

# JOURNAL

OF THE

## SOCIETY FOR PSYCHICAL RESEARCH.

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### NEW MEMBERS AND ASSOCIATES.

#### CORRESPONDING MEMBER.

DESSOIR, MAX, 27, Köthener Strasse, Berlin, W.

#### MEMBERS.

BERENS, REV. RANDOLPH, 14, Prince's Gardens, London, S.W.

BIDDULPH, THE LADY ELIZABETH P., 19, Ennismore Gardens, London, S.W.

#### ASSOCIATES.

COLCHESTER, REV. H. B., 1, Leinster Place, Paddington, London, W.

GELL, REV. A. W. HAMILTON, M.A., Winslade, Exeter.

### MEETINGS OF COUNCIL.

At a Meeting of the Council held on the 22nd of July, the following Members were present :—Messrs. George P. Bidder, Walter H. Coffin, Edmund Gurney, Frank Podmore, H. Arthur Smith and J. Herbert Stack. Mr. Stack was voted to the chair.

The Minutes of the previous Meeting were read and signed as correct.

On the proposition of Mr. E. Gurney, seconded by Mr. F. Podmore, Herr Max Dessoir, of 27, Köthener Strasse, Berlin, W., was elected a Corresponding Member.

Two new Members and two new Associates, whose names and addresses are given above, were elected.

One volume was on the table as a present to the Library, for which a vote of thanks was passed to the donor.

The Cash Account for the previous month was presented in the usual form.

It was agreed that the Council should meet again on Friday, the 29th.

At a Meeting of the Council held on the 29th of July, the following Members were present :—Messrs. Walter H. Coffin, Edmund Gurney, Frank Podmore, and H. Arthur Smith. Mr. Smith was voted

to the chair. The Minutes of the last Meeting were read and signed as correct.

At this and at the previous Meeting of the Council, and also at one or two subsequent Meetings of an informal character, the question of future arrangements as to premises was under consideration, and the results of numerous inquiries were before the Members.

It was finally decided to take a suite of rooms on the third floor, at 19, Buckingham Street, Adelphi, W.C.; an agreement for which, for three years from the 29th of September, 1887, with the option on the part of the Society of continuing the tenancy on the same terms for five or seven years, has been signed. The Library, &c., has now been moved to the new rooms.

It is desired to sub-let three of the rooms, unfurnished, at a moderate rent. Some reader of the *Journal* may perhaps know of a possible tenant.

#### SOMMEIL A DISTANCE.

The following is a letter written to me last winter by M. Ch. Richet.—Ed.

Je vais vous donner quelques détails sur les expériences de sommeil à distance que j'ai faites. Elles sont inédites; mais elles seront publiées d'ici à deux ou trois mois. Néanmoins, faites en tel usage que vous voudrez.

Vous connaissez Léonie (Mme. B); on en a parlé assez souvent et F. Myers l'a vue. Elle est venue à Paris, chez mon ami H. Ferrari, et j'ai pu alors faire sur elle diverses expériences. Je laisse de côté tout ce qui n'est pas sommeil à distance. Il y a eu assurément des faits curieux; mais ce qui est le plus important c'est le sommeil à distance. Voici comment j'ai procédé. J'ai d'abord essayé de la soumettre et de l'habituer à moi complètement—c'est-à-dire, pendant 15 jours à peu près, tous les jours pendant deux, trois, quatre, ou même cinq heures, je l'ai tenais magnétisée: Alors j'ai pu essayer (sans la prévenir) de l'endormir à distance, et j'ai eu la série suivante:—

|                          |                            |
|--------------------------|----------------------------|
| 1 <sup>o</sup> Exp. .... | Le succès presque complet. |
| 2 <sup>o</sup> Exp. .... | Demi-insuccès.             |
| 3 <sup>o</sup> Exp. .... | Demi-insuccès.             |
| 4 <sup>o</sup> Exp. .... | Un succès complet.         |
| 5 <sup>o</sup> Exp. .... | Un succès complet.         |
| 6 <sup>o</sup> Exp. .... | Succès incomplet.          |
| 7 <sup>o</sup> Exp. .... | Succès presque complet.    |
| 8 <sup>o</sup> Exp. .... | Succès presque complet.    |
| 9 <sup>o</sup> Exp. .... | Un succès complet.         |

Si l'on laisse de côté la neuvième expérience, qui est défectueuse pour une cause spéciale, que je connais, on voit qu'il y a eu une amélioration progressive, et assez régulièrement progressive, dans les résultats.

Je vais vous raconter la sixième expérience, qui est probablement la meilleure—c'est celle qui m'a le plus frappé, quoique je ne l'appelle que succès incomplet. Le soir (Lundi) entrant chez moi à 11 heures du soir, je

