

JOURNAL

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"WILL-TRANSFERENCE."

FACT—OR FANCY?

BY THE REV. P. H. NEWNHAM.

In the very interesting and important paper on "Some Higher Aspects of Mesmerism," by Messrs. Gurney and Myers, as published in Part IX. of the *Proceedings* of our Society, and at the top of p. 417, we read the following sentence :—

"Again, we have probably all of us heard someone claim to have made someone else look round, in church or theatre, by fixing an intent gaze on him ; but such cases must clearly be reckoned as mere illusions of *post hoc propter hoc*, of successes noted and failures forgotten."

I am aware that it must appear presumptuous in me to challenge the deliberate conviction of such authorities on all questions of telepathy ; but I nevertheless feel bound, in the interest of what I conceive to be the truth, to place on record a very respectful *caveat* against the acceptance of this dictum.

The uniform experience of some 40 years convinces me that the power alluded to really does exist ; and I must acknowledge that I read the sentence above quoted with feelings of utter amazement, as the habitual exercise of this power has been a thing familiar to me as long as I can remember, and I have always accepted it as such a matter of course that I have taken for granted that everybody else recognised it also.

As a lad and youth of from about 14 to 22, I remember constantly amusing myself with it, especially at church. On two occasions I utilised it (once at the request of my tutor), in order to cure persons of a disagreeable habit of staring in church. I made them look at me, as soon as they had entered their pew, and kept on doing so until I had

succeeded in rendering them thoroughly nervous and uncomfortable. They never stared at our pew again!

I frequently made *practical* use of the power, in after years, in church, when I had forgotten something that I needed. (N.B.—*Not* my sermon!) The first occasion on which I remember doing this was about 1872, when I had left in my study some special form of prayer which was ordered to be used on that Sunday. When I discovered that I had come to church without it, I willed my gardener to look at me, which he did in a few seconds, and I then beckoned him up, and sent him for the missing paper. I have done the same thing during service time, scores of times, when I wanted a door or a window shut, or a person shown to a pew, &c., &c. I have *never* failed, except with one person, and with him I could only succeed about once in three times, as the more uncomfortable he grew, the more he kept his eyes sternly fixed upon his book.

I was once talking of this power in company, when a young lady expressed her doubts of its real existence. Being seated opposite to her shortly afterwards, I made her look at me six times in succession, carefully avoiding catching her eye until the sixth time, when I met her gaze and told her what I had been doing, and she at once acknowledged that she had found herself constantly looking up at me, without knowing why.

I regret exceedingly that I have kept no written memoranda of some of these experiments, but I never thought there was anything unusual in them, or anything that was worthy of record.

In 1873, I distinctly remember experimenting at a series of concerts to which I was invited. The company consisted of persons almost all of whom were entire strangers to me, and I tried the power almost exclusively on those who were sitting *in front* of me, and who, therefore, could not catch my eye by just lifting their heads. It was very interesting to see them first fidget about in their seats, and at last turn their heads round and look about them, as if to see whence the uncomfortable feeling that influenced them was proceeding.

I opened a correspondence with Mr. Myers on this subject, in February, 1886. I felt very strongly the objections against the *scientific* value of such more or less vague memories as I have above referred to, which are based upon no written memoranda, and are only recalled through the glorifying mists of personal gratification at success, as viewed in the vista of past years. And, therefore, although conscious that I no longer possess the power so fully as I used to do in former days, I resolved to make some fresh experiments, and carefully note the results.

Under my present mode of life, I can only make these observations

during the time of service in church. I have endeavoured to lay down certain conditions under which these observations shall be made. It will be seen that these conditions are such as to handicap the observer very heavily; and I think that successful results obtained under such conditions will fully establish my claim that the power is something that really exists, and merits scientific observation.

1. My first condition was, that no observations are to be made from the pulpit, or during any time in the service when the eyes of the congregation naturally turn, more or less frequently, to the speaker or reader. I observed only during the singing of hymns or canticles, when the eyes of the congregation are fixed on their books, and their attention occupied by the words that are being sung. I may add that my congregation is singularly musical, and takes a rapt delight in the singing.

2. My second condition was, never to attempt to influence those with whom I am specially intimate, or with whom I have been brought into *religious* communion, as in Confirmation and Bible-classes, sick and dying beds, &c., &c.

