PROCEEDINGS OF THE GENERAL MEETING ON

Monday, July 5, 1886.

The twenty-first General Meeting of the Society was held at the Rooms of the Society of British Artists, Suffolk Street, Pall Mall, on Monday, July 5, 1886.

PROFESSOR BALFOUR STEWART, F.R.S., PRESIDENT, IN THE CHAIR

The following paper was read :—

IV.

THE POSSIBILITIES OF MAL-OBSERVATION IN RELATION TO EVIDENCE FOR THE PHENOMENA OF SPIRITUALISM.

BY C. C. MASSEY.

In his opening address at the first general meeting of this Society, the President, Professor Sidgwick, while expressly evading "the difficulties of determining in the abstract what constitutes adequate evidence" of the phenomena called Spiritualistic (as well as of thought-reading and clairvoyance) nevertheless concluded with the following general statement of the sort of proof at which we ought to aim.

"We must drive the objector," he said, "into the position of being forced either to admit the phenomena as inexplicable, at least to him, or to accuse the investigators either of lying or cheating, or of a blindness or forgetfulness incompatible with any intellectual condition except absolute idiocy." [1]

As I am about to maintain that much of the existing evidence for the phenomena in question already places objectors in the dilemma thus succinctly indicated by Professor Sidgwick, I must ask leave to point out, with some approach to particularity, how, and under what circumstances, I conceive the dilemma to arise. This is the more necessary, because it will have at once occurred to all of us that the dilemma does not arise in the case of conjuring tricks, to which the

[1] *Proceedings.* (Vol. i., p. 16.)

phenomena we are considering are usually referred by the incredulous. No one thinks the worse of his own or another's intelligence for not discovering a conjurer's trick; but most of us would feel ashamed of mistaking a conjurer's trick for a genuine manifestation of an unknown force. Nor is there, so far as I am aware, any mediumistic phenomenon on record which absolutely defies simulation under all circumstances and all conditions of observation. The whole evidence is a question of these circumstances and conditions, and to demonstrate that a conjurer can baffle observation under inferior conditions of these phenomena is quite beside the mark. We have to judge the evidence, as to answer an argument, at its best. The success of the conjurer with even the most intelligent spectators, depends on their overlooking the true conditions of the performance, and this again depends on their attention not being directed to the particular operation which decides, or is the condition of, the result. Any spectator who knew *exactly* what to observe would have already discovered the trick, and a very little practice in observation would enable him to detect the actual *tour de force* by which it was accomplished. This remark, of course, does not apply to the secrets of machinery, or elaborate scientific apparatus, and it is perhaps true that pseudo mediums and thaumaturgists have availed themselves of such mechanical means. But none of the phenomena relied upon by Spiritualists and the maintainers of a psychic, or nerve, force are at all explicable by contrivances which could baffle the well-informed observation of even an adept. If the medium is a conjurer, he may, of course, have some simple preparations, but to bring them into play he must succeed, as other conjurers do, by the ignorance of the witnesses of the particular thing to be done, on which all depends. By this particular thing I mean, as will appear when we come to consider the opportunities of a conjurer at a mediumistic séance, one definite act or operation which, under the circumstances of the experiment, has become the indispensable *condition* of the conjurer's success. In an ordinary conjurer's performance this never is known, and observation, therefore, wavers and is distracted by this uncertainty. The most important thing is, perhaps, just what never would occur to the mind as important at all. I shall endeavour to show (1) that at mediumistic sittings, under the best conditions, this uncertainty does not and cannot exist; and (2) that even inferior powers of observation, equipped with knowledge of the exact thing to be observed, and associated with average intelligence, are competent to baffle any conjurer in the world, provided only that the conditions of observation are physically easy. There must be sufficient intelligence to know that a conjurer's sole chance in that case lies in the possibility of withdrawing your attention from the single perception required of you. Very little will is required to be secure against this, because a dominant idea, even if for a moment in abeyance, is immediately re-excited by any foreign action, possibly designed to lay it completely asleep. This especially applies, as I know by my own experience in the slate-writing sittings, to offers of conversation, changes of hand induced by fatigue, and so forth; jealous vigilance is aroused by the smallest modification in the conditions.

In the June number of our *Journal*, only issued a few days ago, Mrs. Sidgwick takes up a position apparently opposed to the reception of general testimony of these phenomena, so far as they occur in the presence of professional mediums, and must be established by observation of any degree of continuity. This is a plain issue, and one on which it behoves us to have a clear opinion.

Now there is one broad distinction between the medium and the conjurer which makes it possible to get evidence with the one which the performances of the other can never afford. On the hypothesis of mediumship we should expect to be able to reverse one *essential* relation of conjurer to spectator, so that the latter shall be no longer a mere observer or looker-on, but shall be himself the principal actor in all the preparations, while the physical activity of the medium is reduced to the *minimum*. The conjurer can only mask his essential performance by his incidental and apparent performance. By this activity he obtains two indispensable advantages. For, first, he imposes on the spectator a multitude and succession of observations in uncertainty of the precise essential point to which attention should be directed to prevent or detect trickery. And, secondly, he is enabled to distract attention, or to impose inferior or impossible conditions of observation with regard to the particular operations which have to be concealed. We may, therefore, be quite sure that in order to baffle a conjurer it is only necessary to undertake all preliminary manipulations ourselves, and so to make our arrangements that mere observation has only to be directed to a single fact of sense perception, or at most to two or three such facts well within an average capacity of simultaneous or successive attention; and, further, that the conditions of this observation should be the easiest possible. If, moreover, we can reinforce the confidence which everyone must feel in his own senses up to a certain point by adequate contrivances to dispense with actual observation of any important particulars, we shall reduce the problem to the most extreme simplicity that human experience admits of. For testimony to phenomena obtained under such conditions to be of the highest evidential value, it is only necessary that the witness should in some way assure us that the observation, thus simplified and directly designated by the preparations, was in fact made, or that when this assurance is not explicitly given, it is only because failure of the observation, under the circumstances, would have been inconsistent with a sane and waking condition. If there is any possibility left for observation to guard against, we must be satisfied that it was either such as *could not* have escaped attention, or one to which attention *was actually* directed. In that case, he only can question whether observation has really performed its office who doubts the capacity of the human mind and senses to take in the most elementary facts of perception.

Now I submit that testimony of this highest value exists, and exists even in abundance. But it will be perfectly idle to adduce cases in illustration of this proposition, if every case in which the evidence is apparently free from defect is assumed to be incorrectly described. That is the assumption which Mrs. Sidgwick is prepared to make, because in her view observation is defective, not only in what it omits, but in what it seems to assert.[1] I shall presently endeavour to show that this can only be true of general statements which fail to discriminate the elements of an observation, and which under the name of observation give us only a mental result instead of testifying to individual and indivisible acts of perception. And as to important elements which are assumed to be lost for observation, we shall have to see of what nature they must be, of what character and dimensions—in order that they may affect the result. And then the appeal must be to universal experience of the degree to which the senses can and cannot be stimulated by external occurrences without arousing attention sufficient for lively perception with notice by a waking man. It is true that mental preoccupation is *pro tanto* sleep in regard to everything upon which the mind is not actually engaged, and this preoccupation it is which the conjurer is supposed to induce. But it is always the nature of the particular act in each case to be performed by the conjurer unobserved, which must determine the degree of preoccupation in the witness necessary for the accomplishment of the former's purpose. Now, as regards this, if the *positive* observations of the witness respecting the physical conditions are generally trustworthy, we get thereby a measure of the conjurer's indispensable physical interference, and thus of the degree of stimulation to the witness's senses by such interference.

[1] "The juggler's art consists largely in making things appear as they are not. Can we suppose that it has caused facts which did not occur to be imagined, and facts which did occur to be overlooked, to the extent required to make the cases before us explicable by ordinary human agency?" (*Journal* p. 332.) As Mrs. Sidgwick has "no hesitation in attributing the performances" (those with which she was dealing) "to clever conjuring," though she says that "certainly some of the phenomena *as described* seemed to be inexplicable by the known laws of nature," this *positive* conclusion evidently requires the *positive* assumption of mis-description.

In proportion to that stimulation must be the degree of preoccupation for observation to fail. So that it will not do to urge the abstract truth or experience of the liability of the mind to momentary preoccupation during a prolonged observation : we must in each case compare the degree of preoccupation supposable with the degree that is then and there requisite for the conjurer's purpose. And here the appeal must again be to common experience.

Having regard to the limits of our time, I am obviously unable to do more on the present occasion than offer a few samples from the bulk, and even as to some of these I must content myself with a brief reference to the essential character of the evidence as illustrating the points I have in view.

Now I will first take two or three of the experiments devised and instituted by the late Professor Zöllner with the medium Slade, selecting the briefest suitable accounts that I can find. The following will be found at p. 39 of the translation entitled *Transcendental Physics.* Zöllner says :—

I took a book-slate, bought by myself ; that is, two slates connected at one side by cross-hinges, like a book, for folding up. In the absence of Slade, I lined both slates within, on the sides applied to one another, with a half-sheet of my letter-paper, which immediately before the sitting, was evenly spread with lamp-black soot. This slate I closed, and Slade consented to my laying it (which I had never let out of my hands after I had spread the soot) on my lap during the sitting, so that I could continually observe it to the middle. We might have sat at the table in the brightly-lighted room for about five minutes, our hands linked with those of Slade in the usual manner *above* the table, when I suddenly felt on two occasions, the one shortly after the other, the slate pressed down upon my lap, without my having perceived anything in the least visible. Three raps on the table announced that all was completed, and when I opened the slate there was within it, on the one side, the impression of a *right* foot, on the other side that of a left foot.

And this was just what Zöllner had himself desired with a view to obviate possible objections to a similar phenomenon obtained previously under inferior conditions.

Now I submit that this experiment reduces the supposition of mal-observation to the extreme of absurdity. It would appear from the account that the experiment was proposed to Slade only immediately before it was tried, so that there was no time for the preparation by Slade of a slate to be substituted for Zöllner's. But as we are now on the point of observation, I will suppose for the moment that possibility. It will then be seen that Zöllner's statement expressly excludes the possibility of a substitution *before* he placed the slate on his lap, so that Slade would have to effect it with his feet afterwards, and that though the slate was all the time partly in Zöllner's view, and when the least sensation would have instantly drawn his eyes to the spot.

I pass to another case from the same source (p. 81).

The experiment, says Zöllner, was as follows :—

I took two bands cut out of soft leather, 44 centimètres long (about 15 inches) and from five to 10 millimètres broad (⅕ to ⅖ inch), and fastened the ends of each together, and sealed them with my own seal. The two leather bands were laid separately on the card-table at which we sat ; the seals were placed opposite to one another, and I held my hands over the bands (as shown in the plate). Slade sat at my left side, and placed his right hand gently over mine, I being able to feel the leather underneath all the time. Presently, while Slade's hands were not touching mine, but were removed from them about two or three decimètres (from eight to 12 inches), I felt a movement of the leather bands under my hands. Then came three raps on the table, and on removing my hands the two leather bands were knotted together. The twisting of the leather is distinctly seen in the plate, copied from a photograph. The time that the bands were under my hands was at most three minutes. The experiment was in a well-lighted room.

Here the arrangements had reduced the office of observation to the simple points (1) whether the bands lying before his eyes on the table were in fact connected at the moment Zöllner covered them with his hands; (2) whether Slade could and did touch them when they were thus covered; (3) whether Slade could or did either knot them at the moment Zöllner removed his hands, or then substitute others for them. If anyone thinks that either of these things could have happened unobserved, I can only say that I am sure he will not get any honest conjurer in the world to agree with him.

The following fact, from my own experience with the same medium, Slade, may be fitly adduced here.

It was in New York, on the evening of the 14th October, 1875, and was publicly recorded by me shortly afterwards, from notes taken immediately on my return to my hotel after the sitting. And my recollection of it is still perfectly distinct. It was at Slade's own room, brightly lighted with gas. The floor was carpeted. We sat at a table in the centre of the room, three of us, Slade opposite to me, my friend Colonel Olcott at the end on my left and on Slade's right. There was no one else present. Slate-writing experiments were proceeding between Olcott and Slade, when a chair *on my right*—at the end of the table opposite Olcott—was thrown down by some undetected force. I got up, felt round the chair for any attachments, and then, producing a tape measure I carried with me for the purpose of my investigation, I took the shortest distance between the medium and the chair, as the latter lay upon the floor. It was just five feet, and on resuming my seat I could see a good clear space between the table and the prostrate chair. Meanwhile, Slade had not moved from his seat, and I requested him not to stir, and asked that the chair, which lay on my right, and which I could watch as nothing intervened between me and it, *might be picked up and be placed by me.* There was an interval of perhaps two minutes, during which time the medium, still engaged with Colonel Olcott, remained seated in the same position, as I know, because my range of vision from where I sat took in the whole general situation, though, as the prostrate chair and the free space of floor between it and the table were the main things to be observed, I kept my eyes steadily in that direction, and never lost sight of chair and floor for a moment. Suddenly I saw the chair move along the ground a few inches towards me, and in a direction slightly oblique to the table, and then, as I watched it and the open space between it and the table, medium, and everything else, it was jumped upon its legs and deposited at my right side, just as if some one had picked it up in order to take a seat beside me. No mediumistic phenomenon that I have witnessed has made stronger or more lasting impression upon me than this one.

On another occasion I was sitting alone with Slade in bright daylight, when his chair was drawn suddenly and considerably back, with him upon it. I at once pushed back my own chair from the table so as to command a full view of Slade's whole person. I then asked that my chair, with me upon it, might be drawn back. This was done almost immediately, to the extent of two or three inches. There could be no question either of Slade's agency in this, or of any unconscious action of my own, as I could, and did, see Slade from head to foot, and there was no time for gradual tension of the muscles of my own legs and feet against the floor in analogy with the process which no doubt often occurs in table-turning or tilting with contact of hands. I could multiply instances from my own experience in which observation has been similarly simplified and facilitated. When this is the case—and it will be found to be the case in a very large number of records—I contend that it is perfectly indifferent whether we are experimenting with a professional or with a private medium; and that the largest margin we can rationally allow for unknown possibilities of conjuring cannot prevent the issue being reduced, as is desired, to one simply of the *veracity* of the witness.

I must, therefore, take exception to the statement of Mrs. Sidgwick, in the paper read at our last meeting, that the evidence is "so seldom experimental; that is, that the observer so seldom knows beforehand what will be the precise phenomena and conditions."[1] The precise pheno-

[1] Abstract of Mrs. Sidgwick's Paper in the May number of the *Journal.* I had not before me the full text, now published in this volume.

menon in the case of the slate-writing mediums, for instance, is always known beforehand, unless we confuse the term "phenomena" and "conditions," *i.e.*, conditions of observation. The only variation is in the possibility of imposing tests supplementary to ocular observation, and these usually originate with the observer himself. I may instance a case recorded only the other day (*Light*, May 22nd), in which the observer, Major le Taylor, went three times to Mr. Eglinton, each time obtaining the writing under a new test premeditated by himself. He did this on the very principle recommended by Mrs. Sidgwick, of allowing a very large margin for conjuring and for defects of observation. As to the conditions of observation, they are known beforehand in all those cases—and very numerous they are—in which the phenomenon is obtained under conditions of observation prescribed by the observer himself. In Zöllner's above cited cases (and others could be adduced from his book) phenomenon, test, and conditions of observation, were all prescribed by himself. In both my cases of the chairs (especially the first mentioned) the phenomenon was prescribed by myself, and, equally in both, the conditions of observation were the best conceivable, because the very simplest. Mr. Eglinton's mediumship is especially remarkable for successes obtained under tests and conditions imposed by observers. In addition to Major le Taylor's case, may be mentioned, as illustrations, several others with this medium. Thus, on January 5th, of last year, Mr. D. H. Wilson, M.A., goes with his wife and sister to Mr. Eglinton—these four being the only persons present. Mr. Wilson suggests obtaining by psychography an extract from a closed book.

Accordingly (he says) Mrs. Kimber (his sister) wrote on a slate the number of a *page;* Mrs. Wilson the number of a *line*, and it remained for me to choose the book from which Mrs. Wilson's line of Mrs. Kimber's page was to be written by psychography on the slate. For this purpose, with closed eyes,[1] I took a book from the medium's shelves, which held about 200 volumes. A crumb of pencil was placed upon the slate, on which Mrs. Kimber and Mrs. Wilson had written the number of the page and line respectively. A second slate of exactly the same size and form was placed over this one, and the book was put by myself on the top of the two slates.

Mr. Eglinton and Mrs. Kimber rested their hands on the book.

It should be noted that :—

1. Precaution had been taken that no one besides Mrs. Kimber knew what number she had written on the slate to express the page to be recited, the same being true of the number Mrs. Wilson had written to express the *line* of that page.

[1] The experiment was partly devised to test the presence of an intelligence outside the minds of all the sitters.

2. The slates and book were all on the top of the table immediately before the eyes of all present. (The sitting was by daylight.)

3. The medium did not touch the book until the moment when he and Mrs. Kimber rested their hands thereon. It had been handled by myself alone.

After the lapse of a few seconds the sound of writing was heard within the slates. Upon the usual signal of three raps (also seemingly within the slates) to indicate the end of the experiment, I examined the slates, and found the following sentence, written on the under one, with the pencil resting on the full stop at the end. (I may mention that all the writings throughout the entire séance were conscientiously punctuated, and that every *t* was crossed and every *i* dotted.)

"Page 199, line 14, is a table, the last word is '0'."

Mrs. Kimber had written 199, and Mrs. Wilson had written 14.

I then opened the book (*Ghose's Indian Chiefs, Rajahs*, &c., Part II.) and turned to p. 199, which commences thus: "Table A. Estates belonging to the Hon. Maharaja Jotundra Mohun Tagore Behadur," &c.

The 14th line is as follows:—

"Shikharbâte, 24 Pargannas, 210 0 0."

Now though the form of Mr. Wilson's statement that the book had been handled by himself alone, before he put it on the slates as they lay upon the table before the eyes of all present, does not expressly or necessarily import that it had never been out of his hands from the moment he removed it from the shelf, I do not think anyone can seriously suggest that Mr. Eglinton had the several opportunities unobserved:—

1. Of reading page and line on the slate, although we are told that precaution (very easy to take) was taken against this very thing.

2. Of getting possession of the book, opening it, and finding page and line.

3. Of writing those 12 words and figures with their six *t's* and *i's* all crossed and dotted on the slate.

Were that possible, my own conclusion would be that human observation, under the simplest and easiest conditions, and with attention directed to the self-devised tests to be guaranteed by the observation, is absolutely worthless for any purpose and under any circumstances whatever. And I would here refer to the sensible remarks of Mr. G. A. Smith, upon a similar experience of his own with Mr. Eglinton, which will be found at p. 301 of the *Journal*.

Other investigators with Mr. Eglinton have obtained tests similar to the above, with variations devised by themselves, making the operations to be performed unobserved by the medium still more complicated. I will only here refer to the experiment recorded by Mr. J. S. Farmer and Mr. J. G. Keulemans in *Light* of October 17th, 1885. It is too long to quote, but should be referred to as showing what elaborate and ingenious arrangements observers can sometimes make for their satisfaction with results entirely successful. Other cases will be found in the June number of the *Journal.* The following instance, recorded by Mr. Alfred Russel Wallace in the *Spectator* of October 7th, 1877, is another illustration of the security an investigator can command by taking all the arrangements into his own hands. The medium was Dr. Monck. Mr. Wallace says :—

The sitting was at a private house at Richmond, on the 21st of last month. Two ladies and three gentlemen were present, besides myself and Dr. Monck. A shaded candle was in the room, giving light sufficient to see every object on the table round which we sat. Four small and common slates were on the table. Of these I chose two, and after carefully cleaning and placing a small fragment of pencil between them, I tied them together with a strong cord, passed around them both lengthways and crosswise, so as effectually to prevent the slates from moving on each other. I then laid them flat on the table, without losing sight of them for an instant. Dr. Monck placed the fingers of both hands on them, while I and the lady sitting opposite placed our hands on the corners of the slates. *From this position our hands were never moved till I untied the slates to ascertain the result.* After waiting a minute or two, Dr. Monck asked me to name any short word I wished to be written on the slate. I named the word "God." He then asked me to say how I wished it written. I replied "Lengthways of the slate," and then if I wished it written with a large or small g. I chose a capital G. In a very short time writing was heard on the slate. The medium's hands were convulsively withdrawn, and I then myself untied the cord (which was a strong silk watchguard, lent by one of the visitors), and on opening the slates found on the lower one the word I had asked for, written in the manner I had requested, the writing being somewhat faint and laboured, but perfectly legible. The slate with the writing on it is now in my possession.

The essential features of this experiment are that I myself cleaned and tied up the slates, that I kept my hands on them all the time, that they never went out of my sight for a moment, and that I named the word to be written and the manner of writing it after they were thus secured and held by me. I ask, how are these facts to be explained, and what interpretation is to be placed upon them?

ALFRED R. WALLACE.

I was present on this occasion, and certify that Mr. Wallace's account of what happened is correct.

EDWARD T. BENNETT.

In other cases it is the character itself of an unexpected phenomenon which leaves no escape from the evidence other than suppositions of mendacity or hallucination. The following instance of this from Zöllner is so remarkable that at the risk of again quoting what is already known I must give it at length, which I am the rather induced to do, because Mrs. Sidgwick has apparently not thought the evidence of this distinguished man of science to be worthy of any special mention.[1] The séance was at the house of Zöllner's friend, Herr von Hoffman mid-day on May 6th, by bright sun-light. Zöllner says:—

I had, as usual, taken my place with Slade at the card table. Opposite me stood, as was often the case in other experiments, a small round table near the card-table, exactly in the position shown in the photograph illustrating further experiments to be described below. The height of the round table is 77 centimètres (about 2ft. 4in.), diameter of the surface 46 centimètres (about 16in.), the material birchen wood, and the weight of the whole table 45 kilogrammes. About a minute might have passed after Slade and I had sat down and laid our hands, joined together, on the table when the round table was set in slow oscillations, which we could both clearly perceive in the top of the round table rising above the card table, while its lower part was concealed from view by the top of the card table. The motions very soon became greater, and the whole table approaching the card-

[1] This was true so far as my recollection went, from hearing the paper read. But it will be seen from the text as now published, that Mrs. Sidgwick does advert, in some detail, to parts of Zöllner's testimony. So far as her objection to it refers to the absence of tests excluding the necessity of all "continuous observation," it would be obviously beyond the scope of a paper designed to vindicate the trustworthiness of observation to reply to it. But with regard to the objection (see foot-note, *ante* p. 65) to the celebrated experiment of the true knots in an endless cord, I think the value of the objection will be best appreciated by a reference to some conditions of the experiment, as the latter is not to be confounded with the one with the leather bands, of which I have given the account above. (I have italicised the word "*immediately*," in Zöllner's statement, for its obvious importance in relation to any suggestion of substitution *before* the experiment actually began. The emphasization of other words is by Zöllner.) After describing the cord, its dimensions, mode of knotting, and sealing the ends, &c., Zöllner says: "The above described sealing of two such strings, with *my own* seal, was effected *by myself* in my apartments, on the evening of December 16th, 1877, at nine o'clock, under the eyes of several of my friends and colleagues, and *not* in the presence of Mr. Slade. Two other strings of the same quality and dimensions were sealed by Wilhelm Webber with *his* seal, and in his own rooms, on the morning of the 17th of December, at 10.30 a.m. With these four cords, I went (17th December) to the neighbouring dwelling of one of my friends, who had offered to Mr. Henry Slade the hospitalities of his house, so as to place him exclusively at my own and my friend's disposition, and for the time withdrawing him from the public. The séance in question took place in my friend's sitting-room *immediately* after my arrival. I myself selected one of the four sealed cords, and, in order never to lose sight of it before we sat down at the table, I hung it round my neck—the seal in front always within my sight." The knots were obtained in a few minutes, the seal and Slade's hands having never been out of sight. The suggestion being that Slade substituted a previously prepared cord of his own, it is to be observed that such a substitution was *the very possibility* which Zöllner showed that he had in view by his precaution of hanging the cord round his neck. As there was *no delay*, such as, supposing Zöllner to have previously parted with the custody of his cords, would have

table, laid itself under the latter, with its three feet turned towards me. Neither I, nor, as it seemed, Mr. Slade, knew how the phenomenon would further develop, since during the space of a minute which now elapsed nothing further occurred. Slade was about to take slate and pencil to ask his "spirits" whether we had anything still to expect, when I wished to take a nearer view of the position of the round table lying, as I supposed, under the card-table. To my and Slade's great astonishment we found the space beneath the card-table completely empty, nor were we able to find in all the rest of the room that table which only a minute before was present to our senses. In the expectation of its re-appearance we sat again at the card-table, Slade close to me, at the same angle of the table opposite that near which the round table had stood before. We might have sat about five or six minutes in intense expectation of what should come, when suddenly Slade asserted that he saw lights in the air. Although I, as usual, could perceive nothing whatever of the kind, I yet followed involuntarily with my

imposed on him the task of "continuous observation" of them, and have *conceivably* afforded a conjurer an opportunity, we cannot put the supposed substitution *before* the experiment. But Mrs. Sidgwick's suggestion that it may have been *afterwards*, *i.e.*, "after the string was taken off the neck again, perhaps while it was being arranged on the table," is equally inadmissible, (1) because we can say, with as near approach to certainty as possible, that the presence or absence of the four knots must have been ascertained at the moment of removal from the neck, or already before the removal, when the indication of success would induce an *instant* examination; and (2) because there is no interval assignable for "continuous observation" in the ascertainment of so simple a fact as the presence or absence of knots on a cord in a clear light, even if the fact had not been already ascertained by sight or touch before Zöllner actually took the cord from his neck. I confess it would not have occurred to me to anticipate such a suggestion as this. Nor can I see the least necessity for Zöllner mentioning the fact of trials on previous days. He showed his own appreciation of that fact, and of the supposable possibilities consequent upon it, by the very precautions taken. Indeed, I think the fact of former trials still further evinces Zöllner's extreme caution, since he would not trust to the strings already used, but either sealed new ones, or at least re-sealed the old, on the eve of the successful experiment. This circumstance, the then careful and elaborate sealing of the cords, even suggests that this particular precaution was a new one altogether, for which Slade would be unprepared, especially as Zöllner emphasises the fact that the sealing was performed in the absence of Slade. But the evidence stands in no need of this inference, for the reasons already stated. *Logically*, there was of course no obligation upon Zöllner to mention a fact which it would be legitimate to *suppose* in criticising evidence of this character, if the evidence did not expressly *exclude* it. The reader will judge whether there is any opening for Mrs. Sidgwick's inference that the possible importance of the fact had not occurred to Zöllner, or whether her consequent assumption that he may "not only have omitted to mention, but failed to see, the importance of even obvious precautions" is as violent and unwarranted as it seems to me to be. And I may here add the remark, that if "continuous" observation means *prolonged* observation, none was necessary in this, as in many other successful experiments; whereas if Mrs. Sidgwick's definition refers to *any* interval, however short, it would apply to all observation whatever, and the word "continuous" is misleading.

gaze the direction to which Slade turned his head, during all which time our hands remained constantly linked together on the table; under the table my left leg was almost continually touching Slade's right in its whole extent, which was quite without design, and owing to our proximity at the same corner of the table. Looking up in the air, eagerly and astonished, in different directions, Slade asked me if I did not perceive the great lights. I answered decidedly in the negative; but as I turned my head, following Slade's gaze up to the ceiling of the room behind my back, I suddenly observed, at a height of about five feet, the hitherto invisible table, with its legs turned upwards, very quickly floating in the air upon the top of the card-table. Although we involuntarily drew back our heads sideways, Slade to the left and I to the right, to avoid injury from the falling table, yet we were both, before the round table had laid itself on the top of the card-table, so violently struck on the side of the head, that I felt the pain on the left of mine fully four hours after this occurrence, which took place at half-past 11.

But I am not prepared to admit that it is necessary to have recourse to exceptional manifestations, or even to manifestations under exceptional conditions of observation, to establish these facts in rational belief. With regard to psychography, for instance, I contend that locked slates, tied up slates, folding slates, your own slates, slates above the table when the writing is obtained, are all really dispensable precautions. What we most require, in order to be secure that the essential facts are within the compass of our observation, and that observation itself has not been distracted or relaxed, is that the phenomenon shall occur with simplicity and directness. If there is delay with changes of conditions, you must regard every such change as the beginning of a new sitting, and make a careful re-examination of the slates. If you do this effectually, not merely taking a careless glance to be able to say you have done it at all, the task of observation is thoroughly simplified under usual conditions. The following case from my own experience with Mr. Eglinton will show the extent of the claim I make for average powers of observation as against the possibilities of conjuring. The sitting was on April 10th, 1884. I wrote the account of it in the evening of the same day, and it was reported in *Light* of April 19th. The only other sitter besides myself and the medium was one of our Vice-Presidents, the Hon. Roden Noel, who fully corroborated my statement. We sat in broad daylight. We used Mr. Eglinton's slates, of which there was a pile upon the table at which we sat. I sat next to the medium, on his right, Mr. Noel was on my right. Passing over some preliminary experiments, in which writing in small quantities was obtained, I desire to challenge judgment on the question of mal-observation in what follows, which I copy from my own report in *Light*:—

Mr. Eglinton now laid one of two equal sized slates ($10\frac{5}{8}$ inches by $7\frac{5}{8}$)

flat upon the other, the usual scrap of pencil being enclosed. *Both* slates were then, *as I carefully assured myself*, perfectly clean on *both* surfaces. He then *forthwith, and without any previous dealing with them*, presented one end of the two slates, held together by himself at the other end, for me to hold with my left hand, on which he placed his own right. I clasped the slates, my thumb on the frame of the one (⅞-inch), and three of my fingers, reaching about four inches, forcing up the lower slate against the upper one We did not hold the slates underneath the table, but at the side a little below the level. Mr. Noel was thus able to observe the position. Mr. Eglinton held the slates firmly together at his end, as I can assert, because I particularly observed that there was no gap at his end. I also noticed his thumb on the top of the slates, and can say that it rested quite quietly throughout the writing, *which we heard almost immediately*, and continuously, except when Mr. Eglinton once raised his hand from mine, when the sound ceased till contact was resumed.

We heard the sound of writing distinctly, yet it was not, I think, quite so loudly audible as I remember with Slade. When the three taps came, denoting that the "message," was finished, *Eglinton simply removed his hand from the slates, leaving them in my left hand*, also quitting contact of his other hand with my left, I took off the upper slate, and we saw that the inner surface of one of them was covered with writing, 20 lines (118 words), from end to end written *from* the medium, and one line along the side by the frame, and "good-bye" on the other side. The writing was in straight lines across the slate, all the lines slanting from left to right. It begins about an inch from the top; from the bottom it is continued along one side (one line) and then there are three lines in the inch-deep space at the top, written in the reverse direction to that of the body of the message. The ability to produce the writing in any direction is thus shown. The writing is flowing, easy, and with a distinct character, as of an educated penman. I took the slate away, with me, and it is now in my possession.

I am glad that I took this latter precaution, for a reason to be mentioned. Everyone, I suppose, will agree that the production of all this writing, as described, by the medium while we held the slates, was absolutely and entirely impossible. The question is thus *apparently* reduced to the single point to which I wish to reduce it, whether such average powers of observation as mine and Mr. Noel's would be so deceived as to make our statement that Mr. Eglinton, after enclosing the pencil within the slates which we *then* "carefully assured" ourselves were *both* quite clean on *both* surfaces, "forthwith" and "without any previous dealing with them," presented those *same* slates to me to hold—whether, I say, our observation could be so deceived as to make that statement inconclusive on that important point. But as it is imaginable that a thin sheet of slate, already inscribed on one side, might be loosely fitted into the frame of one of the slates used, clean surface uppermost, so as to fall into the frame of the other slate, written side uppermost, when the first was placed upon the second, it is fortunate that I was able to exclude that suggestion by my possession of the slate on which the writing appeared, which, by-the-bye, was wrapped in paper, either by myself or by Mr. Eglinton—under my eyes, at my request, and carried away by me, *immediately* after we had examined the writing, the sitting being then closed.

The above case, therefore, aptly raises a question which I think has been greatly confused by vague apprehensions of unknown possibilities of conjuring, apprehensions, I may add, not at all sanctioned by the pretensions of conjurers themselves. So far as the art of conjuring relies on the fallibility of observation, the success of the conjurer depends on his being able to impose the conditions of observation at the critical stage in his proceedings. For very simple observations, such, that is, as are resolvable into two or three elementary acts of perception, are not fallible if these acts of perception are really performed. The conjurer has to prevent their being performed, while he deceives the mind into the impression that they have been performed. Under certain conditions this is easy to him; whereas under conditions not imposed by himself it is totally impossible. Now in studying evidence adduced by others there is one sure test for determining whether the conjurer's opportunity is or is not excluded by the evidence—I mean in cases where the statements of the witness, if taken simply at their *verbal* worth, would sufficiently exclude all possibilities of conjuring. It is only the best testimony—perfect *honesty* of statement being supposed —of which the verbal or apparent worth is a true measure of its real worth. And the reason of this is that very composite facts are often not analysed by the witness, and that an observation comprising several distinct acts of sense-perception is stated generally, as though it were a single and indivisible perception. We have then imposed upon us as evidence a conclusion of the witness's mind in place of an observation of his senses. The proof is not then reduced, as we desire to reduce it, to a question of veracity. For this purpose we must have particularity of statement, evidence that the witness has himself analysed the observation into the acts of perception constituting it, and that at the time of the observation. But however people may unconsciously misrepresent or exaggerate—as undoubtedly happens—this innocent looseness or inaccuracy belongs only to *general* statements of matters of fact, and as soon as the demand is made upon the witness for greater definitude, either at least a confessed lapse of memory exposes the worthlessness of the evidence, or the latter degenerates into conscious mendacity. Much of the value of cross-examination in judicial proceedings, for instance, depends upon the presumption that precise and definite misstatements cannot be *bonâ fide.* And the art of cross-examination—so far as this has for its genuine aim the discovery of truth—largely consists in reducing a general statement to the particular ones which it really involves. Now a scientific statement of fact is such a statement as leaves nothing to be elicited by this sort of cross-examination. And in considering the evidential value of the observations with which we are now concerned, we have always to see if possibly essential facts in the narration are capable of further analysis. The note of an uncritical judgment, either in making or receiving statements which should be scientifically accurate, is the unconscious presumption of the component elements of the fact stated, or to speak more accurately, of the several facts of observation by which the resultant fact is ascertained.

I submit that we have here the whole secret of the possible success of a conjurer who is without confederates or artificial appliances. We have at the same time a sure test for determining the value of observations with professional mediums, who must continue under the suspicion of being conjurers till these phenomena are generally recognised, which will perhaps not be until the laws of their occurrence are a little understood. I therefore respectfully urge that the objection to rely upon investigations with professional mediums is especially unworthy of the scientific spirit in which this Society professes to examine evidence. Our standard should be the highest, our criticism the severest; but the best testimony will leave no room for suggestions of mal-observation, and then it will only remain to see if, supposing the allegations to be strictly honest, the facts are still explainable by any recognised agency. We have heard of the necessity of allowing a wide margin for unknown possibilities of conjuring, and that sounds plausible enough until we come to ask what conjuring means, and must mean, under the conditions of these experiments. We then see that the margin for possibilities of conjuring is really a margin for possibilities of mal-observation. But when we get to the ultimate unit of observation —the indivisible, elementary fact of sense-perception—mal-observation by the attentive mind is no longer possible, and testimony which shows that there existed a mental direction to these particulars is testimony which excludes the margin for everyone who will not cheat himself with words for the evasion of his critical responsibility. I am, of course, aware that what I have here called "the indivisible, elementary fact of sense-perception" is further resolvable with regard to the primary functions of mind and sense; but for all that, the simplest nameable fact remains the starting-point of all experience, and illusion in experience begins with the mental combinations of which that is the unit. For all mere illusion or misinterpretation in relation to this simplest element of experience—as when a rope upon the path is taken for a snake— results from imperfect conditions of observation, or (what is the same thing from the subjective side) from pre-occupation of the mind by its own concepts. It follows that as long as the attention is given to an indivisible fact under proper conditions of observation, the conjurer's opportunity has not arisen. It arises first with the opportunity of the observer's own mind for self-deception. And if the witness is strictly veracious, it is logically certain that his evidence will itself betray to the critical eye the point or points at which the conjurer's operations were possible, if possible they in fact were.

But as general remarks on such a subject as the present require to be illustrated, let us consider what may be supposed to happen on a particular occasion, and what, in that case, an honest witness will and will not say. Suppose that at a conjuring performance for the simulation of psychography, the conjurer has already succeeded in writing unobserved upon one side of the slate, and wishes now to make you believe that both sides are clean before depositing the slate, with the inscribed side downwards, on the table, to be turned up when the phenomenon is supposed to have come off in that position. Now, if at this critical moment you do not prescribe your own mode of examination, either by taking the slate in your own hand and turning it over, or by seeing that the conjurer turns it slowly round before your eyes, he may be able, by a little manipulation, aided by a little talking and delay, or with the assistance of another slate for purpose of confusion, to present the same side to you twice over and make you think that you have seen both sides. (This, I should say, is the explanation recently suggested by the famous German conjurer, Hermann, of Berlin,[1] of the *modus operandi* in such a case.) But if that were so, the witness *could* not innocently use terms *expressly and definitely* inconsistent with what really happened; he could not, for instance, honestly say, as I said in the report I have read to you, that the medium did something "forthwith," "without any previous dealing with the slates," which the witness "*then* carefully assured himself" to be "both clean on both sides," whereas it was in the very fact of delay, of previous dealing, and of neglect of "careful" assurance that the supposed medium has found his fraudulent opportunity. The honest witness could not so frame his statement, because, though he might honestly forget, he could not honestly *invent* specific and positive acts of perception, for the appearance of which no mental inference or interpretation could be responsible. But we have an instance—an actual instance—ready to our hands of how he *might* express himself in such a case.