me décide à endormir Mme. B. le lendemain matin ; mais je ne l'avais pas prévenue de mes intentions, d'autant moins que je l'ignorais moi-même. Alors, le mardi matin, à 8 heures, étant encore dans mon lit, je tire au sort avec un jeu de cartes, pour savoir à quelle heure il faut l'endormir, en me laissant une incertitude allant de 8 heures du matin à 8 heures du soir. Le sort désigne 9 heures. A 9 heures de 9 heures à 9.10 je fais effort pour l'endormir ; puis pour ne modifier en quoi que ce soit les habitudes de Mme. B., je ne vais pas chez elle. A 1 heure je vois un de mes amis ; je lui raconte cette histoire, et je lui fais le simulacre de l'action du sommeil à distance de 1 heure à 1.20. A 5 heures je vais chez Mme. B. ; voici ce qu'elle raconte : A 9.5 du matin, pendant qu'elle s'habillait pour descendre déjeuner, elle est prise d'un mal de tête insurmontable, fatigue, lourdeur, paralysie des jambes ; elle ne peut pas se trainer. Elle descend cependant, estimant que cette malaise va se dissiper. Point. Elle augmente si bien qu'à 9.20 elle est forcée de remonter dans sa chambre, de se coucher tout habillée sur son lit. La somnolence dure ainsi, sans qu'on puisse la décider à descendre (à midi) déjeuner, jusqu'à 1.15, 1.30 environ ; alors elle tombe en état de somnambulisme, et elle y est encore à 5 heures, quand j'arrive, et que je me fais raconter par elle ces divers détails, sans qu'elle puisse rien soupçonner de mes intentions.\*

La septième expérience est vraiment excellente. Le vendredi j'arrive tard chez Mme. B. A 6 heures et un quart, comme je devais aller au théâtre, et qu'elle était encore très souffrante, je lui dis qu'il ne faut pas la magnétiser ; et je m'en vais. Mais à peine suis-je sorti de la chambre, étant en dehors de la porte, que je me repens de ma décision, et je dis : " Tant pis pour le théâtre et pour les nerfs de Mme. B. La science l'emporte ! je veux qu'elle dorme." Détails enfantins peut-être, mais qui indiquent bien que quand j'ai vu Mme. B. j'avais fermement l'intention de ne pas l'endormir. Donc rien dans mes paroles n'a pu la mettre sur la voie. Je fais ainsi, sans qu'elle puisse me voir ou entendre en quoi que ce soit, pendant vingt minutes de 6.25 à 6.45, l'ordre mental du sommeil ; puis, sans faire de bruit, à 6.45 je constate *de visu* que Mme. B., qui ne peut pas me voir, est endormie. Vers 6.40 elle s'était sentie prise d'un épais sommeil, avait trempé ses mains dans l'eau pour essayer de s'y soustraire (voyez son observation par M. Janet et par M. Ochorowicz), mais vainement ; elle n'avait pu résister au sommeil, et à 6.45 elle dormait profondément, le coude appuyé sur la table, quoique il y eut autour d'elle des personnes allant et venant.

Dans la huitième expérience le jour et l'heure ont été tirés au sort. L'heure désignée est 2 heures, heure qui est aussi incommode pour moi que pour elle. De 1.38 à 1.50 (étant chez moi) je fais effort pour l'endormir, et quand j'arrive à 2.5 je la trouve endormie, et disant qu'elle a été endormie par moi vers 1.45. (J'ai établi la concordance des heures en calculant le travail de couture qu'elle a fait depuis le moment où elle s'est mise au travail, 1 heure, jusqu'au moment où elle a cessé de pouvoir coudre.)

\* Il faut noter que cette expérience l'a tellement fatiguée qu'elle est restée malade, et assez malade, avec un atroce mal de tête et un état nerveux des plus pénibles, pendant *trois jours*.

Telles sont mes trois expériences importantes. J'espère que vous les trouverez satisfaisantes. Celle qui m'a le plus frappé c'est la sixième, mais je crois que la septième est la meilleure. Faites-en tel usage que vous voudrez, et si vous avez besoin de quelque supplément d'instruction, je serais heureux de vous l'envoyer.

In answer to inquiries, M. Richet adds:—

Quant à l'action à distance sur Mme. B., je vous dirai que depuis le jour où je l'ai vue, c'est-à-dire le 26 Decembre jusqu' au 26 Janvier, pas une seule fois elle ne s'est trouvée en état de somnambulisme en dehors des cas où je l'endormais volontairement (près d'elle ou à distance). Deux fois, cependant, elle s'est endormie dans les conditions suivantes: Une première fois elle était à côté de moi, en voiture, et je n'avais pas l'intention de l'endormir. Malgré moi et malgré elle, probablement, elle s'est endormie; mais cela n'est pas étonnant, car j'étais à côté d'elle, et pendant tout ce temps il est possible qu'il y ait en une sorte d'auto-suggestion de sa part, et une suggestion inconsciente de la mienne.

La seconde fois qu'elle est tombée en état de somnambulisme sans que je le veuille, c'est le 25 Janvier, la veille de son départ. J'arrive à 2 heures et je la trouve endormie, les yeux ouverts, ne répondant pas aux questions. Elle tenait à la main une montre en or (qu'on l'avait donnée la veille) et elle regardait fixement le couvercle. Elle m'a dit que ce qui l'avait endormie, c'était la montre; et en effet elle est sujette à être ainsi actionnée par les objets brillants, et l'or lui donne des crises, si bien qu'elle ne peut pas entrer dans la boutique d'un bijoutier sans avoir une sorte de crise hystéro-épileptique.