Under these conditions I made a series of observations on Sunday morning, February 7th, 1886, and wrote down the results as soon as possible after my return home. The original memorandum was completed, and dated, at 2.30 p.m. of that day. This memorandum, as furnished to Mr. Myers, contains the actual names of the persons experimented on; but, for obvious reasons, these cannot be printed. With this exception, the following schedule is a strict copy of the original draft; nothing new being inserted except a few connecting or explanatory words.

The experiments took place at five different times during the service.

I. *While singing the "Te Deum."*

1. Girl of 13. In the choir. Attends a Bible-class that I hold; but is shy, and not familiar with me. I have never attempted to "impress" her religiously, in the least. She always sings with her head bent down, and eyes on her book; and I have in vain attempted to cure her of this habit.
She looked up after about one minute, and *stared vacantly*.
2. Boy of 12. In the choir. Goes to school in the next parish. I scarcely know him, even to speak to.
Looked up in from 10 to 15 seconds.

II. *While singing the "Jubilate."*

3. Strange man: name unknown: about 35-40. Never seen before.
Looked up, *slightly vacant*, in about 20 seconds.

4. Young married woman, about 32. Used to sing in my choir nine years ago. Now lives in the Isle of Wight, and was on a visit to friends.
Looked up, *decidedly vacant*, in about one minute.

III. *While playing tune of first hymn.*

5. Unmarried lady, about 70. On a visit in the parish. I know her to speak to; but no more.
Looked up sharply, in about 20 seconds; first to right, then to left, as if to see where something was; then looked, *vacantly*, towards me.

IV. *While singing the first hymn.*

- 6, 7. Two women, unmarried, about 25 and 40 respectively, domestic servants in the parish. I only know them to speak to.
My eyes were looking just between these two, as they stood side by side, and I had scarcely determined which of the two to operate on, when both looked up simultaneously, certainly in not more than five seconds.
8. Boy, aged 10, attends school in another parish, and I scarcely know him.
Looked up, *vacant*, in less than 30 seconds.

V. *While singing the second hymn.*

9. Unmarried woman, age about 33; almost a stranger to me, as she lives out of the parish, and generally attends church elsewhere.
Looked up in less than 10 seconds.
10. Unmarried woman, aged 63. Rather hard in character; and somewhat antipathetic to myself and my parish work.
Began to fidget in 20 seconds; but kept her eyes fixed on her book, and sang steadily. I must have looked at her for two minutes, and was just on the point of giving up, when she looked up, *vacant*.

VI. *After Service.*

11. I wanted to speak to one of the churchwardens; but as it was the Sunday when I retained the offertory, I knew he would not come into the vestry. I therefore willed him to look at me as I was leaving the church. I did not look at him or towards him; I simply willed.

I passed into the vestry, some 25 or 30 feet from him, as he stood in the body of the church, and as I was in a line with him I turned my head, and he looked up and caught my eye at the same moment.

NOTE.—I consider this case, *per se*, as having no evidential weight, as he might have looked that way out of respect to the clergyman passing. I only record it as connected with the foregoing series of invariable successes. *Valeat quantum.*

I think that such a series of experiments as those which are thus recorded are sufficient to establish a *prima facie* claim to the careful investigation of my assertion, that this power of "will-transference" is something more than imaginary, and that there is something in it beyond mere coincidences noted and failures forgotten.

But, in experiments conducted under conditions such as these, there is still the possibility of coincidence to be carefully considered. A clergyman who occupies a conspicuous station in the church is, of course, a centre of mental gravitation, and it is probable that many eyes are constantly being turned in his direction. Of course, the selection of the intervals of singing for the experiments reduces the probability of such coincidences to a minimum, but still it is a factor that must be dealt with.

On February 21st, I therefore set myself carefully to note how many persons looked in my direction during the same intervals of the service as I had experimented in on the 7th. It was not a very easy experiment to conduct, as I had to keep my eyes always ranging round, without allowing them to rest long enough on any one person to have a chance of materially influencing them. It must be remembered, too, that if there be any telepathic influence thus transmissible, the very fact of my self-consciousness would predispose very sensitive subjects to absorb the influence beforehand, and so almost to anticipate my glance.

During the singing of the "Te Deum" one small boy looked at me. He is only seven years old, and never looks at his book, but is always staring round the church. Of the 50 persons within range of my eye, no one else even glanced up for a moment.

During the singing of the hymns about 70 or 80 persons were well within my range of sight. Of these, two looked up at me. One of these, a young man of 24, never sings, and never looks at his book during the singing. The other was a delicate and hysterical girl of 22, upon whom I should consider it unfair for me to attempt any experiment.

