

JOURNAL

OF THE

SOCIETY FOR PSYCHICAL RESEARCH.

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NEW MEMBERS AND ASSOCIATES.

CORRESPONDING MEMBERS.

- BRAUNIS, PROFESSOR H., 29, Rue des Ecuries d'Artois, Paris.
 LIÉGEOIS, PROFESSOR, Nancy.
 RIBOT, T. H., Office of the *Revue Philosophique*, Paris.
 TAINE, H., Menthon St. Bernard, Haute Savoie, France.

MEMBERS.

- OMAN, CHARLES W. C., M.A., All Souls' College, Oxford.
 WOOSNAM, W. BURGESS, M.A., Petitor House, St. Mary Church,
 Torquay.

ASSOCIATES.

- BACCHUS, MRS., Sherbourne Villa, Leamington.
 BLACKWOOD, MISS CECILIA GRACE, Shortlands, near Bromley, Kent.
 BREWSTER, MRS. F., 10, Park Terrace, Nottingham.
 GREAVES, R. H., Bengal Civil Service, care of Messrs. Grindlay and Co.,
 55, Parliament Street, London, S.W.
 HASTIE, MISS, Luscar, Dunfermline, N.B.
 KIRBY, MRS. GEORGINA B., Santa Cruz, California, U.S.A.
 KNOX, NATHANIEL ALEXANDER, Adelaide Club, Adelaide, South Aus-
 tralia.
 PONSONBY, MISS, Glensouthwell, Rathfarnham, Co. Dublin.
 REICHEL, PRINCIPAL HENRY RUDOLPH, M.A., University College
 Bangor, North Wales.
 RODGER, W. WALLACE, 31, Tavistock Square, London, W.C.
 ROSENBACH, DR. PAUL, Kaiserliche Medicinische Akademie, St.
 Petersburg.
 STAPLEY, ALFRED M., St. John's College, Cambridge.
 TREVOR, REV. THOMAS WARREN, M.A., Penmon Vicarage, Beaumaris,
 North Wales.
 WARRAND, MRS., Bught, Inverness, N.B.
 WHITON, JAMES M., Ph.D., Tremont, New York City, U.S.A.
 YOUNG, PROFESSOR WILLIAM H., 76, Lange Strasse, Baden Baden.

MEETINGS OF COUNCIL.

The first Meeting of the Council after the summer recess was held on the 15th of October. The following members were present:—Professor W. F. Barrett, Professor H. Sidgwick, the Rev. W. Stainton Moses, and Messrs. Edmund Gurney, Richard Hodgson, F. W. H. Myers, Frank Podmore, H. A. Smith, J. Herbert Stack, and Hensleigh Wedgwood. Professor H. Sidgwick occupied the chair.

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

One new Member and twelve new Associates, whose names and addresses are given on the preceding page, were elected.

Information was received with regret of the death of Professor Boutlerof, one of the Corresponding Members of the Society, and of Mrs. Balmain, an Associate.

Mr. Gurney stated that Mr. C. C. Massey desired to resign his seat on the Council. His resignation was accepted with regret.

A pamphlet of Dr Féré's, sent by the author, was accepted for the Library, with thanks.

Cash accounts for the months of August and September were presented in the usual form, and the needful accounts passed for payment.

A letter from Mr. Percy Wyndham relating to Mrs. Sidgwick's paper, read before the Society on May 3rd, 1886, was laid before the Council; but being informed that a reply from Mrs. Sidgwick was to appear in the forthcoming (October) number of the *Journal*, the Council did not consider it needful to take any steps in the matter.

The next Meeting of the Council was fixed for Friday, the 5th of November.

At the Meeting of the Council on the 5th of November the following Members were present:—Messrs. Walter H. Coffin, Edmund Gurney, Richard Hodgson, Frank Podmore, H. Arthur Smith, and Hensleigh Wedgwood. Mr. Wedgwood was voted to the chair.

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

Mr. Edmund Gurney proposed the election, as Corresponding Members of the Society, of Professors Beaunis and Liégeois, and of Messrs. Ribot and Taine, whose names and addresses appear in full on the preceding page. The resolution was unanimously carried.

One new Member and four new Associates, whose names are included in the list on the preceding page, were also elected.

A cash account for the month of October was presented in the usual form.

At this and at the preceding Meeting of the Council the question of

the tenure of the premises at 14, Dean's Yard, engaged attention. The existing lease to the Society terminates on the 25th of March next. Dr. Stone's lease of the whole premises terminates on the 29th of September next. Dr. Stone having expressed his willingness that the present arrangements between himself and the Society should be extended from the 25th of March, 1887, to the 29th of September, 1887, it was resolved that this should be done, and the House and Finance Committee were instructed accordingly. The question of future arrangements in regard to 14, Dean's Yard, was left in the hands of the House and Finance Committee, who will report as soon as any definite proposal has been obtained from the landlord or from Dr. Stone.

The next Meeting of the Council will be held on Friday, the 3rd of December, at 4.30 p.m.

REPORT OF THE GENERAL MEETING.

A General Meeting of the Society was held on the evening of October 29th, at the rooms of the Society of British Artists, Suffolk Street, Pall Mall.

Professor H. Sidgwick occupied the chair. He called upon Mr. F. W. H. Myers, who had been announced as about to read a paper on "Multiplex Personality."

Mr. Myers began by apologising for the fact that the paper in question had appeared that morning in print in the *Nineteenth Century* for November, some days before the nominal date of issue of that review. But though he could not read them the paper itself, he would be glad further to illustrate some of the views contained in it by an account of some experiments recently witnessed in Paris by Mr. Gurney, Dr. Myers, and himself. The following is an expanded version of a part of the account given :—

On Monday, Oct. 25, we attended a meeting of the Société de Psychologie Physiologique, at which two papers were read of great importance for our researches. One of them was an account of a case of lucidity, or transposition of the senses, recently observed by a physician at Toulon. When this paper is printed we shall have to discuss it carefully. It is the only well-attested case of the kind of which we have been able to hear during the whole course of our inquiries. The second paper was read by Dr. Babinski, a physician at the Salpêtrière, giving an account of some experiments in the transference of hysterical symptoms, without suggestion of any kind, but by the aid of a magnet, from one patient to another. It is obvious that if this experiment really succeeds, and is capable of repetition, it is of quite capital importance for our researches. It is necessary, therefore, that

we should understand it thoroughly and follow its development with care. In order to do this it will be needful briefly to retrace some of the previous experiments which have led up to Dr. Babinski's.

I must here pass over the earlier inquiries (Andry and Thouret, 1777, Trousseau, 1833, Burq, 1851, &c.) and go at once to the French Commission of 1877, which concluded that certain unilateral troubles of sensibility were capable of being transferred from one side of the body to the other by the application of a magnet or a plate of metal to any part of the body. And here at once we are treading on somewhat uncertain ground. This alleged specific power of metals and magnets has never been fully accepted in England; and it is greatly to be desired that a series of English experiments should be instituted to confirm or disprove it,—electro-magnets being employed, and other means taken to obviate the possibility of suggestion.*

Starting, however, from this datum of the transferability of unilateral nervous perturbations by the magnet's action, M.M. Binet and Féré have tried many experiments with hypnotized patients, of which I subjoin a short summary. I am not able to concur with M. Féré as to the distinct and uniform characterisation of each of three stages of hypnotism, or the view that the opening of the eyes will in all cases change "lethargy" into "catalepsy," or that the rubbing of the top of the head will in all cases change "catalepsy" into "somnambulism." But I shall employ the terms usual at the Salpêtrière.

I. X is plunged in lethargy; then left eye opened so that she is hemi-cataleptic on left side. Left fore-arm is raised and left in vertical position. A hidden magnet is placed almost, not quite, in contact with the right fore-arm. The right fore-arm, which had been lying on the table, raises itself in a vertical position, the left arm trembles and falls on the table. The catalepsy has been transferred to the right side.

