saying he was better or worse as the case might be.

Late last autumn my husband and I were staying at the Tynedale Hydropathic Establishment. One evening I suddenly laid down the book I was reading, with this thought so strong upon me I could scarcely refrain from putting it into words: "I believe that Mr. C. is at this moment dying." So strangely was I imbued with this belief—there had been nothing whatever said to lead to it—that I asked my husband to note the time particularly, and to remember it for a reason I would rather not state just then. "It is exactly 7 o'clock," he said, and that being our dinner hour, we went downstairs to dine. The entire evening, however, I was haunted by the same strange feeling, and looked for a letter from my sister-in-law next morning. None came. But the following day there was one for her brother. In it she said: "Poor old Mr. C. died last night at 7 o'clock. It was past post time, so I could not let you know before."

E. M. DAVY.

December 27th, 1883.

I have a perfect recollection of the night in question, the 20th of October, 1882, when my wife asked me to tell her the time. I told her the time, as she "had a reason for knowing it," she said. She afterwards told me that reason. HERBERT DAVY.

In all these cases the idea conveyed to the percipient, though it doubtless involved a faint sensory image and had a certain amount of emotional colouring, was at any rate abstract in so far as it was the idea not of an object, but of a *fact* or *event*. The following account presents an interesting contrast in the total absence of an emotional element; while the idea transferred was of the most unpictorial kind. Mr. Keulemans, a scientific draughtsman, of whose accuracy as a witness we have had several examples, has experienced so many of these coincidences that, even before our inquiries quickened his interest in the matter, he had been accustomed to keep a record of his impressions —which, as he informs us, were invariably justified by fact.

In the summer of the year 1875, about 8 in the evening, I was returning to my home in the Holloway-road, on a tramcar, when it flashed into my mind that my assistant, Herr Schell, a Dutchman, who knew but little English (who was coming to see me that evening), would ask me what the English phrase "to wit" meant in Dutch. So vivid was the impression that I mentioned it to my wife on arriving at my house, and went so far as to scribble it down on the edge of a newspaper which I was reading. Ten minutes afterwards Schell arrived, and almost his first words were the inquiry, "Wat is het Hollandsch voor '*to wit*'?" (The words scribbled on the newspaper were not in his sight, and he was a good many yards from it.) I instantly showed him the paper, with the memorandum on it, saying, "You see I was ready for you." He told me that he had resolved to ask me just before leaving his house in Kentish Town, as he was intending that evening to do a translation of an English passage in which the words occurred. He was in the habit of making such translations in order to improve his knowledge of English. The time of his resolution corresponded (as far as we could reckon) with that of my impression.

J. G. KEULEMANS.

We have now, in rough adherence to the common division of man's nature into emotions, will, sense, and intellect, indicated under each of these heads some apparent anomalous and aberrant phenomena, which we have attempted to place in close parallelism with actual *experiment*, and to colligate provisionally under the general conception of telepathy, the precise law of which has yet to be ascertained. Meanwhile, we must specially warn the reader against concluding, from the word *force* which we are obliged to use, that the law is necessarily a *physical* one, or that this distant-working force can in any way be co-ordinated with the recognised forces of the material world. Not only, as with other delicate phenomena of life and thought, is the subjective side of the

problem the only one that we can yet attempt to analyse: we do not even know where to look for the *objective* side. If there really is a physical counterpart to the *fact* of transmission—over and above the movements in the two brains which are the *termini* of the transmission—that counterpart remains wholly unknown to us. The physical analogies hitherto suggested for telepathic impulses are aids to imagination and nothing more. Mr. Knowles' "brain-wave" was a metaphor well chosen for its purpose, namely, to attract public attention to a novel field of inquiry. But the transformation (by Dr. Maudsley) of "brain-wave" into the more ambitious "mentiferous ether" only serves to throw into stronger relief the real absence of provable likeness between the psychical and the physical modes of communication. In fact, the first *nodus* of the problem lies in the relation of telepathy to *space* and to *matter*, in the states in which matter is known to us. Unless *some* such relation can be demonstrated we cannot reasonably speak of a *psychical telergy*—an action of mind on mind at a distance—as correlated with any energy which we have learnt to measure. For even the force of gravitation diminishes with distance, and there is no force whose influence on a distant point is not liable to be intercepted by various forms of matter; while, on the other hand, it seems not improbable that the action of mind on mind operates as easily from London to Melbourne as from this room to the next. It is true that in our actual experiments we have found the physical nearness of agent and percipient to be in the normal state always, and in the hypnotic state usually, a necessary condition of success. But in our experiments we seem to be dealing with weak and incipient stages of a *rapport* which, when thoroughly established, and vivified by adequate stimuli, may be transmitted without appreciable impairment or delay, not only through walls, but over oceans, or through the centre of the earth.

Understanding, then, that from physical conceptions we can hope at present for nothing more than *suggestive analogies*, we prefer to seek those analogies on more sides than one; not only in the conceptions of *radiance and undulation*, but in the conceptions of *attraction and affinity*. The illustration which we should be most inclined to use, as we note the extraordinary intensification of telepathic impulses at the moment of dissolution, would be drawn from "nascent hydrogen"—from some gas, let us say, which, set free by an electrical current from its long union with some less volatile element, shows at its first moment of deliverance an unusual eagerness to unite itself with any suitable substance in its vicinity; but which, in default of such substance, escapes away, recoverable by us no more from its diffusion in the height of heaven.

But the testing or verification of such speculations as these must be

left for a later stage of this inquiry. The achievement which we claim for our Society is not a *theory of causes* but a *colligation of facts.* We claim that it has been shown that certain small experimental results can be produced, and that certain impressive spontaneous phenomena, generally discredited as anomalous, can be plausibly shown to belong to the same class as these small results of experiment. To recur once more to a previous metaphor, we may say that we have produced frictional electricity on a small scale, and indicated the probable connection of lightning with the sparks thus obtained. But we have not yet tracked the birth of the thunderbolt, nor lit our highways with the obedient flame.

Here we must break off. We are obviously as yet only on the threshold of Apparitions as commonly understood—the visible phantoms, externalised in space, which, above all things, our title pledges us to discuss. This further step, it may seem, must surely sever us from the experimental support to which we have hitherto clung, and bring us face to face with quite new problems. But though this is to some extent true, we shall not quit our old basis. We shall still hold fast to our fundamental doctrine of Thought-transference; we shall still seek the origin of the phenomena not in "transcendental physics," but in human psychology. The object of our next Report will be to show after what fashion the minds of men, as already known to us, may be the matrix of these airy crystallisations, the camera whence these phantasmal images are projected upon the waking world—what law is their Summoner and their Disperser, the Hermes which "guides them harmlessly along the darksome way."

II.

SECOND REPORT

OF THE

COMMITTEE ON HAUNTED HOUSES, &c.

Committee:—Rev. W. D. Bushell, M.A.; F. S. Hughes,* B.A.; A. P. Perceval Keep; F. Podmore,* M.A.; Hensleigh Wedgwood, J.P.; and Edward R. Pease,* *Hon. Sec.*

A year has elapsed since the last report of the Committee on Haunted Houses was issued. During this time we have not been idle, although we have no very startling theory to propound, or discovery to announce. We have been occupied in the main in collecting and sifting evidence, in printing those stories which seemed moderately complete, and in making some few personal examinations of houses reputed to be haunted.

We are moreover convinced that at present we are not in a position to attempt anything beyond the collection of evidence and the making of experimental observations. Our subject is an obscure and difficult one; the stories which come before us present very various features, and a considerable experience will be necessary before, with any confidence, we can judge of their evidential value. But, as a whole, the evidence before us unquestionably points to the reality of this class of abnormal phenomena. We are not investigating the origin of fables; we are examining facts; and the quantity of evidence for them, which we are now engaged in sifting, far surpasses our expectation.

Nevertheless, as noticed in our first report, we still find but few opportunities for making experimental investigations of Haunted Houses. Often such houses belong to families who are not anxious to attract attention, and perhaps ridicule, by allowing us to make serious examinations. In other cases, where something abnormal is seen by a tenant, we do not hear of it till he has left the house, which is either occupied by another tenant or else can only be rented by us at considerable expense.

But beyond these there is another more serious obstacle to experimental investigations. It is very seldom that phantoms appear or disturbances occur with any frequency or regularity. On this point we shall have something to say further on. Meanwhile it is sufficient to state that when cases of repeated appearances are investigated, they usually

prove to be in this respect exaggerated. The original account is that the figure was seen or the noise heard on one or two occasions in the course of perhaps a dozen years. The report of such a story that first reaches us often is that a figure is *constantly* seen, or a noise heard *every night.* As therefore there is no satisfactory means of testing the reality of the phenomenon, except upon the rare occasions of its occurrence, we are losing hope that the experimental branch of our inquiries will furnish us with much matter of value.

Nevertheless, we would beg any of our members and friends who are so fortunate as to inhabit haunted houses, to afford us, if possible, some opportunity for investigation. It may be that we are not likely to obtain results, that is, to see or hear anything abnormal. But there is some small chance of success, and for this chance we are willing to incur much trouble and expense. If one of us could have some actual experience of these phenomena, we should feel amply rewarded.

In these circumstances we are obliged to fall back on the testimony of those fortunate persons who have seen phantasms or heard abnormal noises. Now this evidence is not necessarily of small value. If a phantasm is seen by several trustworthy witnesses, their account of the matter is not vitiated because they do not happen to be members of the Haunted House Committee.

We have made an analysis of the 65 stories which are printed as being provisionally complete, but which, of course, form but a small proportion of our collection. We have classified their evidential value under four qualities, A B C and D. Of these 28 are A, or first-class stories, for which the evidence is clear and strong, and the witnesses for which we regard as worthy of credence. This does not, of course, represent the true proportion of A stories amongst the total number which reach us, since vague, unsatisfactory, and second-hand stories are not usually worth printing. We now propose to give some particulars of these 28 A stories, for the purpose of showing, not so much what they are, as what they are not.

In the first place the whole of these stories are first-hand. In every instance we have received the account from the actual witness of the occurrence reported, and, in most cases, our informant is known personally to some member of the Haunted House Committee, or of the Literary Committee.

We are accused of obtaining our evidence principally from women. This is, no doubt, true to some extent, and very good reasons could be given why women should be our most frequent and accessible witnesses. Nevertheless, for these 28 cases we have the evidence of 40 witnesses, of whom 14 are men and 26 women.

The appearance of figures is recorded in 24 of these stories, *whilst 4 record* noises only. This is, of course, a proportion the reverse of

what obtains amongst our stories as a whole. Noises are more abundant than visual appearances, but they are far less easily verified. If a phantom is seen it can be clearly described, and must generally be explained as an hallucination, or as a dream, or as something abnormal. But noises may be all these, and, as well, may be rats, or wind, or wood starting, or a dozen other things. Therefore, it is only now and then that we obtain accounts of haunted houses where nothing has been seen, which are sufficiently clear and detailed to be treated as first-class evidence.

Omitting the four cases of noises only, and five other cases which are of an exceptional nature, we have left 19 stories, all as regards evidence, of the A or first class. In these stories we shall now point out the presence of certain common features, and the absence of a good many other features which might have been expected by anyone fresh to the subject.

In the magazine ghost-stories, which appear in such numbers every Christmas, the ghost is a fearsome being, dressed in a sweeping sheet or shroud, carrying a lighted candle, and speaking dreadful words from fleshless lips. It enters at the stroke of midnight, through the sliding panel, just by the blood stain on the floor, which no effort ever could remove. Or it may be only a clanking of chains, a tread as of armed men, heard whilst the candles burn blue, and the dogs howl. These are the ghosts of fiction, and we do not deny that now and then we receive, apparently on good authority, accounts of apparitions which are stated to exhibit some features of a sensational type. Such cases are, however, very rare, and must for the present be dismissed as exceptional.

In examining the normal type of apparition, we find in the first place that these appearances scarcely ever bear any relation to special times. In none of the 19 cases we are considering is the apparition seen at any known fixed time, either of the day or of the year. There are, no doubt, stories that seem to be authentic of apparitions on special days and at particular hours. One of the cases now published records the occurrence of noises at a fixed time. But in general this would seem to be an exceptional feature. It may indeed exist in some cases where it has not been observed, but on the other hand it is a sensational detail which might easily be added in cases where it is really absent.

This fact, if such it be, is important. We are usually told that phantasms appear on the anniversary of some tragic occurrence, reputed as their origin. But if subsequent observations show that the supposed time-relation has no real existence, our task will be simplified when we have to ascertain the cause of these phenomena.

Another characteristic which these 19 cases present to our

notice is the apparently casual and objectless nature of the appearances. A figure is seen, and that is all. It does not act as if alive; it does not speak or use its limbs. If it moves, it is usually described as "gliding," that is, moving in an indefinite manner. It seems to resemble a magic-lantern figure more closely than anything else we can suggest. We may mention in this connection that the production of ghosts as practical jokes, whether by magic lanterns or by any other means, would seem to be very uncommon. Only one distinct case has come under our notice.

Turning to the descriptions of the apparitions, we notice that in 12 cases the figure is taken to be a living person, whilst in 7 cases only it is recognised at once as a phantom. Again, in 13 cases it is seen distinctly, and described in detail; in 6 cases only it is stated to be shadowy and indistinct. On these points, therefore, the evidence is not decisive; and for the present we can only say that the figure, if seen distinctly, is found to appear in such clothes as are now, or have recently been, worn by living persons.

We find that in 7 out of the 19 cases a sudden death, often either a murder or a suicide, appears to be connected with the cause of the apparition. In 7 other cases long residence in the locality, and often a peculiar attachment to some special house or room, seem to be similarly connected with it; whilst in the remaining 5 cases no explanation of its origin is suggested to us.

As regards frequency of appearance, the A stories are not a fair test, since the fact that we have good first-hand evidence for several occurrences of the same apparition is, of itself, generally a sufficient reason for giving the story a place in the first class. Yet in 8 out of these 19 cases, we have records only of a single appearance. And even when a figure is seen more than once, it is often only at intervals during a considerable period. The instances where an apparition is seen on several occasions during one year are very few indeed.

The generalisations which we have now laid before you are professedly made from the 19 cases under consideration, but in reality we have drawn them from our knowledge of the whole mass of evidence in our possession. We find that a large majority of the stories which prove to be genuine, possess certain features; and, on the other hand, we find that in nine cases out of ten, when a story differs widely from the type it proves to be incorrect, or unattainable from an authentic source. We are not prepared to affirm that all exceptional stories are unworthy of credence. But we are inclined to believe that a larger collection of material, and a more careful study of it, will, before long, enable us to lay down with some certainty and precision the laws of the occurrence of these phenomena.

In conclusion, we must repeat that we offer no hypothesis to account

for the facts which we are discussing. We have avoided the convenient word "ghost" because it might be understood as conveying the idea that phantoms are due to the presence of departed spirits. To this hypothesis we desire to give no countenance. Until our collection of facts is larger, and our examination of them more complete, we absolutely decline to theorise.

We have now to bring before you two narratives, the first of which is a fair example of the normal type. It is a good case, because we have accounts of the apparition on three distinct occasions, and from three separate persons. Moreover, the story is of recent date, and the witnesses are all alive, and personally known to our President and to Professor Barrett.

The evidence is as follows :—

In relating simply what I saw one July morning in the year 1873, I will first describe the room in which I saw it. It is a bedroom with a window at either end, a door and a fireplace at opposite sides; the door is nearer the front window, and the room is on the upper storey of a two-storied house, as you know, some miles from the city of D——. The house is an old one, said to have been built before the Rebellion; it has all the appearance of dating as far back as that, at least, the walls being unusually thick, and the roof high-pointed and uneven. A large old-fashioned garden lies in front, and a yard opening upon the public road at the rear. The occupants at the time I speak of were my brother Henry, myself, and a servant woman. The latter slept in a room in the basement storey. A lobby divided my brother's room from mine. On the night before the morning above-mentioned, I had locked my door, as usual, and having undressed and put out my light, I fell into a sound dreamless sleep. I awakened, I should think, about 3 o'clock in the morning—of the hour I am not sure—with my face to the front window. One of my oddities consists in disliking cross lights, so that I had got into the habit of having the blind of the back window drawn and the shutters closed at night, and of leaving the blind raised and the shutters open towards the front, liking to see the trees and sky when I awakened.

Opening my eyes now I saw right before me the figure of a woman, stooping down and apparently looking at me. Her head and shoulders were wrapped in a common grey woollen shawl. Her arms were folded, and they were also wrapped, as if for warmth, in the shawl. I looked at her in my horror, and dared not cry out lest I might move the awful thing to speech or action. I lay and looked and felt as if I should lose my reason. Behind her head I saw the window, and the growing dawn, the looking-glass upon the toilette table, and the furniture in that part of the room. After what may have been only seconds—of the duration of this vision I cannot judge—she raised herself and went backwards towards the widow, stood at the toilette table, and gradually vanished. I mean she grew by degrees transparent, and that through the shawl and the grey dress she wore I saw the white muslin of the tablecover again, and at last saw that only in the place where she had stood. I have heard people speak of lying "without moving.

a muscle." For hours I lay as I had lain on first awaking, not daring even to turn my eyes lest on the other side of the bed I should see her again, and first had courage to stir when the servant came to call me. Now there is one thing of which I could take my oath, and that is, that I did not mention this circumstance either to my brother or our servant. I was almost morally certain that if I had done so the latter, whom we valued, would leave us, and that the former would turn me into ridicule. We had been only about four months in that neighbourhood, and had few acquaintances, and I am equally certain that I mentioned the occurrence to none of them. At the distance of about a mile and a-half from us lived a family with whom I had long been intimate, but to whom my brother had not then been introduced. After passing a sleepless night with a perfect glare of lamp-light about me, and not daring even to close my eyes, I started on the second day to see my friends and consult with them. As we sat at luncheon I told them about it, half dreading the ridicule of my host, but though he smiled at first he went gravely into it, and proved in a sensible, matter-of-fact way, entirely to his satisfaction, that the apparition was attributable to the state of the drainage or that of my stomach. His wife and daughter were sorry for my evident nervousness, and, I fancy, set down a good deal of it to a very imaginative mind. However, I went home considerably relieved, and struggled hard to obliterate my vivid remembrance with my friend's arguments.

Exactly a fortnight afterwards, when sitting at breakfast, I noticed that my brother seemed out of sorts, and did not eat. On asking if anything were the matter, he replied: "No, but I've had a horrid night-mare. Indeed," he went on, "it was no night-mare. I saw it early this morning, just as distinctly as I see you." "What?" I asked. "A villainous looking hag," he answered, "with her head and arms wrapped in a cloak, stooping over me, and looking like this——" He got up, folded his arms, and put himself in the posture I remembered so well. "O, Henry," I said, "I saw the same a fortnight ago." "And why did you not tell me before?" he asked. "Because," I said, "I was sure you would only laugh." "I should be sorry," he said, "if anyone laughed at me if I mentioned this; it has quite upset me." He then described how the figure moved towards the door and disappeared. I asked him if she wore a cloak or a shawl, and he said it might have been either—he was chiefly struck by her malevolent face and her posture.

About four years afterwards, in the month of July, one evening about 7 o'clock, my second eldest sister and two little children were the only people at home. The eldest child, a boy of about four or five years, asked for a drink, and on leaving the dining-room to fetch it my sister desired the children to remain there till her return, leaving the door open. Coming back as quickly as possible, she met the boy pale and trembling on his way to her, and asked why he had left the room. "Oh," he said "who is that woman, who is that woman?" "Where?" she asked. "That old woman that went upstairs," he answered. She tried to convince him that there was no one else in the house, but he was so agitated and so eager to prove it *that she* took his trembling hand in hers and brought him upstairs, from *one room to another*, he searching behind curtains and under beds, still

maintaining that a woman "did go upstairs." My sister thought that the mere fact of a woman going upstairs in a house where he was a stranger would hardly account for the terror of the child. My brother had then married and gone to live at a distance.

A gentleman with whom we became acquainted in the neighbourhood, started when we first told him of what we had seen, and asked had we never heard that a woman had been killed in that house many years previously, and that it was said to be haunted. He is a sober-minded, religious man, an Englishman of middle age, the most unimaginative man I ever met, but he says he firmly believes we actually saw what he described.

I have heard that the house was previously for years unlet owing to having got the name of being haunted, and this was our only reason for keeping the story very much to ourselves. About three years since, I mentioned what we had seen to the sub-agent, a respectable man, and he seemed annoyed and asked if anyone had been telling us anything of the kind, to which we replied in the negative. We had been awakened night after night by the sound of heavy blows such as I have heard produced by breaking up peat with a cleaver, and, day and night, heavy footsteaps going up and down stairs. One night as I was going to bed—my youngest sister had been in about 10 minutes previously to say good night—I heard a loud knocking at the panel of my door—someone knocking with the knuckles—and I called out, "Come in! As there was no answer I called out louder, "Come in!" taking for granted it was my sister come back for some reason. As she did not answer and was not at my door when I opened it, I went across the lobby to her room, saying, in rather a tone of annoyance, "Why didn't you come in?" when I saw the room was dark, and getting a light saw she was sound asleep.

Another night, while undressing at the far end of the room, I heard the sound of a large bunch of keys flung violently on the toilette-table. At first I was startled, but took my light over to examine it, thinking I might have left keys or some heavy metal in such a position as to cause a fall like what I had heard, but still could not get over the idea that they were *flung* violently. There was nothing of the kind to be seen on or near the table. Two or three small keys hung in one of the drawers of my chest of drawers, and were not in motion. Frequently, in that room, both in day-time and at night, my dress has been pulled as if grasped by some hand.

On one occasion, as I was laid up with erysipelas in my foot, my youngest sister had a bed moved into my room and slept there to take care of me. When one night, as I lay wide awake, my foot being rather painful, there was a loud sound beside my bed—as if some heavy body had fallen—indeed, the whole room shook with the fall. My sister started up out of a sound sleep, and cried out, "What is it? What is it?" in a frightened voice. "What did you hear?" I asked. "Something heavy falling there," she said, and pointed to the spot at which I heard it—which was, in fact, the very spot on which my visitant appeared to me. She got up and searched the room, but found nothing to account for it.

May 23rd, 1883. H. C. S. B.

About three years ago, on the night of the 7th July, I was awakened from a sound sleep by someone speaking close to me. I turned round saying, "What is it, Emily?" thinking that a sister who slept in the room next mine had come in. I saw plainly the figure of a woman who deliberately and silently moved away towards the door, which remained shut as I had left it. She disappeared, and not till then did the slightest doubt of my visitor being supernatural enter my mind. Then I got really frightened, saying to myself it must be the "Grey Lady," as we had for years called our ghost, the story of which I had often turned into ridicule. A few minutes afterwards I heard the hall clock strike two. I may mention that neither shutters nor blind were closed. I thought it best to say nothing of what had occurred to the only sister then at home, but the next evening when talking to a gentleman who was with us, she said suddenly to me, "Did you hear any noise last night? I thought it sounded as if in your room." I said, "No—at what hour?" "Two o'clock." Soon she left the sitting-room, and I told the friend with us, under the promise of secrecy, what I had seen.

M. B.

July 5th, 1883.

So far as I recollect, it was about this time 10 or 11 years ago, I was asleep in the house in question, and suddenly about 6 o'clock on a fine summer's morning I was awakened by a feeling or presentiment of approaching evil. I opened my eyes and distinctly saw the form of a darkly-clad elderly female bending over me with folded arms, and glaring at me with eyes of the most intense malevolence and malignity. I tried to scream, and struggled to withdraw myself from her, when she slowly and silently receded backwards and seemed to vanish through the bedroom door. I cannot say whether the door was locked; I generally keep it so at night, but it was certainly closed tight.

August 1st, 1883. H. B. B., Solicitor.

The next narrative is a remarkably clear account of a haunted house where noises were heard, but nothing was ever seen. It was written by a well-known Church dignitary, from whose widow we have received it. She was cognisant of all the facts related, and confirms the accuracy of the account in every detail. The case is remarkable as showing a periodicity in the noises, which is, as we have previously stated, by no means a common feature.

About 18 years ago, having completed the probationary period of two years from my ordination as deacon, I was in search of a curacy. Amongst others which came under my notice was one in the south-west of the county of S. The parish was extensive and the situation very retired. It was a sole charge, and a commodious house was at the disposal of the curate. The curacy was accepted, and in due time my wife and I proceeded to take possession of our new home. We reached it on the afternoon of a dull February day.

The vicarage we were to occupy was a square spacious building, surrounded by lawn and shrubberies, garden and orchard. The house was detached, situated a short distance from the village, and separated by a road from two

or three cottages which were the nearest dwellings. Our rooms were large and sufficiently lofty, everything was in good repair, and we congratulated ourselves on having secured a comfortable home.

It was, I remember, a Friday afternoon on which we arrived, and we worked with a will, and had two or three rooms fit for occupancy by Saturday evening.

Night fell, shutters were fastened, bolts shot and keys turned, and my wife and I retired to bed on that Saturday, not reluctantly, for we had worked for a couple of days as hard as porters in a warehouse.

We had not as yet engaged a servant, and had, therefore, availed ourselves of the help of an honest country woman who lived hard by. When I made all fast on the Saturday night, that honest country woman, my wife, and myself were—to the best of my knowledge and belief—the only three living beings within the four walls of the vicarage. Long before twelve we were all in the land of dreams, and probably some way beyond it, in that realm of sleep to which no "extravagant and erring" dream ever finds its way. Suddenly, however, there broke on our drowsy ears a sound which murdered sleep. In a moment, almost before consciousness had come, I was out of bed and on my feet, and even then it seemed as if that strange noise was only just passing into the accustomed silence of deep night. My wife was as abruptly and completely roused as myself, and together we listened for some repetition of what had disturbed us, or for some further token to guide us to the discovery of its cause. But nothing came. It was obviously my business to make an investigation without delay, for the natural solution of the mystery was that some one or more persons had made their way into the house.

Accordingly I hurried on a few articles of dress and set out on an exploring expedition. Before doing so, however, I looked at my watch, and found that it was just 2.5 a.m. I wish to call particular attention to this fact. I made a thorough search over the whole house. I examined the fastenings of the doors, the shutters of the windows. All was safe, all was quiet, everything was in its place. There was nothing left for me to do but to return to my room, go to bed, and think no more of the disturbance. This last was not so easy. Neither my wife nor I could persuade ourselves that it was a mistake. The sound was so palpable, broke on our sleep with so peremptory a summons, pealed on our half-awakened senses with so prolonged a crash, that neither could its reality be doubted nor its impression thrown off.

It struck me, then and afterwards, as being like the crash of iron bars falling suddenly to the ground. Certainly there was a sharp metallic ring about it. Moreover, it was prolonged, and, instead of coming from some fixed point, it seemed to traverse the house like a succession of rattling echoes, treading hard on one another's heels.

I speak of it not specially as it impressed me on the particular occasion to which I am referring, but from my general estimate of its character; for I may as well say at once that my acquaintance with it was not limited to the experiences of that one early Sunday morning. Of course—on my return to my room—when we talked the matter over, it occurred to us to ascertain whether the good woman from the village had also been roused by the din.

However, as she had not herself given any signs of alarm, we resolved to wait to see whether she had any tale to tell in the morning.

Well! the remaining hours of darkness passed away quietly enough, and when morning came we found that the third member of our household had been a sharer with ourselves in the mysterious visitation. She, like us, had been rudely awakened, and had long lain awake in a state of considerable disquietude and alarm.

To her, however, the thing was not quite so strange and unlooked for as to us. "Oh dear," she said, "I've heard tell of it afore, but never till last night did I hear it, and don't want to again."

She had heard tell of it before. But there was not much more to be got out of her, and she seemed unwilling to discuss the subject. "It was a conceit," she said, and that was all she chose to say about it. On one point, however, she was clear enough, and that was the necessity of going home that evening to look after her house and children. She would give us her services during the day, but she could not well be spared from home at nights. To this effect, therefore, an accommodation was made with her, and my wife and I stood committed for the coming night to be the sole garrison of the vicarage, whether it was to be assailed by tangible force or impalpable sounds. The Sunday duties were duly discharged. I met my parishioners in their church for the first time; looked round with satisfaction on a large and attentive, though not perhaps especially intelligent congregation, and could not help wondering whether any of those stolid young farmers and peasants, whose faces were turned so impassively towards the pulpit, had been indulging in a grim practical joke at my expense.

In due time, my wife and I found ourselves alone in the vicarage: the darkness of a winter night without, a snug wainscoted parlour, a bright fire, and sundry creature comforts within. Thus we sat, till about 8 o'clock. It then occurred to us to make an examination of the house, though we had taken care—as soon as it became dark and our handmaid had left us—to make everything, as far as possible, secure. We rose, then, and set off together, and passing out of our sitting-room, found ourselves in the square entrance hall, the door of which opened into the garden. Scarcely were we there before we heard a noise which made us pause and listen. The sound came from the long passage upstairs into which all the bedrooms opened, and was simply the sound of human footsteps walking slowly but firmly along the passage. There was no mistake about it. Bold, distinct, and strong, each footfall reached our ears. At once, candle in hand, I dashed upstairs, three steps at a time, and in a moment was on the landing and in full view of the passage. But there was nothing to be seen. My wife, of course, followed me, for she was becoming nervous. Together, therefore, we entered and searched the bedrooms. But our search was fruitless. If anybody had been there he had contrived by some way inexplicable to us to make his escape. A more complete and anxious examination of the house was the necessary consequence of this adventure, and we pretty well satisfied ourselves that, whatever might have caused the sounds we had heard, we were not the involuntary entertainers of any unbidden guest of flesh and blood. To make *assurance doubly* sure, I unbarred the yard door and took a survey of the *outside premises*. From this work, however, I was rather hastily recalled by

my wife, who announced that the inexplicable footsteps were again in motion, and though on my return they had ceased, yet once more that night they did us the favour of letting us hear them before we went to bed. Now at this point I am bound in honesty to say that when we returned to our parlour fire, which had a very encouraging and comforting look about it, my wife and I, in discussing the matter, did hint at the possibility of our having fallen in with "a haunted house." And it is only fair to add that we neither of us were so settled in all unbelief of the supernatural, as without further consideration to scout the notion as absurd. But assuredly we did not jump at once to any such conclusion, and were content with simply passing a resolution to the effect that the disturbances were somewhat extraordinary and rather disagreeable than otherwise.

That night we experienced no further annoyance, and indeed for a week or two there is nothing of any particular significance to record.

In the meantime we found ourselves fairly settled. One strong and willing female servant did all that we needed to have done indoors, and a lad of about 14 years of age was engaged to look after a couple of ponies and to do the sundry odd jobs. This boy, it must be observed, did not sleep in the house, so that unless we had a visitor, which did not often happen, the number of the inmates was only three. Our female servant was a stranger from a village at some distance, and had not, as far as we knew, any acquaintances in the place.

For some little time, as I have intimated, we were not much disturbed. The unexplained sound of footsteps we occasionally heard, but we troubled ourselves as little as possible about it, believing that whatever it might be it was at all events very inoffensive and not likely to interfere much with our comforts or prerogatives.

However, in due time we were favoured with a new development and that, too, of a kind which was sufficiently distinct and obtrusive. There was, it must be understood, a range of attics at the top of the house reaching over the full extent of it. We found them empty and in good repair, and we converted them into store-rooms for our boxes, packing cases, &c. They were reached by a small staircase opening off the main passage upstairs; and having deposited in them everything that we wished to put out of the way we secured the staircase door.

We had gone to bed one night as usual, and were about quietly to drop asleep, when all at once there commenced a tumult overhead, which very soon made us as wide-awake as we had ever been in our lives. The noise was, confessedly, of the most vulgar, commonplace, and substantial kind. It was—or rather I should say it seemed to be—the result of the tossing about over the attic floors of all the boxes, cases, and bundles stored there. It was loud, boisterous, and persistent. There was a bump, and a rattle, and a roll, and a crash. Of course an investigation was an obvious necessity, but an investigation discovered nothing. All was quiet. Everything was apparently undisturbed and as much in order as it ever had been, or, in such a place, could be expected to be. We were confessedly perplexed, and moreover,—as far as that, as well as the other occurrences went, we were condemned to the humiliation of remaining in a state of unrelieved perplexity.

But, besides, some supplementary entertainments were provided for our benefit. From time to time a succession of distinctly audible knocks would greet our ears. These knocks varied in their type. At one time they were hurried, eager, impatient; at another, slow and hesitating. But, however, in one style or another we were treated to them, I should say on the average, four nights a week during our sojourn at C——. These were, of all the phenomena, the commonest. I am bound, in justice to the unknown cause of them, to say that we were seldom disappointed in our expectation of hearing them. They were not very alarming, certainly, and after a little familiarity had bred the requisite measure of contempt, they were not particularly disturbing.

One feature about them, however, deserves to be noticed. Sometimes, while lying awake, an involuntary listener to their tattoo, I was provoked to the use of a little sarcasm or what school-boys would call "chaff." I would, for instance, address the hypothetical agent and bid it "be quiet, and not disturb honest people in their beds," or I would challenge it, if it had any request to make or any complaint to lay, "to come out and do it in a manly, straightforward way." Somehow or other these remonstrances were not well received. They always led to louder, more hurried, and if we may use such a term, more passionate knocking. The reader may smile at the notion of any connection between any wild words and the intensified rappings, and I do not wish to assert that there must necessarily have been any connection. I simply state the fact that coincidently with my challenge, the rappings intensified. I do not theorise, I tell a round, unvarnished tale. Possibly it was a coincidence and nothing more.

Did we—it may be asked—say anything to our neighbours about what we were so frequently experiencing? For a considerable time we did not. We had determined to hold our tongues for several reasons. In the first place if we talked about what had so much of the mysterious about it, we might give rise to exaggerations, and excite alarms which would make it a difficult matter to keep a servant or to get one. Moreover, we knew little of the characters of the people amongst whom we had come, and we thought that if it was the result of a trick we should, by saying nothing about it, be more likely to discover it, or to tire out the performers by assumed indifference. Hence, though our servant, who was a stout-hearted country wench, sometimes dropped hints of nocturnal disturbances, we always put aside the subject and discouraged her attempt to talk about it. So far I have strictly confined myself to what came under my own observation—to what I heard with my own ears. And I think that the experience of my wife and myself does not reach beyond the rappings, the confused noises in the attics, the well defined pacing of footsteps about the house, and that grand satanic crash. On these the changes were from time to time rung.

They began soon after our arrival, they were kept up with tolerable activity during our stay, and for anything I know we left them behind us when we departed. The great noise which greeted us on the first Sunday morning, as it was the most startling of all the phenomena, so it was the least frequent. Weeks sometimes passed without our hearing it at all. But *whenever we did* hear it—if we took the trouble to ascertain—we always *found that it* occurred at *two* o'clock on a Sunday morning. In the course of

time, we had incontrovertible evidence that it might manifest itself to some persons in the house, without my wife or myself being conscious of it.—Knowing how overwhelming the sound always appeared to me when I did hear it, I cannot but consider this fact one of the most wonderful things in the whole business. I will show, however, that it was so.

As the winter passed away, and our country became more attractive, we had a few visitors; amongst the earlier comers was a young lady, a very near relative of my wife. We agreed to say nothing to her about our own experiences, partly because we did not want her to be frightened by anticipation, and partly because we wished for a little independent, unprejudiced and spontaneous testimony. We very soon got it; our friend had not been many nights with us, before she began to put questions as to why we had made such a stir in the house after everybody, as she supposed, had retired to rest. Our answers to these inquiries were, as might be expected, a little vague and unsatisfactory. Once or twice she asked whether there was to be a funeral, for she had heard under her window what she concluded to be the sexton digging a grave, and she expressed a little surprise that he should choose to ply his melancholy trade during the hours of darkness. She was, of course, assured, as was indeed the case, that no funeral was about to take place, and, moreover, that whatever she heard under her window, it was at all events not the process of grave-digging, for the churchyard lay on the other side of the house. This was conclusive enough, no doubt, against her theory, but she did not the less persist in asserting that on several occasions she had heard a noise beneath her window, and that that noise was, in her judgment, the result of some form or other of spade-husbandry. I have no doubt of the reality of the impression made on her mind, but I never myself heard the sounds which she described.

I was not, however, particularly surprised, when, on another occasion, she told us that someone had walked along the passage, and knocked at her door, but that in answer to her call of "Who's there?" no reply had been vouchsafed, and no attempt at entrance into her room had been made.

At length Sunday morning arrived, and we met at the breakfast table.

"Whatever was the matter last night?" was our kinswoman's earliest greeting. "What a clatter somebody made! I was so thoroughly awakened, that I got up and should have come out of my room to see what had happened had it not been that I was afraid of encountering your dogs! However I was so much disturbed that I could not easily compose myself again to rest, and as I stood at my window, peering into the darkness, *I heard the church clock strike two.*" Hereupon my wife and I exchanged very significant looks. Our friend had heard that night—though we had not—what we had begun to call "The Great Sabbath Alarum." We then told her something of our own experience, and her impression of the sound harmonised with our own. I shall only mention one more incident collateral to what we ourselves observed, for it is on our *personal* experience that I rest the value and the interest of my story.

We were absent from home for a week or two during the autumn, and on our return our servant reported the following occurrences:—

One evening she had gone out into the village to do some business and had left *the servant boy in sole* charge of the house. He was seated by the

kitchen fire, when he heard some one,—as it seemed—tramping about the passages. He went to ascertain who it was, and what might be his business, but finding no one, he returned to the kitchen and tried to fancy that he was mistaken. Presently he again heard the apparent and palpable sound of human feet, and again he ventured to explore the premises, though with nerves a little more unsteady, and with glance more hurried and retrospective. Again he made a bootless quest. But when from his quiet seat in the chimney corner he heard for a third time the same mysterious echoes, it was too much for boyish flesh and blood. He rushed out of the house, hurried down the village, and never stopped till he told his breathless tale to the gaping inmates of his father's cottage. I have already mentioned that for some time I said nothing to any of my parishioners on the subject of these nocturnal disturbances.

Ultimately, however, I introduced the subject in a conversation with a very excellent Christian woman, a long and patient sufferer from a bodily infirmity, which altogether confined her to her bed. She had seen better days, was a Churchwoman of a good old type, full of a calm and sober religious spirit. Her cottage was just opposite to the vicarage, and the window of the little room in which she lay commanded a full view of it.

I told her what from time to time we had heard and asked her if any reports of such matters had ever reached her ears. She at once said that there had often been talk of such disturbances, and that some, at least, of my predecessors in the curacy had been a good deal annoyed by them. Moreover she added, what I am sure she would not have said if she had not thoroughly believed it—that she had herself at times seen flickering and intermittent light at the attic windows. Now it must be borne in mind that, during my occupancy of the house, these attics were not used, that I never myself entered them at night but on the occasions when I sought to discover the cause of the noise heard there, that there was but one possible entrance to the whole suite, and that we had made that secure, and as far as we could judge, had the means of admission exclusively in our own power.

My informant further told me of certain transactions which had taken place in that house in the last century, and of which she had heard from her elders, which, if they could be verified, and could be fairly connected with the disturbances in the relation of cause and effect, would certainly assist in enabling one to arrive at a theory as to the nature of the disturbances themselves.

But it is not my object to theorise, but simply to relate phenomena and leave them to be judged on their merits. For the facts related, I again say I can honestly vouch; for their causes I am almost as much thrown on conjecture as my readers, for with all the pains I took I never could make any discovery. The explanations which will probably suggest themselves to many, did not fail to suggest themselves to us. There was first of all the possibility of a practical joke. But supposing that with the care I took, and the watch I kept any persons could have gained admission to the house, they must have been the most patient and dreary jokers that ever gave their unrequited and unappreciated services to the genius of mischief. To say nothing of former years, only fancy any one troubling himself to keep up for

12 months at all hours of the night (and *occasionally in the day-time*), a succession of incoherent and inarticulate noises. Methinks a performer of average ability would have tried the experiment once or twice in way of a *visible* manifestation.

Then again there is the resource in such cases of *rats*. Well! I have a great respect for the capabilities of rats in the way of nocturnal clamour. If, however, they really achieved all that came under my own observation, then I must say that their abilities are wonderful. How, for instance, did they accomplish —and how did they so exactly time—the Great Sunday Crash? There is a circumstance that deserves to be considered by any one who may care to suggest an explanation of what I have related. I have always been something of a dog-fancier, and I had at that time, two Skye terriers of pure breed, excellent house-dogs, uncompromising foes to vermin, ready for any fun, with no delicacy as to letting their sweet voices be heard, if they saw good reason for speaking out. Once during our sojourn at C——, they did speak out to good purpose. The winter was a rough one, times were not good, and there were several robberies of houses in the neighbourhood. An attempt was made on the vicarage. My trusty dogs, however, gave prompt alarm, I was roused by their fierce barking, reached a window in time to see more than one dark figure on the lawn below, and was able to address such a remonstrance to them as led to a retreat, expedited in some measure by the discharge of a few shots from a pistol. I mention this incident simply to contrast the behaviour of the dogs on that occasion with their conduct in the presence of the mysterious noises. Against these they never once by bark or otherwise made any demonstration. Perhaps they did not hear them. It would seem otherwise, however, for when at such times, in making search about the house, I came where they were, I always found them cowering in a state of pitiable terror. Of this I am quite sure that they were more perturbed than any other members of the establishment. If not shut up below they would make their way to our bedroom door and lie there, crouching and whining, as long as we would allow them.

Our experience of the phenomena, which I have described, extended over a period of 12 months. At the end of that time I was appointed to a benefice in another part of England, and consequently resigned my curacy. We turned our backs on the vicarage, not sorry, it must be confessed, to be done with our nocturnal alarms, but disappointed at not having been able to discover the cause of them.

I have never visited the place since, and never had the opportunity of learning whether the attentions paid by those secret and invisible agents to us have ever been renewed in favour of our successors.

III.

PROCEEDINGS OF THE GENERAL MEETING ON

May 28, 1884.