I imagine that some of my readers will say that I must have a "model congregation." I cannot help such a suspicion arising. Facts are facts, and my business is simply to record "facts as they are."

There is one point to which I must ask special attention. It will be observed that in six out of the first 10 cases I note that the look is more or less *vacant*. I find that in a large proportion of instances this is the case. The first glance is unintelligent. Even when the eyes of

the persons operated upon look me full in the face, their intelligence does not seem, for a moment, to recognise what they are doing. If I want to arrest their attention I have to make some distinct sign; on perceiving which they appear for the first time to become aware that they really are looking at me. In some cases this rolling of the eyeball, and the sightless stare that follows, is very marked. Surely this fact alone practically removes the question outside the region of coincidence, and indicates the first commencement of some form of hypnotism, induced by the transmission of the operator's will, pure and simple.

One very interesting experiment that I tried, later on, illustrates this point admirably. During the singing of one of the canticles at evening service, a few Sundays later, I experimented on a young lady of 14 who has been totally blind since she was about three years old. In a very few seconds I perceived her eyeballs roll, and then she mechanically turned her sightless orbs straight in my direction. It was a very touching sight; and I shall never forget it.

For obvious reasons, I do not care to multiply these experiments among my own parishioners; as I conceive that though the experiments have been few, yet their *invariable* success is sufficient to establish my claim to a full investigation of the question. But, happening to be in one of our cathedrals, one day in May last, in a city where I was a perfect stranger, I took occasion, during the singing, to experiment on nine different persons, who were sitting opposite to me—both males and females. Again in every case was I successful, in periods of time varying from a few seconds to not more than one minute.

It seems to me, I must confess, that with our present knowledge of ordinary thought-transference, and of the special will-transference of the mesmeriser, it would be strange if the phenomenon I have been trying to illustrate did not exist.

And, again, as we know that there is "no smoke without some fire," do we not need this simple fact as the foundation for the world-wide traditions of the "evil eye"; "malocchio"; "overlooking," and numerous other words of a like significance, which we meet with everywhere?

I therefore venture to put at the head of this paper the name of "Will-transference," as a necessary complement to the name of "Thought-transference"; and as designating what I would suggest as simply another "*mode of*" telepathic "*motion*."

[The substance of the foregoing paper was sent to the Editor of the *Journal* some months ago; but the MS. was unfortunately destroyed in the fire at the offices of the National Press Agency.]

MESMERIC EXPERIMENTS.

Last term a professional mesmerist, Mr. d'Auquier, gave some public entertainments at Cambridge, in the course of which were exhibited what he professed to be thought-transference between him and his subject, and hypnotic phenomena which seemed interesting. From what was known of Mr. d'Auquier, there appeared to be good reason for believing that he was a trustworthy person, who would honestly join in scientific investigation; and therefore Mr. Myers, Mr. Langley, and Professor and Mrs. Sidgwick thought it worth while to try to secure his services for private experiments. Mr. d'Auquier agreed to come to Cambridge for these experiments for six days, from Monday, January 3rd, to Saturday, 8th, inclusive, bringing subjects with him, for sixteen guineas. Besides those already named, Mr. Wingfield, Mr. Hodgson, Mr. Gurney, and Dr. Gaskell took part in the investigation. Mr. d'Auquier's subjects were a lad named Johnny, and a Miss N.

The thought-transference experiments were conducted as follows:—Mr. d'Auquier hypnotised Miss N., who then lay back in her chair breathing heavily. He took up his position, standing or sitting, at some distance behind her, and a paper was then handed to him by one of the investigators with the name of a playing card written upon it. He professed to concentrate his mind on this card, and after some time Miss N., who had a pack of cards in her hands, or on the table beside her, roused herself, and selected a card out of the pack—usually the one required. Sometimes she failed and often she hesitated between two.

On the first occasion Mr. Hodgson thought that the effect might be produced by a code of signals consisting of a combination of Miss N.'s heavy breathings with sounds made by Mr. d'Auquier. He communicated his suspicions to the rest of the party, and all watched. At each experiment the code became clearer to them. By Thursday it was completely known. Not only could every member of the party discover from the signals with more or less ease and certainty cards unknown to him, but the signals were telegraphed by Mr. Langley and Dr. Gaskell to an adjoining room and there automatically recorded, and the record correctly interpreted by Mr. Wingfield, who had not been in the room with Mr. d'Auquier and Miss N. during the experiment at all. It seemed needless after this to waste time in watching the transfer of diagrams which Mr. d'Auquier also offered to exhibit. The code for the cards was an ingenious and simple one. Mr. d'Auquier made slight noises—such as coughing, sighing, or rustling of paper,—counting between two such noises a number of Miss N.'s breathings which represented the card, after which the suit was similarly indicated.