Mrs. Sidgwick quotes accounts from a lady friend of hers of several conjuring experiments in slate-writing as illustrating the fallibility of

[1] As this paper is going to press, I have received information that the Hermann here referred to (author of the article in the German *Sphinx*, from which the above and a subsequent statement is taken) is not the true Hermann of conjuring renown, but only a manufacturer of conjuring apparatus. The true Hermann is said to be now in London and about to experiment with Mr. Eglinton.

observation.[1] Now I think every careful reader of these accounts will be struck by the abbreviated form of them, and by the frequent violation of the canon of evidence above mentioned, namely, that a composite observation shall not be stated generally, as if it were a single and indivisible perception. We should want to cross-examine this lady upon nearly every line of her statement in order to appreciate its evidential worth. But I will here confine myself to the single point of due examination of the slates in the experiment in which the writing was apparently on one of the same slates of which the lady says: "We examined them when they were placed the second time on the table and satisfied ourselves that they were clean." Continuous observation of the slates after they were thus deposited the second time is not *alleged* nor is any interval of time stated. But assuming that one of the slates was then already inscribed,[2] everything depended on the observation of their condition at that critical moment. Now you can only ascertain that a slate is "clean" by successive examination of both its surfaces, the evidence of which must, in the reasonable intendment of the witness's language, exclude all possibility of deceptive manipulation by the conjurer while the surfaces seem to be displayed. Otherwise there is nothing to show that the witness appreciated the prime importance of this observation. And as it is perfectly possible for a conjurer under certain conditions, or if he is allowed his own way, to make it seem to a spectator that slates are clean when they are not, so it is perfectly possible for an honest witness in such case to use this form of expression: "We examined the slates and satisfied ourselves that they were clean." But with every approach to definiteness and particularity of statement, we approach the limit beyond which honest mis-statement is no longer possible. How these particular tricks were performed exactly, I do not profess to know.[3] But so far as we have

[1] Mrs. Sidgwick's own observations on these occasions are not given in detail in her paper. As the criticism of them I read at the meeting referred to an account she had sent me, and which I erroneously supposed to be part of her paper, that criticism is now omitted.

[2] As is very doubtful upon the evidence, even without having to suppose such a failure of observation as would permit the writing to be performed after the slates were deposited. For there is *no* evidence that the slates then deposited (the second time) were both the *same* slates afterwards ascertained to be the lady's ("Miss Z.'s"). The "message" may have been written on one of her slates at an earlier period of the sitting, when the slates were under the table, and when, as I learn from the account sent me by Mrs. Sidgwick, one of "Miss Z.'s" slates was for a time discarded, *no observation of it meanwhile being alleged.* In that case, the substitution of the inscribed slate ("Miss Z.'s") for one of those upon the table is easily supposable in the absence of any *averment* of continuous observation of them. It is just such defects of testimony *on the face of it*, in the case of conjuring, which illustrate and confirm my argument.

[3] I had only the first case before me when my paper was written. As to the second and third I will only point out that we are not told that the slates were

the evidence positively before us, it is rather useful as an illustration of what evidence ought not to be than of what it commonly is, or as affording any ground whatever for distrusting other evidence which on the face of it is free from defect.

In the course of her paper, Mrs. Sidgwick urged that the medium has an advantage over the avowed conjurer in being allowed to fail should the conditions be inconvenient. Now if the medium-conjurer could confidently foresee at the beginning of a sitting either that he would or could not get *all* the conditions required for success in the several successive operations he might have to perform, this privilege of failure would no doubt be very advantageous. But in many cases, especially in the slate-writing, the conjurer's conditions may break down at any point, and should strict conditions of observation be insisted upon at a late stage, no harmless failure, but exposure, must result. If, for instance, we suppose that "Miss Z.'s" slate was already

continuously under the hands of the whole party, or even that they seemed to be continuously observed at all. Before we are called upon to criticise evidence, it must at least present a *primâ facie* case for explanation. In the fourth case it was "Mr. A." who "slipped" the sheet of paper, on which the writing was found, into the locked slate, and this appears to have been done *after* "Mr. A." was told the page and line selected. I cannot agree with Mrs. Sidgwick that this case "is, perhaps, more surprising." (It will be understood that I do not attempt to exhaust the possible opportunities of the conjurer, with regard to evidence which seems to me so entirely lacking requisite exactitude and detail.) Passing to the account of the (other?) conjurer's performance in "Mr. X.'s" case, the simultaneous use of two slates apart from one another offers us a rather easy explanation without supposing such a *total* abstraction of attention for "two or three minutes" out of "some few minutes" (the duration of the whole experiment) as is suggested. We are told nothing of the position of the conjurer's hands (a point seldom omitted in the mediumistic reports), and it is not difficult to suppose that by successive feints he could first excite "Mr. X.'s" suspicions in relation to one slate, and then in relation to the other, thus getting him to fix attention on one at a time while the other was being written upon. The "whisking away" of the slate held by "Mr. X." was probably necessary on account of the writing having, under the conditions, to be executed on the upper surface and having to be made to appear on the reverse. A still easier supposition would be that the writing was indeed thus performed— probably a very few words—on the held slate with a much shorter diversion of attention to the other one, and that the latter—the locked slate—was a trick slate with message as described all prepared beforehand. A quite inexperienced observer with two separate objects to watch may easily be self-deceived as to continuous observation of both on one and the first occasion. But a *total* abstraction of attention from a *single* object, and that for two or three minutes out of some few minutes, and with perfect ignorance of the fact, the witness believing himself intent on observation all the time, could only be abnormal. But that is what we should have to suppose in a large proportion of the genuine slate-writing séances, nay, that the same thing could happen *repeatedly*, with experienced observers, and even with two or three such observers at the same time!

written upon when it was to be deposited on the table, where would "Mr. A." have been, if "Miss Z." or Mrs. Sidgwick had resolved to examine the slates in her own way, and not as "Mr. A." chose that she should seem to do so? The conjurer in such a case has really two tricks to perform for one success, and usually he will have parted with the privilege of failure as soon as he has performed the first. So that though now and then an ingenious professional or amateur may succeed in one way or in another, repeated observations, reflection, and public discussion would soon lay bare all his resources, and there would be an end of him. The professional conjurer has a large repertory of tricks, and is constantly inventing new ones with all the aid which mechanical appliances, confederates, and his own stage, can afford. He can drop a trick as soon as it is in danger of discovery, and vary his entertainments indefinitely. The public go for amusement, and do not study or hear of the discoveries made by critical experts, by which the conjurer is soon warned off dangerous ground. Nor are professional experts interested in exposing each other's performances, but in repeating them for their own benefit; whereas against the medium they are all, with a few exceptions, banded. The medium, on the other hand, is especially developed for a comparatively few phenomena, which recur with him for many years as the main feature and attraction of his mediumship. A certain proportion of his visitors are habitual students of the subject, whose attention is open to every explanation that is put forward, and who have the advantage of their own systematic observations with the same and similar mediums. They are constantly obliged to defend themselves from the charge of credulity and mal-observation; each time they go to a séance they have the keenest inducement to obviate some objection to their own or others' evidence, or to meet some more or less possible suggestion as to the *modus operandi*. They improve their methods of observation, they direct it to fresh points, they devise and obtain new tests. Psychography alone has now been before the public of this country for 10 years. Some of the most famous conjurers, and many acute minds have engaged in criticism of the facts and of the evidence, and yet it has survived the ordeal as no single trick, or variations of a single trick, of such a character and under such conditions as this slate-writing could possibly survive it.

To deal at length with general objections to the genuineness of these phenomena is not within the limits of my present subject. Yet I may be allowed to advert to two or three which have been lately brought before us by Mrs. Sidgwick. There is the detected trickery—real and reputed—of mediums. As Eduard von Hartmann has pointed out, occasional trickery is antecedently to be expected from the exigencies of professional mediumship, having regard to the uncertainty with which the true force is developed. And the whole theory of mediumship points to influences and conditions which must result sometimes in actual deception, and sometimes in the mere appearance of it. It is a mistake to suppose that we can make this branch of psychical research quite independent of psychology. And there are features in this trickery which should make us look a little deeper than the conjuring and fraud theory for its explanation. Slade, for instance, now often cheats with an almost infantile audacity and *naïveté*, while at the same or the next séance with the same investigators phenomena occur which the most consummate conjurer might well envy. Then it is made an objection that tests designed to dispense altogether with observation in the presence of the medium have not been obtained, although they could not be conceived to present greater physical difficulties to a genuine occult agency than things actually done. There is in this a quiet assumption that we have not here to do with independent wills and intelligences, or with laws other than physical, which is quite illegitimate at the outset of our researches. But without having recourse to such suggestions, I need only point out that if human observation under the easiest conditions is at all to be relied upon, the evidence can become perfect without these tests, and can only be illogically prejudiced by the absence of them. A third objection which weighs with many is the failure of mediums with some investigators who, of course, on that account are credited, if they do not credit themselves, with too much astuteness, and with too great powers of observation for the medium to venture on his tricks with them. It is a remarkable illustration of this theory that Mrs. Sidgwick, who tells us that personal experience has made her form a very low estimate of her own as well as of others' powers of continuous observation, and who failed to detect the opportunities of an amateur expert in slate-writing, although she knew that a trick was to be performed, is one of those with whom that accomplished conjurer, Mr. Eglinton, has been uniformly compelled to exercise his "privilege of failure." It is another commentary on this view that I myself, and others upon whom Mr. Eglinton has found it very easy to impose, have had with him as many failures as successes, under precisely the same apparent conditions in both cases. The causes of failure as of success are at present too obscure for such arguments to be other than prejudicial and opposed to the scientific character at which we aim. No doubt it is a disappointment—and perhaps no one has felt that more severely than myself—that some of the most distinguished members of this Society have failed to obtain evidence through Mr. Eglinton. But we must remember the idea with which we started, and which was so well expressed by Professor Sidgwick in his first address to us. It was never supposed that these phenomena had the scientific character of being reproducible with certainty for any and every one who took the trouble to sit for them a few times. We were to accumulate testimony, to overcome opposition by the gradual accession of witnesses of good intelligence and character. There was no necessity for that if we could say to all the world—go to this or that medium and we guarantee to you personal evidence. The physicist does not rely upon testimony or ask others to rely upon it. But we pre-suppose that the phenomena with which we deal are not accessible to all. If, then, they are not accessible to some of ourselves, is our position in relation to them altered? No; we are estopped from making that demand of personal experience, and from making that objection of personal failures—we are "hoist with our own petard"! Seeing that innumerable observations, by new witnesses of undoubted character and intelligence, have accumulated since Professor Sidgwick first addressed us four years ago, it will be asked, it has been asked, whether there was indeed a mental implication in his words, so that the new evidence which was to subdue the world must be that of himself and a few especial friends. I suppose that would be disclaimed, but is it disclaimed in favour of a criticism which discovers all other evidence to be bad? By further and further depreciating the powers of human observation, by more and more magnifying the resources of conjurers, it is nearly always possible to suggest a chink or cranny for escape in this case, and another and different chink or cranny in that case. But the very object of accumulating evidence is to make such suppositions increasingly violent the larger the area of experience which they have to cover, until the hypothesis of mal-observation becomes the last resort of those who will not or cannot credit testimony until their own senses have had cognisance of the facts. I believe that distrust of human observation, to the extent to which that distrust is now carried, is not justified by experience, which would be almost impossible for the simplest acts of attentive perception if it were justified. Surely there is a larger view, a deeper insight into this already long chapter, swelling to a prodigious volume, of human evidence, than is afforded by this miserable theory of conjuring, and cheating, and imbecility. Are we not shocked by its inadequacy, by its disproportion to the total effect? That effect is dwarfed in popular imagination for a time, because the dominant culture has refused to recognise it, and has encountered the facts with the very narrowest conceptions in the armoury of its intelligence. But the effect is already one of the appreciable influences on human life and thought. Many a delusion has perhaps been that, but not delusions of observation which depend for their vitality upon an ever springing supply of recurrent fraud. Again and again has phenomenal Spiritualism been "exposed" and "explained"; every such incident, every such attempt, has been a new instruction to investigators, a new difficulty to the supposed conjurer. Yet fresh observers, with full knowledge of all that has happened and of all that is suggested, go to mediums and come away with the certainty that the phenomena are genuine. Even the first of living German conjurers, Hermann of Berlin, who had considered the subject of this slate-writing very carefully, went to Slade, and after witnessing the phenomenon under very ordinary conditions, professed his present inability to explain it.[1] He adds, I am glad to say, that he is to have a series of sittings with Mr. Eglinton in a few months, the results of which will be published. Dr. Herschell, a well-known amateur, has recently written to Mr. Eglinton in the following terms:—

For some time after my first sitting with you, I candidly confess that I worked very hard, both by myself and in consultation with well-known public performers, to find out a method of imitating psychography, and I do not think that there is a way that I have not tried practically. I have come to the conclusion that it is possible to produce a few words on a slate if the minds of the audience can be diverted at the proper time (a thing perfectly impossible under the eyes of conjurers, who know every possible way of producing the result by trickery, without instant detection). Beyond this, conjuring cannot imitate psychography. It can do nothing with locked slates, and slates fastened together. It cannot write answers to questions which have not been seen by the performer, as you are constantly doing. At the best it only produces a mild parody of the very simplest phenomena under *an entire absence of all the conditions under which these habitually occur at your séances.*

Allow me also to take the present opportunity of thanking you most sincerely for the opportunities you have given me of satisfying myself of the genuineness of psychography by discussing openly with me, as you have done, the various possible ways of imitating the phenomena, and of letting me convince myself, in detail, that you did not avail yourself of them.

I hope that you have had a successful visit to Russia, and that your health is now quite re-established.—With kind regards, yours sincerely,

GEORGE HERSCHELL, M.D.

W. Eglinton, Esq.

Our English conjurer, John Nevil Maskelyne, has publicly testified from his own experience, to the existence of an unrecognised force productive of physical effects.[2] But with the acknowledgment of such a force in the human organism must disappear the presumption against those more developed manifestations which depend on its relations to intelligence and will. The ascertainment of those relations are among the highest

[1] See an article by Hermann in the June number of the German magazine *Sphinx*. (But see note, *ante*, p. 91.)

[2] See correspondence in *Pall Mall Gazette*, Mr. Maskelyne's letter, 23rd April, 1885.

functions of a society for psychical research, and I am not alone in believing that we should have found our scientific reward in beginning with a provisional faith in the material of our inquiries. In this region the laws and conditions are still almost wholly obscure, but of one thing in it we may be generally sure—that there can be no greater mistake than to investigate phenomena of psychical origin with a total disregard of psychical conditions. We are false to our hypothesis if we assume that adequate precaution against fraud is the *prime* condition of success, and that beyond this it is only necessary to bring an unprejudiced mind to the investigation. These are indeed *indispensable* conditions, but there may well be other and more positive ones not less indispensable. If we entertain the hypothesis of mediumship at all—and why else are we investigating?—it must mean for us something more than that in the mere presence of certain persons certain phenomena may occur. A medium is not like a bar magnet which can and must exhibit its special charactersistics under certain exclusively physical conditions. It is antecedently probable that something more is required of the investigator than the attributes of a fair-minded judge—a co-operation, namely, which will be best if it include some contribution of that unknown force on which the phenomena primarily depend, but which shall at any rate favour, and not repress, the development of that force in the medium. This sort of co-operation is a mental disposition perfectly consistent with the most scientific vigilance, and which, in my own case, I have found even promotive of it, because I was well resolved not to be conducive to my own deception.

It would be strange if in this Society we were to ignore the probable application of telepathy to the phenomena now in question. For telepathy in its principle must be far more than a mere emotional or ideal transfer upon special occasion. The inter-action of our psychical natures must be more intimate and influential than superficial consciousness betrays. I once heard it remarked, jestingly or seriously—I hardly know which—that the composition of an ideal circle for the investigation of these phenomena would be a man of physical science, a professional conjurer, a detective policeman, and an Old Bailey barrister. That suggestion represents the spirit which brings failure, and must bring failure, to every investigation of this character. And if you as a society wish for useful original research by your own agents, you must not choose your agents upon that principle. They must be persons thoroughly impressed with the great importance of exact observation and exact statement, but who combine with these pre-requisites some positive experience and some reasonable regard to the hypothesis on which you are investigating at all.

But original research is not necessary in the first instance. Many, of whom I am one, are of an opinion that the case for these phenomena generally, and for "autography" in particular, is already complete. And probably many of yourselves are of opinion that the time has arrived for your Literary Committee to deal with this question as it has already dealt with other heads of evidence. It might begin with the evidence of this "writing at a distance." But unless it is to arrive at a foregone negative conclusion, its judgment must not be guided by those who think that human observation, with the most express direction of the mind, is not to be trusted to ascertain the fact that a slate has been untouched for five minutes on a table before the eyes; or who are prepared, when they have before them exact statements of facts of observation, to assume that the facts have been mal-observed and misdescribed. For that way lies interminable doubt, and not progressive science.

NOTE ON MR. MASSEY'S PAPER.

In the paper that precedes this note Mr. Massey refers to certain remarks made by me at the first meeting of our Society, in a manner which suggests that he has misunderstood their drift. If Mr. Massey has misunderstood me, it is likely that others also may have done so: and since his comment on my present attitude is thrown in the form of a reported question that challenges an answer, it seems convenient that I should at once answer him by explaining the phrases that have been misunderstood. Mr. Massey begins his paper by quoting a sentence in which I described the sort of proof at which we ought to aim; he then gives several specimens of what he seems to regard as unexceptionable evidence for the genuineness of the physical phenomena of Spiritualism; then, on pp. 95-96, he refers to me (correctly) as urging the Society to accumulate testimony, to overcome opposition by the gradual accession of witnesses of good intelligence and character; and finally says, "Seeing that innumerable observations, by new witnesses of undoubted character and intelligence, have accumulated since Professor Sidgwick first addressed us four years ago, it will be asked whether there was a mental implication in his words, so that the new evidence which was to subdue the world must be that of himself and a few especial friends."

My answer is there was no such "mental implication"; but that the evidence which Mr. Massey affirms to have been accumulated, and of which his paper contains examples, is not the kind of evidence which I intended to urge the Society to accumulate. The evidence I had in view was evidence obtained in private circles of relatives or friends, where no professional medium was employed. That this was before my mind is apparent from several passages of my address:—*e.g.*, from the sentence preceding the one first quoted from me by Mr. Massey; in which I say that "it is due to the private families or private circles of friends whom we hope to persuade to allow us to take part in their experiments" that we should bring our evidence to the highest possible pitch of cogency.

So far as I know, there has been no important accumulation, during the last four years, of the kind of evidence which I had in view: the testimony of which Mr. Massey has spoken is testimony to marvels occurring in the presence of persons who exhibit them professionally for money. Now when I addressed the Society at its first meeting I intended to make it plain that we ought, in my opinion, to avoid paid mediums "as much as possible"; I did not indeed think that it would be wise to preclude ourselves by a hard and fast rule from employing the services of such persons: but I certainly hoped that we should be able to confine our investigation to phenomena "where at any rate"—as I said—"no pecuniary motives to fraud can come in." It is, in my opinion, upon evidence of this latter kind that the *primâ facie* case for investigating the physical phenomena of Spiritualism mainly depends. Certainly, if we had nothing but testimonies to marvels occurring in the presence of persons who charge a guinea a séance for exhibiting them, I for one should never have thought it worth while to consider seriously whether such reported marvels were due to anything more than skilful trickery on the one side and defective observation and memory on the other. The testimony that excited my interest in the subject was mainly testimony to phenomena occurring in private circles composed of persons who were very unlikely to have plotted to deceive each other or the public, or very unlikely to possess a high degree of conjuring skill. There exists already some noteworthy evidence of this kind—enough, in my opinion, to justify further inquiry, though not enough to constitute an adequate scientific basis for the momentous conclusion to which it points. I hoped that the operations of our Society might be directed towards improving the quality and increasing the quantity of this kind of testimony; and it was this hope that I intended to express in the address to which Mr. Massey has referred.

But this is not all. The cases which Mr. Massey has brought forward do not merely exemplify a kind of experiment different from that to which I announced that our Society's attention would *in the main* be directed; they exemplify a kind of experiment which I hoped that we should avoid altogether. The three persons through whose mediumship Mr. Massey's marvels are supposed to have been produced are not merely persons who make a trade of exhibiting phenomena: they are persons to whom imposture has been brought home by irresistible positive evidence. We learn from the *Spiritualist* (November 3rd, 1876) that when Monck was charged at Huddersfield in 1876 with imposture under the Vagrancy Act, it appeared that conjuring apparatus had been found in his room; and Mr. Henry Lodge and another well-known resident in Huddersfield deposed on oath that Monck had confessed to them that he practised deception on sitters. In the case of Slade, Mr. Massey himself admits that he "now often cheats," though he pleads that this cheating—when discovered—shows an "almost infantine audacity and naïveté": for my own part, I cannot doubt that Slade attempted to cheat me in 1876, in a manner which, though "audacious" was not exactly "naïve." As regards Eglinton —if Mr. Massey has read the statements of Archdeacon Colley in the *Medium and Daybreak* (November 1st and November 15th, 1878), and the reports in the *Spiritualist* (February 14th and March 21st, 1879), of statements by Mr. Owen Harries, he will scarcely doubt that Eglinton was, some 10 years ago, engaged in the manufacture of spurious "materialisations" with the aid of a false beard and muslin: and I think it clear that in 1882 Eglinton co-operated with Madame Blavatsky in the production of a spurious Theosophic marvel.

If it had occurred to me, when I addressed the Society four years ago, that we should be seriously urged to investigate the performances of "mediums" whose trickery was proved and admitted, I should certainly have repudiated the suggestion with all the emphasis that I could command. But I then believed—and ventured to say—that Spiritualists had been impressed by the "evidence accumulated in recent years to show that at least a great part of the extraordinary phenomena referred to spiritual agency by Spiritualists in England and America are really due to trickery and fraud of some kind." I hoped, therefore, that educated Spiritualists would generally agree with me in condemning what I called "the obstinacy with which mediums against whom fraud has been proved have been afterwards defended," and in regretting that such persons should, as I said, "have been able to go on with their trade after exposure no less than before." I never thought that we should be called upon to give direct encouragement to this trade by undertaking a formal investigation of the "phenomena" exhibited by such persons.

H. Sidgwick.

FURTHER DISCUSSION BETWEEN MR. MASSEY AND PROFESSOR SIDGWICK.

Since I have misunderstood Professor Sidgwick as to the *exclusive* character of the evidence he proposed we should accumulate, I can only urge, after careful re-perusal of his first address to the Society, that I had some excuse. For, in the first place, the suggestion in that address is not that we should "confine" our investigations to phenomena occurring with private mediums, but that we should "as much as possible direct" investigation thereto. Secondly, Professor Sidgwick had said in the same address :—"I do not presume to suppose that I could produce evidence better in quality than much that has been laid before the world by writers of indubitable scientific repute—men like Mr. Crookes, Mr. Wallace and the late Professor de Morgan," and he went on to urge that evidence of this superior quality should be accumulated. Now it is notorious that the authorities named appealed largely and chiefly to evidence they had obtained through mediums who, at one time or another, were professionals, and against some of whom, moreover, acts of imposture have been alleged on apparently strong grounds. Then, again, when Professor Sidgwick said :—"But we can no longer be told off-hand that all the marvels recorded by Mr. Crookes, Professor Zöllner, and others, are easy conjuring tricks, because we have the incontrovertible evidence of conjurers to the contrary," I was surely entitled to infer that evidence thus referred to—Professor Zöllner's being exclusively with Slade—was part of the *primâ facie* case of the Society. There is nothing in the address at all suggestive, even, of the proposition that evidence with professional mediums cannot be raised to a point at which suppositions of "skilful trickery on the one side, and defective observation and memory on the other" would bring the investigator's intellectual condition within the description of "absolute idiocy."

It is also allowable, I think, to refer to the facts that Professor Sidgwick himself, and several other active members of the Society, have, since the date of that address, made repeated attempts to obtain personal evidence of the phenomenon of 'Psychography" with Mr. Eglinton, and that several conjurers have been employed by or on behalf of some of these gentlemen to investigate with the same medium. I am quite unable to understand on what ground a conjurer could be employed, if not the supposition that he might encounter conjuring. It is also to be observed that "conjuring" and "cheating" are not convertible terms. It is rather a strange inference that because a man has been detected in trickery he is therefore a consummate conjurer. And the known trickery of mediums is of such a character as to raise no presumption whatever that they are conjurers. The trickery has been most frequent in so-called materialisations, when it was facilitated by the worst conditions of observation, and by the absence of precautions against the introduction of disguises, &c. And with all respect for Professor Sidgwick, I should say that if he detected Slade in attempts to cheat him in the slate-writing, the conjuring could scarcely have been of a high order, or such as (in his own words), "conjurers cannot find out." The fact probably is that conjuring, like other arts, is rarely self-taught from the first, but requires instruction by trained experts. Now the early antecedents of most of the better known mediums have been ascertained, and not only is there no trace of any connection with conjurers, but usually their mediumship for the simpler—but not therefore easily simulated—phenomena has been observed in their childhood or very early youth, before they could be credited with ability to carry out habitual deceptions, and before the pecuniary motive could present itself. I may add that though Professor Sidgwick now rests his objection to professional mediums chiefly on a presumption of their conjuring capabilities, I find nothing of that in his first address, the preference for private mediums being there put merely upon the absence of ordinary—or at any rate pecuniary—motives to fraud. I have always thought this a weak point in his position, if our aim is to obtain exact proof. There would be, I think, more force in his present objection, if (1) the presumption of conjuring ability were legitimate, which I believe it is not, and (2) if, admitting that presumption, it can in no case be repelled by observations, or by precautions combined with observation. My paper was an attempt to deal with this second question, and will no doubt be appreciated at whatever worth the argument may possess, in connection with Professor Sidgwick's statement of his own position.

C. C. Massey.

In pointing out Mr. Massey's misrepresentation of the drift of my remarks, I said nothing to imply that it was an inexcusable misrepresentation. I had no wish to raise this personal question; but, as Mr. Massey has raised it, I may perhaps make my position—which he still misunderstands—clearer by answering it. I think, then, that Mr. Massey was not justified in representing me as having *urged the accumulation* of the kind of evidence with which his paper deals—the records of the "phenomena" exhibited by paid mediums admitted to be tricksters—in the face of my distinct statement of opinion that we ought to work with private mediums "as much as possible," and my expression of surprise at the encouragement given by Spiritualists to detected impostors. But I quite admit that it was excusable in him to suppose that evidence of this kind might have more weight with me than is in fact the case: for in the address which he quoted, while I tried to trace clearly the lines of investigation which our Society ought—in my opinion—to adopt, I intentionally left obscure my estimate of the value of the evidence that had already been collected. My reason for this reserve will be readily understood. I was speaking as president of a society newly formed by the combination of two heterogeneous elements—persons convinced of the genuineness of the alleged effects of spiritual or occult agency, and persons, like myself, who merely thought the evidence for their genuineness strong enough to justify serious inquiry. In this situation, I thought it my duty to lay stress on the points on which—as I hoped—the audience I was addressing might agree, leaving in the background the points on which I knew that we differed. I hoped we might agree on the manner in which evidence was to be collected in future; I knew that we differed on the value of the evidence that had been collected in the past. Hence I expressly disclaimed any intention of discussing the weight to be attached to this evidence; in speaking of the past I merely said on behalf of my new allies what might in my opinion be said with truth. They had been stigmatised as dupes of coarse and bungling tricksters; it seemed to me only fair to point out that some of the tricks had, at any rate, baffled experts in conjuring. Taken alone, indeed, this fact would have seemed to me of little importance. I have no great difficulty in supposing that certain unscrupulous persons, skilful enough in certain peculiar kinds of trickery to baffle the insight of conjurers, find the best market for their skill in exhibiting their tricks, at a guinea a séance, to Spiritualists and investigators: at any rate, this suggestion is not so improbable as to render it necessary to resort to the hypothesis of spiritual agency or occult forces in order to avoid it. But, taken in connection with the testimonies to private mediumship, these inexplicable phenomena of professional mediums seemed to me worth noting.

Mr. Massey further quotes a sentence in which I disclaim the presumption of supposing that I could produce evidence better in quality than much of that produced by men like Mr. Crookes, Mr. Wallace, and De Morgan; and infers that as these gentlemen largely experimented with professional mediums, some of whom lie under grave suspicions of imposture, *therefore* I must have intended to encourage investigation with paid mediums and detected impostors, in spite of my explicit statements to the contrary. This inference seems to me strained and unreasonable. In uttering the disclaimer in question I was not thinking at all of the character of the mediums employed—that was a point I intended to discuss afterwards—but merely of the scientific position of the investigators and the impressiveness of their accounts. The phrase was, indeed, stronger than any I should now use, after four years' additional experience. Still, if I had now an opportunity of repeating, with a private medium of unblemished character, some of Mr. Crookes' "further experiments on pyschic force" (see his *Phenomena of Spiritualism*, pp. 36, 37), or De Morgan's most striking experiment with Mrs. Hayden (see p. xliii. of the preface to *From Matter to Spirit*), I would spare no pains to avail myself of it; and if I could obtain similar results with sufficient repetition and variation of conditions, I should regard them as evidentially important. But I should certainly not put them forward as evidence if I knew the supposed medium to be a detected impostor. Nor should I seek evidence from such tainted sources;—not because I hold that evidence involving tricksters *cannot* be raised to a pitch that would exclude explanation by trickery, except on the supposition of the investigator's idiocy; but because an extended experience has led me to regard the chance of its being so raised as too slight to counterbalance the palpable evil of encouraging an immoral trade. Suppose that such descriptions as Mr. Massey and others have given of Eglinton's slate-writing had been given of the performances of an avowed conjurer: surely no one would have suggested that we were forced to the supposition of idiocy or mendacity or hallucination on the part of the observers: and if not, the supposition cannot be any more necessary in the case of Eglinton.

Mr. Massey holds that my preference of private mediums to admitted impostors is a "weak point in my position" if "our aim is to obtain exact proof." It is clear from this that he mistakes my position. He regards unblemished character and stringency of tests as alternatives: I regard them as conditions which we should aim at combining. But, as I have often said, I do not expect to obtain cogent proof of an unknown law of nature by a single experiment: I do not hope to get it by anything less than a large accumulation of experiments of the best attainable quality.

Mr. Massey further suggests that I have changed my ground in now resting my objection to paid mediums partly on a presumption of their conjuring capacities. He will find, however, that I have drawn attention to this characteristic, as belonging to professional but not to private mediums, in an address which I delivered a year later. (See *Proceedings*, Vol. I., p. 249.) The reason that I did not mention this, as well as the pecuniary motive to fraud, in prescribing the lines of investigation in my first address, was merely that it seemed less easy to eliminate with certainty. We can be sure that we have not paid a given person, but we cannot be sure that he has not long practised trickery, though in some cases we can show it to be highly improbable that he has practised it sufficiently to become an expert trickster.

By the way, I entirely agree with Mr. Massey that cheating—even successful cheating—and professional conjuring are quite different things. I do not suppose that Slade and Eglinton could succeed as rivals of Maskelyne or Verbeck. But I have no reason—nor has Mr. Massey offered any—for regarding their powers of slate-writing as altogether self-taught; nor do I think it marvellous that, even without any training by avowed conjurers, they should have acquired a high degree of skill in this special line during their many years of practice.

Mr. Massey seems to think it inexplicable, supposing Slade to be a *mere* trickster, that I should have seen through him on one occasion (in 1876), whereas some of his performances have baffled professional conjurers. I cannot think that the art of finding out unknown tricks is so entirely technical as this inference assumes; nor does it seem to me improbable that Slade should be sometimes careless with persons who appear easy to take in, or sometimes clumsy in adapting himself to the supposed tastes of his customers. In my case, as I conceive, he hoped to impress an academic mind by presenting unasked a slate inscribed with five sentences in different modern languages, obviously taken out of a conversation-book, and one phrase out of the Greek Testament. I did not exactly see the trick done; but I saw when substitution might have taken place; and, considering the performance in the light of later exposures, I cannot doubt that it was a prepared trick.

Finally, Mr. Massey is surprised that, my views being what they are, I should have attempted to obtain personal experience of Eglinton's "phenomena," with the assistance of experts in conjuring. I certainly should not have done this, had I known what I now know of Eglinton's antecedents; nor, I think, even without this knowledge, if it had not been for the situation in which I was at the time placed, as President of the Society. In accordance with my wishes—expressed in the address above referred to—our "Physical Phenomena Committee" avoided the employment of paid mediums; but their efforts to obtain evidence elsewhere led to no satisfactory result, and murmurs began to be heard from Spiritualists among us that we were neglecting an unequalled opportunity of obtaining conclusive phenomena through the mediumship of Eglinton. I was anxious that our committee should adhere to their rule, so far as their official investigation went, and that none of the Society's funds should go in paying guineas to a professional slate-writer; but I thought it better to make some concession to the murmurers, and I preferred to make it by arranging privately for a series of experiments with Eglinton. Having come to this resolution, it seemed clearly desirable to seek the co-operation of a conjurer. The scientific object of any such investigation must be to exclude possible known causes of the apparently inexplicable phenomena. In the case of slate-writing, the most obvious of such causes was trickery, at any rate somewhat similar to a conjurer's: I therefore thought it important to get the aid of an expert in conjuring as a means of bringing our experiments up to the highest attainable pitch of conclusiveness, whether the result was positive or negative. And I thought that we were fortunate in obtaining the assistance of an accomplished amateur—Mr. Angelo J. Lewis—who was prepared to enter on the investigation with a perfectly open mind. That he obtained no satisfactory result does not surprise me, knowing what I now know of Eglinton.

H. SIDGWICK.

I am sorry to be obliged to take up some further space in consequence of Professor Sidgwick's latest remarks. Here is the passage, in his first Address, which *immediately* follows the already quoted reference to the evidence of Crookes, Wallace, and de Morgan, and on which I chiefly base the representation considered by Professor Sidgwick to be without justification (italics are mine) :—" But it is clear that from what I have already defined as the aim of the Society, *however good some of the evidence may be in quality, we require a great deal more of it.*" If the recommendation, that we should as much as possible direct our investigation to phenomena with private mediums, is to be read as a "distinct statement of opinion" that we ought to avoid paid mediums as much as possible, I can only remark that that either is or is not consistent with what Professor Sidgwick said elsewhere in the same Address. In my view, it is consistent, because we may well prefer investigation with private mediums, and may yet attach high importance to the accumulation of the best evidence with professional mediums—such evidence as that of Zöllner, &c.—whether obtained within or without the Society. It is to be observed that I was not speaking of *original* research by the Society, and that I said nothing to imply that Professor Sidgwick had encouraged this or that sort of direct investigation by the Society. We get our facts—our evidence—from alien sources at least as much as from our own experience. Estimating more highly than I do the difficulty of avoiding imposture with paid mediums, Professor Sidgwick might well deprecate the regular employment of them, as bad economy of time and resources, and might nevertheless recognise the importance of accumulating testimony equal to that of which he said we want "a great deal more of it." Professor Sidgwick has therefore not quite correctly stated the inference I drew from his words, and it is hardly necessary for me to insist on such passages as "it is highly desirable that the investigation of these matters should be carried on by men who have tried to acquaint themselves with the performances of conjurers," though I fail to see the high desirability of this if investigation is to be restricted as Professor Sidgwick thinks it should be. And further, as he even now admits that evidence with those he calls tricksters *may* be raised to a pitch that would exclude explanation by trickery, it is obvious that my worst mistake lay in supposing that he would feel obliged to acknowledge that such evidence had been accumulated since he addressed us in 1882, and not in the supposition that he had recognised the possibility of this happening with paid mediums who had been under suspicion. I thought we had reached the pitch of evidence at which the question of the sort of medium would be admittedly as indifferent to Professor Sidgwick as it is to me. He thinks otherwise. But I am still unable to see my "misrepresentation."

I need not now dwell on what seems to me the *petitio principii* involved in the supposition of such descriptions as I and others have given of Eglinton's slate-writing being given of the performances of an avowed conjurer, because much of my paper was an attempt to show that such a supposition is an impossible one. No mere conjurer has ever yet submitted, and none ever will submit, to some conditions under which the slate-writing has been repeatedly observed with Eglinton, or will ever undertake to produce the *appearance* of such conditions, so as to induce a witness to give such an account as I consider really good evidence.

Professor Sidgwick seems to have misunderstood the bearing of my observation with regard to his supposed detection of Slade (which it seems was no detection at all). It is not in the least "inexplicable" to me that a good conjurer should occasionally be careless, and should thus be detected by one who is not an expert. But my argument was that such trickery, so detected, certainly raises no presumption of consummate conjuring capabilities. I do not say it *excludes* the hypothesis of the latter, though I think it is decidedly unfavourable to it. All I say is that you must have other grounds to go upon, and that even if you think you have such other grounds, the detected cheating is rather in your way than otherwise. It is a fact *prima facie* so far at variance with great conjuring capabilities that it would have to be exlained in some such way as that in which Professor Sidgwick explains it.