The eighth General Meeting of the Society was held at the Garden Mansion, Queen Anne's Mansions, S.W., on Wednesday, May 28, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

The following address was delivered by the President:—

The last time that I addressed you at any length I endeavoured to define the nature and grounds of our claim that we are investigating in a scientific manner phenomena which in the recent progress of physical science have been too long and too persistently neglected. Since then, in consequence of an article which has appeared in the *Nineteenth Century* by two of my colleagues, and of a lecture which I was expressly asked to deliver on this subject at the London Institution, some discussion of our work from this point of view has been carried on in journals that are for the most part hostile to our endeavour; and it appears that I might with advantage take up again the subject that I dealt with about a year ago, and make one or two more remarks on our general scientific position. In so doing I have no intention of occupying your time by any comments on the misrepresentations of fact or the blunders in logic which our opponents have committed: our aim, in my opinion, should rather be to consider whether we can learn anything from our critics—even from ignorant and prejudiced critics—which may assist us in the novel and difficult work in which we are engaged. We may at any rate see what appear to the careless glance of outsiders to be the weak points of our position, and give them a careful reconsideration.

The first point that it is important to get clear is the exact relation in which the conclusion that we have, to our own satisfaction, established, stands to the generally accepted conclusions of physical science. Is it true, as an opponent has asserted, that if Thought-transference, as affirmed by our Committee, were admitted to be a fact, "physiology would be overthrown"? The statement might pass as a loose and hasty way of characterising the extreme strangeness of our results; but I cannot conceive its being deliberately maintained by anyone actually acquainted with physiological investigation. An instructed physiologist

would know that supposing it generally accepted that ideas and feelings can under certain special and rare conditions be conveyed from one mind to another otherwise than by the recognised channels of sense, all ordinary physiological research would go on exactly as before. No "working hypothesis" of physiological method would have to be abandoned; no established positive conclusion of physiological inquiry—nothing that has been ascertained as to the nature of the process by which visual, auditory, tactile, or other sensations and ideas are ordinarily produced in the mind—would have to be modified. What would have to be given up would be merely the single negative conclusion that ideas and sensations could not be transmitted from one mind to another except in certain ways already known. It was very natural for physiologists to form this conclusion provisionally in default of evidence to the contrary; but to abandon it in view of the presentation of such evidence would be a mere enlargement, not in any sense an overthrow of existing physiology.

The question, then, is merely whether evidence enough has been produced. And here I have always admitted, and indeed emphatically maintained, that what we allege to be facts are so contrary to the analogy of experience—at least so far as experience has been systematised by science—that until a large number of mutually corroborative testimonies are collected we cannot expect the scientific world to be converted; they will say, and reasonably or at least plausibly say, that it is less improbable that the testimony to these facts should be false than that the facts as testified to should be real. And I think that the case is one in which no one can say exactly how much evidence is wanted; we have to balance conflicting improbabilities; and the improbabilities are of a kind that we have no scales to weigh exactly. Indeed the improbability on one side necessarily appears greater or less to different persons, according to what they know of the witnesses personally. Hence though I am myself convinced of the trustworthiness of our records of experiments, I do not complain that other persons who do not know the witnesses are not yet convinced. And I have always been anxious to urge on our members and friends—many of whom are rather inclined to think that we have already collected facts enough to convince a "fair mind"—that we cannot precisely define the requirements of a fair mind in dealing with matters so unfamiliar; and that we ought to continue patiently piling up facts and varying the observers and conditions, until we actually get the common sense of educated persons clearly on our side.

At the same time, I am obliged to add that none of our critics appear to me to appreciate the kind and degree of evidence that we have already obtained. They often imply that the experiments in Thought-transference are such as could be performed by "cheating mediums or

mesmerists," by the simple means of a code of signals which the investigating committee cannot find out; quite ignoring such cases as that given in Part I., pp. 22-3, where the cards guessed by one of the Creerys were unknown to any one but the four strangers who went to witness the experiment; and where, therefore, as I have before said, the investigators must either have been idiots, or one or other of them in the trick. Similar remarks may be made about the experiments reported in the last part of our Proceedings; where four or five different persons must either have been guilty of unveracity or collusion, or of most abnormal stupidity, if the phenomena were not genuine.

Again, our opponents leave out of account that besides our own experiments in Thought-transference between persons in a normal condition, and the records of spontaneous telepathic phenomena, "apparitions, &c.,"—of which we have collected a very large number on first-hand evidence—we have the experiments in Thought-transference in the mesmeric state, in which we have only obtained over again results repeatedly affirmed by others. And here I think we may put forward an irresistible claim that this mesmeric evidence of a generation ago, which undoubtedly failed to satisfy orthdox medical opinion at the time, should be carefully reconsidered; the ground of our claim being the now universally admitted fact that in the controversy which took place from 1840 to 1850 between the mesmerists and the accredited organs of medical opinion, the latter were undoubtedly to a great extent wrong; that they repudiated sweepingly an important part of the phenomena reported by the mesmerists, which no instructed person now denies to be genuine. No instructed person now questions the genuine reality of the hypnotic or sleep-waking state as a special abnormal condition of the human organism, in which the hypnotised person is, in a quite peculiar way, subject to delusions suggested to him from without, and can in some cases be made as perfectly insensible to pain as he can by inhaling chloroform or laughing gas. But at the time I speak of the *Lancet* and other medical organs refused to admit the genuineness of these phenomena, as decidedly as any of them now refuses to admit the reality of community of sensation. When the most painful surgical operations were successfully performed in the hypnotic state, they said that the patients were bribed to sham insensibility; and that it was because they were hardened impostors that they let their legs be cut off and large tumours cut out without showing a sign even of discomfort. At length this unbelief, in all but the most bigoted partisans, gave way before the triumphant success of Mr. Esdaile's surgical operations under mesmerism in the Calcutta Hospital: and hence, when subsequently a German professor (Heidenhain) reported that he had obtained results similar to Braid's,—which had been previously neglected,—orthodox medical science willingly

allowed the hypnotic state to take a recognised place in physiological works. The existence, indeed, of a peculiar *rapport* between the mesmeriser and his patient—such as the transference of sensation manifests—has still the weight of medical authority against it; but this weight is surely diminished by the fact that it was so long and obstinately thrown into the wrong scale as regards the hypnotic state generally.

When confronted with this mass of testimonies, the argument of our opponents sometimes takes a new turn. They say that our very demand for *quantity* of evidence shows that we know the *quality* of each item to be bad. But the quality of much of our evidence—when considered apart from the strangeness of the matters to which it refers—is not bad, but very good: it is such that one or two items of it would be held to establish the occurrence, at any particular time and place, of any phenomenon whose existence was generally accepted. Since, however, on this subject the best single testimony only yields an improbability of the testimony being false that is outweighed by the improbability of the fact being true, the only way to make the scale fall on the side of the testimony is to increase the quantity. If the testimony were not good, this increase of quantity would be of little value; but if it is such that the supposition of its falsity requires us to attribute abnormal motiveless deceit, or abnormal stupidity or carelessness, to a person hitherto reputed honest and intelligent, then an increase in the number of cases in which such a supposition is required adds importantly to the improbability of the general hypothesis. It is sometimes said by loose thinkers that the "moral factor" ought not to come in at all. But the least reflection shows that the moral factor must come in in all the reasonings of experimental science, except for those who have personally repeated all the experiments on which their conclusions are based. Any one who accepts the report of the experiments of another must rely not only on his intelligence but on his honesty; only ordinarily his honesty is so completely assumed that the assumption is not noticed.

Here, however, some say that we ought to get evidence that can be repeated at will; that they will not entertain the idea of "rare, fitful and delicate" phenomena which cannot be reproduced at will in the presence of any number of sceptics. But I have never seen any serious attempt to justify this refusal on general principles of scientific method. The phenomenon of Thought-transference—assuming it to be genuine—depends *primâ facie* on the establishment of a certain relation between the nervous systems of the agent and percipient respectively; and as the conditions of this relation are specifically unknown, it is to be expected that they should be sometimes absent, sometimes present, in an inexplicable way; and, in particular, that this peculiar function

of the brain should be easily disturbed by mental anxiety or discomfort of any kind.

Still we should be very glad to get evidence of this kind; we ought to relax no effort to obtain it. And one special source of interest for us in the marvels related by the Indian Theosophists—with whose doctrines, I may remark, we are in no way concerned—lies in the fact that they are alleged to consist largely in the production at will of "telepathic" phenomena; similar in kind to those of which, as occurring spontaneously, a large collection has been made by our Literary Committee.

(A provisional Report on some of these Indian cases was then laid before the meeting.)

IV.

FOURTH REPORT OF THE LITERARY COMMITTEE.

Committee:—W. F. Barrett, F.R.S.E.; Chas. C. Massey; Rev. W. Stainton Moses, M.A.; F. Podmore, M.A.; and Edmund Gurney,* M.A., and F. W. H. Myers,* M.A., *Hon. Secs.*

A THEORY OF APPARITIONS.

Part II.

Our last Report—it may be remembered—brought us only to the threshold of the subject of Apparitions, as popularly understood. In that introductory paper we approached our main theme by three distinct steps. We first considered the general state of opinion with respect to it, the *à priori* arguments and assumptions which tend to preclude inquiry into it, and the method which we hold that the inquiry ought to pursue. We then explained that we intended to base our own theory on an experimental basis, and to connect the striking phenomena of death-wraiths with quite humble and unemotional forms of Thought-transference—embracing the whole set of facts, large and small, experimental and spontaneous, under the term "telepathy." And finally we justified this interconnection of the phenomena, and showed by examples that distinct effects—similar to those obtained in experimental Thought-transference—have been spontaneously produced on the emotions, the will, the senses, or the intellect of one person, by some corresponding affection of another person at a distance.

But among effects produced on the senses, one particular class was purposely deferred—that, namely, which concerns the sense of *sight.* It is this deferred class of telepathic disturbances that we have now to consider. Among these we find undoubtedly the furthest and most eccentric of the phenomena which the telepathic theory can be made to embrace; and our account of them will require that the theory, as so far stated, should be somewhat expanded. But for all that, they will not drive us from our old basis. We are about to treat visible apparitions as "transferred impressions." Viewed in this light, it will be found that even the most startling of them are not without experimental analogy; and that, moreover, we can lead up to these extreme cases by *quite gradual steps,* starting from a point where the experimental analogies are perfectly obvious.

To begin with the commonest and simplest form of experiment—

that where the impression of a card or number is transferred, without sensory communication, from one mind to another. Here the percipient sees the object "in his mind's eye," not as external to himself at all. Now we find an exact parallel to this lowest grade of visualisation in cases where the impression originates, not in the fact that the agent is concentrating his attention on a card or number, but in the fact that he is dying. Such a case is the following, given to us by Mr. Robert Rawlinson, of Lansdowne Court West, Cheltenham.

I was dressing one morning in December 1881, when a certain conviction came upon me that some one was in my dressing-room. On looking round I saw no one ; but then, instantaneously, in my mind's eye (I suppose), every feature of the face and form of my old friend William Stanley, of Ponsonby Hall, Cumberland, arose. This, as you may imagine, made a great impression on me, and I went at once into my wife's room and told her what had occurred, at the same stating that I feared W. S. must be dead. The subject was mentioned between us several times that day. Next morning I received a letter from George Stanley, then consul-general at Odessa, whom I did not know to be in England, saying that his brother had died at a quarter before 9 o'clock that morning. This was the very time the occurrence happened in my dressing-room. It is right to add that we had heard some two months previously that W. S. was suffering from cancer, but still we were in no immediate apprehension of his death.

Mrs. Rawlinson has kindly confirmed the fact of her husband's coming into her room, and describing his experience, at about a quarter to 9 on the morning in question. She adds that the name of W. S. had not been mentioned by anyone for weeks ; and that her husband "is the last person to imagine anything, as he had always been particularly unbelieving as to anything supernatural." *

In this case, the spontaneous picture—originating, as we hold, in the condition of the dying friend—was not more definite and vivid than that which the unexcited mind of the mere experimentalist has often been able to transmit. A very important point of difference does, no doubt, exist; for the spontaneous picture did not represent anything on which the mind of the agent was at the moment concentrated ; we cannot conceive him to have been gazing at his own face and form in a mirror. This point, however, may be postponed till we have completed our sketch of the graduated stages in the process of visualisation.

In the following examples, the vision was not of a single figure, but of a *scene*, vividly flashed upon the sense, and for the moment engrossing the attention, but still rather inward than outward, and not in any way confounded with the objective world, or located in the actual place where the percipient was at the sime.

* We cannot exclude this expression when quoting the words of our informants. We ourselves, of course, regard all these occurrences as strictly *natural*.

The first case is from Miss Henrietta Wilkinson, Enniscorthy, Ireland.

I live in Ireland, my nephew in London. At the end of October or beginning of November, 1881, when he was eight years old, he went one day with his mother and sister to Kensington Gardens. While playing there he had a severe fall on his back ; his mother had to call a cab and take him home, then send for the doctor. He was very ill for three or four days, lying in a dark room and kept perfectly quiet. The accident happened on a Saturday, I think. On the Sunday his mother wrote to tell me of it, which letter I received on Tuesday. On the Monday night I was in bed, dropping off to sleep, when I opened my eyes with a start, and saw quite distinctly a London street, leading from Kensington Gardens to my nephew's home. All the people, cabs, and horses were running very fast in one direction, towards my sister's house. Amongst them were my sister and her two children, also running. They stopped a cab, got in, and arrived at their own house. I saw no more, but exclaimed, " Maurice is hurt !" why, I do not know, as my nephew looked all right in the street. It all seemed to come from outside myself. I thought it very strange, and told it to my family next morning, before my sister's letter arrived. I am not perfectly sure of the day of the week, but know it was the day after the accident my sister wrote, and that it was the night of the day after she wrote that I saw what I tell you.

I think it was my nephew's thoughts of me that gave me the vision, I being the person he would think of, next to his father and mother.

Asked whether she had ever, on any other occasion, had a dream of death or accident which had impressed her, she says :—

No, I remember none. It was quite unique. But why call it a dream, when I was wide awake ? Had it been a dream I don't think it would have made the same impression on me.

The following corroboration is from Miss Wilkinson's sister, Castle Hill, Enniscorthy.

January 8, 1884.

I distinctly remember my sister relating to us (myself and another sister) her vision or dream *before* she got any letter. It made a great impression on her, and she told us with surprise and a little alarm. She told us on Tuesday morning, and the letter telling of the accident arrived soon after.

MARTHA WILKINSON.

The next account was sent to us by the Rev. A. Shaw Page, Vicar of Selsley, Stonehouse, Gloucester, in the words of his sister, Miss Millicent Anne Page. We have slightly shortened it.

I was staying with my mother's cousin, Mrs. Elizabeth Broughton, wife of Mr. Edward Broughton, Edinburgh, and daughter of the late Colonel Blanckley, in the year 1844, and she told me the following strange story :—

She woke one night and roused her husband, telling him that something dreadful had happened in France. He begged her to go to sleep again and

not to trouble him. She assured him she was not asleep when she saw what she insisted on then telling him—what she saw, in fact. First a carriage accident, which she did not actually see, but what she saw was the result, a broken carriage, a crowd collected, a figure gently raised and carried into the nearest house, and then a figure lying on a bed, which she then recognised as the Duke of Orleans. Gradually friends collecting round the bed, among them several members of the French Royal family—the Queen, then the King—all silently, tearfully watching the evidently dying Duke. One man (she could see his back, but did not know who he was) was a doctor. He stood bending over the Duke, feeling his pulse, his watch in his other hand. And then all passed away : she saw no more. As soon as it was daylight she wrote down in her journal all she had seen. From that journal she read this to me. It was before the days of electric telegraph, and two or more days passed before the *Times* announced " The death of the Duke of Orleans." Visiting Paris a short time afterwards, she saw and recognised the place of the accident, and received the explanation of her impression. The doctor who attended the dying Duke was an old friend of hers ; and as he watched by the bed, his mind had been constantly occupied with her and her family. The reason of this was an extraordinary likeness—a likeness which had often led to amusing incidents—between several members of the Broughton family and members of the French Royal family who were present in the room. " I spoke of you and yours when I got home," said the doctor, "and thought of you many times that evening. The likeness between yourselves and the Royal family was, perhaps, never so strong as that day when they stood there in their sorrow, all so natural ; father, mother, brothers, sisters, watching the dying son and brother. Here was the link between us, you see."

We have placed these two " transferred impressions " together on account of their essential similarity, though the occasion was in one case but the tumble of a little boy in the park, in the other the tragic death of a " son of France." For in both cases, it will be observed, the scene was not flashed from mind to mind *at the moment of its occurrence*, but considerably later, though at a time when the agent's thoughts were deeply concentrated (as we *know* in one case and may *presume* in the other) on a mental renewal of the agitating scene, coupled with a thought of the very person to whose perception that scene was in fact transferred. This *deferment* of the impression is certainly not a point which any one would have invented in order to add to the marvel of a story. To the ordinary reader it would seem a mere confusion and weakening of the tale. But we need hardly say that to those who have grasped the conception of telepathy this very point is of the utmost interest and importance. It shows us one of the precise phenomena to which our actual experiments point—the translation from agent to percipient of a *represented* image with almost the distinctness of an actual sensation—as where a diagram which the agent is merely recalling to memory is transferred with pictorial vividness to the percipient's mind. *In the* Kensington Gardens story the very inaccuracy of the scene, as

represented to the percipient's mind, suggests the manner in which it has already been modified in the agent's memory. The confusion of people, cabs, and horses, "running very fast in one direction," strongly suggests the half-delirious recrudescence of the agitated scene in the mind of the little invalid.

We shall now give an example of a less unusual type, where there is more distinctly a transference of actual sensation. It has a resemblance to the experiments where the percipient is able to reproduce a diagram at which the agent is actually gazing; or, again, to our previously cited case, where Mrs. Severn felt the precise pain suffered by her husband at a distance, from an accidental blow on the mouth. The account was sent to us by the Rev. Canon Warburton, The Close, Winchester.

Somewhere about the year 1848 I went up from Oxford to stay a day or two with my brother, Acton Warburton, then a barrister living at 10, Fish Street, Lincoln's Inn. When I got to his chambers I found a note on the table apologising for his absence, and saying that he had gone to a dance somewhere in the West End, and intended to be home soon after 1 o'clock. Instead of going to bed, I dozed in an arm-chair, but started up wide awake exactly at 1, ejaculating "By Jove, he's down!" and seeing him coming out of a drawing-room into a brightly illuminated landing, catching his foot in the edge of the top stair, and falling headlong, just saving himself by his elbows and hands. (The house was one which I had never seen, nor did I know where it was.) Thinking very little of the matter I fell a-doze again for half an hour, and was awakened by my brother suddenly coming in and saying, "Oh, there you are! I have just had as narrow an escape of breaking my neck as I ever had in my life. Coming out of the ball-room, I caught my foot and tumbled full length down the stairs."

W. WARBURTON.

In a second letter Canon Warburton adds:—

My brother was hurrying home from his dance, with some little self-reproach in his mind for not having been at his chambers to receive his guest, so the chances are that he was thinking of me. The whole scene was vividly present to me at the moment, but I did not note particulars, any more than one would in real life. The general impression was of a narrow landing brilliantly illuminated, and I remember verifying the correctness of this by questions at the time.

This is my sole experience of the kind.

Here the actual scene, intensely realised in the moment of imminent peril, seems to have flashed itself from mind to mind with startling but evanescent distinctness. We may remark that these sudden and vivid impressions in a state between sleeping and waking (of which we have many examples) do not fairly fall under the category of *dreams*. Their analogue is rather to be found in the rare and curious *illusions hypnagogiques* of oncoming sleep, or in the occasional prolongation of dream-

images into the first waking moments—the difference lying, of course, in the fact that in our cases the scene observed is one which was actually passing elsewhere at the moment.

In the next stage of visualisation the percipient sees a face or figure projected or depicted, as it were, on some convenient surface—the image being thus truly externalised, but in an unreal and unsubstantial fashion, and in a bizarre relation to the real objects among which it appears. In this respect it might be compared to the "after-image" of the sun, or of some object that has been intently scrutinised through a microscope, which we involuntarily import into our view of the surrounding scene.

We will begin with an example taken from the "Memoirs of Georgiana, Lady Chatterton," by E. H. Dering (1878), pp. 100-102.

My mother had not been very well, but there was nothing alarming in her state. I was suffering from a bad cold, and went early to bed one night, after leaving her in the drawing-room in excellent spirits, and tolerably well. I slept unusually well, and when I awoke the moon was shining through the old casement brightly into the room. The white curtains of my bed were drawn to protect me from the draught that came through the large window, and on this curtain, as if depicted there, I saw the figure of my mother—the face deadly pale, with blood flowing on the bedclothes. For a moment I lay horror-stricken, and unable to move or cry out, till, thinking it might be a dream or a delusion, I raised myself up in bed, and touched the curtain. Still the appearance remained (although the curtain on which it was depicted moved to and fro when I touched it) as if reflected by a magic lantern. In great terrior I got up, and throwing on a cloak I rushed off through some rooms and a long passage to my mother's room. To my surprise, I saw from the further end of the passage that her door was open and a strong light coming from it across the passage. As she invariably locked her door when she went to bed, my fears were increased by the sight, and I ran on more quickly still, and entered her room. There she lay just as I had seen her on the curtain, pale as death and the sheet covered with blood, and two doctors standing by the bedside. She saw me at once and seemed delighted to see me, though too weak to speak or hold out her hand. "She has been very ill," said the doctor, "but she would not allow you to be called, lest your cold should be made worse. But I trust all danger is over now. . . . . The sight of you has decidedly done her much good." So she had been in danger, and would not disturb me! Oh! how thankful I felt to the vision or fancy, or whatever it may have been.

It will be seen that the picture, though not producing the impression of a solid and independent object, was still no mere illusion, no mere momentary translation of the folds or pattern of the drapery into a human face. It was accurate and persistent enough to resist a touch which shook the curtain on which it was shown.

The next case carries us perhaps a step further still, as the image *appeared* with somewhat more of apparent relief—though certainly not

yet as co-ordinate in any natural fashion with the other objects in the percipient's field of vision. We received the account from Mr. Richard Searle, Barrister, Home Lodge, Herne Hill, who tells us that it was his sole experience of a hallucination.

One afternoon, a few years ago, I was sitting in my chambers in the Temple, working at some papers. My desk is between the fireplace and one of the windows, the window being two or three yards on the left side of my chair, and looking out into the Temple. Suddenly I became aware that I was looking at the bottom window-pane, which was about on a level with my eys, and there I saw the figure of the head and face of my wife, in a reclining position, with the eyes closed and the face quite white and bloodless, as if she were dead.

I pulled myself together, and got up and looked out of the window, where I saw nothing but the houses opposite, and I came to the conclusion that I had been drowsy and had fallen asleep; and after taking a few turns about the room to rouse myself, I sat down again to my work and thought no more of the matter.

I went home at my usual time that evening, and whilst my wife and I were at dinner she told me that she had lunched with a friend who lived in Gloucester Gardens, and that she had taken with her a little child, one of her nieces, who was staying with us; but during lunch, or just after it, the child had a fall and slightly cut her face so that the blood came. After telling the story, my wife added that she was so alarmed when she saw the blood on the child's face that she had fainted. What I had seen in the window then occurred to my mind, and I asked her what time it was when this happened. She said, as far as she remembered, it must have been a few minutes after 2 o'clock. This was the time, as nearly as I could calculate, not having looked at my watch, when I saw the figure in the window-pane.

I have only to add that this is the only occasion on which I have known my wife to have had a fainting fit. She was in bad health at the time, and I did not mention to her what I had seen until a few days afterwards, when she had become stronger. I mentioned the occurrence to several of my friends at the time. R. S.

November 2nd, 1883.

Mr. Paul Pierrard, at whose residence, 27, Gloucester Gardens, W., Mrs. Searle fainted, tells us that the cause of her doing so was the sight of an accident which befel her little niece. He also describes hearing from Mr. Searle, on the next day, that at the precise time of the fainting "a peculiar feeling overcame him, and he distinctly saw—as it were in a looking-glass—the very image of his wife leaning back in a swoon."

The last two narratives are specially noteworthy. When it first became evident to us that a number of strange heterogeneous narratives might be explained and connected by supposing them to represent the various *stages of externalisation* of a telepathic impact in the percipient's mind, we were quite ignorant of the existence of such cases as those of Lady Chatterton and Mr. Searle. Our chain of argument seemed fairly

complete without them. We should have gone on from scenes flashed before the mind to phantoms visualised "out in the room," with a sense that there was no real interruption of continuity, although the step was a long one to make on such uncertain ground. The moment, however, that these *externalised pictures* are described, it becomes plain that they supply exactly the connecting link, the want of which was vaguely felt. The picture on the window-pane or the bed-curtain comes precisely midway between the *mental image* and the *apparently solid figure.* It represents (in our language) a telepathic impression which has been externalised, but not yet completely objectified; which presents itself as something at which the percipient gazes, but which yet is not "taken for real," or localised in three dimensions among the familiar objects around him. And as compared with the two equally crude views between which we steer—that phantoms are all morbid nonsense, or that they are all "the spirits of the dead"—we think that our explanation is strongly supported by such intermediate cases as these. Our aim is to trace the connection between the most trivial phenomena of thought-transference, or confused inklings of disaster, and the full-blown "apparition" of popular belief. And, once on the track, we find group after group of transitional experiences, illustrating the degrees by which a stimulus, falling or fallen from afar upon some obscure sub-conscious region of the percipient's mind, may seem to disengage itself from his subjectivity, and to emerge into the waking world.

And now we come to the final class of cases, where the percipent sees the phantasmal figure as an apparently solid object among the familiar objects which surround him, and holding to those objects just such a relation as a figure of flesh and blood might have held. We received the following example from Mr. George Marchant, Linkfield Street, Redhill, formerly a large farmer and miller, and now an admirable specimen of shrewd and vigorous old age.

About 2 o'clock on the morning of the 21st of October, 1881, while I was perfectly wide awake, and looking at a lamp burning on my washhand-stand, a person, as I thought, came into the room by mistake, and stopped, looking into the looking-glass on the table. It soon occurred to me it represented Robinson Kelsey, by his dress and wearing his hair long behind. When I raised myself up in bed and called out, it instantly disappeared. The next day I mentioned to some of my friends how strange it was. So thoroughly convinced was I, that I searched the local papers that day (Saturday) and the following Tuesday, believing his death would be in one of them. On the following Wednesday a man, who formerly was my drover, came and told me Robinson Kelsey was dead. Anxious to know at what time he died, I wrote to Mr. Wood, the family undertaker at Lingfield; he learnt from the brother-in-law* of the deceased that he died at 2 a.m. He was my first cousin, and

* This brother-in-law has kindly confirmed the accuracy of the above *dat*, but has now forgotten what was the *hour* of death, not having been actually present.

was apprenticed formerly to me as a miller ; afterwards he lived with me as journeyman ; altogether, eight years. I never saw anything approaching that before. I am 72 years old, and never feel nervous ; I am not afraid of the dead or their spirits. I hand you a rough plan of the bedroom, &c.

To our inquiries as to whether Robinson Kelsey had been in his mind, and on various other points, Mr. Marchant replies :—

I had not been thinking about him, neither had I spoken to him for 20 years. In the morning after seeing the apparition, I spoke about it to a person in the house. In the evening I again spoke about it to two persons, how strange it was. It was several days after our conversation about what I had seen that I heard of his death. These people will confirm my statement, for after I heard of the death I spoke of it to the same people, that my relation died the same night as I saw the apparition. As the apparition passed between my bed and the lamp I had a full view of it ; it was unmistakable. When it stopped looking in the glass I spoke to it, then it gently sank away downwards.

We have received the following confirmation of this incident —

We are positive of hearing Mr. Marchant one day say that he saw the apparition of Robinson Kelsey during the previous night.

ANN LANGERIDGE,* Linkfield Street, Redhill.
MATILDA FULLER, Station Road, Redhill.
WILLIAM MILES, Station Road, Redhill.

Mr. Marchant has never had any other "hallucination," and laughs at the very idea of such things. In a personal interview he entered further into detail, pointing out *in situ* the exact line that the figure took, and how it momentarily hid the lamp in passing in front of the washhand-stand. He describes Kelsey's long and bushy back-hair as a very distinct peculiarity ; and he thinks that the figure was visible for nearly a minute.

Here, then, at last, we have the orthodox *apparition.* And we note at once that the completeness of the externalisation is not the only point in which this phantasm differs from the preceding ones. It is more durable, and it is apparently more independent. Reading the account of it, one cannot resist the question, "Should I have seen it, had I been there?" And the question cannot but lead on to another. *If* the apparition could have been seen by more than one person, what will be the effect on our theory of the transference of an impression from mind to mind? Can we conceive of this rare telepathic sympathy as affecting *two* minds at the same moment and in the same way? or are we driven

* One of us has visited Mrs. Langeridge, who is a sensible person, with very little belief in "ghosts." She at once volunteered the remark that Mr. Marchant described his vision to her next morning.

to assume some independent agency, operating quite outside the mind of either percipient?

Up to this point it will be observed, this fundamental problem has not presented itself. The phantasms with which we have thus far been dealing have not been such as to force on us the question whether two or more percipients would be likely to share them. But in the case of the completer or more objective phantasms, we have no longer any assurance that they are perceptible to one person only, unless we have actual evidence that other persons were present at the time and failed to perceive them.

Now, as a matter of fact, each variety of these *fully externalised* phantasms is liable (as we find from numerous instances) to be perceived by anyone who happens to be present. This phenomenon of *collective percipience*, of a sight seen or a sound heard* by several persons at once, can be shown, we think, to be not inconsistent with the substantial truth of our theory; but the problem is a formidable one, and we cannot here do more than indicate its existence.

Meantime, the very fact that we have been able to arrange the phenomena in a graduated series must be admitted to be strongly suggestive of a common origin for them all; and we shall endeavour to treat the solid-looking figure of Robinson Kelsey, no less than the fleeting vision "in the mind's eye" of Mr. Rawlinson, as in some way the product of the percipient's own mind, projected (so to speak) under the stimulus of an impact from the mind of the dying friend. But this explanation is something more than a natural conjecture: we have, as we stated above, a certain amount of *experimental* support even for the extreme cases where the apparition is externalised in the most complete way. We should hardly have ventured to make so positive an assertion on the ground of previously recorded cases; as those cases are few in number, and their correctness cannot now be tested in detail. But we cannot doubt the genuineness of the case which we published in the Proceedings of the Society for Psychical Research (Vol. I. p. 120), where a friend of our own, without having given the slightest hint of his intention, concentrated his mind for some minutes on the idea of appearing to two distant friends, in no way subject to hallucinations; who volunteered the information, when next he visited them, that they had distinctly seen him in their room at precisely that time.† Such a

* It would be cumbrous to introduce at every turn the words necessary for extending what is said about vision to impressions of *hearing* and *touch*. The reader will have no difficulty in perceiving where the application to these further senses is possible, and is to be taken as understood.

† We cannot ignore the strength of the contemporary testimony for the *occurrence* of similar events in the East; but the remoteness of the locality and *other difficulties* prevent us from here dwelling on them.

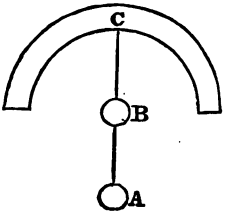
result is specially instructive in connection with more than one of the spontaneous cases quoted above. We noted, in passing, how Mr. Rawlinson's case differed from the more ordinary forms of experimental Thought-transference, in that the agent was not directing his attention to that which appeared to the percipient. The same remark applies to Mr. Searle's case; and in Lady Chatterton's case, though a portion of what she herself saw depicted—namely, the effusion of blood—was doubtless prominent at the moment in her mother's mind, the mother's own face and aspect can hardly have had any conscious place there. What, then, are we to conceive to have been in the agent's mind in these instances? It cannot be unreasonable to suppose (as Canon Warburton supposes in the case of his brother) that part of its content, at any rate, was a forcible idea of the percipient and of the agent's self in relation to the percipient. Lady Chatterton's mother, it is clearly implied, was thinking of her daughter, and in the other two cases we should naturally imagine a similar though more transient occupation of the agent's mind with the absent husband or friend; for it has often been noted that, in the sense of helplessness and collapse that immediately precedes fainting or death, the idea of distant scenes and persons is apt to recur in very vivid flashes. And if this be granted, the parallelism with our friend's experimental case becomes very marked. For the idea in *his* mind was of himself—not his *aspect* particularly, but his personality—in relation to the percipients; while the impression in *their* minds was of his *aspect*.

Mr. Marchant's case (which is of a common type) does, however, undoubtedly carry us beyond this analogy. We cannot pronounce it impossible that Kelsey's dying thoughts reverted strongly to his old employer; but, considering the length of time during which they had held no intercourse, we should prefer to suppose that the pre-existent and latent *rapport* between the two sufficed to effect the transference of the image, without any conscious direction of the agent's attention. We can hardly hope, however, that this hypothesis will look plausible, until the *rationale* of the projection of the image has been more fully considered. To this, then—the necessary supplement or expansion of our telepathic theory—we may at once proceed.

Let us first clearly realise the facts. Something is presented as apparently an independent piece of matter in the material world; but no piece of matter is really there, and the appearance is a phantasm. How would such an experience be most naturally described? Surely as a *hallucination* of the sense of sight—the creation of a mind which is in some abnormal state. This is precisely what we hold it to be; the abnormal state being, however, not—as in the case of morbid hallucinations—a mere pathological condition of the percipient, but a peculiar form of disturbance produced by an unusual condition in some

distant person. In virtue of their having their real cause *outside* the percipient, and so in a way conveying true information, we may describe death-wraiths and the like as *veridical* hallucinations; but as projections of the percipient's own mind, by which his senses are deluded, we hold them to be altogether on a par with morbid hallucinations. They are thus, merely a species in a larger genus; and our most hopeful course will, therefore, be to trace the natural history of the genus—to decide what a "hallucination of the senses" really involves. It is inevitable that this endeavour should carry us for a brief space into the region of physiology, but the accompanying diagram will enable the least instructed reader to master at a glance all the technical information required. We accept the commonly accepted doctrine as to the localisation of brain functions; but the general tenor of our explanation might, we think, hold good, even if that doctrine came to be modified.

Let A represent the retina of the eye which in itself has no more power of seeing than a mirror has. Let B represent the group of cells in the brain which constitutes the "visualising centre," and which is excited into activity whenever sight takes place. And let C represent the cortical or external part of the hemispheres of the brain, part of which is excited into activity whenever any of the higher psychical faculties—intelligent perception, imagination, comparison, memory, volition—are called into play. A is connected with B by the fibres of the optic nerve, and B is connected with C by other nerve-fibres. Now *any* disturbance of the cells at B which reaches a certain intensity will be accompanied by the sense of sight; and, when this disturbance is propagated onwards in the natural course from B to C, this sense will become a complete perception—an object for the mind—which can be reflected on, compared with other objects, and remembered. But the central, indispensable fact—the disturbance at B—may itself originate in at least *two* quite distinct ways. On the one hand it may originate in a nervous impulse *sent up* along the fibres from A, owing to some change which has taken place at A—whether it be a blow on the eye, which makes us see sparks, or the stimulus of external rays of light, which makes us see surrounding objects. Or, on the other hand, it may originate in a nervous impulse *sent down* along the fibres from C, owing to some change that has taken place at C; and then we shall have a hallucination—a sense of seeing, and of seeing, it may be, with great vividness and completeness, though what is seen has no existence external to the percipient's organism. This may happen either voluntarily or involuntarily. Painters have sometimes imagined a face or a scene with such force as actually to externalise it in space and see it



before their eyes. But far more often the hallucination is involuntary, as in purely morbid cases, in dreams, and in the species of apparitions with which we are dealing in this paper.

Now, so far, the matter seems plain enough. The particular cells at B, whose activity is necessary for the sense of sight, may be stimulated or exploded either by an impulse from *without*, started by light, in which case we see objects that are really there; or by an impulse from *within*, started by some spontaneous cerebral change, in which case we see objects that are not really there. But with regard to the latter case there is just one point that needs careful notice. We are supposing that the impulse which results in a hallucination is started by some change *in the cells* at *C*. Now what mental event does this physical change at C imply? Clearly not the *sight* of the object, for that only takes places in association with the physical change *in the cells at B*. The mental event associated with the change at C is not the *sight*, but the *idea*, of the object, as it might present itself in imagination or memory. A certain low degree of visualisation is, no doubt, involved in the very idea, and probably implies a slight downward escape of current from C to B; that is to say, the sluice-gates are never entirely closed. But the idea only becomes *completely* visualised—only becomes a *precept*, so as to suggest the real external presence of the object—when the downward impulse is of a far stronger kind, and produces as large a change at B as an upward impulse from the retina would do—the whole complex process being, however, for consciousness, a single and instantaneous event. And this being so, we are at once able to assign to the different parts of the process their respective characters. The *origin* of the hallucination is no doubt in the imagination—that is, in physical terms, at C; but it is not the imagination—or C as the physical organ of the imagination—that is to *blame* for the hallucination. There is nothing *delusive* in the mere idea or memory of a visible object; and in serving as a basis to such an idea or memory, however vivid, the cells at C are merely performing their normal functions. The *blame* attaches to the escape of a strong nervous current *in the downward direction* from C to B, in the rending of the sluice-gates (so to speak) at the point where the line drawn from B touches the semicircle. This is the abnormal event. If it did not take place, there would be no delusion, for there would be a mere idea or memory of the object. When once it has taken place, delusion is inevitable; for instantaneously a strong stimulus is communicated to the cells at B, and such a stimulus involves the sense of *seeing* the object. It may well be only the *sense*, and not the *judgment*, that is deluded; many persons who have suffered from hallucinations have perfectly well known that the figures seen by them were only phantasms. But for all that they *saw* them: to the sense of sight the figures were completely real—so

much so that the visual sensation was even referred to the external eye and the figure could be made to appear double by squinting, or by pressure on the side of one eyeball.

The physical details of the process which we have called the "rending of the sluice-gates," and the "escape of current from C to B," will perhaps never be known. Certain general conditions that favour the process are indeed recognised: hallucinations of the senses are common events in sleep, in insanity, and in the delirium of fever; and they may be produced by the immoderate use of various drugs, such as haschish and opium. But even in these cases the broad fact is almost all that is known. Of the actual physiological process we can say no more than that in insanity it is part of the general cerebral derangement, and is due in the other cases to some change in the constitution or distribution of the blood. But there is one important class of sensory hallucinations of which even thus much cannot be said—a class of which singularly little notice has been taken by scientific writers—namely, the quite transient and casual hallucinations of sane and healthy persons. Though rare, in the sense that only a small percentage of the population have had experience of them, these occurrences are absolutely numerous. Scores of them take place in England in the course of a year. But what determines their occurrence to this or that person, at this or that moment, we are often quite unable to say. Now in this state of ignorance there certainly need be no difficulty in supposing that *one* means by which the "escape of current" from C to B may be determined is a *telepathic impulse*. Supposing the evidence for the telepathic production of hallucinations—*e.g.*, for apparitions at the time of death—to be in itself sufficient, physiology need make no difficulties about the process in the percipient's brain, for in its general outline that process is just as intelligible to us, and in its minute details just as obscure, as in any other case of hallucination. Taking a broad view of the matter, we may even say that the difficulties are *less* in the case of death-wraiths than in cases where the hallucination is purely casual, and depends on no assignable conditions at all. For we at any rate succeed in connecting the *particular rare effect*—the peculiar "escape of current," and the consequent occurrence of a hallucination to a sane and healthy person—with a *particular rare cause*—the peculiar condition of a distant friend.

This, happily, is all that need be said in the way of physical explanation. For we have now got what we wanted—a ready way of connecting apparitions with the simpler forms of Thought-transference, even where (as in Mr. Marchant's case) the connection might at first seem most difficult to establish. How, we asked above, could the idea of Kelsey be impressed on Mr. Marchant's mind with such force as to embody itself in a visible phantom, when Kelsey's mind was presumably not occupied either with Mr. Marchant or with himself in relation to

him? From a physiological point of view the difficulty vanishes, on the supposition that the primary effect on Mr. Marchant took place not at B but at C; at the part of the brain which is the great storehouse of old impressions; in the part, moreover, where an appropriate physical basis may be found not only for distinct and recognisable images, but for subconscious ideas and memories, and for the most distant and intangible associations. In the register of the brain it is seldom that a record, once made, is so utterly obliterated that, under suitable conditions, it may not be revived. And if once a relation be established between two persons, and the records of it registered in their two brains, we see no reason why the same harmony should not occasionally manifest itself between those records—even though they be long sunk below the level of conscious attention—as between the *immediate* impressions of the moment; and, this once granted, we have seen how the physiological process may lead on to the projection of the visible phantom. In psychical terms, we see no reason why subconscious ideas and memories which are in no distinct way present to consciousness, such as Kelsey's sense of his old relationship to Mr. Marchant, should not evoke similar blind movements in Mr. Marchant's mind, which, gathering strength, might lead him to body forth the vision of his old acquaintance.* On this view it would become quite intelligible that he should *see* the figure even before he *recognised* it. And in a similar way we should interpret some at least of those cases of death-wraith (of which we have a good many specimens) where the figure seems to form by a *gradual* process. We should say that there the *idea*, at first but dimly conveyed and vaguely apprehended, was working itself into definiteness (as so often occurs in processes of abstract thought), and that the character of the projection underwent a corresponding change.