Tests were applied with the object of ascertaining whether Miss N.

was really hypnotised during these experiments, but they were inconclusive. There is, however, no reason to think that she was not in a light hypnotic trance.

Johnny was undoubtedly a good hypnotic subject, but no thought-transference, real or imitation, succeeded with him. In his normal state he could read numbers in Mr. d'Auquier's eyes, but the movements which enabled him to do so were very obvious. Experiments were made with him with a view to elucidating the mental processes involved in recognising, in the hypnotic state, the spots to which hallucinatory photographs, &c., were attached; also to ascertain if Johnny exhibited any hyperacuity of vision such as that attributed to his subjects by M. Bergson of Clermond-Ferrand. The indications of this in Johnny's case were very slight. He appeared when hypnotised to distinguish the subjects of photographs prepared as microscopic slides rather better than people with normal sight, but not in so clearly marked a degree as to make it certain that there was any abnormality of vision. Experiments were also made on rigidity and anæsthesia induced in Johnny's hands by passes while he was otherwise in a normal state. In some of these the object was to observe the effect of different kinds of passes, and in others the order in which different sensations disappear. In others again the object was to ascertain whether, apart from suggestion, Mr. d'Auquier possessed any special power of influencing his subject by will or otherwise. Mr. d'Auquier seemed able to affect Johnny in at least a different degree from others, but it is difficult altogether to exclude suggestion when, as appeared to be the case here, contact in making the passes is indispensable; and probably it would in any case be impossible to make such experiments satisfactory except with an operator whose *bona fides* could be relied on.

CORRESPONDENCE.

To the Editor of the JOURNAL OF THE SOCIETY FOR PSYCHICAL RESEARCH.

SIR,—May I offer a few explanatory remarks in connection with Mr. Hodgson's dissection of my report in January's *Journal*?

After describing the first experiment, he proceeds to say: "I suppose that Eglinton held the slate alone for some time, during which he wrote upon the slate, afterwards requesting F. to assist in holding it; and this supposition is even suggested by the first part of Mr. Bentall's description."

Although Mr. Hodgson's "supposition" is not an unfair inference from my loosely-worded description, I beg to state that the meaning I intended to convey is that the word "Heybridge" was asked for *after* F.'s hand was holding the slate against the table. We are both positive that such was the case, and I submit that my report states nothing which is incompatible with such a view.

In criticising the phenomenon of the writing obtained between two slates held above the table, Mr. Hodgson, after quoting from the report, goes on to say, "I suppose that Eglinton first held the slates under the table, and wrote the words, and then no sound of writing having been heard, held them above the table, and asked F. to hold them with him."

In this case I do not see how Mr. Hodgson can justify his "supposition," which is diametrically opposed to my plain statement, that "the slates were held above the table in full view all the time." I bought the slates on purpose for this experiment, and arranged it with F. beforehand, our sole object being to get writing between slates held above the table. Eglinton did not hold the slates alone for a second, Frost extending his hand at the same time, and taking the opposite corner. Had they gone underneath the table, even for a moment, we should have looked upon the experiment as a failure, and I should have noted the fact, as I have done in recording another experiment in a passage which Mr. Hodgson has italicised. If Mr. Hodgson will consider that we both entered upon this experiment determined to have writing above the table, that the slates were our own, and produced only at the moment of experiment, and that Eglinton had not sole possession of them for a second, he will agree that to "suppose" that we allowed these slates to be manipulated under the table, and then were either ignorant of the fact, or knowing it, suppressed it in writing the report, involves a grave charge against either our veracity or sanity. I do not suppose for a moment that Mr. Hodgson wishes to make such a charge, but I cannot see how any other view can be taken by any one who reads Mr. Hodgson's remarks on this incident.

With regard to the tumbler experiment, I may say we did not look under the table at Eglinton's suggestion, but from curiosity.

I regret that my rough notes have not been kept. I made them on odd scraps of paper at Eglinton's after each sitting. They recorded the dates, the circumstances under which the manifestations occurred, and a copy of all messages. I need not say they were brief and not in a form fit for publication. The report was compiled from these from about 10 days to a fortnight after.—I remain, &c.,

F. W. BENTALL.

P.S.—Mr. Frost wishes me to state that he endorses the above remarks.