II. Witt— is in the somnambulant state. The hypnotizer suggests to her to make figures, 1, 2, 3, &c., with her right hand. She is awakened; a magnet being hidden near her left hand. She writes the numerals up to twelve with her right hand, then hesitates, takes the pen in her other hand, and begins to write in mirror-writing with her left hand. I have witnessed this experiment, (Aug. 25th, 1885). M. Féré had already remarked (*Rev. Phil.*, Jan. 1885) "nous avons souvent

* For some references to the literature of the subject, see my paper on "Telepathic Hypnotism" in *Proceedings* Part X. The account of M.M. Binet and Féré's experiments which follows is mainly taken from their article on "*Le Transfert Psychique*," in the *Revue Philosophique*, Jan. 1885. See also Dr. Bernheim's new work "*De la suggestion et de ses applications à la thérapeutique*," pp. 207 *sqq.*

répété cette expérience ; la malade s'est perfectionnée à mesure." One cannot help thinking, therefore, that the mere fact of finding herself writing figures with the *right* hand is now enough to suggest to Witt—that she is intended to change hands and write with her *left*.

III. Witt— is in the somnambulic state. It is suggested to her to repeat the numbers up to 100 aloud. She is awakened and a magnet placed near her right arm. She counts up to 72, then stops, turns her head to the left, and is apparently unable to speak. After ten minutes the magnet is applied to her left side, in two minutes she turns her head to the right and speaks again, complaining of her previous inability to do so.

IV. Witt— in somnambulism is told to do a number of things (opening drawers and the like) with her right hand. Magnet applied to right arm. She then obeys the commands with her left hand instead of her right. This shows, in M. Féré's view, that the resolve or intention to act in a particular way,—that is to say a *virtual* act, an "acte qui est en quelque sorte en puissance dans les cellules cérébrales de la malade," possesses a "material substratum," and is capable of being transferred from one side of the brain to the other, in just the same way as a completed or actual action.

V. And in M. Féré's view this transferability by the magnet's influence extends not only to the acts suggested by a hypnotizer, but to acts which appear to the subject herself to be entirely spontaneous. Witt—, in her normal state, is *asked* (not hypnotically commanded) to sit at a table leaning on her right elbow, to have her portrait taken. A magnet is concealed near her right elbow. After two or three minutes she withdraws her right elbow ; hesitates ; assumes a symmetrical position, leaning on the left elbow instead of the right. She says that the right arm is tired. Here, in M. Féré's view, we have a quite voluntary act, modified by the magnet's influence, while the subject still believes that she is acting from her own impulsion.

Moreover, M. Féré has successfully repeated others of these experiments with Witt— in her waking state.

VI. Suggested paralysis can similarly be transferred : involving physiological symptoms (tumefaction, perspiration, &c.) incapable of voluntary induction.

VII. Unilateral hallucinations, localised patches of anæsthesia, and so-called "systematic paralysis" can similarly be transferred. "Tous les phénomènes de la psychologie," concludes M. Féré. "sont justiciables de l'aimant, à la condition d'être unilatéraux."

I do not feel sure what is the true interpretation of these transferences. It may be admitted, I think, that they are not (at least in all cases) due to mere fraudulent simulation, for the symptoms are some-

times (though not always) beyond any one's power to induce on his own person.

But I can hardly think that *suggestion* is quite excluded. We cannot, I think, be sure that a subject, in however deeply lethargic a condition, does not understand what is being said in reference to herself. MM. Binet and Féré further urge that the transferences effected are anatomically too exact to be effected by a mere suggestion working itself out in the patient's mind. The patient, for instance, would not know that an impulse to speak having been given, and the consequent cerebral activity then transferred to the *right* hemisphere, speech would become impossible.

But even admitting that a suggestion works itself out in a manner which the subject cannot consciously determine,—with an anatomical exactitude of sequence which is beyond her knowledge,—it does not therefore follow that it is more or other than a suggestion. It does not follow, I mean, that the sequence of nervous changes has been set going by an agency (like the magnet) of a purely physical kind.

On the contrary, analogy makes it probable that a suggested paralysis, a suggested aphasia, will fulfil itself by unconscious channels in just the same way as a suggested character or personality (cabman, opera-singer, or what-not) fulfils itself in a better dramatic representation of that *role* than the subject's conscious effort could have given. A suggestion which takes effect at all takes effect *unerringly*;—that is to say, the subject's nervous apparatus carries out the idea as well as it can, unhindered by the mistakes or clumsiness of the conscious mind.

The *expression of countenance*, for instance, corresponds to the suggested character with much more dramatic propriety than the subject could assume in ordinary life. Various facial muscles, that is to say, over which the voluntary control of most men is very imperfect, are set in action in obedience to the pervasive conception of a specific character which has been conveyed by suggestion to the brain.

We may now have some conception of the base from which Dr. Babinski's new departure is taken. He maintains that just as the magnet will transfer hysterical affections, &c., from one side of A's body to a symmetrical position on the other side, so it will also transfer these from A to a symmetrical position on B. This is an experiment which does not *look* very different from Dr. Féré's experiments;—which *seems* to be a kind of development of results already obtained. But in reality it involves crossing the whole gulf which separates telepathy from the recognised laws at once of psychology and physiology.

To suppose that a magnet transfers a suggested paralysis from A's right hand to A's left hand is no doubt startling enough. But if established, it only shows that a physical agency, not hitherto generally

recognised as influencing the nervous system, does affect the nervous system in the same way as other agencies (hysterical disease, hypnotic suggestion,) are known to affect it. Strange and important though such a conclusion would be, it would be in a certain sense analogous with previous discoveries. But if it is proved that a magnet transfers a sensation from one nervous system to another nervous system, this, though not *contradictory* to any previously established knowledge, is not *analogous* thereto. It is not analogous to anything except similar telepathic transferences, such as are described in *Phantasms of the Living*, and which I do not venture to term *established*, although I hope that that book, and our *Proceedings*, may go some way towards establishing them as a proved reality.

I need hardly say that in thus pointing out the wideness of the interval between M. Féré's experiments and M. Babinski's, I am not seeking to *discredit* M. Babinski's. On the contrary, I am anxious both that full credit should be given to M. Babinski for his forward step (if he succeeds in making it good,) and also that the essential character of the experiments should be understood, in order to guide the observers who may repeat them.

M. Babinski attempted an experiment at the meeting of the Société de Psychologie Physiologique to which I have already referred. But—and this is a remark which I commend to those of our members who very naturally wish to see experiments at our Society's General Meetings—the conditions of such an assemblage are quite unsuited to delicate experimentation with human subjects. The experiment succeeded, indeed; but no fitting arrangements could be made to avoid suggestion; and we accepted gratefully Dr. Babinski's permission to witness a repetition of the experiment at the Salpêtrière on the following day.

Two hysterical women, Gr. and Cl., were selected,—they are, in fact, the usual subjects,—and we prepared to witness the magnetic transference of any one amongst some dozen hysterical affections, communicable by suggestion, from one to the other patient.

I was asked to select the affection which should be suggested. I chose *mutism* as less conspicuous than a paralysis or contracture. Gr. was then admitted, set down in a chair with her head leaning against a screen, hypnotized, and told that she could not speak. The suggestion at once took effect; but instead of remaining tranquil under the infliction she made sundry writhings and bumped her head against the screen. Cl. was then brought in, set down on a chair on the other side of the screen, but with her head resting against the screen, and hypnotized. A magnet was placed on a table in contact with her left arm. We spoke to her once or twice and she responded at first normally. But

in some three minutes she began to show difficulty in speaking, then anger and inability to speak. About the same time Gr. began to mutter and talk. It was impossible to say that Gr. had not muttered before Cl.'s mutism was markedly shown. There might thus have been suggestion from Gr. to Cl. that *dumbness* was the affection intended to pass from the one to the other. Moreover, the contact of the two heads with the screen obviously facilitates signalling, had the two women been in collusion.