But here we find ourselves fairly launched on a very wide and interesting question—namely, how far the primary idea may be modified, wrought on, or worked out, in the percipient's own mind, before it becomes embodied as a visible phantom. That mind is no mere collection

* It might seem out of the question to obtain any *experimental* support for a transference of impression apart from consciousness on the "agent's" part. Yet support of a kind has actually presented itself. We requested our friend above mentioned secretly to determine, before going to sleep at about 10.30 p.m., that his form should appear at *midnight*—that is, at a moment when he would be actually asleep—to one of the persons whom he had before succeeded in affecting, and whom he had not even seen for some time. On the 22nd of March he did so, determining not only to appear but to touch his friend's head. The result is thus described by the latter:—"On Saturday night, 22nd of March, 1884, I had a distinct impression that Mr. B. was in my room. I distinctly saw him, whilst widely awake. He came towards me and touched my head." Since this Report was written, we have received an account, from another "agent" and "percipient," of a similar successful experiment.

of separate compartments, into which new ideas will fit and then rest in a passive way; but an organism of interacting parts, where any change or any intruding element may set in motion whole trains of images and associations. We know what small and dim suggestions will sometimes set large tracts of mental machinery to work; and we may therefore well credit the vaguer or subconscious order of telepathic impressions with such a power. Now if a visible phantasm results, in the manner above suggested, what more natural than that these further images and associations should be embodied in it? We may compare the process to what takes place in the one form of hallucination with which we are all familiar—in dreaming. A strong impression, whether received before sleep (as from a recent sorrow or a piece of exciting work) or during sleep (as from a knock at the door or an uncomfortable posture), will mingle itself in dreams with all sorts of scenes and ideas that the sleeping mind supplies. Now we would suggest that the mind, *even in a waking state*, may unconsciously react, as in a dream, on the crude material presented to it, may invest the nucleus of a "transferred impression" with its own atmosphere and imagery; nay, even that the apparent externality of the phantasm—the very fact in virtue of which we call the *impression* a *hallucination*—may itself be merely a radical instance of such investiture. We should thus have a ready explanation for many degrees of distinctness and individualisation, and many diversities of character, in the sensory phantasm. Suppose the same kind of real event—say the peaceful death of an aged parent—to occur in twenty cases, and in each of them to produce a real and unique sort of disturbance in some absent person's mind; then, if that disturbance clothed itself in some *sensory* form—or, in our language, if it reached the point of causing a *hallucination*—such hallucination might take twenty different forms. One percipient may hear his parent's voice; another may imagine the touch of his hand upon his head; a third may see him in his wonted dress and aspect; a fourth may see him in his dying aspect: a fifth may see him in some transfigured aspect; and others may invest the disturbing idea with every sort of visible symbolism, derived from their minds' habitual furniture and their wonted trains of thought.

Striking narratives of visible phantasms thus mingled with a dream element do in fact exist. In the case of sailors, for instance, the conditions of whose lives are so different from those of invalids on land, the death-wraith often seems to "suffer a sea-change," and to reflect in fantastic wildness the perils of the deep. Such is a narrative (sent to us by Engineer Dunlop, of Bangkok, Siam) of an apparition seen "when the ship was under all plain sail off the pitch of Cape Horn," when the seaman who had "started aloft to bend the fore-top-gallant flung his arms round the top-gallant shrouds and held on without

moving . . . till he was lowered on deck in the bight of a bowline." For as he "kept looking to the windward at the squall, suddenly in the midst of it he saw his sweetheart dressed in white flowing robes, who came flying down toward him before the wind," and who, as it afterwards proved, had died in England at that very time.

We cite this last narrative, not as itself evidentially strong, but on account of its theoretical interest as illustrating (among others more directly attested) the dream-like mode in which the telepathic impression may take shape from the scene around; and "the wet sea-boy, in an hour so rude," may transfer to "cradle of the rude imperious surge" the figure whose life is ebbing in the quiet bed at home. For we hold it legitimate to use in this purely *illustrative* manner many narratives on which we should hesitate to base our argument if they stood alone. When we review the 600 cases which (exclusive of dreams) we have already printed as material for our book on "Phantasms of the Living" alone—cases of which a large proportion come first-hand from persons known to us—we can hardly doubt that sooner or later the general fact of these distant impressions will be accepted by the majority of candid minds. Our evidence is eminently *cumulative*; but until its cumulation can be shown at full length, we must aim rather at showing its *coherence;* at indicating the way in which intermediate instances and cross analogies interlock even the most apparently isolated and grotesque of *bonâ fide* narratives of this kind.

Another group of cases in which the percipient's mind seems to modify the impression received is that where the phantasmal imagery is drawn from the appurtenances of death, or the accepted beliefs as to resurrection. Of course symbolism of this kind, which is common to the agent's mind as well as to the percipient's, affords no positive proof as to whose mind it is from which the phantom derives its shape and consistency. But the *least marvellous* explanation—the nearest to experimental analogy—will be that which assumes that it is the percipient's own activity which invests the transferred impression with its clothing of imagery. We have space for only two instances of this class. The first was given to us by Lieut.-Colonel Jones, 8, Sussex Place, N.W., who confirmed the narrative to us verbally, and showed us a letter written at the time in which his father alludes to the apparition. Colonel Jones has never experienced any other hallucination whatever; and it seems to us that this case alone would severely try the theory which explains all such phantoms as this by mere chance-coincidence.

In 1845 I was stationed with my regiment at Moulmein, in Burmah. In those days there was no direct mail, and we were dependent upon the arrival of sailing vessels for our letters, which sometimes arrived in batches, and occasionally were months without any news from home.

On the evening of the 24th of March, 1845, I was, with others, dining at a friend's house, and when sitting in the verandah after dinner, with the other guests, in the middle of a conversation on some local affairs, I all at once *distinctly* saw before me the form of an open coffin, with a favourite sister of mine, then at home, lying in it apparently dead. I naturally ceased talking, and every one looked at me with astonishment, and asked what was the matter. I mentioned, in a laughing manner, what I had seen, and it was looked upon as a joke. I walked home later with an officer very much my senior (the late Major-General George Briggs, retired, Madras Artillery, then Captain Briggs), who renewed the subject, and asked whether I had received any news as to my sister's illness. I said no, and that my last letters from home were dated some three months prior. He asked me to make a note of the circumstance, as he had before heard of such occurrences. I did so, and showed him the entry I made opposite the day of the month in an almanack. On the 17th of May following I received a letter from home announcing my sister's death as having taken place on that very day—viz., the 24th of March, 1845.

As to the coincidence of *hour*, Colonel Jones only learnt that the death occurred in the morning of the 24th. His vision was seen after an early dinner, so that, allowing for longitude, the correspondence of time was certainly near, and *may* have been exact. There had been a very close attachment between sister and brother.

The next case is from our friend, Miss Summerbell, 140, Kensington Park Road, W., who has never had any other hallucination.

I have been, for many years, on terms of close intimacy with the family of a Dutch nobleman, who reside in Holland. Early in July last I received a letter from the eldest daughter of the house saying that her father was seriously ill. From that time I received news of his condition every day. On the 27th of July, 1882, I received a postcard saying that he was slightly better. I was staying at the time at the Spa, Tunbridge Wells, and suffering much from neuralgia. On the night of the 27th I was lying, unable to sleep from pain; no doubt I dozed now and then, but I firmly believe that I was awake when what I am about to relate occurred. It was beginning to be light, and I distinctly saw every object in the room. I do not know whether it is necessary to say that in Holland, when a person of distinction dies, a *prieur d'enterrement* is employed. This man is dressed in black, with dress coat, knee breeches, and cocked hat, with bands of crape hanging from the corners. It is his office to go to all the houses where the deceased was known and announce the death. On the morning of which I speak, I saw the door of my room open and a *prieur d'enterrement* enter. He said nothing, but stood with a long paper in his hand. I remember distinctly wondering whether I had fallen asleep and was dreaming; I looked round and saw the furniture, and the window, with the dim light coming through the closed blind. I looked at my watch, it was nearly 5 o'clock. I looked towards the man, but he was gone. It was nearly six years since I had *lived* for any time in Holland, and I had forgotten the custom of announcing

deaths; at least, I had not thought of it for years. But on that morning, at 3.20, my friend died.

I afterwards questioned my friend, Mme. Huydecoper, about what happened at the time of her husband's death, and I find that the first thing they did was to discuss how they should send the tidings to me. I told the friend with whom I was staying at the time what I had seen; she spoke of it several times during the day, and reminded me of it when the news of the death arrived.

Here the telepathic impression, instead of connecting itself with some familiar image, such as a coffin, seems to have evoked slumbering memories which associated Dutch customs with Dutch friends in the percipient's mind.*

Whatever be the view taken of each separate *symbolical* case of this kind, it is to be hoped that the collection of a great number of them may throw further light on the laws of association which govern these rare events—on the path and barometry of these psychical storms. There is, perhaps, scarcely any visual phantom of a dying person from which some hint of scientific value might not be drawn, were the figure carefully scrutinised in every detail. Unfortunately, this scrutiny is much less easy in the case of veridical than in the case of morbid hallucinations. Morbid hallucinations are much more often *durable*, and much more often *repeated*. Moreover—and we commend this point

* An analogous incident was narrated more than once by the late Dr. Pusey, in a way which led his hearers to believe that the experience was his own. Our friend, the Rev. G. B. Simeon, of Gainsborough, having undertaken to ask Dr. Pusey what truth there was in the rumour that he had seen an apparition in High Street, Oxford, Dr. Pusey replied that the report was probably founded on the following fact:—

Two clergymen, A. and B., great friends, were at a distance from one another. One afternoon, A., who was in his garden, saw the figure of B. approach, and heard him say, "I have been in hell for half-an-hour, because I loved the praise of men more than the praise of God." It turned out that B. had actually died suddenly on that day, shortly before the appearance in the garden.

We were not satisfied with this evidence, as Dr. Pusey did not actually affirm that A. was himself; and we have lately discovered what we think must be the original of the story, in an old copy of the *Imperial Magazine*. As, however, the main facts agree, and Dr. Pusey vouched for their truth, we may be allowed to refer to them for purposes of illustration. It is plain that, on our theory, the phantom's speech must be treated in the same way as the coffin and the *prieur d'enterrement*. Miss Jones was not actually lying in her coffin when she appeared to her brother in that position; nor, happily, is it necessary to assume that B.'s words were a transcript of literal reality. Just as in the one case it was the imagery of *death* which forced itself into prominence, so, in the other case, the conception of what *follows* death may have started into dream-like vividness when the telepathic impact from the dying friend arrived, and awoke, perhaps, a slumbering judgment on him *in foro conscientiæ*, which affection had long kept hidden in the unacknowledged background of thought.

to those who regard veridical hallucinations as merely transient morbid affections occurring *by chance* at the moment of death—morbid hallucinations seem, in a majority of cases, to be *unrecognised* faces and figures of *strangers*, and they thus excite curiosity rather than emotion. In a large majority of our cases, on the other hand, the percipient has recognised the phantom. It is thus to the *countenance* that they have for the most part looked. The dress and surroundings are not minutely observed, or it seems a mere irreverence to dwell on them in the presence of what is so sacred and beloved. Often, too, the phantom is merely momentary, and the result is that in very many instances we have only the vague description, "I saw my father as I was used to see him," "It was my mother, as she lived." In such cases there is nothing to guide us as to the origin of the phantasmal image. It is such as the percipient might most readily have shaped for himself; but it is such, also, as the agent might most naturally have transmitted, if such images are transmitted in their entirety and, so to speak, ready-made.

There is, however, one large and important class of death-wraiths whose peculiarity it is that the dress is, as it were, an integral part of them, forming an element in the apparition so conspicuous and unexpected as to attract a full share of attention. We mean the cases where the phantom appears in the dress, or with the aspect, which the dying man actually wore at death, and of which the percipient was not previously aware. Here it undoubtedly seems as though the agent had transmitted a finished picture of himself—an *imago*, as Lucretius would say, thrown off from his whole circumference—which needed no reinforcement from the percipient's shaping imagination before it stood complete and evident in the open day.

This reproduction of actual costume or actual aspect passes through all stages of distinctness and unexpectedness. We begin with the cases where the dying person is merely seen clad in *white*, as it were the vague reflection of the night-dress in which he lies; and we go on to instances where the dress worn at death is altogether strange and unusual, or where a violent end has inflicted recognisable scars or mutilation; or where the image reiterates with phantasmal urgency some task which the living mind regretfully leaves unaccomplished, as it sinks into the stupor of the dim death-day.

The first of these cases that we shall select is one which curiously illustrates the changes effected by half a century of oral tradition in a narrative of this kind. The occurrence in question, belonging to a well-known noble family and to a tragic accident, has been often cited in society when such topics are discussed, and we obtained a written account of it from a member of the family known to us—both he and we being ignorant that a contemporary record of the matter had already

been printed in "The Journal of Thomas Raikes, Esq., from 1831 to 1847" (London, 1856), Vol. I. p. 131. We are thus, as it happens, able to compare a traditional account 53 years old with an account written down 15 months after the occurrence :—

We give first our friend's account :—

My grandfather Sir J. Y. was drowned by the upsetting of a boat in the Solent, in or about the year 1830. On the day of his death Miss M., a great friend and connection of his, was at one of the Ancient Concerts in Hanover Square Rooms. During the performance she fainted away, and when she came to declared that she had seen a corpse lying at her feet, and though the face was turned away she knew the figure to be that of my grandfather. Communication in those days was not, of course, as easy as now, and her fears were not verified till some days after the event. Such is the family story, which I heard often from my father, and had verified by my mother when last I saw her.

Here, as will be seen, there is nothing to indicate either the coincidence of *hour* or the *dress* in which the phantom was seen. Let us now turn to the contemporary account, which has the two advantages of being half a century nearer to the facts and of coming from the side of the actual percipient, Miss Manningham, whose name we are now allowed to print for the first time. In "A Portion of the Journal kept by T. Raikes, Esq., from 1831 to 1847," Vol. I. p. 131, we read :—

Wednesday, 26th December, 1832.—Captain —— recounted a curious anecdote that happened in his own family. He told it in the following words :—It is now about 15 months ago that Miss M ——, a connection of my family, went with a party of friends to a concert at the Argyll Rooms. She appeared there to be suddenly seized with indisposition, and though she persisted for some time to struggle against what seemed a violent nervous affection, it became at last so oppressive, that they were obliged to send for their carriage and conduct her home. She was for a long while unwilling to say what was the cause of her indisposition ; but on being more earnestly questioned, she at length confessed that she had, immediately on arriving at the concert room, been terrified by a horrible vision which unceasingly presented itself to her sight. It seemed to her as though a naked corpse was lying on the floor at her feet ; the features of the face were partly covered by a cloth mantle, but enough was apparent to convince her that the body was that of Sir J—— Y——. Every effort was made by her friends at the time to tranquillise her mind by representing the folly of allowing such delusions to prey upon her spirits, and she thus retired to bed ; but on the following day the family received the tidings of Sir J—— Y—— having been drowned in Southampton River that very night by the oversetting of his boat, and the body was afterwards found entangled in a *boat-cloak*. Here is an authentic case of second sight, and of very recent date.

On a critical comparison, it will be seen that the *contemporary* or *old* account presents two small apparent discrepancies from the *tra-*

ditional or *new* account, and also (which is more surprising) contains two further circumstances of the highest interest. The discrepancies are these. The new account says that *Miss M.* did not receive the news for some days; the old account says that the *family* received it the next day. Now the journey from Southampton to London occupied only one day in 1830, and probably the *old* account is correct. *Miss M.*, however, may not have been informed for another day or two; so the *new* account may be correct also. The other discrepancy is in the name of the building where the vision was seen—the "Argyll Rooms" according to the old account, the "Hanover Square Rooms" according to the new. Now from Crickley's "Picture of London" (1831), p. 93, we learn that "the Argyll Rooms, Regent Street, burnt down in the early part of last year, have been again restored to their former splendour. They are devoted to concerts, balls, and exhibitions, and are much frequented by persons of rank and fashion." It also appears that the Hanover Square Rooms were open for concerts at the same time, so that either account might be correct. But as the title "Argyll Rooms" has long ceased to suggest a high-class concert hall, it is likely enough that it may have been unconsciously replaced in Lord and Lady——'s minds by the more apparently suitable appellation.

These trifling discrepancies in unimportant points are such as must needs accrue in 50 years' tradition. But it is a much more remarkable thing to find that in this case tradition, so far from *exaggerating*, has *minimised* the cardinal points of the story, It is in the *old*, not in the *new*, account that we hear that the accident took place "that very *night*," so that it is at any rate possible that the coincidence of *hour* was exact. Moreover—and this is the point with which we are here specially concerned—it is in the *old*, not in the *new*, account that we hear of the cloth mantle wrapped about the visionary form, the boat-cloak whose prototype was at that time entangling beneath the dark estuary the limbs of the drowning man. Note also in the old account the combination of dreamlike symbolism with reflex of actual fact. The corpse was seen as if naked, no part of the dress being visible except that one article whose significance was destined to be afterwards understood too well.

The comparison of these two narratives may serve to illustrate a generalisation which has gradually been suggested to us by many similar collocations of older and newer versions. Tradition, we find, when it deals in modern times with such accounts as these, tends primarily to *shorten* and *simplify* them. Sometimes this *simplification* may also involve *exaggeration* of the marvellous element. For instance, a coincidence of death and death-wraith, which was really not traced nearer than to the same *day*, will be sometimes repeated as if it had been proved to be also at the same *hour*. Or a figure *vaguely*

resembling a dying person will be represented as *precisely* resembling him. Often, again, this process of simplification is unimportant in its effect on the narrative; as when in the well-known Wynyard case, a man who *strikingly like* Wynyard (and whose *likeness* is the only important point) is represented as having been Wynyard's *twin brother*. But often, also (as in the case first given), this simplifying process tends to lose the very points on which we are most concerned to dwell—the undesigned indications which so often at once strengthen the evidence of the narrative and double its theoretic interest. If the narratives with which we are dealing were the offspring of chance and credulity, then the further we get them from their original sources the better, it might be thought, they would suit us. But if they are the offspring of law and fact, we shall expect—and the case is so—that the nearer to absolute accuracy, absolute contemporaneity, we can obtain them, the better will they fit in with other facts, and range themselves beneath general laws.

Our next case shall be a first-hand one, from a physician, Dr. Rowland Bowstead, of Caistor, who tells us that he has never experienced any other hallucination.

In September, 1847, I was playing at a cricket match, and took the place of long-field. A ball was driven in my direction which I ought to have caught but missed it, and it rolled towards a low hedge; I and another lad ran after it. When I got near the hedge I saw the apparition of my brother-in-law, who was much endeared to me, over the hedge, dressed in a shooting suit with a gun on his arm; he smiled and waved his hand at me. I called the attention of the other boy to it; but he did not see it, although he looked in the same direction. When I looked again the figure had vanished. I, feeling very sad at the time, went up to my uncle and told him of what I had seen; he took out his watch and noted the time, just 10 minutes to 1 o'clock. Two days after I received a letter from my father informing me of the death of my brother-in-law, which took place at 10 minutes to 1. His death was singular, for on that morning he said he was much better and thought he should be able to shoot again. Taking up his gun, he turned round to my father, asking him if he had sent for me, as he particularly wished to see me. My father replied the distance was too far and expense too great to send for me, it being over 100 miles. At this he put himself into a passion, and said he would see me in spite of them all, for he did not care for expense or distance. Suddenly a blood-vessel on his lungs burst, and he died at once. He was at the time dressed in a shooting suit and had his gun on his arm. I knew he was ill, but a letter from my father previous to the time I saw him told me he was improving and that he might get through the winter; but his disease was consumption, and he had bleeding from the lungs three months before his death. ROWLAND BOWSTEAD, M.D.

Here the dress is a very distinct one, not associated with invalids or death-beds, and reproduced with apparent exactness. The agent's

impression of his personality seems, in fact, to have carried with it the details of his actual aspect as well as the symbolism of his imagined farewell; and nothing was left to the percipient's imagination. It will be observed that the coincidence of time is close to a minute, and was noted on the spot. It would, we think, be difficult to express in figures the enormous unlikelihood of a merely morbid hallucination, unique in the percipient's experience, and involving by accident such coincidences as these.

Our final instance shall be one which illustrates the possibility of *collective* percipience. We received it from Miss K. M. Weld, of The Lodge, Lymington, one of the two original witnesses.*

Philip Weld was the youngest son of Mr. James Weld, of Archers Lodge, near Southampton, and a nephew of the late Cardinal Weld. (The chief seat of that ancient family is Lulworth Castle, in Dorsetshire.)

He was sent by his father, in 1842, to St. Edmund's College, near Ware, in Hertfordshire, for his education. He was a well conducted, amiable boy, and much beloved by his masters and fellow-students.

It happened that on April 16th, 1845, it was a playday (or whole holiday), and some of the boys went out on a boating expedition upon the river Ware.

On the morning of that day, Philip had been to Holy Communion at the early Mass (having just finished his retreat), and in the afternoon, accompanied by one of the masters and some of his companions, went to boat on the river, which was a sport he enjoyed much.

When one of the masters remarked that it was time to return to the college, Philip begged to have one row more; the master consented, and they rowed to the accustomed turning point.

On arriving there, in turning the boat, Philip accidentally fell out into a very deep part of the river, and, notwithstanding every effort that was made to save him, was drowned.

His corpse was brought back to the college, and the Very Rev. Dr. Cox (the president) was immensely shocked and grieved; he was very fond of Philip, but what was most dreadful to him was to have to break the sad news to the boy's parents. He scarcely knew what to do, whether to write by post or to send a messenger.

At last he made up his mind to go himself to Mr. Weld, at Southampton. He set off the same afternoon, and, passing through London, reached Southampton the next day, and drove from thence to Archers Lodge, the residence of Mr. Weld; but before entering the grounds he saw Mr. Weld at a short distance from his gate, walking towards the town.

Dr. Cox immediately stopped the carriage, alighted, and was about to address Mr. Weld, when he prevented him by saying:—

"You need not say one word, for I know that Philip is dead. Yesterday afternoon I was walking with my daughter, Katherine, and we suddenly saw him. He was standing on the path, on the opposite side of the turnpike road, between two persons, one of whom was a

* In the present issue, this case replaces one where further examination showed an apparently fundamental error in one of the dates.

youth dressed in a black robe. My daughter was the first to perceive them and exclaimed, 'Oh, papa! did you ever see anything so like Philip as that is?' 'Like him,' I answered, 'why it is him.' Strange to say, my daughter thought nothing of the circumstance, beyond that we had seen an extraordinary likeness of her brother. We walked on towards these three figures. Philip was looking, with a smiling, happy expression of countenance, at the young man in a black robe, who was shorter than himself. Suddenly they all seemed to me to have vanished; I saw nothing but a countryman, whom I had before seen *through* the three figures, which gave me the impression that they were spirits. I, however, said nothing to anyone, as I was fearful of alarming my wife. I looked out anxiously for the post the following morning. To my delight, no letter came. I forgot that letters from Ware came in the afternoon, and my fears were quieted, and I thought no more of the extraordinary circumstance until I saw you in the carriage outside my gate. Then everything returned to my mind, and I could not feel a doubt that you came to tell me of the death of my dear boy."

The reader may imagine how inexpressibly astonished Dr. Cox was at these words. He asked Mr. Weld if he had ever before seen the young man in the black robe at whom Philip was looking with such a happy smile. Mr. Weld answered that he had never before seen him, but that his countenance was so indelibly impressed on his mind that he was certain he should have known him at once anywhere.

Dr. Cox then related to the afflicted father all the circumstances of his son's death, which had taken place at the very hour in which he appeared to his father and sister; and they felt much consolation on account of the placid smile Mr. Weld had remarked on the countenance of his son, as it seemed to indicate that he had died in the grace of God and was, consequently, happy.

Mr. Weld went to the funeral of his son, and as he left the church, after the sad ceremony, looked round to see if any of the religious at all resembled the young man he had seen with Philip, but he could not trace the slightest likeness in any of them.

About four months after, he and his family paid a visit to his brother, Mr. George Weld, at Seagram Hall, in Lancashire.

One day he walked with his daughter Katherine to the neighbouring village of Chipping, and after attending a service at the church called on the priest.

It was a little time before the rev. father was at leisure to come to them, and they amused themselves meantime by examining the prints hanging on the walls on the room. Suddenly, Mr. Weld stopped before a picture which had no name, that you could see, written under it (as the frame covered the bottom), and exclaimed, "*That* is the person whom I saw with Philip; I do not know whose likeness this print is, but I am *certain* that it was *that* person whom I saw with Philip."

The priest entered the room a few moments afterwards, and was immediately questioned by Mr. Weld concerning the print.

He answered that it was a print of St. Stanislaus Kostka, and supposed *to be a very good likeness* of the young saint.

Mr. Weld was much moved at hearing this, for St. Stanislaus was a Jesuit, who died when quite young, and Mr. Weld's father having been a great benefactor of that Order, his family were supposed to be under the particular protection of the Jesuit saints; also, Philip had been led of late, by various circumstances, to a particular devotion to St. Stanislaus.

Moreover, St. Stanislaus is supposed to be the especial advocate of drowned men, as is mentioned in his life.

The rev. father instantly presented the picture to Mr. Weld, who, of course, received it with the greatest veneration, and kept it until his death.

His wife valued it equally, and at her death it passed into the possession of the daughter who saw the apparition at the same time he did, and it is now in her possession.

In answer to an inquiry as to whether she had ever had other hallucinations of the senses, Miss Weld replied that the above was a totally unique experience.*

We have no explanation of the third phantasmal figure; but the apparition of the second figure seems to lend itself with special facility to the telepathic hypothesis; since we can conceive that the idea of his favourite saint may have been actually present to the mind of the drowning man.

Our theory of Apparitions has, we hope, been now made sufficiently clear. It of course makes no claim to be exhaustive. Resting as it does on an experimental basis, we think that it ought to be pressed to the furthest possible point; but we are far from dogmatically asserting that *all* phantasmal indications of death are of identical nature, and that becuse Thought-transference explains some of them, it must needs explain all. Just as morbid phantasms may be variously produced by fever, by insanity, by opium, so also veridical phantasms may be ultimately traceable to more than one originating cause. But whatever further departures may hereafter be needful,

* The following reference to the same incident, from a physician residing at Florence, is useful as illustrating the slight inaccuracies which may creep into a narrative, without the least affecting the essential point:—

"I was mentioning this" (*i.e.*, a similar case) "to Baron F., or rather we were talking over the incidents connected with it, when he told me of a strange occurence which happened at the school were he was, near Ware, in England, a Catholic college,—president a Dr. Cox. There was a boy there of the name of Weld, a very well-known Catholic family. This boy was accidentally drowned. The father and mother were at the time at Southampton, and on the day in question were walking on the Quay near the shipping. They suddenly saw the said boy approaching, and hurried to meet him, but immediately he appeared to fade away, so that they could see the masts of the ships, and through what had seemed to be his body. The next day, or the day following, Dr. Cox called on them, when Mr. Weld said, "I know why you are here, it is to tell me that *my son is* dead; I saw him yesterday, and knew then that he had *departed.*"

we trust that the generalisations already made will continue valid. We trust that permanent acceptance will be accorded to the thesis that *some* of those hallucinations of one or more of the senses, which correspond with objective events at a distance, and which we therefore term *veridical*, are caused by a telepathic impact conveyed from the mind of an absent agent to the mind of the percipient, and rendering itself cognisible by the percipient's senses in various stages of externalisation, and with various admixtures of a dreamlike or symbolical element. We claim that this thesis possesses the *prima facie* characteristics of a true scientific generalisation. It is not contradictory of any previously established law; it has been vaguely foreshadowed by many earlier observers; it is more or less directly confirmed by two separate lines of actual experiment—those, namely, which indicate the existence of a similar reception of impressions in the *normal* and in the *hypnotic* state; and it adapts itself to facts recorded for centuries in all quarters of the globe, and forming a convergence of testimony without any possible concert or prearrangement. The theory will, we hope, be discussed, modified, and extended by many persons capable of dealing with it in all its bearings. Meantime, there is little to be learnt from the mere *à priori* negation with which it is sometimes met, and which would, without inquiry, explain all telepathic experiments by fraud, and all veridical hallucinations by mere chance-coincidence. This position of pure negation, indeed, becomes daily more difficult to maintain, as experiments are multiplied and death-wraiths recur. The force of *à priori* denunciation such as this lies mainly in its first confident expression, and in the amount of diffused prejudice which it can begin by arraying on its side. Time fights for inductive reasoners; and if (as we may fairly hope) an increasing body of informants continue to supply us with the first-hand evidence on which our induction depends, we may trust that each year will make our position stronger, and our own views more clear.

But we must find space before we close for one or two of the reflections to which the narratives cited in these papers naturally give rise—reflections which may jar, we fear, alternately on some religious and on some scientific prepossessions, but which may not be unacceptable to those who hold that these two modes of regarding the universe do but point from different sides to a higher, a reconciling unity.

In the first place, then, with all respect to those who in every age have held these death-wraiths as proof of a special Providence—intimations of the pitying indulgence of a beneficent Power—we must say that the evidence, as presented to us, does not seem to support the conclusion. Such indulgence would involve some distinct reference to the percipient's affections and emotions. But in Mr.

Marchant's case, for instance, there was no question of affection; and the only emotions that his experience caused him were surprise and curiosity. Can we suppose that his old *employé* was expressly permitted to overstep the lot of man, merely to manifest himself on an errand so bootless, and as a phantom so undesired? No, even in this hyperphysical region, and in the very vanguard of our advance upon the Unseen, we are forced to believe that *Dieu n'agit pas par des volontés particulières*—we are forced to surmise the presence of a law which, though obscure, is immutable; which is a factor in the fabric of things, and was not framed, nor is suspended, in the special interest of any one of us.

But, at the same time—and now we fear that a certain section of the scientific world may in their turn find our suggestions distasteful—the theory of telepathy does undoubtedly afford an unexpected support to a certain school of religious conceptions. For there are two very different theological views (often obscured by vagueness of language) as to the manner in which unseen powers exercise influence on the visible world. Some dogmatists have insisted that such influence is, in the strictest sense of the word, *miraculous*; that it involves a *suspension* of the laws of nature, an *interference* with the established course of things; and that, in fact, on such non-natural or miraculous character its sanctity and value depend. Now against this creed, Telepathy, like any other correlation under law of facts previously supposed to be arbitrary exceptions to law, does but accumulate one more presumption. But there have been other theologians, from Augustine to Archbishop Trench, who have formulated the claim of theology in a wiser way. Such men maintain that an influence is in truth exercised by the invisible on the visible world; but that it is exercised according to laws, which, though unknown to *us*, do in fact regulate and determine the action of higher intelligences, whose volition thus intervenes in human affairs in a fashion as strictly *conditioned* as any volitions of our own.

Now the evidence which we have been discussing certainly does not supply any direct confirmation of this view either. We have found no need to postulate the existence of any intelligences except human minds, and human minds, not in hell or heaven, but on earth as we know them. But, nevertheless, *if* other intelligent beings besides those visible to us do in fact exist—if man's own soul survives the tomb—then, no doubt, our telepathic experiments and our collected cases of apparitions, interpreted as we interpret them, do suggest analogies of influence, modes of operation, which (it is hardly too much to say) would throw a quite novel light over the long controversy between Science and Faith. It is only in some form of idealism that that controversy can find a close. And we are far too sensible of the problem

involved in the relation of our own will to the facts and forces of nature to meet any idealistic hypothesis of the relation of *other* wills to those facts and forces with a direct denial. We cannot call a hypothesis *unphilosophical*—however much *unproved*—if it introduces into the great problem no difficulty which is not already there, and is compatible —which the cruder theory of miracle is *not*—with the known facts of the universe, viewed in that connected manner which alone can give stability to thought.

But we shall do more than indicate this line of reflection. We have no wish to take wing as *chimæræ bombinantes in vacuo*—full-blown explainers of the universe—but rather to be accepted as hewers of wood and drawers of water in a territory which inductive science has yet to clear for her own. Nay, we have preferred to submit to the inconvenience of an arbitrary restriction of our subject, rather than to risk the dangers which might attend its further extension. Of apparitions *after death* we say nothing here; we choose rather to defer all discussion of such evidence as is alleged for them (though we receive and examine it) until we have learnt everything that it may be possible to learn of those *phantasms of the living* which do not tempt us among agencies so obscure and unknown. It is true that even of these incidents *death* is the central fact. It is in this profoundest shock which human life encounters that these phantasms are normally engendered; and, where not in death itself, at least in one of those special moments, whether of strong mental excitement or of bodily collapse, which of all living experiences come nearest to the great crisis of dissolution. Following the track not only of logical sequence but of imaginative interest, our evidence has carried us from the slightest to the gravest of human things, from the curiosities of an afternoon to the crises of a lifetime, from petty experiments and seemingly aimless mysteries up to the experience which there is no refusing, and into the heart of the supreme mystery which surrounds and overshadows us whether we speculate about it or no. But in the light of advancing knowledge that mystery may appear—if no less profound than ever—at any rate less appalling. We have drawn on no creeds; we have appealed to no "supernatural agencies;" but new facts cannot leave old facts exactly where they found them; and we have at any rate discovered in *death* the great and peculiar source of phenomena which —however we interpret them—are essentially *vital*. With this reflection we may pause on the threshold—*vestibulum ante ipsum primisque in faucibus Orci*—till our eyes, which still can look into daylight, have grown accustomed to the darkening air. Not here, indeed, any more than elsewhere, shall we find the "Elysian road" which will conduct man undoubtingly to such beliefs as his heart most craves. *Centauri in foribus stabulant.* There will, we doubt not, as discovery replaces

imagination, be found much that will startle, something that will alarm or repel. But in this age, if in any, it may surely be affirmed that "Truth, after all, is the prime passion of mankind"; and the audience, the fellow-workers, to whom we look are those who in these deep matters are weary alike of unproved dogma and of uninquiring negation; who have faith enough in the methods and in the future of science to feel confident that the same humble, candid, persistent collection and colligation of facts—without disdain of the smallest things or fear of the hardest—which in one century has so changed our outlook on the world, may be rewarded hereafter by the opening of horizons wider still,—by a more indisputable insight, a more assured penetration into the "chief concerns of man."

V.

PROCEEDINGS OF THE GENERAL MEETING ON

June 30, 1884.

The ninth General Meeting of the Society was held at the Garden Mansion, Queen Anne's Mansion, S.W., on Monday, June 30, 1884.

PROFESSOR BALFOUR STEWART, F.R.S., VICE-PRESIDENT, IN THE CHAIR.

The following address was delivered by the Chairman :—

Residing as I do at Manchester, I cannot refrain from adverting to the loss which our Society has suffered in the death of Dr. Angus Smith. It has never been my lot to meet with a man of greater simplicity and purity of character, or of greater love for truth. Whilst we all deeply mourn his loss, it is a source of satisfaction to think that the name of one so eminent in many ways should be found on the roll of our Society. It is likewise cause for much satisfaction that men of science of undisputed eminence have recently consented to join our ranks. And I have been much gratified by the unanimous verdict of scientific candour and honesty which a perusal of our memoirs has called forth from men who are, nevertheless, not disposed to join us at present.

Being myself engaged in physical science I should like to make a single remark on that part of our programme which refers to the production of peculiar physical phenomena. I know that the investigations in this direction, upon which several members of this Society are engaged, have not yet been developed sufficiently to be brought before us for discussion; but my remark is of a general nature, and can in no way prejudice that which is now going on.

Those who have discussed the subject of what I will call Free-will, may be divided into two classes or schools.

First. The Materialistic, embracing those who believe that all acts of will, all desires and aspirations of the Ego, are the results of certain material transformations in the brain, which transformations take place according to ordinarily-understood physical laws.

Secondly. The Spiritualistic schools, or those who believe that something in the Ego is theoretically, as well as practically, above ordinary matter, and is the cause rather than the effect of certain changes in the brain.

It is rather of the Spiritualistic school, as above defined, that I would now speak. I cannot, of course, tell how this school will view evidence tending to prove a peculiar action of mind upon matter, but I think I can tell how they ought to view it. Believing as they do that something in the Ego is theoretically as well as practically above matter, they must believe that to a greater or less extent the usually-received physical axioms are broken by it. That is to say, they have been driven, it may be by ethical and metaphysical views, into an assertion with reference to Physics which they nevertheless believe to be quite unsupported by physical evidence. Surely then they ought above all others to welcome observations tending to show that there may possibly be an action of mind over matter in other regions than that of the brain.

For my own part, while I do not dispute the truth of the position held by the advocates of what I call Free-will, I yet acknowledge the difficulty of its being held permanently as a single isolated exception, incapable of verification. Exceptions are not dead units, but have a family life of their own, with their own peculiar traditions and places of resort; and just as the naturalist, who has got hold of a unique beetle, goes next day to the same hunting-ground in the hope that he may obtain its fellow, so—I am prepared to maintain—should the investigator who thinks he has discovered, no matter how, an undoubted exception, explore the most likely places for its fellow, which, if there be truth in his position, he is almost certain sooner or later to secure.

VI.

AN ACCOUNT OF SOME EXPERIMENTS IN THOUGHT-TRANSFERENCE.

By Oliver J. Lodge, D.Sc.,

Professor of Physics in University College, Liverpool.

Members of the Society for Psychical Research are all perfectly aware of the experiments in Thought-transference which have been originated and carried out by Mr. Malcolm Guthrie, in Liverpool.

Perhaps it may not be considered impertinent, since it bears on the question of responsibility and genuineness, if I state that Mr. Guthrie holds an important position in Liverpool, being a Justice of the Peace, and an active member of the governing bodies of several public institutions, among others of the new University College; that he is a severe student of philosophy, and the author of several works bearing on the particular doctrines of Mr. Herbert Spencer. I may also say that he is a relative of Professor Frederick Guthrie, and that he has exhibited in this experimental research such care and systematic vigilance as might perhaps have been expected on Mr. Francis Galton's principles, and such as would, if properly directed, have placed him in a high rank of experimental philosophers. I may also remind you of what he himself has here said, viz., that he is a partner in the chief drapery establishment in Liverpool, and that it is among the employés of that large business that the two percipients hereafter referred to were accidentally discovered.

Let it be understood that the experiments are Mr. Guthrie's, and that my connection with them is simply this:—that after Mr. Guthrie had laboriously carried out a long series of experiments and had published many of his results, he set about endeavouring to convince such students of science as he could lay his hands upon in Liverpool; and with this object he appealed to me, among others, to come and witness, and within limits modify, the experiments in such a way as would satisfy me of their genuineness and perfect good faith.

Yielding to his entreaty I consented, and have been, I suppose, at some dozen sittings; at first simply looking on so as to grasp the phenomena, but afterwards taking charge of the experiments—Mr. Guthrie himself often not being present, though he was always within call in another room, ready to give advice and assistance when desired.

In this way I had every opportunity of examining and varying the minute conditions of the phenomena so as to satisfy myself of their genuine and objective character, in the same way as one is accustomed to satisfy oneself as to the truth and genuineness of any ordinary physical fact.

I did not feel at liberty to modify the experiments very largely, in other words to try essentially new ones, because that would have been interfering with Mr. Guthrie's prerogative. I only regarded it as my business to satisfy myself as to the genuineness and authenticity of the phenomena already described by Mr. Guthrie. If I had merely witnessed facts as a passive spectator I should most certainly not publicly report upon them. So long as one is bound to accept imposed conditions and merely witness what goes on, I have no confidence in my own penetration, and am perfectly sure that a conjurer could impose on me, possibly even to the extent of making me think that he was not imposing on me ; but when one has the control of the circumstances, can change them at will and arrange one's own experiments, one gradually acquires a belief in the phenomena observed quite comparable to that induced by the repetition of ordinary physical experiments.

It is only on these grounds that I have been asked to report progress to-night, and it is only on these grounds that I have consented.

After this long preamble you may be disappointed to hear that I have no striking or new phenomenon to report, but only a few more experiments in the simplest and most elementary form of what is called Thought-transference; though certainly what I have to describe falls under the head of "Thought-transference" proper, and is not explicable by the merely mechanical transfer of impressions, exhibited before large audiences, signalised by sensational articles in the daily Press, and more properly described as muscle-reading.

In using the term "Thought-transference," I would ask to be understood as doing so for convenience, because the observed facts can conveniently be grouped under such a title; but I would not be understood as implying that I hold any theory on the subject. It is a most dangerous thing to attempt to convey a theory by a phrase, and, probably, if I held any theory on the subject, I should be more guarded in my language, and should require many words to set it forth. As it is, the phrase describes correctly enough what appears to take place, viz., that one person may, under favourable conditions, receive a faint impression of a thing which is strongly present in the mind, or thought, or sight, or sensorium of another person not in contact, and may be able to describe or draw it more or less correctly. But how the transfer takes place, or whether there is any transfer at *all, or what* is the physical reality underlying the terms "mind,"

"consciousness," "impression," and the like; and whether this thing we call mind is located in the person, or in the space round him, or in both, or neither; whether indeed the term location, as applied to mind, is utter nonsense and simply meaningless,—concerning all these things I am absolutely blank, and have no hypothesis whatsoever. I may, however, be permitted to suggest a rough and crude analogy. That the brain is the organ of consciousness is patent, but that consciousness is located in the brain is what no psychologist ought to assert; for just as the energy of an electric charge, though apparently on the conductor, is not on the conductor, but in all the space round it; just as the energy of an electric current, though apparently in the copper wire, is certainly not all in the copper wire, and possibly not any of it; so it may be that the sensory consciousness of a person, though apparently located in his brain, may be conceived of as also existing like a faint echo in space, or in other brains, though these are ordinarily too busy and pre-occupied to notice it.

The experiments which I have witnessed proceed in this sort of way. One person is told to keep in a perfectly passive condition, with a mind as vacant as possible; and to assist this condition the organs of sense are unexcited, the eyes being bandaged and silence maintained. It might be as well to shut out even the ordinary street hum by plugging the ears, but as a matter of fact this was not done.

A person thus kept passive is "the percipient." In the experiments I witnessed the percipient was a young lady, one or other of two who had been accidentally found to possess the necessary power. Whether it is a common power or not I do not know. So far as I am aware very few persons have been tried. I myself tried, but failed abjectly. It was easy enough to picture things to oneself, but they did not appear to be impressed on me from without, nor did any of them bear the least resemblance to the object in the agent's mind. [For instance, I said a pair of scissors instead of the five of diamonds, and things like that.] Nevertheless, the person acting as percipient is in a perfectly ordinary condition, and can in no sense be said to be in a hypnotic state, unless this term be extended to include the emptiness of mind produced by blindfolding and silence. To all appearance a person in a brown study is far more hypnotised than the percipients I saw, who usually unbandaged their own eyes and chatted between successive experiments.

Another person sitting near the percipient, sometimes at first holding her hands but usually and ordinarily without any contact at all but with a distinct intervening distance, was told to think hard of a particular object, either a name, or a scene, or a thing, or of an object or drawing set up in a good light and in a convenient position for staring at. This person is "*the agent*" and has, on the whole, the hardest time of it.

It is a most tiring and tiresome thing to stare at a letter, or a triangle, or a donkey, or a teaspoon, and to think of nothing else for the space of two or three minutes. Whether the term "thinking" can properly be applied to such barbarous concentration of mind as this I am not sure; but I can answer for it that if *difficulty* is an important element in the definition of "thinking," then it is difficult enough in all conscience.