And this brings me to Professor's Sidgwick's references to my admissions of trickery by mediums. Now, in the first place, I believe that a very great deal of what seems to be trickery is only apparently such. And I hold that the appearance of it is not only explainable, but is actually necessitated by the hypothesis of mediumship. To make good this remark would require a distinct paper. But even where the physical agency of the medium is undeniable, I cannot, upon grounds well understood by Spiritualists, always, or even commonly, infer that the agency is voluntary. That there is a residue of conscious, intentional fraud I am, of course, aware. But as regards Mr. Eglinton in particular, I must in justice say that I have made no admissions, and that I do not believe he has *ever* tricked—consciously or unconsciously—in the slate-writing, though I am quite prepared to hear that with him, as with other mediums, deficient power has had its usual accompaniment of "suspicious" results. But as my opinion of his "mediumship" is quite independent of any estimate of his character, I am exempt from the obligation to form a decided judgment on certain of his alleged antecedents—a judgment I should find more difficult than Professor Sidgwick has found it.

C. C. MASSEY.

Should any reader still feel an unexhausted interest in the question whether Mr. Massey's misrepresentation of my advice was justifiable, I must ask him to read the address itself—which he will find at the beginning of the first volume of our *Proceedings*—along with the polemical reference to it in Mr. Massey's paper. He will, I think, easily convince himself (1) that the "we" who were urged to "accumulate fact on fact" were precisely and palpably the same "we" who had previously been advised to "direct investigation, as much as possible, to phenomena where no pecuniary motives to fraud can come in;" (2) that my aversion to encouraging the trade of detected impostors was expressed quite unmistakably; and (3) that the complimentary phrase in which I referred to the investigations of our predecessors could not reasonably be understood to qualify my subsequent distinct recommendations. But I can hardly imagine that any reader will take this trouble; it is now very unimportant—even to myself—whether I expressed my opinion as clearly as I intended four years ago. What I chiefly desire is to prevent any further misapprehension of my views. I have long held that the great scandal of modern Spiritualism is the encouragement it has always given to the nefarious trade of professional impostors. I feared that the formation of the Society for Psychical Research would almost inevitably have some effect of this undesirable kind; and I determined, at any rate, to do all I could to reduce the extent of the evil. I did not propose a rigid rule of avoiding "paid" mediums or "subjects"; partly thinking that some pecuniary compensation for loss of time might be found necessary, in the case of prolonged investigation with any persons of limited leisure. But I certainly hoped that we might avoid altogether the kind of evidence on which Mr. Massey's paper entirely relies—the reports of the "phenomena" of persons like Monck, Slade, and Eglinton. In the case of Monck, Mr. Massey tacitly admits that imposture has been proved; that Slade "often cheats" he has expressly stated; as regards the evidence against Eglinton he declares that he would have some difficulty in forming a decided judgment. This point, then, I must leave for the reader to decide, after studying the evidence to which I have directed his attention;[1] if he should still think it right to spend his guineas on Eglinton, he will at any rate—I hope—not suggest that he is acting in accordance with my recommendation.

For my own part, I have come to the conclusion—not by *à priori* reasoning, but from much personal experience and examination of the experience of others—that it is only under very exceptional circumstances that the serious student of Spiritualism should investigate the

[1] Members and Associates of the Society for Psychical Research will find the evidence given in the *Journal* for June, 1886, pp. 282-287.

"phenomena" of a professional and paid medium. That any one who is induced by narratives of marvel to enter on this line of investigation should first take all the pains in his power to acquaint himself with the possibilities of producing by trickery the appearance of such marvels—this proposition, I conceive, will be generally admitted. But Mr. Massey at least seems to think that no trouble of this sort need be taken by the investigator who confines his attention to private mediums. This is not my view. On the contrary, I hold that in the case of private, no less than professional mediums, it is very important that the investigator should be, if possible, competent to judge how far the results that he describes—or rather his impressions of them—could be produced by trickery; at least, if his evidence is to afford any effective corroboration of the medium's own assertions. I am far from aying that the study of conjuring will always enable him to judge correctly on this point, nor do I even think that it would be in all cases the best method of training his judgment, but I think that it is likely to be useful in most cases; it would at least tend to prevent his testimony from being vitiated—as much Spiritualistic evidence now is—by expressions of confident reliance on the most palpably inadequate tests.

H. SIDGWICK.

EXPERIMENTS IN MUSCLE-READING AND THOUGHT-TRANSFERENCE.[1]

BY MAX DESSOIR.

There appeared recently in Leipzig a work by the well-known Professor of Physiology, Dr. W. Preyer, entitled "The Explanation of Thought-Reading." In this book the author gives a detailed explanation of muscle-reading, as exhibited in the late performances of Messrs. Bishop and Cumberland. but denies the possibility of any other kind of thought-transference. It may be not out of place, then, to describe some experiments which I made in the summer of 1885.

I began my investigations by seeking to determine the range of muscle-reading, and I found that—apart from all other modes of contact—a gentle touching of the shoulder sufficed for definite guidance. In what follows, the person willing and thinking is spoken of as the "agent," and the person searching, or receiving the "transference," is spoken of as the "percipient."

1.—SITTING ON JUNE 15TH, 1885.

Agent:—Ewald Weiss, Stud. Mus., Berlin, S. W. Wilhelmstrasse, 28.

Percipient:—Max Dessoir.

Herr Weiss thought of this—that the percipient was to go through several rooms to a bronze figure, take it down from a cupboard, stroke it, and then put it down. He was then to go further, and sit down on a particular chair. Complete success.

It is clear how the result was attained. The percipient has his eyes bandaged, and his attention concentrated upon himself. By unconscious muscular guidance he is led to the bronze figure.

The question now arises, how can there possibly be a guidance *upwards?*

As regards this point, I have had the following instructive experiences: First, if the percipient wants to move away from the spot, the agent always guides him back, so that he notes: "There is something more to be done here." Secondly, the pressure on his shoulders diminishes, since the hands of the agent involuntarily rise a little, in consequence of his thoughts being fixed on the higher position. The percipient concludes with certainty from these signs that his activity is to be concentrated in an upward direction. The stroking of the figure, which at first sight seems remarkable, is explained by the fact that every agent has, as it were, a code of confirmatory muscular movements expressive of satisfaction. When I let my hands slip down along the figure—entirely by accident—I was clearly sensible of this approving pressure; this induced me to repeat the movement until a cessation of the pressure indicated to me that this part of my task was accomplished. I was then guided by the unwitting

[1] The original, of which the following is a translation, was sent to us at the close of 1885.

agent to the chair which had been chosen, and a strong downward pressure impelled me to the natural movement of sitting down.

2.—Sitting on June 25th, 1885.
Agent:—Ewald Weiss.
Percipient:—Max Dessoir.

The percipient was to fetch a walking-stick out of the corridor, carry it to the window, and lay it there in the window-groove (*Fensterrinne*).

Complete success. Nevertheless (my notes continue), when the percipient came to the window he wanted first to place the stick in the corner, then he hung it to the window-sill (*Fensterbrett*), and afterwards twice moved it about over the sill. Then, finally, he laid it down correctly.

The first part of the experiment was obviously successful on the principles with which we are already familiar; but the hesitation in the second part deserves further consideration. The chief condition of course is that the percipient shall above everything be as far as possible "without thought" (*gedankenlos*), in order to submit completely to the guidance; but if he is compelled to take a line of his own, he will try whatever it is easiest and most natural to do under the circumstances. Acting on this canon of experience, I first placed the stick in the corner; but as I was about to move away, the pressure on my shoulders prevented me, and I knew that I had made a mistake. I then tried until I discovered the right thing, and could then describe this trial also as successful. After these indications, the one additional experiment which I select for attention, out of many others, is easily comprehensible; it shows, however, an interesting variation. I quote it from the notes.

3.—Sitting on June 10th, 1885.
Agent:—Heinrich Biltz, Student of Chemistry, 14, Schellingstrasse.
Percipient:—Max Dessoir.

A match-box had to be found, a match struck, and a candle in another room to be lit with it.

The percipient found the match-box, opened it, took a match from it, and seized the right candlestick. But then it occurred to him—as he immediately said himself, before he knew what he ought to have done—that there was no candle in the candlestick, and that hence it was useless to strike the match. He therefore left it undone.

This case shows clearly how detrimental it is for the percipient to depend upon deliberate reflection (*regelrechte Ueberlegungen*) instead of following his instinct. A single trial would have sufficed to show whether the match ought to be kindled or not.

In the experiments which now follow, any unconscious muscular movement, such as I have described in the preceding cases, is altogether excluded. They were so arranged that agent and percipient sat at one table at a distance from each other of between half a metre and three metres. There was either no contact at all, or *in a few cases* the agent placed his hands gently upon those of the percipient. Under these conditions, experiments were made in guessing numbers. The percipient did not, of course, write the number down, but spoke it. When the percipient wished to speak, the contact sometimes made was discontinued, lest the pressure of the hand should afford any clue.

4.—Sitting on June 1st, 1885.

Agent :—Heinrich Biltz.

Percipient :—Max Dessoir.

THOUGHT.					GUESSED.
2	...	...	...	...	4
2	...	...	...	...	7
10	...	...	...	...	53
20	...	...	...	...	75
35	...	...	...	...	35, 85
19	...	...	...	...	18
78	...	...	...	...	11

The percipient was [previously] informed whether the number consisted of one digit or of more.

5.—Sitting on June 25th, 1885.

Agent :—Ewald Weiss.

Percipient :—Max Dessoir.

THOUGHT.			GUESSED.
8	...	...	1, 8.
33	...	...	Percipient continually sees a "3" in all possible shapes, but cannot discover the second figure, which in truth was also "3."
6	...	...	6.
10	...	...	Nothing.
11	...	...	44.
3	...	...	"I see a 7, but it oscillates above." Pause. Then—"3."

Insignificant as these results may be, I think that some conclusions may still be drawn from them. Whereas at the first sitting of this sort the only success was a single half-correct result out of the whole seven trials; 24 days later, out of six trials one was right at the first attempt, two at the second attempt; another was half right; and only two were failures. Even of these two, the case 11—44 should not count as an absolute failure, owing to the great similarity in the appearance of the two numbers.

Thus there is no doubt as to an increase in the capacity of the percipient, and I am convinced that nothing more than further practice was necessary in order to get splendid results.[1] In this, as in other matters, practice makes perfect. In the course of this paper, we shall encounter yet further proof that the susceptibility to affection from the thoughts of others can be developed by practice. The best proof is, in the first place, that the results were always the poorest at the beginning of any special class of experiments, and, in the second place, that the classes of experiments only more recently practised had comparatively the fewest results to show.

Experiments in the discovery of objects thought of, where there was no contact with the agents, show no better results than the ordinary proba-

[1] This opinion seems far too confident; and it is very doubtful whether Herr Dessoir's theory as to the effect of practice heightening the percipient's susceptibility is at all generally borne out.—Ed.

bilities would warrant. But as I have conducted only 11 experiments of this kind, the question of practice and improvement does not enter; and it has, moreover, to be remembered that the effort to perceive is apt to bring the would-be percipient out of the completely passive state in which he ought to be. At any rate, I have seldom succeeded in perceiving mentally an object (pencil, pen, &c.) thought of; and the number of trials (8) has been too small for any safe conclusion. Unfortunately, my time did not enable me to go thoroughly then into every branch of the experiments; and I thought it better to make myself familiar with some portions, instead of going on with all of them and getting only small results.

I proceed to give my few observations on the transference of *words* thought of.

6.—Sitting on June 16th, 1885.

Agent:—Ewald Weiss.

Percipient:—Max Dessoir.

In the first two cases, the percipient was told that the words were names of towns; in the others only that they were nouns.

THOUGHT.	GUESSED.
Rome	Hamburg.
Como	The first is round—towards the left; the second like an *a*; then something which I can't distinguish; then an *a* or *o*.
Antwort	A ———.
Lesen	*Ehre.*
Ja	The word has only two letters. The first is a K or J, the second like a small *d*.

Here also, I think, a progress is unmistakable,[1] although not one of the cases can be described as entirely successful.

Similarly in the following experiments, which I will not consider in any further detail, there can be no question of a satisfactory result;[2] it is, indeed, only the beginning of a series, which I was unable to continue.

The *modus operandi* was as follows:—The experimenters sat two metres apart; the agent imagined the particular card plainly on the ground; the percipient had, *as also in every other case*, his eyes bandaged with a thin silk handkerchief.

7.—Sitting on May 24th, 1885, and on the following Days.

Agent:—Heinrich Biltz.

Percipient:—Max Dessoir.

THOUGHT.	GUESSED.
1. Knave of Spades	1. Queen of Spades.
2. Ten of Spades	2. Nine of Diamonds.
3. King of Hearts	3. King of Clubs.

[1] The trials are too few to justify any such conclusion, even had the success been more appreciable than it was.—Ed.

[2] It is not clear what is meant. The series shows an amount of success beyond what chance would be likely to produce. But it is all too short; and moreover the cards were not (as they always should be) selected at random from a pack, but were apparently fixed on at will by the agent, who almost confined himself to aces and court cards.—Ed.

THOUGHT.		GUESSED.
4. Knave of Diamonds	... ...	4. Queen of Diamonds.
5. Eight of Diamonds	... ...	5. Eight of Spades.
6. Queen of Diamonds	... ...	6. Knave of Spades.
7. King of Spades ...	... ...	7. King of Hearts.
8. Knave of Diamonds	... ...	8. Ten of Clubs.
9. Ten of Spades ...	... ...	9. Ace of Diamonds.
10. Ace of Hearts ...	... ...	10. Ace of Hearts.
11. Queen of Hearts ...	... ...	11. Queen of Hearts.
12. King of Clubs ...	... ...	12. King or Queen of Clubs.
13. Ace of Spades ...	... ...	13. Ace of Hearts, Ace of Spades.

Without wishing to draw any conclusions from these trials, I pass on at once to that branch of the inquiry to which I have given the most attention, the reproduction of *diagrams*.

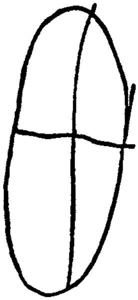
The *modus operandi* was as described above. Herren Weiss and Biltz acted alternately as agents, except in No. 7, where Herr Wilhelm Sachse [of 10, Kirchbachstrasse, Berlin, S.W.] was the agent. Twenty-one trials[1] were made in all, the following account of which I for the most part quote from the notes. I have copied the diagrams[2] as accurately as possible from originals which I have preserved, and give them in the order of the experiments. I may remark that the success was affected by the mood of the agent and of the percipient at the time. The sittings were held between June 4th and June 20th, 1885 ; as the experiments, therefore, are spread over *very many* days, it is more difficult than in the previous observations to estimate any development.

[1] The 21 include all the cases where Herr Dessoir himself was either agent or percipient, but do not include three trials in which Herr Biltz tried to act as percipient, and which were failures. These must be set against three successes of Herr Biltz (Nos. iii., vi., and x.) in the series given. In that series, two attempts of Herr Dessoir as percipient—one of them (to the eye) a success, and one a failure—are omitted, owing to some uncertainty as to the conditions.—ED.

[2] The diagrams on pp. 116-123 are not taken from Herr Dessoir's copies, but from the originals themselves, which Herr Dessoir forwarded for the purpose at our request.—ED.

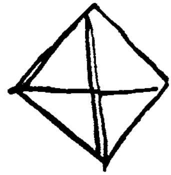
I.

ORIGINAL.



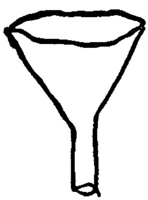
Agent: H. B.

REPRODUCTION.



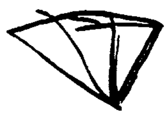
II.

ORIG.



Agent: H. B.

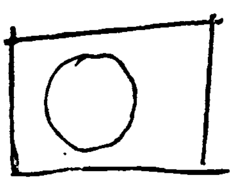
REP. 1.



REP. 2.

III.

ORIG.



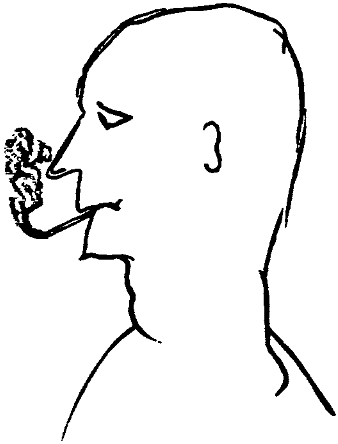
Agent's name omitted.

REP.

It appears here that the agent's image included an impression of the left part of the frame. M. D.

IV.

ORIG.

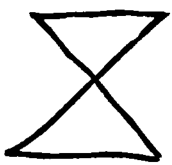


REP.

Agent: H. B.

V

ORIG.



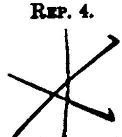
Agent: H. B.

REP. 1.

REP. 2.

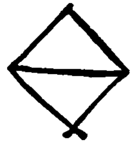
REP. 3.

REP. 4.



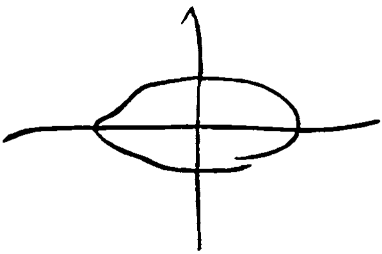
VI.

ORIG.

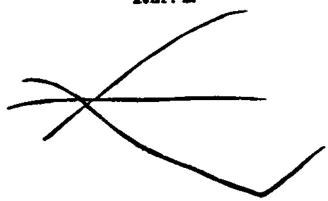


Agent: M. D.

REP. 1.

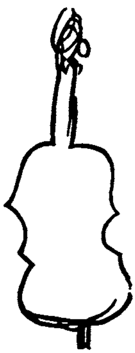


REP. 2.



VII.

ORIG.



Agent: W. S.

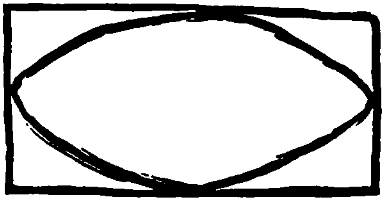
REP. 1.

REP. 2.

While the second reproduction was proceeding, an interruption occurred which prevented its completion.

VIII.

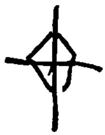
ORIG.



Agent: H. B.

REP. 1.

REP. 2.

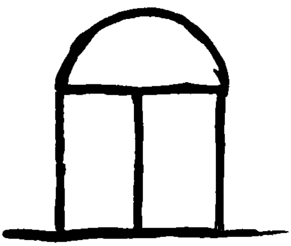


REP. 3.

REP. 4.

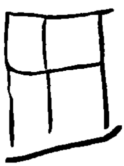
IX.

ORIG.

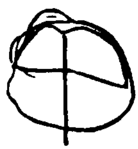


Agent: H. B.

REP. 1.



REP. 2.

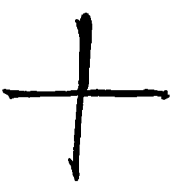


REP. 3.

The percipient said, "It looks like a window."

X.

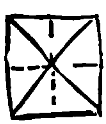
ORIG.



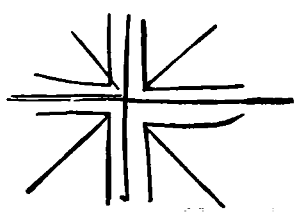
Agent: M. D.

REP. 1.

REP. 2.

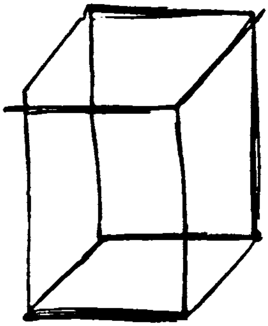


REP. 3.



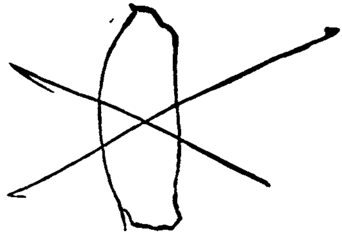
XI.

ORIG.

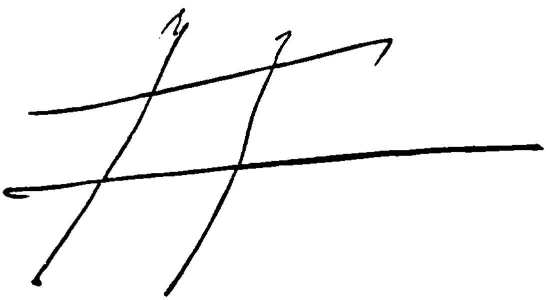


Agent: H. B.

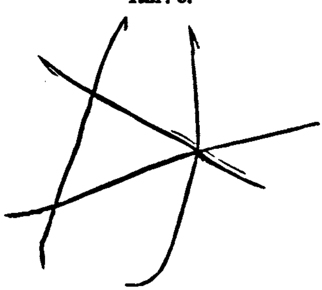
REP. 1.



REP. 2.

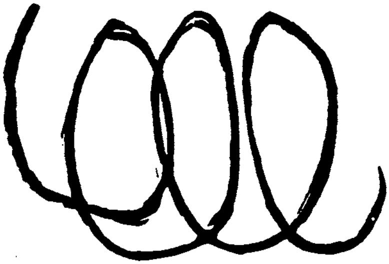


REP. 3.



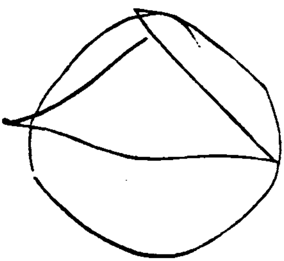
XII.

ORIG.

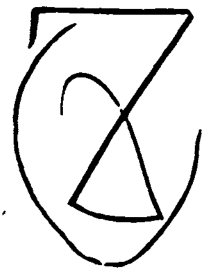


Agent: H. B.

REP. 1.



REP. 2.

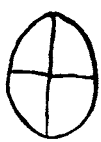


XIII.

ORIG.

Agent: E. W.

REP. 1.

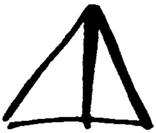


REP. 2.

The percipient said, "It looks like a window."

XIV.

ORIG.

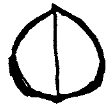


Agent: E. W.

REP. 1.

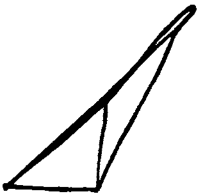
REP. 2.

REP. 3.



XV.

ORIG.



Agent: E. W.

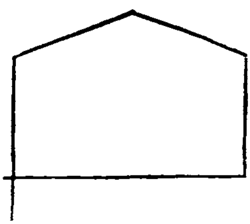
REP. 2.

REP. 3.

The *first* attempt at reproduction appears to have been a failure.

XVI.

ORIG.



Agent: E. W.

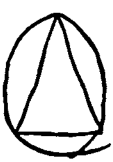
REP. 1.

REP. 2.

REP. 3.

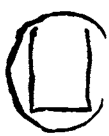
XVII.

ORIG.

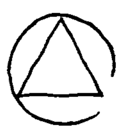


Agent: E. W.

REP. 1.

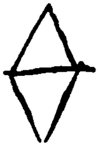


REP. 2.



XVIII.

Orig.



Rep. 1.

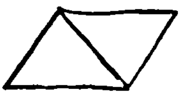
Rep. 2.

Agent: E. W.

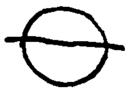
The percipient said, "I see two bright triangles, but I cannot tell exactly how the second is situated."

XIX.

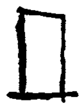
Orig.



Rep. 1.



Rep. 2.



Rep. 3.

Agent: E. W.

In concluding this brief account, I will summarise the results which I would venture to draw from my experiments.

I have always, as has been seen, taken part myself in the experiments, and have never been a mere looker-on. This was for the purpose of guarding against every form of deception to which I might otherwise have fallen a victim, and of finding the key to the explanation of the phenomenon. Although this last hope has not been fulfilled, owing to the small amount of time that I could devote to these observations, I have, nevertheless, noticed some not unimportant points.

The preliminary conditions of a successful sitting appear to me to be :— A very quiet locality, with plenty of fresh air and a moderate temperature. Only persons ought to take part in the experiments whose presence is agreeable to the percipient, and who he knows will not be disturbed or annoyed by occasional failure. The percipient must be in a calm and contented frame of mind ; the agent must be in sympathy with him, and must himself have the knack of conducting the experiments easily and pleasantly. The eyes of the percipient should then be bandaged with a light silk handkerchief, in such a way that the bandage passes also over the ears.

The agent and percipient then proceed respectively in the manner described above.

The agent should now form, as vividly as possible, a mental picture of the object—best imagined as a shining white on a black background. This picture he should hold fast with the greatest energy, and let no other thoughts interfere with it. The percipient, on the contrary, must endeavour above everything not to strain expectation in looking for the emergence of the image, but simply to wait quietly. He should empty his brain, as it were, of all disturbing imaginations, and gaze with closed eyes into a deep darkness. There will then soon emerge in it images of objects, diagrams, &c., which seem to change into one another. He should be patient until one of these remains quietly before him, and seems definite to him. Then he should take the bandage off, and draw what he has seen.

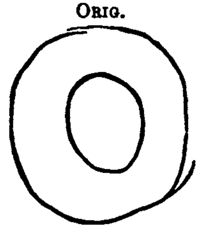
Frequently, at the moment of drawing, the image disappears, and cannot be correctly fixed on the paper. In this case another trial ought to be made. When he has drawn he should ask, "Is it right?" And the answer should be only No or Yes. If he now desires it, a second experiment may be attempted; but more than two should not be made, as this fatigues both agent and percipient.

I have only one further remark to make—that deception, conscious or unconscious, is altogether out of the question as regards the foregoing cases. The above-named gentlemen, as well as myself, pledge their word to that effect.[1] MAX DESSOIR.

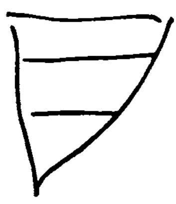
The following shorter record is taken from the monthly journal *Sphinx* (Leipzig), for June, 1886, and we have not seen the original diagrams. The experiments were made at the house of Baron Dr. von Ravensburg, whose wife was the percipient. Herr Max Dessoir drew the originals on the spur of the moment, out of the Baroness von Ravensburg's sight, and taking care that his pencil should move noiselessly. He and the Baron then concentrated their attention on the figure, which the Baroness, sitting at another table, endeavoured to reproduce, after a time varying from 20 to 45 seconds. (The Baron did not take part in the first experiment, which, it will be seen, was a failure.)

I.

ORIG.



REP. 1.

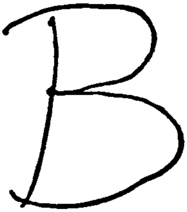


REP. 2.

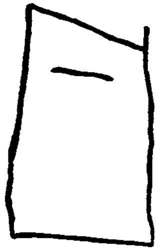
[1] Herr Weiss and Herr Biltz are known to us, through correspondence, independently of these experiments. They and Herr Sachse have sent us certificates of the accuracy of the record of the experiments in which they were respectively concerned.—ED.

II.

ORIG.



REP. 1.



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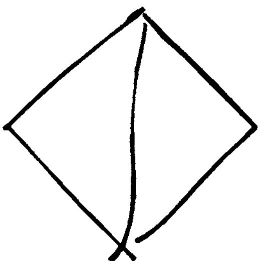
III.

ORIG.



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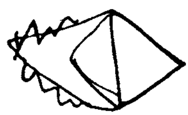
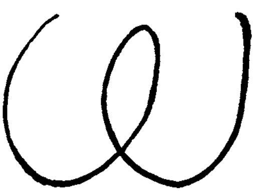


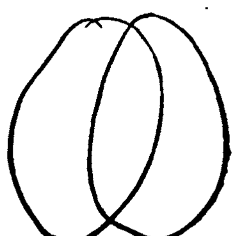
The correction was made by the percipient before the original was shown to her.

IV.

ORIG.



REP.



V.

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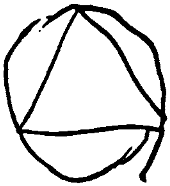

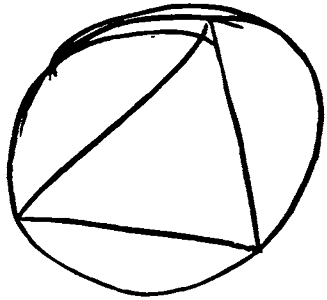

The percipient said, "It is a circle outside, and there is something else inside it;" then, after a pause, "A triangle." She then drew the reproduction, and added that the circle was an imperfect one.

With respect to these experiments, the Baron and Baroness von Ravensburg have sent a note of corroboration, of which the following is a translation:—

"18, Zietenstrasse, Berlin, W.

"July 9, 1886.

"We certify that the report of our sitting for a trial of thought-transference, which appeared in the sixth number of *Sphinx*, is throughout in correspondence with the facts, and has been drawn up with complete accuracy.

"FREIHERR GOELER VON RAVENSBURG.

"ELIZABETH, FREIFRAU GOELER VON RAVENSBURG."

VI.

ON TELEPATHIC HYPNOTISM, AND ITS RELATION TO OTHER FORMS OF HYPNOTIC SUGGESTION.

By Frederic W. H. Myers.

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§ 1. The nucleus of the following paper consists of some personal observations of a remarkable hypnotic subject—observations which the kindness of Dr. Gibert and Professor Pierre Janet enabled me to make at Havre, April 20-24, 1886.

The most striking feature in this case was the *sommeil à distance*, or, if I may so term it, *telepathic hypnotism* ;—the production, that is to say, of sleep and other hypnotic phenomena by the will, or mental suggestion, of a person at a distance from the subject.

This is not, of course, the first time that such a phenomenon has been observed. In *Phantasms of the Living* (Vol. I., Chap. 3 ; Vol. II. Supplement, Chap. 1 ; and Additional Chapter) will be found a collection of the more trustworthy cases; and Mr. Gurney has pointed out their analogy to the *spontaneous* cases of telepathy, of which that book furnishes many examples. But from the side of *hypnotism* no attempt whatever, so far as I know, has been made to correlate this hypnogenous [1] force or suggestion at a distance with hypnogenous agencies employed in the subject's actual presence,—hypnogenous suggestion which actually reaches his ear. The mesmerists proper, talking of their vital influence, have said, "This influence can sometimes act over great distances." And more recently the *suggestionists*, if I may so term them, have sometimes spoken of this distant command as though it were merely a form of suggestion—as if it fell under that heading with as little difficulty as the mere *deferred suggestion*, which works itself out at a distant time, instead of working itself out at the same time, but at a distant point of space.

The confusion involved in both these modes of expression is great. The mesmerists have ignored the difficulty of supposing that an effluence which they hold actually to emanate from eyes and fingers can operate through stone walls and across streets filled with the interfering influence of other men and women. And the suggestionists seem to me never to have analysed what is meant by *suggestion*—a word of indispensable convenience, but which, as I shall endeavour to show, has been used to include methods of hypnogeny which differ widely from one another.

[1] I must adopt from the French the word *hypnogeny* for the production of hypnotic states : *hysterogeny* for the production of hysterical states ; *dynamogeny* for the production of increased nervous activity ; *æsthesiogen* for a substance whose contact or proximity gives rise to unexplained nervous action.

After narrating, therefore, my observations on Madame B.'s *sommeil à distance*, I felt unwilling to leave the case as a mere isolated marvel, and unwilling also to connect it with more familiar forms of hypnotism by what seem to me mere vague phrases about an extension of "the range of mesmeric influence," or of "the scope of suggestion." So I have briefly reviewed some other recent cases—Dr. Héricourt's, Dr. Dusart's, Drs. Bourru and Burot's—and have then endeavoured, in a provisional and very imperfect manner, to analyse the various forms of hypnotic suggestion, and to correlate them, in an intelligible series, with the numerous and disparate methods of experimentally inducing the hypnotic trance which have been practised by competent observers. I have been obliged to do this very briefly, and to omit any discussion of the true definition and limit of "hypnotic phenomena" themselves. This, too, needs doing on a more comprehensive plan than has been yet attempted.

§ 2. Before giving my own notes on Madame B.'s case, it will be necessary to furnish some account of M. Pierre Janet's previous observations, as recorded in his "Note sur quelques phénomènes de somnambulisme." (*Bulletins de la Société de Psychologie Physiologique*, Tome I., p. 24.)[1] Professor Janet was kind enough to allow me to peruse his notes, taken mainly at the actual moment of observation; and, although I am naturally not at liberty to print any matter as yet unpublished, I can vouch for the scrupulous care with which he has compiled his account of the case.

I had also the advantage of conversation with Dr. Gibert and his family, who are well acquainted with Madame B. Dr. Gibert is the "Doyen du Syndicat Médical de la France," and a leading physician at Havre. He has long practised hypnotism, which he has directed mainly to therapeutic ends. Madame B., while at Havre, is received into the house of a sister of Dr. Gibert's; and his family, who have access to the somnambule at all hours, confirm Professor Janet's estimate of her simplicity and honesty of character.

Of the genuineness of the induced somnambulism in this case no doubt has been felt, so far as I know, by any observer. The anæsthesia, the contractures, the variations in reflex action, &c. (as well as the woman's previous history), supply sufficient evidence on this point. But even after this fundamental fact has been proved (which in the present state of our knowledge of hypnotism is not very difficult), there remains a question, less definite indeed, but highly important, as to the temper of mind which the subject carries with her into the trance. Thus, for instance, there is, of course, no doubt as to the reality of the trance of "la nommée Wit-"—whom, thanks to Dr. Féré's kindness, I have

[1] See also *Phantasms of the Living*, Vol. ii., p. 679.

observed at the Salpêtrière—the asylum, or rather the arena, of her hystero-epileptic exploits. But "Wit-" is the very type and culmination of the hysterical diathesis, and her trickiness and love of notice are so integral a part of her that while she runs through her phases of catalepsy, lethargy, and so on, one still suspects (if I may so say) a cataleptic cunning and a lethargic vigilance as to the operator's will.

Madame B., the subject of these researches, is of a very different type. She is a heavy, middle-aged, peasant woman, with a patient, stolid expression, and a very limited intelligence and vocabulary. She has, indeed, been more or less somnambulic from childhood, and a Dr. Féron, since dead, and other persons, seem to have experimented on her long ago. But she has never made hypnotism her business; she was drawn to Havre by some medical kindness received from Dr. Gibert; and care is taken that she shall not make money out of her stay. Her trance-state is never mentioned to her in her normal state; nor does she in any way seek notice as a "sensitive"; on the contrary, she plainly dislikes being sent to sleep from a distance, and has repeatedly tried to prevent it.[1] I have seen her only in the trance-state, and I share the general impression that what she says in that state is naïvely and sincerely said, and probably gives a true account of her own feelings and actions.

I will now briefly summarise M. Janet's principal results.

α. Induction of trance in presence or close proximity of subject.

Sleep usually induced by holding her hand. She is then only responsive to the operator. He alone can make contractures disappear, &c. Gaze from operator's eye unnecessary. Slight pressure of thumb suffices; but no pressure (except *severe* pressure on thumb) is efficacious without mental concentration—operator's *will* to put her to sleep. "This influence of the operator's *thought*, extraordinary, as it may seem, is here quite preponderant; so much so that it can take the place of all other influences." Will *without* touch induces sleep. Taking precautions to avoid suggestion, it is found that (1) M. Janet, while sitting near her, sends her to sleep when, and only when, he wills it; (2) M. Gibert from adjoining room sends her to sleep, M. Janet remaining near her, but not willing; there is evidence that the sleep is of *M. Gibert's* induction, for she is in *rapport* with him only; whereas had sleep come from suggestion of operator's proximity, the suggestion would probably have been derived from *M. Janet's* close presence. Nevertheless, she did know that Dr. Gibert was in the house. (The question as to degrees of proximity will be discussed later on.)

β. Induction of trance at a distance from subject.

[1] M. Janet says (*Rev. Philosophique*, August, 1886) that Madame B., when awake, is not aware that she can be hypnotised from a distance. My remark applies to her knowledge and acts in the incipient or completed trance.

Oct. 3, 1885. M. Gibert tries to put her to sleep from distance of half-a-mile; M. Janet finds her awake; puts her to sleep; she says, "I know very well that M. Gibert tried to put me to sleep, but when I felt him I looked for some water, and put my hands in cold water. I don't want people to put me to sleep in that way; it puts me out, and makes me look silly." . She had, in fact, held her hands in water at the time when M. Gibert willed her to sleep.

Oct. 9. M. Gibert succeeds in a similar attempt; she says in trance, "Why does M. Gibert put me to sleep from his house? I had not time to put my hands in my basin." That the sleep was of M. Gibert's induction was shown by M. Janet's inability to wake her. M. Gibert had to be sent for.

It is observable, however, that MM. Janet and Gibert can *now* (April, 1886) operate interchangeably on the subject; her familiarity with both seems to enable either to wake her from a trance which the other has induced.

Oct. 14. Dr. Gibert again succeeded in inducing the trance, from a distance of two-thirds of a mile, at an hour suggested by a third person, and not known to M. Janet, who watched the patient.

γ. Influence exercised from a distance during trance.

On Oct. 14 she had been put to sleep at 4.15, as aforesaid. At 5, at 5.5, and at 5.10 she rose, exclaimed, "Enough, don't do that," then laughed once, and added, "You can't; if you are the least distracted I recover myself," and fell back into deep sleep. At those moments M. Gibert had attempted to make her perform certain acts in her sleep. Similar results followed from a mental command given in her proximity during her sleep.[1]

δ. Deferred mental suggestion.