To the Editor of the JOURNAL OF THE SOCIETY FOR PSYCHICAL RESEARCH.

SIR,—With regard to Mr. Hodgson's criticisms, which have just appeared in the *Journal* of the Society for Psychical Research, on the reports of some slate-writing séances sent to the Society by Miss Symons and myself (Mrs. L.) and published in the June number, I must in justice to Mr. Eglinton make a few remarks.

Concerning the incident where a prompt reply was obtained, in answer to a question by Miss Symons as to how she could "best develop as a medium," Mr. Hodgson says, "I suppose that the question was directly or indirectly suggested by Eglinton." I must emphatically deny this supposition. Mr. Eglinton did not suggest the question or lead up to it. The single ordinary slates on which writing was obtained were always those purchased by

ourselves, on the day of each sitting. They were privately marked by us, and were examined thoroughly after every sentence of writing obtained, and previously to each question written by us.

The criticisms to which I refer consist so largely of supposition that they appear written with the desire to cast doubt by all possible means on the subject of slate-writing rather than to treat evidence in a fair and impartial spirit. Where Miss Symons says, "We locked the slate ourselves; it was never removed from the table or out of our sight for one single instant," the statement is supposed to be erroneous, because Mr. Harrold Murray, Mr. E. M. C., and Mr. F. Bentall, who describe experiments with the locked slate, say it was held under the table.

Our accounts of the slate-writing séances were always written the same day, or the next day after they took place, and we too have records of occasions when the locked slate was held under the table to obtain writing; but these occasions are quite distinct from the one when there was writing in the locked slate without its removal from its position on the top of the table.—Believe me, yours truly,

A. M. L., Associate S.P.R.

January 6th, 1887.

These two letters were shown to Mr. Hodgson, who sends the following remarks on them :—

To the Editor of the JOURNAL OF THE SOCIETY FOR PSYCHICAL RESEARCH.

SIR,—With regard to the two foregoing letters, I take the opportunity of again emphasising what I have apparently failed to make clear in my previous statements. But I shall first remark upon the second of the cases to which Mr. Bentall specially refers.

I pointed out, both at the beginning and at the end of my criticisms, that I did not profess to "justify" my suppositions, in the way that Mr. Bentall thinks I should be able to do. If the additional statements made by Mr. Bentall, and endorsed by Mr. Frost, concerning the determination with which they entered on this experiment, are correct, notwithstanding the lapse of a year and a-half since the incident occurred, I admit that the particular supposition which I made becomes less likely. Had this important determination been mentioned by Mr. Bentall when he wrote his detailed report a week or two after the event, I should probably have preferred another supposition.

I need not, however, dwell upon this point, since it is plain that the chief difference between the writers of the foregoing letters and myself concerns the trust which we are justified in placing upon certain positive statements made by witnesses under the circumstances involved in the cases before us. I question the accuracy of certain statements made by Mrs. L. and Mr. Bentall, whom I nevertheless suppose to be perfectly sane and veracious. The writers do not believe that their *bonâ fide* recollections of what occurred at the sittings can be so unreliable as I have supposed; but I submit that the amount of reliance which can be placed upon such recollections can be determined only by special investigation. Of course I expected that my suppositions would, in the first instance at least,

if not always, meet with the emphatic denial of the witnesses themselves; but I have thought it possible that readers of the *Journal* for January would see that in some cases the writers of the reports quoted by Mr. Davey have made positive statements which must have been as unreliable as I have supposed those of Mrs. L. and Mr. Bentall to be. I was present at some of the sittings the records of which are quoted in the *Journal* for January; I knew how the tricks were performed; and Mr. Davey and myself agreed concerning the chief mistakes made by the witnesses in their reports. These mistakes—mainly instances of memory-illusion—are as great as those which I have attributed to Mrs. L. and Mr. Bentall; I have no doubt that the persons who made them are veracious and sane; and I have no reason to suppose that they are inferior as witnesses to Mrs. L. and Mr. Bentall.

The "justification," therefore, which I offer for my suppositions which are "diametrically opposed" to certain statements of Mrs. L. and Mr. Bentall, is that I know, from the kind of evidence to which I have referred, that the true explanations of various conjuring performances which I have seen are equally opposed to the statements of the uninitiated witnesses. To make this contention yet clearer, I shall confine myself to a single instance, and shall quote a passage from the report of Mrs. Y., (*Journal* for January, p. 32), concerning Mr. Davey's locked slate.