We asked that next time they might be placed out of contact with the screen—a request to which Dr. Babinski at once acceded. Gr. was then hypnotized as before, and a contracture of the left arm was induced. Cl. was brought in and placed near, but not touching, the screen. The possibility of signalling was, however, not excluded. There were, moreover several students in the room, and one of them touched the contracted arm during the experiment. We cannot, therefore, be quite certain that Cl. did not become aware of the special phenomenon to be transmitted. The transmission took place in due course, and was again followed by a “consecutive oscillation” during which Gr.'s arm was again contracted and Cl.'s again normal.

We now asked that the two subjects should be placed on opposite sides of a door. Dr. Babinski again at once assented, and Gr. was removed to an adjoining room, and then hypnotized, and her left foot turned inwards almost at right angles to its normal position. Mr. Gurney and Dr. Myers accompanied Gr., and state that the experiment was somewhat vitiated by the fact that Gr. exclaimed “You are hurting my foot!” I much doubt, however, whether Cl. (with whom I remained) could have heard this, as there was much noise of moving table, &c. on our side of the door. At any rate Cl. was placed near the door, and in contact with a magnet. In a few seconds she had a violent hysterical seizure, and the experiments were at an end. But we observed during her struggles that her right foot was contracted, after the same fashion as Gr.'s, the contracture persisting through the so-called “clownism” and violent confused movements of the seizure. It looked as though there had been a transfer, but not to the *same* limb; *i.e.*, a transfer of contracture from Gr.'s left foot to Cl.'s right foot.

On the following day, October 17, Dr. Charcot repeated the experiment once in the presence of Dr. Myers. The subjects sat back to back, separated by about a foot, but without any intervening screen or door, and the transference took effect.

I have described these experiments in detail; for I think that we ought to try to repeat them. Without impugning the methods used by the staff of the Salpêtrière in dealing with patients with whom they are thoroughly familiar, I may suggest certain rules

which I think ought to guide any repetition of the experiments which our own Society may attempt.

I. No allusion to the result expected—no verbal suggestion of any kind—should be made before the subjects in *any* of their states. We cannot, as I hold, ever be certain that a subject, however profound her lethargy may appear, does not hear and understand what is said in her presence. She probably will retain no memory of it in her normal state—perhaps not even in lighter hypnotic states—but when she is in the deep state again she will act on the suggestions received in a previous deep state. Mr. Gurney's paper on "Stages of Hypnotism" (*Proceedings*, Vol. II.), contains abundant demonstration of these phenomena of memory, in certain English cases at any rate. And in the Salpêtrière itself I have seen M. Auguste Voisin give an effective suggestion to a subject who was apparently absolutely unsusceptible to any external stimulus, and who gave no sign whatever of having heard what was said to her.

II. Whenever magnets or metals are used, *sham* magnets and *sham* metals should be used also. The electro-magnet obviously affords the best way of testing the reality of a magnet's influence. By the means suggested in my paper on "Telepathic Hypnotism" (*Proceedings*, X.) the indication given by the "magnetic click" can be avoided, and the subject can be kept absolutely ignorant as to whether the electro-magnet is in effect a magnet or no. I do not altogether trust to concealing magnets under towels, secretly affixing or removing the armature, or altering the position of the poles. We should trust as little as possible either to our own good observation or to the mal-observation of subjects who may conceivably be quite as acute as ourselves. It is only, I think, by a long series of experiments with the electro-magnet that we can assure ourselves whether or not the magnet goes for anything in the physiological transference from one subject to another, supposing that such transference is produced.

III. Once more, the experiment should be tried on a considerable number of persons. If, as Dr. Babinski apparently supposes, the transferability of hysterical symptoms from one person to another is a phenomenon characteristic of hysteria in the same way as transferability from one side of a patient to the other side is characteristic, then we ought to find an abundance of subjects with whom the experiment can be repeated at will. On the other hand, I think it likely that we may find that even among hysterical patients, as among normal subjects, this telepathic capacity is a rare and exceptional thing.

It follows from what I have said that we shall probably have to remain for some time in suspense as to the precise value of these

experiments. But nevertheless the mere fact that these experiments are being made, and being made at the Salpêtrière, is a gain to our researches of a most important kind. This powerful group of specialists, —with a mass of subjects ready to their hands such as perhaps is not collected elsewhere in the world,—are now inclining to believe that they can produce on those subjects certain telepathic phenomena,—phenomena, that is, of the very kind whose reality our Society has made it its first and special business to attempt to demonstrate to the scientific world. The physicians of the Salpêtrière are not likely to drop the inquiry; and we may hope that the experiments above described are but the first instalment of what they may yet achieve. I do not know, indeed, that our own experiments have had any large share in determining this new adhesion to Telepathy. But it is characteristic of a true discovery that phenomena which point towards it should occur in independent quarters and in the course of distinct and disparate lines of investigation.

In another ward of the Salpêtrière Dr. Auguste Voisin's courtesy permitted me (Oct. 27) to witness an experiment interesting in a different way. As my readers may remember, this distinguished physician has had unusual success in hypnotizing insane persons, with permanent benefit to their mental and moral condition.

He has at present under his care a young woman whose history, as he narrated it to me, indicates a moral obliquity amounting to what is sometimes called "moral insanity," while at the same time the intelligence, though low in type, is normal in operation. Besides other misdoings, the girl was obstinately indolent, and violent and brutal to her mother, for an assault on whom she had been committed to prison. I did not precisely understand how she had passed under Dr. Voisin's care. He told me that he had hypnotized her without much difficulty, and had thrown her into a state apparently of profound unconsciousness. His view, I gather, is that a suggestible subject will comprehend and respond to suggestion in *every* hypnotic state; even though she may give no sign whatever of hearing what is said.

We went into the ward and the girl in question met us. She was obviously of extremely low type, but her expression was not displeasing, and she ran to meet Dr. Voisin with a sort of doglike anxiety to please and be commended,—telling him what work she had done, asking for more work, and promising to give some little pittance which she earned to her mother. Dr. Voisin hypnotized her by the gaze in a minute or so, and repeated his moralising suggestions—of diligence, filial affection, decent behaviour, &c. She made no response and seemed absolutely insensible and lethargic. To show that she did really take in what was said, Dr. Voisin asked me to suggest some

command which she should fulfil immediately after waking. I requested him to tell her that when she awoke she would come up to me—a foreign visitor—and ask me to tell her the time by my watch. Leaving her asleep, we visited two other patients. One was suffering from progressive locomotor ataxy, with occasional onsets of mania, and much pain at all times. Dr. Voisin hypnotized her easily, and told me that he habitually thus procured for her remissions of pain of some hours' duration. The next case was a woman suffering from "lypémanie aiguë," acute melancholia, with suicidal tendency. She was much harder to hypnotize; her eyes wild and restless, and her attention hard to fix. Dr. Voisin held her head firmly a few inches from his own and stared fixedly at her for some five minutes. She then fell into the hypnotic sleep. Dr. Voisin told me that at first a very long time and very great effort had been necessary in order to hypnotize her. He considered that after each hypnotization she was quieter, and that the suicidal tendency was kept in check.

We then returned to the first subject, who was still sleeping profoundly. Dr. Voisin woke her, and she at once came up to me, with a dazed expression, and asked me what time it was by my watch. I showed surprise and some offence; but she persisted in a kind of mechanical way till I had told her the time. The deferred suggestion, that is to say, worked itself out in the usual way, the subject's state of mind while obeying it being intermediate between the somnambule and the normal condition. As soon as she had obeyed the forgotten, but still operative, command, she seemed quite normal and ran off at once to her companions.

For my part, I must say that I regard Dr. Voisin's experiments as among the most interesting of all developments of hypnotism. If found capable of repetition they will stand at the head of what I may term *suggestive therapeutics*.