Very frequently more than one agent is employed, and when two or three people are in the room they are all told to think of the object more or less strenuously; the idea being that wandering thoughts in the neighbourhood certainly cannot help, and may possibly hinder, the clear transfer of impression. As regards the question whether when several agents are thinking, only one is doing the work, or whether all really produce some effect, I have made a special experiment, which leads me to conclude that more than one agent can be active at the same time. We conjecture that several agents are probably more powerful than one, but that a confusedness of impression may sometimes be produced by different agents attending to different parts or aspects of the object: this, however, is mere conjecture.

Most people seem able to act as agents, though some appear to do better than others. I can hardly say whether I am much good at it or not. I have not often tried alone, and in the majority of cases when I have tried I have failed; on the other hand, I have once or twice apparently succeeded. We have many times succeeded with agents quite disconnected from the percipient in ordinary life and sometimes complete strangers to them. Mr. Birchall, the headmaster of the Birkdale Industrial School, frequently acted; and the house physician at the Eye and Ear Hospital, Dr. Shears, had a successful experiment, acting alone, on his first and only visit. All suspicion of a pre-arranged code is thus rendered impossible even to outsiders who are unable to witness the obvious fairness of all the experiments.

The object looked at by the agent is placed usually on a small black opaque wooden screen between the percipient and agents, but sometimes it is put on a larger screen behind the percipient. The objects were kept in an adjoining room and were selected and brought in by me, with all due precaution, after the percipient was blindfolded. I should say, however, that no reliance was placed on, or care taken in, the bandaging. It was merely done because the percipient preferred it to merely shutting the eyes. After recent experiments on blindfolding by members of the Society, I certainly would not rely on any form of bandaging: the opacity of the wooden screen on which the object was placed was the thing really depended on, and it was noticed that no mirrors or indistinct reflectors were present. The only surface at all suspicious was the polished top of the small table on which the opaque screen usually stood. But as the screen sloped backwards at a

slight angle, it was impossible for the object on it to be thus mirrored. Moreover, sometimes I covered the table with paper, and very often it was not used at all, but the object was placed on a screen or a settee behind the percipient; and one very striking success was obtained with the object placed on a large drawing board, loosely swathed in a black silk college gown, and with the percipient immediately behind the said drawing board, and almost hidden by it.

As regards collusion and trickery, no one who has witnessed the absolutely genuine and artless manner in which the impressions are described, but has been perfectly convinced of the transparent honesty of purpose of all concerned. This, however, is not evidence to persons who have not been present, and to them I can only say that to the best of my scientific belief no collusion or trickery was possible under the varied circumstances of the experiments.

A very interesting question presents itself as to *what* is really transmitted, whether it is the idea or name of the object or whether it is the visual impression. To examine this I frequently drew things without any name—perfectly irregular drawings. I am bound to say that these irregular and unnameable productions have always been rather difficult, though they have at times been imitated fairly well; but it is not at all strange that a faint impression of an unknown object should be harder to grasp and reproduce than a faint impression of a familiar one, such as a letter, a common name, a teapot, or a pair of scissors. Moreover, in some very interesting cases the idea or name of the object was certainly the thing transferred, and not the visual impression at all; this specially happened with one of the two percipients; and, therefore, probably in every case the fact of the object having a name would assist any faint impression of its appearance which might be received.

As to *aspect*, *i.e.*, inversion or perversion, so far as my experience goes it seems perfectly accidental whether the object will be drawn by the percipient in its actual position or in the inverted or perverted position. This is very curious if true, and would certainly not have been expected by me. Horizontal objects are never described as vertical, nor *vice versâ*; and slanting objects are usually drawn with the right amount of slant.

In proceeding to the details of the actual experiments, it would take far too long to recount the whole—failures as well as successes; I shall only describe a few from which a more or less obvious moral may be drawn.

The two percipients are Miss R. and Miss E. Miss R. is the more prosaic, staid, and self-contained personage, and she it is who gets the best quasi-visual impression, but she is a bad drawer, and does not reproduce it very well.

Miss E. is, I should judge, of a more sensitive temperament, seldom

being able to preserve a strict silence for instance, and she it is who more frequently jumps to the idea or name of the object without being able so frequently to "see" it.

I was anxious to try both percipients at once so as to compare their impressions, but I have not met with much success under these conditions, and usually therefore have had to try one at a time—the other being frequently absent or in another room, though also frequently present and acting as part or sole agent.

I once tried a double agent—that is, not two agents thinking of the same thing, but two agents each thinking of a different thing. A mixed and curiously double impression was thus produced and described by the percipient, and both the objects were correctly drawn.

Description of some of the Experiments.

In order to describe the experiments briefly I will put in parentheses everything said by me or by the agent, and in inverted commas all the remarks of the percipient. The first seven experiments are all that were made on one evening with the particular percipient, and they were rapidly performed.

A. *Experiments with Miss R. as Percipient.*

First Agent, Mr. Birchall, holding hands. No one else present except myself.

Object—a blue square of silk.—(Now, it's going to be a colour; ready.) "Is it green?" (No.) "It's something between green and blue. . . . Peacock." (What shape?) She drew a rhombus.

[N.B.—It is not intended to imply that this was a success by any means, and it is to be understood that it was only to make a start on the first experiment that so much help was given as is involved in saying "it's a colour." When they are simply told "it's an object," or, what is much the same, when nothing is said at all, the field for guessing is practically infinite. When no remark at starting is recorded none was made, except such an one as "Now we are ready," by myself.]

Next object a key on a black ground.—(It's an object.) In a few seconds she said, "It's bright. . . . It looks like a key." Told to draw it she drew it just inverted.

Next object three gold studs in morocco case.—"Is it yellow? . . . Something gold. . . . Something round. . . . A locket or a watch perhaps." (Do you see more than one round?) "Yes, there seem to be more than one. . . . Are there three rounds? . . . Three rings." (What do they seem to be set in?) "Something bright like beads." [Evidently not understanding or attending to the question.] Told to unblindfold herself and draw, she drew the three rounds in a row quite correctly, and then sketched round them absently the outline of the case; which seemed,

therefore, to have been apparent to her though she had not consciously attended to it. It was an interesting and striking experiment.

Next object—a pair of scissors standing partly open with their points down.—"Is it a bright object? . . . Something long ways [indicating verticality]. . . . A pair of scissors standing up. . . . A little bit open." Time, about a minute altogether. She then drew her impression, and it was correct in every particular. The object in this experiment was on a settee behind her, but its position had to be pointed out to her when, after the experiment, she wanted to see it.

Next object—a drawing of a right angle triangle on its side.—(It's a drawing.) She drew an isosceles triangle on its side.

Next—a circle with a cord across it.—She drew two detached ovals, one with a cutting line across it.

Next—a drawing of a Union Jack pattern.—As usual in drawing experi-



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ments, Miss R. remained silent for perhaps a minute; then she said, "Now I am ready." I hid the object; she took off the handkerchief, and proceeded to draw on paper placed ready in front of her. She this time drew all the lines of the figure except the horizontal middle one. She was obviously much tempted to draw this, and, indeed, began it two or three times faintly, but ultimately said, "No, I'm not sure," and stopped.

[N.B.—The actual drawings made in all the experiments are preserved intact by Mr. Guthrie.]

[END OF SITTING.]

Experiments with Miss R.—continued.

I will now describe an experiment indicating that one agent may be better than another.

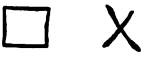
Object—the Three of Hearts.—Miss E. and Mr. Birchall both present as agents, but Mr. Birchall holding percipient's hands at first. "Is it a black cross . . a white ground with a black cross on it?" Mr. Birchall now let Miss E. hold hands instead of himself, and Miss R. very soon said, "Is it a card?" (Right.) "Are there three spots on it? . . . Don't know what they are. . . . I don't think I can get the colour. . . . They are one above the other, but they seem three round spots. . . . I think they're red, but am not clear."

Next object—a playing card with a blue anchor painted on it slantwise instead of pips. No contact at all this time, but another lady, Miss R——d, who had entered the room, assisted Mr. B. and Miss E. as agents. "Is it an anchor? . . a little on the slant." (Do you see any colour?) "Colour is black . . . It's a nicely drawn anchor." When asked to draw she sketched part of it, but had evidently half forgotten it, and not knowing the use of the cross arm, she could only indicate that there was something more

there but she couldn't remember what. Her drawing had the right slant exactly.

Another object—two pair of coarse lines crossing; drawn in red chalk, and set up at some distance from agents. No contact. "I only see lines crossing." She saw no colour. She afterwards drew them quite correctly, but very small.

Double object.—It was now that I arranged the double object between Miss R——d and Miss E., who happened to be sitting nearly facing one another. [See *Nature*, June 12th, 1884.] The drawing was a square on one side of the paper, a cross on the other. Miss R——d looked at the side with



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the square on it. Miss E. looked at the side with the cross. Neither knew what the other was looking at—nor did the percipient know that anything unusual was being tried. Mr. Birchall was silently asked to take off his attention, and he got up and looked out of window before the drawings were brought in, and during the experiment. There was no contact. Very soon Miss R. said, "I see things moving about . . . I seem to see two things . . . I see first one up there and then one down there . . . I don't know which to draw. . . . I can't see either distinctly." (Well anyhow, draw what you have seen.) She took off the bandage and drew first a square, and then said, "Then there was the other thing as well . . . afterwards they seemed to go into one," and she drew a cross inside the square from corner to corner, adding afterwards, "I don't know what made me put it inside."

The next is a case of a perfect stranger acting as agent by himself at the first trial. Dr. Shears, house physician at the Eye and Ear Infirmary, came down to see the phenomena, and Miss R. having arrived before the others, Mr. Guthrie proposed his trying as agent alone. Dr. Shears, therefore, held Miss R.'s hand while I set up in front of him a card: nothing whatever being said as to the nature of the object.

Object—the five of clubs, at first on a white ground. "Is it something bright?" (No answer, but I changed the object to a black ground where it was more conspicuous.) "A lot of black with a white square on it.' (Go on.) "Is it a card?' (Yes.) "Are there five spots on it?" (Yes.) "Black ones." (Right.) "I can't see the suit, but I think it's spades."

Another object at same sitting, but with several agents, no contact, a drawing of this form—



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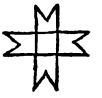


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"I can see something, but I am sure I can't draw it. . . . It's something with points all round it. . . . It's a star, . . . or like a triangle

within a triangle." Asked to draw it, she expressed reluctance, said it was too difficult, and drew part of a star figure, evidently a crude reproduction of the original but incomplete. She then began afresh by drawing a triangle, but was unable to proceed.

I then showed her the object for a few seconds. She exclaimed, "Oh yes, that's what I saw. . . . I understand it now." I said, "Well now draw it." She made a more complete attempt, but it was no more really like the original than the first had been.



Experiments at a Sitting in the room of Dr. Herdman, Professor of Zoology at University College.

Object—a drawing of the outline of a flag.—Miss R. as percipient in



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contact with Miss E. as agent. Very quickly Miss R. said, "It's a little flag," and when asked to draw, she drew it fairly well but perverted. I showed her the flag (as usual after a success), and then took it away to the drawing place to fetch something else. I made another drawing, but instead of bringing it I brought the flag back again and set it up in the same place as before, but inverted. There was no contact this time. Miss R——d and Miss E. were acting as agents.

Object—same flag inverted.—After some time, Miss R. said, "No, I can't see anything this time. I still see that flag. . . . The flag keeps bothering me. . . . I shan't do it this time." Presently I said, "Well, draw what you saw anyway." She said, "I only saw the same flag, but perhaps it had a cross on it." So she drew a flag in the same position as before, but added a cross to it. Questioned as to aspect she said "Yes, it was just the same as before."



Object—an oval gold locket hanging by a bit of string with a little price label attached.—Placed like the former object on a large drawing board, swathed in a college gown. The percipient, Miss R., close behind the said board and almost hidden by it. Agents, Miss R——d and Miss E. sitting in front; no contact; nothing said. "I see something gold, . . . something hanging, . . . like a gold locket." (What shape?) "It's oval," indicating with her fingers correctly. (Very good so far, tell us something more)—meaning

ticket at top. No more said. When shown the object she said, "Oh yes, it was just like that," but she had seen nothing of the little paper ticket.

Next object—a watch and chain pinned up to the board as on a waistcoat.—This experiment was a failure, and is only interesting because the watch-ticking sounded abnormally loud, sufficient to give any amount of hint to a person on the look out for such sense indications. But it is very evident to those witnessing the experiments that the percipient is in a quite different attitude of mind to that of a clever guesser, and ordinary sense indications seem wholly neglected. I scarcely expected, however, that the watch-ticking could pass unnoticed, though indeed we shuffled our feet to drown it somewhat, but so it was; and all we got was "something bright . . . either steel or silver. . . . Is it anything like a pair of scissors?" (Not a bit.)

I have now done with the selection of experiments in which Miss R. acted as percipient; and I will describe some of those made with Miss E. As a rule, these seemed perhaps less satisfactory and complete at the time, but there are several points of considerable interest noticeable in connection with them.

B.—Experiments with Miss E. as Percipient.

Object—an oblong piece of red (cerise) silk. Agent, Mr. B., in contact.—"Red." (What sort of red?) "A dark red." (What shape?) "One patch." (Well, what shade is it?) "Not a pale red."

Next object—a yellow oblong. Agent as before.—"A dusky gold colour. . . . A square of some yellow shade."

Object—the printed letter r. Told it was a letter; agent as before.—"I can see R." (What sort of R?) "An ordinary capital R."

This illustrates feebly what often, though not always, happens with Miss E.—that the idea of the object is grasped rather than its actual shape.

Another object—a small printed e.—"Is it E?" (Yes.) But, again, she couldn't tell what sort of E it was.

Object—a teapot cut out of silver paper.—Present—Dr. Herdman, Miss



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R——d, and Miss R., Miss R. holding percipient's hands, but all thinking of the object. Told nothing. She said, "Something light. . . . No colour. . . . Looks like a duck. . . . Like a silver duck. . . . Something oval. . . . Head at one end and tail at the other." [This is not uncommon in ducks.] The object, being rather large, was then moved further back, so that it might be more easily grasped by the agents as a whole, but percipient persisted that it was like a duck. On being told to unbandage and draw, she drew a rude and perverted copy of the teapot, but didn't know what it was unless it was a duck. Dr. Herdman then explained

that he had been thinking all the time how like a duck the original teapot was, and, in fact, had been thinking more of ducks than teapots.

Next object—a hand mirror brought in and set up in front of Miss R——d. —No contact at first. Told nothing. She said, "Is it a colour?" (No.) "No, I don't see anything." Object then shifted for Miss R. to look at herself in it, holding percipient's hand. "No, I don't get this." Gave it up. I then hid the mirror in my coat, and took it out of the room. Dr. Herdman reports that while I was away Miss E. begged to know what the object had been, but the agents refused, saying that I had evidently wished to keep it secret. Half annoyed, Miss E. said, "Oh, well, it doesn't matter. I believe it was a looking-glass."

Next object—a drawing of a right-angled triangle. No contact.—"Is it like that?" drawing a triangle with her finger (no answer). "It's almost like a triangle." She then drew an isosceles triangle.

Next object—a drawing of two parallel but curved lines. No contact.—"I only see two lines," indicating two parallel lines. "Now they seem to close up."

Next object—a tetrahedron outline rudely drawn in projection—"Is it



another triangle?" (No answer, but I silently pass round to the agents a scribbled message, "Think of a pyramid.") Miss E. then said, "I only see a triangle." . . . then hastily, "Pyramids of Egypt. No, I shan't do this." Asked to draw, she only drew a triangle.

Object—a rude outline of a donkey or other quadruped.—Still no contact at first. "Can't get it, I am sure." I then asked the agents to leave the room, and to come in and try one by one. First Miss R——d, without contact, and then with. Next Miss R., in contact, when Miss E. said hopelessly, "An old woman in a poke bonnet." Finally I tried as agent alone, and Miss E. said "It's like a donkey, but I can't see it, nor can I draw it."

C.—Experiments with both Percipients at once.

In addition to the experiments with single percipients, I tried a few with both percipients sitting together—hoping to learn something by comparing their different perceptions of the same object.

But unfortunately the experiments were not very successful; sometimes they each appeared to get different aspects or the parts of object, but never very distinct or perfect impressions. The necessity of imposing silence on the percipients, as well as on the agents, was also rather irksome, and renders the results less describable without the actual drawings.

I still think that this variation might convey something interesting if pursued under favourable circumstances. Whether greater agent-power is necessary to affect two percipients as strongly as one; or

whether the blankness of mind of one percipient re-acts on the other, I cannot say.

With regard to the feelings of the percipients when receiving an impression, they seem to have some sort of consciousness of the action of other minds on them; and once or twice, when not so conscious, have complained that there seemed to be "no power" or anything acting, and that they not only received no impression, but did not feel as if they were going to.

I asked Miss E. what she felt when impressions were coming freely, and she said she felt a sort of influence or thrill. They both say that several objects appear to them sometimes, but that one among them persistently recurs, and they have a feeling when they fix upon one that it is the right one.

Sometimes they seem quite certain that they are right. Sometimes they are very uncertain, but still right. Occasionally Miss E. has been pretty confident and yet quite wrong.

One serious failure rather depresses them, and after a success others often follow. It is because of these rather delicate psychological conditions that one cannot press the variations of an experiment as far as one would do if dealing with inert and more dependable matter. Usually the presence of a stranger spoils the phenomena, though in some cases a stranger has proved a good agent straight off.

The percipients complain of no fatigue as induced by the experiments, and I have no reason to suppose that any harm is done them. The agent, on the other hand, if very energetic, is liable to contract a headache; and Mr. Guthrie himself, who was a powerful and determined agent for a long time, now feels it wiser to refrain from acting, and conducts the experiments with great moderation.

If experiments are only conducted for an hour or so a week no harm can, I should judge, result, and it would be very interesting to know what percentage of people have the perceptive faculty well developed.

The experiments are easy to try, but they should be tried soberly and quietly, like any other experiment. A public platform is a most unsuitable place; and nothing tried before a mixed or jovial audience can be of the slightest scientific value. Such demonstrations may be efficient into putting money into the pockets of showmen, or in amusing one's friends; but all real evidence must be obtained in the quiet of the laboratory or the study.

VII.

AN ACCOUNT OF SOME EXPERIMENTS IN MESMERISM.

By Edmund Gurney.

1.—*Local Anæsthesia.*

In the second report of the Committee on Mesmerism, an account was given of some experiments in the production, by mesmerism, of local rigidity and anæsthesia, under conditions which precluded the subject from knowing which particular part of his body was subjected to the mesmeric process. The object of these conditions was, of course, to eliminate the factor of expectancy. If the "subject" knows what part of his body is being operated on, that knowledge may alone be quite enough to produce rigidity and anæsthesia in the part; just as one may see a sensitive "subject" go into violent spasms at the touch of a coin, or on drinking water, which he believes to have been "magnetised," even though that belief is erroneous. But if the "subject" is ignorant what particular part of his body has been selected for the purpose, and if rigidity and anæsthesia in this part results from the making of passes over it or from the mere proximity of the operator's hand, then the phenomenon must be attributed to some direct and specific influence passing from the mesmeriser to the "subject." It may be remembered that the method adopted was to seat the "subject" in front of a table, on which his ten fingers were extended; while his body was covered in front by a very thick paper screen, extending far above his head, with holes in it for his arms to pass through. In this manner it was easy to make it perfectly certain that he could not see his hands. Different fingers, or combinations of fingers, were then mesmerised in succession, by passes made without contact, and so quietly as to prevent the "subject" from discovering, by means of currents of air, which of his digits was being operated on. To make assurance doubly sure, one of ourselves would make similar movements over some other finger or fingers than those on which the mesmeriser was at work. The experiments, as so far reported, were made with two "subjects," and have since been repeated with a third; and in every one of the numerous trials the mesmerised finger, or fingers, proved insensible to pain, so far as could be judged by the application of very severe tests. In every case, also, when the "subject" was told to double up his fist, the mesmerised member remained sticking out in

helpless rigidity, and so, for the first time, made its owner aware of the abnormal condition into which it had passed.

It will be observed that this second phenomenon—the rigidity—is, as a test, even more completely satisfactory than the first, the insensibility to pain. For as regards the insensibility, though nobody who witnessed the experiments was able seriously to doubt its genuineness, the objection always remains that very extraordinary feats in the endurance of pain have been known to be performed without any assignable motive. But with the rigidity a similar objection would have no weight; for though, of course, the rigidity itself might be easily simulated, the "subject" had—as I have explained—no means of knowing which was the right finger or pair of fingers to simulate with; and it is clearly out of the question that a mere guess on this point should have been unfailingly correct.

But though the test afforded by telling the "subject" to double up his or her fist was thus evidentially the more complete, the test of insensibility is capable of being made very convincing on its own account. What is wanted is some mode of infliction which a person whose finger was in a normal state would be quite unable to endure without flinching, but which at the same time will leave no painful or unpleasant traces behind, when the finger had been demesmerised and resumed its normal condition. Fortunately such a mode of infliction is afforded by electricity. The shock of an intermittent current can be made strong enough to defy the most hardened powers of endurance, while producing no prolonged ill effects such as would follow a stab or a burn. Accordingly this test was adopted by Dr. Myers and myself on the 26th and 27th April last. The mesmeriser was Mr. G. A. Smith, and the subject was the young baker, Fred Wells, with whom most of the previous experiments had been made.

The same precautions as before were taken to prevent the "subject" from seeing his fingers, and thus to preclude the operation of expectancy. The result was entirely satisfactory. By gradually moving out the regulating-tube of an induction-coil, we could mark the stage at which the pain produced by the current began to pass our own powers of endurance, and we could then immensely increase its strength.* Eleven trials were made, the finger to be mesmerised being each time selected by ourselves; and in every case there was very marked loss of sensibility in this finger. In ten of these trials the particular finger proved insensible to the very strongest shock that could be obtained from the apparatus; and in the eleventh to all but the very

* The apparatus consisted of an induction coil, 4in. long 1½in. diameter, with 10 yards primary wire (No. 18 Birmingham gauge), and 50 yards secondary wire (No. 35 gauge); a regulator; and two quite new pint bichromate cells.

strongest. But there was sometimes a curious additional result. In five cases, when the mesmerised finger was being subjected to the full current, Wells said that he felt a weak effect in another part of his hands. When the middle finger of the left hand, and when the forefinger of the same, was the mesmerised member, this weak effect was felt in the thumb; on the other three occasions it was felt in the palm—once even in the palm of the other hand, and once in the palm of both hands. Wells described the shock, thus localised at a little distance from the mesmerised finger, as about as strong as the very slight shocks which we administered to the unmesmerised fingers just to make sure that they remained sensitive—showing that the strong current was producing an effect immensely below the normal. I may mention that I once by accident touched one of the unmesmerised fingers with the terminal wire for the fraction of a second, when the current was at its full strength; and the violent wince and exclamation which resulted were a pretty sure index of what was felt.

There was a further point of importance in the last four of these trials. The effect was then produced *without any passes at all*, the operator merely holding his hand downwards over the destined finger of the "subject," from which the tips of his own fingers were about two inches distant. Wells's hands are tolerably pachydermatous; and it is extremely difficult to believe that, under these conditions, any physical indication, such as a very slight difference of temperature, could have made him aware which of his ten fingers was nearest to the operator's hand.

We then proceeded to test a hypothesis to which attention was drawn in the second report of the Committee. We there referred to the question how far these finger-experiments bore out the idea that some special *effluence* or some special form of local "nervous induction," passed between the fingers of the operator and those of the "subject." If this were the case, then apparently it ought not to be necessary that the operator should himself *know* over which of the subject's fingers he is making passes, or holding his hand; the effect ought to be producible when he is looking away, and his hand is guided to the appropriate position or movements by a third person. If, on the other hand, this blind and passive mode of operating prove unavailing, and the *knowledge* of the operator as to which of the "subject's" fingers is to be affected, with a distinct direction of attention to that finger, prove to be indispensable conditions of success, then the theory of a special effluence or special local induction seems inapplicable, or at any rate insufficient. Now trial showed that these conditions *were* indispensable; the selected finger remained perfectly sensitive and flexible when passes were made over it by my guiding Mr. Smith's hand, while his eyes and attention were turned in another direction.

The deductions from this fact are very interesting; for if the operator's knowledge and attention are necessary for successful results, then those results seem inevitably to fall under that capacious category of Thought-transference, of which members of this Society hear so much. It is true that the thought is not conveyed *as* a thought; for no impression as to which is the selected finger is conveyed to the subject's intelligence. But among the evidence of telepathy which the Literary Committee have collected, there are several instances where the emotional or volitional condition of one person has produced actions in another person which the latter has been quite unable to account for, and has performed in obedience to a blind impulse. And it is to the same class that the local effects which I have been describing seem to be partly referable. For it certainly does not seem impossible to suppose that if a marked effect may be telepathically produced by the condition of one person on the *motor* system of another, without the transference of any distinct idea, a like effect may also be produced on the *sensory* system. It will probably be granted by those who at all accept the principle of telepathy, that the operator's knowledge as to which particular finger he is operating on is an idea which might be communicated by Thought-transference; it will be further granted by those who realise the power of suggestion and expectation to produce physical effects, that such an idea, if present to the "subject's" consciousness, might lead to local paralysis of movement and sensation in the finger thought of; and, finally, the analogy of the cases of blind telepathic impulse, to which I have just referred, makes it conceivable to us that this very same train of events might take place with the *omission* of the psychical element on the subject's part—with the omission, that is, of his knowledge or conscious idea of the particular finger that was being affected. If this were so, we should have established one more link of connection between the *spontaneous* telepathic communications, between persons at a distance, and the *experimental* communications, occurring otherwise than through the recognised sensory channels, which may be obtained within the four walls of a room.

At the same time, this supposition does not altogether meet the case, so far as the recent experiments are concerned. For while these experiments showed—as I have described—that the mere proximity of the mesmeriser's hands was ineffective if his attention was not directed to his work, they also showed that mere concentrated attention on his part, without any manual process, was equally ineffective. When Mr. Smith attempted to produce the local effect by steadily gazing at the selected finger, without approximating his hand to it at all, the normal condition of the finger remained quite unchanged, and it proved sensitive to the very weakest shock. It appeared, therefore, *that the local* physical process, whatever it may be, though not producible

apart from thought and attention, was still in itself indispensable; and thus the suggested analogy between experimental and spontaneous phenomena is here far from complete.

II.—*Community of Sensation.*

The following experiments in transference of pains and tastes were also made by Dr. Myers and myself, on April 26th, the agent being Mr. G. A. Smith, and the "subject" a very intelligent young cabinet-maker, named Conway, who had been thrown into a light hypnotic trance. For the first set Mr. Smith was in light contact with Conway, behind whom he stood. No hint was given to Conway as to whether his answers were right or wrong: he was simply asked by Dr. Myers or myself what he felt. Mr. Smith kept perfect silence throughout.

1. Mr. Smith was pinched, by one of the experimenters, on the right upper arm. Conway localised very nearly the corresponding place on the left arm, and then the right spot on the right arm.

2. Mr. Smith's right foot was pressed. Conway began to move his right leg uneasily, and complained of pain from the foot upwards.

3. Mr. Smith's right little finger was pinched. Conway complained of pain in the right shoulder.

4. The lower lobe of Mr. Smith's left ear was pinched. Conway complained that the hair above his right ear was being pulled.

5. Mr. Smith's right upper eyelid was pinched. Conway complained of pain in the forehead.

6. Mr. Smith's left popliteal space was pinched. Conway complained of pain in the lower third of the left thigh.

7. Mr. Smith was pinched in the right lumbar region. Conway complained of pain in the left hypochondrium and lumbar region.

In the next set of trials there was no contact whatever between Mr. Smith and Conway. Nor was Conway (who was still in the hypnotic state) informed before the experiments began of what nature they were to be. Standing at some distance behind him, I suddenly and silently gave Mr. Smith some salt, motioning to him to put it into his mouth. He did so; and Conway instantly and loudly exclaimed, "What's this salt stuff?"

I now gave Mr. Smith in succession—	Conway said—
Sugar	"Sweeter; not so bad as before."
Citric Acid	"Bitter; something worse—a little reminds me of cayenne—sweety."
A Raspberry Drop	"A sweetish taste—like sugar."
Salt	"I told you I liked sweet things, not *salt*—such a mixture!"
Clove	"Don't like it; hot—little bit of honey mixed with it."

Salt... ..... "Something acid, salty—first one thing, then another—like brine."

Powdered Ginger..... "Hot; dries your mouth up. Don't like it,—reminds me of mustard."

Sugar..... "A little better—a sweetish taste."

Powdered Alum..... "You call that sweet, do you? Brackish and bitter this—enough to skin your mouth out; bitter."

Cayenne Pepper..... "It's hot, and there is some sugar in it just to soften it over a bit. It is hot—*you* would feel hot, I can tell you."

Cloves..... ..... "Not so very much better, but it's sweeter; it's sugar, only something else with it."

Vinegar..... Conway had sunk into a deeper hypnotic sleep, and made no remark.

Throughout the series Mr. Smith preserved perfect silence; and the only remarks made by Dr. Myers and myself were brief inquiries as to what Conway tasted, with an occasional word calculated to mislead him.

These are not picked results; only one other series of experiments has been made with Conway, and these are fully reported in Part V. of the Proceedings. There are practical difficulties in the way of obtaining either him or Wells for regular and frequent trials; as both are in positions of responsibility, and their time is very fully occupied.

VIII.

DIAGRAMS

ILLUSTRATIVE OF THOUGHT-TRANSFERENCE

The following diagrams, illustrative of a special form of Thought-transference, are part of a series referred to in Part V. of the Proceedings, p. 7, under the head of results obtained by independent investigators. Professor Barrett has seen the "agent," Mr. J. W. Smith, of Brunswick Place, Leeds, and his sister, the "percipient," and has carefully explained to them the necessary precautions; but their description of the mode in which they had worked before this interview convinced him that those previous trials had been conducted with due care, and that the results were genuine. The experiments have been made throughout without contact. The first four of the diagrams here engraved were made before Professor Barrett's visit; the last four have been made since his visit.

It may be added that whenever the Committee on Thought-transference obtain evidence of a case of this kind, where the power seems tolerably continuous, and the conditions, as reported, seem satisfactory, they will do all in their power to obtain opportunities for personally conducting the experiments. Meanwhile, they cannot too strongly urge the importance of having trials made as widely as possible in families and private circles. They are continually hearing of results —obtained without contact or the possibility of sensory indications— which show that the genuine faculty is by no means extraordinarily rare; but often the scientific value of the experiments is not realised, and no written record has been kept. If only every indication of this sort were carefully followed up, a body of evidence might be collected which would greatly hasten the general acceptance of the facts.

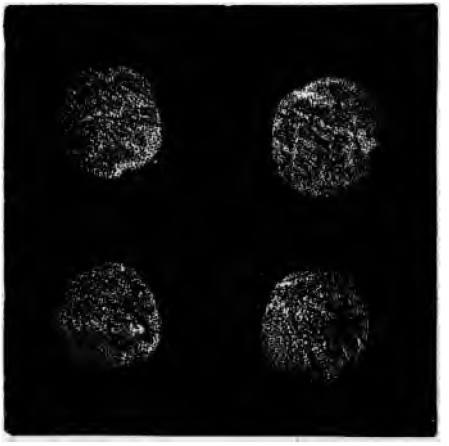
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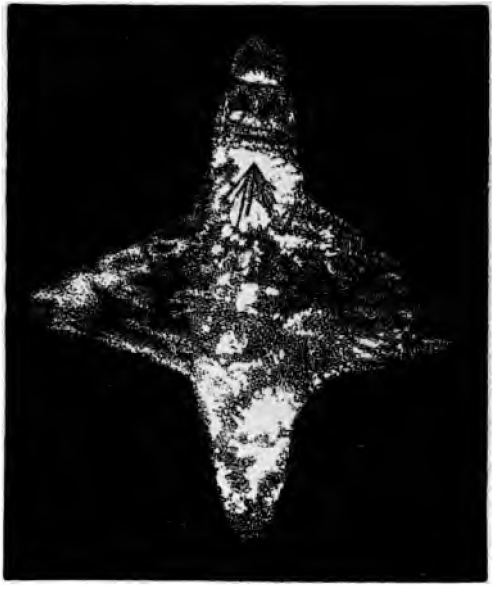
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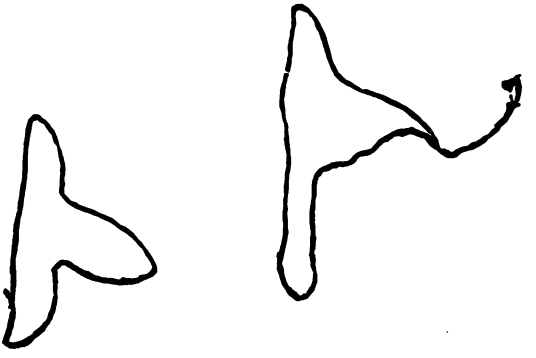
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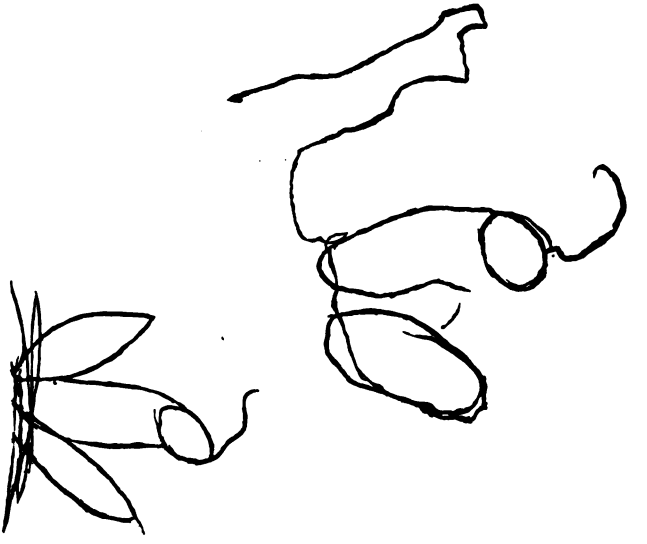
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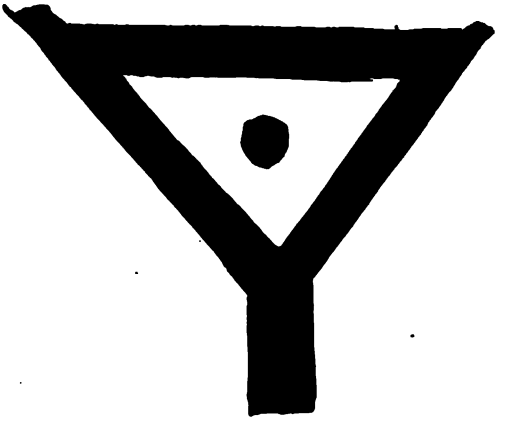
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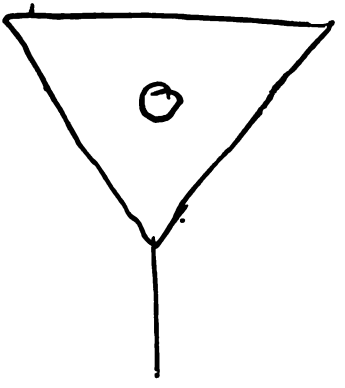
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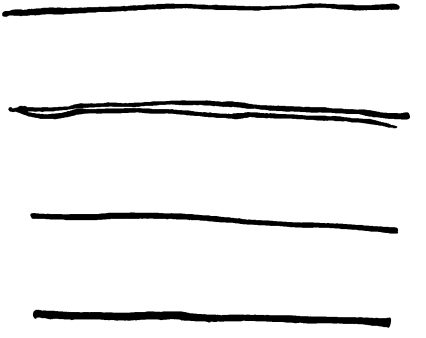
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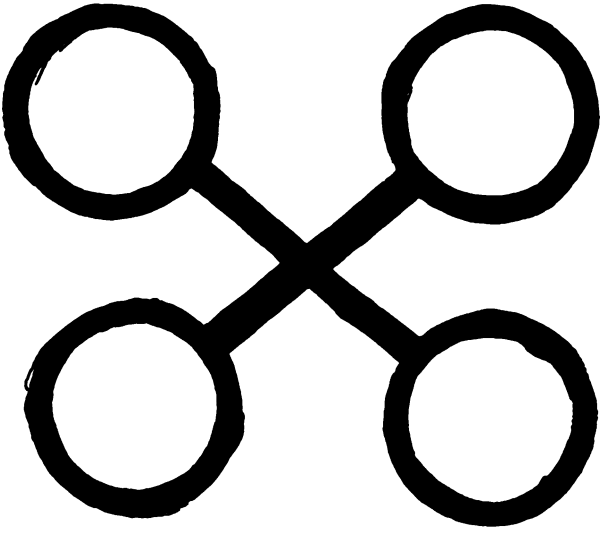
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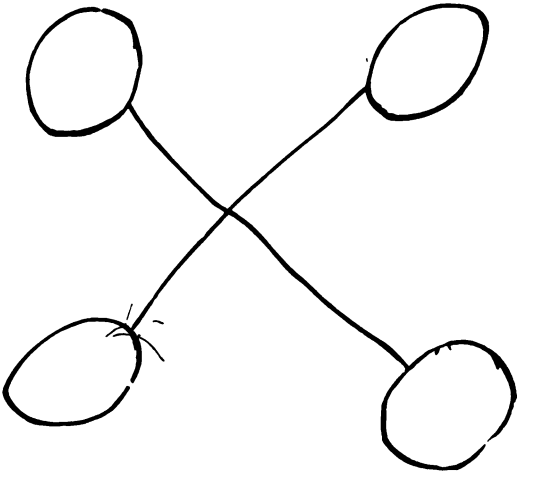
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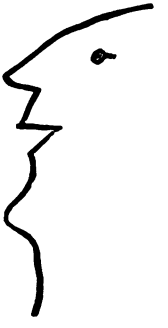
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The following table of results was filled up by Mr. J. W. Smith on one of the forms supplied by the S.P.R.

Experiments in Thought-Transference.

Agent or Agents.		Percipient.	
J. W. SMITH.		KATE SMITH.	
Date.	**Object selected.**	**First trial.**	**Second trial.**
March 11.	Figure 8.	Correct first time.	
—	Figure 5.	Correct first time.	
—	Black cross on white ground.	Correct first time.	
—	Colour blue.	Correct first time.	
—	Cipher (0).	Correct first time.	
—	Pair of scissors.	I did not say what (*i.e.*, what form of experiment—figure, colour, or object) was to be next, but carefully and without noise laid a pair of scissors on a white ground, and in about one minute and a-half she exclaimed, "Scissors!"	
Group.	**Total No. of experiments.**	**No. right on first trial.**	**No. right on second trial.**
3 figures. 1 colour. 1 object.  1 pair of scissors.	6	6	

Observations: Whilst sitting at dinner (March 11th, 1884), I was thinking about a person who had died. Just then one of my sisters (not the thought-reader) exclaimed: "I wonder if it will be a large funeral." We had not spoken about that subject previously. After that I thought to myself I would see if I could make her hum a tune. I then (to myself, that is to say, without uttering any sound whatever) hummed a tune, and to my surprise she also sang aloud the *same* song. After that, I resolved that she should lift her fork up (she had finished her dinner); she did so immediately. I do not know whether this was an instance of Thought-transference or not, but if it were not so it seems an astounding coincidence. (No contact.)—J. W. S.

PROCEEDINGS OF THE GENERAL MEETING ON

November 28, 1884.

The tenth General Meeting of the Society was held at the Rooms of the Society of British Artists, Suffolk Street, Pall Mall, on Friday, November 28, 1884.

PROFESSOR W. F. BARRETT, VICE-PRESIDENT, IN THE CHAIR.

The Chairman, in his opening address, described a recent visit to Canada and the United States, in the course of which he had devoted much time and trouble to spreading a knowledge of the Society's work. He had met with many indications of a genuine interest in Psychical Research, and he spoke hopefully of the prospect that allied societies might before long be formed at several important centres.

I.

ON A TELEPATHIC EXPLANATION OF SOME SO-CALLED SPIRITUALISTIC PHENOMENA.

BY F. W. H. MYERS.

PART I.

It is an obvious fact, but it is nevertheless a fact which we must repeat as often as possible, that in no way can psychical research be better aided than by constant and varied experiments on Thought-transference in every form. We have got, as we hold, a definite fact to start from, a fact of immense and unknown significance. If, as we believe, we can truly say "mind acts on mind otherwise than by the recognised organs of sense," this is probably a statement far more pregnant with consequences than the statements, "rubbed amber attracts straw," or, "the loadstone attracts iron." And it must be our business to turn our new fact over in every direction, to speculate upon it in every way, or rather in every way which can possibly suggest a new form of experiment. We must remember that the experimental cases which we have already collected are probably only what Bacon calls "ostensive instances;" "instances," as he expressed it, "which show the nature under investigation naked, in an

exalted condition, or in the highest degree of power" ;—and which are, so to speak, mere emergent summits from a great ocean, which lies beyond our present reach of observation, and perhaps even beneath the level of our consciousness.

And our investigation, in the case of a power so novel and obscure, must take two different, though converging lines. We must, in the first place, vary our actual deliberate experiments as widely as possible ; in turn introducing and excluding as many separate conditions as seem likely to have a bearing on the result. And we must, in the second place, scan the whole range of psychical phenomena already alleged to occur (though interpreted on some different principle) with telepathy in our minds as a new *vera causa*, which will probably be found to be the real solution of many problems hitherto in dispute.

And in this form of *fishing interrogatory*, or survey from a new standpoint of a wide and miscellaneous field, we shall do best to begin with a few definite groups of phenomena, and to disentangle them as distinctly as may be from any dubious theory with which they may have become involved, and to look at them in the light of telepathy so as to give that theory the chance of explaining them.

In this process we shall probably, in some directions, press the theory of telepathy farther than subsequent knowledge will justify. But it is only by endeavouring to ascertain how many phenomena, inexplicable by generally recognised laws of nature, may be explained by this new *vera causa* that we shall learn at what point other, yet unknown, causes (if any) begin to intervene.

We have already begun to make one survey from the telepathic standpoint of previously-recorded phenomena—namely, our inquiry into the nature of *apparitions* and other (auditory or tactile) phantasms of living persons. We have seen reason to connect many of these with telepathy ; and we are pressing that explanation as far as it will go, though without prejudice to any other explanation which certain apparitions may hereafter seem to require.