Voici les dates des expériences de sommeil à distance, avec les heures, et les effets obtenus (à l'heure indiquée au dernier tableau).

|                     |           |    |     | Effort d'action. |           | Effet<br>ressenti. | Retard.        |
|---------------------|-----------|----|-----|------------------|-----------|--------------------|----------------|
| 1 <sup>o</sup> Exp. | Mercredi  | 12 | ... | de 9h.           | à 9h. 10  | ...                | 9h. 20 ... 10' |
| 2 <sup>o</sup> Exp. | —Vendredi | 14 | ... | de 3h. 10        | à 3h. 45  | ...                | 3h. 30 ... 20' |
| 3 <sup>o</sup> Exp. | —Samedi   | 15 | ... | de 11h. 1        | à 11h. 8  | ...                | 11h. 4 ... 3'  |
| 4 <sup>o</sup> Exp. | —Lundi    | 17 | ... | de 11h.          | à 12h. 4  | ...                | Rien. ... —    |
| 5 <sup>o</sup> Exp. | —Mardi    | 18 | ... | de 11h.          | à 11h. 25 | ...                | Rien. ... —    |
| 6 <sup>o</sup> Exp. | —Mercredi | 19 | ... | { de 9h. 11      | à 9h. 26  | ...                | 9h. 18 ... 7'  |
|                     |           |    |     | { de 1h. 15      | à 1h. 40  | ...                | 1h. 35 ... 20' |
| 7 <sup>o</sup> Exp. | —Vendredi | 21 | ... | de 6h. 20        | à 6h. 52  | ...                | 6h. 45 ... 25' |
| 8 <sup>o</sup> Exp. | —Lundi    | 24 | ... | de 1h. 38        | à 1h. 50  | ...                | 1h. 45 ... 7'  |
| 9 <sup>o</sup> Exp. | —Mardi    | 25 | ... | de 6h. 55        | à 7h. 10  | ...                | Rien. ... —    |

Ainsi sur sept expériences il y a eu sept fois un retard—qui est en moyenne de 12 minutes.

Si l'on prend la moyenne des retards dans les dix principales expériences de M. Janet et de M. Gilbert, on trouve un retard qui a été de neuf minutes en moyenne.

L'identité très saisissantes de ces deux chiffres, et la constance du phénomène retard, indiquent que n'y a pas là une simple coïncidence. Ainsi sur 17 expériences de Janet et de moi, 17 fois il y a eu retard et pas une seule fois avance. N'est-ce pas un bien bon argument contre l'hypothèse de hasard? Qu'en pensez-vous?

CHARLES RICHTER.

## CORRESPONDENCE.

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

DEAR SIR,—In my paper entitled “The Possibilities of Mal-Observation from a Practical Point of View,” published in Part XI. of the Society for Psychical Research *Proceedings*, I called attention on pp. 415, 416, to the following editorial note in *Light*, September 4th, 1886 :—

“If he [Mr. Davey] or any other conjurer can produce the *appearance* of the conditions which he *seemed* to observe with Mr. Eglinton, and the writing under such *apparent* conditions, so as to induce an inexperienced witness to write such a report as those he wrote himself, it will be time enough to talk of mal-observation as a possible explanation.”

If it can be pointed out in what way I have failed to carry out the above prescription in the essential details of my experiments with certain inexperienced witnesses to my conjuring performances quoted in the *Proceedings*, Part XI., I shall then be happy to devote further time and study to the question.

In the meanwhile, as no such attempt to my knowledge has been made, nor to answer the clear charges of imposture already recorded against Mr. Eglinton, a further expenditure of time on my part seems unnecessary, for the conclusion to be drawn is obvious.

I have been asked to reproduce certain extraordinary results in the presence of those who claim to have witnessed them with Eglinton.

Apart from other considerations, it does not seem to have occurred to those who have recently made this proposition that it would be just as difficult for me to duplicate under similar conditions certain avowed conjuring performances, as for instance the following trick performed by Harry Kellar, the American conjurer, before three of the Seybert Commissioners. (See the “Seybert Report on Spiritualism,” pp. 78, 79.)

“Professor Thompson was asked to write a question, which he did while the side of the slate on which he wrote was turned away from Mr. Kellar.

“The slate was not turned over, the written question remaining on the under side, and it was held at the usual place under the table, Mr. Kellar’s thumb remaining above the table in full view, while the fingers held the slate up under the table.

“A moment after the placing of the slate under the table, it was withdrawn to admit of a small pencil being placed upon it, Mr. Furness having remarked the absence of the pencil. The slate was not otherwise withdrawn from under the table above two inches until its final withdrawal, and the question was always seemingly on the under side.

“When the slate was brought out a communication was found upon it in answer to Professor Thompson’s question. The answer was on the upper side of the slate.” (Mr. Kellar afterwards revealed his methods to Dr. Howard Furness.)

I may also point out that there are, undoubtedly, methods for producing slate-writing unknown to general conjurers. I have already quoted on pp. 412, 413 *Proceedings*, Part XI., the testimony of a well-known professional conjurer in regard to certain experiments I performed before him. I may

now supplement this by quoting the testimony of Dr. James Henry Lewis, an experienced amateur conjurer, with whom, personally, I was unacquainted prior to the séance.

Staines.

June 16th, 1887.

Yesterday I attended a séance given by Mr. S. J. Davey, of Beckenham, for slate-writing. The results were simply marvellous. Writing was produced on a locked slate with a genuine Bramah lock, and also between two ordinary school slates unprepared, and which I thoroughly examined, tied together myself, and sealed. Although I know nearly every conjuring trick as applied to slate-writing, I feel quite convinced Mr. Davey did not use any of these, and without an explanation from him I am at a loss to account for the means of production.

JAMES HENRY LEWIS, Ph. D.

The following is the evidence of an amateur conjurer of exceptional ability :—

“ I have to-day seen Mr. Davey produce pencil marks within Mr. Spielman’s folded slate,\* padlocked, corded, and sealed, and having rebates in the frame, so that nothing can be introduced between the frames, and I confess that I cannot imagine any possible method of doing so.

“ GEORGE HERSCHELL, M.D.

“ June 25th, 1887.”