And here I may call attention to the rapid change which is taking place in the mode in which hypnotism is being applied as a curative agent. Until a few years ago there were two main views, which we have repeatedly discussed in the *Proceedings*. The hypnotists proper believed that the therapeutic results obtained were due wholly to the nervous change involved in hypnotic trance, however that trance was induced. The mesmerists proper believed that a specific effluence or influence passed from mesmeriser to subject, and possessed a *vitalising* power quite independent of the nervous changes involved in the state of trance.

This question still possesses a very high theoretical interest, and it is to be hoped that other inquirers may again take it up, and criticise or corroborate the experiments recorded from time to time in our

Proceedings, which point towards the *mesmeric* (at present the less fashionable) interpretation. Hardly any attention, however, has been given to this point of late. For the centre of *therapeutic* interest has been shifted in a direction which neither Braid nor Elliotson foresaw. It is not now to the *trance per se* nor to the *passes per se* that we look mainly for benefit to health. Rather it is to the suggestions made to the subject by word of mouth during the *trance*;—suggestions which work themselves out after he awakes, not only so far as they affect *voluntary* muscles, (as when Dr. Voisin's patient was told to ask me the time,) but also to a great extent when they affect *involuntary* muscles; and indeed vaso-motor, circulatory, digestive, and nervous processes, within limits at present very imperfectly known.

It is to Dr. Liébeault more than to any one man that the credit of this discovery is due. Professors Beaunis and Bernheim have called effective attention to it during the last three years, and Professor Bernheim's book, above cited, is at present the best collection of cases thus treated.*

But at the head of all these hypnotic suggestions must be placed the suggestions which have a *psychical*, a *moralising* effect; which influence the "springs of conduct," or supply a kind of "contra-impulsive power" which aids the subject, to resist a habitual temptation. Here, too, Dr. Liébeault was the pioneer; but Dr. Auguste Voisin seems at present to have obtained the most striking results. I am inclined to think that this may be due to some efficacy personal to himself; but that is not Dr. Voisin's own view. He holds that the essential is patience and perseverance, and that there are many lunatics who could be hypnotized by an attempt prolonged, as some of his have been, over several hours. Here he is in accord with Elliotson and the old English school of mesmerists; and it seems likely enough that the ease and rapidity with which most subjects are hypnotized in France may have led operators to neglect some cases where more of effort and "education" was required.

Be this as it may, there can, I think, be no doubt, that in some cases his hypnotic suggestion has effected a real improvement in the patient's self-control and sanity of behaviour. And the troubles which

* I am sorry not to have more English names to quote. It seems to me surprising that so few English physicians have taken part in these new inquiries. There is some good work—though small in quantity—by Dr. Hack Tuke and Mr. Langley, but I cannot find out that, except this, anything whatever has been done of late in England in hypnotic investigation outside the inquiries described in our *Proceedings*. The article on "Animal Magnetism" in the new edition of the *Encyclopædia Britannica* does not contain—so far as I can make out—the record of one single original experiment.

he has thus attacked—dipsomania, morphinism, &c.—are in themselves so serious and hard to cure that no pains should be spared to give the new method a fair trial. I understood from Dr. Voisin that, without of course guaranteeing any kind of success, he would be willing to make an attempt on any serious case of this kind which might be placed in his hands.

Professor Sidgwick said that he wished to make a few remarks on the important crisis which we had now reached in the history of the Society, and on the temper in which we ought to meet it.

In a day or two the book which had been for some time announced, and on which Messrs. Gurney, Myers, and Podmore had been labouring almost from the outset of our career as a society—the *Phantasms of the Living*—would be in the hands of the public. For the first time, the scientific world would have before it in a complete form the grounds for the momentous conclusion which his friends some time ago announced—and in which he was entirely disposed to concur,—that the mental state of one person might affect another, otherwise than through the recognised channels of communication by the senses, and even at a distance so great as to render a physical mode of communication very difficult to conceive.

If this conclusion were to be generally accepted and to become a part of the recognised teaching of modern science, he thought that even those who were now most opposed to our work would admit that it was difficult to exaggerate the importance of our achievement. But he was far from anticipating any such sudden conquest of the scientific world; on the contrary, though he hoped we might produce a certain effect, especially on younger and more impressible minds, he did not doubt that the great majority of scientific men would regard “telepathy” as a wild dream just as confidently *after* the appearance of our book as they did before.

In many cases no doubt this result would be reached by the simple process of paying no attention to our evidence and our reasoning. But he thought—and he might say that he *hoped*, as an alternative very preferable to neglect—that there would be not a few who would read the book and yet remain altogether unconvinced. If anyone thought this too despondent a view, he would ask him to read the thoughtful and instructive address by Professor Newcomb, the President of the American Society for Psychical Research, which appeared in the *Proceedings* of that Society published last July. No one could doubt that Professor Newcomb had given serious attention with a candid mind to the evidence which we had so far published; a man was not likely to take the trouble involved in the position he had taken, and incur the

risk of disparaging sneers which he supposed the position carried with it for a student of physical science in America as well as here, unless he considered the subject deserving of fair and full attention. And yet Professor Newcomb's conclusion, as he stated it, after a "critical and careful study of our work," was that the work of our Society "has almost entirely removed any ground which might have existed for believing thought-transference a reality"!

The first impression produced on our minds by this verdict, from the President of the sister—or daughter—Society in the United States, is inevitably one of disappointment. But Professor Sidgwick hoped that having got over our disappointment, we should agree that the right attitude to take up towards Professor Newcomb, and those who agreed with him, was that of inviting and cordially welcoming the most severe and searching criticism that they could produce of the evidence and the reasonings contained in the *Phantasms of the Living*. He did not mean that we ought to accept such criticism when it did not commend itself to our minds; but that we should receive it not only without the slightest heat but without any *parti pris*, or impulse to defend any weak places that might be pointed out in our argument. At present, Professor Newcomb's main ground for rejecting our conclusion appeared to be of the general and negative kind, from which it was difficult to derive much immediate guidance. He objected that we had "constructed no theory of thought-transference," no "statement of general laws, setting forth the conditions under which thought-transference can be brought about." His own view, Professor Sidgwick confessed, had been that the mere establishment of the fact, the mere exclusion of all communication through the recognised channels of sense, was so difficult a matter that we had better concentrate attention on this at first; otherwise we might find ourselves in the ridiculous position of spinning an elaborate theory to explain phenomena supposed to be novel, but really due to known and familiar causes. Still, he quite admitted that whenever a clear case of thought-transference was made out, it was of the utmost importance to repeat and vary the experiments, with a view to the ascertainment of its conditions; and he hoped this would be borne in mind in any fresh experiments.

But the most important point, as yet, was to exclude rigorously all communications through the recognised channels of sense; and it was here that he hoped Professor Newcomb, and others who shared his opinion, would help us with their criticism, and tell us exactly how, in their view, we could make the experiments more conclusive. Nor ought we, he considered, to object even to very strained hypotheses as to the manner in which our results might have been brought about

by natural causes; our conclusion was so improbable, from its opposition to the general drift of the scientific movement of modern thought, that it was not unreasonable to embrace even very improbable suppositions in order to avoid it. The question would ultimately be decided by a balance of opposing improbabilities; and we ought, in his view, patiently to go on accumulating and improving experiments, and getting spontaneous experiences more freshly and exactly recorded, till the load of accumulated improbabilities on the other side became intolerable.

We need not, however, look only at the prospect of antagonistic—he would not say hostile—criticism. The amount of positive co-operation that we were receiving was very encouraging. Even from America, thought-transference experiments of a very promising kind were recorded; Mr. Myers had brought before us the work in France, and the last number of our *Proceedings* had contained an account of interesting experiments in Germany. Returning to the cases of spontaneous telepathy, he expressed his opinion that we would not be blamed for want of care, considering the novel nature of the inquiry. The attack would be mainly on the recollection of our witnesses; and even in this respect he thought it would be admitted that we had some strong cases, but more would be wanted. His view was that if we were right, the quality of the evidence would certainly be improved in consequence of the new seriousness which he hoped we should impart to the subject, and our willingness to take any amount of pains in collecting fresh evidence while the occurrences themselves were still fresh; and he would urge us to patience and perseverance in repeating experiments and collecting experiences.