"This test seemed to me *perfect*. The slate was under my own eye on top of the table the whole time, and either my daughter's hand or my own was placed firmly upon it without the intermission of even a second. Moreover, we closed and opened it ourselves."

This statement is erroneous. Similarly I have supposed the statement about Eglinton's locked slate, which Mrs. L. quotes in her letter, to be erroneous—though not for the reason which Mrs. L. attributes to me, as the reader may at once see on referring to the Supplement to the December *Journal*, p. 507. The question is not whether my particular supposition concerning Eglinton's dealings with the locked slate is correct or not, a question which I think we have no means now of answering satisfactorily, but whether such statements as that quoted by Mrs. L. about the locked slate may be as inaccurate as I have supposed. It was necessary for me to make particular suppositions for the purpose of exhibiting the magnitude of the errors which I had concluded might be made by the witnesses under the peculiar circumstances involved; and the reports quoted by Mr. Davey are enough to suggest that equally trustworthy witnesses are liable to errors of the magnitude which I thus exhibited.—I am, &c.,

RICHARD HODGSON.

To the Editor of the JOURNAL OF THE SOCIETY FOR PSYCHICAL RESEARCH.

SIR,—Mr. S. J. Davey informs us that the performances described in the last number of the *Journal* were "due to his own unaided powers as a slate-writing conjurer." Consequently, if we believe Mr. Davey, we are bound to believe that he can write not only on locked slates without touching them with either of his hands, but also that he can write what is in other men's thoughts, and that too in a language of which he himself knows nothing. But this is

not all. He can detect the word that lurks in another mind and write it on a locked slate, without himself being conscious of having done so. "He had forgotten," says one of his witnesses, "that Mr. Padshah had asked him to ask his name before tea." He had forgotten to do this part of his conjuring, but still the thing was done, and, as he himself testifies, by his own unaided powers.

I presume Mr. Davey wrote his paper to expose the tricks of Spiritualistic mediums, and I also presume that you admitted it into your *Journal* with the same object in view. Now I am a plain man and know nothing of Spiritualism, except what I have read about it, and I have earnestly clung to the hope that the S. P. R. would throw some light on the subject. But if my only alternative to believing in Mr. Eglinton is to believe that Mr. Davey's performances have been done by conjuring, I am placed, as it were, between Scylla and Charybdis. In this predicament is it unreasonable for me to ask that Mr. Davey will perform the same feats as are recorded in the last number of the *Journal* without the accompanying "tricks" of "electric shocks," "chattering of teeth," and so on; or at least without the Spiritualistic circle? And will he do this in the presence of some well-known Spiritualists, affording them proof sufficient to satisfy reasonable men that he performs his feats by his own unaided powers as a slate-writing conjurer?

That Mr. Davey is a medium of no ordinary power, his own account of himself makes, I think, tolerably certain. If, therefore, he either cannot or will not do what I propose, I must say for myself that if I am obliged to suspect any one of "imposture" it will not be Mr. Eglinton.—I am, your obedient servant,

GEORGE HARPUR,

Associate of the S. P. R.

[The above letter calls for a few remarks. (1) The writer seems to ignore the possibility of failure of observation and memory in *bonâ fide* witnesses. It is just because this possibility is so often more or less ignored that it was worth while for Mr. Davey to institute his experiments. (2) Mr. Hodgson has been present during some of the sittings, and having been initiated into the trick, has been able to observe the whole process. He is thus aware what the phenomena really were of which the descriptions puzzle Mr. Harpur, as they probably puzzled most of the readers of the January *Journal*. (3) It is proposed to discuss the subject further in the *Proceedings*, and to point out where the chief failures of some of the witnesses occurred; though, for obvious reasons, Mr. Davey does not wish to make his *modus* (or rather *modi operandi*) generally known. (4) Mr. Harpur's remarks are of importance, as showing that the question has at last been brought to the decisive point. The descriptions of Mr. Davey's performances were printed to show that *bonâ fide* records of tricks might closely resemble *bonâ fide* records which have been accepted as conclusive evidence for genuine occult phenomena; and there could not be a better proof of the closeness of the resemblance, or of the urgent need of its recognition, than the fact that on the strength of the descriptions the tricks should be supposed to be genuine.—ED.]

The Rev. W. S. Grignon has been good enough to sanction the printing of the following private letter:—

The Grove, Pluckley, Kent,

January 5th, 1887.