Eglinton has asserted in *Light*, October 16th, 1886, that he refused an offer of 2,000 guineas per annum to perform his manifestations nightly upon a public platform, and he instances this as a proof that his manifestations are not the result of conjuring.

A similar munificent offer was recently made to myself, if I would repeat before a public audience certain slate-writing tricks I had performed in a private studio. I declined this offer upon the ground that conditions essential for the success of my performance could never be obtained in full sight of a public assembly.—Yours, &c.,

S. J. DAVEY.

August 24th, 1887.

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

SIR,—I wrote to you in June last, but as you informed me “ the July number was made up before the arrival of my letter,” I now write again, and shall be glad if you can find room for this letter in your October number.

I need hardly say that I read the last *Proceedings* with great interest, but I am free to confess that I cannot even now, after this rather long interval of time, see the object of your efforts to show that Eglinton is an impostor. Even if you succeeded in doing so (and as yet you are very far from having

\* This slate was specially prepared by a gentleman, with whom I am unacquainted, for a séance with Eglinton. I understand that Eglinton had failed to produce any result whatever, and it was shown to me as being apparently trick proof. After Dr. Herschell had secured it, as he describes, I requested a few seconds non-observation to produce the result referred to.—S. J. D.

succeeded—at least, in my judgment), what would you have gained? I presume it is not suggested, even by Messrs. Hodgson and Lewis, that *all* the “mediums” that ever appeared have been the clever conjurers that they would have us believe Eglinton is. Spiritualism does not stand or fall with Eglinton, as you and some writers of the Society for Psychical Research seem to imagine. Even if you could show to demonstration that he is all which you suspect him to be, you would not have taken one step in advance. You do not, I presume, expect your readers to believe that girls of fourteen, for instance, can write in locked slates answers to questions which they have never heard or seen, and in languages which they don’t understand, by their own unaided powers of conjuring; and, if not, what advantage would you have gained if you could succeed in showing that Eglinton can do so? “His phenomena may be taken as typical,” you say, “of the evidence on which the Spiritual belief rests, and an examination of his claims is, therefore, an important step in the examination of the subject.” This sentence is the key-note of all that follows, and its argument is founded on a fallacy. Whately would have denounced it as involving an “undistributed middle,” and Mill as a “fallacy of ratiocination.” If it can be shown that Eglinton performs his feats by conjuring, then his phenomena are *not* “typical of the evidence on which the Spiritual belief rests,” unless it can be shown that other mediums—the girl of fourteen, for instance, or the uneducated female—are at least as likely as he is to do theirs by conjuring also. And this, I presume, no one will attempt to show.

I am not concerned to defend Eglinton. I have no personal knowledge of him whatever, but I have, I hope, an Englishman’s love of fair play, and fair play is just that which in my judgment he has not received at your hands.

In a former letter I asked that Mr. Davey should do his feats in the presence of some well-known Spiritualists. I find, however, that Mr. Hodgson, who appears to be his “guide, philosopher, and friend,” will not listen to such a proposal. He is afraid of the “psychical condition of the Spiritualists.” That condition “might be a bar to Mr. Davey’s performances,” he says. Well, sir, here we have surely discovered a new thing under the sun—a conjurer depending for his success on the psychical condition of his audience! If this does not out-Herod Herod, I should like to know what does.

And in the last number of the *Journal*, I perceive that an answer has been discovered that will meet any and every question that may be raised as to the difficulty of accounting for Eglinton’s phenomena, viz., Mr. Davey can do something quite as wonderful, or even still more wonderful by mere conjuring. And when Spiritualists modestly ask to be permitted to witness those feats of skill, they are met with the rebuff—“No, you must take our word for it. We have no reason to regard you persons as experts for the purpose of this inquiry. True, *we* do not take *your* word for what is done in Eglinton’s presence, but that is a different matter altogether. We are experts, and therefore every investigation for the subject of this inquiry must be reduced to the well-known formula—Heads we win, tails you lose.”

In a second letter I asked how the one word Boorzu was written. I

confined my inquiry to one word in order that I might get, if possible, a definite reply. And now it appears that after all the flourish of trumpets with which that word was paraded before our wondering eyes, no such word was ever written at all. It was "books," we are told, and not Boorzu, though what the word "books" happened to be doing there at that particular time is another of the mysteries that still remain unfathomed. There is nothing whatever about books in the context. But whether it was books or Boorzu we ought clearly to be thankful for its appearance, inasmuch as it has afforded Mr. Hodgson an opportunity of enlightening us as to the classes and degrees into which conjuring may be divided. These degrees, it seems, are three, and of these three the highest and most wonderful is that in which the spectator does the conjuring for himself, and stands astounded at his own mistake. Something has happened by chance, as, for instance, it chanced that books looked like Boorzu. It is not owing to the conjurer's design or foresight or cleverness that the thing has taken place. He deserves no credit for it whatever it is, and the great triumph of his craft is to accept the credit which he knows he does not deserve, and assent by his silence to what he knows to be untrue. Mr. Davey wrote "books," and Mr. Padshah mistook the word for Boorzu—one of his own names known only to himself, and the crowning act of conjuring consisted in reaping the fruits that sprang from Mr. Padshah's mistake and pocketing the gains. So much for Mr. Hodgson.

As to the séances of Mr. Lewis with Eglinton I turned to them, expecting to find the charges of imposture so frequently made in your pages—not established indeed, for I had met with no evidence to lead me to the belief that those charges could be established—but at all events I expected to find some new light thrown on the subject—something said to strengthen the suspicion which may perhaps not unnaturally lurk in the minds of many of your readers, I was disappointed, however, and with your leave I will state the reasons why I was disappointed.