In the same way I now propose, in a series of two or three papers, to consider how far the already recorded phenomena of automatic writing and the like may be explained by telepathy, but without prejudice to any other yet unknown causes which may afterwards show claim to acceptance.

I have selected *automatic writing* as the subject for inquiry in great measure because of its direct bearing on one of the most interesting of telepathic problems,—viz., the relation of *consciousness* to telepathy ; the extent to which the hypothetical impact is consciously present, whether as will, thought, emotion, or sensation in either mind. Throughout all these investigations we must keep *unconscious cerebration* steadily in view, and we shall, I think, find ourselves confronted with many of its

results, and be induced continually to enlarge its field of action. We shall, I venture to say, come to regard this term less and less as expressing a *subsidiary*, more and more as expressing a *substantive and primary* operation of our intelligence; and we shall come, perhaps, to find *super-conscious* as necessary a term as *sub-conscious*, if we would indicate the true relation to each other of the processes in which our being consists.

And it is to my mind one of the chief interests in telepathy that it seems sometimes to reveal our unconscious to our conscious selves; that it acts like the strong inwardly-directed ray of light which enables the surgeon to see reflected in a mirror some obscure recess of his own inward structure.

With these possibilities in our mind, let us first consider how the question of *consciousness* stands in the cases of telepathy which we have already collected. And, for the sake of simplicity, we will, in this first paper, take the *percipient's* consciousness alone into account.

In our recorded *experiments* on telepathy with percipients in the normal state the percipient has necessarily been conscious of the impact, inasmuch as it has been by his description, or his voluntary representation of the impact, (as by drawing a picture which he feels to be telepathically presented to his mind's eye), that the success of the experiment has been tested. No argument can be drawn from these cases, however, as to the *necessary* emergence of the telepathic impact in consciousness. For here consciousness was voluntarily concentrated on the reception of that impact; all competing impressions being, as far as possible, excluded.

But in one class of our experiments a singular difference occurs. Thought-transference is one of the characteristic phenomena of the mesmeric trance or *sleep-waking* state, where, although there seems to be a certain consciousness at the time, that consciousness, in most cases, leaves no trace whatever in the subject's normal memory. And, more than this, there seems reason to believe that in many cases of hysteria and similar affections where the subject is said to be *clairvoyante*, the phenomenon is in reality referable to thought-transference only. An interesting case, recently published by Dr. Taguet, of Bordeaux, affords a good instance of the confusion of thought at present prevalent on such subjects. His hysterical patient manifested during her *accès* an extraordinary hyperæsthesia of smell and hearing. Her eyes, meantime, were shut, or at any rate the eyeballs were completely upturned. But, nevertheless, she manifested what Dr. Taguet rather oddly describes as "hyperæsthesia of vision," under the following circumstances. Dr. Taguet stood completely behind her, and fixed his eyes on certain words in a book. A sheet of cardboard was held before her face, in such a position that, had it been a mirror, the book would have been reflected

therein. The girl then, in her trance, took the cardboard for a mirror, and spelt out the words which Dr. Taguet was reading, as though they were reflected in the cardboard before her. Now Dr. Taguet does not attempt to explain this, further than by calling it hyperæsthesia of vision. But he can hardly mean that she really saw the words reflected in the cardboard. Perhaps the only other solution which suggested itself to him was that she saw the words clairvoyantly, and this solution he did not like to adopt. And there is in fact no reason, as the facts are reported, for assuming clairvoyance. Thought-transference would amply suffice to explain the phenomenon. If, then, we find telepathy occurring in mesmeric trance and spontaneous trance, we may infer that it is not inseparably linked with the ordinary stream of normal consciousness. If it appears as an element of the consciousness or quasi-consciousness of abnormal states, which themselves form mere *lacunæ* in the main life-memory, it may be surmised to exist beneath the threshold of consciousness in normal states also.

It is, naturally, not very easy to get direct evidence of this. Amongst our phantasms of the living we have, no doubt, many instances where the supposed thought or impulse of the agent is represented very vaguely and imperfectly in the percipient's mind, and prompts, for instance, to some mistaken form of action. But this is not quite the same thing. *Something* comes into the percipient's consciousness, though it may be only an imperfect transcript of what the agent's mind contained, or attempted to transmit.

In a few cases, however, the impact seems to affect the percipient without exciting anything which can be called an *idea*. In these cases it is merely felt as a motor impulse, determining to some action whose purport is not consciously realised. We have elsewhere given the case of Mr. Skirving, who was irresistibly impelled to leave his work and go home—why, he knew not—at a moment when his wife was, in fact, calling for him in the distress of a serious accident. But we may give here one other illustrative case,—which no one, I think, who has accepted the general fact of telepathy will be disposed to regard as a mere coincidence; a case where consciousness is still *less* invaded by the telepathic impact, and where the motor impulse is described as having been as nearly *automatic* as any movements so prolonged and complex can easily be. The case comes from Mr. Morgan, of Nugent Hill, recommended to us by Dr. Paul Chapman as a most accurate and careful informant.

Nugent Hill, Bristol, *July 11th*, 1883.

DEAR SIR,—

The matter to which Dr. Chapman refers is as follows :—

On Monday, February 14th, 1853, I was listening to a lecture by the late Geo. Dawson, of Birmingham, in the Broadmead Rooms in Bristol. I frequently spent my evenings at lectures, concerts, &c., and often took a

little walk afterwards on my way home. I had lived nearly all my life (27 years) at home with my mother, whom I strongly resemble in face and in many characteristics. We were much attached to each other.

I was thoroughly interested in the lecture, and had so little intention of leaving before its conclusion, that I remember noticing a friend among the audience and making up my mind for a walk with him on my way home.

The lecture must have been more than half through—I was not tired, and had no reason to move—when I noticed, at the side of the platform farthest from the back entrance to the hall, a door which I had never seen before, flush with the panels, and it suddenly became the most natural thing that I should walk half the length of the room, and away from the main entrance, in order to see if this door would open. I turned the handle, passed through, closed the door gently behind me, and found myself in the dark among the wooden supports of the platform.

I clambered along towards a glimmer of light at the other end, passed round a side passage, crossed the end of the hall to the main entrance without any thought of the lecture, which was still going on, and walked home quietly without excitement and "impression" of any kind, and quite unconscious, till long after, that I had done anything unusual.

On opening my door with a latch-key I smelt fire, and found my mother in great alarm. She had also noticed the strong burning smell, had been over the house with her servant, and was longing for my return.

On going upstairs I saw flames issuing from a back window of the next house, immediately gave the alarm, removed my mother to a safe distance, and then had two or three hours' struggle with the flames. The adjoining house was destroyed, but mine only slightly damaged.

The point which has seemed to me most striking whenever I have recalled this occurrence, is the entire absence of any presentiment or impression on my mind. I should probably have shaken off anything of the kind had I been aware of it, and refused obedience.

Neither was there on my mother's part any intentional exertion of her will upon me, only a strong wish for my presence, which must have begun about the time I left my seat. I do not know that there is anything to add. If anything occurs to you as being worthy of further inquiry, I shall be happy to say what I can about it.

FREDK. MORGAN.

Mr. Morgan adds, in reply to our regular inquiry, that he has never had any other presentiment or impression of the kind.

He also sends a plan of the lecture-room, which shows that he walked in a dark passage round nearly three sides of the hall.

"But *going home*," he adds, "was not in my thoughts when I moved."

Well, then, let us consider whether there is any kind of minor automatic motion which we can hope to evoke experimentally by telepathic impact, without the invasion of the percipient's consciousness by any definite *idea?* The movements practised in the "willing game" —finding of objects and the like—at once suggest themselves, and the writing of the numbers of bank-notes, &c., as practised in certain public performances. But, as we have so often remarked, any contact betwee-

agent and *percipient* at once vitiates these as telepathic experiments; and even if we get the finding of objects, &c., without contact, it seems difficult to exclude all risk of *guessing*, where the possible modifications of movement are, comparatively speaking, so few. We want to find some class of movement which shall be readily capable of being performed *automatically*, and at the same time shall admit of a very large range of variations.

Probably the first action of this kind which will suggest itself to all of us is the act of *writing*. Handwriting is, in many ways, a kind of summary expression of a man's being. It is one of the best instances of an aptitude at once acquired and hereditary; of a manual dexterity which obeys limitations of idiosyncrasy as well as of will. John Smith cannot paint like Raphael. That, we say, is because Raphael's command of hand and eye is immensely greater than John Smith's. But it does not follow that Raphael can write like John Smith. Very probably no manual or mental superiority will give him the power of appropriating what is veritably idiosyncratic in the hurried scrawl with which John Smith signs his name, with scarcely a glance at the paper. Handwriting, that is to say, is a *deep-seated* thing. It is likely to have secrets to tell us.

First, let us consider what is the *rationale* of automatic writing from the ordinary physiological point of view. It is simply an extension of the tricks of unconscious action which, to some extent, are common to every one, and which in nervous and excitable persons often attain an extraordinary degree of complexity. It is, of course, well known that in moments of vivid emotion the surplus nervous energy escapes in involuntary channels, which often bear some traceable connection with the habitual modes of thought or action of the person concerned. To take a typical case, an accomplished pianist, if stirred by some sudden emotion while seated at the piano, will sometimes play a long passage without any consciousness of having done so; and, moreover, the passage thus unwittingly selected will be one which is in vague general harmony with the new current of emotion. Now the act of writing being one of the commonest of the more complex acquired acts, we shall naturally expect that many half-conscious or unconscious tricks will be connected with it.

And this is notoriously the case. Persons seated with pens or pencils in their hands round a table where discussion is going on will generally scrawl or sketch on the paper before them; while if that paper was suddenly withdrawn, and they were asked what words they had written thereon, most of them would be unable to reply. Students of language are, as might be expected, particularly liable to this trick; and many an odd Greek word, going its way, so to speak, from some [illegible] has been unconsciously scribbled on the edge of

composition papers in the nervousness of examination. In this case it is the strong concentration of the current of attention elsewhere which allows the writing faculty to manifest itself automatically,—permits, that is to say, the unconscious cerebral action to discharge itself along the well-worn track which leads to the formation of written words. And something of the same kind takes place also when the current of attention, instead of being concentrated into a narrow channel, is choked in the sand; or (to vary the metaphor) when an explosion of the more complex brain elements (as in an epileptic fit) has left the less complex in possession of the field, and the actions are not guided, or not wholly guided, by conscious will. There are many varieties of this *morbid* automatism. Thus a patient of this kind may write a word, say, "horse," without any consciousness of writing at all (as in some slight epileptiform seizures, where the act of writing is automatically *continued*); or he may write "cow" when he wishes to write "horse" and *not* perceive his error; or he may write "cow" for "horse," although he perceives his error as he is in the act of committing it. Now, in both these classes of cases, in the graphic automatism of mental abstraction and the graphic automatism of cerebral disease, the passages written are usually very *short*; in the first case because the abstraction is transitory, in the second case because the writing impulse is feeble. The anxious classical honour-man could not scribble down a whole ode of Pindar without becoming aware of what he was doing. And in the morbid cases there is nothing but a residual impetus, soon exhausted, or a painful effort of the imperfect will. Let us consider, however, whether there are any cases which indicate that the unconscious graphic impulse may be prolonged and in a sense *systematic*. We find that precisely such cases are afforded by somnambulists, who not unfrequently write long compositions with much manual rapidity and accuracy. Sometimes these compositions are a kind of written dream, rambling and incoherent; sometimes they are on the level of waking thought; sometimes they seem to overpass it, as when the solution of a baffling problem is written out during sleep.

We see, then, that automatic writing is a phenomenon liable to be originated in various ways in the human organism. And we shall not, therefore, be surprised to find that certain human beings are very much more liable to its occurrence than their neighbours. For we are gradually learning, (what was, of course, antecedently probable), that the gamut of natural capacity is just as far-reaching when we deal with things trifling and useless, as when we deal with the most impor-

spicuous things. The differences in human faculty were
atters important to human welfare; but they exist quite
the obscurest corners of our constitution. There is the
ler just as surely as the born runner; there is the born

visualiser of numbers—to take one of Mr. Galton's instances—just as truly as the born painter or sculptor. We can set no definite limit beforehand to any of the veins of unexplored faculty which crop up at intervals from the subterranean realms of our being.

It will, therefore, by no means surprise us to learn that there are certain persons who occasionally feel an impulse to write automatically when they are merely sitting quiet with a pencil in their hands. This is not really much more odd than that there should be persons who occasionally feel an impulse to *imagine a tune* inwardly when they are merely sitting quiet with nothing to do. The imagined tune often externalises itself, so to speak, in rhythmical movements of the head and body, involving a good deal of muscular action, of which the subject is nearly or quite unconscious. The case of automatic writing, however, differs in this way from the dumbly-imagined tune,—in that the written words, falling immediately under the writer's eye, tend to arrest his attention and to evoke a *conscious* train of thought,—an anticipation of what is coming next, which strongly tends to check the automatic flow. The little instrument called *Planchette* is mainly useful in precluding this kind of interruption. It is, of course, simply a piece of board supported on three legs, one of which legs is a pencil, so that if a hand be placed flat on the board, and if that hand be then moved as though tracing letters, the board will move accordingly, and the pencil will rudely trace out the letters which the hand's movement figures. Of course it is perfectly easy to write *consciously* with a Planchette, and to be aware of the letters which the pencil is shaping. But the point is that *if* there is a tendency already existing to *automatic* writing, it is much easier to write automatically or unconsciously with the Planchette than without it. A slight tremor of the hand will set the Planchette running, and the scrawled characters are generally quite too rough and confused to catch the operator's eye, and suggest conscious anticipations.

Now suppose I am writing with a Planchette. Let us consider what theories are logically possible as to the source of what I write. The words which I am writing may conceivably be :—

1. Consciously written in the ordinary way, and chosen by my deliberate will.

2. Automatically written, and supplied by my own unconscious cerebration, as in dreams.

3. Automatically written, but supplied by some higher unconscious intelligence or faculty of my own, as in clairvoyance.

4. Automatically written, but supplied by telepathic impact from other minds.

5. Automatically written, and supplied by "spirits" or extra-human intelligences.

Now as to hypothesis 1, of course we are all agreed. It is per-

ctly easy to write consciously with Planchette, and to look as if you ere writing unconsciously. The *proof* (to others than the actual riter) that the writing is automatic can only lie in the production of ames or facts unknown to the writer. But it is easy for competent bservers, under certain circumstances, to satisfy themselves that what hey write, although containing no facts new to them, has *not* passed hrough their consciousness. I am speaking at present, of course, of ases where one person alone is writing, either with or without Planchette.

The first incipient stage of automatism is described in the following ccount by Mr. H. Arthur Smith, author of "The Principles of Equity," nd a member of the Council of the Society for Psychical Research.

In experimenting alone, since, I have not succeeded in getting any notion out of the instrument *aliunde;* but I think I have observed that when my hand rested on it, the wrist being grasped by the other hand, a word, on which I concentrated my attention, was written without any conscious volitional effort. I am doubtful as to this, as it is a difficult thing to be sure of the absence of volition, but such is my decided impression.

Next we have the experience of Mr. A. (a friend of the writer's), who can often write words by mere attention without any voluntary muscular action whatever. He fixes his mind on a word, and his hand writes it with energy,—as though by an involuntary spasm or true *cacoethes scribendi*,—while he is studiously avoiding all intentional impulse. This experience is interesting, as being precisely what might have been anticipated, as an intermediate step between writing which is wholly voluntary and writing which is wholly automatic. It seems probable that there might be a point at which consciousness extended to the idea related with the movement, but not to the movement itself, at which there was still *attention*, but not voluntary muscular action. We have next to find an example of writing wholly automatic, indicating if possible by its very substance that it has not consciously passed through the writer's mind. It is naturally not very easy to fix on written matter of which we can affirm both that it is such as the writer's *unconscious* cerebration might have produced, and that it is such as his *conscious* cerebration had no share in modifying. There are frequent cases in which the writer affirms that he is not aware of the letters which he is writing until he is in the act of writing them; or sometimes until they are written. In such cases the nervous process which causes the act of writing would seem to be unconscious; although the mental act required to produce the formation of a letter is so simple and rapid that it is hard to be sure that there was not a semi-consciousness of it almost immediately forgotten. Sometimes however, in the midst of

writing of this kind 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 ;—and in such cases we have, I think, to suppose that a rather complex process of unconscious cerebration has taken place. An excellent instance of this kind has been sent to the Society by the above-mentioned Mr. A, on whose accuracy we believe that we can thoroughly rely. We give it in full because it is a good instance of the capricious half-nonsense which believers in Spiritualism often unhesitatingly refer to the agency of spirits, but which every intelligent Spiritualist would surely be glad to be able to explain in some other way.

The indisputable evidence for complex unconscious cerebration which this case seems to me to furnish lies in the fact that here the Planchette writes not only unintelligible abbreviations but absolute anagrams of sentences ; 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.

Clelia, or Unconscious Cerebration.

The following experiment will be regarded by some as a beautiful proof of unconscious cerebration; by others as an indubitable proof of the existence of spirits. Others, again, will, like myself, remain halting between the two opinions, with a decided leaning to the scientifically more orthodox. I wished to know if I were myself an automatic writer, or so-called writing medium. The experiment was made Easter, 1883, upon one day, and after an interval of a week, continued upon three consecutive days ; upon four days in all. Upon the first day I became seriously interested ; on the second puzzled ; on the third I seemed to be entering upon entirely novel experiences, half awful and half romantic ; upon the fourth the sublime ended very painfully in the ridiculous.

First Day.

Q. 1. Upon what conditions may I learn from the unseen ?
A. 1. _____

My hand immediately moved, though not to a very satisfying issue. But, as my expectation of the answer had been that the condition was a strict adherence to the absolute rule of right, holiness in short, I took this answer to be at any rate consistent with my expectation, and continued :

Q. 2. *What* is it that *now moves* my pen ?
A. 2. Religion.
Q. 3. What moves my pen to write *that* answer ?
A. 3. Conscience.

Q. 4. What is religion?
A. 4. Worship.

Here arose a difficulty. Although I did not *expect* either of these three answers, yet when the first few letters had been written I expected the remainder of the word. This might vitiate the result. Cons—, for instance, might have ended as consciousness, had I thought of that word instead of thinking of conscience. As if to meet the difficulty, just as if an intelligence wished to prove by the manner of answering that the answer could be due to *it* alone, and in no way and in no part to mere expectancy, my next question received a singular reply.

Q. 5. Worship of what?
A. 5. wbwbwbwbwb——
Q. 6. What is the meaning of wb?
A. 6. Win, buy.
Q. 7. What?
A. 7. Know (ledge).

Here I knew the letters which were to follow, and the pen made a sudden jerk, as if it were useless to continue.

Q. 8. How?
A. 8. _____

Here I was referred to the first answer. Although startled by the fifth and seventh answers, which *at first sight* seemed to show an independent will and intelligence, yet, as I had learnt nothing new, I concluded the whole to be due to unconscious cerebration and expectancy. Having, then, put a few questions as to matters of fact unknown to me, but easily discoverable, and the replies being either illegible or wrong, I was confirmed as to the complete naturalness of the phenomena.

SECOND DAY.

Q. 1. What is man? (*i.e.*, What is the nature of his being?)
A. 1. Flise.

My pen was at first very violently agitated, which had not been the case upon the first day. It was quite a minute before it wrote as above. Upon the analogy of *wb*, I proceeded.

Q. 2. What does F stand for?
A. 2. Fesi.
Q. 3. l?
A. 3. le.
Q. 4. i?
A. 4. i v y.
Q. 5. s?
A. 5. sir.

Q. 6. e?
A. 6. e y e.
Fesi le ivy sir eye.
Q. 7. Is this an anagram?
A. 7. Yes.
Q. 8. How many words in the answer?
A. 8. 4.

I tried for a few minutes to solve it without success. Not caring to spend much time in trying to solve what might have no solution, I gave it up.

THIRD DAY.

Q. 1. (rep.) What is man?
A. 1. Tefi Hasl Esble Lies.

This answer was written right off.

Q. 2. Is this an anagram?
A. 2. Yes.
Q. 3. How many words in the answer?
A. 3. V (*i.e.*, 5).
Q. 4. What is the first word?
A. 4. See.
Q. 5 What is the second word?
A. 5. Eeeeee——
Q. 6. See? Must I interpret it myself?
A. 6. Try.

Presently I got out

"Life is the less able."

Next I tried the anagram given upon the previous day, and at last obtained

"Every life is yes."

But my pen signified that it preferred the following order of words,

"Every life yes is."

Some remarks concerning the probable genesis of these oracular sentences will be made in the sequel. I do not know whether any other interpretations can be given to the letters. But these fulfil the requirements as to the number of words; and the action of the pen, assisting in the process of interpretation, pointing to the letters, accepting these and rejecting those combinations, left no doubt in my mind that I had hit the meaning.

But now I was so astonished at the apparently independent will and intellect manifested in forming the above anagrams that, for the nonce, I became a complete convert to Spiritualism; and it was not without something of awe, that I put:

Q. 7. Who art thou?
A. 7. Cleio!!
Q. 8. Thou art a woman?
A. 8. Yes.
Q. 9. Hast thou ever lived upon the earth?
9. No.

Q. 10. Wilt thou ?
A. 10. Yes.
Q. 11. When ?
A. 11. Six years.
Q. 12. Wherefore dost thou speak with me ?
A. 12. E if Clelia e l.

It has been already said that when I experimented I had a certain fever of speculative pessimism upon me. It was, therefore, with increasing excitement that I perceived as an interpretation

"I Clelia feel."

But upon my asking whether this was right "Clelia" wrote again thus

"E if Clelia e l."

20

Q. 13. Is 20 your age ?
A. 13. ∞. (She was eternal.)
Q. 14. Then 20 what ?
A. 14. Words.

I was obliged to reserve the reception of these till the morrow. I am writing not a tale of Edgar Poe, but a scientific narration of fact. Therefore, nothing shall be said of my feelings and ideas upon this occasion. It was evident that I was in communication with a—beautiful ?—spirit of romantic name, who in six years time was to be born upon the earth. My snatches of sleep that night were few and far between.

Fourth Day.

I began my questioning in the same exalted mood, but, to my surprise, did not get the same answer.

Q. 1. Wherefore dost thou speak with me ?
A. 1. [Wavy line. Repetition and emphasis : Wherefore dost *thou* speak with *me?*]

However, I thought this "a solemn and piercing rejoinder," and proceeded to consider my motives, and purify them from all earthly and unspiritual alloy. Then—

Q. 2. Wherefore dost thou answer me ?
A. 2. [Wavy line. Wherefore dost *thou* answer *me?*]
Q. 3. Do I answer myself ?
A. 3. Yes.
Q. 4. Is Clelia here ?
A. 4. No.
Q. 5. Who is it, then, now here ?
A. 5. Nobody.
Q. 6. Does Clelia exist ?
A. 6. No.

Q. 7. With whom did I speak yesterday ?
A. 7. No one.
Q. 8. Why didst thou lie ?
A. 8. [Wavy line. Why didst *thou* lie ?]
Q. 9. Do souls exist in another world ?
A. 9. m b.
Q. 10. What does m b mean ?
A. 10. May be.
Q. 11. What was that last answer of yesterday again ?
A. 11. Eif Clelia o el,

i.e., I feel no Clelia ;
or, the original may have been

"I Clelia flee."

And the 20 meant *no*, negativing my interpretation.

My pen now became altogether wild, sometimes affirming and sometimes denying the existence of Clelia, and finally performed as follows :—



I wrote ce, and u.c., and placed my pen in the middle between them. It refused to point to either, but upon my writing "partly each" above, my pen underlined the words.

Possible Scientific Explanation.

The desire for an answer was sufficient in the first place to put brain and muscles in motion.

A desire soon arose to avoid the effect of expectancy. To meet this desire the brain unconsciously formed anagrams ; one of these anagrams, "Life is the less able," is a sentiment only too frequently met with in the spiritualistic literature which I had been lately reading.

The other, "Every life is yes," is similarly derived from Spinoza, whom I had just been reading. He has much about all existence being affirmation of the Deity.

Thus both these expressions were imbedded in my brain by late reading, and this fact supports the supposition of u.c. being the agent.

With regard to "Clelia," I had written cl when the rest of the word flashed upon me, and this may have vitiated the answer through expectancy ; and all that followed about Clelia may have been a structure upon a false foundation ; which, therefore, my brain having unconsciously considered during the night, proceeded upon the next day to demolish.

POSSIBLE SPIRITUALISTIC INTERPRETATION.

That upon the first day it was perhaps u.c. acting for the most part.

But on the second day, the great agitation of the hand, the complicated intellectual process, the insistance ("See," "Try") would point to a spirit's operating upon the cerebral particles.

That this spirit chose to call herself Clelia, and that having manifested herself she fled, leaving me upon the fourth day to the unassisted operation of u. cerebration.

Although as I have said, I incline strongly to the scientific explanation, that inclination does not rise to absolute belief. But I must confess I should not expect myself to be able to hit upon the right explanation, and it is with the hope that some psychologist may clearly explain it that I wish to call attention to it. There is another reason, more singular, for my calling attention to it, namely, that the automatic power has left me, and almost the last anagram I received was

Wvfs yoitet — testify, vow.

SCIENTIFIC EXPLANATION OF WHY THE POWER HAS CEASED.

The ever present desire not to write a letter which I expect beforehand, acts as a neutraliser to all power of writing. Seeing myself about to form a letter, say o, I fear that I have expected it, thereupon the pen at once stops, or sometimes, between the opposing tendencies, describes a curve. This fear was not thought of in the first experiment, and reasoning upon the phenomena has since paralysed the power of producing them.

SPIRITUALISTIC INTERPRETATION.

The spirit is weary of my unbelief, and I am weary of her coquetry.

Note 1. I simply took a pen into my hand. Since, I have tried with the Planchette, but without any success.

Note 2. I have never known any one named Clelia.

Note 3. I have not been in the habit of writing anagrams, though I have done so in boyhood.

It will be observed that Mr. A., though quite alive to the mode of interpretation which we have here adopted, retains himself some little doubt as to the intervention at one point of an external agency. This seems mainly owing to the subjective sensation of an *agitation* or *insistance*, which seemed to him too powerful to have existed in his own mind entirely below the threshold of consciousness. Were such agitation never more marked than in the present case, it need not, perhaps, interpose much difficulty; but we shall come to instances where it becomes dominant and violent to a perplexing extent. At present we must merely notice that the stream of automatism does not always run smoothly on.

We now come to the case where several persons, with their hands

united on the Planchette, obtain answers which are not consciously present to the minds of any one of them.

But here we must first ask ourselves: Is this in reality a new case? Will it not be simpler to suppose that one agent alone is concerned, and that the rest are merely passive?

Undoubtedly this would be simpler; and this would be what we might have expected. But actual experiment seems to show that there are cases where motion does not ensue when either A or B places his hand on Planchette, but does ensue when both so place their hands. If the motion which ensues is of a merely random kind—no definite words being written—we may refer it to a mere summation of two ordinary unconscious muscular actions, either of which actions singly was unable to move the machine. But if definite words are written it is hard to explain this by a mere *summation* either of physical or psychical influences. Why should the same word arise in both minds? Why should both hands move automatically in the same direction? If A's hand will not write a word when he alone touches the Planchette, why should the superposition of B's finger—equally powerless alone—lead to the production of rapid sentences?

It may still be suggested that B's finger may contribute just the *initial shove* which A needs in order to set his own more complex automatism going. But this supposition would hardly meet the cases where Planchette will write *for no one but A, and for A only just so long as B touches it also;* stopping short in its vigorous career the instant that B's finger is removed. Here the first shove has long since been given; the writing has begun; yet B's withdrawal stops it, although B can get no writing *himself*, either alone or with C, D, &c.

In cases like this where a novel effect is produced by the juxtaposition of two quantities whose simple summation is not antecedently seen to be capable of producing the said effect, two hypotheses suggest themselves. The summation of the two quantities may raise their actual volume to a point necessary for the appearance of some new force or quality, which may have been virtually or actually present in each quantity severally, though in a degree beneath our limit of observation. Or, again, the juxtaposition may in itself involve alteration as well as addition; the two bodies may influence each other and give rise to phenomena which neither of them could have originated alone.

The Spiritualist theory of Planchette-writing assumes the former of these two hypotheses,—something in this way. There is an external agency which desires that the word "Byron" should be written. It acts by possessing itself of a certain internal force, which varies in each individual. That force in A is insufficient to enable the "spirit" to write the word through A's hand. The same is the case with B.

But when the two hands can be used to guide the same pencil, the "spirit" uses the sum of both forces, and the pencil moves. More than two persons may be necessary. C's force and D's force may have to be added to A's and B's. On the other hand, E's force may be sufficient by itself to enable the external agency to write the word, either by means of Planchette, or by direction imposed upon the pen.

On the other hand, I shall, at present, prefer to conjecture that there may have been some development of a new energy of a telepathic character. In some unknown way, I would suggest, the two minds are acting on each other, and the writing is a resultant of their unconscious mutual play. Neither theory is at present capable of direct proof. They are merely of use to direct our observation to the important points in the cases submitted to us. Here is another from Mr. H. A. Smith, who has had the patience to analyse many of these communications, which, be it observed, are generally quite equally puerile what ever be the operator's own level of intelligence.

Mr. Smith and his nephew placed their hands on the Planchette, and a purely fantastic name was given as that of the communicating agency.

Mr. Smith asked:

"Where did you live?" A. "Wem." This name was quite unknown to any of us. I am sure it was to myself, and as sure of the word of the others as of that of any one I know.

Is it decided who is to be Archbishop of Canterbury? A. Yes.

Who? A. "Durham." As none of us remembered his name, we asked.

What is his name? A. "Lightfoot." Of course, how far the main statement is correct, I don't know. The curiosity at the time rested in the fact that the name was given which none of us could recall, but was found to be right.

Now, this is just one of the cases which a less wary observer might have brought forward as evidence of spirit agency. An identity, it would be said, manifested itself, and gave an address which none present had ever heard. But I venture to say that there cannot be any real proof that an educated person has never heard of Wem. A permanent recorded fact, like the name of a town which is to be found (for instance) in Bradshaw's Guide, may at any moment have been presented to Mr. Smith's eye, and have found an unobtrusive lodgment in his brain.

The phenomena of *dreams* should teach us to be slow to draw negative conclusions as to what facts may *not* have thus imprinted themselves unconsciously on the sensorium. For we find, by the accounts sent to us, that a class of dreams on which Abercrombie and others have laid stress, tends to recur pretty frequently at the present day. The type is this: A man has mislaid an

important letter. He looks in all likely places in vain; then dreams that it is in some special and very *unlikely* place; and finds it there when he wakes. Now here we must assume that he has put the letter in the unlikely place in what we call "a fit of absence"; but that the distraction of mind which has prevented the act from forming part of his current of normal memory has *not* prevented it from recording itself in his brain, and reappearing in sleep, when the correlated elements (so to say) in his brain have been excited by his fruitless search.

I hold, therefore, that the word "Wem" may have been already lodged in the mind of one of the writers, and may have presented itself at this particular moment by what is called *accident;* that is to say, by virtue of some still more *underground* form of that same sub-conscious action which often brings some well known but irrelevant name or word suddenly into our consciousness, especially in moments of inaction or drowsiness.

In the answers "Durham" and "Lightfoot," we have the same kind of *attempt to meet expectation* which we noticed in Mr. A's case. This reminds us of cases where in a dream we ask a question with vivid curiosity, and are astonished at the reply; which nevertheless proceeds from *ourselves* as undoubtedly as does the inquiry.

This preliminary discussion has, it may be hoped, left us with some notion of the kind of opportunity which Planchette-writing affords for telepathic action. We have, perhaps, formed the conception of an obscure ocean of sub-conscious mental action;—hidden waves whose shifting summits rise for a moment into our view and disappear. And we shall not be surprised to find that any influences which may habitually stream from one mind to another may manifest themselves here more *easily*, though perhaps more *confusedly*, than in the upper air of our full waking intelligence.

I will begin with a case where the intermingling of telepathy with ordinary unconscious cerebration is doubtful, but not impossible.

From Mr. H. Arthur Smith, 2, New Square, Lincoln's Inn, W.C.

Present—H. A. Smith (A), R. A. H. Bickford-Smith (B), another gentleman (C), and two ladies (D and E).

R. A. H. B.-S. having, on previous occasions, exhibited considerable aptitude for automatic writing with a Planchette, it was designed to apply this instrument as a means of testing the transference of thoughts. No exact record having been made at the time of the whole of the results obtained, it would be of little service now to record isolated instances of success. Sometimes names thought of were correctly reproduced, sometimes not; but the proportion of successes to failures cannot now be accurately stated. The following incident, however, very much struck us at the time, and seems worthy of record.

Our method of procedure at the time was as follows:—C, sitting at one

end of the room, wrote down a name of an author, showing it to no one in the room ; B had his hands on the Planchette, no one else being in contact with him or it. C fixed his attention on the written name, and our design was to see whether that name would be written through the medium of the Planchette. The ladies were meanwhile sewing in silence, and taking no part in the experiments. It happened that one of the ladies had at the time, owing to some painful family circumstances, the name of a gentleman (not present) painfully impressed on her mind. The name was not a common one, and though all present knew something of the circumstances, they had not been mentioned during the evening, and no one had mentioned the name in question, which we will call "Bolton." C then wrote "Dickens" on his paper, and was "willing" B with all his might to write this, when, to the surprise of everyone, Planchette rapidly wrote "Bolton." This was not only surprising to us, but painful ; and no comments were made at the time, the subject being changed as rapidly as possible. It would appear from this that the effect of C's volitional concentration was overmatched by the intensity of the lady's thought, though not directed to the same object.

H. ARTHUR SMITH.

April 27th, 1883.

Now here I think that it is possible that the lady's mental action may have contributed, as Mr. Smith supposes, to the very result which she so little desired. Perhaps, however, the word written may have emanated from the unconscious cerebration of the *writer* alone.

In our next case, assuming the accuracy of the account, we are almost forced to adopt the telepathic hypothesis. The conditions are much the same as in our own recorded experiments ;—the minds of the agents, that is to say, are intently fixed on some word or picture, and the percipients (who are represented by the unconscious writers with the Planchette) reproduce a corresponding word or initial. The account is from Mrs. Alfred Moberly, Tynwald, Hythe, Kent, and it is corroborated, with some additional examples, by two other ladies present at the time.

May 9th, 1884.

The operators were placed out of sight of the rest of the company, who selected—in silence—a photograph, one of an albumful, and fixed their attention on it. We—the operators—were requested to keep our minds a blank as far as possible and follow the first involuntary motion of the Planchette. In three out of five cases it wrote the name or initial or some word descriptive of the selected portrait. We also obtained the signatures to letters selected in the same manner. We both knew perfectly well that *we* were writing—not the spirits, as the rest of the company persist to this day in believing—but had only the slightest idea what the words might prove to be.

We have tried it since, and generally with some curious result. A crucial test was offered by two gentlemen in the form of a question to which we couldn't possibly guess the answer. "Where's Toosey?' The

answer came "In Vauxhall Road." "Toosey," they explained, was a pet terrier who had disappeared; suspicion attaching to a plumber living in the road mentioned, who had been working at the house and whose departure coincided with Toosey's.

Of course, in the case of the inquiry after the lost dog, we may suppose that the answer given came from the questioner's own mind. Mrs. Moberly and her friends seem to have been quite aware of this; and were little likely to fall into the not uncommon error of asking Planchette, for instance, what horse will win the Derby, and staking, perhaps, some pecuniary consideration on the extremely illusory reply.

I shall devote the next case—the last which I shall cite at present—to the illustration of this special point, namely, the manner in which the questioner's own anticipation is reflected in the reply, even when that reply professes itself to come, or is supposed by the experimenters t come, from some independent source. And since it is always satisfactory if one can illustrate one's own view by a statement whose author intended it to prove a view directly *opposite*, I will take a case which was sent to Professor Barrett by a convinced Spiritualist, as a proof of the reality of intercourse with the departed. The names were stated but as I am citing the narrative in a sense differing from that which its writer meant it to bear, I will not now give them, and will only say that all three persons concerned are of very good position.

March 22nd.

One evening, a few years ago, I had with me two young friends, Mademoiselle de P., now Lady S., and Mademoiselle de P——n, her cousin, who is Grande Gouvernante to the daughters of the Crown Prince of Germany, both complete unbelievers in Spiritualism. To amuse them, however, as I sometimes write under occult influence, I asked Mademoiselle de P., to fix her thoughts on some one I did not know, to see whether my hand would write something true concerning him or her. She did as I requested, and soon my hand wrote, "His life has been overshadowed by the act of another." She looked astonished and said that the person she was thinking of had had a brother to whom he was much attached, who had committed suicide.

She then asked if she could be told where she had met him for the first time. My hand wrote, "It was at the foot of a marble staircase splendidly illuminated by a July sun; as you went up he gazed after you as one gazes on the track of a dazzling meteor." This also was correct; she had met him, she said, for the first time at the foot of the staircase of the Ministère de la Guerre, in Paris, and her cousin added that he had been much struck with her. The only inaccuracies were that the staircase was not a marble but a stone one, and that it was a *September* sun that shone.

When I write in this way the ideas do not come (*consciously* at least) from my mind, and my hand seems to be gently moved by some external influence.

Now I confess that this description of the staircase, and the meteor, and so forth, suggests to me as its source, not so much a male spirit disembodied as a female spirit still in the flesh.

The romantic tone of the communication seems to reflect the mood of the persons present; and the slight inaccuracies are just such as we have found to occur in the experimental transference of *words*, &c., from one mind to another.

But here I must, for the present, pause. We have reached a stage at which the nature of the problem presented to us is somewhat changing. Up to the present point we have been aiming mainly at two things. The operation of unconscious cerebration in automatic writing has been illustrated with some fulness; and it has thus been shown that *some* of the effects which Spiritualists ascribe to spirits are referable to the unconscious action of the writer's own mind.

And we have seen unconscious cerebration begin to blend, as it were, into telepathic influence, and have noted certain cases where, judging by the analogy of our previous experiments, Thought-transference suggests itself as the most probable explanation.

In a second paper I hope to give some more strongly-marked cases of telepathic influence, showing itself through automatic writing and other unconscious muscular movements. It has been by tracking the writer's own unconscious cerebration as far as it would carry us that we have begun clearly to recognise a limit beyond which that explanation no longer holds good. We have next to explore the full extent of the field which telepathic communication covers, and to consider whether there is still a residuum of phenomena which need the intervention of intelligences other than the intelligences of living and breathing men.

PROCEEDINGS OF THE GENERAL MEETING ON

December 30, 1884.

The eleventh General Meeting of the Society was held at the Rooms of the Society of British Artists, Suffolk-street, Pall Mall, on Tuesday, December 30, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

II.

The President opened with the statement of a proposed change in the relations between the Council of the Society and the investigating Committees. The Council, he said, had come to the conclusion that it was desirable that, for the future, the responsibility for both the facts and the reasonings in papers communicated to the Society, and published in the *Proceedings*, should rest entirely with their authors, and that the Council, as a body, should refrain from expressing or implying any opinion on the subjects thus brought forward. The reason for this change was to be found in the position which the Society had now reached. On the one hand, by the work which had been accomplished it had obtained the adhesion of a certain number of men of scientific reputation, whose services on the Council would be of the highest value, but who, though generally sympathising with the Society's aims and methods, would be reluctant to pledge themselves to the conclusions of any investigation in which they had not personally been concerned. On the other hand, a necessity was now strongly felt of concentrating effort on the most difficult and obscure part of the task originally undertaken—namely, the phenomena of so-called Spiritualism. In this region it could hardly be hoped to maintain, even in the Council itself, the amount of agreement which had been arrived at in the investigation of Thought-transference and Mesmerism.

Professor Barrett read a paper on certain narratives which had been received from various correspondents, containing some remarkable instances of success in the "willing-game," and also a few cases of apparent Thought-transference without contact. In this paper, and in the discussion that followed it, special attention was called to the conditions which must be fulfilled in order to give the records of such experiments an evidential value. A careful account of the experiments should be written down *at the time* that they are made; and this account should *in every* case include the *whole set* of trials made at that time, failures

as well as successes. The importance of excluding contact, or any other opportunities of unconscious indications, was again emphasised.

Mr. Edmund Gurney then gave a brief abstract of the following paper :—

III.

M. RICHET'S RECENT RESEARCHES IN THOUGHT-TRANSFERENCE.

The number of the *Revue Philosophique* for December, 1884 (Félix Alcan, éditeur, 108, Boulevard St. Germain, Paris), contains an account, by the well-known *savant*, M. Ch. Richet, of some most interesting and original experiments in Thought-transference, conducted by himself and a group of his friends. The title of his paper is "La Suggestion Mentale et le Calcul des Probabilités." By *mental suggestion* he means exactly what we term Thought-transference—the communication of ideas from one mind to another otherwise than through the recognised sensory channels.

Beginning with some general remarks on the limitations of our knowledge, M. Richet shows the unwisdom of attempting to set limits to the possibilities of Nature on *à priori* grounds, and the importance of keeping the mind open to evidence for novel facts. He further points out that experiments in delicate psychical phenomena necessarily differ from experiments on the matter and forces of the inorganic world, in that the results can never be predicted with certainty. When it has once been discovered that iodide of sodium is decomposed by chlorine, this result can for ever after be produced with absolute certainty under certain simple conditions. But in the case of Thought-transference, the conditions of success are inscrutable and unstable; out of 20 trials, the faculty, even if it exists, may only once take appreciable effect.

After this brief preface, M. Richet comes to the details of his own investigation. And here his originality does not immediately show itself in the *form* of the experiments. They are divided into three main groups; and in the first of these the plan was one which has been repeatedly adopted by our own Committee on Thought-transference. A card being drawn at random out of a pack, the "agent" fixed his attention on it, and the "percipient" endeavoured to name it. The novelty of M. Richet's method was this; that though the success, as judged by the results of any particular series of trials, seemed slight (showing that he was not experimenting with what we should consider "good subjects"), he made the trials on a sufficiently extended scale to bring out the fact that the right guesses were *on the whole*, though not *strikingly*, above the number that pure accident would account for, and that their total was considerably above that number.