DEAR MR. GURNEY,—I have been reading with great interest, and on the whole with much satisfaction, the "Statement of the Literary Committee" in the *Journal* for this month. I hope it will succeed in keeping some of the pronounced Spiritualists in connection with the Society. But will you allow me, as a very cordial, though, I fear, very inefficient, supporter of the Society's work, to point out wherein, as it seems to me, you come short of meeting the fair claims of what I may call the Spiritualistic interest? I do so as an impartial bystander. My own connection with Spiritualism consists in subscribing to *Light*, and reading now and then a stray number of the *Medium*, and attending quite anonymously a public séance occasionally—perhaps on an average once in three or four years. I have further tried a few experiments at intervals of years in private séances, and made myself fairly acquainted with the literature of the subject. With the phenomena of Mesmerism or Hypnotism I have been practically familiar for thirty-six years past. Indeed, my recollections go back nearly ten years earlier—to the days when Baron Dupotet was making a sensation in London by his experiments. My personal experience of séances and mediums has not been satisfactory; still, I have convinced myself that results are attained from time to time which are not explicable by a simple theory of imposture. Of course, as a believer in the Christian faith, I am in one sense of the word a strong Spiritualist. Such is my standpoint. I fully agree with the distinction you make between Spiritualism as a religion and as a science, and I feel very sure that the hold modern Spiritualism will ultimately take, or fail to take, of the world will not depend upon strictly scientific evidence one way or the other. But it does strike me as a fair claim on the part of Spiritualists in our Society that scientific method should be strictly and quite impartially applied to the arguments on both sides of the question.

Now, a fairly careful study of Mr. Hodgson's and Mrs. Sidgwick's comments on cases of "slate-writing" has led me to gather that they build on the ground that the popular idea of the value of human testimony is much too high, the power of observation at the time, of recollection afterwards, and of selection of what ought to be observed and recollected, being much lower than it is commonly taken to be. Mr. Hodgson's explanations of psychographic cases in the *Supplement* to the December *Journal* come sometimes to this: "You say you tore a corner off the card; no doubt you think so; but I feel sure Eglinton tore it off." "You tell me you had hold of those closed slates from the moment you entered the room till you opened them yourself. My dear sir, you think so, but in point of fact you let Eglinton get hold of them and do what he would with them." Mr. Hodgson may be quite right. I say nothing to the contrary. I seem to myself to see clearly that, if he is right, our juries ought to have "reasonable doubts" in many cases in which they have none. I do not quite see how your and Mr. Myers' most interesting researches in Hypnotism can have any value at all in face of criticism framed on similar lines. But let that pass. I will assume the method to be sound. With Mr. Hodgson and Mrs. Sidgwick I will smile at the blunders of the common herd, myself included, who are so mistaken about the laws of evidence. But then I shall

also call upon those scientific purists in the matter of human testimony not themselves to bring forward any evidence which will not bear their own tests. The unscientific many may plead their ignorance, the scientific few are bound by their own superiority. *Noblesse oblige.*

Now with this feeling I turn to Mrs. Sidgwick's article in the last published volume of *Proceedings*. There, in the first paragraph of p. 47, I find myself called on to accept, solely on Mrs. Sidgwick's observation and recollection, a "discovery" which impugns Mrs. Jencken's truthfulness, and this a discovery not of facts visible and palpable, but of mental states and purposes of the medium. Into any conclusion about these the observer's own mental state enters as a very important and very indeterminate factor. I must say that it seems to me that this paragraph should have been backed up by a statement, as far as possible corroborated by the evidence of others, of every detail; or, better still, altogether suppressed. As it stands it is not science, but rhetoric. The same remark applies to the next paragraph. I am now in my sixty-fourth year and can remember all the disputes that gathered round Elliotson's dealings with mesmerism, and how savagely and contemptuously the medical profession assailed the facts and the men who brought them forward. To me the conclusion drawn by three American doctors in 1851, even though supported by their alleged experience of some unknown lady "who could exhibit all the phenomena of the sounds," is worth nothing as evidence. Then there is that extraordinary sentence describing the "raps" as "loud double knocks, acquiring a special sound from the table, floor, door, or other object on which they appear to be made." That knocks acquire a special sound from the objects on which they are actually made, we all know; how they can be affected by objects on which they only appear to be made, I fail to see, unless the difference lies in the fancy of the hearer. Rather they appear to be made on different objects, because they actually differ, and, on the theory of the three doctors, one is called on to believe that the three sisters could not only produce sounds in the way suggested, but could modify those sounds almost at pleasure, could serve up table-raps, door-raps, floor-raps, china-raps, &c., &c., out of their joints, much as the pie-man in *Pickwick* served up "beefs," "muttons," or "weals" out of kittens. Is there not something rhetorical rather than scientific in the way in which Mrs. Sidgwick slides over this serious difficulty? I am not surprised that convinced Spiritualists have felt hurt at this tone which more or less runs through the whole article, the more so because the rhetoric derives its real force from the already existent popular conviction that a medium is of course a humbug.