On Oct. 8 M. Gibert pressed his forehead to hers, and gave a *mental* order (I omit details, precautions, &c.) to offer a glass of water at 11.30 a.m. next day to each person present. At the hour assigned she showed great agitation, took a glass, came up from the kitchen, and asked if she had been summoned, came and went often between *salon* and kitchen; was put to sleep from a distance by M. Gibert; said, "I had to come; why will they make me carry glasses? I had to say something when I came in." Two somewhat similar experiments were made October 10th and 13th.[2]

§ 3. Thus far M. Janet's account of the autumn experiments, postponing any description of the *stages* through which the subject

[1] Before our arrival in April, 1886, Dr. Jules Janet effected a curious transference of sensation. He went into an adjoining room and burnt his right wrist severely. Madame B. uttered piercing cries, and clasped her wrist in the same place. See *Rev. Phil.* for August, 1886, p. 222, for details.

[2] Some further cases are given in *Rev. Phil.* for August.

passed. In February and in April, 1886, Madame B. was again brought to Havre, and some successful experiments (tabulated below) were made before my arrival on April 20th.

I give next my own notes of experiments, April 20-24th, taken at the time in conjunction with Dr. A. T. Myers, and forming the bulk of a paper presented to the Société de Psychologie Physiologique on May 24th.

"I have been asked to write an account of some instances of somnambulic sleep induced at a distance, which I observed at Havre, through the kindness of Dr. Gibert and Professor Pierre Janet, April 20-24th, 1886. This account is founded on notes taken by me at the time, and revised on the same or following days by Dr. A. T. Myers, who was present at the experiments throughout. Other observers were Dr. Gibert, Professor Paul Janet, Professor Pierre Janet, Dr. Jules Janet, Dr. Ochorowicz, and M. Marillier, some of whom have given, or are about to give, independent accounts.

"I shall confine myself to the cases of production of sleep at a distance by mental suggestion, with one case of deferred mental suggestion of an act to be performed. In order that the phenomenon of *sommeil à distance* may be satisfactory, we have to guard against three possible sources of error, namely, fraud, accidental coincidence, and suggestion by word or gesture.

"The hypothesis of *fraud* on the part of operators or subject may here be set aside. The operators were Dr. Gibert and Professor Pierre Janet, and the detailed observations of Professor Pierre Janet, elsewhere published, sufficiently prove the genuineness of Madame B.'s somnambulic sleep. And, in fact, to anyone accustomed to hypnotic phenomena the genuine character of Madame B.'s trance is readily apparent.

"The hypothesis of *accidental coincidence* would be tenable (though not probable) did the events of April 20-24th constitute the whole of the observed series. But the number of coincidences noticed by Dr. Gibert, Professor Janet, and others has been so large that the action of mere chance seems to be quite excluded. It is to be observed that, as Professor Janet tells us, the subject has, during an observation of several weeks (maintained by Mlle. Gibert when Professor Janet is not present), only twice fallen *spontaneously* into this somnambulic sleep (when no one willed her to do so); once before our arrival, on looking at a picture of Dr. Gibert, and once on April 21st, as narrated below.[1] On the other hand, the observed cases of sleep deliberately

[1] Of the spontaneous sleep on April 21 (mentioned in *e.g.* Case i.), M. Janet writes (*Rev. Phil.*, August): "Elle se rendormit spontanément deux heures après avoir été réveillée, mais elle était dans une période où je l'endormais tous les jours plusieurs fois, et elle avait simplement été mal réveillée. D'ailleurs, pendant ces deux heures d'intervalle, elle n'avait pu ni parler ni manger : elle était donc restée malheureusement dans un état de demi-sommeil."

induced from a distance amount, I believe, to at least a dozen.[1] I exclude, of course, the very numerous occasions when sleep has been induced by an operator present with the patient, by holding her thumbs, looking at her, &c. This, however, brings us to the third source of doubt, whether the sleep may not on *all* occasions have been induced by some *suggestion*, given perhaps unconsciously, by word or gesture. It was thus that I was at first inclined to explain Cases I. and II. among those that follow, but the other cases here given seem to negative the supposition.

"I still, however, would explain by mere suggestion all the experiments which I saw made with the *magnet*. On one occasion, when I had gone into an adjoining room with the magnet, and this was known to all present, Madame B. followed me, as though attracted. She was taken back to her place, and shortly afterwards I came and sat beside her with the magnet in my pocket, no one knowing that it was there. No effect whatever was produced on the subject. I made some other experiments with the magnet, with a similarly negative result. I would strongly recommend that when magnetic experiments are made with sensitives the following precautions should be used, which our experience in the Society for Psychical Research has shown to be necessary for the exclusion of suggestion.

"1. Only electro-magnets should be employed, in order to effect sudden and noiseless transitions from the presence to the absence of magnetic force.

"2. The operator in charge of the commutator should be in a different room from the subject.

"3. Care should be taken that no indication as to the state of the magnet should be drawn from the 'magnetic click' which accompanies the magnetisation of the electro-magnet. [The subject's ears may be stopped, or the click repeated many times running, so that it is impossible to tell whether there have been an even or uneven number of clicks, and consequently whether the condition of the instrument is or is not changed.]

"It is not necessary here to go into further detail. Suffice it to say that it is not safe to trust to an apparently lethargic or anæsthetic state in the subject as a guarantee against her gathering suggestions from the words or manner of persons present. If, moreover, she be susceptible of mental suggestion, the effects of such suggestion may be mistaken for the effects of magnetic influence.

"I. I pass on to describe the first case of *sommeil à distance*, April 21st. At 5.50 p.m. (an hour which was selected by drawing lots among various suggested hours), Dr. Gibert retired to his study and endeavoured to send Madame B. to sleep in the Pavillon, at a distance of

[1] This number, as will be hereafter seen, has since been increased.

about two-thirds of a mile. She was to fall asleep in the *salon;* whereas she habitually sits in the *kitchen* of the Pavillon (a house occupied by Dr. Gibert's sister).

"It was supposed that the command would take about 10 minutes to operate, and at about six Professor Janet, Dr. Ochorowicz, M. Marillier, my brother and myself entered the Pavillon, but found that Madame B. was not in the *salon* but in the kitchen. We immediately went out again, supposing that the experiment had failed. A few minutes later Professor Janet re-entered with M. Ochorowicz, and found her asleep in the *salon.* In the somnambulic state she told us that she had been in the *salon,* and nearly asleep when our arrival startled her, and had then rushed down to the kitchen to avoid us; had returned to the *salon* and fallen asleep as soon as we left the house. These movements were attested by the *bonne,* but it of course seemed probable that it was merely our arrival which had suggested to her that she was expected to fall asleep.

"On this day she was ill and exhausted from too prolonged experiments on the previous days. In the afternoon she fell asleep of her own accord, and in the late evening (11.35 p.m.), when she had long been in bed, M. Gibert willed that her natural sleep should be transferred into somnambulic, and that she should dress and go into the garden of the Pavillon. Nothing followed on this attempt, unless an unusually prolonged sleep and complaints of unwonted headache next day were to be in any way connected herewith. On the whole, had I left after these experiments only I should have referred the phenomena to suggestion of the ordinary hypnotic kind.

"II. On the morning of the 22nd, however, we again selected by lot an hour (11 a.m.) at which M. Gibert should will, from his dispensary, (which is close to his house,) that Madame B. should go to sleep in the Pavillon. It was agreed that a rather longer time should be allowed for the process to take effect; as it had been observed (see M. Janet's previous communication,) that she sometimes struggled against the influence, and averted the effect for a time by putting her hands in cold water, &c. At 11.25 we entered the Pavillon quietly, and almost at once she descended from her room to the *salon,* profoundly asleep. Here, however, suggestion might again have been at work. We did not, of course, mention M. Gibert's attempt of the previous night. But she told us in her sleep that she had been very ill in the night, and repeatedly exclaimed: 'Pourquoi M. Gibert m'a-t-il fait souffrir? Mais j'ai lavé les mains continuellement.' This is what she does when she wishes to avoid being influenced.

"III. In the evening (22nd) we all dined at M. Gibert's, and in the evening M. Gibert made another attempt to put her to sleep at a distance from his house in the Rue Séry,—she being at the

Pavillon, Rue de la Ferme,—and to bring her to his house by an effort of will.[1] At 8.55 he retired to his study; and MM. Ochorowicz, Marillier, Janet, and A. T. Myers went to the Pavillon, and waited outside in the street, out of sight of the house. At 9.22 Dr. Myers observed Madame B. coming halfway out of the garden-gate, and again retreating. Those who saw her more closely observed that she was plainly in the somnambulic state, and was wandering about and muttering. At 9.25 she came out (with eyes persistently closed, so far as could be seen), walked quickly past MM. Janet and Marillier, without noticing them, and made for M. Gibert's house, though not by the usual or shortest route. (It appeared afterwards that the *bonne* had seen her go into the *salon* at 8.45, and issue thence asleep at 9.15: had not looked in between those times.[2]) She avoided lamp-posts, vehicles, &c., but crossed and re-crossed the street repeatedly. No one went in front of her or spoke to her. After eight or ten minutes she grew much more uncertain in gait, and paused as though she would fall. Dr. Myers noted the moment in the Rue Faure; it was 9.35. At about 9.40 she grew bolder, and at 9.45 reached the street in front of M. Gibert's house. There she met him, but did not notice him, and walked into his house, where she rushed hurriedly from room to room on the ground-floor. M. Gibert had to take her hand before she recognised him. She then grew calm.

"M. Gibert said that from 8.55 to 9.20 he thought intently about her; from 9.20 to 9.35 he thought more feebly; at 9.35 he gave the experiment up, and began to play billiards; but in a few minutes began to will her again. It appeared that his visit to the billiard-room had coincided with her hesitation and stumbling in the street. But this coincidence may of course have been accidental.

"IV. Later in the evening M. Gibert made to her a *mental suggestion*, by pressing his forehead against hers without other gesture or speech. The suggestion (proposed by me) was that at 11 a.m. on the morrow she should look at a photographic album in the *salon* of the Pavillon. She habitually sat in the kitchen or in her own bedroom and sewed; so this was an unlikely occupation for a morning hour.

"On April 23rd, MM. Marillier and Ochorowicz went to the Pavillon before 11 and ensconced themselves in a room opposite the *salon*. At 11 Madame B. entered the *salon* and wandered about with an anxious, preoccupied air. Professor Janet, Dr.

[1] It will be seen from the synopsis of experiments given below that the afternoon and not the evening, was the time of day usually chosen.

[2] It was not unusual for her to sit in the *salon* in the evening, after the day's occupations were over.

Myers, and I entered the Pavillon at 11.10 and found her obviously entranced; eyes open, but fixed; anxious; wandering.

"She continued thus till 11.25. We remained in a room where she could not see us, though, by looking through the partially-opened door, we could see her. At 11.25 she began to handle some photographic albums on the table of the *salon;* and at 11.30 was seated on the sofa fixedly looking at one of these albums, open on her lap, and rapidly sinking into lethargic sleep. As soon as the talkative phase of her slumber came round she said, 'M. Gibert m'a tourmentée, parce qu'il m'a recommandée—il m'a fait trembler.'

"I believe that this was a genuine instance of deferred mental suggestion. But where a suggestion is known to so many persons as was the case here, it is hard to feel sure that no word has been uttered by any one which could give a clue to its nature.

"V. On this same day, 23rd, M. Janet, who had woke her up and left her awake, lunched in our company, and retired to his own house at 4.30 (a time chosen by lot) to try to put her to sleep from thence. At 5.5 we all entered the *salon* of the Pavillon, and found her asleep with shut eyes, but sewing vigorously (being in that stage in which movements once suggested are automatically continued). Passing into the talkative state, she said to M. Janet, 'C'est vous qui m'avez fait dormir à quatre heures et demi.' The impression as to the hour may have been a suggestion received from M. Janet's mind. We tried to make her believe that it was M. Gibert who had sent her to sleep, but she maintained that she had felt that it was M. Janet.[1]

"VI. On April 24th the whole party chanced to meet at M. Janet's house at 3 p.m., and he then, at my suggestion, entered his study to will that Madame B. should sleep. We waited in his garden, and at 3.20 proceeded together to the Pavillon, which I entered first at 3.30, and found Madame B. profoundly sleeping over her sewing, having ceased to sew. Becoming talkative, she said to M. Janet, 'C'est vous qui m'avez commandée.' She said that she fell asleep at 3.5 p.m.[2]

Professor Janet's paper in the *Revue Philosophique* for August, 1886, enables me to give a conspectus of the experiments on *sommeil à distance* made with Madame B. up to the end of May. M. Janet makes his total 22 trials, 16 successes, but he seems to have omitted the experiments of October, 1885. The distance was in each case between ¼ mile and 1 mile.

[1] M. Gibert was not with us; but M. Janet often came to see her after M. Gibert had hypnotised her.

[2] On these two occasions (V. and VI.) no one actually saw her asleep before we entered the Pavillon, since we desired Mlle. Gibert not to watch her, for fear that she might guess that an experiment was going on.

No. of Exper'-ments.	Date.		Operator	Hour when given.	Remarks.	Success or Failure.
	1885.					
1	Oct.	3	Gibert	11.30 a.m.	She washes hands and wards off trance.	1
2	,,	9	do.	11.40 a.m.	Found entranced 11.45.	1
3	,,	14	do.	4.15 p.m.	Found entranced 4.30: had been asleep about 15 minutes	1
	1886.					
4	Feb.	22	Janet		She washes hands and wards off trance.	
5	,,	25	do.	5 p.m.	Asleep at once.	1
6	,,	26	do.		Mere discomfort observed.	0
7	March	1	do.		do. do.	0
8	,,	2	do.	3 p.m.	Found asleep at 4: has slept about an hour.	1
9	,,	4	do.		Will interrupted: trance coincident but incomplete.	1
10	,,	5	do.	5.-5.10 p.m.	Found asleep a few minutes afterwards.	1
11	,,	6	Gibert	8 p.m.	Found asleep 8.3.	1
12	,,	10	do.		Success—no details.	1
13	,,	14	Janet	3 p.m.	Success—no details.	1
14	,,	16	Gibert	9 p.m.	Brings her to his house: she leaves her house a few minutes after 9.	1
15	April	18	Janet		Found asleep in 10 minutes.	1
16	,,	19	Gibert	4 p.m.	Found asleep 4.15.	1
17	,,	20	do.	8 p.m.	Made to come to his house.	1
18	,,	21	do.	5.50 p.m.	My case I.: trance too tardy.	0
19	,,	21	do.	11.35 p.m.	Attempt at trance during sleep: see my case I.	0
20	,,	22	do.	11 a.m.	Asleep 11.25: trance too tardy: my case II.: count as failure.	0
21	,,	22	do.	9 p.m.	Comes to his house: leaves her house 9.15: my case III.	1
22	,,	23	Janet	4.30 p.m.	Found asleep 5.5, says she has slept since 4.30: my case IV.	1
23	,,	24	do.	3 p.m.	Found asleep 3.30, says she has slept since 3.5: my case V.	1
24	May	5	do.		Success—no details.	1
25	,,	6	do.		Success—no details.	1
						19

We have thus 19 coincidences and 6 failures[1]—the failures all more or less explicable by special circumstances. During Madame B.'s visits to Havre, about 2 months in all, she once fell into ordinary sleep during the day, and twice (as already mentioned) became spontaneously entranced, one of these times being on April 21, a day of illness and failure. She never left the house in the evening except on the three

[1] Cases 1 and 4 were practically successes, but I have counted them as one success and one failure.

occasions on which she was willed to do so (experiments 14, 17, 21). Trials of this kind had to be made after dark, for fear her aspect should attract notice. The hours of the other experiments were generally chosen at the moment, to suit the operators' convenience; sometimes, as I have said above, they were chosen by lot.

§ 4. I pass on to a brief analysis of a similar case contributed by Dr. Héricourt to the Société de Psychologie Physiologique, November 30th, 1885 (*Bulletins*, Tome I., p. 35.)[1]

α. Induction of trance in presence or proximity of subject. M. Héricourt obtained trance by holding Madame's D.'s hand, then by touching her hand, then by willing alone. Passes or grasp of hand had no effect unless accompanied by will. He induces trance without visible suggestion, from one end of the room to the other, by sudden effort of will while she is talking, &c.; if he relaxes his will she recovers herself.

β. Induction of trance at a distance. One day he tries to send her to sleep from his own house, at Perpignan (say 300 yards off), at 3 p.m.; then forgets her; then at 5 p.m. wills to wake her. She tells him spontaneously that at 3 she fell asleep (quite unusual with her during the day); servant came in; could not wake her; shook her; made her partly conscious, with violent headache, which suddenly disappeared at 5 p.m. Experiment tried from adjoining room in a way to avoid expectant attention. Success.

γ. Influence exercised from a distance.

M. Héricourt could impress no definite suggestion from a distance, but when he thought fixedly of Madame D. she experienced severe pain in the præcordial region. This gradually increased, and led to cessation of the experiments. [Compare cases where an abortive epileptic attack is replaced by epileptiform migraine.]

§ 5. The next case which I shall notice is perhaps the most remarkable of all. I quote it from a paper presented, December 28th, 1885, to the Société de Psychologie Physiologique, by Dr. Gley, the well-known physiologist.[2] But the case is Dr. Dusart's, recorded by him in the *Tribune Médicale*, May 16th and 30th, 1875. Dr. Gley knows Dr. Dusart; and assures me that the account is compiled from careful notes taken at the time.

α. Induction of trance in presence or proximity of subject.

Dr. Dusart induces trance on Mlle. J. by passes: observes that they are ineffectual without his *will*: tries will alone: succeeds more than 100 times: never fails.

β. Induction of trance at a distance. M. Dusart forgot one day to order Mlle. J. to sleep till a certain hour on the morrow, which he usually did.

[1] See also *Phantasms*, &c., Vol. ii., p. 683.
[2] See also *Phantasms*, &c., Vol. ii., p. 685

At a distance of 700 metres from her house he remembers the omission; gives order mentally to sleep till 8: finds her asleep next morning. "How is it that you are asleep?" "I am obeying you." "You mistake; I left you without giving any order." "True; but 5 minutes afterwards I perfectly heard you tell me to sleep till 8 o'clock." To test this Dr. Dusart leaves her asleep, telling her to sleep till he gives order: gives mental order two miles away, at 2 p.m.: she wakes at 2 p.m. Experiment successfully repeated several times: once he keeps her awake by mental order, from a distance, while her father tries to induce trance; she is aware that his influence is at work.

§ 6. These cases, and others like them, seem, then, to enforce on us the conclusion that telepathic hypnotism is a fact—that certain of the phenomena commonly described as hypnotic are occasionally produced by the influence of an operator at a distance, under such conditions that no previous suggestion could have been given.

We must return, in fact, to the conclusion arrived at by the committee of the French Academy of Medicine which sat, as is well-known, from 1826 to 1831, and reported through M. Husson. The fifteenth section of that Report runs as follows:—

"Lorsqu'on a fait tomber une fois une personne dans le sommeil magnétique, on n'a pas toujours besoin de recourir au contact et aux passes pour la magnétiser de nouveau. Le regard du magnétiseur, sa volonté seule, ont sur elle la même influence. Dans ce cas, on peut non-seulement agir sur le magnétisé, mais encore le mettre complètement en somnambulisme et l' en faire sortir à son insu, hors de sa vue, à une certaine distance et au travers des portes fermées."

This, however, is a statement which, taken by itself, would not be likely to obtain effective lodgment in the mind. That it may become truly credible, it must be co-ordinated with cognate facts; it must be presented, not as a mere isolated anomaly, but as an item in some wider group of phenomena. It suggests two inquiries; first, whether other non-hypnotic cases of telepathy exist; and secondly, whether the hypnotic agencies already recognised can be so arranged as that this telepathic agency, this hypnotisation from a distance, should be presented as the culminant phenomenon in a continuous series. Now to the first of these questions, our whole work on *Phantasms of the Living* supplies the answer. There do in fact exist (we maintain) so many cases and classes of telepathic influence that this hypnotic class falls naturally into its place as a species which we should have felt bound to look for on merely analogical grounds. To the *second* question (as I have already said) no answer, so far as I know, has been yet attempted. I do not recall any attempt to correlate this telepathic hypnotism with the other methods of hypnotisation on which different experimenters have relied. The elder English school of *Mesmerists*—

believers, I mean, in a specific, vital effluence or influence—(as opposed to the *Hypnotists* proper, or believers in a mechanical agency alone in the induction of trance),—the mesmerists, I repeat, have altogether missed the distinction between the exercise of vital influence in the presence of immediate proximity of the subject, and its exercise at a distance, say, of half-a-mile. When they have had cases of this distant kind to record they have mentioned them as mere extensions of the specific vital power, without even attempting to show how it can be that an effluence emanating from one man's nervous system, and pervading another man's nervous system by some sort of actual diffusion, can operate in precisely the same manner at distances across which no physiological activity (with the one exception of the skunk's) has been known to project itself.

And the difficulty which is thus ignored by Elliotson and Townshend is consciously dismissed as insoluble by the more cautious observers whom I have been quoting in this paper. For convenience' sake they use the analogy of *suggestion* and speak of "suggestion at a distance"; but they make no attempt to connect this *distant* suggestion with that suggestion in the subject's actual *presence* with the efficacy of which we are now so abundantly familiar.

And I need hardly say that in my own view, also, no complete solution of the problem is possible. We are entirely ignorant of the nature of the force which may be supposed to be operative in the production of telepathic phenomena,—to impel or facilitate the passage of thoughts or sensations from one mind to another without the intervention of the recognised organs of sense.

§ 7. Yet it seems to me that there is something which it is possible to attempt; something which must needs be now attempted, in however fragmentary and provisional a manner, if there is to be any unity, any sense of *ensemble* in hypnotic experimentation; if the results which different observers obtain in such different ways are to throw upon each other the light which they are capable of affording. Some attempt, I repeat, must be made to show the possibility of a transition from the merely mechanical hypnogeny which the majority of modern writers admit, to the vital or mesmeric hypnogeny which Mr. Gurney and I (in accordance with Cuvier, Esdaile, Elliotson, &c.) have defended in these *Proceedings*, and which now again (apart from our directer arguments) is receiving a kind of reflected or inferential probability from these well-accredited cases of "*sommeil à distance.*" And, furthermore, such an attempt should show also what kind of connection is empirically found between this *vital* hypnogeny and the *telepathic* hypnogeny with which this paper is mainly concerned—between the effect, that is to say, of the operator on the subject in the subject's presence, and his effect at a distance. And in order to get any clearness into our notions, we must attack at once the extremely difficult question: What do we mean by *suggestion?* When we say that a subject is hypnotized by *suggestion*, what is the nature, what are the analogies of the force which we suppose to be brought to bear upon him?[1]

First, then, we must observe that the word *suggestion*, as a cause of the hypnotic trance, may have at least four different meanings, viz., (1) verbal suggestion, (2) self-suggestion, (3) mental suggestion from a person present, (4) mental suggestion from a person absent.

Ordinary *verbal suggestion* is, of course, a method of inducing the trance as to which all are agreed. No one accustomed to these experiments is surprised if when an operator says "Sleep!" to a subject whom he has often previously hypnotized, that subject falls into the trance. Very little attempt, however, has been made to co-ordinate this method of hypnogeny with the other methods generally admitted (viz., monotonous stimulation and similar mechanical processes), and confusion is frequently introduced at this point by mixing up psychical with neural terms—by talking in the same breath of "inhibition of nerve-centres" and of "expectant attention." But when we come to consider any of the wider problems—such as whether or not mechanical stimulation is the sole originator of hypnotic phenomena—we find ourselves obliged to reduce our terms to a common denominator, and (however vaguely or hypothetically) to form some conception of the *neural* side of each operation.

Such an inquiry of course takes us on to very insecure ground. We know next to nothing of the neural correlates of ideas or states of mind so complex as some of those which occur in the hypnotic state. We are dealing with a problem which bristles with unknown quantities,—where the physiological correlate of "will," if I may so say, is x, and of "consciousness" y, and of "attention" z. But the hopeful peculiarity of hypnotic phenomena is that in them the unknown $x\ y\ z$ are mixed up and interchanged in all kinds of ways with—I do not say known,—but less completely unknown—elements, namely with sensory stimuli of various kinds, familiar and unfamiliar, which form a sort of $a\ b\ c$,—quantities indefinite indeed, but varying only within certain assignable limits. And though no complete solution to our equations is possible, we may so manipulate them as to get a rather better notion of what $x\ y\ z$ are likely to be than if, (as the pure metaphysician seems sometimes to do), we merely arranged and rearranged the unknown symbols

[1] The chapter on "Sujets et Procédés" in Dr. Cullerre's *Magnétisme et Hypnotisme* (Paris, 1886) may serve as an example of the *incoherence* of our present knowledge of hypnogenous processes. Dr. Cullerre has taken pains to collect a good many recent experiments, but he arranges them in a confessedly empirical—almost haphazard—fashion. [See, however, Dr. Chambard's scheme in my Addendum.]

as fancy prompts. If, however, we try to use only terms of *neural* action, we incur a new danger—the danger of using words, originally definite, in such a way as to imply more knowledge than we really possess. The use that has been made of the word "inhibition" has often, I think, landed us in quite as much vagueness as the "expectant attention" which is the common *psychical* attempt to give the *mot de l'énigme.* For myself I hold that the enigma of hypnotism has no single answer which solves it. I do not believe that the methods by which hypnotic phenomena can be induced—any more than those phenomena themselves—form a *distinct class,* or can be altogether separated from other modes of acting on the nervous system. And I shall therefore prefer myself to use a quite general expression, and to speak throughout of "stimulation of nervous tracts." For all these nervous changes involve, at any rate to begin with, some sort of stimulation, and it is presumable that few or none of them affect the whole brain, or the whole nervous system, in an identical manner throughout. But after making these explicit reservations I must ask the reader to bear them in mind once for all,—since their repetition in every paragraph would render this paper, already cumbrous, altogether unreadable.

Once more. In the remarks which follow I shall class as "hypnotic" or as "quasi-hypnotic" certain phenomena which may seem to have little connection with the familiar phases of the hypnotic trance—to be more plausibly referable to the all-embracing category of *hysteria.* In a survey such as I am attempting, some such laxity of demarcation is, I think, unavoidable I am not going to attempt a formal definition either of hysteria or of hypnotism—an attempt from which those writers have abstained most carefully who have had the widest acquaintance with both affections. The word hysteria, as has been often remarked, designates a mere congeries of nervous symptoms. We cannot deduce these symptoms the one from the other; we cannot present them as radiating from a central lesion. And as regards hypnotism we are scarcely more advanced. We have not reached—we shall probably be long in reaching—any physiological conception which can co-ordinate its Protean phenomena. From the view which would class the "névrose hypnotique" as a mere branch of hysteria I dissent strongly and altogether. I hold emphatically that hypnotic changes are primarily physiological rather than pathological;—supernormal, let me say, rather than abnormal; that while on the one hand they may gradate imperceptibly into hysterical and epileptic instabilities, yet on the other hand they may resemble, or even surpass, the beneficent and developmental changes which follow a judicious moral and physical regimen.

But while thus repudiating a conception of hypnotism which seems to me to result from a too exclusive practice among subjects already diseased, I admit, and even maintain, that the French hospital experiments of the last few years have thrown much additional light on the connection between hypnotism and various abnormal states. They have shown us intermediate cases, interchangeable symptoms, unsuspected transitions of every kind. As the briefest way of illustrating what I mean I will describe a single case of Dr. Pitres'—a case which seems to stand just halfway between what is definitely hysterical and what is definitely hypnotic.

Albertine M., one of Dr. Pitres' best hysterical subjects,[1] is liable, beyond the ordinary hysterical accidents, to a rarer affection of her own. Every now and then she irresistibly falls asleep. Her sleep is perfectly placid, her pulse and respiration normal. Closer inspection reveals two singularities. Her eyelids constantly tremble. A limb raised into the air remains in the attitude where it is placed. Both of these are characteristically hypnotic symptoms. And yet more conclusive characteristics remain. Speak to her, and she will reply. Suggest hallucinations, and she will adopt them. Blow on her eyes, and she will awake.

This seems the description of a spontaneous hypnotic trance. And when we learn further that Albertine can in fact be hypnotized by ordinary means, and that in the induced hypnotic trance and in this spontaneous trance she presents certain phenomena both of anæsthesia and of sensibility to metals which she presents in no other state, the identity of the two trances may seem established.

Yet Albertine's history shows us that the "attaque de sommeil" is in reality the survival or residue of hystero-epileptic attacks of the ordinary kind, which have disappeared under treatment. When she first came under Dr. Pitres' care she suffered several times a day from such attacks, preceded by a complex aura, and including a *phase épileptoïde*, a *phase de convulsions cloniques*, and a *phase délirante*. In the *phase délirante*, (like many similar sufferers,) she maintained the attitude imposed on her, and could reply to questions, and was susceptible of provoked hallucination. And gradually the attacks have dwindled down, so to say, into the *phase délirante* without the delirium, —into the state of gentle sleep, which has never yet spontaneously terminated, in which I first described her. The prodromic aura remains recognisable; but this too has suffered transformation; it has assumed a more prolonged, a more psychical character; it is diluting itself, if I may so say, into a mood of mind.

I have thought that this concrete example might best illustrate the points of contact between hysterical and hypnotic states. In what

[1] See his two tractates, *Des suggestions hypnotiques* (1884); *Des zones hystérogènes* (1885).

follows I shall keep as far as possible to what must be deemed hypnotic, avoiding, at any rate, the distinctly *morbid* causes, the distinctly *degenerative* phenomena, to which the name hysterical is with least question to be applied.

§ 8. Let us try, then, to arrange the various modes of hypnogeny in the order of their simplicity.

As simplest of all I should place mere *massive* stimulation.[1] I suppose that most animals and most men are capable of being "thunder-struck"—of being thrown into a state which our ancestors called *sideration*, and which we now call *cataplexy*.[2]

It has been pointed out that it is probably in some state of this kind that animals really are when they are supposed to be "shamming dead," the shock of terror having exhausted for the time their nervous energy, and rendered them incapable of motion.

This is the condition so often induced at the Salpêtrière, where the sudden sound of a gong, or a bright light suddenly introduced, will throw many of the hystero-epileptics into a quasi-cataleptic state. The *Iconographie de la Salpêtrière*, and Dr. Paul Richer's treatise on *La Grande Hystérie*, contain striking pictures of this sudden conversion of the excited woman into the senseless statue. A crash of a brass band, the bark of a dog, will sometimes check the fierce volubility as if a spring had snapped. As in Virgil's battle of the bees,

> Hi motus animorum atque hæc certamina tanta
> Pulveris exigui jactu conpressa quiescunt.

This instantaneous inhibition may be made useful in various ways.

One of Dr. Paul Richer's plates represents a whole string of women assembled to be photographed, and then immobilised in attitudes of astonishment and terror by a sudden stroke of the gong. On one occasion a thief was accidentally detected by this method. Her hand was in a drawer when a gong sounded, and she was found some time afterwards dumbly and fixedly grasping the pilfered goods.

Madame B., I may add, was herself susceptible to this form of massive stimulation. On April 24th, while she was in the sleep-talking state, a clap of thunder was heard, and immediately induced in her

[1] In opposing massive to localised or to specialised stimulation, we shall of course remember that all stimulation is more or less local and more or less special in character. Under massive stimulation I place the cases where the *quantity* seems more operative than the *quality* of the stimulus applied. With some subjects shock of any strong kind seems equally effective; others will respond to feeble stimulations of special kinds, but are unaffected by the loud noises, bright lights, &c., which are here ranked as massive stimuli.

[2] This word was coined, I believe, by Preyer, and applied to the condition of hens staring at a chalk line; but it is now more commonly used for sudden nervous shock which immobilises the subject.

violent convulsions and marked opisthotonos of the usual hysterical kind.

These experiments on the cataplectic effect of massive stimulation have naturally been made for the most part either on animals or on hysterical subjects, who can be thus impressed by shocks which (like the gong's sound or the electric flash) are in themselves insignificant, and leave no injurious results. To apply the requisite degree of shock to a healthy subject would be quite indefensible. But occasions of terror and astonishment will sometimes spontaneously arise; and real life offers many an intermediate stage between the minute of stunned bewilderment and the *stupor attonitus* of the asylums, where one overwhelming moment seems to have paralyzed the mind for ever after.

Shocks of this generalised kind can admittedly initiate almost any amount of visceral, circulatory, vaso-motor disturbance. It is, therefore, antecedently probable that other hypnogenous methods—being, as it were, secondary or specialised forms of general shock—will be able to exercise a powerful influence of this same kind. Remembering (say) the effects of fright on a rabbit, we need not be surprised that in Dr. Liébeault's practice on the impressionable poor of Nancy hypnotic *suggestion* should be found a cheap and easy substitute for cathartics. Remembering that a startling disgrace may set up diabetes, we need not think it incredible that slight temporary bleeding or blistering should follow the command impressed on a previously-sensitised subject. On purely analogical grounds it is, I think, probable that every constitutional disturbance (and some such disturbances result in good, and not in evil) which sudden shock can produce, will be capable of being reproduced or adumbrated among the results of technically hypnotic methods—of methods, that is to say, which by concentration and specialisation *economize* the amount of shock necessary to affect the system. And here it may be well to point out that I am classing the effect of sudden shock as *hypnotic*, although such shock does not, perhaps, directly produce the most interesting phase of hypnotism,—namely, the sleep-waking, or somnambulic state.[1] It *directly* produces the two phases known as catalepsy and lethargy,[2] and some-

[1] Despine (*Etude Scientifique sur le somnambulisme*, p. 205) cites a case where a fright induced first "léthargie lucide" then "somnambulisme."

[2] See Richer, *La Grande Hystérie*, p. 524, sqq. I may say here that though on some important points I cannot bring myself into accord with the school of the Salpêtrière, I recognise in the fullest manner that theirs is the leading collection of instances; that Dr. Richer's book embodies a greater mass of skilled observation of these abnormal states than is contained in any other single volume. Dr. Féré's numerous and important observations have issued from the same school. [As this paper passes through the press Dr. Bernheim's new and larger book on Suggestion takes a foremost place among works dealing with the *therapeutic* aspect of hypnotism.]

times a state of wandering dream. And this (with M. Richer) I consider amply enough to rank sudden shock among hypnogenous agencies. No one can feel more strongly than I the primary interest of the somnambulic state. But it is not a *constant* element in the hypnotism produced by any method whatever. There is, in fact, no one symptom which by itself can be taken as essential to a hypnotic case. As the results of shock we have cataleptic retention of attitudes impressed on the limbs, open and fixed eyes, anæsthesia, suggestion from gesture, contractures, increased muscular irritability,—all these in various combinations, suddenly checked by means which dissipate hypnotic states, and leaving no trace in the memory. This is enough, I think, to justify us in treating the effect of *shock* as a kind of rude primary type of hypnotic change.

§ 9. Next in order to general shock, or the change or exhaustion produced by *massive* stimulation, may be placed *monotonous* stimulation, as from the tick of a watch, &c. This is usually alleged to be much the same thing—the exhaustion being now effected by repeated small stimuli, instead of by a single stimulus of excessive strength.[1] The process is an interesting one, though it has not often been successfully applied. It is noticeable that Dr. P. Richer dismisses it in a few lines when he is discussing the various methods in use at the Salpêtrière.[2] Yet monotonous stimulation is frequently spoken of as if it were the accepted type of hypnotic procedure. What is the reason of this? The reason, as I take it, is a curious and complicated one; namely, that two common modes of procedure have been classed under monotonous stimulation, whereas this phrase is an inadequate description of one of them and a misleading description of the other. The two modes of procedure to which I refer are Braid's upward and inward squint, and the "passes" of the ordinary mesmerist.

And first as to Braid's method of the fixation of the gaze on a point above and between the eyes. It is at once obvious to anyone who tries the experiment that the sensation thus induced is totally different from the sensation of hearing a watch tick. The ordinary person will not be hypnotized by either the one or the other, but, while he will very soon become unconscious of the watch's tick, he will feel a constantly growing and very peculiar fatigue as he continues the upward convergent gaze. This fatigue is, of course, in part merely muscular, from the strain put simultaneously on the two internal *recti* muscles, but there is

[1] I do not myself feel sure that the exhaustion usually alleged in such cases actually exists. I think that the ticking is very likely a mere form of suggestion. Life is full of monotonous stimuli (the movement of the screw in a steamer, &c.) which very many people have attended to for long periods, without any record of trance thus induced.

[2] *La Grande Hystérie*, p. 530.

also an ache which can be imitated by sharp pressure with thumb and finger on each side of the root of the nose, and which partly results, as I conceive, from the actual pressure of the ball of the eye against the nerves of the orbit—when the eye is, as it were, *jammed* into the position into which it normally sinks gently in sleep. And the correctness of this interpretation is sustained by two facts,—one well-known, and the other a personal observation of my own, which I doubt not that many other persons have made before me. The first fact is that mere pressure on the ball of the eye (*pression des globes oculaires*) is a frequent method in France for inducing hypnotic trance,[1] especially in hysterical subjects. The second fact is that a pinch applied to the root of the nose, or a strong pressure between the eyes, is often similarly effective in the mesmerisation of healthy subjects,—more effective than the "passes," whose monotony is sometimes deemed so essential.[2]

And passing on from these closely-analogous pressures, we come next to Richer's pressure of the vertex (first advocated by Dr. Richer in 1878), which he finds to produce hypnotic trance and contracture; and the pressure on the heads of hens, which the practical henwife employs before any operation of minor surgery on her restless brood. The jealously-concealed *attouchements* of Rarey,—perhaps even the cruder practice of dropping leaden plugs down the ears of horses,[3]—belong to the same category. And these lead us on to Pitres' and Charcot's doctrine of "hypnogenous zones,"—special points, that is to say, which are found (symmetrically arranged or otherwise) on the

[1] M. Lasègue is cited as the originator of this method.—Richer, *op. cit.*, p. 524.