REPLY TO MRS. SIDGWICK.

BY H. WEDGWOOD.

Mrs. Sidgwick, who has had three sittings with Mr. Eglinton and has seen nothing, has no hesitation in attributing to clever conjuring those wonderful exhibitions of slate-writing, which have been accepted as genuine by so many scores of eye-witnesses having the best opportunities of observation, including among them several specially skilled in the arts of the conjurer, who went with every expectation of being able to detect the illusion. This confident assumption of the universal roguery of the slate-writing medium is mainly founded on the sweeping rejection of all evidence depending on the unbroken observation of the Medium during the continuance of the experiment, a clearance which, in Mrs. Sidgwick's view of the matter, would leave as the only kind of evidence worthy the attention of the sceptic, such as relates to the

production of writing in a closed receptacle wholly inaccessible to the Medium. "Attempts to obtain evidence of this kind," says Mrs. Sidgwick (*Journal of the Society for Psychical Research*, II., p. 333), "have been constantly made. All these attempts have failed with the single apparent exception of one of Mr. Wedgwood's, and in this case there seems to me a serious flaw in the evidence. Mr. Eglinton saw the slates at the first séance when nothing occurred, and even if it could be proved that they had not been tampered with in the interval between the two séances, I cannot perceive that we have any means of knowing that a pair was not prepared in imitation and substituted at the second sitting. It is surely significant that there should be but a single instance of writing in securely closed slates, and that a dubious one." To the same effect on the previous page she says, "It will be remarked that all the evidence here presented (with one doubtful exception) depends on continuous observation, and the same is true, so far as I can learn, of all the evidence published elsewhere." It is surprising that a person who has paid so much attention to the subject should suppose that mine was the only recorded instance of writing in securely closed slates, but how she could have written the passage which I have italicised above I am at a loss to imagine. At the time she wrote it Mrs. Sidgwick must certainly have been in possession of the clearest testimony to the production of writing in perfectly closed receptacles in two instances, through the mediumship of Monck and Slade respectively, the accounts of which, recorded in the *Medium* newspaper of the 6th and 27th October, 1876, I myself sent to her, and she subsequently returned me the cuttings without a word as to their having come too late for her criticism.

The first of these extracts is from a letter to the *Banner of Light* written by Mrs. Louisa Andrews, a well-known writer on matters of this kind.

She says:—

"During my stay of over two months in the house with [Slade] last summer I took a folding slate into my bedroom, and with it a screw and a screwdriver, having previously had screw-holes made in both frames. On one of the inner sides of this folding slate I wrote a few lines addressed to a friend in spirit-life, after which I placed a fragment of pencil within, and then fastened the two leaves securely together. In this condition I took it downstairs and placed it on the top of the table at which the Medium was seated. Almost immediately we heard the scratching sound made by the pencil in writing, and after the séance was over, on opening the slates (which I did not do in the presence of the Medium, but after returning to my room where I had left the screwdriver), I found a reply to what I had written, signed with the Christian name of the spirit I had addressed, whether written by this spirit or not I cannot say, and any opinion I might form on that point would be

worthless except to myself. What I know is, that some power caused writing to be done on the inner side of a folding slate, which did not leave my possession, and which remained firmly screwed together till I myself unfastened it."

It is impossible to have clearer testimony to the fact of writing produced on a surface entirely inaccessible to the Medium. The evidence in the case of Monck is not less complete. It is published in the *Medium* of the 26th October, 1876, in the form of a communication signed with their names and addresses by 10 residents of Keighley, who thus mutually pledge their credit for the truth of their account.

"We, the undersigned, have just sat at a séance held at 39, Devonshire Street, Keighley [the residence of Mr. Clapham, one of the witnesses]. Dr. Monck was the medium. By raps we were directed to procure a hammer and tacks, and Mr. J. Clapham supplied both. Mr. Greenwood Lonsdale then moistened, and with his handkerchief thoroughly cleaned and dried both sides of a common deal-framed slate. Six of the company wrote their initials in ink on the frame. The slate was then held before a full jet of gas-light, so that all in the room could distinctly see that both sides were absolutely blank. Directions were next given by the spirit-raps to nail a piece of board on to the slate, and the board was nailed down, five tacks being driven to their heads so as to secure it to the frame of the slate, which lay on the table in view of all the whole time till this was done. Mr. G. Lonsdale then placed his hands on the boarded top of the slate so as to cover the entire surface. The gas was turned out for a brief time, and on being relit the raps said, 'What shall I write?' A book lay on the table, and this Mr. J. Clapham opened at a venture at p. 133. We asked for a quotation from that page. The gas was burning and was put on at full. We then all distinctly heard the sound of rapid writing for a few seconds on the covered slate.

"As soon as this ceased Mr. J. Lonsdale removed his hands, which up to that moment had not been moved from the time the board had been nailed on. The board was now forced off in view of us all, and we found on the surface of the slate beneath it ten parallel lines of very fine distinct writing between inverted commas. At the foot was the following:—'P. 133, Samuel.' The figures 133 were found to be the number of the page in the book which Mr. Clapham had opened at a venture, and the contents of the slate were found to be an exact quotation from that particular page. The first six of the undersigned witnesses then examined and identified their initials on the frame of the slate."

What better evidence could be required? The account purports to be written just after the sitting. The facts are of so simple a nature that they could as well be observed by any ordinary intelligence as by the most scientific member of the Society for Psychical Research. The slates, by the plainest inference from the words of the narrative, were

never in the possession of Monck from the commencement of the experiment, and above all, there was no indication of the matter required to be written until the slate was securely nailed up.

If the testimony of a scientific witness is entitled to greater weight, Mrs. Sidgwick must surely be well acquainted with the *Transcendental Physics* of Professor Zöllner. In that work, at pp. 44 and 198 of the translation by Mr. Massey are the details of two instances in which writing was obtained between slates securely bound together, untouched by Slade.

“On the evening of the 13th December, 1877,” says Zöllner, “two slates were bought by myself, marked, and carefully cleaned. They were then—a splinter from a new slate-pencil having first been put between them—bound tightly together crosswise with a string four millimetres thick. They were laid on and close to the corner of a table of walnut-wood, which I had shortly before purchased myself. While now W. Weber, Slade, and I sat at the table, and were busied with magnetic experiments, during which our six hands lay on the table, those of Slade being two feet from the slate, very loud rapping began suddenly to be heard between the *untouched* slates. When we separated them, there was upon one of them the following words, in nine lines—”

Again, at p. 198, “Slade now desired me to take two of the new slates, to lay a splinter of slate-pencil between them, and then to seal these two slates firmly together. I did this after having satisfied myself that the slates were perfectly clean. The sealing was in four places on the long sides, and now I laid these slates with the bit of pencil between them on the corner of the card-table most remote from our hands. The latter we joined over one another on the table, so that Slade’s hands were covered by mine, and were thus prevented from moving. Scarcely had this happened when the untouched slates were raised many times upon one of the edges, which was clearly perceived by us both by the bright light of a candle standing in the middle of the card-table. Then the two slates laid themselves down again on the card-table in a somewhat altered position, and now writing between the slates began to be immediately audible, as if with a slate-pencil guided by a firm hand. After the well-known three ticks had announced the conclusion of the writing, we sundered our hands, which up to this time had been continually and closely joined, closed the sitting, and betook ourselves with the double slate, which I had immediately seized, to the next room where Herr v. Hoffmann and his wife awaited us. In presence of these persons the slates shortly before sealed by me were opened. *Both* sides were completely written over in English.” Then follows the passage written, consisting of 147 words.