Mr. Lewis undertakes to show how Eglinton's writing is done in one position only, viz., with the slate held under the table. Now if we are to believe scores or rather hundreds of witnesses writing is often done in Eglinton's presence while the slate is *on* the table, or held out from it between Eglinton's hand and that of one of the sitters. But these conditions, which are by far the most satisfactory and convincing, are altogether ignored by Mr. Lewis; and thus his whole performance reminds one of the play of *Hamlet* with the Prince of Denmark left out.

But still we may take his account, such as it is, and see what it amounts to. I will ask your attention to the well-known book and slate "trick." Mr. Lewis took a book at random from the shelves and then wrote on a slate, "p. 27, line 13, word 2 red, 3 white, 4 blue." He then turned the slate upside down and called Eglinton into the room; the latter having entered, put three bits of chalk, red, white, and blue, with the book on the slate, "put the whole under the table, and taking my left hand in his left, began the séance."

Now let us just remember that this was done in broad daylight, and that Eglinton had only one hand to hold both book and slate under the table.



In order to perform the trick, he had in the first place to turn the slate over so as to see the words on the other side, then he had to open the book, and as it was not likely he could open it just at the page required, he had to turn over the leaves till he came to p. 27. This being done, he had then to count thirteen lines from the top of the page, and find out the second, third, and fourth words in the thirteenth line. Having done all this he had then to select his piece of chalk three times in succession so as to write each word in the prescribed colour.

Well, the thing was done. Mr. Lewis does not deny the fact, and what has he to say about it? This is what he tells us. After the first pencils had been jerked off the slate, I suppose by Eglinton's "shudderings," "fresh bits were placed along-side of the book," and the séance began *de novo*. "He" (Eglinton) "pushes his arm with the slate far under the table and then, bringing it back towards him again, looks down" (as if to read the book). "Now he brings the slate up against the under side of the table and puts thumb above. I now hear the sound of writing."

This is all that Mr. Lewis has to tell us about the book and slate trick, and he is astounded at the simplicity of the performance. It was all clearly done by Eglinton's thumb-nail during that spasmodic thrust of his whole hand under the table! Mr. Lewis does not tell us how long Eglinton's hand was under the table; so it may have been half-an-hour or half a second. Perhaps it did not suit his purpose to say how long; vagueness suited it better. At all events he has presented us with a picture well calculated to excite our admiration. There sits Eglinton looking down with a quiet eye, examining the slate, turning over the leaves of the book and deliberately choosing out his bits of pencil, while Mr. Lewis sits furtively watching him with the "tail of his eye" in dumb wonder. And all this time, good easy man, it never seems to occur to him to "look down to see what Eglinton is doing." Is he restrained by a high sense of honour and afraid of being caught eavesdropping? Certainly, to an ordinary reader like myself who am ignorant of the ways of experts, it does seem a little singular that when he suspected Eglinton to be carefully turning over the leaves, or scrutinising the page, it never occurred to him to take a peep even in that direction. Or shall we think that he was too busy writing notes to have time to look after what Eglinton was doing under the table? And does this also account for the fact that he did not join with Eglinton in holding the book and slate? This is almost invariably done, I understand, and Eglinton never objects to it. Now if Mr. Lewis knew that he should not be able to take this needful precaution in consequence of his being otherwise engaged, why did he insist on being alone with Eglinton? Why did he not take a friend with him who could have either written the notes or helped to hold the book and slate against the table, and so have made it impossible for Eglinton either to read the writing or open the book? Why did a professional conjurer adopt this slipshod way of investigating a great subject? I think I can tell you why. Mr. Lewis doubtless loved the discovery of truth much, but he loved the display of his own shrewdness and cleverness more. But what could add a taller feather to his cap than to succeed in unmasking the world-renowned medium, Eglinton? Mr. Lewis did not take the most ordinary precautions against

imposture, and why? Why, simply that he might be able triumphantly to tell us when and where acts of imposture might have been perpetrated.

But, after all, what does Mr. Lewis's account prove, granting it to be correct and trustworthy in every respect? It is clear that his experience with Eglinton is an exceptional one. Eglinton's conduct, as described at the séance—his thrusting his whole hand some eight inches under the table—his stooping down and looking earnestly at the slate, &c., could not have escaped the notice of the most careless observer, and it is incredible that he could have acted in this way in the presence of the hundreds of witnesses who have recorded their experience. Mal-observation on their part is out of the question. Either those witnesses have told us what they knew to be false, or Mr. Lewis's experience is an altogether exceptional one. Granting it, therefore, all that you or he can claim for it, it does not go one hair's-breadth towards proving the non-reality of psychography in Eglinton's presence. The most it can prove, then, on the most uncharitable hypothesis, is, that Eglinton is not above having recourse to trickery when his psychical powers happen to fail him.

But if I were a Spiritualist I should (in view of Mr. Lewis's experience being an exceptional one) adopt another line of argument, which, indeed, is easily pooh-poohed, but, on the Spiritualistic theory, is sufficiently valid for all that. Mr. Lewis was alone with Eglinton—he had insisted on being alone. Here, then, we can conceive a struggle for the mastery between the two personalities (or whatever the word ought to be) and the weaker being dominated by the stronger. If this be so, and if the superiority was on the side of Mr. Lewis, as it probably was, we can easily conceive that Eglinton, though normally an honest man, was, when partially in the trance state, controlled for the time being by a spirit of frickery.