[2] The precise mechanism and effects of the upward and inward squint have never been satisfactorily worked out. Professor Macalister has kindly promised to make certain *post-mortem* experiments bearing on these points, and to communicate the results to the Society. Pending an exacter inquiry, he thinks it possible that the forced upward gaze involves a strain on the capsule of Tenon, and consequent drag on the sclerotic, with intra-ocular tension of the vitreous chamber;—as well as intra-orbital pressure on certain branches of the fifth nerve. If this be so, Braid's squint would *combine* the intra-ocular tension of the French pressure of the eyeball, with the pressure on the fifth nerve which is effected by squeezing the root of the nose. I find that this squeeze is used empirically for checking hysterical attacks, quite apart from any belief in "zones hystéro-frénatrices." It is a familiar fact that the same pressure arrests a sneeze.

[3] Of course the stupifying effect of sudden deafness is operative here; but see Dr. Taguet's case (*Ann. Médico-Psych.*, Vol. xi., 1884, p. 328), where the "occlusion du conduit auditif à l'aide d'une boulette de colon," finds its place as a hypnogenous agency along with "la compression digitale des opercules," and the "pression sur l'ovaire droite,"—this last a typical instance of ordinary action on a hysterogenous or hypnogenous zone.

surface of many patients, of such a nature that pressure on them induces hypnotic trance.[1] Braid's squint, therefore, is, I think, best defined as a pressure on a rudimentary hypnogenous zone of wide diffusion,—a region, that is to say, which is in many persons endowed with that peculiar sensitiveness which sets up a sudden, and suddenly-removable, nervous disturbance of a pervasive kind. As we shall see later on, this amounts to saying that Braid's squint is a form of *localised*, rather than of *monotonous*, stimulation.

Secondly, as to the passes of the ordinary mesmerist. Here, as it seems to me, there has been a curious antagonism of theories, extreme on one or the other side, which has obscured the actual phenomena encountered in practice. On the one side the ardent believers in a "vital effluence" have often exaggerated the importance of "passes"; have often spoken as though every detail of manipulation produced a separate specific effect. And, on the other side, opponents of Mesmer's theory have sometimes been anxious rather to explain away the effect

[1] I cannot go into the elaborate experiments which Charcot, Richer, Pitres, &c., have made on these "zones hypnogènes, hystérogènes," &c. But a few words may indicate the connection thus established between hysterical and hypnotic phenomena. There have been few observations of the kind in England, where hystero-epilepsy is comparatively rare, and in the following sentences I shall mainly follow Dr. Pitres. If a hystero-epileptic patient be carefully examined it will almost always be found that there are one or more points or tracts pressure on which either provokes or arrests the hysterical attack. Among these points are often the top of the head, the ball of the eye, the orbital region; just the points with pressure on which we are so familiar in hypnotic experiment. That these points are not merely imagined by or suggested to the patient is shown by such incidents as the fall of hair growing thereon, or by their accidental discovery while the patient is unconscious. [See also Ch. Féré in the *Progrès Médical*, 1882, p. 42.] They disappear temporarily under the influence of electricity, local anæmia, &c., and have sometimes been permanently abolished by *suggestion* in the hypnotic trance,—another indication of their kinship with hypnotic changes. Patients possessing these zones are also frequently exposed to attacks on hearing some word which recalls a particular set of memories,—the specialised suggestion having the same effect as the localised pressure. [See Pitres, *Des zones hystérogènes*, p. 30.] Furthermore, on the same or other patients points are frequently found, pressure on which will not produce a hysterical attack, but will produce a sleep of the nature ordinarily classed as hypnotic, or will modify the phases of such a sleep, or will awaken into ordinary life patients previously hypnotized by other means. [These last points are styled *hypno-frénatrices*; for they check, instead of generating, the hypnotic trance; but I shall avoid all terms not absolutely needful.] Sometimes a *slight* pressure on a certain point induces a hypnotic trance (which can be conducted through its characteristic phases), while a *violent* pressure on the same point induces hysterical convulsions. Galvanism, local anæmia, hypodermic injections, &c., are found to abolish for a time the hysterogenous zones but to leave the hypnogenous unaltered. On the whole, it would seem that the hypnotic effects are not indeed a mere branch or a mere commencement of the hysterical effects, but are nevertheless related thereto in a manner as yet unknown.

of such passes than to explain it, rather to show that no vital effluence was proved thereby than to determine by experiments of their own how much of truth might lurk in those enthusiastic assertions. And since these mesmeric passes were of the nature of slight stimuli to sight, touch, or hearing, many times repeated, it seemed that *monotonous stimulation* was an obvious and sufficient cause for the effect produced.

Yet I venture to say that persons who have themselves practised this form of hypnotization will be inclined to ascribe its effect to any one of several causes rather than to the monotony of the procedure. As commonly practised now, by Dr. Liébeault, for instance, who has mesmerised some thousands of persons during the last 20 years, the passes and touches made are brief and variable, and although Dr. Liébeault was till lately a strong opponent of Mesmer's theory, his actual experience prevented him from crediting the results of his procedure to monotonous stimulation, and he ascribed them rather to *suggestion*, to the concentration of the subject's mind on the idea of going to sleep.[1]

§ 10. And, in fact (as I have implied above in speaking of the value of pressure between the eyes), there would often be much more reason to attribute the effect of mesmeric manipulation not to mere "passes" but to pressure on a *hypnogenous zone*, of which I have already spoken, and which, under the heading of *localised* stimulation, I would place next to the massive and the monotonous stimulation which we have already considered. For here we have a pressure applied to certain points or tracts, empirically discovered and varying in each case, which serve, as it were, as the trains of gunpowder to fire, if a nervous explosion is to be induced. I speak of a nervous explosion, because these specially-endowed points are more often hysterogenous than hypnogenous,—that is to say, it is oftener possible to induce a hystero-epileptic attack by localised pressure than to induce a mere hypnotic trance.

"I have shown," said Brown-Séquard,[2] who was one of the first to draw effective attention to these zones, "that certain points in the cerebro-spinal centres are able to cause the disappearance of the properties of other parts of the nervous system; and that the same or other points are endowed with another property not yet studied, by virtue of which irritative lesions of these points can augment the activities, or intensify the properties, of more or less distant parts. The influence thus manifested is *dynamogenic.*"

As an instance of well-defined inhibitory points, I will cite the case of Louis V——. In him, as in most cases of hysterical

[1] *Du sommeil et des états analogues*, p. 18, &c.

[2] *Comptes Rendus de l'Académie des Sciences*, March 29th, 1880. See also *Comptes Rendus* for 1879, pp. 657, 888.

hemiplegia, the inhibitory points vary symmetrically with the transference of the hemiplegia from the one to the other side. When he is in his state of dextral hemianæsthesia and hemiplegia, a finger (query, also a stick or other neutral substance?) applied to his left forehead produces "the immediate and complete arrest of the functions of the life of relation";—of all perception on the patient's part of the world outside him. So long as the finger remains pressed to the forehead he is senseless and motionless,—"suspendu et inhibé" altogether.[1]

Here then, by a localised pressure of the simplest kind can be induced at once the very maximum of inhibition. And here again, as in the case of massive stimulation, I would point out that we may well expect that a number of minor phenomena and earlier stages of sleep may in other cases be produced by a method which, in one case at least, is so profoundly effective.[2]

The subject of localised stimulations is one of wide importance in these studies. I must refer the reader to the researches of Brown-Séquard, Pitres, and Paul Richer, and will merely observe that we have here a *specialised hyperæsthesia* of a very significant kind. It appears that the peripheral terminations of various nerves, without inducing any modification of the surface which is obvious to ordinary inspection, have acquired the power, when pressure is applied to them, of setting on foot definite and varied processes of systemic change. The effect produced by touching some of these inhibitory points can only be compared to the effect of an electric shock.

§ 11. And this brings me to the fourth head of my enumeration of hypnogenous agencies.

We have discussed the *massive* stimulation of the whole nervous system; *monotonous* stimulation, tactile, auditory, or visual, and *localised* stimulation of specially sensitive points, any one of which methods may induce in appropriate subjects the state of trance.

And now we come to a class of cases where the agencies—still testifying by the suddenness, vigour, and specificity of their effects to the existence of certain internal tracts of supernormal sensibility—gradually come to indicate something as yet unreached. They gradually cease to be mere exaggerations of influences felt by ordinary persons,—they show powers of affecting the organism possessed by substances which we have

[1] Dr. Brullard, of Nancy, has met with a case where the mere closure of the subject's eyes, apparently by any person, induced somnambulism.—Brullard, *Considérations générales sur l'état hypnotique*, p. 84, (Nancy, 1886).

[2] Madame B.'s two thumbs are hypnogenous points. Pressure on them (by whomsoever exerted) induces trance, sometimes accompanied by convulsions. This was so, at least, when Dr. Myers and I saw her, but one would desire always to trace such cases from the *beginning*, to make sure that *suggestion* has nowhere intervened.

been accustomed to regard as inert, or as operating only in other ways. Here, then, the interest centres rather on the specialisation *without* the subject than on the specialisation *within* him;—we can more easily hope (that is to say) to detect or classify the "æsthesiogens" which affect the organism thus supernormally than to detect or classify the centres of internal susceptibility whose ready response disturbs the subject's nervous equilibrium.

And here, too, recent experiments are to some extent bridging over a gulf which at first appeared insurmountable. The effect of *medicamentous* substances, in mere contact, is so extending our conception of hyperæsthesia that the effect of *metals* in contact is not so absolutely isolated a conception as when it was first observed that the touch of gold or iron induced or removed spasmodic rigidity.

I must confess that in this region I depend entirely on the experiments of others. I have seen nothing myself of a nature to persuade me that the external application of a substance habitually inert when thus exhibited can have any effect upon the nervous system. Nor can I here enter fully into the evidence which has nearly convinced me that such is the case.[1] It must suffice to say that the advocates of these specific influences (almost all of them French medical men) seem to me to be at present in possession of the field; and that the palmary case

[1] The question as to the possibility of proving the influence ot metals, &c., seems to stand somewhat as follows.

On the affirmative side it is necessary to show that,—

1. Definite æsthesiogens have produced definite and constant effects, while other substances have produced no effects.
2. The subject has not been aware (either by permission or by fraud) of what the substance under trial was.
3. There has been no suggestion, by any word or gesture of the operator, that any given result was expected. To this it is desirable to add a fourth condition, namely,
4. That the operator himself has no expectancy of the result attained. This last proviso is meant to guard against thought-transference. Unless, indeed, there is some independent evidence of thought-transference between the operator and subject, this explanation can hardly be *pressed;* but I suspect, nevertheless, that thought-transference has vitiated many experiments.

On the negative side it has sometimes been shown :—

1. That on some subjects any substance indifferently produces the supposed effect (*e.g.*, *mustard*, Dr. Adler, of Berlin, in "A contribution to the Study of the Bilateral Functions," &c., in *British Medical Journal*, Vol. i. of 1879).
2. That many cases depend on suggestion only; *e.g.*, Dr. Hughes Bennett, *British Medical Journal*, Vol. ii. of 1878., p. 759 [only *one* case cited]. Dr. Carpenter, *British Medical Journal*, Vol. ii. of 1871, p. 867. Dr. Reynolds, *Lancet*, Vol. i. of 1877, p. 728.

There have, no doubt, also been instances of fraud, though I cannot find any case cited in detail. [Mr. Wakley's so-called exposure of the Okeys, still cited of Louis V——, reported upon concordantly by at least four physicians, must be taken into account in any future discussion of this subject.

Hypnotic or quasi-hypnotic phenomena can be induced in Louis V—— by certain metals, by certain medicaments (other than metals), by magnets, and by electricity.[1]

I must not enter into the details of the elaborate experiments which have now been made on him and on other subjects by MM. Bourru, Burot, Mabille, Richet, &c.

There are two points only which I need notice here. One is the confirmation which certain earlier experiments—English, French, and German—say, for instance, those recorded in the *Zoist*—afford to these more recent and exact observations. Mr. Gurney and I have elsewhere pointed out (*Proceedings*, Vol. III.) the reservations with which Dr. Elliotson's evidence in the *Zoist* should be taken—his eagerness to ascribe any improvements in his patient's health to mesmerism; his impatient neglect of the precautions necessary to establish a real connection of cause and effect. But though he had the faults of a headstrong temperament, there was no doubt either as to his capacity

in 1877 by the *Lancet* in an editorial (Vol. ii. of 1877, p. 646) as valid, was altogether inconclusive.]

It must be remembered that suggestion is undoubtedly a *vera causa* of effects of this kind. In some cases the metals may be really inert, and the suggestion may produce all the effects. In other cases the metals, &c., may be operative, but suggestion may either exaggerate or counteract their operation. The question is whether there are or are not cases where suggestion is excluded, and I think that there is a sufficient residue of such cases to justify a provisional belief. Without going exhaustively into the subject, it may be enough to give the following references in support of this view. Report of Committee of Société de Biologie (Charcot, Luys, Dumontpallier) in *Gazette Médicale*, April 28th, 1877; Dumontpallier and Magnin, *Comptes Rendus, Société de Biologie*, 1881, p. 349; 1882, p. 147; Charcot's lectures, translated in *Lancet*, Vol. i. of 1878, pp. 81, 158, 302, 393; Wilks, *British Medical Journal*, Vol. ii. of 1878, p. 102; McCall Anderson, *Lancet*, Vol. ii. 1879, p. 41, Vol. ii., 1880, p. 207; Stone, *St. Thomas' Hospital Reports*, Vol. ii., 1880 (cited *Proceedings Society for Psychical Research*, Vol. ii., p. 59); Landouzy and others cited by Chambard, *Dictionnaire Encyclopédique des Sciences Médicales*, third series, Vol x., p 367 ("Influence of Magnet"), and especially Bourru and Burot's case, above cited. As regards the transference of hemi-anæsthesia by magnets (the form of æsthesiogeny which has been most debated) the reader should especially consult Féré's *L'Hypnotisme chez les Hystériques* (*Revue Philosophique*, Vol. xix., p. 1). See also Vigouroux, "*Métalloscopie, métallothérapie, esthésiogénes*" (*Archives de Neurologie*, 1881), and for transference in healthy subjects, Rumpf, *Archives de Neurologie*, January, 1885.

[1] Phosphorised water, for instance, produces sleep and hallucinations; jaborandi produces catalepsy; and these and other states can be made to pass into a typical hypnotic somnambulism, in which, and in which alone, the memory of the states induced by the æsthesiogens is retained. (Berjon, *La Grande Hystérie chez l'homme*, Paris: Baillière, 1886, p. 65, &c.)

or his good faith, and his scattered observations on the effect of the contact of gold, iron, &c., on his patients—observations which are not pressed into the support of his own therapeutic theories—still possess, I think, a serious importance.[1]

My second reflection is obvious enough, though I do not remember to have seen it in print. It is that if these specific influences of a long range of substances, some of them previously supposed to be inert (as here applied) in reference to the human organism, be admitted, then it becomes far more readily conceivable that the human organism itself is not inert in reference to another human organism;—that there *is* some specific vital influence such as the mesmerists have claimed.

In Dr. Berjon's tractate we find records of the influence on Louis V——, and on a female patient, of the following substances or forces. With contact: Copper, platinum, gold, hydrogen, sulphate of copper, potassium bromide, potassium iodide, sulphur, antimony, ammonium, chloride, carbonic acid, electricity (dynamic and static); magnet: human hand. Without actual contact: Gold, mercury, hydrogen, chloride of gold, acid nitrate of mercury, cyanide of mercury, sulphate of iron, perchloride of iron, iodine, opium, chloral, and other narcotics; tartar emetic, &c.; scammony and podophyllin; champagne, and other alcoholic drinks; valerian, cantharides, camphor, jaborandi, pilocarpin; magnet; or a human hand held near the body.

[1] See, for instance, his paper on "A Cure of Convulsive and Rigid Fits," *Zoist*, Vol. ix.

[2] Among the actions of medicaments at a distance which Dr. Berjon records there is one which is all the more curious, inasmuch as the physicians concerned do not appear to have been aware of its special oddity.

Among the medicaments which were held in stoppered (not sealed) bottles, at about 3 inches from the back of the neck of Louis V—— and a female subject were laurel-water and nitro-benzol ("*essence de mirbane*"). Now these are very odorous substances, and we cannot exclude the supposition that the subject smelt them, and was led by the mere suggestion to act in a certain definite manner, when he or she smelt that special odour. But there is a difficulty here. Nitro-benzol is to ordinary senses pretty nearly identical in smell with laurel-water,—and is, in fact, habitually used in the cheap confectionery and other trades as an inexpensive substitute for oil of bitter almonds, the scent of prussic acid being precisely reproduced. But these two subjects, the Jewess and Louis V——, invariably distinguished nitro-benzol from laurel-water, and acted consistently in each case. There was a remarkable hyperæsthesia of some kind, and, considering the effects of the non-odorous substances, it is far from clear that the sense of *smell* was even in this case primarily concerned.

Dilute nitro-benzol provoked convulsive shocks of the arms, and movements as though of drawing with a pencil. Laurel-water with Louis V—— gives rise to convulsive movements of the chest, hiccough, salivation, and tingling of the chest. With the Jewess it gives rise to a religious ecstacy which takes about a quarter of an hour to run its course. Her eyes are upturned and fill with tears; her arms and hands are raised heavenwards; her face expresses beatific

A further and fuller account of these experiments is still expected. I may here give a few samples of the results obtained. The first experiments were made with metals, which were applied to the skin, to see whether transference of the hemiplegia could thus be produced. Lead, zinc, and silver produced no effect. Copper produced a temporary return of sensibility, and a temporary vaso-motor modification, so that a prick on the skin which had not previously bled, bled while a sheet of copper was superposed. Platinum produced an itching sensation, but no transference. Steel produced transference. Gold produced transference, but along with the transference it produced severe pain. The objective character of this influence of gold was shown by a curious incident. Dr. Mabille one day supported Louis V— during a "crise," and the doctor's gold ring touched the patient's hand for several minutes. When the epileptic recovered consciousness he complained of pain in that spot, a "brûlure" appeared there, and the redness lasted for several weeks. "Les phénomènes physiques persistants," says Dr.

vision. She then prostrates herself in adoration and weeps with her head on the ground. Finally she throws herself backwards, with convulsive movements of chest and diaphragm and an expression of grief. This ends in sleep, and she can be thrown into somnambulism and questioned on what she has seen. She says that she has seen the Blessed Virgin in a blue dress starred with gold; that "malheureusement elle n'est pas de sa religion," (for she is not a *converted* Jewess), and that the Virgin has reproached her with her misdoings (which exist, in fact, independently of any form of creed), and has thrown her down on the ground as a sinner. When awakened to ordinary consciousness, "elle se moque des personnes qui lui parlent de la Vierge."

By varying the substance applied, the experimenters have discovered that it is the essential oil of laurel which produces the ecstacy, while the hydrocyanic acid produces the convulsions.

Now I need hardly remind my readers of the prominence of the *laurel* in the descriptions of the procedure of the Pythoness at Delphi. The δάφνη, indeed, generally means the bay or *laurus nobilis*, but in such vague traditional descriptions as we have in Plutarch *(Pyth. Orac.* 6) of the *burning* of laurel leaves before vaticination, or in Lucian *(Bis. Accusat.* 1) of the Pythia's *chewing* the laurel leaf, it is impossible to be sure what genus is meant. [For vaticinatory dreams generated by laurel, see Bötticher, *Baumkultus der Hellenen*, p. 346.] The *prunus lauro-cerasus*, or cherry laurel, may perhaps have grown along with the bay, γυάλοις ὕπο Παρνησσοῖο. And it becomes, surely, a very possible supposition that some early Pythia was accidentally susceptible to something of the same specific influence as these hysterical patients at Rochefort; and that some part at least of the tremors and ecstacies of later prophetesses consisted in a repetition by suggestion or tradition of the excitement which in some πρωτόμαντις was genuine and uncontrollable. We should thus have a *hysterical succession*, such as that which (if we are to trust the comments of the rival school of Nancy) "la nommée Wit--" is likely to found among many generations of patients in the hystero-epileptic wards at the Salpêtrière. [As this paper passes through the press, similar instances of the effect of magnets and medicaments at a distance are given by Dr. Dufour; *Ann. Méd.-Psych.*, Sept., 1886.]

Berjon, (*Op. cit.*, p. 19) "rendaient toute simulation inadmissible." The next step was the discovery that gold would act at a distance. "Il suffit d'approcher un objet d'or, une montre, une pièce de vingt francs à 10 centimètres, pour que le sujet, qui n'a pas vu ce qu'on lui présente, accuse une vive douleur." Mercury, acting through the glass bulb of the thermometer, was similarly painful. Hydrogen, on the other hand, produced a quite different effect. "Une éprouvette contenant du gaz hydrogène est mise au contact de la main; le malade manifeste une vive satisfaction et il rit; le rire est continu et spasmodique:—aucun phénomène de transfert ne se produisit."

The medicaments were for the most part held in stoppered (not sealed) bottles, wrapped in paper, a few inches from the back of the subject's neck. The effects produced were curiously connected with hypnotism by the fact that though they often constituted a crisis which left no waking memory, and could not, at its height, be suspended by hypnotization, yet when the effect of the drug was declining it was possible to throw the subject into the somnambulic state, and then to obtain from him an account of the sensations which the drug had produced. The effects of the several drugs were roughly analogous to their known effects, but presented some new and constant features. It is claimed that these characteristic effects were produced when the experimenter was not aware what drug he was holding in his hand; nay, even that when the experimenter was mistaken as to what drug he held in his hand, the phenomena were still such as the drug actually presented should induce.

These experiments are still under discussion; nor have I myself seen any effects of this kind which might not have been due to suggestion. Nevertheless, as already implied, the evidence for the specific effect of contact with *gold*, for instance, on certain subjects seems to me very strong; and I therefore recur to the point urged above; namely, that it seems not unreasonable to suppose that if a human body is so abnormally sensitive as to enter into contracture at the touch of gold, and to distinguish gold by contact alone, or by proximity alone, from other metals, it may not be altogether insensitive to the touch of another human body—another centre, that is to say, of forces and perceptions like its own.[1]

[1] Between susceptibility to metals, &c., and susceptibility to the influence of *living* bodies, susceptibility to the proximity of *dead* bodies would occupy an intermediate place. Perhaps we may thus explain the following narrative sent to us as a "ghost story" of unusual type and good attestation. It comes from Mrs. Wheeler, 106, High-street, Oxford, who is known to Mr. Podmore.

"In the summer of 1874 we moved into the house we now occupy, 106, High street, Oxford. We had the house on lease for some years, but had never lived in it, having let the upper part of it.

We took as our bedroom the lower of two rooms built over an archway at

Rather it seems probable that just as the hypnotic effect of mere massive or mere monotonous stimulation may be connected by the intervening link of localised stimulation, with the hypnotic and other neural effects of the contact or even the proximity of specific inanimate substances or specific non-vital forces. viz., magnetism and electricity, even so may these specific effects be themselves connected with the specific effects of *vital* contact, *vital* proximity.

the side of the house. On the first night that we slept there I woke up just at 12.45 (I heard a church clock striking the quarters), with a feeling that there was something terrible up in the roof. I don't know what it was, but I lay awake with this feeling for nearly an hour, and then I woke my husband and told him of it, and he fetched me some brandy.

I found, however, that I could not shake off the feeling and go to sleep again. I could not even stop in the room, but came out into the sitting-room, and sat up there until 5 a.m., when I went back to bed. I did not have the horrid feeling at all when I was out of the room.

The next night I woke again at 12.45 a.m., with the same dreadful feeling, though not quite so bad as the first night. The same thing happened night after night for some weeks, and I woke up at the same time with the same feeling of something horrible up in the roof. I did not sleep any night, I believe, from the time I awoke—12.45—till after 5 o'clock.

Once, I remember that I went up into the passage over our room, and tried to get at the space under the roof, but found that I could not do so; and once, in the day-time, I tried to get into the space under the rafters, through the bed-room, where there had once been a means of communication, but I found that it was built up, and that I could not get there.

At last my health would not stand it any longer, and I went away on a visit to a brother at Cambridge. Whilst there, I heard that the roof over the two bed-rooms had fallen in, and forced a bedstead through the floor of the upper room into our own bedroom. That I took as a sufficient explanation of my feeling of horror.

It was not for some weeks afterwards that I learnt by accident (my husband had purposely concealed the fact from me, fearing the effect on me in my weak state of health), that the dead body of a child had been found, hidden under the rafters of the roof, over our bedroom. The body was dried up like a mummy, and the head was twisted round. It was evidently the body of a new-born child that had been murdered and placed there for concealment.

ELLEN M. WHEELER."

"Mr. Wheeler fully confirmed his wife's account of the incident, and assured me that he recollected distinctly his wife's distress of mind, and her saying that she felt sure that there was something wrong up above their heads.

"F. PODMORE.

"*May 24th*, 1884."

The following corroboration is extracted from a local paper :—

"*Oxford Times*," *Saturday*, *26th September*, 1874.

"A SKELETON IN THE ROOF.—Yesterday week (*i.e.*, the 18th) the decayed rafters of a corner of the roof of premises in the occupation of Mrs. Wheeler, bookseller, High-street, suddenly fell in, when the skeleton of a child was dis-

§ 12. But here let us pause; for we have arrived at a point where we may hope to get some insight into the mechanism of *suggestion;* that is to say, of the first of the four forms of suggestion which I distinguished earlier in this paper—the verbal suggestion of a person present with the subject.

And first let us try to realize exactly what verbal suggestion will do.

covered, which appeared from its condition to have been secreted there for a number of years."

Somewhat similar is another case, received from a lady well known to me, who prefers not to give her name.

"*June* 11*th*, 1883.

"In the summer of 1872, my father occupied a rectory house not far from Blisworth, in Northamptonshire, for a few weeks, and I went down to spend three days with him and my mother at Whitsuntide; my two children and their nurse being already there. The room given to me was over the dining-room; next door to it was the night nursery, in which my nurse and children slept, the rest of the inmates of the house being quite at the other end of a rather long passage. I hardly slept at all the first (Saturday) night, being possessed with the belief that some one was in my room whom I should shortly see. I heard nothing, and I saw nothing. The next morning, Sunday, I did not go to church, but betook myself to the dining-room with a book. It was, I remember, a perfectly lovely June morning. Before I had been a quarter-of-an-hour in the room, and whilst wholly interested in the book, I was seized with a dread, of what I did not know; but in spite of the sunshine and the servants moving about the house, I found it more intolerable to sit there than it had been to remain in the room above the night before, and so, after a struggle, and feeling not a little ashamed, I left the room and went to the garden. Sunday night was a repetition of Saturday. I slept not at all, but remained in what I can only describe as a state of expectation till dawn, and very thankfully I left on the Monday afternoon. To my father and mother I said nothing of my two bad nights. The nurse and children remained behind for another week. I noticed that the nurse looked gloomy when I left her, and I put it down to her finding the country dull, after London. When she returned she told me that she hoped she would never have to go to stay in that house again, for she had not been able to sleep there during the fortnight, being each night the prey of fears, for which she could not account in any way. My father left this rectory at the end of the summer; and some time afterwards he was talking of the place to me, and mentioned laughingly that before he entered it the rector had 'thought it right to let him know that that end of the house in which I and my children were put up was said to be haunted, my room especially, and that several of his visitors—his sister in particular—had been much troubled by this room being apparently entered, and steps and movements heard in the dead of night. I do not like to let you come in,' he added, 'without telling you this, though my own belief in it is small.' Within, I think, a year or 18 months at most of my father's leaving, the house had to undergo considerable repair, and amongst others, a new floor had to be laid in the dining-room. On taking up the old boards four or five (I forget which) skeletons were found close under the boarding in a row, and also close to the hearth-stone."

Some analogous cases are recorded by Stilling and other writers. This physical explanation would apply only to a small proportion of the narratives sent to us as indicating the continued operation of deceased persons.

Here again we may most conveniently begin with its most advanced or conspicuous effects,—cases where the mere utterance by a casual bystander of one special but apparently harmless word, like "frogs" or "telegram,"[1] throws the subject instantly into the convulsions, delirium, and insensibility of a hystero-epileptic attack. At first sight this might seem the strangest of all effects of verbal suggestion; yet it is soon seen to be a mere intensification of familiar phenomena,—an exaggeration of the brain's reflex irritability quite in keeping with the other *exaggerations* which characterise the hysterical state.

We are all familiar with the extraordinary sensitiveness which a particular group of memories may acquire in healthy minds,—the mother's sudden start, at her child's wail, from the slumber which her husband's snoring has left undisturbed,—the access of blinding unconsciousness to the surrounding scene which follows on the casual mention of some secretly-loved or secretly-dreaded name. Here, then, the touch, so to say, which falls on a definite region within the brain,—the region occupied by that hypersensitive group of memories, — produces an effect analogous to the effect produced by a touch on some hypersensitive peripheral tract,—say the drum of the ear or the scar of an old wound. And just as this natural or traumatic sensitiveness of particular points on the surface is (so to say) parodied and exaggerated by the morbid and arbitrary sensitiveness of the hysterogenous zone, and the patient is thrown into convulsions by a touch which would merely have tickled the healthy subject, even so the instinctive or acquired sensitiveness which certain groups of memories in most of us possess is parodied and exaggerated by the morbid and arbitrary sensitiveness of the girl who because her companions once put frogs in her bed cannot hear the word "frogs" without a hystero-epileptic attack.

If then we can thus compare the hysterogenous suggestion to the pressure on a hysterogenous zone, may we compare the *hypnogenous* suggestion, which more directly concerns us, to pressure on a *hypnogenous* zone? To a great extent I think that we may. Note in the first place that hypnogenous suggestion is not really so simple and easy a thing as is sometimes represented. I doubt whether it is ever the case that *non-hysterical* patients can be hypnotized for the *first time* by a mere verbal command, without the *gaze or touch or will* of the operator. I think that all that we can fairly say is that when a subject has been previously hypnotized by other means, or has previously undergone hysterical attacks which involve, or at least predispose to hypnotic changes, that subject can often be hypnotized again by the mere verbal revivification of that group of organic memories which have been originated by the previous trance. If this be so, the hypnogenous suggestion

[1] Pitres, *Des Zones Hystérogènes*, p. 30

would be allied to the hysterogenous suggestions somewhat as the pressure on the one class of zones was allied to the pressure on the other,—the lesser and more definite effect not being, indeed, a mere branch or commencement of the larger and more confused effect, but being related thereto,—say somewhat in the way in which the act of pressing the foot on the pedal of a piano is related to the act of sitting on its keyboard.

But the hypnotic suggestions with which we have to deal comprise many other suggestions besides that of falling asleep. They comprise the definite hallucinations, the definite commands, of which we have of late had so many examples. But here again it would be a mistake to assume that induced hallucinations, for instance, are a mere outcome or incident of the hypnotic state. Rather we may say that even as pressure on the pedal modifies the loudness and continuity of the sounds produced by striking each individual note, so (and in a much greater degree) do the general nervous changes of the hypnotic trance increase the definiteness, isolation, persistency, of the faint instinctive impulse to belief which follows when we hear a statement confidently made.[1] Hallucinations, though more easily induced in hypnotized persons, can often be induced in persons in the waking state, who have previously been hypnotized, and sometimes on persons who have never been hypnotized at all. I have myself repeatedly made a certain subject believe for a minute or two that she both saw and smelt a hole singed in her dress by an imaginary coal, although I could not hypnotize her, nor had she ever been hypnotized by anyone. The sight of children, or the remembrance of one's own early childhood, is enough to explain this state of mind. I can remember my own feelings at four years old, when a respected elderly friend stated that he was a bear, and simulated to some slight extent the movements of that quadruped. I knew all the time that it was Mr. S.; but the idea of bears, pre-existing in my mind, was so strongly stimulated that I was paralyzed with terror. It was an incomplete hallucination, induced not in a hypnotized but in an immature brain by a definitely-localised stimulation—by a touch on a group of exciting mental pictures which experience had not yet sufficiently partitioned off from the milder scenes involving only old gentlemen and sofas.

The susceptibility to suggestion, then, which characterises the hypnotic trance, involves in effect an exaggeration of the sensibility of groups of images within the brain, which—in the absence of control from inhibitory or co-ordinative centres—develop with greater readiness into hallucinatory perception, impulsive acts. Observe, however, that with hardly any subject are all suggestions equally efficacious.

[1] See Bernheim "*De la suggestion dans l'état hypnotique et dans l'état de veille,*" (Paris, 1884), on this topic.

Even when the operator seems to have obtained complete control—to be able to prompt a well-bred subject to theft and murder—he may still be unable to prompt to rudeness or indecorum. The explanation of this odd fact I take to be that the nexus of habit which opposes the infraction of rules which we constantly obey, though constantly in a position where we could break them, is stronger than the nexus of habit which opposes an act which we theoretically disapprove, but have never been within measurable distance of committing.[1]

Somewhat similarly, we find that hypnogenous zones generally occur in seldom-touched parts of the body, where the habitual link between sensation and responsive action is not strongly established.

I venture, then, to suggest that were the whole plexus of brain-operations unrolled before us, we should see the specific sensibility gaining one ideational centre after another, as suggestion is repeated, very much as one point after another on the periphery may become modified into a hypnogenous zone. And the stimulus of appropriate suggestion,—still in the first place peripheral, as given by voice or gesture, through ear or eye,—touches, as I conceive, a hypnogenous tract within the brain, and though no longer massive like the gong, makes up in precision for what it lacks in volume and intensity.

Thus far I am supposing that the subject will accept the suggestion to sleep, or other suggestions, with equal readiness at anyone's command. But this is not universally the case,—with healthy English subjects is almost never the case. Far more frequently there is a further specialisation,—again a specialisation *without* the subject superadded to the specialisation *within* him,—and we find that he can only be entranced by certain definite persons,—possibly by one person alone among very many who make the attempt. On what does this difference depend? What are the qualities in that complex entity, the human

[1] Different subjects vary greatly in this particular, affording sometimes, as Professor Beaunis has observed, a singular insight into the relative vigour of their inward motives. Some experiments of Mr. Langley's on animals, interesting in this connection, are described in his paper "On the Physiological Aspect of Mesmerism," read before the Royal Institution, March 14th, 1884. "In man," he says, "the phenomena of mesmerism are of a very much more striking character than they are in the lower animals. Speaking generally, this seems to be due to a greater interdependence of the various parts of the nervous system in the lower animals. In these, when any one centre is stirred up by exciting impulses, an irradiation of exciting impulses is apt to take place to all other centres, and the mesmeric state is in consequence apt to be broken. And, on the other hand, when a centre is inhibited, an irradiation of inhibitory impulses is apt to take place, and the whole nervous system is in consequence apt to be inhibited. Hence the activity, or suppression of activity, of particular parts of the central nervous system, which forms so conspicuous a feature of mesmerism in man, can be only partially produced in the lower vertebrates."

operator, whose presence or absence involves these differing degrees of hypnogenous efficacy?

Heidenhain has suggested that differences in warmth or moisture between the *hands* of various operators are enough to account for these variations in power. To say nothing of other objections to this view, it is disproved by the fact that the touch or immediate proximity of the operator's hands is by no means an essential part of the mesmeric process. Again, this apparent *electivity* has been explained by suggestion of the ordinary kind—as though when A can entrance a subject and B and C cannot, this were because the subject had an idea to begin with that A alone could entrance him. But this view also has been, I think, sufficiently disproved by experiments on unconscious persons, on sleepers, and on young children.

And if previous suggestion—preconceived idea—cannot explain the fact of entrancement by one special person rather than another, then neither can it explain the incidents of the *rapport* which continues to exist during (or even after) the trance between the subject and the operator thus elected. That *rapport* shows itself in various ways. There may be a special sensitiveness to the operator's *voice*, so that his mere whisper is heard and recognised amid a Babel of other sounds. There may be, as in Dr. Taguet's case and in several of Elliotson's cases, a special sensitiveness to the *scent* of the operator, recalling the dog's power of discovering articles which his master has touched. There may be a vaguer sensitiveness to his presence,—or even, as I am inclined to hold, to his mere approach,—till the perception verges into what can no longer be called hyperæsthesia, but rather a clairvoyance or clairaudience, which does not strictly follow the lines of any special sense, but specialises itself in what seem arbitrary and unexpected ways, and detects qualities which have never before proved so directly discernible.

And, moreover, besides these sensory (or supersensory) elements in the mesmeric *rapport*, there is commonly what is called a psychical connection as well, an obedience of the subject to the operator's will, and his will only—a concentration of the enthralled attention into that single channel.

Various as are these phenomena, and impossible as it at present is to co-ordinate them adequately, I may perhaps venture to give to all of them alike the title of "selective hyperæsthesia,"—implying that the sensitised organism of the subject responds to one particular class of stimuli with more than normal readiness, and in other than normal ways. And I should compare the special sensitiveness to the operator's scent or voice to Louis V——'s sensitiveness, say, to opium in mere external contact; while the subject's sensitiveness to obscurer facts concerning the operator (as his mere approach or his maladies) may be paralleled with the sensitiveness of other subjects to the touch or proximity of gold, or of other substances which, when thus exhibited, are ordinarily inert.