Again, the admirable method adopted by the Society in dealing with alleged phantasms hardly seems to have been applied here. So far as I could judge from the *Journals* and *Proceedings* every witness was in a perfectly friendly spirit allowed the opportunity of strengthening his evidence, if he could, where it seemed weak. I may be mistaken, but I feel that I am at all events impartial; and I cannot but say that the impression made on me is that there is a strong tendency somewhere to criticise destructively, and in no degree constructively, the testimony given to "physical phenomena."

No weight seems to have been given to what strikes me as a singular fact, that Eglinton, who, if a conjurer merely, must have for years been in daily and hourly peril of exposure, has nevertheless, so far as I can learn, never been "exposed." Nor has even his method, or any one of his methods, been clearly explained. Why does not Mr. S. J. Davey explain precisely *how* in each case he (Mr. D.) deluded his visitors? Such explanations would knock Eglinton's business on the head, if he be a mere conjurer.

You say most truly in your "Statement" that "persons to whom Spiritualism has long been a *faith*, will not care to respond to your appeal." I fear that many to whom Spiritualism is far from being a faith will hesitate to send their evidence into what will strike them as an atmosphere of hostile, or at least, sub-hostile criticism, where not the judgment only, but the will, of the critic will be against them. I could myself state certain simple observations and experiments which have served to convince me that the "physical phenomena" of Spiritualism are not always explicable on the theory of conjuring *pur et simple*. But, though I am tolerably thick-skinned, I do not care to be publicly told "My dear friend, allow me to hint to you, with the most diplomatic politeness, that you are little better than a fool."

Pardon the length at which I have written, if, indeed, you have had patience to read thus far. I venture to write thus because I esteem highly the work the Society has done, and can do in the future. Just now the Spiritualists and half-and-quarter-Spiritualists in the Society and out of it are like a shy horse when he sees a man approaching him whip in hand, not to lash him of course, only to flick a troublesome fly off his flank; still he does not like it. Could not a quatern of corn be somehow substituted for the whip?—Believe me, yours very truly,

WM. S. GRIGNON.

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- BARAGNON (P. Petrus) *Etude du Magnétisme Animal.* 2nd Edit. *Paris*, 1853
- BINET (Alfred, et Ch. Féré,) *Le Magnétisme Animal**Paris*, 1887
- DE SÉRÉ (Dr. G. Louis) *Application du Somnambulisme Magnétique au Diagnostic et au Traitement des Maladies de Nature*.....*Paris*, 1855
- GIBIER (Dr. Paul) *Le Spiritisme. Etude Historique, Critique et Expérimentale*.....*Paris*, 1887
- JOLY (Prof. Henri) *L'Imagination : Etude Physiologique**Paris*, 1883
- LAFONTAINE (Ch.) *Mémoires d'un Magnétiseur.* 2 vols.*Paris*, 1866
- LEGUÉ (Gabriel) *Urbain Grandier.* 2nd Edit.....*Paris*, 1880
- LIÉGEOIS (Prof. Jules) *De la Suggestion Hypnotique dans ses Rapports avec le Droit Civil et le Droit Criminel**Nancy*, 1885†
- LUCAS (Dr. Prosper) *Traité Philosophique et Physiologique de l'Hérédité Naturelle.* 2 vols.*Paris*, 1850
- NOIZET (Général) *Mémoire sur le Somnambulisme et le Magnétisme Animal**Paris*, 1854
- SENSO (Marc) *La Vérité sur le Magnétisme Animal*.....*Lausanne*, 1881
- TISSOT (Prof. J.) *L'Imagination : ses Bienfaits et ses Egarements surtout dans le Domaine du Merveilleux*.....*Paris*, 1868
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- GESSMANN (Gustav) *Magnetismus und Hypnotismus**Vienna*, 1886†
- KAANE (Dr. Hans) *Ueber Beziehungen zwischen Hypnotismus und Cerebraler Blutfüllung*.....*Wiesbaden*, 1885

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