And now, sir, I should like to make a proposal, or challenge if you prefer to call it so, by way of conclusion. Mr. Davey has revealed to Messrs. Hodgson and Lewis the secrets of his conjuring feats, and the *modus operandi*, &c. As an ordinary reader, and as far as possible from being an "expert," I find it exceedingly difficult to believe that Mr. Davey's tricks are done by mere conjuring. It is clear that he is what is called a "medium" or psychic, and it seems difficult to decide where his conjuring powers end and his psychical power begins. Now I will not challenge Mr. Hodgson to do the "tricks" which he says he has learned to do. He has not perhaps had practice, and I suppose practice is indispensable as well as theory. But Mr. Lewis is just the man for the purpose. As a professional conjurer he cannot plead that his brain is too sluggish and his hands too clumsy to learn to do any "trick," however difficult, with a little practice. I propose, therefore, that in order to settle this question, Mr. Lewis will perform those feats which you tell us Mr. Davey performs by mere conjuring. I do not suspect Mr. Lewis of being a medium, and if he succeeds in doing all that Mr. Davey is reported to have done it will be necessary to admit that it has been done by conjuring. If he declines to do this, I would suggest that arguments against Eglinton, drawn from Mr. Davey's performances, should no longer occupy the pages of the Society for Psychical Research.—I am your obedient servant,

GEORGE HARPUR.

[Mr. Harpur cannot have studied with much care the notices of the Eglinton phenomena to which he refers, or he would not have confounded Professor Carvill Lewis with Mr. Angelo J. Lewis, or described either gentleman as a "professional conjurer"; nor would he have failed to observe that, as long as Professor Carvill Lewis obviously paid strict attention to what was going on, nothing remarkable occurred (which accords with Mr. Angelo J. Lewis's experience, as recorded in the *Journal* for June, 1886); and that it was only when he simulated the non-attention of which Mr. Harpur complains that he obtained any "phenomena" at all.

Mr. Harpur's question as to the *gain* of showing Eglinton to be an impostor is easily answered. The gain is the discovery of the truth in respect of certain phenomena, the nature of which the Society for Psychical Research expressly set itself to probe. In the original programme of the Society, Mr. Harpur will find an expression of the belief of its founders that "amidst much illusion and deception" a nucleus of genuine phenomena might perhaps be found; and in every branch of the research the first obvious duty of the investigators has been to separate the phenomena due to illusion and deception from those (if any such there were) which had some other origin. For further reply on this head it will be enough to quote the sentence of mine which Mr. Harpur has misquoted. I say that "Eglinton's phenomena may fairly be taken as typical, so far as professional mediumship is concerned, of the evidence on which the Spiritualistic belief rests." Mr. Harpur substitutes "Spiritual" for "Spiritualistic," and omits the essential clause "so far as professional mediumship is concerned."

Mr. Harpur seems to have missed the instructive point of the Boorzu incident. What amount of adroitness and presence of mind Mr. Davey displayed is a comparatively unimportant question. What is important is that an apparently inexplicable phenomenon—one which was expressly emphasised as inexplicable by Mr. Harpur himself—proves to admit of an extremely simple explanation.

Mr. Harpur speaks of a girl of fourteen who can write in locked slates answers to questions which she has never heard or seen, in languages which she does not understand. If he really possesses evidence of such a case, it is to be hoped that he will lose no time in sending the details, properly authenticated, to Professor Barrett, the hon. secretary of the Committee for the Investigation of the "Physical Phenomena" of Spiritualism.—Ed.]

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

SIR,—To show how difficult it is to arrive at any conclusion as regards Spiritualism, it should be noted that not only do the phenomena of the séance-room seem in a state of flux, but also the opinions of persons who are apparently calm investigators. It may be that the very fact of such people unaccountably changing front while without doubt being perfectly honest about it, helps to show that there is something supernatural in Spiritualism. I am now referring especially to the letter of Mr. J. G. Keulemans on professional mediumship, in the *Journal* for last June.

Some two years ago now or more, being anxious to inquire into Spiritualism,

I was advised to call at a house in Great Portland-street, on a certain evening, when one of the members of the then Spiritualist Alliance would be there to give advice to inquirers.

I went round with a friend, Mr. O. A. Fry (of 4, Hare Court, Temple, barrister-at-law), who was as keen about the subject as myself, and we set out, as many have done before us, determined to sift the subject to the bottom, if it should cost us weeks of time.

The evening we went round, we happened to find Mr. Keulemans present, who gave us much kind attention, and answered our crude questions at length.

As neither of us knew anything of the subject what we gathered was forcibly impressed upon us—at any rate I can speak for myself; and what we were then told by him differed considerably from what is to be read in his present letter. As the difference is more in gradation of opinion than actual divergence, perhaps he would not be sorry to be reminded of his former more hearty belief, and others may see in his change of opinion a reflection of their own.

I cannot pretend at this distance to recall all that was said in a two hours' talk, but there are some things which, from their novelty at the time, have not been forgotten either by Mr. Fry or myself.

On referring to the letter in the *Journal*, it would appear, though it does not actually say so, that Mr. Keulemans has no strong belief in the genuineness of the manifestations of John King and other so-called spirits.

I can well remember Mr. Keulemans telling us that John King was well known to him (Mr. Keulemans), and that he looked upon him in the light of a friend, and on Mr. Fry asking the probable cause of J. King's death, Mr. Keulemans replied that he would not think of hurting his feelings by asking such a question of John King, seeing that the life he had led on earth, as a buccaneer, might have been terminated in a manner unpleasant to acknowledge, and he added that though John King had left the body, yet that was no reason why he should be treated with disrespect; he also told us that he had observed a distinct improvement in the moral character of John King, and that such being the case it was not unlikely at some future date, sooner or later, he would leave the sphere he was now in for a higher one, and we might then see him no more.