§ 14. May not this notion of selective hyperæsthesia, when carefully dwelt upon, suggest to us that the "mesmeric *rapport*," the "vital effluence" of which Elliotson, &c., were wont to speak as of a single force or entity, may in reality be a varying complex of different elements? and that among these elements may be something which is neither "will-power" nor "character" in any psychical sense, but simply the specific effect of the proximity of certain living organic compounds? The subject feels Brown's "influence" and not Jones's, or feels Brown's influence soothing and Jones's distressing. But Brown and Jones, whatever else they may be, are at any rate aggregates of organic compounds, and it is possible that to these hyper-sensitive perceptions, Brown may differ as much from Jones as podophyllin from jaborandi.[1]

And thus we gradually find ourselves led up to the conception of a vital influence or effluence by a method which does not present this vital effluence as a unique, isolated, incomparable force, but rather as an advanced term in a series of influences, each one of which needs to be discovered by direct experiment, while the discovery of any one of them corroborates the efficacy of the rest. It is in this way, perhaps, by gradually fitting together a number of results obtained at separate points of a far-reaching line, that modern science has the best chance of confirming whatever may be sound in the interesting but enigmatical experiments of Reichenbach,—experiments which the writers in these *Proceedings* have never consented to set aside as illusory, though we have thus far found them almost impossible to repeat.

Returning to the special inquiry on which we are now engaged,—as to the true *modus operandi* of hypnotic suggestions of various kinds, let us note the results now reached. We have seen that suggestion in its simplest form—the verbal suggestion of any person taken at random—is analogous to the *localised stimulation*,—stimulation of a simple kind, but applied to particular nervous tracts,—which in some patients induces hypnotic or hysterical phenomena. And we have seen, again, that suggestion somewhat more differentiated,—the verbal suggestion of some one person who is alone capable of reaching the subject's attention, —is analogous to the *specialised stimulation*, as from metals or drugs

[1] Dr. Despine maintains (*Etude Scientifique sur le Somnambulisme*, pp. 134, sqq.) that "convulsive epidemics" are propagated by actual infection,—without sight or other suggestion of an ordinary kind. His instances seem to me inconclusive; but the point needs attention. (See *Ann. Méd.-Psych.*, 1879, Vol. ii., p. 141.)

exhibited in a manner which would ordinarily leave them inert, which in certain patients is followed by results of such a singular kind.

§ 15. We come now to the third class of suggestion which we mentioned at the outset, namely *self-suggestion;* giving this name to cases where the trance does not, so far as we can tell, follow on any hint supplied to the patient by another mind, or on any external stimulus whatever,—where the subject does not suppose that anyone orders him, or desires him, or expects him to fall into the abnormal condition, but falls into it, as we say, "of himself."

This self-suggestion is one of the most difficult, and one of the least explored, branches of our subject. It involves, in fact, the whole connection between what we call *spontaneous* and what we call *induced* nervous changes; and moreover, as is commonly the case when the word *spontaneous* is introduced, the chief of all dilemmas—*free-will or necessity?*—looms in the background of the inquiry.

First, let us consider what are the cases which we have to explain. Under what circumstances do hypnotic or quasi-hypnotic nervous states appear without manifest external stimulus? I am bound to add the word "quasi-hypnotic," for I have insisted on the close connection between hypnotism and certain hysterical states, such as Albertine's "attaques de sommeil"; so that I must face the difficulty of *their* origination as well.

We find, then, that there are a few cases where subjects can throw themselves into trance by a mere effort of will. There are a few cases where subjects can maintain themselves in the trance by a mere effort of will; and there is a case where a subject in a state of complete inhibition originates phenomena otherwise producible only by the strongest hypnotic suggestion.

Again, we find that there are cases where there is no question of deliberate choice on the subject's part; where the special nervous condition is hereditary, as in a famous family where "the sons in their nightly discursions ran against and awakened each other"; or where the trance appears as an idiopathic symptom in an otherwise normal person, as is so often the case with ordinary somnambulism; or where a tendency to trance and similar conditions follows upon some definite injury.

Now I think that most of these instances may be shown to be analogous to the "deferred suggestion" of recent hypnotic experiments,—nay, I hold that the perception of that analogy is essential to our comprehension of the experimental "deferred suggestion" itself. Thus much, therefore, I am bound to try to show. But I suspect that this is not the only analogy involved. I suspect that in some of the cases of self-suggestion where the element of *will* is markedly present, we have an analogy to another mode of hypnogeny, namely, to the induction of trance by an actual "vital effluence" from operator to subject. At any rate, whether this be so or no, I feel sure that we shall not get at the root of the matter without trying to realise what would be meant by such an hypothesis.

First, then, let us try to understand deferred suggestion, and its connection with heredity. We start with the fact, which to us is at present ultimate, that living matter has a tendency not only to respond to a stimulus A, but to be so modified by that stimulus that it thereafter responds in a rather different manner to A, when A recurs—and sometimes also to other stimuli, B, C, D. This is rudimentary or unconscious memory. As we get higher in the scale consciousness is superadded, and the memory of A persists while B, C, and D are occurring, even if it does not actually modify them. Suppose a physician says to me at dinner, "You have heart disease; you must go upstairs very slowly or you may die." This is a deferred suggestion to me to move slowly when the time comes for me to go upstairs. I *need* not think of it till that moment comes. But it impresses my mind so much that I continue to think of it while I am merely sitting at dinner—the consciousness of A persists through B, C, and D, though it will only modify my action when the cognate moment recurs—the going upstairs, which is a sort of A′ if the physician's warning were A. Here we have a deferred suggestion, consciously received, and consciously borne in mind till the time to work it out in action arrived.

But neither of these intrusions of consciousness is necessary to the efficacy of a deferred suggestion. In the first place, we know from common experience that the brain receives many impressions which do not at once rise into consciousness, but do so when a favourable opportunity offers itself. Only a small proportion of the impressions which certain powerful stimuli (as incipient asphyxiation or certain forms of fever) can summon into retrospective consciousness do actually rise into consciousness at the moment when they are received. These are deferred suggestions, not consciously received. Take the most trivial and familiar case. I glance down a list of books to see if a book by *Helmholtz* is in it. It is not, and I remember absolutely nothing of the list. Some time afterwards I see a book by *Herzen* advertised. I at once feel that I knew that there was an author of that name. His name, in fact, was on that former list, and I unconsciously received the deferred suggestion—only capable of revival by seeing the word *Herzen* again—that there was a scientific author of that name. Now deferred suggestion in the hypnotic state offers an exaggeration of such a case as this. I suggest to the hypnotized subject that when he leaves my room (after an hour of waking life) he will perceive that his hat is blue instead of black, or that he will send a telegram to tell me that it is a fine day. (For my present argument there is no difference between the suggestion of a hallucinatory percept or of an unmotived action.) When the fixed moment comes—the moment whose arrival is as A′ to my suggestion A,—that suggestion recurs and works itself out, though during B, C, and D, the events of the intermediate hour, no knowledge of A was present nor could be summoned into the conscious mind.

Yet between this absurd act and my forgetfulness of the name *Herzen* there is only a difference of degree. In the hypnotic subject's case one loom (so to say) in the vast manufactory within him has been disconnected from the general system of driving-gear, attached to driving-gear of its own, and set to turn out a special pattern, independently of the orders executed by the remaining looms in the factory. And similarly in my case the little bobbin (so to say) with the name *Herzen* inscribed on it, went on spinning by itself without connection with my general scheme of memories. Only it was so small an item that its disconnected action and its subsequent attachment to the main system attracted no notice, or, at least, excited no surprise. *Deferred* hypnotic suggestion, in fact, like the *immediate* hypnotic suggestion of which I have already spoken, is an advantage taken of the increased dissociability of mental elements which results from the inhibition of certain co-ordinative mental centres or activities in the hypnotic trance.

There are other conditions also in which suggestion can take a hold of the mind which the co-ordinative centres cannot check. I have spoken already of the excessive sensitiveness of certain groups of memories in childhood; for instance, the idea of bears. Well, this sensitiveness, prior to the age of co-ordination, may be taken advantage of to effect a deferred as well as an immediate suggestion. Just as my friend who told me that he was a bear, gave me for the moment something approaching to a hallucinatory percept of himself as a bear, so do some nurses talk to children about ghosts in a way which implants a deferred suggestion of horror in traversing churchyards, which no adult reason can overcome.

> "Trunken mussen wir alle sein;
> Jugend ist Trunkenheit ohne Wein,"

says the poet. And just as youth, with its strong irreflective impulses, its organic exhilaration, resembles the state of incipient intoxication, so does childhood, with its ready receptivity and want of co-ordinative power, resemble the hypnotic or sleep-waking condition. By hypnotizing the adult we restore to him the trustfulness of childhood, much as by slightly intoxicating the elderly we restore to them (as Plato has it) the vigour and enterprise of youth.

There is, however, a state in which we are even more susceptible to suggestion, have even less of co-ordinating faculty, of resisting power, than either in childhood or in the hypnotic trance. I allude to our condition before we are born—to the period at which the various looms are actually being placed in our manufactory, in rough accordance with the arrangement of similar looms in the factories of ancestral minds. And we most of us know to our cost that although we may improve the working of these looms in detail we have little power to modify their general type, or the principal driving-gear which connects them. And, to take the defect with which we are now concerned, some original weakness in that driving-gear may subject us to hystero-epileptic attacks, with their concomitant hypnotic phenomena.

I have perhaps said enough to explain what I mean by extending the analogy of deferred suggestion to this wide range of hereditary and (so-called) spontaneous manifestations of hypnotic phenomena. The impulse, as I hold, which ultimately induces those phenomena, has been already given to the organism,—either in that organism's first inception, or during the course of life,—in much the same way as the hypnotizer can communicate the impulse to sleep, or to perform some act, at a future moment which he determines by his own choice.

It is probable that many of those who accept this analogy at all may consider that it covers the whole extent of self-suggestive phenomena. They may think that *spontaneous* cases include *voluntary* cases, as a mere sub-division distinguished by the concomitance of a purely subjective sensation of *choice.* They may say that the pre-existent conditions of the organism determine in every case—in the so-called voluntary cases as fully as in the involuntary—the phenomena which that organism proceeds to manifest.

I do not here directly controvert this view; but I cannot help suspecting that there *is* a difference between hypnotic phenomena which occur spontaneously and those which seem self-induced by a direct effort of will;—or at any rate that this second class, in the apparent mode of their operation, afford an analogy with the influence which the "silent will" of an operator can sometimes—as our experiments have gone to show—exert on a sensitive subject.

I am not sure, however, that such words as *will*, *mental effort*, and *attention*, are the right words to use in such a connection. I think that we must not take for granted that this influence is necessarily accompanied with ordinary consciousness on the agent's part. It appears, for example, that a person himself in the sleep-waking state can mesmerise another person,—exerting apparently the same kind of influence, and the same kind of volition in directing it, as a normally-waking man can exert. Yet, in the case to which I specially allude, when the sleep-waker "came to himself" he was quite unconscious that he had mesmerised—and had for a long time refused to demesmerise—the other subject. It would seem then that in considering the genesis of hypnotic phenomena which are apparently *self-induced*, yet not precisely spontaneous, (as hereditary hysteria is spontaneous,)—we shall do well to consider phenomena originated in various nervous conditions besides that of normal wakefulness.

I shall cite three cases. In the first the subject was in a state of general "suspension and inhibition" profounder than any ordinary phase of the hypnotic trance. In the second the subject was already hypnotized. In the third the subject was in the normal state.

The first of these three cases may seem from one point of view to be eminently *in*voluntary,—eminently a case where the previous history of the organism determined the recrudescence of an impressive scene. But—as will be seen—it has some puzzling features. Oddly enough we have here, and here alone, an actual suggestion made in so many words by a man to himself;—and here, too, we have the remarkable production, without external stimulus, of those vaso-motor disturbances which form at present the extreme limit of the hypnotizer's power over the subject's physical organism. Who could expect the somnambule to direct his spontaneous energy to the production of hæmorrhage or vesication? Yet the history of Louis V——, one of the most important documents which Nature has ever submitted to the psychologist, affords an example of self-suggestion pushed to this almost incredible length.

In the first place it must be explained that, according to the testimony of four physicians who have tried experiments with Louis V—— at two separate asylums,[1] hæmorrhage and bleeding stigmata can be evoked in him by *external* suggestion. The first recorded experiment of the kind was as follows. The patient being in the somnambulic state, one of the doctors said to him: "At four o'clock you will fall asleep, come into my study, sit on that chair with your arms crossed, and bleed at the nose." The crossing of the arms was, of course, to prevent his touching his nose. When four o'clock came he fell into the hypnotic trance, went and sat in the chair with his arms crossed, and after a few moments began to bleed at the nose.

Again, the doctor traced the subject's name on his two fore-arms with a blunt instrument, and told him, in the somnambulic state, "At four o'clock you will go to sleep and bleed on the lines which I have traced on your arms."[2] Shortly before four o'clock he was

[1] See Drs. Bourru and Burot, *Comptes Rendus de le Sociétè de biologie*, 12 juillet, 1885, for experiments at Rochefort, and Dr. Mabille, *Progrès Médical*, 29 août, 1885, for experiments at La Rochelle. See also Berjon, *op. cit.*, p. 36, sqq.

[2] The fact that the arm was *touched*, though with a blunt instrument, may suggest that the subsequent redness and even bleeding were the mere effect of

examined, and his arms were then without marks. At four he fell asleep, and on his left (non-paralysed) arm the tracings stood out in red relief, and a few small drops of blood oozed from them. On the right, or paralysed, side, there was nothing apparent.

And this leads us to the phenomenon of self-suggestion on which I wish to dwell. I translate from Dr. Mabille's paper, read at the Congrès scientifique de Grenoble, 1885, and reprinted in the *Progrès Médical*, as already cited. "On August 5, 1885, at my visit, about 8.30 a.m., in presence of Dr. Ramadier, assistant physician of the asylum of Lafond (La Rochelle), and of M. Chauvelot, house-physician, I plunged V—— into somnambulism, and, anxious to check his sleeplessness, I said to him: 'This evening, at eight, you will say to the attendant, Ernest: "Ernest, come and help me to bed, I want to sleep." Then you will go to bed and you will sleep till five in the morning. During your sleep you will hear nothing, see nothing, feel nothing. You understand me, V——?' 'Yes, sir.'

"At about 7.57 p.m., V——, who was walking in the court-yard, stood still with a fixed gaze, underwent some slight convulsions of the face, as is usual with him when the moment fixed for a suggestion draws nigh,

contact. We do not, in fact, know how far vaso-motor reflex excitability may go in the production of phenomena analogous to the so-called "tache cérébrale," or red mark produced by pressure on the skin in many morbid conditions. It would have been more satisfactory had Dr. Mabille, &c., explicitly taken account of this possibility. Nevertheless, it certainly appears :—

1. That Louis V—— was so frequently touched in various ways that any tendency to *tache cérébrale*, or subcutaneous hæmorrhage, must have been observed; and if the marks lasted for *months* (as is recorded of the marks on the arm, Berjon, p. 36), he must have been covered with such marks.
2. That precautions were taken (p. 36) to prevent his touching himself for "some minutes" before the bleeding appeared. The *tache cérébrale*, so far as I know, appears within two or three minutes after the touch, if at all.
3. Professor Beaunis' experiments (*Recherches Expérimentales*, &c., Paris, Baillière, 1886, p. 29) on the production of redness and cutaneous congestion on Mlle. A. E. by suggestion, are confirmatory of Dr. Mabille's. Here also Professor Beaunis does not state that he tried whether other points touched without suggestion would become equally red, no doubt considering that it was obvious that Mlle. A. E., who leads an active life, was not thus affected. Fortunately, on Sept. 3, 1885, I had myself an opportunity of trying the experiment on this same subject. I quote from Mr. Gurney's note made on the same day :—

   "Mlle. A., hypnotized by M. Liébeault in about three seconds, and immediately afterwards *most severely* pinched by E. G. on the arm, without giving the slightest sign of sensation. Liébeault slightly pricked the knuckle of the middle finger of the right hand, and told her that a patch of redness about the size of a 50-centime piece would form there; and also that a similar patch would form on the corresponding and fell into the sleep, or rather into that intermediate state which M. Dumontpallier has described. His hyperæsthesia of the left side disappeared. He repeated to his attendants the words previously cited, and at 8 p.m. precisely slept profoundly. From that moment onwards, while I was unable to wake him, for he saw, heard, felt nothing, and the pressure of his hysterogenous zones had no effect, V—— spontaneously renewed this series of experiments to which he had been previously submitted. Thus, he pressed with his fingers on the balls of his eyes, as if to be thrown into lethargy, opened his eyelids to pass into catalepsy, rubbed the top of his head to pass into somnambulism, and entered on the following dialogue, putting the questions and making the answers himself.

"Q. You hear me? A. Yes, sir.

"Q. Give me your arm. A. Yes, sir.

"Q. V——, a quarter of an hour after you wake there will be a V on your arm at the place which I mark (here he marked a place on his arm), and it will bleed. You understand, I wish it to bleed. A. Yes, sir.

"Q. V——, count to ten and awake at *seven.*

part of the other hand. The other hand was not pricked or touched in any way. F. W. H. M. also gave her a scratch on the arm, in order to see whether this would also become red after a short interval. She was then woke, and in about 15 minutes the patches had formed as predicted; that on the left hand being a little less red and distinct than that on the right. Half an hour after this both marks had completely disappeared. There was no redness where the scratch had been given. Mlle. A.'s hands were not under the strictest scrutiny throughout, but she was close to us, and talking during the whole time that elapsed between her waking and observation of the patches, and anything like continuous rubbing must, we think, have been noticed. M. Liébeault has complete confidence in her integrity, and all his experience of her goes to show that she retains no memory on waking of what has passed during her hypnotic sleep."

The following rules, I think, should be observed in experiments of this kind:—

1. Before asserting that a result is obtained by *suggestion alone*, repeat the experiment on the same subject with all the other circumstances, but without the suggestion.
2. Before asserting that a result is due to a *particular process alone* (as shutting or opening the subject's eyes, rubbing the top of the head, or special points on the head, &c.), let that process be repeated on a subject who does not know what to expect by an operator who has no theory on the subject whatever.

The rigid application of these rules might, I fear, reduce certain well-rounded theories to a somewhat lean and scrappy condition.

Professor Delbœuf (*Revue Philosophique*, August, 1886) has amusingly shown how readily one subject will imitate while entranced the hypnotic phenomena which he has observed in another.

"V—— counts 1, 2, 3, 4, 5, 6, 7, seems to wake from his sleep; then continues to count 8, 9, 10 and stops. Loud snores then indicate that he is asleep. Then, about a quarter of an hour after this dialogue V—— was seized with the crisis which we usually observe in him when the stigmata have been suggested to him. At the end of this crisis we examined his arm and we saw a V, and the V was covered with blood. This bloody effusion was produced at a place where a V had been suggested by me on August 3, in the presence of Drs. Barth and Delarue, of La Rochelle, according to the method of Drs. Bourru and Burot.

"The same phenomena were twice produced during the same night, in the same place, and by the same procedure. V—— then awoke exactly at 5 a.m., without knowing that he had been asleep, and with the conviction that he had just been picking flowers in the garden of the asylum. We have here, then, a hæmorrhage produced during induced somnambulism, without any external agency, in the place of the pre-existing stigmata, by what I think I may call *auto-suggestion*. And this auto-suggestion (as well as all the other phenomena which I observed during the night of August 5-6 in the presence of Dr. Ramadier and M. Chauvelot) was of *cortical origin*, since the initiation of peripheral impressions was for the time suppressed. It was like the awakening and exteriorisation of sensations already stored up in the organism."

Here at last, I may observe incidentally, is the true explanatory parallel to the case of Louise Lateau. Here is a case where there is no pretence of miracle and no possibility of fraud; a case where the very mechanism of stigmatisation is laid bare from beginning to end, and the asylum-patient retraces the doctor's visit with the same reality of starting drops of blood with which the pious ecstatic renews the story of the Crucifixion.[1]

Passing from this exceptional state of profound entrancement to the commoner phases of hypnotism, I will next cite a remarkable case

[1] The difficulty of keeping abreast of modern experiment,—and the danger of confident negations,—are illustrated by the following passage from a little book of Dr. Maudsley's, entitled *Natural Causes and Supernatural Seemings*, (London, 1886).

"There is not on record," he says (p. 261), "a single well-authenticated case, nor is there any sound argument to justify the preposterous opinion, which has been broached by some quasi-scientific authors, that these stigmatic bleedings might be produced naturally by the exceeding and specific intensity of the imagination acting upon the particular areas affected. The supposition that the zinc [in a well-known case of fraud] was perforated by the intensity of the imagination, would be scarcely less preposterous." Whether an idea is "preposterous" or not is a subjective question, and depends on what goes before and what behind it in the speaker's mind. But "quasi-scientific" is a more objective term; and Dr. Maudsley's use of it here seems to prove that he has not had time to acquaint himself with the experiments of Drs. Bourru and Burot, nor with those of Professor Beaunis, contained in the *Recherches Expérimentales* of Dr. Pitres'[1], where the subject, in the somnambulic state, was able to suggest to herself that it was impossible to awaken her.

Dr. Pitres' subject, "Albertine," (already mentioned,) is liable to accesses of hysterical aphasia, which she greatly dislikes. One day Dr. Pitres suggested to her, in the hypnotic trance, that she would be unable to speak when she awoke, and would continue aphasic for 24 hours. The suggestion succeeded; all that day she could not speak. Some time afterwards Dr. Pitres again hypnotized her, and made the same suggestion. But she rebelled, and said that if he persisted in giving her this order she would not let herself be awakened. He did not believe that she could prevent him from waking her, and blew on her eyes and tried other accustomed means. Nothing would wake her, and he was obliged at last to promise that she should only be aphasic for five minutes. Then he woke her without difficulty. The experiment was repeated many times.

Here, then, we have a well-marked case of self-suggestion in the hypnotic trance itself. M. Pitres does not state whether Albertine can hypnotize herself, by a mere effort of will, in the waking state. From the account given, it rather appears that the trance itself has facilitated the self-suggestion,—that the same condition of supernormal susceptibility which renders her subject to the commands of her hypnotizer renders her subject also to a command of her own. And I would point out that this accords with my hypothesis that in self-suggestion the subject may be exercising on himself, from within, a force or influence truly analogous to the force or influence which the hypnotizer exercises on him from without.

Passing on, again, from the self-suggestion of the subject already hypnotized to the self-suggestion which throws the waking subject into trance, we find ourselves on a path of "yogism" and mysticism which would lead us far from the present discussion. I shall cite one case only, where the act of will is strongly marked,—the waking choice even determining the hallucinations which are to adorn the self-induced trance.

Dr. Liébeault[2] was acquainted with a "somnambule naïf,"—a deaf-mute of the name of Loué, who apparently, even before the scene which I am about to relate, was able to entrance himself by an effort of will. Dr. Liébeault told this man that the dead could appear to the living, and that, if he concentrated his mind on his father, his deceased

already cited. Applied to Drs. Bourru and Burot, such an epithet would be unwarrantably discourteous. Applied by Dr. Maudsley to Professor Beaunis, it would provoke a smile. But I repeat that it would be unjust to charge any such intention on Dr. Maudsley,—who wrote, it is clear, in mere ignorance of the recorded facts.

[1] Pitres, *Des suggestions hypnotiques*, Bordeaux, 1884, p. 54.

[2] *Du sommeil et des états analogues*, pp. 259, 282.

father would probably appear, and that he (Loué) would remember the interview. "Le somnambule," says Dr. Liébeault, "se mit aussitôt à baisser la tête ; sa respiration devint bruyante, sa figure prit une expression sérieuse, et au bout de quelques minutes il se leva, l'œil fixe, et se dirigea vers la porte de l'appartement. Les témoins de son rêve et moi nous le vîmes tendre la main, déposer un baiser dans le vide,'—and, in fact, he went through the scene of an interview with the phantom of his evocation.[1] From that date he frequently repeated the process, firmly believing that it was veritably the dead who came. He profited also by his new-found power to procure himself imaginary interviews with living personages of a less sacred character. These interviews, though they also seemed real at the time, he always knew to have been mere hallucinations. But as regards the interviews with the dead, the kindly doctor, kinder than those unwelcome friends of Horace's *halluciné*,—allowed the poor man to rest in a belief whose destruction would have robbed him of his most valued joy.

'Pol me occidistis, amici,
Non servastis,' ait, 'cui sic extorta voluptas
Et demptus per vim mentis gratissimus error.'

Cases like this,—where choice and effort seem so deeply engaged, do certainly take us far away from the mere 'attaques de sommeil,' for instance, of the congenitally-unstable Albertine. They bring us face to face with the question : Is this *will* or *attention* of the subject's, which he uses to induce the trance, a new force introduced amongst the forces of his organism from some source independent of or pre-existent to that organism? This, of course, leads us at once to the old problem as to the existence or non-existence of anything which may be properly termed a *soul*, independent of the known physical organism.

But, nevertheless, without directly grappling with such a problem as this, we can, I think, discern an instructive difference between this direct self-suggestion and the mere accidental stirring or gradual maturation of external excitations received in the past.

[1] A parallel instance of self-suggestion will be found in the *Archivio Italiano per le Malatie Nervose* for 1883, Part 4: thus summarized in *Ann. Méd.-Psych.*, 1884, Vol. ii., p. 467. "Une fillette de dix-sept ans, à demi idiote, est, quelques heures avant la mort de son père prise d'un sommeil magnétique ; hallucinations célestes (?). On arrive plus tard à 'lhypnotiser ; les mêmes scènes se reproduisent, mais on cesse les expériences, la malade ayant tenté à la suite de l'une d'elles à se suicider. Actuellement cette jeune fille se met elle-même en état d'hypnotisme, et s'en tire par les méthodes connues qu'elle pratique spontanément, tant l'extase lui est devenue agréable. On est obligé de l'en déshabituer."

Dr. Charles Despine's patient, Estelle L——, could also induce and modify trance at will.—*Observations de Médecine Pratique*, p. 62, &c. (Anneci, 1838.).

First, let us suppose that when Dr. Liébeault's deaf-mute Loué, for instance, throws himself into a trance by an effort of will, without any mechanical stimulus, that effort is really a new force, cognisable by us only on the psychical side, and imported into the sum of forces previously manifest. In that case the new force will be a vital force exercised by himself on himself; and its nearest apparent analogy will be to the assumed vital force which Dr. Liébeault can exercise on him.

Next, let us suppose that he does *not*, by this act of will, import any fresh force from an unknown plane of being. Nevertheless he effects, by unknown means, some kind of change in a previously-existing vital equilibrium. He transforms some kind of latent capacity into a force which, after intermediate steps unknown to us, ends by throwing him into that special condition into which the "vital influence" of an external operator can also throw him. May we not, then, here again conjecture, as the simplest way of correlating the two phenomena, that the self-suggestion which sends the subject into sudden trance, at the bidding of his mere caprice of the moment, may involve some disengagement within him of the same force which, when exercised upon him from without, is that very "vital influence" or "effluence," in whose real existence we have independent grounds for believing?

A few more words may make my meaning clearer. In most cases of successful hypnotism there is some amount of voluntary attention on the subject's part. He co-operates with the operator by "fixing his mind" on the process; and, in fact, some theorists have thought that his fixation of his mind was the sole cause of the effects which follow. But what makes him fix his mind? He *chooses* to do so; that is all we can say. And it is this apparent act of choice, this voluntary direction of the attention to a particular idea, which is the stronghold of those who maintain that there are mental processes within us which are not inevitably determined by physical antecedents. I am not arguing either for or against this view; I am only reminding the reader that this voluntary attention of the subject's is something separate from the mere reflex psychical response to the operator's suggestion, and that it is an element which enters in varying degrees into all inductions of hypnotic trance, except those effected by sudden shocks, or upon sleeping, unconscious, infant, or insane persons or animals. And the question is whether we can get a little deeper than this mere statement, whether we can detect an analogy between this idiosyncratic impulse, this "*réaction personnelle,*" and any hypnogenous agency which a man can exert upon others than himself.

And to this question I tentatively reply that perhaps when I *attend* to a thing, or *will* a thing, I am directing upon my own nervous system actually that same force which, when I direct it on another man's nervous system, is the "vital influence" of mesmerists, or the "tele pathic impact" of which Mr. Gurney and I have said so much. And when I say "directing the force on my own nervous system," I am not speaking of myself as a kind of *angelus rector* without and above my organism. How the force is generated is an open question in the case of the suggestion which I exert *within* myself as in the case of that which I exert *without* myself. But whether the essence of those acts of will, or of attention, be an illation and infusion of forces which do *not* pre-exist in my organism, or a concentration or conversion of forces which *do* so pre-exist, I urge that that essential element may be the same in the one case as in the other.

§ 17. Whatever, then, the precise explanation of this form of self-suggestion may be, it forms a step between mere mechanical hypnogeny and that "vital influence," which in its turn serves as the starting point for so many fresh perplexities.

For no sooner do we fix our attention on what we have defined as the *third class* of suggestions—suggestions made *mentally* by persons in the presence or proximity of the subject—than we recognise that the theory of vital influence, already assailed on one side by the advocates of a merely *mechanical* hypnogeny, is assailable also, on the side *further* from ordinary experience, by anyone who should choose to maintain that the true agency which travels from man to man is a cause unconnected with corporeal proximity—that is to say, that it is never the "pass," or the gaze of the eyes, or the touch of the fingers which sends the subject into trance, but always an agency of that unknown "psychical" kind which our evidence shows sometimes to accompany the exercise of intense *thought* or *will*.

The cases which bring this question into prominence are especially those where the influence of the mesmerist (as in Dr. Esdaile's mesmerisation of a blind man) has been exercised from the distance of some yards, by gazing steadily at the subject.

Are we to suppose that there was here a real ὀμμάτων ἀπορροὴ—a veritable efflux of nervous energy from Dr. Esdaile's eyes, which impinged in some physical manner on the blind and unsuspecting subject? Or was the transfer of a purely telepathic kind? and was the direction of Esdaile's eyes on the subject a mere aid to concentration of thought or will?

Mesmer would have said that a real efflux from the eyes was here the efficient cause. The writers in the *Zoist* (including Esdaile himself), so far as they faced the problem distinctly, would have made the same reply. And recently Dr. Baréty (*Force Neurique Rayonnante*, 1882) has attempted to show that actual "neuric rays" are emitted by eyes and fingers, which are susceptible of reflection from mirrors, concentration by lenses, &c. "Nous croyons," he says (p. 37), "qu'il n'y a aucune témérité à conclure . . que la force neurique . . n'est qu'un mode particulier de mouvement . qu'elle agit par l'intermédiaire de cette matière subtile . que les physiciens appellent *éther*."

I think that there *is* temerity in such a conclusion, and I do not like to see the *ether*—the *deus ex machinâ* of a certain school of modern theorists—dragged in, if I may so say, by the head and shoulders, to explain anything, whether physical or psychical, which particularly puzzles us. Yet I think that Dr. Barêty's experiments should be repeated, and I am quite sure that we can have no *à priori* certainty that such rays as he alleges do not exist.

On the other hand Professor Janet's experiments with Madame B. (and many older experiments,) seem to show that the action of thought or wish may overbear the influence of proximity, even when operator and subject are sitting side by side.

"Cette influence de la pensée de l'opérateur," he says,[1] "quelque extraordinaire que cela paraisse, est ici tout à fait prépondérante, à un tel point qu'elle peut remplacer toutes les autres. Si on presse la main de Madame B. sans songer à l'endormir, on n'arrive pas à provoquer le sommeil; au contraire, si l'on songe à l'endormir sans lui presser la main, on y réussit parfaitement."

Professor Janet is, of course, alive to the danger of mere *suggestion* in such a case; and (as already mentioned) he goes on to say that he thinks that this supposition is met in several ways, especially by the fact that in sleep thus induced by *will*, Madame B. was under the influence of the person who had really willed her from the next room—not of the person who had been placed near her, and to whom, if mere *suggestion* induced the sleep, that suggestion might naturally have pointed.

Again, in the experiments in which we obtained localised anæsthesia, of a single finger only, and in a manner which seemed to exclude both monotonous stimulation and expectant attention as operating causes,[2] it nevertheless appeared that it was necessary for Mr. Smith to concentrate his attention on the subject's finger, as well as to point his own fingers towards it. But the concentration of attention did not suffice without the physical approach of the fingers as well.

The approach of Mr. Smith's fingers, it will be remembered, was concealed from any ordinary cognizance of the subject's senses. But before we can be sure of a specific vital influence we have to guard ourselves against possible *hyperæsthesia* of the ordinary senses;—and this is difficult to do while the operator, however hidden, is himself present with his subject. We must desire cases where his influence shall in

[1] *Bulletins de la Société de Psychologie Physiologique*, Vol. i., p. 27. (Paris, Alcan.)

[2] See *Proceedings*, S.P.R., Vols. i., iii., reff. in Index.

some way persist after his own removal, or become embodied in some substance otherwise inert.

§ 18. The most direct way, therefore, of testing the reality of "vital effluence" would certainly seem to be the so-called "mesmerisation of objects." Personally, I have witnessed very few experiments of this kind which led to any positive result;[1] but (as Mr. Gurney and I have elsewhere insisted,) the testimony of Esdaile and others in the matter is too strong to be set aside without diligent attempts to imitate their proceedings. Yet even here new ambiguities present themselves as our knowledge of *deferred* and of *telepathic* suggestion increases.

Professor Beaunis, of Nancy,[2] gave some counters to Mlle. A. (already often mentioned) in her waking state, and said: "If you at any time put one of these counters into a glass of *eau sucrée* and drink it, you will go to sleep." Mlle. A. forgot the liquid thus ordered. She tried wine, water, wine and water, in vain; at last she tried putting the counter into a glass of *eau sucrée*, drank it, and went to sleep at once. The suggestion, that is to say, which had disappeared from conscious memory was still operative below the threshold of consciousness. But if a case like this had occurred before the efficacy of deferred suggestions was understood, it would probably have been thought by the mesmerists of the day that the counter had been mesmerised by Professor Beaunis' contact, and that the *eau sucrée*, for some inscrutable reason, was a liquid peculiarly qualified to draw that counter's mesmeric virtues out.

So much for the possible confusion between *deferred suggestion* and *vital effluence.* And if we introduce the hypothesis of mental or *telepathic suggestion*, such as Dr. Gibert's mental suggestion to Madame B., that she should look at a certain book at a certain hour, a fresh source of ambiguity is introduced. Suppose that Professor Beaunis had *mentally suggested* to Mlle. A. that when she put the counter into *eau sucrée* and drank it she would go to sleep; and that this suggested result had actually occurred. It would then look precisely as though the *eau sucrée* had undergone some mysterious change; whereas the only change would have been that telepathically impressed on Mlle. A.'s mind by the mental suggestion previously made.

It would, of course, be easy so to arrange the experiment as to avoid possibilities of this kind; but it is important that they should be recognised; for experimenters who have been fortunate enough to encounter the rarer phenomena of mesmerism, have often overlooked the precautions which are needful if any conclusive proof of one or other hypnogenous agency is to be acquired.

§ 19. It is plain that we have here arrived at one of the hardest

[1] See *Proceedings*, S.P.R., Vol. i., reff. in Index.

[2] *Recherches Expérimentales*, ii., p. 89. (Paris, Baillière, 1886.)

knots in the whole inquiry. It resolves itself into this: Is there one and only one form of influence which is not communicated by the ordinary channels of sense? Or is there an influence which is felt by a percipient only when the agent is very near him? and another influence which the percipient feels equally when the agent is at an indefinite distance? Readers of *Phantasms of the Living* will see that this problem—there dealt with on its psychical side alone—presents itself in an urgent manner when we attempt to establish an analogy between our experimental results in thought-transference and those spontaneously-arising impressions or apparitions which sometimes coincide with the death or crisis of a distant friend. Our parallel halts in so far that we have not yet succeeded in experimentally obtaining (in the sense of the direct communication of a thought, image, or sensation from one person to another,) thought-transference otherwise than between persons in close proximity. In *Phantasms of the Living* this difficulty has been pointed out, but no complete solution has been attempted.[1] Speaking for myself only, I am inclined provisionally to accept the hypothesis that more than one form of force, or at least more than one form of receptivity, is concerned in the phenomena.

The action of medicaments at a distance—at a distance measured by inches—on Louis V—— and others, has led MM. Bourru and Burot to suggest, in a tentative manner, that there may be a zone, immediately surrounding the person of a hyperæsthetic patient, to which his hyperæsthesia extends; so that certain objects, when placed within that zone, exercise a direct effect on his nervous system.

Hypotheses somewhat similar have been suggested by various other experiments, and analogy seems to me to point (though not decisively) in this direction. On the other hand one obvious objection to the view, namely the complexity which it introduces into the conception of telepathic action, does not seem to me to be important. We have given the name telepathy to a mass of phenomena which have in common only the fact that they involve transmission of thoughts or feelings from one mind to another, without the agency of the recognised organs of sense. But it would be rash to go beyond this, and to assume that we have at once lit on a single, simply-acting force or energy. So long as we can detect forces only by their influence on *ourselves*, not on registering instruments, we are likely not only to ignore what may be the most characteristic action of each force, but to confuse together disparate forces which exert on *us* something of the same effect.

§ 20. But here, I repeat, we come to one of the most difficult points in the whole inquiry. What is the relation between the supersensory transmission of thoughts and feelings in close proximity, and a similar

[1] See Vol. i., pp. 96-97. Note the case of Mr. S. H. B.

transmission between persons separated by the whole diameter of the planet?