I will add, on account of the clearness and conciseness with which it is told, one instance of writing between screwed slates from the evidence collected by Mr. Eglinton and published in *Light* of 16th October, 1886, p. 495.

“On one occasion, having purchased two slates similar to those you generally use, I took them home and thoroughly cleansed them. I then

screwed the frames together after inserting a small piece of pencil. At my next séance you then held the two slates so fastened by one corner; my sister held the opposite corner. After a short space of time the sound of writing was distinctly audible, and on unscrewing the slates with a screw-driver which I had brought with me, the words 'This is true' were found written on that part of the slate furthest from you.—GEORGE SEYMOUR, 41, St. Augustine's Road, Camden Town."

Another perfectly clear account of writing produced between a pair of hinged slates padlocked together is given in the same collection, p. 492.

With so many authentic records of writing between effectually closed slates I should not have thought it worth while to defend the reliability of my own evidence on the subject, except for the purpose of showing to what straits Mrs. Sidgwick is driven in order to avoid the effect of evidence opposed to her prejudices. The suggestion that the slates might have been tampered with while reposing in my cupboard between the two sittings seems to me simply absurd. It is incredible that Eglinton should subject himself to the indignant rebuff which he would meet with in nine cases out of ten if he were to go to any respectable house and offer to bribe the servants to give him access to some packet in their master's keeping, of which of course they would know nothing. And if he failed he would infallibly be denounced to the person he was taking so much trouble to deceive, and would irretrievably ruin his own character.

The only other suggestion of a weak point in my testimony is the supposition that Eglinton, at the hasty glance at the slates which he had at the first sitting, was able to fix every particular so firmly in his memory that two months afterwards, when I applied for a second sitting, he was able to prepare a pair in exact imitation, and to foist them upon me in exchange for my own as we sat opposite each other at the second sitting. When I left after the first sitting, he had no reason to suppose that I should make a second attempt, for I had no thoughts of doing so until I wrote for a second appointment. So he had no reason to attend minutely to the aspect of the slates. As it happened, they were a pair of old shopkeepers which I had bought at the nearest news-shop, of an uncommon size and pattern, and he might have gone to fifty shops without being able to find a similar pair. Then he must have recollected my seal, which he had seen on no other occasion, sufficiently to have a counterfeit made exact enough to baffle my observation. And after all this trouble and expense he would have found that I had added the precaution of the fastening with gummed paper, which would have rendered his preparations worthless! Even if he had known that I was going to gum them up, it might have been done in such an infinite variety of ways, with this kind of paper or that (it was

not done with postage paper), with a broad margin or a narrow one, whether covering the seals or leaving them exposed, and so forth, that it would have been a perfect miracle if he had hit on so exact an identity with my slates as to deceive my eye at a time when I had the most vivid recollection of my own studied preparations. He never could have run the risk of my almost certain discovery of the fraud. I could not be mistaken as to the individual peculiarities of my own handiwork, and I can aver with complete assurance that the slates on which I obtained the writing were the very pair I prepared for the experiment.

I have confined my remarks to the single case of writing obtained in a securely closed receptacle, because, as Mrs. Sidgwick avows, that is the only quarter in which her mind is open to evidence for the genuineness of the writing. Effectual precautions for excluding all possibility of access to the interior of the slates during the continuance of the experiment are easily devised, and the observance of them made manifest to the witnesses. When in the face of such precautions writing is obtained, the personal character of the Medium obviously becomes an irrelevant element in the evidence of the transaction, and therefore, if all that is alleged against the Mediums by Professor and Mrs. Sidgwick were firmly established, it would not tend in the least to invalidate the evidence in the instances above-cited, where writing was produced upon closed slates, under conditions which made the physical agency of the Medium clearly impossible.

I do not, of course, expect to make any impression on prejudices like those avowed by Mrs. Sidgwick. "I can hardly imagine," she says, "being myself convinced that it [Mr. Eglinton's writing] was genuine, except by * * * the testimony of thoroughly competent and trustworthy witnesses that in several cases it had been produced under circumstances which dispensed with the necessity of continuous observation." If I supplied her with 20 such cases, she would probably find that nothing under 30 would serve her turn; but my object is to show to the bulk of the Society, by a few concise specimens, that there is no such dearth as Mrs. Sidgwick supposes, of the very class of evidence which she calls for as ideally conclusive.

THE SECOND VOLUME OF THE *JOURNAL*.

The second volume of the *Journal* will end with December, and a title-page and index will be issued with the January number. Covers will also be ready about the middle of January, and may be purchased at 1s. each. Application to be made to the Assistant-Secretary, 14, Dean's Yard, S.W.

ON THE REPORTS, PRINTED IN THE *JOURNAL* FOR JUNE,
OF SITTINGS WITH MR. EGLINTON.*(Continued.)*

BY RICHARD HODGSON.

From this brief consideration of Mr. Davey's accounts published in *Light*, let us pass to a brief comparison of the main points of these with the corresponding passages in his later reports.

Taking the sitting of June 30th, we read in his earlier account :—

1. I procured two ordinary slates at a stationer's shop.
2. These did not leave my possession during the séance.
3. But the best test of all was when I put a crumb of pencil on the slate, and then put another slate over that ; holding the two slates together myself, I then asked if I should ever become a medium. [In reply to which he obtained a long message.]

In his later account he tells us :—

1. That Mr. Davey and Mr. Munro "bought four slates"—from which, with the statement above, I infer that Mr. Munro procured two.
2. That the sitting began by Eglinton's holding one of the bought slates alone under the table, Mr. Davey's hands being engaged on one side by Eglinton, and on the other by Mr. Munro.
3. That the slates in connection with which the long message was obtained were unquestionably Eglinton's, and were used in consequence of a supposed "occult" injunction.

In this later account, we may also notice that the subject of the long message is not given, and though we are told that it was a complete reply to Mr. Davey's query, we are not told what the query was ; nor are we told that Mr. Davey had made the acquaintance of Eglinton previous to the day of the sitting. The information as to these points which the earlier account affords us enables us to see at once how easy the previous preparation of the long message may have been. *In the light of this comparison it seems clear that accounts in which the appearance of a long message in reply to some special question is described in general terms, are not to be assumed as good evidence that pre-arrangement was thereby precluded.*

Further, while there are many details in the later account not found in the earlier one, indications are not wanting that some of these details are due to Mr. Davey's *imagination*. Thus his description of what he *heard*, in connection with the long message, plainly shows, to any person who has made experiments in localising the sound as of writing, not merely that he has unduly embodied in his report a very large amount of inference, but that he has actually embodied in his account of what he experienced *before* the long message was seen, certain imaginations which came *after* it was seen.

We could distinctly hear it travelling over the surface until it arrived at the end of the slate, and then I was puzzled to hear it again commence as though from the top.

Mr. Davey's earlier account exhibits throughout, on the face of it, such an absurd negligence of the "conditions" of the sitting that I cannot place any reliance upon his later description of specific precautions which he probably only afterwards inferred that he must have taken, his conjectures becoming interwoven with his true remembrances beyond the possibility of disentanglement. If any of my readers are under the misapprehension that confusions of this kind are not possible in the case of a *bonâ fide* witness, I would remind them of the incident of the officer and the Hindoo juggler, which I recounted in the last number of the *Journal*. Mr. Davey writes, for example:—

I was, however, by no means satisfied, and I requested permission to be allowed to hold the slate with my own hand against the table.

This is perhaps a transfiguration of a suggestion from Eglinton that Mr. Davey should assist in holding the slate, upon which the word or words had already been surreptitiously written; though even were the statement strictly correct, the writing by ordinary means would not have been precluded. It is obvious that Mr. Davey was still, when writing the later account, under the impression "that the writing, as a matter of course, *takes place when the sound of writing is heard*"; for he says:—

During the movement of the pencil, I used my best endeavours to notice any movement of the medium's hand, and entirely failed to detect anything.*

This misconception may have exercised a vitiating influence throughout much of his report. For instance, after describing a careful examination of two slates by Mr. Munro and himself, he says:—

we placed the two slates in exact juxtaposition. They were then held above the table by the medium and myself, and having placed a small grain of pencil between them, I abruptly asked for the figure 9 to be written. Instantly we heard a scratching noise and three sharp taps, and on removing the slates we found a large figure 9 written on the slate.