This observation involves a new and striking application of the calculus of probabilities. M. Richet takes advantage of the fact that, the larger the number of trials made under conditions where success is purely accidental, the more nearly will the total number of successes attained conform to the figure which the formula of probabilities gives. For instance, if some one draws a card at random out of a full pack, and before it has been looked at by any one present, I make a guess at its suit, my chance of being right is of course 1 in 4. Similarly if the process is repeated 52 times, the "probable" number of successes, according to the strict calculus of probabilities, is 13; in 520 trials the probable number of successes is 130. Now if we consider only a short series of 52 guesses, I may be accidentally right many more times than 13, or many less times. But if the series be prolonged —if 520 guesses be allowed instead of 52—the actual number of successes will vary from the probable number within much smaller limits; and if we suppose an infinite prolongation, the proportional divergence between the actual and the probable number will become infinitely small.* This being so, it is clear that if, in a very short series of trials, we find a considerable difference between the actual number of successes and the probable number, there is no reason for regarding this difference as anything but purely accidental; but if we find a similar difference in a very long series, we are justified in surmising that some condition beyond mere accident has been at work. If cards be drawn in succession from a pack, and I guess the suit rightly in 3 out of 4 trials, I shall be foolish to be surprised; but if I guess the suit rightly in 3,000 out of 4,000 trials, I shall be equally foolish *not* to be surprised.

* M. Richet mentions one condition as calculated to interfere with the rule of pure accident, in a series of guesses made in the way described. He remarks, truly enough, that the guesser, if *told*, after each guess, whether he has been right or wrong, may have an unreasonable instinct that the next card or suit will be different from the last; and he adds: "Mais cet instinct trompe plus souvent qu'il ne sert, de sorte qu'en règle générale, on dit moins bien quand on devine, que quand c'est le hasard qui parle pour nous." But whatever goes on in the guesser's mind—even if he laboriously follows some complex system, as is so frequently done by gamblers, still, if the cards are drawn absolutely at random, the rightness or wrongness of the guess must remain a matter of pure accident. For the rightness or wrongness of the guess involves a relation between *two* things, of which the guesser can only control *one*. The drawer supplies a card; the guesser supplies a card's name; correspondence between the two events means a success. Now, *ex hypothesi*, nothing but pure accident governs the first event—the drawing of the card; therefore, any guess that is made has exactly the same chance of being right as any other, whatever be the principle or no-principle on which the guesser acts. And if his mental processes are irrelevant to his chance of success in each particular case, they are, of course, equally so to the percentage of his successes in the aggregate.

Now M. Richet continued his trials until he had obtained a very large total; and the results were such as at any rate to suggest that accident had not ruled undisturbed—that a guiding condition had been introduced which affected in the right direction a certain small percentage of the guesses made. That condition, if it existed, could be nothing else than the fact that, prior to the guess being made, a person in the neighbourhood of the guesser had concentrated his attention on the card drawn. Hence the results, so far as they go, make for the reality of the faculty of "mental suggestion." The faculty, if present, was clearly only slightly developed, whence the necessity of experimenting on a very large scale before its genuine influence on the numbers could be even surmised.

Out of 2,927 trials at guessing the suit of a card, drawn at random, and steadily looked at by another person, the *actual* number of successes was 789; the *probable* number, had pure accident prevailed, was 732. The total was made up of 39 series of different lengths, in which 11 persons took part, M. Richet himself being in some cases the guesser, and in others the person who looked at the card. He observed that when a large number of trials were made at one sitting, the aptitude of both persons concerned seemed to be affected; it became harder for the "agent" to visualise, and the proportion of successes on the guesser's part decreased. If we agree to reject from the above total all the series in which over 100 trials were consecutively made, the numbers become more striking. Out of 1,833 trials, we then get 510 successes, the probable number being only 458; that is to say, the actual number exceeds the probable number by about $\frac{1}{10}$. Some further experiments, where the particular card, and not merely the suit, was to be guessed, gave a number of successes slightly, but only slightly, above the probable number. Out of 782 trials the actual number of successes was 17, while the probable number was 15; and there was a similarly slight excess over the probable number when only the *colour* of the card was to be guessed; whence M. Richet surmises that there may be a field of choice too wide, and a field of choice too narrow, for the influence of "mental suggestion" to have effective play.

Clearly, however, no definite conclusion could be based on such figures as the above; they at most contain a hint for more extended trials, and would be valueless if they stood alone. M. Richet himself is careful to state this in the most explicit way; we shall find, indeed, as we proceed, that he sometimes carries caution of tone to a point which may not be excessive, but which is certainly extreme. At the same time he appears to me to press the calculus of probabilities to a point where it ceases to be instructive, when he deduces from these results a provisional estimate of the probability of "mental suggestion" as a fact in Nature

Taking the number of successes on the whole as about $\frac{1}{10}$ (apparently misprinted $\frac{1}{6}$) above the probable number, he concludes that "mental suggestion is, in a certain degree, probable, but with a degree of probability of only about $\frac{1}{10}$." Such a statement could only have a practical meaning, if we could view the results in oblivion of all the legitimate objections to which the hypothesis of so novel a faculty is exposed. Nor, indeed, is any immediate estimate, of the sort that M. Richet seems to intend, possible; for when a result is due to either A or B (*i.e.*, here to either chance alone or some further cause) the calculation of the probability that B has produced it cannot be made without assuming some value (as small as we please) for the *a priori* probability that B exists. It seems better, therefore, to regard a result like M. Richet's simply as indicating the most probable measure of the influence of the faculty (for these cases) *if* it exists, rather than as measuring the probability of its existence.*

The persons who took part in the above experiments, as has been said, were not specially distinguished by any natural or acquired aptitude for "mental suggestion."† M. Richet regrets that he has not been able to experiment in the same way with any really sensitive "subject;" and at this point he falls back on the reports of our own Committee on Thought-transference. He dwells specially on the case reported in the Proceedings, Part I., pp. 22, 23, where cards, drawn at random from a pack, and seen only by the members of the investigating Committee, were correctly named by the percipients in 9 out of 14 trials. But while admitting that our results, if genuine, are more conclusive than his own, he rightly prefers to take his stand on trials which he has personally supervised.

In his next batch of experiments, cards were replaced by photo-

* In n trials, with a probability p of success in each, the most probable number of accidental successes is np, which would give $n-np$ failures. If, then, the number of successes in excess of np be represented by a, the fraction which expresses the most probable measure of efficiency of an unknown cause, supposed to exist, is $\frac{a}{n-np}$ (not $\frac{a}{np}$, which would represent M. Richet's $\frac{1}{10}$).

† We are often asked by acquaintances what they can do to aid the progress of psychical research. These experiments of M. Richet's suggest, at any rate, one answer; for they can be repeated, and a valuable contribution made to the great aggregate, by any two persons who have a pack of cards and a little perseverance. One person should draw a card at random from a pack, and regard it steadily; the other should try to guess it, and his success or failure should be silently recorded. The pack should be shuffled after each trial, and the number of trials made at any one time should be limited to 50. The total number of trials contemplated (1,000, 5,000, 10,000, or whatever it may be) should be specified beforehand; and in order that the guesser's mind may be in as blank and receptive a state as possible, he should not be allowed to see the record of results until the whole series is completed.

graphs of landscapes, pictures, statues, &c., all mounted in an absolutely similar manner—the object of the change being to occupy the eye and mind of the "agent" with something more vivid and interesting than a mere playing-card. Different numbers of these photographs were used on different occasions; and as M. Richet gives the probability of a right guess as $\frac{1}{4}$, $\frac{1}{5}$, &c., according to the number employed, it is to be inferred that the guesser was always accurately informed, before the experiment began, what his field of choice was. Out of 218 trials of this class, in which the probable number of successes was 42, the actual number was 67. This total consisted of 28 series; and in only 6 of these did the actual number of successes fall below the probable number.*

We pass now to the second main division of M. Richet's work. Here he boldly confronts the possibility that the impression made by "mental suggestion" may act, not on the conscious but on the unconscious part of the percipient's mind—"sur les facultés inconscientes de l'intelligence." That this is what often happens in the case of spontaneous telepathic impulses has been more than once suggested in the

* M. Richet has calculated the probability of the number of right guesses actually given in each of these 28 series. In one particularly successful case, it was as small as $\frac{1}{2600}$; in another it was $\frac{1}{130}$; in another $\frac{1}{100}$, and in others some larger fraction. In strictness, what we want, for each case, is a fraction expressing the probability, not of that *one particular result*, but of *at least that amount of success;* which fraction may then be compared with the fraction of ideal probability, $\frac{1}{4}$. M. Richet proceeds to illustrate his results in the following way. He supposes a row of urns containing balls, the number in each corresponding to the denominator of one of the fractions. Thus there will be one urn with 2,600 balls; a second with 130; a third with 100; others with various lower numbers; and several with only one, which is to express the wholly unsuccessful series where not a single right guess was given. Each of the urns contains one white ball; and in those which contain more than one ball, the rest are all black; except in the sixth urn where three are white and one black, to correspond with a series where the actual number of right guesses fell below the probable number. M. Richet then asserts that the probability of his results may be represented as the probability that a person who draws a single ball at random out of each urn will in every case draw a white one. Now even if the number of balls in each bag were made to correspond with the correction given above, this method of representing the *totality* of the success seems clearly illegitimate. The error may be most easily seen if we consider the case of the sixth urn. In that urn there are several white balls; but still the chance of drawing a white ball from each of the first six urns must clearly be less than that of drawing a white ball from each of the first five. That is to say, the urn representing a series where the amount of success was *below* the probable amount that accident would give, is made to *heighten* the probability that something beyond accident was at work. This illustration, however, is in no way vital to M. Richet's argument.

reports of our Literary Committee.* Experimental analogies are, therefore, of extreme interest to us. Now, if we are to have experimental evidence of an *unconscious* mental impression, received from another mind, it must show itself in some bodily affection, either sensory or motor. In our *Proceedings*, Part VI., pp. 203-4, a *sensory* case is given. The fingers of a "subject" having been concealed from him by a paper screen, anæsthesia and rigidity were repeatedly produced in one or another of them, by a process in which the concentrated attention of the "agent" on the particular finger proved to be an indispensable element. A psychical account of this result seems possible, if Thought-transference can work, so to speak, underground. And as specimens of the *motor* affection, we have in our collection several cases where a mental question on the part of some one present has been answered in writing, with a *planchette* or a simple pencil, without any consciousness of either the question or the answer on the part of the person whose hand was automatically acting.† It is to the *motor* form of experiment that M. Richet's contributions belong. But he has developed it in a quite novel way; and here again, as in the case of the card-guessing, he has brought the calculus of probabilities to bear effectively on various sets of results, which, if looked at in separation, would have had no significance. The "subjects" of the experiment, as has been said, were persons possessing no special aptitude for "mental suggestion"; and this being so, it was clearly desirable that the bodily action required should be of the very simplest sort. The formation of words by a *planchette*-writer requires, of course, a very complex set of muscular co-ordinations: all that M. Richet sought to obtain was a single movement or twitch. He accordingly applied the principle of the divining rod, in the following ingenious way. The two ends of a wand which had a certain appreciable flexibility, were held by the would-be "percipient" in his two hands. An article having been hidden by the "agent" in one of a certain number of specified places (underneath the box of one of a row of orange trees in a garden near Paris), the hiding place was revealed to the percipient as he passed in front of it, by a move-

* *Proceedings*, Part II., p. 140, where it is remarked how the effect of such an impulse seems sometimes to remain latent for several hours, until a season of stillness and passivity allows it to emerge into consciousness. See also Part VI., pp. 121 and 171. For cases where the emotional or volitional condition of one person seems to have produced unusual actions in another person at a distance, see Part VI., pp. 124-126.

† Some of these cases have been cited above by Mr. Myers, and others will be given in the sequel to his paper. Mr. Myers and the present writer have both witnessed the phenomenon, under conditions which left no possible doubt of its genuineness.

ment of the wand due to an extremely slight muscular tremor which brought his two hands nearer together. In 25 trials, where the number of successes, according to the calculus of probabilities, should have been 4, the actual number was 12. One of the diviners, apparently the most sensitive of the group, obtained 4 successes in 4 trials, the probability of each success being $\frac{1}{6}$; M. Richet himself, as diviner, under similar conditions, obtained 6 successes in 13 trials. He made some more trials of the same sort, using the shelves of his bookcase as the hiding-places; and again, hiding the object on the person of some one present. Taking the total results of these three sets, we find that out of 67 trials, where the probable number of successes was 16, the actual number was 34. The improbability of obtaining such a result by accident is enormous; and though M. Richet is again careful to point out that an immensely larger number of trials would be necessary before the influence of "mental suggestion" could be held to be proved by such a method, the importance of the method is incontestable.* It is, however, one which needs very special precautions, owing to a danger to which M. Richet is fully alive—the possibility of unconscious indications. Hints from the expression or attitude of the "agent" may be prevented by blindfolding the "percipient" and in other ways; but if the two are in close proximity, it is harder to exclude such signs as may be given by involuntary movements, or by changes of breathing.

The form of the experiment was again varied by placing a number of toy-pictures—of medals, sabres, scales, animals, &c.—in a row on a table; the diviner then, passing along the row, had to discover from the movement of his wand on which of these objects the agent had fixed his thoughts. The actual success here was only slightly above what accident might probably have given. But there were special points of interest in several of the failures. The set of objects contained only two pictures of medals; when one of these was the selected object, the other one was "divined." One trial was made with 78 cards, two of which represented a man on horseback, and one of these two was the selected object; the diviner narrowed down his field of choice to these two, but

* It may be worth while to remark that either the selection of the particular hiding-place ought to be settled each time by lot, or the percipient ought to be prevented from knowing whether or not his divination has been correct. Otherwise the chances of success may be really affected in the way which M. Richet imagined in the case of the card-guessing. If we allow the *mind* of the agent to govern the selection, then a process in his mind may find its counterpart in the mind of the percipient; for instance, the agent may decide to be a little crafty, and to hide the object twice running in the same place; and the percipient's mind, following a similar track, may reckon that this amount of craft has been exercised, or has not been exercised. Here then (as in the game of *mora*) we should have the operation of something which is neither mental suggestion nor accident.

finally guessed the wrong one. In 6 other cases, out of a total of 31, there was a similar close approach to correctness; and in 3 more, M. Richet, who was closely observing the process, believes that the error was due to the wrong interpretation of the movements of the wand—the object guessed being next in the row to the right one. If these facts were allowed for, the actual number of successes would be 10, while the probable number would be about 2. But M. Richet, with his usual candour and caution, admits the danger of applying the calculus to results which have been rectified in this manner. His reason for drawing attention to the close resemblance between the object guessed and the right object is the intrinsic interest of the fact. The explanation of this fact by the hypothesis of "mental suggestion" involves, however, certain difficulties which must not be ignored.

M. Richet does not inform us whether the agent was *watching* the diviner as he passed in front of the various objects. If he was, we should find the readiest telepathic explanation of the successes in a movement of his *will*, whereby the diviner's organism was affected. But then in that case the diviner's frequent selection of an object *closely resembling* the right object loses all significance; for the agent would not be *willing* the selection of the resembling object any more than of any other of the wrong objects. If, on the other hand, the agent was *not* watching the process, then we might perhaps conceive a connection between the idea of the *object* and the idea of *movement* to be firmly established in the agent's mind, and to be a preparation for the particular connection in the percipient's mind. We should, however, have to extend our view of the operations of Thought-transference in a decidedly novel manner. It is not merely that a composite idea, consisting of an image and an impulse, is transferred to the percipient's "unconscious intelligence"; but the one element, the impulse, is to reveal itself when—and only when—the other element, the image, finds its fellow in a conscious percept, the actual picture of the object.

But the above, I imagine, is not the way in which M. Richet himself conceives the event; for he says nothing about an impulse of will or an idea of movement. I infer from his language that in such cases he would consider the chance of success through "mental suggestion" to be just the same, if the agent were unaware of the precise form of experiment adopted, and exercised his will, not to produce a movement, but merely in the sense that every agent in experimental Thought-transference exercises it—that is, in a concentrated desire that the idea of the object in his mind should reappear in the percipient's mind. But then the percipient's sub-conscious or unconscious idea of a medal, or a man on horseback, or whatever it may be, could apparently have no more tendency to produce a movement than any other of the ideas, conscious, sub-conscious, or unconscious, of which, at the time, his

mind is the theatre; and it is hard to see what difference in this respect could be made by the fact that the idea had been transferred from the mind of the agent, instead of occurring spontaneously. But even if we grant—and it is very possible that in time we shall have to grant—that the mere fact of a telepathic impression may entail peculiar physiological effects, why should the effect of the unconscious idea manifest itself only at the moment when the percipient's mind becomes consciously occupied with a similar idea—that is, when his advance along the row brings him opposite to the actual picture of a man on horseback? To put the case briefly, if the idea of movement is not in the agent's mind, one can only attach psychical significance to the frequent selection of a wrong object which resembles the right one, by making two suppositions: (1) that the telepathic fact, which produces a physiological effect in the percipient apart from his consciousness, may be pictorial, not volitional—statical, not dynamical—in character; and (2) that the effect is produced only after a sort of parley has taken place between the conscious and unconscious parts of the percipient's mind. That these suppositions are not mere speculative curiosities will be plain when we proceed, as it is now time to do, to the last and most important division of M. Richet's work.

In the final group of experiments the place of the flexible wand was taken by a table: and M. Richet prefaces his account by a succinct statement of the scientific view as to "table turning." Rejecting altogether the three theories which attribute the phenomena to wholesale fraud, to spirits, and to an unknown force, he regards the gyrations and oscillations of séance-tables as due wholly to the unconscious muscular contractions of the sitters. It thus occurred to him to employ a table as an indicator of the movements that might be produced, by "mental suggestion." The plan of the experiments was admirably conceived. Three persons (C, D, and E) took their seats in the semi-circle at a little table on which their hands rested. One of these three was always a "medium"—that is to say, a person liable to exhibit intelligent movements in which consciousness and will took no part. Attached to the table was a simple electrical apparatus, the effect of which was to ring a bell whenever the current was broken by the tilting of the table. Behind the backs of the sitters at the table was another table, on which was a large alphabet, completely screened from the view of C, D, and E, even had they turned round and endeavoured to see it. In front of this alphabet sat A, whose duty was to follow the letters slowly and steadily with a pen, returning at once to the beginning as soon as he arrived at the end. At A's side sat B, with a note-book; his duty was to write down the letter at which A's pen happened to be pointing whenever the bell rang. Things being

arranged thus, the three sitters at the first table engage in conversation, sing, or tell stories; but at intervals the table tilts, the bell rings, and B writes down the letter which A's pen is opposite to at that moment. Now, to the astonishment of all concerned, these letters, when arranged in a series, turn out to produce rational words and phrases. M. Richet has not given us any examples of these words and phrases in his present paper; but he promises a future one, in which the omission will probably be made good. Meanwhile, how is such a phenomonen to be regarded? To whom or to what is it due? A and B are mere automata. C, D, and E are little more, being unconscious of tilting the table, which appears to them to tilt itself; but even if they tilted it consciously, and with a conscious desire to dictate words, they could not possibly succeed. For they have no means of ascertaining at what letter the pen is pointing at any particular moment, and they might thus tilt for ever without producing more than an endless series of incoherent letters, or at best meaningless syllables. It is surely hard to imagine a more convincing display of the *unconscious intelligence* on which M. Richet insists.

This phrase is no doubt a somewhat equivocal one, and it is necessary to know in every case exactly what is meant by it. It may be used in a purely *physical* sense—to describe the unconscious cerebral processes whereby actions are produced which as a rule are held to imply conscious intelligence; as, for instance, when complicated movements, once performed with thought and effort, gradually become mechanical. But it may be used also—as M. Richet seems to use it—to describe *psychical* processes which are severed from the main conscious current of an individual's life. Unconsciousness in any further sense it would be rash to assert; for intelligent psychic process without consciousness of *some* sort, if not a contradiction in terms, is at any rate something as impossible to imagine as a fourth dimension in space. The events in question are outside the individual's consciousness, as the events in another person's consciousness are; but they differ from these last, in that there is no continuous stream of conscious life to which they belong; and no one, therefore, can give an account of them as belonging to a self. They are essentially fragmentary, unappropriated, and inarticulate; and they can only be inferred to exist from certain sensible effects to which they lead.

But it may be asked what right we have to make any such inference; since, *à la rigueur*, the effects, being sensible and physical, do not require us to suppose that they had any other than *physical* antecedents. It is true that it is impossible to *demonstrate* that the physical antecedents, which undoubtedly exist, have any psychical correlative: but the dogmatic assertion that they have *not* loses much of its force from two considerations: (1) Analogy fails. The results in question are often wholly dissimilar to the effects of practice, and the various other auto-

matic actions which may be fairly attributed to "unconscious cerebration": they are *new* results, of a sort which has in all our experience been preceded by intention and reflection. I advance this view with some hesitation; but we shall find considerable justification for it in some of M. Richet's experiments. (2) The undoubted phenomena of what has been called "double consciousness" have familiarised us with the idea of a *double* psychical life connected with a *single* organism. In those cases the two lives which know nothing of each other are *successive*; but no new difficulty is introduced by conceiving fragments of such mutually exclusive existences as *simultaneous*.*

However, it is not with the proof of "unconscious intelligence," but of "mental suggestion," that we are here more immediately concerned. Subject to the above explanation, we may accept M. Richet's view on the former question, and confine ourselves to the bearing of his experiments on the latter. And in the case already described, we have already a pretty plain indication that something of the nature of "mental suggestion" was at work. For to whatever source we ascribe the rational words and phrases which B.'s note-book records, it is impossible not to admit that the idea of the letters necessary for their formation had an existence, though an unconscious one, in the mind or brain of one or more of the three sitters at the tilting table. The letters had at any rate the same existence for them as those written by the *planchette*-writer have for him, when he produces a string of words and sentences without knowing what they are. But the peculiarity of the present case is that the letters could only obtain this existence in the mind or brain of the "medium," (C, D, or E) *through* a "mental suggestion" from A. No one but A (or possibly B) knew the moment at which alone the next letter required could be caught and fixed; and the "medium's" involuntary seizure of that critical moment to tilt the table cannot possibly be disconnected from this knowledge in A's mind. In M. Richet's words, "The *unconscious person* in the 'medium'—whose conscious Ego is thinking of something else—follows mentally, with rigorous precision, the movements of the other who points to the successive letters."

* These suggestions of "unconscious intelligence" are drawn from facts observed in connection with a single individual. But they may be reinforced by a consideration drawn from the phenomena of telepathy. It has been more than once pointed out in our *Proceedings* that, while a coherent account can be given of telepathic phenomena on purely *psychical* ground, there are peculiar difficulties in conceiving any complete *physical* account of them. Now this remains just as true when the percipient is unconscious of the "mental suggestion" as when he is conscious of it. This being so, it seems specially arbitrary and unnecessary to insist on confining such phenomena to the physical plane—the very place where the difficulties are thickest—and to deny their psychical existence, in respect of which our ideas of them can at any rate be made orderly and consistent.

But the experiment was carried to a much further point than this; and in its further development the calculus of probabilities is again turned to most striking account. A sixth operator (whom we will call F) is introduced, who stands apart both from the tilting table and from the alphabet; and *his* thought is now the source of the "mental suggestions" which lead to the same connected series of events as before—the tilting of the table, the ringing of the bell, the fixing of successive letters, and the gradual formation of rational words in B's note-book.

For the sake of comparing the results with those which pure accident would give, M. Richet first considers some cases of the latter sort. He writes the word NAPOLEON; he then takes a box containing a number of letters, and makes 8 draws; the 8 letters, in the order of drawing, turn out to be U P M T D E Y V. He then places this set below the other, thus:—

N A P O L E O N
U P M T D E Y V

Assuming the number of letters in the alphabet to be 24, the probability of the correspondence of any letter in the lower line with the letter immediately above it is, of course, $\frac{1}{24}$; and the probability that in the whole series there will be one such success is $\frac{8}{24}$ or $\frac{1}{3}$. If we reckon as a success any case where the letter in the lower line corresponds not only with the letter above it, but with either of the neighbours of that letter in the alphabet* (*e.g.*, where L has above it either K, L, or M), then the chance of a single success in the series is three times greater than before; that is, it is 1. In the actual result, it will be seen, there is just 1 success, which happens to be a complete one—the letter E in the sixth place. It will not be necessary to quote other instances. Suffice it to say that the total result, of trials involving the use of 64 letters, gives 3 exact correspondences, while the probable number was 2.7; and 7 correspondences of the other type, while the probable number was 8. Thus even in this short set of trials, where accident had full scope, the experimental result very nearly coincides with the strict theoretic number.

We are now in a position to appreciate the results obtained when

* This procedure of counting neighbouring letters seems to require some justification. It might be justified by the difficulty, on the theory of mental suggestion, of obtaining an *exact* coincidence of time between the tilting and the pointing. But I think that M. Richet does justify it (p. 654) by references to the other more striking experiences of which he has not here given us any examples, where neighbouring letters also appeared, but where there seems to have been no room for doubt, in the reader's mind, as to what the letter should have been.

the factor of "mental suggestion" was introduced. In the first experiments made, M. Richet, standing apart both from the table and from the alphabet, selected from Littré's dictionary a line of poetry which was unknown to his friends, and asked the name of the author. The letters obtained by the process above described were J F A R D ; and there the tilting stopped. After M. Richet's friends had puzzled in vain over this answer, he informed them that the author of the lines was Racine; and juxtaposition of the letters thus—

J F A R D
J E A N R

shows that the actual number of successes (of the type where neighbouring letters are reckoned) was 3, the probable number being only $\frac{5}{6}$. One would be glad to know whether M. Richet was actually concentrating his thought on the author's Christian name. Even if he was not, it probably had a sub-conscious place in his mind, which might sufficiently account for its appearance. At the same time, accident has of course a wider scope when there is more than one result that would be allowed as successful.

It is, of course, better—with the view of making sure that F's mind, if any, is the operative one—not to ask a question of which the answer might possibly at some time have been within the knowledge of the sitters at the table ; and in the subsequent experiments the name was silently fixed on by F. Two more specimens may suffice.

Name thought of: D O R E M O N D
Letters produced: E P J Y E I O D.

Here the probable number of successes (of the type whose neighbouring letters are reckoned) was 1, and the actual number was 4.

Name thought of: C H E V A L O N.
Letters produced: C H E V A L.

Here the probable number of exact successes was $\frac{1}{4}$, and the actual number was 6.

Taking the sum of 8 trials, we find that the probable number of exact successes was a little over 2, and the actual number 14 ; and that the probable number of successes of the other type was 7, and the actual number 24. It was observed, moreover, that the correspondences were much more numerous in the *earlier* letters of each set than in the later ones. The first three letters of each set were as follows—

J F A—N E F—F O Q—H E N—C H E—E P J—C H E—A L L
J E A—L E G—E S T—H I G—D I E—D O R—C H E—Z K O

Here, out of 24 trials, the probable number of exact successes being 1, the actual number is 8 ; the probable number of successes of the other type being 3, the actual number is 17. The figures become still

more striking if we regard certain consecutive series in the results. Thus the probability of the 3 consecutive correspondences in the first experiment here quoted was $\frac{1}{512}$; and that of the 6 consecutive correspondences in the last experiment quoted was about $\frac{1}{160,000,000}$.

And now follows a very interesting observation. In some cases, after the result was obtained, subsequent trials were made *with the same word*, which of course the agent did not reveal in the meantime; and the amount of success was sometimes markedly increased on these subsequent trials. Thus, when the name thought of was D O R E M O N D, the letters produced on the first trial were E P J Y E I O D.

„	„	second	„	E P F E I.
„	„	third	„	E P S E R.
„	„	fourth	„	D O R E M I O D.

Summing up these four trials, the probable number of exact successes was just over 1, and the actual number was 9; the probable number of successes of the other type was just over 3, and the actual number was 18. The probability of the 5 consecutive successes in the last trial was about $\frac{1}{8,000,000}$. The complete accuracy of this last fraction may, however, be called in question; since M. Richet, who was the agent, tells us that he had imagined the name as spelt thus—d'Ormont. Even so, however, the amount of success is very large; and the admission introduces a new point of interest. It recalls to mind the numerous cases where automatic writers, or table-tilters, have obtained responses with mistakes or peculiarities of spelling, sometimes persistently repeated, to which their conscious selves were in no way prone. And if the analogy be a just one, then in M. Richet's experiments one might discover a hint of an "unconscious person" in the agent, as well as in the percipient.

The experiment was repeated 4 times in another form. A line of poetry was secretly and silently written down by the agent, with the omission of a single letter. He then asked what the omitted letter was; it was correctly produced in every one of the four trials. The probability of such a result was less than $\frac{1}{3,000,000}$.

A result of a different kind was the following, which is specially noteworthy as due to the agency of an idea that was itself on the verge of the unconscious. M. Richet chose a quotation at random from Littré's dictionary, and asked for the name of the author, which was Legouvé. The letters produced were J O S E P H C H D, which looked like a complete failure. But the quotation in the dictionary was adjacent to another from the works of Joseph Chénier; and M. Richet's eye, in running over the page, had certainly encountered the latter name, which had probably retained a certain low place in his consciousness. Another very interesting case of a result unintended by the agent, though due to something in his mind, was this. The

name thought of was Victor: the letters produced on three trials were

D A L E N
D A M E S
D A N D S

seemingly complete failures. But it appeared that while the agent had been concentrating his thoughts on "Victor," the name of a friend, Danet, had spontaneously recurred to his memory. We should, of course, be greatly extending the chances of *accidental* success, if we reckoned collocations of letters as successful on the ground of their resemblance to any one of the names or words which may have momentarily found their way into the agent's mind while the experiment was in progress. Here, however, the name seems to have suggested itself with considerable persistence, and the resemblance is very close. And if the result may fairly be attributed to "mental suggestion," then, of the two names which had a certain lodgment in the agent's mind, the one intended to be effective was ineffective, and *vice versâ*.

These latter results have very great theoretic importance. It is to be remembered that we found a certain difficulty in conceiving the telepathic production of movements by what is at most an idea, and not a volition, on the agent's part. But it is now difficult to resist the evidence that this is what actually has taken place; and even that the very idea may be below the threshold of conscious attention. Nor is it only in these exceptional cases that this startling hypothesis has to be faced. For we must remember that in a sense A is throughout more immediately the agent than F; it is what A's mind contributes, not what F's mind contributes, that produces the tilts at the right moments. But this is of course through no *will* of A's; he is ignorant of the required word, and has absolutely no opportunity of bringing his volition into play. His "agency" is of a wholly passive sort; and his mind, as it follows the course of his pen, is a mere conduit-pipe, whereby knowledge of a certain kind obtains access to the "unconscious intelligence" of the "medium." If, then, the knowledge manifests itself as impulse, can we avoid the conclusion that in this particular mode of access—in "mental suggestion" as such —a certain *impulsive* quality is involved?

It may perhaps be objected that F might produce the movements, in a similar way to that proposed above *à propos* of the cases where an object *resembling* the right one was divined. It was there suggested that in the agent's mind the idea of the *object* had been combined with the idea of the *movement*, and that this complex idea was what was transferred and what ultimately took effect. This explanation, if sound, might possibly be extended to the far more complicated cases where—as in the word-experiments—there are as many objects as there

are letters; and where, further, the actual perception of the resembling object, when the percipient comes opposite to it in a row, is replaced by an unconscious image of it received from a third mind. But it is surely hard to include under the notion cases where a word is produced which, though latent in F's mind, has no resemblance to the word whose production he is willing. The transference of the idea of the latent word, even to the exclusion of the right word, can be quite conceived; but can we suppose that, sub-consciously or unconsciously, an idea of *movement* was combined with the idea of its letters in the agent's mind, at the very moment when that on which his attention was fixed, and with which *ex hypothesi* the conscious idea of movement *was* connected, was a quite different set of letters? Can we suppose that the idea of movement overflowed into the unconscious region of his mind, and there on its own account formed an alliance with alien elements, the effect of which on the percipient would prevent the effect intended? It must be remembered that where a word which is not the one intended gets transferred from F to the "medium," there is no knowledge, conscious or unconscious, on F's part, as to what that word will be. A number of words are latent in his mind; one of these finds an echo in another mind. But how should the idea of movement find out which particular one, out of all the words, is destined thus to find an echo, so as to associate itself with *its* letters and no others? And if we suppose the association to be between the unconscious idea of movement and the unconscious idea of *letters in general*, this is no less dissimilar and opposed to anything that the conscious part of F's mind has conceived. For it is not in letters as such, but in the exclusive constituents of a particular word, that he is interested; if indeed he is interested in anything beyond the word as a whole. The difficulty here seems yet further to justify the suggestion—with which I imagine that M. Richet would agree—that the physiological impulse does not depend on any idea of *movement*, or any special direction of the agent's will to *that* result. This might be tested, if F were a person ignorant of the form of the experiment, and out of sight of the table.

But of course the relation between F and the "medium" does play a necessary part in the result; the impulse to tilt when a particular letter is reached only takes effect when it falls (so to speak) on ground prepared by "mental suggestion" from F—on a mind in which the word imagined by him has obtained an unconscious lodgment. And whereas we spoke above of "a parley between the conscious and unconscious parts of the percipient's mind," we shall now have to conceive the *unconscious* part, if not as dual, as at any rate the scene of confluence of two separate streams of influence, which proceed to combine there in an intelligent way—one proceeding from F's mind, which produces

unconscious knowledge of the word, and the other proceeding from A's mind, which produces an unconscious image of the successive letters. Such a conception seems to support what I said above, as to results of the supposed "unconscious intelligence" which go beyond the received results of mere unconscious cerebration. Unconscious cerebration is amply competent to produce such seemingly intelligent actions as ordinary writing; but what is now done more resembles the formation of a word by picking letters from a heap, or type-writing by a person who is unused to his instrument. The process is not one in which every item is connected by long-standing association with the one before and after it; every item is independent, and implies the recognition, at an uncertain moment, of a particular relation—that between the next letter required for the word and the same letter in its place in a quite distinct series. There is, no doubt, an alternative to the hypothesis just suggested; we might suppose that F's thought affects, not the "medium," but A, or conversely, that A's thought affects, not the "medium," but F; that A obtains unconscious knowledge of the word, or that F obtains unconscious knowledge of the letter, and so is enabled to communicate an impulse to the "medium" at the right moment. But we should then have to suppose a secret understanding between two parts of A's or F's mind, the part which takes account of the letters of the alphabet, and the part which takes account of the letters of the word—the former being conscious and the latter unconscious, or *vice versâ*, according as A or F is the party affected.*

One hesitates to launch oneself on such conceptions; but the only alternative would be to question the facts from an evidential point of view. So regarded, they are of an extremely simple kind; and if their genuineness be granted, we are reft once and for all from our old psychological moorings. The whole question of the psychical constitution of man is opened to its furthest depths; and our central conception —telepathy—the interest of which, even in its simpler phases, seemed almost unsurpassable, takes on an interest of a wholly unlooked-for kind. For it now appears as an all-important method or instrument for testing the mind in its hidden parts, and for measuring its unconscious operations.

M. Richet holds out hopes that on a future occasion he may return to this aspect of the subject, and treat the problems on a more extended scale. We ask for nothing better. Meanwhile, I cannot withhold an opinion that he decidedly underrates the general effect of what he has

* There is yet one other conceivable view of the process which, in spite of its desperate improbability, should not be left out of account—namely, that the "medium" perceives the movements of the pointer by unconscious *clairvoyance*. It would be worth while to try whether A is indispensable, and whether similar results could be obtained when the pointer is moved by mechanism.

so far accomplished. He concludes his account by a synopsis of his numerical results, as measured in three ways;—by the *actual number* of successes in the several series; by various instances of several *consecutive* successes; and by the *probability* of the amount of success in the more striking cases, which is represented by very small fractions.* Summing up from these data, though purposely keeping below the mark, he suggests that "the probability in favour of the reality of mental suggestion may be represented by $\frac{2}{3}$"; that is to say, if he were compelled to decide one way or the other, under pain of death if he were wrong, he would allow chance to decide for him in this way—that he would put two white balls and one black ball into a bag, and then draw at random; if he drew a white ball, he would declare for the reality of "mental suggestion"; if he drew the black one, he would declare against it. One cannot but think that if it came to the pinch, he would make his reckoning differently. Exception was taken above to interpreting a result which indicates the most probable efficiency of mental suggestion, *if* it exists, as the measure of the probability of its existence†; and with an increased number of trials, the illegitimacy of such a proceeding becomes still more manifest. Whatever be the number of trials, the *efficiency* of the new cause, as calculated from a certain ratio of the actual to the probable number of successes, will be the same; but if this ratio remains undiminished with a continually increasing number of trials, the probability of the *existence* of the new cause continually increases. Otherwise, results might go on for ever deviating in the same direction from the probable results of accident, and yet without producing even approximate certainty that anything beyond accident was at work. Suppose that a pair of dice are thrown a billion times in succession; and that the total result shows that half the total number of throws have been sixes. We should, of course, conclude that the dice were unevenly weighted; but on M. Richet's mode of reckoning, such a conclusion would only amount to a considerable probability, on which no sane man would willingly stake anything he cared about. In so extreme a case, to estimate the probability as falling appreciably short of certainty, would argue, not an excess of caution, but ignorance of a fundamental principle. M. Richet's estimate of the probability of "mental suggestion" is not, of

* These fractions are not absolutely correct, for the reason pointed out on p. 243. But the correct fractions would also be very small. The improbability of the *runs* of success is also overstated. Allowance should be made for the fact that they mostly occurred as part of a longer series; *e.g.*, the improbability of obtaining 5 successes running is less in a series of 10 than in a series of 5.

† This is practically what M. Richet has done in the present case. For he gives the fraction $\frac{2}{3}$ as approximately expressing the excess of the actual number of successes over the probable number, divided by the total number of *the trials.*

course, open to such a charge as that; but still he certainly seems to have shown caution of statement in the wrong place. Granted the *genuineness* of the results—granted, that is, the *bona fides* of all concerned and the perfection of the conditions—the probability that "mental suggestion" was at work, or in other words, the improbability that the results were purely accidental, is, when we consider the multitude of trials, something enormous. If it falls appreciably short of certainty—as to which opinions might differ—it still cannot possibly be represented by such a proportion as 2 to 1. So much may be stated boldly. The place for caution is rather in estimating the chances of a flaw in the conditions; and though speaking on this point with considerable confidence, M. Richet puts in more than once a judicious note of warning. He insists that the experiments must be repeated; and the importance of this cannot be too strongly urged. Even if the experimenter's assurance as to the perfection of his conditions be complete, it is in the nature of things impossible that strangers, who only read and have not seen, should be infected with it. It is on this side, then, that the case for "mental suggestion" needs fortifying; and the fortification (as has been frequently pointed out in these *Proceedings*,) can only consist in spreading the responsibility—in multiplying the number of persons, reputed honest and intelligent, who must be either knaves or idiots if the alleged transference of thought took place through any hitherto recognised channels.

It is needless to remark in conclusion that, such being the means by which an overwhelming proof may in time be attained, M. Richet's investigations form a most important step towards its attainment. His paper can hardly fail to be a permanent landmark in the slowly widening domain of psychical discovery.

NOTE.

On the very eve of our going to press, I have received the following communication from Professor Lodge and Mr. Alfred Lodge:—

Liverpool, January 8th, 1885.

DEAR GURNEY,—In cogitating over your remarks anent the valuable suggestion of M. Richet, that feeble thought-reading powers or slight mental reverberation may be possibly detected in most persons by applying the laws of probability to a great number of guesses made by them, at a limited series of objects, while under the influence (real or aginary) of another mind strongly cognisant of these objects in random

succession; we have emancipated ourselves from the fallacy under which M. Richet appears to labour, and which you have already detected, and think that a statement of our present position may be not uninteresting.

The evidence in favour of an unknown law, which can be legitimately deduced from a definite departure from probability in a large number of cases, is an important matter in connection with such experiments, and it is by no means obvious what such evidence really is.

Thus M. Richet finds, as the result of a number of series of guesses, that the number of successes actually attained was in a large majority of cases above the theoretically probable number; and he sums up, as the cumulative evidence of the whole number, an extraordinarily great probability in favour of law, in an illegitimate manner. The inference to be drawn from a number of series of varying success will seldom be greater than that to be drawn from the most remarkable of those series;* but, indeed, no single unsupported series of a few trials can afford any legitimate inference at all.

The whole validity of the method depends on a great number of trials being made, and before any short series can be used as an argument it must be confirmed again and again by repetition. But the division of the whole number of trials into series of so many guesses each is unnecessary and deceptive.† All the series, or at any rate all

* This may no doubt be true, if "varying success" be taken to include results where the number of right guesses is *below* as well as *above* the theoretically probable number. But suppose that a die has been thrown 600 times, in 100 batches of 6, and that in each of these batches the ace has appeared either 2 or 3 times, except in the first batch, where it appeared 4 times. The presumption that the die is unevenly weighted is of course stronger at the end of the 600 throws than it was at the end of the first 6. And this is a sort of illustration which M. Richet might perhaps consider fairly justified by his results.

† Supposing the odds against each individual success to be constant throughout (which was not the case in M. Richet's sets of trials), the division into series is, of course, irrelevant to the calculation of the *à priori* probability that chance will produce a certain total of successes. But when we look at certain results obtained, with a view to discovering the probability that something beyond chance has acted, the mode of distribution may sometimes have a real importance. For, supposing a *vera causa* to exist, by which the actual number of right guesses is made to exceed the probable number, there is no *à priori* ground for assuming that it would act certainly or uniformly. Suppose that 1,000 guesses at the suit of a card are made on M. Richet's plan, in batches of 200 each, on five successive days; and suppose that all the 200 guesses are right on the first day, and that 50 more right guesses are dispersed over the other days,—the total number of right guesses, 250, will then not exceed the

those where the probability of success at each individual guess was the same, constitute one large series, which is only available for legitimate inference if it consist of a sufficient number of trials.

This expression "sufficient number" is, however, only susceptible of subjective and psychological definition; rigidly, an infinite number would be necessary for perfect inference, but practically a large number serves.

The result at which we have arrived may thus be stated for practical purposes.