In order to answer this question we ought to be able to compare very many cases of telepathy at varying distances. *Phantasms of the Living* contains, I think, almost all the cases on which reliance can be placed, and they are not yet enough to admit of an assured comparison between the phenomena which occur (1) between persons in contact; (2) between persons in the same room; (3) between persons in adjoining rooms; (4) between persons at a distance of less than a mile; (5) between persons many miles distant from one another. We do not know where the breaks, if any, occur in this chain; we do not know what is the effect, on the one hand, of a material obstacle such as a wall, or, on the other hand, of what I may call a psychical *rapprochement*, as, for instance, a previous familiarity on the agent's part with the scene where the percipient sits. But (speaking in a guarded and provisional manner) we may note the following points:—

(1) Contact seems generally to facilitate transmission of thought and feeling, and the induction of the hypnotic trance.[1]

(2) Presence in the same room seems to be essential to most of our definite experiments in thought-transference, and to the induction (in the first instance) of the hypnotic trance.

(3) Presence in an adjoining room has occasionally sufficed for a direct experiment in thought-transference; and occasionally for the reproduction of the hypnotic trance when it has been previously induced by the same operator.

(4) *Perhaps* hypnotisation at a distance is easier when the distance is (say) of one mile rather than ten. Perhaps, too, there is a certain difference in the quality of the subject's sensations when the mesmeriser is at a distance. Thus Mme. B. (in this resembling one at least of Mr. H. S. Thompson's subjects[2]) always professed, not only to feel who it was that was thus influencing her, but to have a vague sense as of that operator's *presence* in the room. This I heard her say repeatedly in the hypnotic trance (in which she seems always to tell the truth as to her own sensations), nor has she, as I understand, been ever mistaken as to whether it was M. Gibert or M. Janet who had sent her to sleep from a distance.

(5) Lastly, when the telepathic influence is exerted over long distances we have very much less of direct experiment, but

[1] I need hardly repeat here that cases in which contact is permitted are not in themselves a proof of anything beyond unconscious muscular pressure, if the image or action be such as any form of pressure can suggest.

[2] *Phantasms of the Living*, Vol. i., page 99, and see note.

very much more of spontaneous apparition. Our distant cases include, as we have often pointed out, all varieties of objectivation, from the mere sense of presence, or the "mind's-eye" view, to the actual phantom in apparent bodily form. But though these distant cases offer many phenomena not hitherto observed in experimental or contiguous cases, they present also many points of contact. Sometimes, for instance, it seems to be the *will* of the distant agent which impresses his phantasm on the percipient—sometimes the death of a distant friend seems to produce a quasi-hypnotic effect on the percipient, which, in one case, at least, seems to have amounted to a sort of agitated trance.[1]

Now there is, of course, a temptation to simplify the problem by assuming that in all these cases of supersensory transmission a force is acting which in the first place is identical, cognate, or correlated with known forces, and in the second place is the same for all supersensory action whether in contiguity or at great distances. Such expressions as "brain-waves" (Knowles), "mentiferous ether" (Maudsley), "force neurique rayonnante" (Baréty), "ondulationnisme" (Perronet)—to which many others will doubtless ere long be added—testify to this natural, though premature, desire to ticket or identify a force which (in the opinion at least, as I think I may say, of those who have expended most pains on tracing its effects) cannot at present be correlated with nerve-force, or with magnetism, or with ethereal vibrations of any kind, by any true physical demonstration.

And here, again, there is a temptation to the attainment of something of apparent simplicity by a just opposite road. We may say that telepathy is a *psychical* agency, and that there is an impassable gulf between all agencies which can be classed as *physical* on the one hand, and on the other hand, all agencies, whether apparently operating in proximity or at a distance, which we can as yet cognise on the psychical side alone.

But this view seems to me to involve a metaphysical, as those other views involved a physical assumption. I do not like to assume that any effect perceptible to human senses is without a physical cause of some kind—a cause, that is to say, which intelligences of adequate elevation could cognise objectively and deal with mathematically, as we deal with those forms of matter and force which our minds can at present embrace. Such physical cause or basis may no doubt be so remote from our ordinary physical conceptions that the philosopher may be justified in leaving it altogether out of the question, and in dealing with the interrelations of thought and emotion exclusively on the

[1] *Phantasms of the Living*, Vol. ii., p. 42.

psychical side; but it seems to me that telepathy forces us into a position where it is no longer safe to assume any sharply-defined distinction of mind and matter,—where we must rid ourselves of every metaphysical preconception and look to experiment and observation alone.

We must remember that it is only quite recently that we have frankly accepted a physical basis or concomitant for all the operations which go on within our own minds. It was in this century that Lord Jeffrey maintained that "there is not the smallest reason for supposing that the mind ever operates through the agency of any material organs, except in its perception of material objects, or in the spontaneous movements of the body which it inhabits." And even Mill, as is well known, regarded the concomitance of a neural change with *all* mental changes as an open question. We are now pretty well agreed that such concomitance does always and inevitably subsist within us; but we still speak of the interaction of thought and emotion—of the "world of mind"—as of a realm, or of operations, where no physical basis must be assumed. I think it possible that the facts of telepathy may compel us to extend our conceptions of physico-psychical concomitance, and to face the supposition that though forces may exist, and agencies operate, which the ordinary materialistic view altogether denies, yet these also may be correlated—though above the limit of our intelligence—with the force and matter with which our mathematical science already deals.

It will, of course, be apparent from the line of argument here adopted that I do not consider that the problem of the relation of *near* to *distant* supersensory transference admits at present of even approximate solution. Our recorded instances must receive many in addition, —nay, our notions of matter and of mind must pass through many a phase as yet unimagined,—before we can tell in what degree the mesmerist's gaze across the room resembles that strange and mighty impulse which carries the dying father's image across seas and continents to his unexpectant child. All that I have suggested is that there is a presumption in favour of *some* connection, *some* continuity; that the mingled similarity and dissimilarity of the phenomena at differing distances is such as may lead us to conjecture the joint action of cognate causes in varying proportion—of causes cognate but not identical, implying no single capacity of percipience, no single energy of communication.

§ 21. I can but note these points, on which fuller knowledge may come in time. For the present it must suffice to have endeavoured, in the *first* place, to supply additional evidence of the existence of telepathic hypnotism, or hypnotic suggestion at a distance; and, in the *second* place, to trace a kind of series or gradation—often interrupted, indeed, and largely hypothetical—between the simplest and commonest, and the rarest and strangest modes of hypnotic influence.

Some of the hypotheses which I have thus advanced may be *erroneous*, but they cannot justly be said to be *gratuitous*. For they are advanced in order to correlate certain actually-observed phenomena, which must bear *some* relation to each other, and whose relation can only be properly discussed if some tolerably clear scheme is set forth as a basis for such discussion. I have cast, therefore, the suggestions of the preceding pages into a tabular form.

There are, indeed, several items in this scheme as to which the evidence seems to me at present inadequate. But I include these phenomena simply because the authority of competent physicians on the whole supports them. And by competent physicians I mean not men who, though competent in other lines, have made few or no hypnotic experiments, but men who having shown medical or physiological ability of a more general kind, have also taken real pains to experiment for themselves in this special direction. I am sorry to quote so small a proportion of English names; but the fact is that during the last ten years this inquiry has been very eagerly pursued by French *savants*, with some success also by Germans and Italians, but by Englishmen hardly at all. I have no controversial aim; and I desire to see the experiments of the French schools repeated and analysed by as many English men of science as possible.[1] But until such experiments are actually made, and reason shown for explaining the results otherwise than as the school of the Salpêtrière (for instance) explains them, I feel bound to disregard the mere *à priori* negations of men who have done little work of the kind themselves, and are not always familiar with the work done elsewhere. The difference in this matter between England and France is one which a few years will probably do much to remove. In England a practising physician is even now half ashamed of knowing much about hypnotism; the savour of charlatanism still hangs about the topic; and if he writes a book in which he has to allude to it, he shows more anxiety to disclaim error than to discover truth. Much the same was the case in France, till Charcot, Richet, Beaunis, and a few other well-known men took the subject up,—and in Germany, till Heidenhain took it up. The fashion has now changed, and the danger in France is now rather of over-eagerness to register new extensions of the fascinating inquiry. Under such circumstances, it seems plainly the duty of an English student to set forth the outlines of the subject as it stands at present, not vouching hastily for results as yet unconfirmed, but illustrating them whenever he can by some observation or reflection of his own. Such is the course which I have tried to follow; and the more pains which my

[1] The schools of Nancy, Bordeaux, the Salpêtrière, and the Pitié are on many points strongly opposed to each other. I consider such opposition as a necessary and advantageous characteristic of this stage of the inquiry.

instructed readers may take to demolish my "provisional scheme," the more light will there be thrown on a subject which greatly needs it.

§ 22. Provisional Scheme of Modes of Induction of Hypnotic Phenomena.

I. *Massive nervous stimulation.*	Sudden danger; cataplexy of insects; fascination of birds, &c. Sudden noise or light; esp. with hystero-epileptics. Sudden grief: "stupor attonitus."
II. *Monotonous stimulation.*	Tactile; fanning and perhaps manual passes. Auditory; tick of watch, &c. Visual; prolonged gaze on bright object; or human gaze.
III. *Localised stimulation; i.e.,* stimulation of ordinary kinds, but applied to abnormally sensitive points.	Mere touch on unaccustomed parts; holding heads of hens, &c. Braid's upward and inward squint (a kind of strain, or intra-orbital pressure.[1]) Pressure on hypnogenous points or zones, esp. in hystero-epileptics.
IV. *Verbal suggestion; i.e.*, localised stimulation of special tracts of a brain rendered previously sensitive as a whole, or locally.	Command of operator, in waking-life, in trance, or in "veille somnambulique." Deferred commands: (suggestions à longue échéance).
V. *Specialised stimulation; i.e,* from inanimate substances, in contact, but not normally active in this manner; and from non-vital forces.	Medicamentous substances in contact. Metals in contact. Magnets. Electricity: (unless the rare hypnogenous effect of an electric shock falls under Class I.).
VI. *Self-suggestion, i.e.,* determination by causes inherent in the organism.	Hereditary hysteria, &c., as it were the deferred suggestion of ancestors. Idiopathic somnambulism. Self-induced or self-prolonged trance.
VII. *Specialised stimulation from inanimate substances in proximity.*	Experiments of MM. Bourru, Burot, Mabille, Richet. Medicaments and metals without contact.
VIII. *Vital or mesmeric stimulation.*	Touch, of all or certain persons, not necessarily on hypnogenous zones.[2] Passes without contact, or mere proximity or human gaze.[2] Mesmerised objects.
IX. *Mental suggestion without contact, or telepathy; perhaps in more than one form.*	Exercise of will without touch or gaze, in the same or adjoining room, or at longer distances. Tendency to feel presence and to see phantasm of distant operator; link with quasi-percepts telepathically originated by death or crisis of distant agent.

[1] This must be *prolonged*, so comes also under Class II.

[2] Both these methods appear also in Class II.

I must once more remind the reader that the above scheme is expressly intended to be *criticised*, not to be accepted as definitive. Truth, as we know, emerges more readily from error than from confusion, and it may be useful thus to substitute even the rudest ground-plan for the unmapped wilderness of modern hypnotic experiment. All the phenomena which I cite are phenomena which have received strong attestation. If they do not exist, let them be disproved in detail. If they do exist they must bear some relation to each other; they must be capable of some sort of logical arrangement. I shall be grateful for any hints which may enable me to improve this first rough sketch. In the meantime I may at least hope that the "*sommeil à distance*," with which this paper primarily deals, no longer appeals to the reader as an entirely *isolated* hypnotic marvel. It has been connected—by questionable and imperfect links, it may be—but still connected with a great number of other scattered hypnotic experiments, which were themselves, when taken alone, scarcely less strange and incomprehensible.

§ 23. Before concluding this paper I must briefly describe certain other phenomena which I witnessed in the case of Madame B. Besides the fact of their occurrence in the same subject they have thus much of connection with the topics on which I have so far dwelt, that they tend to show how premature at present is any attempt to define the limit of hypnotic phenomena. For the attempt to restrict the nervous changes induced to catalepsy, lethargy, and somnambulism, is, as I hold, equally misleading with the attempt to restrict the methods of hypnogeny to monotonous stimulation and sudden shock.

Madame B. exhibits a phenomenon which is curious enough, even if it be a mere reflection from the mesmeriser's expectation; while if (as M. Janet holds) it is entirely spontaneous, the interest attaching to it is very great indeed. When hypnotised she falls, like most subjects, into a deep lethargic sleep, but, even if left undisturbed, she does not remain long in that condition. She passes through a kind of circular series of changes, sometimes more and sometimes less distinct, which bring her back again into lethargy, only to renew the series once more.

M. Janet has since described these phenomena at length in the *Revue Scientifique*, May, 1886.[1] I will not attempt to reproduce his article, but will give, in a slightly expanded form, the notes taken at the time by Dr. Myers and myself, adopting M. Janet's schematism as our basis. The remarks within square brackets show the degree of confirmation which each point received in our presence. Her first state is—

1. *Léthargie* (*vraie*).

Sight, hearing, and sense of pain absent.

[1] See also M. Richet in *Revue Scientifique*, June 12.

[This appeared to be so, but I did not think it needful to ask for severe tests of anæsthesia.]

Firm pressure on different parts of arm induces contraction of muscles of hand and arm, removed by friction. [Yes.]

If a magnet is applied (say) to contracted left arm, contraction is transferred to right side; and this dextral contraction can then only be removed by stroking the *left* arm, viz., the arm originally contracted. [Yes, but this variety of the familiar experiments of Charcot, &c., in transference of hemianæsthesia, is of course liable to the usual suspicion of being a mere result of *suggestion.*] In one experiment of this kind in our presence the little finger of one contracted hand was not relaxed by the magnet; and, correspondingly, the little finger of the hand to which the contraction was transferred was not stiffened.

2. *Léthargie cataleptique.* Persistence of attitudes impressed from without, and slight contracture of muscle on deep pressure. [This state resembles spontaneous idiopathic catalepsy more closely than do the next two.]

3. *Catalepsie léthargique.* Persistence of *movements* externally initiated. [Simple movements of arm persisted; not so a less familiar movement of foot. I doubt whether this stage can be fitly classed separately.]

4. *Catalepsie, (vraie, ou catalepsie de Charcot.)* Eyes fully open and staring: suggestion of some familiar action (as folding of hands in prayer, raising of finger in command) carried out automatically through various stages, habitual behaviour in church, &c. In this state she was subject to the "prise du regard," *i.e.*, if M. Janet looked at her steadily, and then moved his head, she followed with her eyes. She did not follow the eyes of other persons. But M. Janet believes that if he touches a neutral person,—or if he touches A and A touches B, and B touches C, and C gazes on the subject, then the subject's eyes follow C.'s so long as M. Janet is touching A. [We tried this repeatedly, but the result was doubtful—the subject could not be kept long enough in the required state to eliminate chance.]

5. *Catalepsie somnambulique.* Susceptibility to hallucinations, (subject agitated when told that there are parrots before her), but no power of speech. [Another state as to whose separable individuality I feel much doubt.]

6. *Somnambulisme cataleptique.* Susceptibility to hallucination. M. Janet holds her hand and tells her that there are birds; she calls "Petit! petit!" Here again M. Janet believes that she only sees the birds when he holds her hand, or forms one of a chain. [The hallucinations provoked were so slight that I could not be sure of this. The word "cataleptique" hardly seems in place here.]

7. *Somnambulisme (lucide).* In this stage her eyes are shut, but her

demeanour is alert and lively. She talks spontaneously, exhibiting a childlike character and freedom from shyness not seen in her normal state, and contrasting oddly with the patient, stolid cast of her features.

8. *Somnambulisme léthargique.* In this state contracture is evoked by superficial friction. Suggestions are still possible, but there is apparent sleep instead of apparent waking.

9. *Léthargie somnambulique.* In this state she dreams aloud; imagines that she visits distant places, &c.; and M. Janet loses control over her. From this condition she relapses into the "true lethargy" with which we began.

§ 24. The order in which I have given these states is that in which we saw them follow each other. I perceive that M. Janet in his article (*Revue Scientifique*) gives them in the opposite order; and says, "the pressure of the thumb makes the subject pass through all the states, proceeding from lethargy to catalepsy, while blowing on the eyes makes her pass through them in the direction of catalepsy to lethargy."

Such a distinction as that, I am inclined to think, must be in great measure accidental; some slight association, once set up, probably tends to repeat itself more and more readily. And with regard to all these variations, it certainly seemed to Dr. Myers and myself that they must not be insisted on in detail; that the only fact of clear importance was the subject's tendency to pass spontaneously through a cycle of nervous changes, which was no sooner ended than it began again. A similar tendency to recurrent states has been observed and systematised by Charcot and P. Richer among the hystero-epileptics at the Salpêtrière.[1]

§ 25. M. Janet concludes his description of Madame B.'s phases by some reflections which I will here summarise. "Some observers accord great importance to the phases of hypnotism, and consider them as states entirely distinct; others see in them only insignificant phenomena produced artificially by the operator. Madame B.'s case shows us that the three primitive states (*i.e.*, lethargy, catalepsy, somnambulism) are not of fundamental importance, since other states can be induced, in number as yet undetermined, which are equally definite and durable. On the other hand, I cannot regard these phases as mere accidents; they arise too naturally and recur too regularly for such a supposition. I consider them as stages of sleep *(degrés de sommeil)* through which the subject passes in going to sleep and waking up,—stages at which it is sometimes possible to arrest her. At each different stage, perhaps, different parts of the brain are excited or paralyzed. During lethargy the brain seems entirely paralyzed, and the contractures of this period seem to be mere exaggerated spinal reflex movements; then the different cerebral centres seem to awake gradually

[1] *La Grande Hystérie*, Dr. Paul Richer, second edition, Paris, 1885. See page 147, &c.

during the succeeding phases; the muscular sense, touch, sight, hearing, —then, during the somnambulism, the regions which preside over intelligence and will. Thus all the phases of hypnotism, or even of [spontaneous] somnambulism which have yet been observed, may merely represent different stages of this sleep at which different subjects happen to be arrested."

I concur with the general drift of these remarks,—more philosophical, as it seems to me, than any attempt to fit the constantly-varying phenomena of hypnotism into one Procrustean scheme. Still, in my view, we need to go much further still. We need to free ourselves altogether from the notion that the stages which a hypnotized subject passes through are necessarily *degrés de sommeil*,—that his changes are changes undergone *en s'endormant et en se réveillant*,—or even that there is likely to be between stage and stage any familiar connection, any discernible filiation at all.

But with this hint I must conclude. As I have here tried to analyse some of the methods of hypnogeny, so also I should have liked to try to analyse some of the hypnotic phenomena thus induced. But this paper is already full enough of disputable matter. My hope must be that it may stimulate other observers to fresh experiment,—though they, like myself, may spell out but a few words of the ciphered message in which hypnotism writes for us the secret of the psychical mechanism of man.

ADDENDUM.

Since this paper was in print, I have come across a scheme of hypnogenous agencies in Dr. Chambard's tractate, *Du Somnambulisme en général*, Paris, 1881. Dr. Chambard has since been selected to write the article on "Somnambulism" in the new *Dictionnaire des Sciences Médicales*, and even in his "thèse" of 1881 he speaks from a good deal of experience in several hospitals. I translate his scheme below. As compared with the scheme which has been suggested in this paper both its concordances and its variations may afford us some instructive hints.

Chambard's Scheme of hypnogenous processes.

A. Empirical or mixed processes, termed magnetic.

B. Analytical or simple processes.

1. Psychical action.

   α. Affective. Faith. Expectant attention. (Carpenter.) Moral emotions and expressions. Expressive gaze.

   β. Intellectual. Mental inertia. Fatigue of the attention.

2. Sensorial action.
    α. Suppression of sensorial excitations. Simple closure of eyes. (Lasègue.)
    β. Monotonous sensorial excitations, feeble and repeated.
        a. Of sight. Fixed gaze on an object, brilliant or not.
        b. Of hearing. Musical tone or noise. Watch (Weinholt,) diapason, &c.
        c. Of touch. Contact. Pressure. Temperature. Frictions. Shocks. (Heidenhain.)
        d. Cutaneous excitation determined by passes. (Heidenhain.)
        e. Excitation of erogenous regions; ovarian pressure.
3. Mechanical action.
    α. Modification of intra-ocular pressure.
        a. By convergence of the optic axes.
        b. By compression of the eyeball.
4. Physical actions. Action of magnet. (Landouzy.) Metallic plates. Static electricity.
5. Toxic action.
    α. Anæsthetics; Ether, chloroform, &c.
    β. Inebriants; Alcohol, haschisch, &c.

Now on this scheme I have to remark as follows:—

A.—A number of empirical methods are avowedly left unexplained.

B. 1.—I object, as already explained, to classing "psychical action" separately, as if it were altogether disparate from other effects produced on the nervous system. Such a word as "Faith" tells us nothing, in this connection. Moreover, I do not believe that "mental inertia" produces the hypnotic state at all. And "fatigue of the attention" should surely come under "monotonous excitations of hearing," if, as I suppose, Dr. Chambard means the kind of fatigue which is induced by prolonged listening to the ticks of a watch.

B. 2. α.—"Simple closure of eyes" surely does not produce the hypnotic trance on subjects who have never been previously hypnotized. I should rank it as one of the suggestions which succeed only when *recognised as suggestions*, not as in itself an efficient cause of trance.

B. 2. β. *a* and *b*.—Massive stimulations are here confounded with monotonous stimulations. The gaze at an electric light, or the sound of a gong, is not a *feeble* stimulus, and need not be *repeated*.

B. 2. β. *c*.—*Pressure*, again, is not necessarily a "feeble" or "repeated" form of excitation. Some of Dr. Chambard's own cases (Appendix to his tractate) strikingly illustrate the effect of a single touch on abnormally sensitive regions in a hysterical patient. I doubt the hypnogenous effect of changes of *temperature.* A cold wind will often *wake* a subject, (Esdaile, Elliotson,) but I do not know any cases where a mere rise or fall of external temperature has induced trance.

B. 2. β. *d.*—I have already maintained that on whatever cause the efficacy of "passes" may depend it is certainly not on *monotony* alone. The "passes" may often be varied and interrupted without appreciable detriment. I believe their efficacy to depend partly on the "vital influence" of the old mesmerists, partly on suggestion, and partly on pressure upon hypnogenous zones. Of course, monotonous movement, darkness, silence, and the mere effluxion of time, may contribute to sending the subject to *sleep.* But something more than monotony will generally be needed to cause that sleep to merge into hypnotic trance.

B. 2. β. *e.*—This sub-class is plainly referable not to monotonous excitations but to pressure on specially-sensitive zones.

B. 3.—I agree with Chambard that the fatigue induced by convergence of axes resembles the pain felt on compression of the eyeball. I should class both under the heading of hypnogenous zones. Heidenhain's view that Braid's squint operates by altering the cerebral circulation, (which I shall not here presume to contest,) is not inconsistent with this classification. It is perfectly possible that some or all of these localised pressures act by means of a mechanical influence on the circulatory system as well as by an influence of unknown character on the nervous system.

B. 4.—Chambard's "actions physiques" are included in my class of "specialised stimulations." I do not like his term; for his "sensorial" and "mechanical" actions are physical too. A fresh term is plainly needed to express such actions on the human frame as that of magnets and those of metals in contact (beyond mere effects of weight, temperature, &c.). Pending the suggestion of a more suitable term I should call such actions *supernormal,* as being unusual, but indicative of a more penetrating—not a diseased—sensitiveness on the subject's part.

B. 5.—I entirely agree that the effect of these toxic agencies is *analogous* to the hypnotic trance. But I do not think that trance dependent on *chemical* changes (deficient aëration of blood, diffusion of volatile ethers, &c.,) should be classed as *coördinate* with the trance induced by the hypnogenous agencies

> already discussed. In all of those classes the nervous change has been induced without the introduction of any material substance into the body; and though it is very difficult to define what agencies are to be called hypnotic, it seems plain that here at least a line can be drawn, and that changes dependent on wounds or poisoning—on mechanical rupture or toxic alteration of the tissues or fluids of the body—cannot be classed as hypnotic without making the word too vague to be of any real service.[1]

Finally, it will be observed that Chambard, while decisively admitting several non-telepathic agencies which I have noted as questionable, (magnet, electricity, &c.), has, of course, nothing to say as to telepathic influences. His scheme, therefore, is in my view inadequate to cover the cases adduced in this paper.

I must, therefore, consider Dr. Chambard's arrangement as *shallow* (in his want of coördination of psychical and physical agencies); as *confused* (in his cross divisions, as I have indicated, of the agencies which he does adduce;) and as *incomplete* (in his entire omission of hypnotization at a distance). But I say this with all respect for what seems to me to have been nevertheless a serviceable forward step. If my own classification is thought to be in any degree an improvement, this is due to the rapid advance in hypnotic experiment which the last five years have seen, and more especially to new light thrown, (as I venture to claim,) on all these topics by the establishment of telepathy as an actual and efficient cause. If my proposed scheme should so far subserve precision of thought as to lead to its own speedy supersession by some truer conspectus, its object will have been sufficiently attained.

[1] I may perhaps protest here against an occasional use of the word "hypnotic," both in French and English medical writings, as a mere equivalent for "soporific" or "anæsthetic," in speaking of drugs, &c. This is to obliterate the whole distinctiveness of a word to which it should be our aim to give all the precision possible.

VII.

THE CALCULUS OF PROBABILITIES APPLIED TO PSYCHICAL RESEARCH. II.

By F. Y. Edgeworth.

In a former paper I stated the principal problems which this subject presents, and I showed that they are reducible to, or at least involve the following:—The total number of trials being N, the chance of success at a single trial u, $Nu =$ (as near as may be) m, the number of successes $m+n$ (m and n both integers); what is the probability of at least that degree of success being obtained, supposing that chance is the only agency, under a *régime* of pure chance. I stated that the solution of this problem depends upon the summation of the last $N-(m+n)+1$ terms of the binomial $[u+(1-u)]^N$. As the approximate value of that sum I put the expression—

$$\frac{1}{2}\left[1-\frac{2}{\sqrt{\pi}}\int_o^T e^{-t^2}dt\right], \textit{ where } T=\frac{n}{\sqrt{2u(1-u)N}};$$

a formula of very general application in analogous inquiries. The conditions on which its validity depends are for the most part fulfilled in statistical investigations. They fail, however, in many of the problems which the Editors of this journal have submitted to me. It appears desirable, therefore, to take account of cases which lie outside what may be called the normal case and are not amenable to the received formula above written. Accordingly I propose[1] in this paper, first (I.) to take a theoretic survey of the methods appropriate to different conditions; and then (II.) to diagnose and prescribe for the various cases which arise in practice.

I.

A distinction of great theoretical importance is between those cases where (a) the binomial series which is to be summed (the binomial locus the area under which is to be evaluated) may be adequately represented by a probability-curve according to a well-

[1] For an extension of the following theory, the reader is referred to the writer's paper on "The Law of Error," in the *London Philosophical Magazine*, April, 1886.

known theory[1] of Laplace and Poisson, and ($\bar{a}$) those cases where such an ancillary probability-curve is not available.

a. In order to see how far a law extends, the proper course is to ascend to the sources from which it is derived. In this investigation let us take Poisson[2] for our guide. Let us remount to the rugged heights from which he reasons down to the simple normal case and the received formula. While following in his steps let us adopt his notation; observing that his μ is our N, his p is our u, his q is our $(1-u)$, his m is our $m+n$, his n is our $N-(m+n)$. And we may confine ourselves to the case in which $(N+1)u$, or $(\mu+1)p$, is an integer, both because no theoretical difficulty is presented by the absence of this condition, and because whatever difficulty may arise can, in the researches under consideration, be avoided by taking N properly. This being conceded, we shall have Poisson's ρ very nearly equal to our n, and his r equetable to our T.

It is shown by Poisson that the probability of obtaining at least m successes, at most n failures, is represented by a fraction of which the numerator and denominator are integrals; the subject of integration being the same for both, but the limits different: the subject of integration is of the form $He^{t^2}(h'+2h''t+3h'''t^2+\&c.)$, where h' h'' h'''... are respectively of the order $\frac{1}{\sqrt{n}}$ $\frac{1}{n}$ $\frac{1}{n^{\frac{3}{2}}}$ ....; if m n and μ be regarded as of one and the same order. For instance h' is $\sqrt{\frac{2(\mu+1)n}{(m+1)^3}}$ and h'' is $\frac{2(\mu+1+n)}{3(m+1)^2}$. Here, then, is presented a first condition upon which the applicability of the Laplace-Poisson analysis depends: namely, that m and n should be large enough to allow of their higher inverse powers being neglected.

A second condition is imposed by the inferior limit of the numerator integral. It is

$$\pm\left[n\,log.\frac{n}{q(\mu+1)}+(m+1)\,log.\frac{m+1}{p(\mu+1)}\right]^{\frac{1}{2}};$$

a quantity which Poisson calls k. In the case before us, where n is supposed less than $q(\mu+1)$ (the number of failures less than what might have been expected), the sign[3] of k is positive. The superior limit of the numerator integral is $+\infty$. The limits of the denominator integral are $\pm\infty$.

If we perform the work of integration for the numerator, so far as is possible by the ordinary formulæ of reduction, we shall find that the result consists of two parts, one under, and the other outside, the

[1] The analysis to which I allude is well expounded by Mr. Todhunter at p. 576 of his History.

[2] See Poisson, *Recherches sur la Probabilité*, &c., Chap. iii.

[3] *Cf.* Poisson, No. 76.

sign of integration. The former involves k only as its limit. The latter is affected by k in this wise :—

$$H\bar{e}^{k^2\frac{1}{2}}\Big[2h'' + 3h'''k + 4h^{\text{iv}}(k^2+1) + \&c.$$

The denominator is independent of k, being of the form [1]

$$\sqrt{\pi}\,H\,[h' + \tfrac{3}{2}h''' + \&c.].$$

Hence the quotient will be of the form

$$\int_k^\infty \overset{-t^2}{e}\,dt\Big[\,l + l'' + l^{\text{iv}} + \&c.\,\Big] + \overset{-k^2}{e}\Big[\lambda' + \lambda''k + \lambda'''k^2 \;.\;.\;.\;.\;;$$

where the l's and the λ's descend with the same rapidity as the h's. The convergency of the first part of this expression is unaffected by the size of k. But the convergency of the portion outside the sign of integration is destroyed if $\lambda''k$, $\lambda^{2'''}k^2$, &c., do not constitute a descending series.

At this point a new division presents itself; between (β) those cases where the data allow us to attain the degree of precision usually ascribed to the Laplace-Poisson method : such that fractions (of the sought probability) which are of the order $\frac{1}{\sqrt{\mu}}$, or more correctly $\frac{1}{\sqrt{pq\mu}}$, are retained; and ($\bar{\beta}$) where the regulation degree of precision is not attainable.

$\alpha\beta$. To satisfy the condition imported by β, it is necessary that the series λ', $\lambda''k$, $\lambda'''k^2$, should descend as rapidly as h' h'' h'''; therefore that k should not exceed the order of unity. To satisfy this requirement, it is a necessary and sufficient condition that ρ (the excess of successes over the number most probable on hypothesis of mere chance) should not exceed the order $\sqrt{\mu 2pq}$. That this is a sufficient condition may thus be shown. [2] Put

$$k^2 = \Big(q\,(\mu+1) - \rho\Big)\,log.\left[1 - \frac{\rho}{q\,(\mu+1)}\right] + \Big(p\,(\mu+1) + \rho\Big)\,log.\left[1 + \frac{\rho}{p(\mu+1)}\right]$$

where $\rho = m + 1 - p(\mu+1) = (\mu+1) - n$.

Expand k in ascending powers of ρ; and, putting [3] with Poisson

$$r = \frac{\rho}{\sqrt{2pq\,(\mu+1)}},\text{ you have } k = r\left[1 - \frac{(q-p)}{3\,\sqrt{2pq\mu}}\,r + \tfrac{5}{8}\,\frac{p^2 - pq + q^2}{2pq\mu}\,r^2 - \&c.\right.$$

[1] See Poisson, *op. cit.*, p. 193.

[2] The proof here given presupposes that $\sqrt{pq\mu}$ is considerable; as it is except when p or q are very small. The proposition is, however, independent of this condition. See my paper "On the Law of Error," *Phil. Mag.*, 1886, pp. 313, 320.

Op. cit., No. 78.

Now, as ρ is of the order $\sqrt{2pq\mu}$, r (our T approximately) is of the order unity. Hence the terms of the above written expansion above the second are of the order $\frac{1}{pq\mu}$, and therefore may be rejected. And the value of k thus presented is of the order unity. Again, while this limitation of the size of ρ is sufficient, it is also necessary. For, differentiating k^2 with regard to ρ, we observe that the first differential is continually (from the zero of ρ and k upwards) positive. Hence k (taken positively) continually increases with the increase of ρ. Hence, if k is as large as is allowable when ρ is of the order $\sqrt{2pq\mu}$, k will be too large when ρ is above that order. Hence the stated limitation of the size of ρ is not only sufficient, but necessary.

Here arises a new principle of division: according as (γ) we do not insist upon the full degree of precision which is attainable, but are content with an approximation which does not take account of terms of the order $\frac{1}{\sqrt{\mu}}$; or ($\bar{\gamma}$) we seek the full degree of accuracy to which we are entitled: an approximation true up to the order $\frac{1}{\sqrt{pq\mu}}$, rejecting only terms of the order $\frac{1}{pq\mu}$.

$\alpha\beta\gamma$. In the first case we may reject the second term of the above written expansion of k. For it is evidently of the order $\frac{1}{\sqrt{pq\mu}}$. We may reject also the unintegrated portion[1] of our result. For that portion is an expression of the form $e^{-r^2}\lambda'$, where λ' is of the order $\frac{1}{\sqrt{\mu}}$. And it may be shown that for the values of r with which we are concerned, the ratio of e^{-r^2} to $\frac{1}{\sqrt{\pi}}\int_r^\infty e^{-t^2}dt$ (the integrated portion of our result) is small in comparison with the order $\sqrt{\mu}$. For, as to values of r between 0 and 1, the ratio in question ranges from 2 to $\frac{37}{8}$. And for values of r above unity, the integral may be written (by an approximation due to Laplace):—

$$\frac{1}{\sqrt{\pi}}\left(\frac{1}{2r} - \frac{1}{4r^3} + \text{\&c.}\right)e^{-r^2}$$

Hence the ratio in question is of the order r, which by hypothesis is small with regard to $\sqrt{\mu}$. Hence, in every case (under $\alpha\beta\gamma$) the unintegrated portion of the result may be rejected. And we have thus the formula given in the former paper; which, as the last degree of accuracy is not required for the purpose in hand, the elimination of chance, may be regarded, I think, as the best formula for the case usually occurring in statistical inquiries, the normal case.

[1] Above, p. 191. Poisson, p. 196.

To exhibit the character of the assumptions made, let us consider the following example. Let μ be 999, p $\frac{1}{10}$, q $\frac{9}{10}$, $p(\mu+1)$ 100, and m 126. Then $\rho=27$. $r^2=\frac{\rho^2}{2pq(\mu+1)}=\frac{729}{180}=4{\cdot}5$ $r=2{\cdot}12\ldots$ The value in the tables for

$$\frac{1}{\sqrt{\pi}}\int_r^\infty e^{-t^2}$$

is ·0014. In order to make the approximation correct up to quantities of the order $\frac{1}{\mu}$, we must first put for r $r-\delta$[1] where

$$\delta=\frac{q-p}{3\sqrt{2pq\mu}}r^2=\frac{8 \quad 4{\cdot}5}{10\times3\times13{\cdot}417\ldots}={\cdot}089 \text{ nearly.}$$

Expanding the integral thus modified, we see that in respect of the term before rejected (the second term of the expansion of k), there is to be added to our result $\frac{{\cdot}89}{\sqrt{\mu}}e^{-4{\cdot}5}$. Also in respect of the unintegrated expression there is to be added:[2]

$$\frac{2(\mu+n)}{\sqrt{\pi}\;3\sqrt{2(\mu+1)\;(m+1)n}}=\frac{1}{\sqrt{\pi}}\frac{2}{3}\frac{1873}{\sqrt{2\times1000\times127\times873}}=\frac{1}{\sqrt{\pi}}{\cdot}008 \text{ nearly.}$$

The total addendum then is $\frac{1}{\sqrt{\pi}}e^{-4{\cdot}5}\left\{{\cdot}089+{\cdot}008\right\}$. In ordinary logarithms:

$$
\begin{aligned}
log.\ \frac{1}{\sqrt{\pi}} &= -\ {\cdot}2486 = \bar{1}+{\cdot}7514\\
log.\ e^{-4{\cdot}5} &= -\ 1{\cdot}954 = \bar{2}+{\cdot}046\\
log.\ ({\cdot}089+{\cdot}008) = log.\ {\cdot}097 &= \bar{2}+{\cdot}987\\
\hline
log.\ Addendum &= \bar{4}+{\cdot}8
\end{aligned}
$$

∴ the Addendum = ·0006 nearly, a quantity which, though certainly a considerable fraction of the result before obtained, viz., ·0014, may yet, I think, safely be neglected for the purpose in hand: which is to ascertain the probability of the given phenomenon (a certain degree of success) occurring by accident.

$\alpha\beta\bar{\gamma}$. But, if greater accuracy is required, then we have only to make the two corrections which have been just described.