On the walls of the room in which we were sitting were numerous pictures of spirits, among whom figured John King, and these were drawn by Mr. Keulemans himself, "from life," as he told us. Does he now think his sitters were genuine spirits or was he "taken in" by appearances? One may hardly believe this in so calm an inquirer as Mr. Keulemans is.

About one spirit, however, Mr. Keulemans can have but little doubt, I think, and that is the spirit of the Dutchman who used to come and visit Mr. Keulemans while he was at work, and who used to materialise a voice and try to persuade Mr. Keulemans to come out and drink, so that by a part obsession he might gain a glimpse once more of a pleasure that was now beyond his reach.

This was, be it noticed, told to us most circumstantially, and neither I nor Mr. Fry am likely to be mistaken about the narration of the episode.

Has the Dutchman, too, gone where the rest of the broken idols are?

Again, what has happened to the belief in the spirit drapery which Mr. Keulemans told us he had cut off from the garments of the materialisations and had placed in a glass box, and had watched the piece gradually melt away and disappear? Nor can I forget the story of the little spirit child, who used to come and sit on Mr. Keulemans' knee and play with his watch and chain.

Mr. Keulemans says in his letter he believes that genuine spiritual phenomena occur sometimes. The above cited cases perhaps are what Mr. Keulemans refers to, but with his advantages he would be doing good service to bring prominently forward these experiences of his. It must be remembered that he was talking to novices when he had this conversation with Mr. Fry and myself, and to more experienced persons he might have more to tell.

There is no better time than the present to come forward, and he will have no lack of listeners.—I remain, Sir, obediently yours,

A. G. WITHERBY.

4, Hare Court, The Temple, E.C.

I have read Mr. Witherby's letter (printed above) and can fully corroborate every word of it. Either Mr. Keulemans attempted wilfully to deceive us, or else he is now deceiving the readers of your journal, or else his opinions have undergone a most remarkable modification. In addition to what Mr. Witherby has written, I should like to ask Mr. Keulemans what has become of the little girl spirit which used to sit on his knee, and say to him by way of proving its spirituality, "Put your finger in my eye." According to Mr. Keulemans he used to do so, and the eye would then dematerialise and allow Mr. Keulemans' finger to pass into the child's spiritualistic skull at pleasure. This "fact" Mr. Keulemans gave us as "proof positive" of the spiritual existence. I cannot say that Mr. Keulemans did actually deceive us in all these matters, but we talked to him with open minds, and denied nothing nor admitted anything.

OLIVER A. FRY (M.A. Oxon.)

Temple, E.C., July 22nd, 1887.

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

DEAR SIR,—“H. G. R.” has been studying expectation in the percipient. I considered it only in the agent, in the letters to which he refers. “H. G. R.’s” observations of himself would not, therefore, in any case conflict with my theory. If the percipient’s expectation of the event facilitated the telepathy, it would be difficult to prove it, because such expectation would also tend to generate hallucination. Moreover, in spontaneous telepathy such general expectation as would correspond to “H. G. R.’s” expecting to guess in experimental thought-transference would be hard to distinguish from the *malaise*, the effect of telepathy. But expectation is not essential in the percipient, though it is quite worth while to observe if it has any efficacy.

With regard to "H. G. R.'s" agents, I must gather broadly that where their condition of mind is not stated it corresponded to the percipient's, and that there is an accuracy as to it sufficiently close to be at any rate in concordance with the general accuracy of conception of expectancy itself. It has now to be shown that the facts stated by "H. G. R.," though apparently opposed to, are really in agreement with, my views.

It is well not even to seem paradoxical. Let me, therefore, before explaining how this may be, cull two texts from authority. I once, in a hospital ward, heard read a chemical analyst's report. The words occurred, "Sugar is there, I believe." The physician present said, "That means he has considerable doubt about it."

And here is the converse, the hackneyed

"There lives more faith in honest doubt,  
Believe me, than in half the creeds."

This rises to the dignity of a psychological truth. Real belief is in inverse ratio to conscious belief. Real belief is an unconscious thing. We had it when we were unconscious that we had it. In knowing that we have it, we know that we are habitually unconscious that we have it; though in the few moments we thus allow ourselves to think and know, our belief trembles, even though it be the belief that 2 and 2 make 4. But in doubt there is no conscious belief: hence we may expect to find unconscious or real belief. All this becomes more obvious if for "belief" be substituted "unconceived negative." For what is the pseudo-belief of the analyst above mentioned and of "half the creeds"? It is a conscious belief. The "unconceived negative" is conceived, therefore, in it. It is, in fact, one conception existing in the mind with its negative in antagonism to it, belief—belief, or more exactly a quick alternation of mutually exclusive beliefs. And what is doubt, and in particular experimental doubt? It is positive and negative conceptions existing in the mind, not in antagonism, belief and belief; but so that each may be conceived as possible and as true, and, at pleasure, the one unmolested and unnegated by the other.