Let a series of m guesses be made at a set of n things, and let r of these guesses come right. The inference to be drawn is, that the probability in favour of law or bias, as opposed to pure chance, is,

$$1-\left(\frac{2m-r-k+1}{(n-1)(k+r+1)}\right)^{r-k}$$

where k stands for the integer just smaller than $\frac{m+1}{n}$. Thus, to take an example: let the objects of guess be the nine digits, and let 100 guesses be made, of which 20 turn out right. The value of the above letters is $n=9$, $m=100$, $r=20$, $k=11$; and the probability of bias, or action of Thought-transference instead of mere guessing, is about $\frac{39}{40}$. The outlying possibility of pure chance being only $\frac{1}{40}$. This is still a long way from certainty, and more trials must be made.

The investigation is as follows:—

At every trial let there be n possible guesses, of which b are to be considered right and the rest wrong; what is the chance of guessing right r times in m trials?

The answer is the $(r+1)^{th}$ term of the expansion $\left(\frac{n-b}{n}+\frac{b}{n}\right)^m$, namely:—

$$\frac{\lfloor m}{\lfloor r \ \lfloor m-r} \cdot \left(\frac{n-b}{n}\right)^{m-r} \cdot \left(\frac{b}{n}\right)^r \cdot$$

But it is usually unnecessary to consider b as anything but unity,

probable number; but would it be unreasonable to surmise that on the first day something beyond chance had been at work? From this point of view it is clear that there may be cases where the division into series—a division which accords with actual facts, and is no arbitrary manipulation of figures—cannot be accounted wholly deceptive.

for only one of the possible guesses is generally correct. We can therefore re-write the formula conveniently thus

$$\frac{\lfloor m \quad \cdot \left(\frac{n-1}{n}\right)^m}{\lfloor r \, \lfloor m-r \quad (n-1)^r},$$

where it will be observed that r occurs only in the denominator.

The chance of guessing wrong every time is $\left(\frac{n-1}{n}\right)^m$.

The chance of guessing right every time is $\frac{1}{n^m}$.

The event which is most probable, or that for which the probability is a maximum, depends upon how many trials are made—that is upon $m : n$.

If fewer than n—1 trials are made, the most probable event is that all the guesses will be wrong.

In a set of n—1 trials it is an even chance whether all be wrong or one right.

If more than n—1 but fewer than $2n$—1 trials are made, it is most likely that one of the guesses will be right.

If more than $2n$—1 but fewer than $3n$—1 guesses are made, two of them will be most probably correct.

And in general if $\frac{m+1}{n}$ lies between two consecutive integers, k and $k+1$, the most probable event is to succeed k times in the m guesses. The chance of this event actually happening is

$$\frac{\lfloor m \quad \cdot \left(\frac{n-1}{n}\right)^m}{\lfloor k \quad \lfloor m-k \quad (n-1)^k} = \frac{1}{p} \text{ say,}$$

The chance of an actually observed event, viz., r successes, is

$$\frac{\lfloor m \quad \left(\frac{n-1}{n}\right)^m}{\lfloor r \quad \lfloor m-r \quad (n-1)^r} = \frac{1}{q} \text{ say.}$$

Now, in estimating what is the legitimate inference that can be drawn from an enormous number of trials in favour of the action of some unknown law, as opposed to mere chance, we must compare these

two probabilities; that is, we must find the *relative* probability of the observed event as compared with that of the event most likely to happen on the hypothesis of pure chance. For if mere chance rules, the most probable event is bound to happen in an enormous number of trials, whereas if there be a bias some other event will happen, whose relative probability is $\frac{p}{q}$. The probability in favour of mere chance is therefore $\frac{p}{q}$, and the probability of a disturbing cause is $1-\frac{p}{q}$; or the betting odds in favour of some bias or interference with chance are $\frac{q-p}{p}$.

The value of $\frac{p}{q}$ is

$$\frac{\lfloor k \;\; \lfloor m-k \cdot (n-1)^k}{\lfloor r \;\; \lfloor m-r \cdot (n-1)^r}, \text{ which reduces to } \frac{(m-r+1)\,(m-r+2).....(m-k)}{(k+1)\,(k+2)....r \cdot (n-1)^{r-k}}.$$

When m is very large, it is a long business to find the numerical value of this expression, and an approximation may be conveniently used instead. The following is deduced on the principle of using the arithmetic mean of a series of numbers instead of the geometric mean, and for all practical purposes it is sufficiently close to the true value:

$$\frac{p}{q} = \left(\frac{2m-r-k+1}{(n-1)\,(r+k+1)}\right)^{r-k}.$$

This is easily calculated, and it represents the outstanding probability in favour of pure chance very nearly.

We will recapitulate the meaning of the symbols.

m means the total number of guesses made, all under proper conditions;

n means the number of objects, or specified things, to be guessed at;

k means the most likely number of successes or right guesses on the hypothesis of mere chance (it is the integral part of $\frac{m+1}{n}$);

r means the number of successes or right guesses actually made.

It may be well to illustrate by a simple example. Let the things to be guessed be the four suits of cards, and let 32 guesses be made, 13 of them being right, the rest wrong. What probability of bias can be deduced from such a set as this?

The most likely number of successes in this case is 8 (since $\frac{m+1}{n}$ is $\frac{33}{4}$), and so we have to put in the above formula $k = 8$,

$r = 13$, $n = 4$, $m = 32$; with the result that the probability of mere chance is rigidly $\frac{1288}{4177}$, or from the approximate formula $\frac{32}{243}$, or say $\frac{1}{8}$ roughly. The probability of some law is therefore $\frac{7}{8}$, and the odds in favour of it are 7 to 1.

It is unnecessary to know the separate value of p and q; they can be calculated from formulæ given above; but the arithmetic is in most cases rather long.

Another mode of writing the approximate formula is perhaps just worth recording. Let kn trials be made, and let s be the excess of the actual number of successes over the probable number, that is, let $s = r-k$; then the probability of bias is

$$1-\left(\frac{2k-\frac{s-1}{n-1}}{2k+s+1}\right)^s .$$

Calling the ratio of $s : k$, t, and taking k large, this becomes

$$1-\left(\frac{2-\frac{t}{n-1}}{2+t}\right)^s ,$$

which shows that if $\frac{r-k}{k}$ tends to diminish as the number of trials is increased, the result of an infinite number of trials would be completely in favour of mere chance; whereas, if $\frac{r-k}{k}$ remains constant, or shows a tendency to increase with the number of trials made, the fact is strongly indicative of law.—We are, yours sincerely,

OLIVER J. LODGE.
ALFRED LODGE.

The problem of discovering from the results of a number of series of trials, varying in length and kind as M. Richet's did, what is the probability that some cause beyond chance has acted, should surely admit of a theoretic solution based on this simple proposition:—

An event occurs which must be due to A or B.

The *à priori* probability of the existence of A is P; and the *à priori* probability that, if A exists, such a result will follow is p.

The *à priori* probability of the existence of B is Q; and the *à priori* probability that, if B exists, such a result will follow is q.

Then, on any occasion when the event has happened, the *à posteriori* probability that it was due to A is ..... $\frac{Pp}{Pp+Qq}$,

and the *à posteriori* probability that it was due to B is $\frac{Qq}{Pp+Qq}$.

The following is an attempt at such a solution :—

The result of any particular series represents a certain degree of success; and the attainment of at least that degree of success must be due either to chance alone, or to chance *plus* some other cause, which we will call θ, the amount of whose efficiency is unknown.

The probability of obtaining at least that degree of success is

(1) if chance alone acts, (say) q,
(2) if chance $+\theta$ acts ..... $\frac{1}{2}$,

because in our ignorance of the efficiency of θ, we must suppose it as likely to bring the degree of success up to that point as not to do so.

Now we cannot proceed further without assigning some value, as small as we please, to the *à priori* probability that θ acts. Let this probability $= x$: then the *à priori* probability that chance alone acts $= 1-x$.

Taking now the result of a particular series, the *à posteriori* probability that at least that degree of success has been obtained by chance alone is $\frac{q(1-x)}{q(1-x)+\frac{1}{2}x}$, or $\frac{2q(1-x)}{2q(1-x)+x}$;

and the *à posteriori* probability that at least that degree of success has been obtained by chance $+\theta$ is $\frac{\frac{1}{2}x}{q(1-x)+\frac{1}{2}x}$, or $\frac{x}{2q(1-x)+x}$,

which is greater than x if q is less than $\frac{1}{2}$.

This latter fraction must be taken as a first approximation to the probability that θ has acted. The expression may now be used instead of x, to deduce in a similar way the probability that θ has acted in a *second* series; and the new expression so obtained may in its turn be used in application to a *third* series; and so on. The successive expressions will have a constantly increasing value as truly representative of the probability that θ has acted, in that they will depend on continually widening experimental results.

Now if θ_1, θ_2, θ_3, &c., represent the successive probabilities that θ has acted, obtained from a series of experiments in which q (as above defined) is represented by q_1, q_2, q_3, &c., successively, then

$$\theta_1 = \frac{x}{x+2q_1(1-x)}, \quad \theta_2 = \frac{\frac{x}{x+2q_1(1-x)}}{\frac{x}{x+2q_1(1-x)}+2q_2\left(1-\frac{x}{x+2q_1(1-x)}\right)} = \frac{x}{x+4q_1q_2(1-x)}$$

θ_3 in the same way $= \dfrac{x}{x+8q_1q_2q_3(1-x)}$ and generally

$$\theta_n = \frac{x}{x+2^n q_1 q_2 \cdots\cdots q_n(1-x)}.$$

The final value of θ_n is independent of the order in which the results of the experiments are used.

It will be seen that, if the representative of q in any series is $\frac{1}{2}$ exactly, the probability for the action of θ will be unaffected by that series; and that if the representative of q is $\frac{1}{2}$ throughout, the *à posteriori* probability for the action of θ will be the same as the *à priori* probability—*i.e.*, will be $= x$ throughout. The illustration of the urns (p. 243), on the other hand, would give an immense *à posteriori* probability for the action of θ, even where the representative of q was $\frac{1}{2}$ throughout; in other words, it would give an immense improbability for the exclusive action of chance, even where the result of every series represented no more than the degree of success which the exclusive action of chance was as likely as not to attain. To represent these conditions, each urn would have to contain one white ball and one black one, and the illustration would require all the white balls to be drawn; that is to say, a total amount of success which corresponded with ideal probability would be illustrated by a total amount which was the furthest possible departure from ideal probability. The probability of drawing all the white balls, $\frac{1}{2^n}$, expresses the probability of obtaining, in the sets of trials, not the total amount of success supposed, but that one particular distribution of it.

E. G.

IV.

THE PROBLEMS OF HYPNOTISM.*

By Edmund Gurney.

Of all physical analogies, the one which most constantly suggests itself outside the limits of the physical universe is that of the pendulum. Alike in our sensory experience where excitement leads on to fatigue and satiety begets aversion, and in the wider domains of religion and politics where movements and opinions so constantly tend to one extreme by a mere impetus of repulsion from the other, the rhythmic law of action and reaction is ever at work. But sensation and sentiment by no means exhaust the region to which these further applications of the law extend: we find it operating where it would least be sought, and invading the passionless paths of science herself. The characteristic instinct of the scientific spirit is, of course, to *simplify* and *unify*; as science advances, theories of a multitude of separate agents, whether personal deities or abstract faculties, gradually give way to the recognition of large general laws. But if in the main this tendency towards unity and simplification brings nothing but good, it is inevitable that an end in itself so eminently conducive to intellectual peace and satisfaction should also act as a temptation—that in yielding to the generalising instinct the mind should sometimes be swayed too fast and too far, and so be landed in premature hypotheses. And thus it is that, even as the old pre-scientific speculation sought a transcendental unity of things in such principles as water or fire, so even advanced science may occasionally do injustice to the immense variety of Nature, and, in the determination to formulate a law for some special department of facts, may seek and observe too exclusively the facts which can be made to square with the law. It would be hard to find a better instance of such over-simplification than is afforded by the modern science of Hypnotism. For so short a span of existence, few sciences can have been so prolific in theories, presented often concurrently, and with little attempt at mutual refutation; and the time has perhaps come when the experimental knowledge of the subject which is so rapidly advancing may be usefully supplemented by a brief critical review of its theoretic vicissitudes. If such a review reveals how divergent have been the various paths which speculation has taken, and how one after another

*In the endeavour to give some sort of completeness to the following sketch, it has been found necessary, here and there, to resurvey ground which has already been to some extent traversed in the Reports of the Committee on Mesmerism.

of them seems to leave this or that set of facts on one side, it may at least aid in defining the problems that actually remain.

The facts of hypnotism, it is needless to say, first became prominent in connection with theories which science has with one voice rejected, finding nothing therein but absurd personal pretensions, and an ignorant jargon about forces and fluids. The facts themselves, however, were too indisputable and extraordinary to be neglected; and the first and most comprehensive theory of them, advanced in opposition to those of "mesmeric" influence and "odic" emanations, was that of *suggestion* and *imagination*. The singular mental phenomena which followed "mesmeric" manipulations were ascribed to a temporary suspension of the "subject's" independent powers of will and judgment, whereby both his beliefs and his conduct were left at the mercy of external suggestions. This was the theory crudely set forth exactly a century ago by the Commission of the French Academy of Sciences which was appointed to examine Mesmer's claims. Though presumably regarded by them as an explanation, it clearly contains no explanatory power whatever; it is simply a description (and, as we shall see later, a very imperfect one) of the particular mental condition which the "subject's" actions suggest. The crucial question remains: If the doctrine of a specific influence from the operator be rejected as outside the domain of natural law, what are the natural laws to which this peculiar mental condition can be referred?

This question remained for a long time without an answer; but two answers were at last given by countrymen of our own—one which, as far as it went, was of a clear and definite character, by Braid; the other, of a hazy and unexplanatory character, by Dr. Carpenter. This description may at first sight seem unjust to the latter, inasmuch as he professes general agreement with Braid, and does not seem aware of having adopted a different basis. It will not be hard, however, to justify what has been said.

Dr. Carpenter's explanation ("Mental Physiology," c. xiv.) rests purely on *mental* ground: his argument is concerned with states which (though, of course, like other mental states, they have their physical correlate in the nervous system) he treats throughout in their purely mental aspect. There could be no objection to this treatment, were it successful as far as it goes—the conditions of success obviously being that the phenomena of the mental state for which we seek explanation should be brought into relation with phenomena of other and more familiar mental states; for scientific explanation consists in bringing out identities between new and old knowledge. Dr. Carpenter's failure to realise this condition seems to me to be complete. The region where he seeks the needed identities is the well-recognised one of *reverie* and *abstraction;* and his endeavour is to embrace the phenomena of these familiar states

with those of hypnotism in the common category of "automatic mental action." As an instance of the automatism of reverie, he describes the loose play of fancy to which the poet may resign himself under the influence of some pleasing aspect of Nature. To illustrate the automatism of abstraction, he describes the "absence of mind" which has characterised many clear and profound thinkers,—showing itself in their eccentric conduct in the streets, or in random answers to persons who have addressed them when their whole attention was absorbed in following some complicated train of logical thought. The reader will observe, even before we begin to test the resemblance of these "explanatory" phenomena to the unexplained facts of hypnotism, how confused and confusing the idea of automatism has already become. It is more than doubtful, to begin with, whether "automatism" correctly describes the poet's condition at all. As long as the idea of *will* is absent, "automatic" is an excellent word to describe actions, the conditions of which are inside and not outside the subject of them: such, for instance, is its appropriate meaning in physiology. But the mind is not a cell or a tissue; and, in the present connection, to call the mind's actions automatic, simply because it is taking its own path unsolicited from moment to moment by new sensory impressions, seems very misleading. "Automatism," if it is to serve Dr. Carpenter's purpose and to embrace hypnotic facts, must mean something quite distinct from spontaneous and unsolicited origination of ideas; it must mean nothing less than temporary paralysis of the directive power of the will; and there is nothing to warrant the assumption of this paralysis in the fact that the mind's action for the moment is unimpeded and effortless.

But even if we waive this objection and extend the meaning of "automatism" to cover what is properly expressed by *spontaneity*, the automatism in the described condition of the poet and that displayed by the absent-minded mathematician are surely so far from identity that they present an absolute contrast. The spontaneity or "automatism" of the poetic day-dreamer, in the sense of a free and aimless play of mind, belongs to the essence of his activity; so far as it is a correct description at all, it is a description covering the whole ground of what his mind is doing. The mathematician's mental activity, on the other hand, is just exactly *not* free and aimless, and just exactly *not* automatic. It is the most conscious and strenuously-directed effort, concentrated on successive points in an argument which it may require all the strength of his will to stick to and grapple with; and any automatism that he may display is a mere *accident* of this state, showing itself if external demands happen to solicit an attention which is already irresistibly set in one particular channel. The condition described as "automatic" in the case of the poet is charged with consciousness, which may be of the most vital and delightful kind; it *is*, in fact, itself

the stream of consciousness in a particular aspect—*i.e.*, winding hither and thither in a roving and easy way. The automatism of the mathematician who does or says odd things while solving a problem, is essentially remote from the stream of his consciousness; *that* is engrossed with other things, and his automatic sayings and doings are distinctly reflex actions, the result of suggestions which may never reach even the threshold of conscious perception.

Having thus observed the total dissimilarity, or rather opposition, of the two mental states whose fundamental characteristics Dr. Carpenter treats as identical, we shall not be much surprised to find that the hypnotic state, which he goes on to identify with them—in order by that means to obtain an expression for the less known in terms of the better known—is essentially distinct from either. The looseness of thought which has already made "automatism" cover two quite distinct things very easily extends it to a third equally distinct thing, which, being thus referred to a class, is so far—and all by the magic of a word—explained! The automatism of the hypnotised "subject," in his response to external suggestions, is often automatism in a true sense; in that respect differing *toto cœlo* from those spontaneous or internally-originated impulses of fancy to which, in moments of random reverie, the poet's mind may give the rein. But it differs no less distinctly from the automatic or reflex words or actions with which we saw the absorbed mathematician responding to external impulses. For of those responses, as we observed, the essence was that they were unattended to, the stream of consciousness being rapt away in another direction; while in the hypnotic case, consciousness and attention,* so far from being abstracted from the things which are being done in response to the external suggestion, are directed with even abnormal concentration upon those very things. We might without incorrectness describe the higher hypnotic phenomena as *reflex* action, in respect of the certainty with which particular movements follow on particular stimuli; but they are, and their peculiarity consists in their being, *conscious* reflex action. The central problem of hypnotism lies in the *combination* of those two adjectives; and in the following pages each of them will have to be emphasised in turn. The hypnotised "subject" who carries out complicated orders is a conscious, and often even a reckoning and planning, automaton. Reflex response (if we wish to retain the phrase) is here raised from the merely physical to the mental plane; the external suggestion evokes a particular idea in as certain and as isolated a way as an appropriate electrical stimulus evokes the isolated action of one particular muscle. This isolation of a single object in the

* Consciousness and attention, that is, so far as they are present. The very varying degrees, and in many cases the indisputably high degree, in which they may be present will be discussed a little later.

mind naturally implies abeyance of the normal controlling and relating power. In the normal state, successive vivid points of consciousness are surrounded by a swarm of subordinate perceptions and ideas, by reference to which it is that conduct is instinctively or subconsciously kept rational, even though the attention may be strongly focussed on its immediate aim or object. In the hypnotic state the contact is broken between the predominant idea and this attendant swarm ; and conduct thus ceases to have reference to anything except the predominant idea. And the difference between that isolation of the dominating idea which is the cause of automatic answers and actions in the case of the absorbed mathematician, and the isolation of the dominating idea in the hypnotic automaton—though to a superficial observer the states seem similar just because each produces irrational actions—clearly goes to the very root of the phenomenon, regarded as mental. The mathematician has no fraction of attention to spare for external solicitations ; his mind is in a state peculiarly impregnable to them : the mind of the hypnotic "subject" is absolutely at their mercy. The one mind is working with unusual force and individuality in its self-elected channel, and what its owner says or does in response to external influence is as little attended to by him as the influence itself. The other mind is working with marked absence of individuality in a channel elected by others, and what its owner says or does in response to external influence is that on which his attention is concentrated to the complete exclusion of every other thought.

The attempted explanation of the phenomena on *mental* ground, by bringing the mental condition within the recognised domain of abstraction or automatism, thus falls to pieces. Braid's explanation was a very different one. He fearlessly took *physical* ground, and attributed the hypnotic effects to an exceptional and profound nervous change produced by a particular muscular strain. His experiments and conclusions, which were the foundations of the actual science of hypnotism, are too well known to need recapitulation. They dealt, it is true, chiefly with the lower phenomena—the obvious bodily effects, and Braid's grasp of the subject on the psychical side was certainly very imperfect ; still his claims to have traced to their true source effects which had hitherto been ascribed to imagination and imitation are sufficiently explicit to pass as a suggestion, at any rate, of a physiological basis for the higher phenomena with which we are here chiefly concerned. Since his time, the principal gain to our knowledge has been the proof that it is not necessary that the eye should be the organ employed, or even that the strain should be of a muscular sort at all. With sensitive "subjects," the ticking of a watch held at the ear, and light monotonous passes acting on the nerves of touch, have been found as effective as the fixed gaze. But this, it will be

observed, is a mere extension of Braid's doctrine; for the physiological condition of preparatory nervous adjustment to a regularly-recurring stimulus is really *fixation* in as true a sense as where the employment of muscular apparatus more immediately suggests the word. Concentration of attention is, no doubt, the natural mental concomitant of the physical fixation; it may even be that for the artificial production of the state in man it is a real condition, in the sense that physical fixation alone would not be effective if the attention were kept actively employed on external topics. But Braid never for a moment suggested that the peculiar muscular or nervous strain could in the first instance be dispensed with, or was anything less than the full and sufficient cause of the subsequent phenomena. He is throughout consistent and urgent in his view that the basis of hypnotism is a complete alteration or rebalancing of the nervous system, artificially producible by special means of an obviously physical sort.

Here, then, we seem to have at any rate the beginning of a satisfactory account of many of the facts popularly attributed to "mesmeric" influence. Braid clearly saw—what Dr. Carpenter has failed to see—that the hypnotic state is a unique one and is due to a quite special cause. If the fact is experimentally established that a particular sort of physical process is perpetually followed by an exceptional mental state having no apparent relation to it, the hypothesis of an exceptional nervous change—as a middle term, and as the proximate condition of the mental state—is one which, in the present stage of our knowledge as to the connection between mind and nerve-tissue, we not only may but must make. And so far as the mental change is profound and the mental state unique, to that extent, we are justified in saying, is the nervous change profound and the nervous state unique. Even to enunciate this doctrine may appear somewhat out of date, now that science is attempting to define, what Braid left uncertain, the exact nature of the nervous events—whether, for instance, they consist in "cortical inhibition" or in "local erethism." But there is a special reason for constantly insisting on the more general position. For Dr. Carpenter's is by no means the only attempt that has been made to frame an explanation of hypnotic phenomena out of psychical factors; and such factors have proved themselves peculiarly liable to illegitimate use. Above all, they have tended to confuse the important distinction between the production of the state and the state itself—a distinction which Braid's conception enables us to keep clear.

The psychic factor mainly relied on has of course been that of *attention*. And there are doubtless cases where such reliance might seem justified—cases where the physical means which are successful in producing hypnotism seem much less exceptional and violent than those

described by Braid and others, therefore, the accompanying psychical element of concentrated attention becomes relatively more conspicuous. Even these cases, however, include some where physical means have been altogether absent. We have no record of the production by attention, however concentrated, of the characteristic phenomena of suspension of directive power and loss of memory, unless accompanied by some amount of physical fixation.* And indeed the very nature of the concentration which present seems to involve some such fixation. It would probably be impossible where the bodily state was wholly passive and natural. In its normal manifestation it is itself exceptional—so much so that an educated mind may find the greatest difficulty in attaining to it—and is, in fact, the very opposite of the active sort of attention with which an object is normally contemplated or a train of thought followed. Nor can attention be represented even as an inseparable accompaniment in the production of the state, unless by resolutely ignoring a large part of the hypnotic field. As Mr. Romanes some time ago observed, and as Professor Stanley Hall again pointed out in his most interesting and suggestive paper in *Mind* XXX., it is easy to hypnotise animals, but not easy to credit such an animal as a frog or a crayfish with any true power of mental concentration. And the phenomena of natural somnambulism or "sleep-waking," which in respect of the absorption of the mind in one direction present the closest analogy to those of hypnotism, demand no previous concentration of attention at all. But even if we confine ourselves to cases where attention is actually present during the production of the state, what ground is there for describing it as the *cause* of that state, in the absence of any extraneous empirical proof of a tendency in the antecedent to produce the consequent? The general effects of a one-sided strain of mind or body are pretty well known; and "tonic cramp of the attention" (to adopt Professor Stanley Hall's phrase) may be a very satisfactory description of the one-sided absorption in a particular direction which characterises many isolated stages of the hypnotic trance. But what tendency should the cramp of an attention which is directed to a button held in the hand have to produce, or to facilitate, a fresh cramp or series of cramps when the attention is diverted to quite fresh objects? The cramp of a limb which has been kept too long in one position does not issue in a tendency to move it rapidly into new positions; yet it is just such an anomaly as

* This of course does not apply to the production of the state in sensitive "subjects" who have been hypnotised on previous occasions, and who fall into the trance by attention, not to a button, but to their own memories of past sensations. The power of representing and revivifying past states is one which manifests itself in many directions, and has no special relation to the hypnotic problems.

this that the hypnotic process and its sequel perpetually present. Professor Stanley Hall's excellent remarks—*à propos* of the idea of attention—on the danger of using terms in a manner which necessitates "radically reconstructing the notion of them familiar to common consciousness," would surely be equally in place here *à propos* of cramp. Even the case which he himself describes—where powerful excitement, both physical and psychical, was produced by the effort to change the current of the "subject's" ideas, and where it was necessary to wake and re-hypnotise him before impressions of a new *genus* could be given—presented a feature which seems an odd result of previous rigid attention to a button, namely, "great mobility of attention" within the single genus of ideas suggested. But this necessity for waking and re-hypnotising is so far from being constant that in my experience it is unexampled. I have again and again found the complete change to a new genus of ideas to be absolutely effortless and instantaneous—found, that is, that the attention, which had been as usual fixed during the *process* of hypnotisation, became quite abnormally mobile afterwards. Thus—to give one example out of many—a youth well-known to me, with whom I have made many experiments, was told by the operator, before the proceedings began, that when hypnotised he was to recognise and converse with me. He was then hypnotised by fixation in the usual way; after which he talked to me for a minute quite naturally. Then, with a single sentence, he was taken from my room to a churchyard, and was set to work at trimming a grave, where the grass had grown too long. He put great energy and humour into his task, and he now regarded me as a stranger who wanted to interfere with him and rob him of his job. Another word from the operator, and he was in a boat in a storm, running up an antimacassar for a sail, and lashing his companion to the mast for safety, his comments throughout being extremely vivid and amusing. Another word, and he was engrossed in watching a conjurer spinning plates in the Brighton Aquarium; he indulged in very free criticism, and, while greatly admiring, opined that the plates were loaded and the table made to slope inwards. I now got the operator to introduce me to him, and to place my hand in his, and by this means I obtained sufficient hold on him to make him half believe that he was in church; but he was puzzled by the continuation of the plate-spinning, and at last he compromised his beliefs by saying that, though he would consent to sit in church, he must insist on watching the conjurer. A word from the operator obliterated the latter impression, and brought him wholly to church, where he pointed out various objects and, without the slightest suggestion, began mimicking the manner of a local preacher. I now again addressed him, and he again disowned acquaintance with me, though curiously he regarded me as the same stranger who had interfered with him before. Another

word, and he was at home helping his mother with accounts, and did a sum which I gave him on paper correctly and with rapidity.* Here then the order of mental events, in the whole experiment, was unusual rigidity followed by unusual mobility. But how can we pretend that we *account for* the latter by recognising the former? So long as we keep to physical ground, it will be observed, no such difficulty occurs. If Braid had been asked how it is that fixation of particular muscles or nerves reacts on the higher nervous centres in so remarkable a manner, he might have fairly replied that physiology abounds in puzzles no less special and insoluble. But if I am told that a particular mental attitude—that of fixed or one-sided attention—is the *cause* of certain mental phenomena which are new to me, I am surely justified in demanding that the order of events shall present some perceptible coherence—shall at least not run directly counter to what my general experience would have led me to expect. Such an objection might be pedantic—as against writers who of course have no thought of differing from Braid, or of denying the physical correlative of the attentive attitude—were it not that in their advocacy of attention they have curiously disregarded the *facts*, such as those just recorded, where this want of coherence is evident. The oracular simplicity of Bürger's formula, that the cause of hypnotic phenomena is essentially psychic, would hardly retain its impressiveness in face of hypnotic phenomena which in psychic character are at the precisely opposite pole from their antecedent.

But this objection has yet another side. Suppose we were told that the final result of cramping a limb or a psychic faculty was *paralysis*, where should we expect to find the paralysis? Surely in that limb, or in that faculty. And in the case of the *lower* hypnotic phenomena

* It may be asked what guarantee can be had, in such cases as the above, that the "subject" is not acting a part in a condition of normal wakefulness. The test of pain cannot well be immediately applied, as in the alert stage of hypnotism there is rarely a marked diminution of sensibility. But the sensibility test can nevertheless be effectively brought to bear; for, if left alone at the close of such manifestations as the above, the "subject" will fall, usually with great rapidity, into the deeper stage of trance, in which any amount of such minor torments as pin-sticking and pinching may be applied without arousing him, or his conjunctiva may be touched without evoking more than a feeble reflex response. This is a state into which it cannot be maintained that robust youths are wont to pass at will out of a condition of normal wakefulness. Another test, which I have repeatedly applied, is to inform the "subject," on his complete waking, that he has apparently been dreaming of taking part in various scenes, and to offer him £20 if he will say what the scenes were. It will still, perhaps, be objected that though truly in a hypnotic condition and unable subsequently to recollect what has passed, the "subject" may still at the time be only pretending to be a party to the scenes suggested. This supposition deserves careful attention, and there are cases to which it certainly seems applicable. (See the very interesting remarks in M. Richet's *L'Homme et l'Intelligence*, p. 166.) But even if universally true, it would still leave the fact of the mobility of the attention just where it was.

what happens to the faculty of attention may doubtless be regarded in this light. Thus if a favourable "subject" be allowed to stare at a button undisturbed, he will soon pass beyond the "alert stage," when his imagination and his body might be brought into activity by suggestions, and will simply become torpid and indifferent, though still often capable for some time of rational conversation; the cramped condition of his attention has not resulted in continued and absorbing attention to the button, but in gradual paralysis of the whole perceptive function. We have just observed how different is the case with the attention, if the "subject" be taken in hand and suitably treated for the higher phenomena before this deep state has supervened; but the further point to be now noted about such phenomena is this—that while in them the *attention* is so little paralysed that it is even found to be abnormally mobile after a period of fixation on the button, *other* functions—those namely of choice, and will, and reaction in the way of attraction and repulsion—*are* paralysed. The effect on these reactions admits (as we shall see) of various degrees, but there can be no doubt as to its reality. In psychical terms, then, cramp of the *perceptive* has led to paralysis of the *appetitive* faculty—a fact which it would surely need a very enthusiastic psychicist to regard as self-explanatory.

It may be worth while here to note what I think has been a main reason (though a very illogical one) for the tendency to regard previous fixity of attention as in itself a sufficient ground for the unhinged automatic mental condition of hypnotised "subjects." It is that certain *physical* phenomena, which may at first sight seem more startling (but are in reality far less unique) than the mental condition in question, have undoubtedly been known to follow or to accompany the state of fixed or expectant attention—that attention, however, being then always directed to the part of the body in which the phenomena were actually to appear; as in the familiar case where the steady contemplation of a particular finger leads to a sense of tingling in it. But even in this direction, where the mere attitude of attention and expectancy does actually seem of distinct efficiency, cases occur where the physical change cannot possibly be ascribed to that attitude, inasmuch as not even the vaguest realisation of the bodily part to be affected was in the patient's mind. Such a case is that of a woman who had been hypnotised by Braid for relief of violent pain in the arm and shoulder, and who found, much to his and her own surprise, that an opacity which had been left by rheumatic fever over more than half the cornea of her left eye was gradually clearing. This case has been most unaccountably quoted by Dr. Carpenter as an instance of the curative effects of mere attention. The result seems clearly attributable to that re-balancing or re-direction of nervous energy which Braid regards as characteristic of the hypnotic state—to those nervous events which are

no mere correlate of an act of attention, but the result (as he explains) of a quite special physical cause.

But the objections to the attention-theory are not by any means exhausted by the difficulty of connecting the *process* with the *results* of hypnotisation; on the contrary, they become even more substantial if we confine ourselves to the latter. And they deserve note the more just because the theory here will actually cover so much ground—because so many of the hypnotic phenomena may be truly described as belonging to the "pathology of the attention," and admit of interesting treatment (*e.g.*, in G. H. Schneider's treatise, *Die psychologische Ursache der hypnotischen Erscheinungen*) in connection with other branches of that wider subject. But in the first place, even in the alert stage of hypnotism, where mono-ideism with its accompanying loss of balance and control is often most conspicuous, it must always be borne in mind that this is not the essential peculiarity of the state. The fundamental fact according to our formula is not that the psychic activities are abnormal, but that they are *reflex;* it is not the mere subjection of the mind to a single idea or set of ideas, but the certain production and alteration of that condition *ab extra.* In the second place, results are often loosely set down to the "dominance of an idea," where, if we inquire what the particular idea is, we fail to find it. A hypnotised boy is told that he may have a £5 note if he can pick it up. To suit the theory, the dominant idea must be that of the impossibility of the act; but even if we allow this idea a momentary dominance, in what sense can it be held to continue dominant during a struggle in which every word and gesture express the strongest determination and incredulity? It may, perhaps, be suggested that the words and gestures express no more than an ineffectual effort to resist a nevertheless dominant idea; but to this suggestion we may often oppose the "subject's" subsequent description of his experience. And lastly, there remains the large class of cases which do not belong to what I have called the "alert stage" of hypnotism at all; and where the attention-theory can only be applied by the desperate assumption that unusual deadness of sensibility in one direction necessarily involves unusual concentration in another. If a jet of gas is seen burning specially brightly, it is doubtless reasonable to connect this condition with the fact that the other jets in the chandelier are turned off;* but if no light at all can be perceived, the natural

* *Apropos* of this metaphor, which is often, of course, an entirely just one, the following instances may be worth recording. A hypnotised "subject" who strongly resented being even slightly pinched was impressed with the idea that a person to whom he was attached had died. He showed considerable emotion, and was now completely indifferent to the most savage pinching. Again, several "subjects" who were sensitive to pain in the alert state, were thrown into the deep state, and impressed with a command which was to be executed when they emerged again into the alert state: when the emergence came, they showed entire insensitiveness until the command was duly performed.

hypothesis surely is, not that some jet is burning brilliantly somewhere out of sight, but that *all* the jets are turned off. The energy of attention is not a fixed quantity, bound to be always in operation in one direction or another; nor does the human mind, any more truly than nature, abhor a vacuum. Even in the "alert stage," when the "subject" can be made by an occasional word to enact scene after scene with astonishing truth and vigour, the indications, if he be left alone, are of blankness, not of concentration. He knows where he is, and will answer if spoken to; but otherwise he sits inert and listless, if asked what he is thinking about will usually answer "nothing," and soon passes into the deeper stage with closed eyes, in which, though, still able for a brief period to respond to questions, he is insensible to any ordinary tactile stimuli. It would surely be irrational to refer that insensibility to the strong concentration in some unknown direction of an attention which, even in the previous alert state with open eyes, there was no ground for supposing to be active. I do not urge the cases of obvious reflex action (on which Professor Stanley Hall has made some good remarks in the paper already referred to), since on a theory like that of Herr Schneider—in which the lower centres, so far as they distinguish stimuli, are credited with an embryonic consciousness—the question might there be a mere question of words. The cases which I have in view are those where the results observed cannot, by any stretch of the meaning of attention, be reasonably connected either with the "positive field"—*i.e.*, with unusual absorption of the attention in the line of the result, whether as immediately producing it or as inhibiting its opposite*—or with the "negative field," where insensi-

* I gather from some expressions of Professor Stanley Hall on the subject of "active inhibition," that he holds that the condition of the attention in this positive field may be further subdivided—that the *actual direction* of concentration may be not only towards the production, but towards the inhibition of a particular mental phenomenon. This direct action of inhibition is hard to picture. The activity of active inhibition appears to me always to lie in a determined setting of the mind in some *new* direction: I mitigate a pain not by attending either to the pain or to an imagined absence of the pain, but by clenching my teeth and thinking of something else, *i.e.*, by opening quite new channels of nervous energy. So when Professor Stanley Hall inquires whether when a hand is made insensitive to pain, it is "due to abnormally intense inhibition of sensation or motion by consciousness, or is better conceived as an entire detachment and vagrancy of attention from consciousness, of which it is conceived only as a concentration," I find a difficulty in admitting the possibility of the first alternative, as also, I must confess, in catching the meaning of the second. In the proposed case, at any rate, I should not myself see the necessity of having recourse to either. If the hand is rendered insensible in the ordinary way by faint sensory stimuli, it is surely a case where the theory of direct physical inhibition of the lower sensory centre is exactly in place. The very different case where the manipulations employed do not produce any sensory stimuli at all, as where no contact is used and the arm is thickly enveloped in clothes, is one on which I shall have a word to say later.

bility and irrational conduct are the result of an unusual draining-off of attention from the ordinary sensory or ideational tracts into some other line. They are cases where, if we wish still to hypostatise attention, we must just say that it is paralysed or has fallen asleep. But such a mode of expression is not to be commended. For the sleep and paralysis may invade some faculties and not others, *e.g.*, colour-blindness may supervene while the hearing remains perfect; and even in the deeper state of trance, ideas, and especially commands, may be impressed on the "subject's" mind. What do we gain, then, by employing a general term to describe such special effects? When once the chandelier-metaphor is abandoned—when once it is recognised that in a multitude of cases the quantity of attention turned on in one direction is in no way connected with a withdrawal from any other—the idea of a common psychic factor seems out of place and misleading. The "subject's" ear wakes while his eye sleeps; so in ordinary paralysis the right side may feel while the left does not; and it does not then occur to us to talk about the patient's *attention* being asleep on the left side and concentrated on the right.

And now we must make a sudden transition, from the theories which have unduly magnified psychic functions in hypnotism, to those which have unduly ignored them, and have substituted the shibboleths of physical reflex action and automatic cerebration for that of attention. If we trace the natural logical route of the subject, we shall see that a time was almost bound to arrive when a purely physical account of the whole range of phenomena would be attempted. Up to the time of Braid's death, no serious question seems to have been raised as to the relation of *consciousness* to the hypnotic manifestations. No doubt, at any rate, was expressed as to the presence of consciousness in those higher phenomena which belong to the lighter stage of the trance, and which form by far the most interesting part of the whole subject. Braid himself speaks of "the extraordinary power of concentration of thought," "the rapt contemplation," "the glowing scenes and images" presented to "the fervid imaginations" of his patients. But the very fact of tracing the observed phenomenon, as he did, to a peculiar physical condition must lead on to the question how far the psychical factors of consciousness and volition are really involved in them at all, and how far the suggested idea has any true existence in the "subject's" mind. Granting that his attention has to be directed in the first instance to the monotonous process by which the state is produced, we have seen that the power of attention might naturally be expected to be paralysed by that very process, so as not to survive when the state is once reached; and if the "alert stage" is not caught and used, this is what actually happens, the gradual dulling of the

faculties passing on into comatic unconsciousness.* But there is nothing *primâ facie* unreasonable in supposing attention to have passed away *before* this deep stage is reached—in supposing that it does not survive that profound nervous change which, following Braid, we infer to have supervened as soon as the alert stage is reached. The state of the "subject" is so obviously peculiar that there would seem to be no strong *à priori* obligation on us to interpret what would ordinarily be accounted signs of consciousness in the usual way. Dr. Carpenter's wavering utterances already indicate some suspicion on this point; and the gradual progress in our knowledge of physical reflex action, and of its special connection with the hypnotic state, has naturally given the question a new shape and significance. Hypnotism being, beyond doubt, the field on which such reflex action reaches its furthest limit, where is that limit to be drawn? The consideration of this point will further establish the distinction between the lower and the higher hypnotic phenomena, and will thus further define the fundamental peculiarity of the latter.

If we begin at the bottom of the series of phenomena, we certainly find no reason to suppose that they are accompanied by any distinct consciousness or concentration of attention. If we find it hard to credit the frog with attention during the process of hypnotisation, it is still harder when the process is complete; and the insensibility and immobility of the human "subject," if left to himself in the "deep stage," seem to indicate a mental condition not very different from the frog's. Higher in the scale, actual experiments in reflex action suggest a decided lowering of the psychical functions. The heightening of the reflex responsiveness of the *muscles*, which is often the first symptom of hypnotic influence, does not, it is true, serve as a sign of diminished mental activity, especially as the phenomenon itself—the twitching limbs and the inability to control them—is peculiarly calculated to stimulate the "subject's" attention. But Professor Stanley Hall's recent experiments avoid this difficulty, and give us just the indication that is needed. For, in establishing the diminution, during the alert stage of hypnotism, of the time necessary for *voluntarily* reacting on a stimulus, they suggest that the reaction has become to some extent reflex;† and since this implies that the brain-action associated with conscious attention to the work of reacting is

* A fuller account of the different stages of hypnotism will be found in a former paper, *Proceedings*, Part V., p. 61, &c.

† It is worth observing that this extension of reflex action in one direction may perfectly well co-exist with what might appear a contrary result in another. For instance, Professor Stanley Hall's "subject" (I presume while in the same hypnotic stage as he was in during the reaction-experiments) could gaze at a sunny window for 13 minutes without winking. But the ability to do this doubtless

diminished, we may fairly suppose that the amount of conscious attention is itself diminished. And this leads us on to the more general supposition, that actions which would normally involve very distinct consciousness may be performed by the hypnotic "subject" either with a lesser degree of it or entirely without it. There is thus considerable justification for Heidenhain's explanation of the singular exhibitions known as hypnotic *mimicry*. According to him, the movements or words of the operator, acting on the eyes or ears of the "subject," stimulate as usual the lower sensory centres; but in the hypnotic state, the functions of the higher cortical portion of the brain (to which nervous discharges are supposed normally to pass from the lower sensory centres) are *inhibited*, and consequently no effect is produced in the way of consciousness. At the same time, the disturbance in the lower sensory centres, though thus unaccompanied by consciousness, is sufficient to pass on the nervous discharge to the most nearly associated *motor* centres, which will naturally be those whose activity will produce the *same* words or movements; since clearly no association can well be closer or more constant than that between the sight and sound of a movement or word and the act of producing that movement or word. And since the same inhibition of the cortical functions, which precludes consciousness of the impression, precludes also the normal exercise of the power to direct and control movements,* the mimicry takes place mechanically and unfailingly, *i.e.*, as genuine reflex action. Heidenhain further extends this explanation to the phenomena of what he calls "automatism at command." He attributes the machine-like obedience of the "subject" to a similar inhibition of cortical function, and to the consequent opening of an unimpeded channel of discharge from the lower ideational to the motor centres—*i.e.*, from the place of the nervous discharges which, if allowed to pass on in the normal way, would result in the mental picture of an action, to the place of the nervous discharges immediately associated with the performance of that action.