Considering Mr. Davey's views at that time concerning the relation

* It is an important fact,—from whatever convulsions Eglinton may suffer during the sitting, particularly those convulsions which, to use the words of X— (p. 435), "move the slate about too much"; and whatever other "suspicious" circumstances happen, such as the dropping of the slate on the floor, or the bringing of more than one slate into use at the same time, or the frequent withdrawal or uncovering of a slate, upon his own initiation;—that yet, *when the sound as of writing is distinctly audible*, Eglinton's passivity is unimpeachable, as regards at least the noticeable portions of his organism. At other times much concentration is not desirable, it interferes with the "phenomena"; but *then*, the sitters may focus their attention as much as they please, especially upon the visible parts of the slate and Eglinton's hands, and the "phenomena" proceed just as well, if not better.

between the sound as of writing, and the actual production of the writing, we may suppose that his "abrupt" asking might have been the *second* time of asking; the slates might have been under the table at the *first* time of asking; but as Mr. Davey heard no sound as of writing, of course the writing was not being produced; and the circumstance of the slates' having been under the table, as it had no significance from his point of view, lapsed from his recollection, either temporarily or entirely. We may suppose that after the first time of asking, the slates being held under the table, Eglinton wrote a 9 on the under surface of the bottom slate; that shortly afterwards, placing the slates upon the table and removing the upper slate on the pretence of seeing whether any "writing had come," he used the opportunity, in placing the slates together again, to reverse the positions of the slates, putting the previously under slate over the other; he then turned both slates together so that the originally under slate became again the under one; and as he did so he asked Mr. Davey, this being a convenient reason for his lifting the slates, to hold them also, and repeat his request; whereupon Eglinton produced a sound as of writing, &c., the top slate was removed, and on the upper surface of the bottom slate of course the figure 9 was found.

In the last "experiment" of which Mr. Davey offers any details,

(I then cleaned one of my own slates, and when the medium was out of the room I wrote the name of a deceased relative upon same. I then put the slate against the flap of the table in such a manner that it would have been utterly impossible for Eglinton to have seen what was written.

When he returned I requested an answer to my question. I myself held the slate against the table, and no one else but myself knew what I had written. The usual sound of writing commenced, and on removing the slate I found the following answer: "Your uncle A" (giving the correct Christian name) "is not present."

Now it was utterly impossible for Mr. Eglinton to have seen what I had written, &c.),

the slate was probably held part of the time by Eglinton alone (who read and answered the question in this interval), notwithstanding Mr. Davey's statement: "I myself held the slate against the table" (as he may have done while the sound as of writing was audible), and his assumption that Eglinton was unable to discover "by any ordinary agency" what he had written. The incident may be compared with one described by Miss Symons (p. 313), who says similarly—

I am certain that by no possible means could he have obtained so much as a glance at the written question.*

* Yet, apart from other possibilities, the question might have been read in this case with the help of a small mirror, even while the slate remained writing downwards,—a method which, as I learn, had never occurred to Miss Symons. I do not think, however, that Eglinton used this method in either case.

Mr. Davey writes perhaps more confidently than Miss Symons, and similar extravagant assertions about impossibilities meet us in many other reports of sittings with Eglinton. I think we may say that when these express the opinion of a capable general conjurer, they merit some attention; when they express the opinion of a capable general conjurer who is also a special expert in "slate-writing," they merit most serious attention; but when they express the opinion of an unskilled observer who is manifestly unaware of the difficulties either of continuous observation or of accurate recollection, they deserve almost no attention at all.

Note, in Mr. Davey's description of this incident, his reference to the fact that *the medium went out of the room*. This is not mentioned in his earlier account, and he mentions it in his later one apparently only for the purpose of showing what a splendid opportunity he—not Eglinton—took advantage of, to write some words upon the slate without their being seen by any other person. We cannot feel sure, after this, that Eglinton did not leave the room more than once, and that he may not have used the opportunity to substitute, for clean slates, other slates already previously written upon, or written upon there and then by himself while he was outside; and such a hypothesis would find some corroboration in Mr. Munro's recollection (p. 439) that "once or twice in the middle of the séance Mr. Eglinton had to leave the room to attend to someone at the door." He may also have dropped a slate on the floor once or twice, perhaps the very slate on which Mr. Davey had written the question about his relative, as he dropped one in the case described by Mr. Harold Murray (p. 297), and shortly afterwards indicated by his language that he had acquired, as Mr. Murray apparently thought, and as Mr. Lewis explicitly suggests (p. 372), "a little useful knowledge without the cognisance of the spectators." What are we to think of the testimony of a witness of performances like those at issue, when in one of his accounts he omits to record a fact of such cardinal importance as that the chief performer left the room during the sitting, and in the other account, *barely* and only incidentally records it? What scientific value can we attach to his investigation of "occult" phenomena in a case where the chief possibility to be excluded is confessedly the possibility of conjuring? *If, now, I should find it necessary, in order to explain some particular incident, to suppose that Eglinton left the room during its course, although the witness never refers to such an event in his report, could my assumption be regarded rightly as absurd?* Could it be maintained, without introducing special considerations, that the licence which I so allowed myself was utterly unwarrantable?

Let us now turn to his report of the sitting of October 9th, sent to the Society. In the account in *Light* Mr. Davey says :—

Questions, often beyond the knowledge of the medium, were asked, and the answers received on a small folding slate I had brought for the purpose.

In the later report he writes :—

X— having brought a folding slate we commenced our experiments with this.

And X— himself says, in his letter to Mr. Davey of November 2nd, 1884, (p 436) :—

Our questions were far too general the other day, and could have been answered by anyone, if they had the power of writing.

In these experiments, and also in the experiments with the coloured crayons, and with the locked slate, &c., we need not suppose that the slates were held above the table, or by the medium *and* one of the sitters, during the whole time of the experiment. It may be inferred that when Mr. Davey wrote that the answers were obtained under such conditions, he was taking for granted that the answers were being written while the sound as of writing was distinctly audible. That the slates were under the table much more than is directly stated in Mr. Davey's reports, would appear from the contemporary letter written by X— (p. 435).

The placing the slates beneath the table at all seems to me the worst part in the matter. For what object are they placed beneath the table? Therefore, see that all your communications take place *from the first* in view of all.

Putting these considerations together, it seems not improbable that what occurred may have been somewhat as follows. After Mr. Davey had written a question in the locked slate, it was held under the table by Eglinton and the answer written by him; Eglinton then placed it upon the table, and suggested that Mr. Davey should unlock it and hold the edges just far enough apart to allow him to drop an inked pencil grain within, that the test might be more satisfactory in case any writing should "come"; Eglinton then seized an inked pencil grain, having taken care that it should be already worn, and gave it to Mr. Davey to insert; the locked slate may have remained thenceforward above the table. The fault of Mr. Davey's description of the incident* would, on this supposition, be mainly simple omission.

In the later account of the long message I observe a phrase which

* There is only one writing in the locked slate described in the later report. In his account published in *Light* Mr. Davey says: "Between the famous slate presented to Mr. Eglinton by a distinguished personage, with a strong Brahma lock, securely fastened by myself, we obtained messages in the well-known handwriting of Joey."

strengthens the supposition which I made in the *Journal* for October, that Mr. Davey's request was suggested by Eglinton. He writes :—

I suggested a long answer to be written in reply to my query that it should be *something of interest* and I also said under these conditions I should like some Greek.