To apply this. It appears that "H. G. R.'s" agents were successful with him in first experiments, though they did not expect to be, and unsuccessful in subsequent experiments, though they expected to be successful; and when they varied the experiments they were successful at first, though they did not expect to be, and unsuccessful afterwards, although they expected to be successful. I should conclude so. The condition of mind in first experiments is hard to be retained for subsequent ones. It is a condition of doubt. The consciousness is lazy, there is suspense, doubt, but not antagonism of conceptions. The positive conception secures activity in a dramatic reality, a hypothetical truth, which the negative does not trouble itself to infringe upon. This state is illegitimate, yet in its essence belief, as man is man under whatever nativity. It is "not far" from belief. It runs in the dry bed of belief. But in subsequent experiments the consciousness is aroused, it applies itself more, it brings more *in foro*, more into direct antagonism, the positive and negative conceptions. The positive, if otherwise strengthened by success, has its automatic creative faculty weakened, the negative being strengthened

by the increase of attention. There is belief or expectation, but conscious of itself; that is, again, the "unconceived negative" is conceived. Thus there is impure belief, or two beliefs at war. The positive conception is not left alone. It is momentarily annihilated by that very conception of the negative, which is involved in the conscious negation of the negative. Hence success diminishes, ceases. This agrees with what may be gathered from the records of spontaneous telepathy. The agent never deliberately believes that the percipient is or will be impressed. As I have endeavoured to show, in a state of mind more or less of a dream state, he believes unquestionably without a negative conception that the percipient is impressed, or that the relation A to B which he conceives is a really existent relation.

"H. G. R." says that in the only case of a first experiment where there was confidence of success, there was failure. This confidence (I submit) was too deliberate, too forced. The consciousness was too much aroused, the negative conception thereby drawn *in foro* and rendered antagonistic.

The difficulty is in the use of the words expectation and belief, which have a twofold meaning, pure and impure, according as the negative conception is absent or quiescent, or according as it is present or antagonistic. If "unconceived negative" be substituted for belief, the presence or absence of belief in its purity will be easy to determine; and it will generally be found with the presence of such belief there was no present consciousness of believing, but only subsequent recognition of having believed, and that in experiment confidence of success is a factitious state of which the foundations are undermined by the negative conception.

Again, "H. G. R." says that he occasionally guessed a card which the operator had momentarily intended to choose, instead of that which she did choose. I have observed this; and it has appeared to me one of those happy accidents by which the unnegated conception gets play. For here the operator simply selects a card for the percipient to guess, and then drops it before her mind applies itself to conscious expectation, that is to say, before the advent of the negative conception. That "H. G. R." guesses "instantly if at all" might be similarly explained.

It happens with suspicious frequency also that when the percipient is not informed of the cards which are turned up, he guesses not the card which is turned up, but the card turned up just before. This may actually be due to the dying away of the conscious expectation of the agent (upon failure), and to the mere presence in her mind of the unnegated conception of what might have been, of a card which might have been guessed, of a card guessable.

Finally, let me ask if the conclusion at which "H. G. R." has arrived really differs from mine. He thinks that the agent's success depends upon her power of concentrating thought. But the utmost concentration would, I think, still include, along with the thought, the thought of its transmission, while it would exclude, along with all other thoughts, the thought of its non-transmission, so that the agent would have an unnegated conception or pure belief that the thought would be transmitted.

And even with regard to himself as percipient, "H. G. R." thinks that his success depends upon his power of "excluding other thoughts," that is,

except of himself as percipient, that is, except of an unnegated conception or pure belief that he will perceive.

Upon this last point, if the percipient is quite ignorant that any agent is operating, this of course implies the absence from his mind of the negative conception of non-transmission. If he has, with knowledge of the agent, and with absence of the negative conception, the positive conception of transmission, this may be a still more favourable condition of percipience. But if the negative conception is present and active, opposed to the positive, that is, if there is a conscious and impure expectation in the percipient, this may be a very unfavourable condition.

But with regard to the percipient, I have no opinion. What I have desired to enforce in this letter is that there is a pure and an impure expectancy, and that pure expectancy implies the "unconceived negative." This latter it is which seems to me a necessary mental condition in the agent. But impure expectation I should conclude to be inefficacious in the ratio of its impurity, that is, in the ratio of the agent's consciousness of expectancy.

If it be true that the "unconceived negative" is a necessary mental condition in the agent, then it is a pregnant truth. If it be true also that the will is unconceived negative (see letter in *May Journal*) then a modern notion of the world as will and perception might be varied into a notion of the world as perception and the unconceived negative. Such a notion might both throw light upon and receive it from psychical research. However, the unconceived negative is not anxious to ascend to this metaphysical estate. It desires only that experimenters should put to the test its psychical efficacy.  
—Yours sincerely,

C. DOWNING.

P.S.—Such a letter as "H. G. R.'s," so full of valuable facts, seems ample excuse for toying with, by-the-way, not theory, but generalisation. I hope this elucidation of what expectation appears to be in analysis may suggest further observations, even if they tend to destroy the "unconceived negative."

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\*.\* It will be remembered that the earliest experiments in Thought-transference described in the Society's *Proceedings* were made with some sisters of the name of Creery; and that though stress was never laid on any trials where a chance of collusion was afforded by one or more of the sisters sharing in the "agency," nevertheless some results contained under such conditions were included in the records. In a series of experiments recently made at Cambridge, two of the sisters, acting as "agent" and "percipient," were detected in the use of a code of signals; and a third has confessed to a certain amount of signalling in the earlier series to which I have referred. This fact throws discredit on the results of all former trials conducted under similar conditions. How far the proved willingness to deceive can be held to affect the experiments on which we relied, where collusion was excluded, must of course depend on the degree of stringency of the precautions taken against trickery of other sorts—as to which every reader will form his own opinion. A further notice of the facts here briefly stated will be published in the *Proceedings*.—ED.