But while it is important to note the facts to which this hypothesis

arose not from an inhibition of the normal reflex movement, but from a direct deadening of sensibility in a particular organ. So extreme a deadening in the "alert stage" of hypnotism is rare, though out of several hundred "subjects" I have found two whose eyes remain open even in the deep stage. Such exceptions are valuable as showing the variety that may exist even in the simplest facts of hypnotism.

* Heidenhain has introduced an *équivoque* into the terminology of the subject by calling the hypnotic action on the cortical functions *inhibition*, without pointing out explicitly that the *normal* action of those functions in respect of motion is to a large extent *inhibitory*, and that the complete description of the method by which the automatic reflex responses are brought is thus *inhibition of the inhibitory function*.

will apply, and in which ideation and volition apparently play no part, it is of still greater importance to avoid mistaking this limited portion of the field for the whole. It is no doubt convenient for the theory to conceive the inhibition of directive and volitional power as accompanied by inhibition of consciousness; but the positive grounds on which the sweeping assertion of the unconsciousness rests are so flimsy that, but for the high authority of those who are opposed to me, I should almost have thought it waste of time to discuss them.

The most thorough-going statement of the doctrine in question appears, I regret to say, in a book which for general acuteness and comprehensiveness of treatment is superior to any other on the subject with which I am acquainted—the *Étude Scientifique sur le Somnambulisme* of Dr. Despine, which in 1879 obtained for its author a medal from the Medico-Psychological Society of Paris. The acuteness, it is true, is not unfailing. When a man concludes that the highest psychic manifestations may take place without consciousness, from the fact that the complicated vital functions of the animal and vegetable creation, while seeming to demand a capacity at least equal to that of an intelligent man, nevertheless take place unconsciously, and that the highest human intellect could not construct a butterfly's wing, we may defer our answer till a stomach or a tree begin to reason, or a butterfly's wing to decide knotty points. Popular arguments, moreover, are sometimes caught at in a manner fatal to consistency. Thus an appeal is made to the well-known ability of somnambulists to keep their balance in dangerous places and at giddy heights; which may reasonably be connected with unconsciousness of danger, and so far might pass muster as an argument for the temporary abolition of *all* psychic function. But on Despine's own principles, how should the somnambulist be any the safer for his unconsciousness? If cerebration, even in its most subtle and complicated forms, can go on just as usual without any psychic correlative, why are we to except the particular cerebration that would normally be accompanied by fear, giddiness, and loss of balance? What is to prevent *that* in the given conditions from functioning in the normal way, and so producing a fall? So far from affording a proof of true automatism, this is emphatically a case where the theory of *attention*—of a mind exclusively occupied with the next step and not occupying itself with ideas of falling—seems of most assistance. Dr. Despine's next argument, however, has more force. He finds an indication that even the most complicated psychic phenomena of hypnotism may be purely automatic in the fact that in certain abnormal states the personality seems doubled; as when a person recovering from typhoid fever spoke and sang, while seeming to himself to be listening to another's performance, and without any idea what the next sound to be produced

would be. Similarly religious ecstatics and "trance-mediums" have delivered impromptu discourses without conscious cerebration, and have been the devout and admiring auditors of eloquence whose sense they grasped only after it had issued from their own lips. In these cases Dr. Despine attempts to make the one part of the person—the watching and attentive part—the witness of the automatism of the other part; and since, viewed in this light, the presence of the witness is not necessary to the production of the result, an argument is obtained for the general possibility of similar manifestations without any participation of consciousness. This artifice of making two people, A and B, out of one, in order (in the absence of evidence) to obtain a sort of presumption that A's presence is not a condition of B's actions, is not very convincing; at the same time there need be no great difficulty in admitting the particular possibility claimed. Few will dispute that the talking and singing might appear as purely automatic phenomena; and even the impromptu discourse, with its far more complicated series of actions, may be conceived as producible either in the absence or with a minimum of consciousness—its contents being presumably a string of familiar ideas, closely connected by association, and clothed in a hackneyed phraseology. Despine's error is in sweepingly applying the same argument to hypnotism, without remarking how radically different are many of the phenomena there presented. The psychologist who claims for his study the dignity of a science is surely bound to follow the physicist's example, and to take some trouble to vary the conditions of his observations; and in this question of the presence of consciousness, the very simplest experiments suggest the sort of variation that is necessary. A "subject" is asked a question to which the obvious reply is "yes," and answers "yes": he is asked another to which the obvious reply is "no," and answers "no." "Clearly automatic reflex action," say Despine and Heidenhain, with a great show of reason. But now let us take a case where, though the answer is equally simple, the question itself does not suggest one answer rather than another. For instance, let some one, standing behind the "subject," give a very light clap of the hands at intervals, and let the "subject" immediately before each clap and also at other times between the claps, be asked the question "Do you hear this?" He will be found to answer "yes" when a clap follows, "no" when no clap follows. Now here even to suppose the answer "yes" to be automatically given involves some strain of the reflex theory; for granting that the physical attention might be fixed by the question—*i.e.*, that the nervous events corresponding to expectation of a faint sound might be thus produced—these events would in themselves have no tendency to produce the word "yes" in response to the clap. In the normal state, that answer would involve a sense that a

doubtful point had to be decided by the person himself, and the result truly communicated—a mental operation of some complexity and great delicacy; and if the same result be produced without any psychic concurrence, the physical events must at any rate differ considerably from anything involved in the trance-medium's self-propagating stream of irresponsible verbiage. But when the answer given is "no," the indication of a true psychical event—*viz.*, the consciousness of not hearing—corresponding to it seems almost irresistible; for here the answers, besides involving just the same delicate operations as the former one, would have to be reflexly jogged out not by the stimulation of sound, but by the *non*-stimulation of silence. Similar and far more complicated instances could be easily multiplied *ad infinitum*.

But Despine's principal argument, and Heidenhain's only one, depends on the "subject's" subsequent *defect of memory* as to what has passed during his trance. It does not seem to have occurred to either of them that the requirement, as a test for present consciousness, that its content shall be afterwards remembered, requires itself any justification. Yet if the reality of that test be granted, the question whether a man was conscious when he read an article in the *Times* will depend on whether or not he receives a blow on the head when he has finished it. In his development of the argument, however, Despine shows considerable controversial ingenuity; and it must be admitted that those who have maintained the presence of consciousness in hypnotism and somnambulism have not always been happy in their way of accounting for the subsequent forgetfulness. This has been attributed by Dugald Stewart to the "subject's" defect of attention to the events that are passing; by M. Maury to a mental paralysis brought about by an exhausting concentration of attention on those events,—theories so weak and baseless that we certainly need not grudge Despine the satisfaction of setting them off one against the other. So again, he has no difficulty in disposing of M. A. Lemoine's explanation that memory cannot survive the shock of the sudden change from the somnambulic to the natural state. A single instance, however, will show that his own counter-positions are very little stronger. He adduces the extreme violence of the things which have been done or suffered in the trance-condition, and argues that since these things, though so impressive in their nature, are not remembered, they must have been done or suffered unconsciously. The reply is obvious that equally violent things are done and suffered in *dreams* (which Despine again and again distinguishes from the trance-condition by the presence to them of consciousness and the *ego*), and are forgotten within a minute of waking. Equally obvious is it to notice that Despine's argument quietly begs the whole question; for he is *assuming* for the abnormal state the same *relation* of consciousness to memory as exists in the normal state,

forgetting that that identity of relation is precisely what he has to *prove.*

But there is a more radical objection to all these arguments for the unconsciousness of the "subject" from the fact that subsequent memory is absent—the fact, namely, that it is frequently *present.* The *primâ facie* indications of this subsequent memory, found in correct descriptions by the "subject" of what he has been doing, are too obvious to be long overlooked; and Heidenhain, when he observed them, brought them into harmony with his general theory by supposing that, when the abnormal inhibition of cortical function is removed, the excitation remaining in the lower centres transmits a stimulus to the liberated sensorial ganglion cells—to be psychically represented as memory of the original exciting cause, which, when it actually operated, had no place in consciousness. He holds, however, that some distinct hint or impulse is necessary to bring up this residuary excitation to the requisite strength. And for many of the simpler phenomena this seems a satisfactory hypothesis. Further, the outward indications of remembering, in a new hypnotic state, what occurred in a former one, and the apparent taking-up of an old track of ideas, or even of a connected discourse, at the point where it had been abandoned, have been brought by Despine within the scope of purely automatic brain-action—the renewal of the hypnosis bringing with it the former special excitatory conditions. But other phenomena seem quite beyond the legitimate scope of such a theory. If the "subject" is brought into the "alert state," made to go through the ordinary platform buffooneries, and then re-awakened, there is often not any breach of consciousness at all; and he gives a description, which there is not the slightest ground for calling in question, of his state of mind in performing the actions—as, *e.g.*, that he felt disinclined to do them but could not help it, or that he was aware of their absurdity but could see no reason for not doing them. Again, on re-hypnotisation the events of the previous hypnotic state are sometimes spoken of with fulness, in answer to perfectly neutral questions; nor do they merely recur in unrelated sequence, as by the release of particular springs, but are compared and estimated. There is here no mere rejoining of a temporarily-broken associative chain: the phrases used are to all appearances the normal results of a discursive and critical review of past experiences; and the *onus probandi* rests with those who deny that what gives every sign of being genuine memory *is* genuine memory. When once this hypnotic memory is duly recognised, the study of its varieties will be found of great interest; and in the paper above referred to I endeavoured to make this study the basis of a clear separation between two states—the "alert" and the "deep"—that have commonly been distinguished merely as steps in a single process, continuous changes on the path to hypnotic sleep.

The results there given have an important bearing on the central question as to the respective relations of consciousness and of reflex action to the hypnotic state, or rather states. Under appropriate conditions, we saw reason to recognise true subsequent memory, and therefore present consciousness, even in the lighter stages of what I called the "deep" state; and when we passed upwards to the lower phenomena of the "alert" state—*e.g.*, mimicry, and mechanical obedience—we could often appeal further to the absolute unbrokenness of the conscious stream. Where memory is absent, and where there is a distinct breach in the train of consciousness, arguments may still be drawn from experiments such as the one on hearing above recounted, where a point is left to the "subject's" decision, and the truth of this decision is independently ascertained. Again, the end being suggested, the "subject" will take his own means to accomplish it. He will use his reasoning powers—as in the case of Dr. Hack Tuke's "subject," who was asked if he could not walk forwards, and who "remembers arguing out in his mind, wearily, that it followed from this he was walking backwards." He will even form original theories as to what is told him. For instance, a young man who had been impressed with the idea that he was going to be hanged, was then told that his sweetheart had been blown off the pier and drowned, and that the announcement of the event was in the evening paper. He at once surmised that she had purposely thrown herself off, through grief at his approaching fate. He was now told that the second edition of the paper showed it was a mistake, and he suggested two explanations; the first that the name Newington had been wrongly printed for Newton; the second (in which he testified his belief by seizing a paper and pointing to what he imagined to be the actual passage), that the words in the second edition ran "*almost* blown off the pier, and *almost* drowned." Again the hypnotised person, like the somnambulist, will sometimes go through complex calculations, and bring out a correct result with greater ease and certainty than in his normal state; while the vividness and inventiveness of his imagination, as under the stimulation of questions he pursues aloud the course of his waking-dream, are a source of ever fresh astonishment.

These latter facts alone cannot, of course, be made conclusive against a thorough-going automatic theory: but they at any rate suggest strong probabilities, which ought to be met by something better than bare assertions. First we have Heidenhain's crude attribution to reflex mimicry of all the phenomena which others have attributed to the dominance of an idea—his statement, *e.g.*, that a "subject" will never eat a raw potato on the suggestion that it is a pear, unless the operator makes movements of mastication in his sight. This view produces a [illegible]. A few more experiments are made, and these produce a

practical recantation with an admission of the sufficiency of suggested *ideas* to produce the appropriate bodily movements. Then comes Despine and maintains that even the "ideas" have only a physical existence. We de not dispute the admissibility of his conception in simple cases, but ask in vain why we are to extend it over the whole hypnotic field, and apply it to elaborate actions which have been accompanied by consciousness in the whole of our experience, and present every imaginable sign that the attention is completely engrossed by them? Are we to forego all discrimination, just because it saves trouble to have a simple and thorough-going theory? The situation may be put thus. One set of facts (notably the unbroken persistence of consciousness and memory in the passage into and back from the lightest stages of the trance) show that a hypnotic condition is not *ipso facto* an unconscious one. Another set of facts show that, in hypnotism, the line which separates mechanical and reflex from conscious and volitional actions is considerably shifted, and actions which would normally be above the threshold of consciousness sink below it. But what of that, if a multitude of actions, performed in that lighter stage of the trance to which the most interesting phenomena of hypnotism belong, do according to any natural interpretation imply a state distinctly above the threshold? We readily grant that we cannot draw the new line with certainty, even for a particular case; but all analogy is against supposing it shifted to the utmost limit at the very outset.

I cannot, then, for a moment believe that the automatic theory, in the extreme form which asserts unconsciousness for all hypnotic and somnambulic actions, will hold its ground. But in proportion as the theory becomes less sweeping, it gets into difficulties of detail. Heidenhain, as we have seen, has found himself obliged to recognise the psychic element in the higher hypnotic manifestations: but he seems oddly unaware of the effect of this admission on his exposition of the physical processes involved. The point of that exposition, it will be remembered, was the opening of a *direct* channel from impressional to motor nerve-centres, through inhibition of cortical function; and now we find a vast number of cases where consciousness, though conditioned by cortical activity, is *not* inhibited. It is of course easy to reply that here it is only the functions specially associated with spontaneous control and choice of movements that suffer inhibition. But that goes not a whit further as an explanation than Braid's general assertion of a profound nervous change; it is merely a necessary inference from the palpable fact that spontaneous control and choice have ceased. It is just this cessation—which, translated into physical terms, we should naturally call "inhibition"—that constitutes the novel feature of the case; and nothing that we have otherwise known about inhibition (as Mr. Romanes, in his preface to the translation of Heidenhain's *Animal*

Magnetism, has rightly admitted) could have led us to expect or conceive the results attributed to it. Heidenhain's *explanation*, in fact, like that of the French Commission, is no more than a *restatement* of the problem. As long as the *whole* of cortical function can be supposed to be eliminated, his theory has a certain symmetry and explanatory power. It gives a plausible account, involving neither consciousness nor volition, of the power of the simple sound "go," to produce an immediate corresponding movement of the "subject"—the merit of the explanation being the easily conceivable picture of the nervous events which it supposes, and which are quite on a par with the recognised facts of reflex action. But when, *e.g.*, a long series of orders is thoughtfully and painfully carried out, long after they were given—a phenomenon not uncommon in "mesmeric" exhibitions—it seems impossible to adapt the old (or any other) neat and symmetrical hypothesis of the nervous processes to the new phenomena; and the word "reflex" can be applied to the latter, if at all, only in the peculiar and carefully guarded sense which confines it to their *psychical* aspect. It seems hardly possible that Heidenhain should have missed seeing this, had he waited to formulate his theories until he had witnessed some of the higher phenomena in their more striking forms. As soon as the elements of consciousness and volition are clearly recognised as active in such phenomena, it surely must equally be recognised that the fundamental peculiarity of the condition is simply the absorption of those elements into the one suggested channel of attention or expectancy, and is thus quite removed from the lower plane of physical "reflex."

So far, then, our formula of *conscious* or *psychic reflex* action, as expressing the true peculiarity of the higher hypnotic manifestations, has been defended against two sorts of over simplification; that which ignores the part played by the mind in the phenomena; and that which, accepting the part played by the mind, fails to see that its differentiating feature is the liability to respond to suggestions with the same mechanical readiness as a stimulated muscle displays when the normal inhibitory influence is withdrawn. It is only stating the condition thus indicated in other words, to say that the heart of the problem lies not in *consciousness* but in *will*. And here another important distinction presents itself. The hypnotic automatism must not be conceived as necessarily implying any abrogation whatever of the will, taken as the *sense of desire or impulse*. That element admits of all degrees. It may be absent altogether, and the "subject" may perform his acts, consciously indeed, but with complete indifference—in which case nothing is commoner than for him to believe and to assert afterwards that he could have avoided doing those things if he had chosen; but it may also be present in full force, and may even be

directly opposed to the course of action pursued. What *is* abrogated is not the sense of desire or the power of willing, but the sense of self-determination and the power of choosing. Even here questions of degree come in. In the very lightest stage—as exemplified by the boy who strove to pick up the bank-note—it cannot be said that choice, any more than desire, is abrogated; while even in a deeper stage, a "subject" will sometimes experience such a sense of repugnance as seems to involve some residual power of avoidance; and occasionally he will retain complete power of choice in some isolated particular.* At the same time, this suspension of choice must be accepted as the most marked and central characteristic of the higher form of the hypnotic trance. And for those who regard the intuition of free-will as a subjective illusion, it is a point worth notice that decided abnormalities of conduct should present themselves precisely when, and in proportion as, the sense of having a free-will and being a choosing *ego* disappears. The variations are at any rate concomitant; and if nothing else varies, such concomitance would, outside metaphysics, be held to imply some sort of casual connection. It may perhaps be objected that it would be incorrect to say that nothing else varies—that the essential variation is simply a change in the particular *motive* that assumes prominence; *e.g.*, that when a command is given to put the hand between the bars of the fire, and the determinant motive to a normal mind would be the dread of being burnt, the determinant motive to a hypnotised mind is the desire to obey the controller. But this is not at all in accordance with the evidence of many hypnotic "subjects" who have been able to recall and give an account of their state of mind. They are often conscious of the falseness of what is told them, and of the folly and harmfulness of the things they are bidden to do;† they are even sensible of a strong objection to doing them, and *not* sensible of any positive motive impelling them to act; but it simply does not occur to them that they have a choice in the matter.‡ Even if we abandon free-will and stick to psychology,

* I was recently experimenting with a youth who had formerly been a telegraph-boy, and who had taken a strong dislike to the *métier*. When hypnotised, he was at the mercy of any suggestion or command, except one; nothing would induce him to carry a telegram. In its strength of resistance to the hypnotic mono-ideism, this repugnance really itself reached the mono-ideistic intensity; for the refusal was unaffected by considerations that would certainly have reversed it in the youth's normal state,—*e.g.*, when he was told that the matter was one of life and death, and that he should have £20 for the job.

† See a case reported by Dr. Hack Tuke, in the *Journal of Mental Science*, for April, 1883, p. 70.

‡ The determinist may, no doubt, make a more general objection: he may say that the consciousness of free choice, however interpreted, is a normal antecedent of human voluntary action; and that therefore he would have expected

such facts as these seem decidedly awkward for the mechanical theory of the determination of conduct, at every point, by a motive that represents the greatest balance of foreseen pleasures or immunities from pain.

And we are thus led to the completing step in our description of the higher hypnotic state. That state may be regarded as the most complete exemplification of Professor Bain's fruitful formula—*the tendency of an idea to act itself out.* I cannot regard Professor Bain's own instances (*Mental and Moral Science*, p. 91) as the best examples of the law; they seem to me rather to exemplify the common impulse to produce a marked effect, to "make a scene" of some sort, even at one's own cost. At the same time, I think that his formula represents a reality, the scope of which even in ordinary life has hardly been sufficiently recognised. It seems to me the only possible ground for certain brief phases of sulkiness or perversity—the shade of meaning may be best conveyed by the slang "cussedness"—where a person finds himself persisting in an attitude or a line of conduct which causes him acute discomfort at the time, with a promise of nothing but discomfort as the consequence. But in the case of hypnotism, at any rate, the idea is a most helpful one. For it enables us to bring under rule the cases that seemed most exceptional—where, *e.g.*, a "subject" is told that he cannot do a particular thing, and struggles ineffectually to do it. We saw how absurd it was to represent his mind as throughout possessed by the idea of the impossibility, or his will as paralysed. But there is no great difficulty in supposing that the idea of impossibility obtains a momentary lodgment, and then tends to work itself out *physically*, even after the opposite idea—the idea that the action is possible and *shall* be accomplished—has dislodged it from consciousness. We might fairly compare the automatic continuance of the brain-movements which are evoked by the momentary stimulus of the first idea, to the long-continued and far spreading muscular contractions which in a sensitive "subject" will follow on a brief sensory stimulation: both are signs of the characteristic hypnotic irritability. This view seems strongly confirmed by the fact that, if the boy's muscles be examined, they will be found in the state which corresponds to the first idea—that of impossibility and not to the second. I have myself tested this many times. A boy's arm being flexed, he is offered a sovereign to extend it. He struggles till he is red in the face; but all the while his triceps

its absence to be accompanied by some other abnormality. I must, however, take leave to doubt whether, if taken unawares, he would have evolved this expectation whether he would have regarded the (to him) purely illusive sense of having a free choice among several different courses as an indispensable element of directive force in the line of the one course that is actually taken. The hypnotic facts might therefore have an interest for him, if only because they clearly show the logical necessity of this very odd-looking admission.

is remaining quite flaccid, or, if some rigidity appears in it, the effect is at once counteracted by an equal rigidity in the biceps. The idea of the impossibility of extension, *i.e.*, the idea of continued flexion, is thus "acting itself out," even when wholly rejected from the mind.

It is perhaps well that my space is nearly exhausted; for it might be held unfitting, in a paper on Hypnotism, to do more than hint at results, however simple and precise, which break away from every form of hypnotic hypothesis. Community of sensation between "subject" and operator; the distinction by the "subject" of the operator's faintest whisper, either amid deafening uproar, or among a number of other faint whispers of similar sound; local anæsthesia produced in the absence of expectancy by a process which is itself unfelt; inhibition of speech or memory without a word or sign of any sort;—a writer who owns to having participated in experiments which establish these facts grievously imperils his chance of being listened to on the sober ground of hypnotism. At the same time the *naïveté* and suddenness with which the clamorous facts of hypnotism itself were welcomed within the portals of science, as soon as a *savant* of established reputation took the trouble to learn (very imperfectly) the A B C of them, and to proclaim that they actually were realities, that his own brother had been experimented on, and that it was *not* all cheating, as he had all his life supposed—may perhaps suggest a quiet surmise as to the scientific future of other events which, with all their absurdity and inadmissibility thick upon them, still go through the hollow form of taking place with surprising accuracy. But leaving these matters aside, no sketch of the "problems of hypnotism" could be in the least complete without mentioning certain objections which present themselves in the direct path of orthodox hypnotic experiment, and which concern the very processes on which the hypnotic explanations are made to rest.

At the very outset, there is the difficulty of the vast differences of degree that exist in the power to produce the results—a difficulty which has never been fairly faced, much less surmounted. It has been asserted, and in a sense it may be true, that any one can hypnotise any one: any one, that is, may be competent to make passes in the gentle and monotonous manner which acts on the organism of sensitive "subjects," and with immense perseverance may produce some amount of the hypnotic effect. But let a score of likely "subjects" be taken who have never before been hypnotised, and let a dozen persons who have been instructed in the right method of making the passes be set in turn to operate on them, and let this dozen include one recognised and successful "mesmerist." If the experiment be often repeated, always with fresh "subjects," it may be pretty confidently asserted that in the long

run the successes of the "mesmerist" will outnumber those of all his rivals put together, and moreover will as a rule be far more marked in character and far more rapidly effected. And this is the more noticeable in that, supposing the usual process to be adopted, the condition which on the hypnotic hypothesis would appear to be the most important—the staring fixedly at an object in the hand—would be common to all the attempts, and little if any of the required monotonous stimulation would be derived from the actual passes—the operator's hands not being in contact with the "subject" till perhaps the very final stage of closing the eyelids. But if the truth of the above assertion be denied—and fully to carry out the experiment with a new set of "subjects" daily would involve great practical difficulties—the result of repeated attempts with the same "subject" will afford a still stronger argument. A recognised "mesmerist," after a very few successful trials on favourable "subjects," can send them into the trance in a very short time, sometimes even with a single pass; but except in response to him they will show no particular susceptibility; and no attempt of others, extending only over the few seconds that suffice the successful operator, will produce any effect whatever. To account for facts like these, as Heidenhain has done, by differences in the moisture or temperature of the operating hands, seems little better than childish—as if a somewhat warm and moist hand (even were it indispensable, which it is not) were a sort of *lusus naturæ*. Somewhat more plausible is the suggestion that the facts really exemplify the dominance of an idea—that the "subjects" *believe* that their mesmerist has special power, and as a consequence of that belief succumb to him. But it really seems absurd to suppose that this faith in a single individual's power is unfailingly complete and absorbing in every member of a set of careless boys who are new to the whole business, and whose obedience to the simple directions which they receive, and passive acceptance of what happens to them, certainly do not suggest any nice criticism of the nature and limits of their operator's faculties. There is no difficulty in impressing such "subjects" with the idea that some other person present, who may be of a more dominant and imposing aspect than their recognised controller, is also a powerful "mesmerist"; but this preparatory idea will not be found to invest that person with any of the controller's powers.

The hypothesis of suggestion and expectancy is still more obviously inappropriate where the end in view is not the *production*, but the *ermination*, of the trance-condition. It would be very strained to imagine that the mind of the "subject" is in every case dominated, or even that he *was* dominated at the time when the condition was being produced, by the idea that only the producer of it has the power to put a stop to it. Yet he will often remain completely uninfluenced by the

efforts of others to awake him, and that, too, even when only a light phase of the trance has been induced. The upward passes, or the slap of the hands and sudden call, which are at once effective when used by the right operator, may be repeated in vain by others. This fact has occasionally led to very awkward results; as, for instance, in London some years ago, when one mesmerised "subject" was set to mesmerise some one else, and then, after he had succeeded in producing a state of profound coma, passed himself into a condition in which it was impossible to impress him with the necessity of undoing his own work.

This disagreeable incident suggests another well-known class of phenomena of which no explanation on any purely hypnotic hypothesis seems possible—the so-called "cross-mesmerism" or agitated bewilderment which is apt to result when a mesmerised person is subjected to new treatment from a second operator, before the effects of the former treatment have disappeared. The phenomena are of too alarming and distressing a kind to admit of deliberate experiment, but when once seen, are not easily mistaken; and a slight but sufficient indication of their nature is sometimes afforded in a momentary way by the violence which the "subject," who is perfectly docile in the hands of his mesmerist, will display when accidentally touched or interfered with by a bystander. If this be explained away as an instance of the "dominance of an idea," we ask, of what idea? Is it the idea of the operator, with which the "subject's" mind is so wholly engrossed as to react with violence on any attempt to divert it? But if he is in the alert stage, his mind is so little riveted on his operator that it is abnormally ready to be borne off by any and every suggestion; and if he is in the deep stage, it is an unwarrantable assumption that his mind is engrossed with anything at all. Nor can the view that *suggestion* is the cause of the phenomenon—though a natural enough one to start with and applicable to some cases—survive a prolonged and patient study of the facts. Instances will be found where it is practically certain that no idea, tending to make the "subject" dread interference from all persons save one, had been even remotely suggested to him; and where, if any such idea were really dominant in his mind, it could only itself be an instance of the specific *rapport* which hypnotism fails to account for.

Such considerations as these, though they lie across the threshold of the subject, are apt from their very generality to be disregarded; but it is easy enough to find in single definite phenomena—and these not among the outlying marvels above referred to, but among the experiments which are the stronghold of the hypnotic theories—a starting-point for similar objections. A boy is placed in a chair and is not hypnotised; but his arm is rendered stiff and insensible after a minute of downward stroking. "Reflex irritability," say some of our friends; "the monotonous sensory stimulation has produced the well-known

tonic spasm." A thoroughly sound explanation; but let us try the effect of downward passes made without contact or any possibility of sensory stimulation. The same result ensues; the usual tests of torture and bribes may be applied, with complete impunity, to the "subject's" arm in the former case, and to the experimenter's pocket in the latter. "Expectant attention," say other of our friends; "an interesting example of the power of mind over body; the boy believes in the operator's power, and his mental energy, being absorbed into the single channel of the expected effect, brings that effect to pass." Very probably; but on experimental principles it is surely just worth while, before promulgating this very probable theory, to test it by a single variation of conditions. Let the experiment be repeated, then, with this difference—that the boy is made to read aloud a paragraph from a newspaper as long as the process continues, having been previously warned that he must carefully attend to what he is reading, as he will be examined in it afterwards. After this warning it is not surprising that he should stand the examination successfully; but a little surprising, on the proposed theory, that the stiffness and insensibility should again have supervened.* When such an experiment has succeeded with "subject" after "subject," and when their expressions of astonishment have suggested that in many cases the idea of the result was not even latently present in their minds, it is natural to devise measures for preventing the possibility even of the latent idea; as, *e.g.*, by extending the "subject's" ten fingers on a table in front of him, with a thick screen between them and his eyes, selecting a couple of them (the combination being of course varied each time), and then subjecting the selected pair to the same process as the arm. But I am approaching the region of marvels and the theory of specific influence which I have here forsworn. To relieve one's mind by observing how fairly the mesmeric hypothesis embraces and explains the facts which so violently break away from the hypnotic one, is perhaps not more unscientific than to neglect and ignore those intractable facts; but to those who do not share it, such relief will naturally seem to resemble the escape from subordinate perplexities which the devout Catholic makes by swallowing one huge assumption at the outset.

* With regard to the question how far the idea of his arm was present to the "subject" of this experiment, it was instructive to compare his vivacious reading and subsequent remembrance in this case with his mechanical reading and subsequent oblivion when (as described in *Proceedings*, Part V., p. 71) he had been thrown into the hypnotic state, and then had the idea of his arm prominently brought before his mind. In that case the idea remained truly dominant, and left no room for attention to the reading.

V.

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VII.

CONSTITUTION AND RULES.

1884.

TITLE.

1.—The name of the Society is—The Society for Psychical Research.

OBJECTS.

2.—The objects for which this Society is established are :—

(*a*) To unite students and inquirers in an organised body, with the view of promoting the investigation of certain obscure phenomena, including those commonly known as Psychical, Mesmeric, or Spiritualistic; and of giving publicity to the results of such research.

(*b*) To print, sell, or otherwise distribute publications on Psychical and kindred subjects; to afford information to inquirers into these subjects by correspondence and otherwise; to collect and arrange facts respecting them; to open Libraries, Reading-rooms, and other suitable Premises and Offices; and generally to do all such other things as may be conducive to the attainment of the above objects.

NOTE.—*To prevent misconception, it is here expressly stated that Membership of the Society does not imply the acceptance of any particular explanation of the phenomena investigated, nor any belief as to the operation, in the physical world, of forces other than those recognised by Physical Science.*

GOVERNMENT.

3.—The Society shall be governed by a Council consisting of twenty-four Members. The Council shall elect from amongst the Members of the Society a President, who shall be President of the Society, and an *ex-officio* Member of the Council and of all Committees and who shall retire from office yearly at the first Meeting of the

Council after the Annual General Meeting of the Members of the Society. He shall, however, be eligible for re-election, and shall be deemed as retaining his offices until he shall have been re-elected or his successor appointed. The Council shall also from time to time elect Vice-Presidents, who shall be *ex-officio* Honorary Members of the Society, and shall have the privilege of being present at any of the Meetings of the Council.

CONSTITUTION.

4.—The Society shall consist of—

(*a*) MEMBERS, who shall contribute not less than two guineas annually, or a single payment of twenty guineas, and who shall be eligible to any of the offices of the Society; and shall be entitled to vote in the election of the Governing Council and at all meetings of the Society; to use its Reading-rooms and Libraries; to borrow books from its Libraries; and to the free receipt of any journal, transactions, or periodical publication which may be issued by the Council.

(*b*) ASSOCIATES, who shall contribute not less than one guinea annually, or a single payment of ten guineas, and who shall be entitled to attend all Meetings of the Society, except such as are convened for business purposes only, and to the free receipt of the ordinary published Proceedings of the Society; and shall have free access to its Reading-rooms and Libraries.

Members and Associates shall be entitled to purchase all the publications of the Society at half their published price.

5.—All Members and Associates of the Society shall be elected by the Council. Every candidate for admission shall be required to give such references as shall be approved by the Council, unless he shall have been proposed in writing by two or more Members or Associates, who, on his behalf, and by his authority, shall assent to the Constitution and Rules of the Society, and consent to abide and be governed by them, one of whom shall have certified in writing, from personal knowledge of him, that he is a fit person for admission. Every such certificate having been read and approved at a Meeting of the Council, the election shall be proceeded with. The election to be by ballot, and one black [illegible] to exclude. The Council shall cause the result to be made [illegible] candidate, who, if elected, shall be furnished

with a certificate of election and a copy of the "Constitution and Rules."

6.—The subscription shall become due immediately on election, and afterwards in advance on the first day of January in each year. In the case of any Member or Associate being elected on or after the 1st October, his subscription shall be accepted as for the following year. Any Member or Associate who is more than three months in arrear and who fails to pay after two applications in writing, shall be liable to be struck off the list.

7.—If any Member or Associate desire to resign, he shall give written notice thereof to the Secretary. He shall, however, be liable for all subscriptions which shall then remain unpaid.

Honorary Members and Honorary Associates.

8.—The Council may invite any person who

(i) is either distinguished for knowledge or experience in Psychical Research or otherwise eminent, to become an Honorary Member of the Society; or any person who

(ii) has rendered services to the Society,

to become an Honorary Associate, such person to be eligible for re-election annually.

Honorary Members and Associates shall have the privileges without the obligations attaching to Associates.

Corresponding Members.

9.—The Council shall have power to elect as Corresponding Members, who shall be on the same footing as Honorary Members, persons able and willing to forward the objects of the Society. They shall be eligible for re-election annually.

General Meetings.

10.—The Anniversary or Annual General Meeting of the Members of the Society shall be held in the month of January, on a day to be fixed by the Council, and of which not less than twenty-one days' notice shall be given. The business of such Anniversary Meeting shall be to receive the Annual Report of the Council, and Statement of Assets and Liabilities, to elect New Members of Council, and to discuss questions on the Rules and management. A Member shall not moot any question

on the Rules or management of the Society unless he shall have given at least fourteen days' notice thereof to the Secretary, but amendments to any motion may be brought forward without notice, provided they relate to the same subject. The Secretary shall give seven days' notice to every Member of all questions of which such notice shall have been given to him.

11.—Special General Meetings of the Members of the Society may be convened by the Council, or by the President, or by the Secretary on the requisition of ten Members, and notice of such Meetings, stating the objects, shall be given at least seven days previously, and no other business shall be entered upon at such Meetings than that stated in the notice.

12.—All General Meetings of the Members of the Society shall be convened by circular to the Members.

Proceedings in General Meeting.

13.—The quorum necessary to constitute a General Meeting shall be ten.

14.—The President of the Society shall preside *ex-officio;* in his absence any one of the Vice-Presidents who may at the same time be a Member of the Society; or should no such Vice-President be present a Member of Council. In their absence the Meeting shall nominate its Chairman.

15.—If within one hour from the time appointed for the Meeting a quorum is not present the Meeting shall stand adjourned for one week. At the adjourned Meeting the number present for the time being shall constitute the legal number.

16.—All questions shall, at a General Meeting, be determined by a majority of the Members present, except in the election of Members of Council at the Annual General Meeting, for which election voting papers shall be accepted. The Chairman shall have a second or casting vote at all General Meetings of the Society.

17.—Voting papers for the election of Members of Council shall be sent to all Members of the Society with the notice of the Meeting, and shall be deposited with the Secretary of the Society, at least twenty-four hours prior to the Meeting. They shall be duly signed by the Member voting, and be enclosed in envelopes securely fastened and

marked on the outside "voting paper," and enclosed in a letter sent to the Secretary and signed by the Member voting, and they shall not be opened till so directed by the Chairman at the Meeting.

Election and Business of Council.

18.—The Council shall consist of eighteen members, elected annually at a General Meeting, and of other members co-opted from time to time by the Council, provided that the whole number shall not exceed twenty-four. The names of persons for the first time proposed to be co-opted on the Council shall be brought forward at one Meeting of the Council, and shall be sent round to all members of Council previous to its next Meeting, when the voting shall be by ballot, and a unanimous vote of those present shall be requisite to carry the election. The members co-opted from time to time by the Council shall cease to hold office at the Annual Meeting at which new members are elected, but may be co-opted afresh at the First Meeting of the Council after such Annual Meeting.

19.—Of the eighteen elected Members of the Council of the Society so appointed, six or whatever number may be required to reduce the number of elected Members to twelve shall go out of office at the time appointed for election of Council in the year 1883; one other third, at the time appointed for such election in the year 1884, and the remainder at the time appointed for such election in the year 1885, and the vacancies so made shall be filled by fresh elections. All Members of Council from time to time elected at the annual elections shall go out of office at the time appointed for the annual election in the third following year. The Secretary shall every year, at least twenty-one days before the ensuing Annual General Meeting, send to all the Members of the Society a list of the retiring Members of Council, and a statement whether all, and if not, which of them are candidates for re-election. In all cases the retiring Members shall be deemed as remaining in office until they shall have been re-elected or their successors appointed.

20.—Any Member of the Society who shall have paid up all subscriptions due from him, including that for the current year, shall be eligible for election or re-election, as a Member of Council, provided he shall have been nominated in writing by a Member of the Society, duly qualified, and such nomination shall have been forwarded to the Secretary fourteen days before the time of holding the Annual

General Meeting. A list of the persons so nominated shall be forwarded to all the Members of the Society, at least ten days prior to the meeting. In case more persons, duly qualified, shall be nominated for election at any Annual General Meeting than are required to fill up the vacant places of those retiring by rotation, then such persons shall be preferred and declared elected as shall obtain the highest number of votes.

21.—The Council shall at their first Meeting after every Annual General Meeting, elect a President as provided for in Rule 3. At the same Meeting they shall also elect a Treasurer and Auditor, and such other officers as they may deem expedient, who shall retire from office annually, at the same time and under the same conditions as provided for in the case of the President by Rule 3. They shall from time to time elect Vice-Presidents, Members of the Society, and Honorary Members, as provided for by Rules 3, 5, and 9. They shall have power to appoint a salaried Secretary, and such other paid officers, assistants, and servants as they may deem necessary, and to determine their duties. All the appointments made by the Council, the Council may at their pleasure revoke.

22.—The Council shall elect persons duly qualified to fill up any vacancies which may, from time to time, occur in their own body; and any such persons so elected shall go out of office at the time when the term of office of the persons in whose places they were respectively appointed would have expired.

23.—The Council shall meet monthly, unless otherwise determined. An attendance book shall be kept, and signed by each Member of the Council at the time of entering the Council-room. In all Meetings of the Council four shall be a quorum; all questions shall be decided by vote, and a decision of the majority shall, except where otherwise provided by these Rules, be the decision of the Meeting; the Chairman to have, in addition to his own, a casting vote. The Chair shall be occupied by the President; or in his absence by a Vice-President who is a subscribing Member of the Society, or should no such Vice-President be present, by a Member of Council chosen by the Meeting.

24.—The Council shall have power to appoint for special purposes Committees comp[illegible]embers of the Society or other suitable persons. Ever[illegible]l report its proceedings to the Council

through the Chairman or Secretary of such Committee, one of whom must be a Member of Council, and no report shall be published without the sanction of the Council.

25.—The Council shall have power, by a majority of three-fourths of the Members present, in a Special Meeting of their own body duly convened for the purpose, and of which, and of the objects thereof, not less than seven days' notice shall have been given to each Member of the Council, to add to, suspend, or alter any of the rules, regulations, and bye-laws of the Society; such alteration to be in force only until the next ensuing Anniversary Meeting, unless it be then confirmed by the vote of a majority of the Members of the Society there present.

26.—The Council shall have power to employ the funds of the Society, including any funds obtained by donation, bequest, or otherwise, in any manner consistent with the objects thereof, and they may invest any surplus funds in such securities and in such manner as they may deem proper; and they may sell, employ, or re-invest the said funds.

27.—The Council may establish Branches, and Local or Provincial Societies, in any part of Great Britain or elsewhere, under such regulations, and subject to such limitations as they may deem fit.

28.—The Council shall have power from time to time to co-operate with, or accept the alliance of, other Societies, having similar objects in view.

Auditors.

29.—There shall be two Auditors—one chosen by the Members of the Society, and one chosen by the Members of the Council. These shall audit the accounts of the Society, and report thereon to the Council. The auditors shall be empowered to examine into the particulars of all expenditure of the funds of the Society, where they shall see occasion; and may report their opinion upon the whole or any part of it, whether it has been expended in accordance with the constitution of the Society.

Finance.

30.—The Council shall cause true accounts to be kept of all sums of money received and paid, and shall submit the accounts of the Society, with a statement of the assets and liabilities, to the Annual General Meeting.

Property and Funds.

31.—Every paper accepted by the Society for reading or publication shall become the absolute property of the Society, unless the author's rights be specially reserved.

32.—The property of the Society shall be invested in Trustees chosen by the Council. The Trustees shall be eligible to any other office in the Society.

Notices.

33.—The posting of a notice to the registered address of a Member or Associate shall be deemed service of a notice. Members or Associates residing abroad shall name a place of address in the United Kingdom. In default of such address, the posting of a notice in a conspicuous place at the Offices of the Society shall be deemed to be a notice.

34.—A notice forwarded by post to the Secretary of a Branch or Allied Society shall be deemed a notice to the Members of the Society of which he is Secretary.

Interpretation of Rules.

35.—In the Interpretation of these Rules words importing the singular number only, include the plural, and words importing the masculine gender only, include the feminine.

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SOCIETY FOR PSYCHICAL RESEARCH.

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