$\alpha\bar{\beta}$. We revert now to the case where the data do not admit of our obtaining the regulation degree of accuracy. Here $\frac{k}{\sqrt{\mu}}$ must still be supposed fractional, though no longer a fraction of the order $\frac{1}{\sqrt{\mu}}$. In this case the expansion of k in ascending powers of ρ, the value of r,

[1] *Cf.* Poisson, p. 200, above. [2] Ib., p. 196.

no longer afford us guidance. k may still be small with regard to $\sqrt{\mu}$, though ρ greatly exceed $\sqrt{2pq\mu}$. We may have still a result accurate to the fraction $\frac{k}{\sqrt{\mu}}$, viz.:

$$\frac{1}{\sqrt{\pi}}\left[\int_k^\infty e^{-t^2}dt + \frac{2(\mu+n)\,e^{-k^2}}{3\sqrt{2(\mu+1)\,(m+1)\,n}}\right].$$

There is apt to occur here the difficulty that the ordinary tables do not suffice for the evaluation of the integral, when k is large. The ordinary scales are unsuited to the enormous probabilities (in favour of a cause other than chance) which occur in psychical research. To obviate this, expand the integral in descending powers of t, and write for the sought probability:

$$\frac{1}{\sqrt{\pi}}\,e^{-k^2}\left[\frac{1}{2k}-\frac{1}{4k^3}+\ldots+\frac{2(\mu+n)}{3\sqrt{2(\mu+1)(m+1)n}}\right]$$

To illustrate this case, let us take the following example: as before, $\mu=999$, and $p=\frac{1}{10}$; but now $m=299$, and n accordingly 700. Here k^2, evaluated by ordinary logarithms according to the formula on p. 191 (where the logarithms are Napierian), becomes $66\cdot7352\cdot\cdot\times log._e10=153\cdot757$. $k=12\cdot4$. Meanwhile $r=\frac{\rho}{\sqrt{2pq\mu}}=\frac{200}{13\cdot4}=15$; so that r^2 will no longer do duty for k^2. And, if we resort to the expansion of k in terms of r, we shall have at best a tedious route; since it is not now by hypothesis, nor in fact, safe to stop at the second term of the expansion.

Substituting the values of k μ m n in the formula just written, I find for the sought probability $\cdot0^{66}10\times[\cdot04+\cdot056]=\cdot0^{67}1$, or $\frac{1}{10^{68}}$; where $\cdot0^{66}$ is used as a symbol for 66 [$\cdot0^{67}$ for 67] ciphers intervening between the decimal point and the significant figures of the decimal. By another method, to be mentioned presently, I find that the sought probability is between $\cdot0^{67}15$ and $\cdot0^{67}19$. The present method, therefore, gives a sufficiently accurate value.

$\bar{a}$. Having now exhausted all the branches of case a, we come to its negative, $\bar{a}$; which may be subdivided according as there does (δ), or does not ($\bar{\delta}$), exist a simple approximative form for the sum of the series under consideration.

$\bar{a}\delta$. To this category belongs the case discussed by Poisson in his No. 81; where N (to revert to our own notation) is large, u is very small, and $m+n$ is small, so that the fraction $\frac{m+n}{N}$ is neglectible. If we observe that Poisson's n in this section corresponds not now to the number of failures in our problem, but to the number of successes, a little attention will show that a formula appropriate to our case (where our $m+n$, though small, is still apt to be considerably above Nu) is the second formula given by Poisson (at the foot of p. 206). Only for our purpose to ascertain the probability of the failures being at most $N-(m+n)$, it will be proper to put for the Poissonian n not our $m+n$, but $m+n-1$; and to take not the whole of the second formula referred to, but the latter portion of it. In short, if we put $\omega = Nu$, then according to this method the probability of obtaining at least $m+n$ successes is

$$\frac{\omega^n \times e^{-\omega}}{n!}\left(1+\frac{\omega}{n+1}+\frac{\omega^2}{(n+1)\,(n+2)}+\&c.\right)$$

I have applied this method with success to the table of results given in *Phantasms of the Living*, Vol. I., p. 25; where the condition that $\frac{n}{N}$ should be neglectible was peculiarly well fulfilled.

$\bar{a}\delta$. There remain over the cases which do not possess any simple ancillary form. As far as I know, these cases do not admit of a solution comparable with the methods which have been described, in respect of concinnity and elegance. There exists, however, a rougher procedure, which has the advantage of being most efficacious exactly when the conditions upon which the general method (a) depends are least perfectly fulfilled.

The series which we have to sum is

$$\frac{N!}{[N-(m+n)]!\;[m+n]!}\,(1-u)^{N-(m+n)}\times u^{m+n}$$
$$+\frac{N!}{\Big[N-(m+n+1)\Big]!\;\Big[m+n+1\Big]!}(1-u)^{\left(N-(m+n+1)\right)}u^{m+n+1}$$
$$+\ \&c.$$

(where, as usual, the note of admiration imports the continued product of all the integer numbers up to and including that preceding the note). This sum may be otherwise written:

$$(1-u)^{N-(m+n)}\times u^{m+n}\times\frac{N!}{[N-(m+n)]!\;[m+n]!}\left\{1+\frac{N-(m+n)}{m+n+1}\times\frac{u}{1-u}+\&c.\right\}$$

The second term within the bracket is a proper fraction. For, even if n were zero, the term in question would be only just equal to unity; and by the increase of n the numerator is continually diminished, while the denominator is increased. Call this fraction v. It is easy to see that the third term is less than v^2, the fourth term less than v^3, and so on. Accordingly, the expression within the brackets forms a convergent series; which we can either sum by evaluating as many terms as we think fit, or by putting at once the expression without the brackets as an inferior limit, and the same multiplied by $\frac{1}{1-v}$ as a superior limit, of the sought probability. It will be observed that the series within the brackets is most convergent when v is smallest, and that v is smaller *ceteris paribus* according as n (the excess of success) is larger, and u (the probability of success at a single trial) is smaller. Now these are exactly the conditions upon which the increase of the Poissonian k, and therefore the failure of the Laplace-Poisson approximation, turns; so that the present method may be regarded as complementary to the Laplace-Poisson approximation.

As to the expression outside the brackets in the above-written expression, for the evaluation of $N!$, $(m+n)!$, $[N-(m+n)]!$, recourse may be had to a table of the values of *log.* $\Gamma(x+1)$; such as is given by De Morgan at the end of his treatise on the Calculus of Probabilities. In so far as such a table is only available for small numbers, in this respect again the present method is complementary to method a; of which large numbers form an essential condition.

Where tables of the Gamma-Function are not available, we must, I suppose, fall back upon the formula $x! = x^{x+\frac{1}{2}} e^{-x} \sqrt{2\pi}\left(1+\frac{1}{12x}+ \&c.\right);$ which it is not very troublesome to evaluate by the aid of logarithms.

Let us take by way of illustration the example above (p. 193), solved by another method; where $N=999$, $u=\frac{1}{10}$, $m=299$, $n=700$, the required probability is

$$\left(\frac{9}{10}\right)^{700}\left(\frac{1}{10}\right)^{299}\frac{999\,!}{700\,!\;299\,!}\left\{1+\frac{700}{300}\,\frac{1}{9}+\frac{699}{301}\,\frac{1}{9}+\&c.\right.$$

$$\log.\left(\frac{9}{10}\right)^{700} = \overline{33}\cdot9698 \qquad \log.\ 299\,! = 612\cdot00868$$

$$\log.\left(\frac{1}{10}\right)^{299} = \overline{299} \qquad \log.\ 700\,! = 1689\cdot38418$$

$$2301\cdot39286$$

$$\log.\ 999\,! = 2564\cdot6046$$

$$2233\cdot5744$$

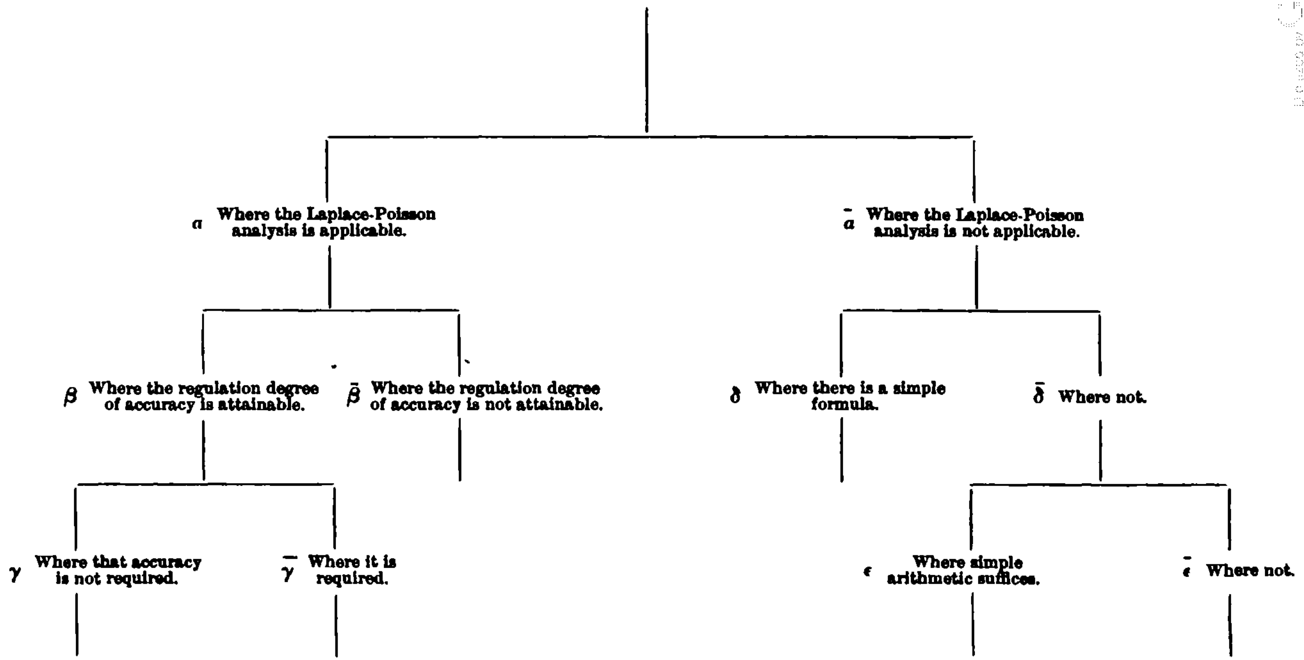
$$2233\cdot5744-(2301\cdot39286) = \overline{68}\cdot1815$$

Hence for an inferior limit to the sought probability we have $\cdot0^{67}15$; and for a superior limit the same quantity multiplied by $\frac{27}{20}=\cdot0^{67}2$.

I have applied this method with advantage to the problems presented in *Phantasms of the Living*, Vol. II., pp. 16-7.

If we distinguish within this category the case in which the numbers are small enough to be manipulated by ordinary arithmetic, we shall, I think, have exhausted all the branches of the subject. They are presented in the following logical tree:—

TREE OF THEORY.



II.

I propose now to sum up the practical conclusions of this and the preceding paper in a form adapted to the requirements of the general reader.

The problems which the subject presents are mainly three. It is required to appreciate by means of the calculus of probabilities the evidence in favour of some extraordinary agency which is afforded by experiences of the following types: (1) One person chooses a suit of cards, or a letter of the alphabet. Another person makes a guess as to what the choice has been. This experiment—a choice by one party, a guess by another—is performed N times. The number of successful guesses exceeds the number which is the most probable on the supposition of mere chance, viz., m, where $m = Nu$ (in the above-mentioned cases respectively $\frac{1}{4}N$ and $\frac{1}{24}N$), by a considerable number n, where $n = Nv$. (2) There are given a second and a third similar series of trials, in each of which the number of successes exceeds the number most probable on the hypothesis of pure chance, viz., $N'u'$ $N''u''$, by n' n'' respectively. Or (3) along with a number of such series there occur some in which the number of successes falls below the most probable number. What probability in favour of the existence of some agency other than chance is afforded by (1) a single series, in which the successes are in excess; (2) a set of series, in each of which the successes are in excess; (3) a chequered set of series in some of which the successes are in excess, in others in defect?

The answer to the first of these problems depends upon the answer to the following question: What is the probability that under a *régime* of pure chance—supposing that there were no disturbing cause at work—the observed excess of successes would occur? Call this probability p. Then the measure of the sought probability—that some agency other than chance has operated—is $1 - p$. [1] The first problem is thus made to depend upon a simple, or at least straightforward, calculation. The second problem is made to depend upon the first—or rather the question upon which the first depends—in the following manner. For each of the given series find the probability corresponding to the p which has been just defined. Call the set of values thus found respectively p_1, p_2, &c. The evidence in favour of a cause other than chance which is afforded by the whole set of series, the complete concatenation of data, is $1 - p_1 \times p_2 \times$ &c. Lastly, the third problem is thus resolvable into the second. Rearrange, without manipulation and cookery, but in a random fashion—the given chequered set of series into a smaller set of larger series: such that each of the new series may present excess. An example of

[1] See Note [1], at the end of Part II.

this process is afforded by the problem mentioned by Mr. Gurney. There the data consisted of a set of ten series: each of them containing a thousand trials, and *one* of them presenting defect of successes—fewer successes than upon the hypothesis of pure chance would be most likely. If we lump this defective series with *any* of the nine remaining, there will result a composite series of 2,000 trials which presents excess of success. Our data will then be eight series of 1,000 and one series of 2,000 trials; each series presenting excess. The data thus modified exemplify our second problem.

Everything then depends ultimately upon the calculation of probabilities such as the above-defined p. In the preceding paper I stated the method of calculation which, as being required and proving sufficient for most, or at least many, statistical investigations analogous to those of *Psychical Research*, may perhaps be described as the general or normal method. In this paper there is attempted a more complete statement of the possible cases, and the treatment appropriate to each.

The cases constituted by the variety of our data may be divided according to the presence or absence of each of the following attributes :—

(*a*) The numbers of trials N small enough to admit of direct arithmetical computation.[1]

(*b*) The quantity $\frac{n}{2Nu(1-u)}$ [2] a small fraction.[3]

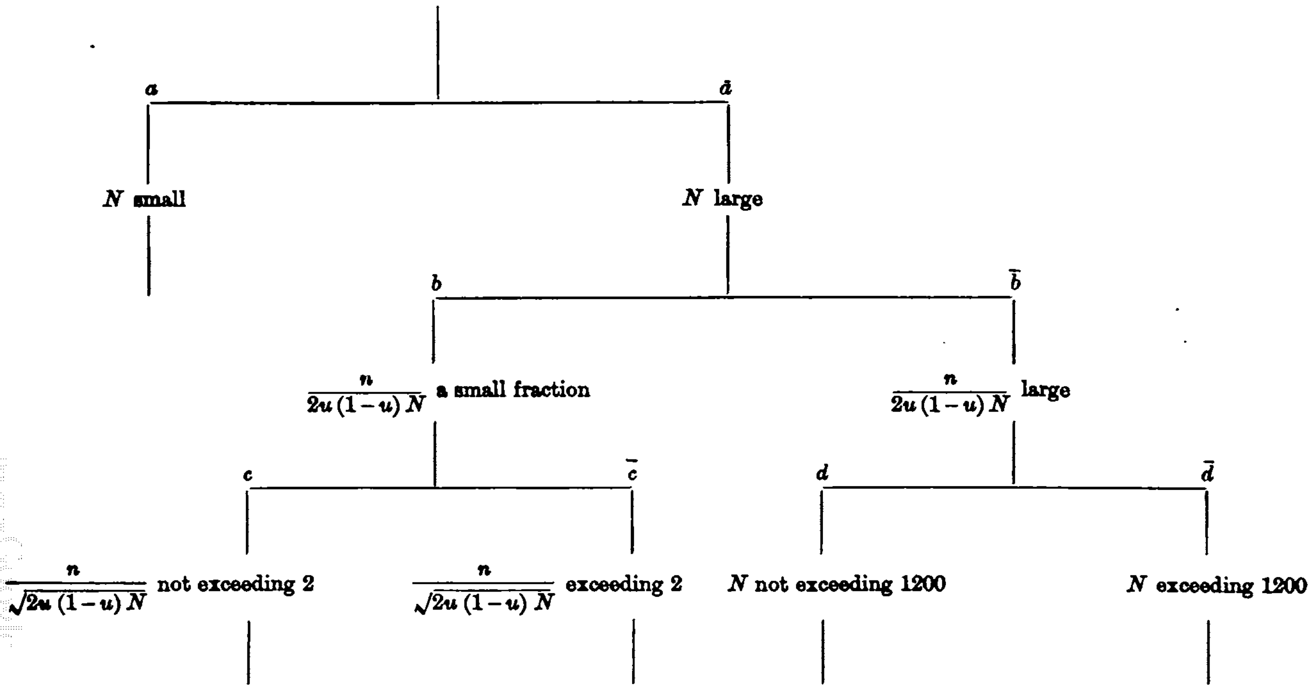
(*c*) The quantity $\frac{n}{\sqrt{2Nu(1-u)}}$ not greater than 2 [or 3].[4]

(*d*) N not exceeding 1200 (or the highest number for which the value of *log.* $\Gamma\,(N+1)$ is given in tables accessible to the operator).[5]

The presence of one of these attributes may be expressed by the corresponding letter, *e.g.*, b; its absence by the same letter with a negative sign superscribed, *e.g.*, $\bar{b}$. Thus the heading $\bar{a}bg$ refers to the case in which N is a large number, the quantity $\frac{n}{2u\,(1-u)\,N}$ is a small fraction, and the quantity $\frac{n}{\sqrt{2u(1-u)N}}$ does not exceed 2. As to the presence or absence of the fourth attribute nothing is stated; N may be either above or below the limit 1200. The variety of species thus constituted are represented by the accompanying *logical tree.*

[1] See Note [a]. [2] For the meaning of these symbols, see p. 198 above.
[3] See Note [b]. [4] See Note [c]. [5] See Note [d].

Tree of Practice.



N = Total number of trials. u = Chance of success at a single trial. n = Excess of successes over Nu (being an integer, or the integer nearest to Nu).

In examining these divisions I propose to give priority to the branches on the left, and to exhaust all the ramifications of each branch before proceeding to the branch next on the right. The order exactly resembles the devolution of real property according to English law. Our table corresponds with that given by Mr. Joshua Williams, in his lucid chapter on the descent of an estate in fee simple; if for the trunk of our tree we put Mr. Benjamin Brown, the purchaser, for a his elder, and for $\bar{a}$ his younger son.

A complete logical tree which has four bifurcations ought to present 16 ultimate divisions. But the task of examining so many cases is abridged by the observation that some of the branches are withered nonentities, and others, though existent, are unfruitful, and will not repay cultivation. Of the first sort is $a\bar{d}$; also $\bar{a}\bar{b}c$, which will be found to involve a contradiction in terms, except in the very rare case where the quantity $2u\,(1-u)\,N$ approaches unity, where, though N is large, u is exceedingly small. That case falls into the second category of rejectible branches. For the distinction of $\bar{a}\bar{b}$ into $\bar{a}\bar{b}c$ and $\bar{a}\bar{b}\bar{c}$ subserves no useful purpose, does not constitute a Natural Kind. To the same category belong the (existent) sub-divisions of a.

I proceed now, without further preface, to prescribe for and exemplify the particular cases.

a.

This is the case of N small (say not exceeding 10).

Rule: Expand $[u+(1-u)]^N$ by the binomial theorem; and add together the last $N-(m+n)$ terms of the expansion, those written in the second of the two following lines:—

$$(1-u)^N + N(1-u)^{N-1}u + \&c.$$

$$+\frac{N(N-1)\ldots\ldots(N-(m+n)+1)}{1\cdot2\ldots\ldots\ldots(m+n)} + \&c. \qquad + N(1-u)u^{N-1} + u^N$$

The sum of the terms in the second line is the required probability (of the observed degree of divergence occurring by mere chance).

Example: In the experiment cited by Mr. Gurney at p. 251 of Vol. II. of the *Proceedings of the S.P.R.*, the "name thought of" was DOREMOND, and the "letters produced" were EPJYEIOD. Here, out of eight guesses, there are four successes; if success consist in guessing either the very letter thought of, or either of its nearest alphabetical neighbours, in short any one of an assigned consecutive triplet. The probability that a letter taken at random should fall within any assigned triplet is $\frac{1}{8}$. Accordingly (on the supposition that chance is the only agency), the probabilities of obtaining in the course of eight trials no successes, one success, two successes, &c., are given by the first, second, third, &c., terms respectively of the binomial $(\frac{7}{8}+\frac{1}{8})^8$. The probability of obtaining at least four successes is equal to the sum of the fifth and remaining terms; that is

$$70\left(\frac{7}{8}\right)^4\left(\frac{1}{8}\right)^4+56\left(\frac{7}{8}\right)^3\left(\frac{1}{8}\right)^5+28\left(\frac{7}{8}\right)^2\left(\frac{1}{8}\right)^6+8\cdot\left(\frac{7}{8}\right)\left(\frac{1}{8}\right)^7+\left(\frac{1}{8}\right)^8,$$

or ·011.

The probability in favour of an agency other than chance is about ·99. The odds against the observed event occurring by mere chance are about a hundred to one.

$\bar{a}bc$.

This is the case of N large, the quantity $\frac{n}{2u(1-u)N}$ a small fraction (say less than $\frac{1}{4}$), and the quantity $\frac{n}{\sqrt{2u(1-u)N}}$ not exceeding 2.

Rule: Put $T=\frac{n}{\sqrt{2u(1-u)N}}$; and find the value of the integral $\frac{2}{\sqrt{\pi}}\int_0^T e^{-t^2}dt$ by means of the[1] tables attached to many treatises on the Calculus of Probabilities. Call that value P. Then $\frac{1}{2}[1-P]$ is the required probability.

Examples: (1) In the instance given by Mr. Gurney, at p. 241 of *Psychical Research*, December, 1884, N is 2927, u is $\frac{1}{4}$, and n is 57. Here $2u(1-u)N=\frac{3}{8}\ 2927=1097{\cdot}6$. $\frac{n}{2u(1-u)N}$ is less than $\frac{1}{19}$ and $T=1{\cdot}7$. The case therefore falls under the category $\bar{a}bc$. Referring to the table given with the article on Probabilities in the *Encyclopædia Britannica*, 8th and 9th editions, I find for the value $\frac{1}{\sqrt{\pi}}\int_0^T e^{-t^2}dt$ corresponding to $T=1{\cdot}7$ the entry ·9838. Hence for the sought probability we have $\frac{1}{2}[1-{\cdot}9838]=\frac{1}{2}[{\cdot}016]={\cdot}008$.

The probability of agency other than chance $={\cdot}992$. The odds against the observed event being purely fortuitous are about a hundred to one.

(2) In the next instance cited by Mr. Gurney in the same passage, $N=1833$, $u=\frac{1}{4}$, and n is 52. Here $2u(1-u)N=\frac{3}{8}\ 1833$. $\frac{n}{2u(1-u)N}$ is less than $\frac{1}{13}$; so that the case belongs to b. But the attribute c is not perfectly fulfilled. For $\frac{n}{\sqrt{2u(1-u)N}}$ is just over 2,

[1] If the values given in the tables consulted are of the integral $\frac{1}{\sqrt{\pi}}\int_0^\infty e^{-t^2}dt$ then put $\frac{1}{\sqrt{\pi}}\int_T^\infty e^{-t^2}dt$ for the sought probability.

namely 2·1. However, *de minimis non curat calculus.* We may take the value given in the tables for $T=2$— *cum grano;* taking into account that we shall be over-rating the probability of mere chance, under-rating the evidence of a disturbing cause. For $T=2$ we have P, as defined in the Rule, ·99532. Hence p the sought probability = (is less than) $\frac{1}{2}[\cdot 00266] = \cdot 0013$.[1]

The probability of a cause other than chance = ·997. The odds against the observed event being purely fortuitous are about five hundred to one.

$\bar{a}b\bar{c}$.

In this case $\bar{a}$ and b are fulfilled as in the former case, but the condition that T should not exceed 2 is no longer fulfilled.

Rule: Evaluate $T^2 = \dfrac{n^2}{2u(1-u)N}$. Put $a = (T^2 \times \cdot 434 \cdots + \cdot 2485)$. The required value is $\dfrac{1}{10^a}\left[\dfrac{1}{2T} + \dfrac{2(2-u)}{3\sqrt{2u(1-u)N}}\right]$; an expression which may be simplified by ordinary logarithms.

Examples: (1) N is 976, u is $\frac{1}{8}$, n is 35.

Here $2u(1-u)N$ is $=271$, $\dfrac{n}{2u(1-u)N} = \dfrac{1}{8}$. $\dfrac{n}{\sqrt{2u(1-u)N}} = 2\cdot 14$, an excess over 2 so slight that the case might safely be referred to the former category.[2] Keeping to the rule, however, we have $T^2 = \dfrac{35^2}{271} = 4\cdot 5203$.

$$a = T^2 \times \cdot 434 + \cdot 2485 = 1\cdot 962 + \cdot 248 = 2\cdot 21.$$

$$\text{Also } \frac{1}{2T} = \cdot 23,\ \frac{2(2-u)}{3\sqrt{2u(1-u)N}} = \cdot 08.$$

Hence for the sought probability $\dfrac{1}{10^{2\cdot 21}} \times \cdot 3$. Which is reducible by logarithms to ·002.

The probability of a cause other than chance = ·998. The odds against the observed event being purely fortuitous are about five hundred to one.

(2) The general result of . . . N is 17653, u is $\frac{1}{4}$, $u=347$.

Here $2u(1-u)N = 6620$, and n divided by this quantity is a small fraction. $T^2 = \dfrac{347^2}{662} = 18\cdot 189$.

$a = T^2 \times \cdot 434 + \cdot 248 \ldots = 8\cdot 14$. Whence for the sought probability $\dfrac{1}{10^{8\cdot 14}}\left[\dfrac{1}{8\cdot 4} + \cdot 01\right]$, which with the aid of logarithms is reducible to ·000,000,000,8.

[1] See Note 4. [2] See Note 4.

The probability of a cause other than chance ·999,999,999,2. The odds against purely fortuitous origination are about ten thousand million to one.

$\bar{a}\bar{b}d$.

This is the case in which N is large, but not so large as to exceed the range of values covered by Table V. at the end of De Morgan's essay on the Calculus of Probabilities (*Encycl. Metrop.*), or a similar table accessible to the operator; while the quantity $\dfrac{n}{2u(1-u)N}$ approaches or exceeds unity.[1]

Rule: Write for the sought probability

$$\frac{\Gamma(N+1)}{\Gamma(m+n+1)\,\Gamma(N-(m+n)+1)}u^{m+n}(1-u)^{N-(m+n)}$$
$$\left[1+\frac{u}{(1-u)}\frac{N-(m+n)}{m+n+1}+\frac{u^2}{(1-u)^2}\frac{(N-(m+n+1))}{m+n+2}\frac{(N-(m+n))}{m+n+1}+\&c.\right]$$

Evaluate the expression outside the brackets by means of a table for *log.* $\Gamma\ (x+1)$ (such as De Morgan's Table V.) and an ordinary logarithm table. Continue the series within the brackets as long as may seem requisite for accuracy. It will usually be sufficient to take account of the second term. Call this term ν, and the expression outside the brackets J. Then J—and, still more accurately, $J(1+\nu)$—is an inferior limit of the sought probability; $\dfrac{J}{1-\nu}$ is a superior limit.

Example: In one of the experiments recorded . . . . . $N=505$, $u=\frac{1}{4}$, $m+n=261$. Here $2N(1-u)u=189$. And, consequently $\dfrac{n}{2N(1-u)u}$, $\dfrac{145}{189}$, is dangerously large.

We resort, therefore, to the method appropriate to $\bar{b}$, *log.* $J=$ *log.* $\Gamma(505+1)-$ *log.* $\Gamma(261+1)-$ *log.* $\Gamma(244+1)+244$ *log.* $(\frac{3}{4})+261$ *log.* $(\frac{1}{4})$. Evaluating these quantities by means of De Morgan's Table V and ordinary logarithms, I find for the logarithm of J. $-38+{\cdot}823$, and for J $\cdot\overset{37}{0}666$. For a fairly accurate value of the sought probability, we have $\cdot\overset{37}{0}66\left[1+\frac{1}{3}\,\frac{261}{245}\right]=\cdot\overset{37}{0}66\times1{\cdot}355=\cdot\overset{36}{0}1$, the symbols $\cdot\overset{36}{0}$ $\cdot\overset{37}{0}$ being employed to denote respectively 36 or 37 *noughts* following the decimal point.[2] For a superior limit $\cdot\overset{37}{0}66\ \dfrac{1}{1-{\cdot}355}=\cdot\overset{36}{0}13$ . . . . . The true value lies between the two given, if my work is correct.

The probability of a cause other than chance $=\cdot\overset{37}{9}$. The odds against purely fortuitous origination are a trillion trillion to one.

[1] See Note *.

[2] Or is it easier to say $\dfrac{1}{10^{37}}$.

$\bar{a}\bar{b}\bar{d}$.

This case is like the former, except that we have not the advantage of the tables for *log.* Γ. We are therefore reduced to the laborious process of evaluating *log.* Γ for ourselves.

Rule: For the expression of the form *log.* Γ $(x+1)$, which occurs in the last case, put now the following[1]—$(x+\frac{1}{2})$ *log.* $x+\cdot 399$, and otherwise proceed as before.

Example: . . . $N=1403$, $u=\frac{1}{90}$, $m+n=162$, $N-(m+n)=1241$. Here $\frac{n}{2u(1-u)N}=\frac{146\cdot 4}{30.8}$, that is far above unity. We have, therefore, an aggravated case of $\bar{b}$. Put for the quasitum

$$J\left[1+\frac{1}{89}\times\frac{1241}{163}\right], \text{ or } J\left[1+\frac{1}{12}\right];$$

where $log.\ J=(1403\cdot 5)\ log.1403-162\cdot 5\ log.162-\cdot 399-1241\cdot 5\ log.1241+162\ log.\ \frac{1}{90}+1241\ log.\ (\frac{89}{90})=1403\cdot 5\ log.\ 1403-162\ [log.\ 162+log.\ 90]-1241\ [log.1241+log.90-log.89]-\frac{1}{2}[log.1241+log.162]-\cdot 399$. Evaluating this expression, I find for *log.* J $-107+\cdot 921$. Whence $J=\cdot\overset{106}{0}8$, (if as before we denote by $\cdot 0^x$ the sequence of x ciphers after a decimal point). And for the answer $\cdot\overset{106}{0}9$.[2]

The probability of a cause other than chance $=\cdot\overset{107}{9}$.[3] The odds against the observed event having a purely fortuitous origin are a [4]trillion trillion trillion trillion trillion trillion (a nonillion nonillion) to one—odds to describe whose vastness "number fails."

NOTES TO PART II.

[1] On the nature of this inference see, in addition to the preceding paper in this *Journal* (No. 8), my paper on "Observations and Statistics" in the *Cambridge Philosophical Transactions* for 1885, p. 148, *et seq.*

[2] It is, of course, impossible to fix where a ends and $\bar{a}$ begins. The boundary, like others in this paper, is not a hard and fast line.

[3] The ground of the distinction between b and $\bar{b}$ is twofold. First and foremost is the circumstance that, when b as defined in the text is realised, then it is allowable to substitute for the troublesome k of

[1] See Note [7]. [2] Or about $\frac{1}{10^{106}}$. [3] See Note [8].

[4] As I understand, a million million is a billion, a million billion is a trillion, a million trillion is a quadrillion, and so on up to a nonillion.

Poisson (*Recherches*, Art. 76) the much more manageable r (*Ibid.*, Art. 78), in our notation $\frac{(m+n+1)-(N+1)p}{\sqrt{2pq(N+1)}}$. For $\frac{k}{r}$ is expansible in ascending powers of $\left(\frac{r}{\sqrt{2pq(N+1)}}\right)$, the first term being 1. Accordingly, if b is present, that is if $\frac{(m+n-Np)}{2pq\times N}$ is a small fraction, the first term only of the expansion need be retained. Further, the Poissonian r may be reduced to the still simpler expression which corresponds to the T of Mr. Todhunter (the Poissonian u, *Recherches*, Art. 79). It will be observed that these simplifications take for granted that $\frac{1}{\sqrt{2pqN}}$ is a quantity of neglectible order. This has not been formally postulated; but it follows from the condition that $\frac{\sqrt{n}}{\sqrt{2pqN}}$ is small (deductible from b), especially if, as universally the case in the problems of Psychical Research, and indeed most problems of the sort, that n is considerably greater than unity.

It follows, then, from condition b, that the integral portion of the sought probability may be written in its simplest form, viz., that employed by Mr. Todhunter in his formula for the central portion of the binomial (*History*, p. 576); in our notation

$$\tfrac{1}{2}\left[1-\frac{2}{\sqrt{\pi}}\int_0^T e^{-t^2}dt\right], \textit{ where } T=\frac{n}{\sqrt{2pqN}}.$$

But, further, the same condition allows us to neglect the term outside the sign of integration. This term, in its unreduced form, is in our notation

$$\frac{\sqrt{2}\,(N+1+N-(m+n))}{3\sqrt{\pi(N+1)\;(m+n-1)\;(N-(m+n)}}\times e^{-k^2}.$$

Now, if condition b is fulfilled, it is allowable to expand the non-exponential part of this expression in ascending powers of $\frac{n}{N}$ and neglect terms after the first. Remembering that $m=Np$ (approximately at least, see above, p.), we have for the first term of the expansion $\frac{\sqrt{2}}{3}\,\frac{(1+q)}{\sqrt{\pi Npq}}\times e^{-k^2}$. And e^{-k^2} may, as above, be reduced to e^{-t^2}. Now this quantity may be neglected in comparison with the integral which is retained. For the latter may be written $\frac{1}{\sqrt{\pi}}e^{-T^2}\left(\frac{1}{2T}+\&c.\right)$ Whence the ratio of the unintegrated to the integrated portion is of the order $\frac{2T}{\sqrt{Npq}}$, or $\frac{n}{Npq}$, to unity. Whence the proposition.

To avoid mistakes, it may be added that if—as we approach the case of $\bar{a}\bar{b}$—it seems worth while to take some account of the term outside integration, then it must be remembered that its primitive form is modified, not only by its own expansion, but also by the change from r to T in the integral part. See Rule $\bar{a}b\bar{c}$.

[4] The ground of the distinction between c and $\bar{c}$ is the fact that in many of the books the table of the integrated error-function does not extend beyond the argument 2; in few or none beyond 3. The circumstance that the tables have not been carried further is connected with an important property attaching to cases where the observed *écart* exceeds two or three times the *modulus*, *i.e.*, to class $\bar{b}$: namely, that in these cases the evidence in favour of a cause other than chance has been regarded by the authorities as amounting to practical certainty. Where, as in the *Encyclopædia Britannica*, editions 8 and 9, the table is continued as far as 3, then the latter limit should be taken for our definition. In this case Example 2 of $\bar{a}bc$ and 1 of $\bar{a}b\bar{c}$ (pp. 202-3) fall unequivocally under $\bar{a}bc$. Employing the tables in *Encyclopædia Britannica*, edition 9, I find for the former example ·0015, and for the latter (interpolating between the entries for 2·1 and 2·2) ·0012.

[5] The second term need only be added when there is some suspicion that the condition b is not perfectly fulfilled; the case considered at the end of note [3].

[6] It will be gathered from the theoretical analysis that it is possible, with due caution, for one who knows the nature of the ground, to advance a great way along the lines of the received method of approximation. Thus many cases of $\bar{a}\bar{b}$ are amenable to the received general exponential formula; corrected not only by taking some account of the term outside integration (as suggested in the rule for $\bar{a}b\bar{c}$) but also by taking account of the second term of the expansion of k (Poisson, Art. 78). For instance, in the example under $\bar{a}bd$ $N=505$, $p=\frac{1}{4}$, $n=135$); if we employ the uncorrected exponential formula $\frac{1}{\sqrt{\pi}}\int_T^{\infty} e^{-t^2}$ we shall get a value $\cdot 0^{43}6$, which exaggerates by about a million times the odds in favour of a cause other than chance. To this value should be added: first $\frac{1}{\sqrt{\pi}} e^{-T^2} \frac{1}{\sqrt{2pqN}}$, about $\cdot 0^{43}14$, which still gives a very inaccurate result. For a better approximation, write

$$\frac{1}{\sqrt{\pi}}\int_{r+\delta}^{\infty} e^{-t^2} + \frac{\sqrt{2}}{3}\,\frac{1+q}{\sqrt{\pi N p q}}\, e^{-r^2}$$

where r has the value assigned on p. 191, and δ is the second term of the expansion of k (Poisson, Art. 78). Here, then, $r+\delta=9\cdot84\left[1-\frac{1}{6}\,\frac{135\cdot5}{189\cdot4}\right]$. Substituting which, we have still an inaccurate result, but which has at least the advantage of erring on the safe side. It might be possible, by proceeding to another term of expansion, to bolster up the familiar formula. But I think that such corrections could not safely be employed by those who might not see the reason of them—the class to whom these rules are addressed. The method recommended in the text has the advantage of being straightforward and uniform. And, doubtless, the feeling of those whom I address is "*sit quidvis, simplex dumtaxat et unum.*" Nor, indeed, is the method here recommended more laborious, while it is much safer, than to correct the generally received formula (as Professor Lazarus proposes, *Assurance Magazine*, Vol. XX.) in some such way as that which I have just indicated.

[7] It need hardly be pointed out that the factor $\overline{e}^{x}$ disappears by division in the expression under treatment; and that ·399 is *log.* $\sqrt{2\pi}$.

[8] By the Poissonian unreduced formula (*Recherches*, Art. 77), I have found for this example $\cdot 0^{106}13$. It may be observed that the formula here prescribed for $\bar{a}\bar{b}$ is very similar to the unreduced Poissonian formula, but in extreme cases at least—I venture to think—simpler. For the sake of simplicity and uniformity, I have thought it best to exclude the less familiar Poissonian solutions (see Part I.) from a Praxis designed for the use of the lay reader.