We cannot lay any stress upon Mr. Munro's recent statement (p. 439), written so long after the event :

Previous to Mr. Davey's asking for some Greek writing Mr. Eglinton had been telling us of a long Greek message he had received at another séance ; but compare Mr. Davey's phrase with that of Mr. Harold Murray in another instance (p. 297) :

Two slates held on upper surface of table. *Mr. Eglinton asked for some communication of interest.* The slate was filled with writing in less than one and a-half minutes.

Further, there is a slight, but especially noteworthy, difference of expression in Mr. Davey's two accounts in referring to the same time-interval. I might have pointed out, in comparing his reports of the sitting of June 30th, that we may not be justified in interpreting the expressions of witnesses very strictly in the face of Mr. Davey's use of the words "a few seconds" in his earlier account, and "upwards of a minute" in his later account, with reference to the same interval, the time during which he heard the sound as of writing the long message ; and it should be noticed that whereas in his later report of the sitting of October 9th he wrote that after the slate was held in position "INSTANTLY the sound of writing became audible" (the word 'instantly' being doubly underlined in the original mss.), he wrote in his earlier report : "Scarcely a minute elapsed before the pencil began to move." It should also be noticed that whereas in this earlier report Mr. Davey's question appears in the order of events *after* the slate was placed in position, in the later report it appears *before*. These discrepancies, I think, sufficiently show *how easily some of the events of a sitting may become transposed, and how rapidly an appreciable and perhaps important time-interval may dwindle out of recollection ; and we may venture to hesitate before accepting the remembrance of a witness, even when doubly emphasised, that the sound as of writing followed immediately after the placing in position of the slate.*

I may now leave a more prolonged comparison of these reports to the reader himself, who may, *e.g.*, find it not easy to discover, in the later report, an instance described with special emphasis in the earlier one, and upon which I commented in the *Journal* for October, p. 430 ; he will then not be surprised to learn, as I have just learnt myself, in a letter received from Mr. Davey in reply to my inquiry, that the

notes (referred to on p. 431) from which his reports were made, "were very short and were merely copies of the messages if I remember rightly with just one or two little items."

To the mistakes, then, of fundamental importance, which were forced upon our notice by a comparison of the independent accounts given by different observers of the same sitting, we have found parallels in the mistakes which appear from a comparison of the different accounts given by the same observer; not merely minor mistakes, of transposition, transfiguration, &c., including the miscalculation of time-intervals, but mistakes which involve the most serious and flagrant and complete omissions of events which were certainly not unobserved at the time by the witnesses, and the descriptions of which, for the discerning reader, afford obvious clues to the performance of the tricks.

(To be continued.)

THE CHARGES AGAINST MR. EGLINTON.

In the *Journal* for June, I prefaced a collection of narratives of slate-writing phenomena witnessed in the presence of Mr. Eglinton, with accounts of two incidents in his career, which appeared to me to justify us in regarding him as an impostor with some years' practice. Mr. Eglinton's reply—such as it is—to these charges, has recently been published in the spiritualistic journal *Light*. This reply need not be repeated here, as it has, I understand, been sent to all Members and Associates of the Society for Psychical Research; but I think it desirable to call attention to one or two points in it which might mislead unwary readers.

First, as regards the muslin and false beard worn by the figure representing "Abdullah," and subsequently found in Mr. Eglinton's portmanteau, he states that "the Council of the late British National Association of Spiritualists dismissed the matter as insufficiently attested," and gives as his authority for this statement an editorial article in *Light* for July 17th of the present year; which article again gives no authority except the recollection of certain members (unnamed) of the Council of the B. N. A. S. If, however, we turn to the reports of the Council meetings at which the subject was discussed, (which are given in the *Spiritualist* for December 27th, 1878, and January 17th, February 14th, and March 21st, 1879; and in *Spiritual Notes* for April and May, 1879), we find that they do not confirm this recollection.

Archdeacon Colley brought two charges against Mr. Eglinton in the letters from which I quoted in June; the one relating to the beard and muslin, from his own knowledge; and the other on hearsay evidence only, to the effect that Mr. Eglinton had "on one notable occasion kept dark by the authorities," been "detected dressed up and playing ghost" at a séance held on the premises of the B. N. A. S. This, Mr. Colley said he had learnt from a well-known member of the B. N. A. S. and from the wife of a

very active member. It was what Mr. Desmond FitzGerald characterised as "an imputation of a disgraceful character" against the authorities of the Association, of hushing up an exposure made at one of their own séances, that caused the Council to take the matter up.

Their inquiries resulted in the *corroboration* of the *first* charge—that made by Archdeacon Colley from his own knowledge—by drawing out the additional testimony of Mr. Owen Harries, in whose house the discovery of the beard and muslin took place.

It does not appear from the report of the Council meetings that the *second* charge against Mr. Eglinton—the one made on hearsay evidence—was either proved or disproved, and I accordingly made no allusion to it in the *Journal* for June.

As to the imputation against the "authorities of the Association," however, Archdeacon Colley admitted that he had made a mistake in so describing the persons to whom he had referred, and the whole subject was then allowed to drop. It is doubtless a confusion between these various charges—two against Mr. Eglinton, and one against the authorities of the Association—which has produced on the unnamed members of the Council of the B.N.A.S. the erroneous impression that the charge of which I gave the details, broke down.

There is, then, clear positive evidence that Mr. Eglinton produced sham phenomena in 1876.

I now turn to what is known as the *Vega* incident, which affords strong presumptive evidence that he was still producing sham phenomena in 1882.

I observe that when Mr. Eglinton has occasion to refer to the *Vega* incident he ignores the fact that his letter was alleged to have gone round by Bombay where Madame Blavatsky was, and the very important and suspicious part played by her in the transaction. Yet this confederacy with Madame Blavatsky (and I may observe that it is quite unnecessary—Mr. Eglinton's suggestion to the contrary notwithstanding—to suppose confederacy in the matter between him and any one but Madame Blavatsky) is in itself almost enough to discredit the whole phenomenon. When to this is added the equally suspicious nature of Mr. Eglinton's own proceedings in substituting for the envelope marked by Mrs. B. one differently marked by himself, we can hardly say that there remains any room for doubt. Mr. Eglinton tries to persuade us that the chances were millions to one against the lady on the *Vega*, Mrs. B., making the mark he wanted, and that it is therefore absurd to suppose that on the hypothesis of pre-arrangement he would have applied to her at all. But that this is not so is shown by what actually occurred. The mark required, to make the letter shown on the *Vega* apparently correspond with those which fell at Bombay and at Howrah, was three crosses in a row on the flap of the envelope. Mr. Eglinton made one cross before asking Mrs. B. to make a mark. This first cross was not, as it seems to me, at all unlikely to suggest to Mrs. B. to make another near it, and I am confirmed in this view by the fact that she actually did make another. If, instead of making it on the top of Mr. Eglinton's, she had made it at the side, he need only have added a third in her presence to produce a test which, though careful investigation would have revealed its weakness, would

probably have appeared flawless to nine readers out of ten. But it did not much matter to Mr. Eglinton whether Mrs. B. made the right mark or not. To ask her to make one was the easiest way of satisfying Mrs. Gordon, and he must by experience have known Spiritualists well enough to be aware that he was playing a game in which he might win, and could not materially lose, so far as their support was concerned. If he was honestly desirous that the envelope should be marked according to Mrs. B.'s independent wish, why, after he had destroyed the one marked by her—a proceeding for which inconsistent reasons have been given,—did he not at least mark the second in the same way in which she had marked the first? “Incapacity to understand the important element of test conditions” has been urged on his behalf; but I cannot myself think that his intelligence is as much below the average as this would imply in the case of such simple test conditions as are here involved.

Mr. Eglinton only appeals further to the absence of assignable motive to induce him to arrange a phenomenon of the kind. It scarcely needs pointing out that if the prospect of obtaining with Spiritualists and Theosophists the credit which he actually did obtain were too feeble a motive, Madame Blavatsky may well have had the means of supplementing it.

I have returned to this subject, and brought out these various points, because it seems to me very important; that on the one hand, charges of imposture should never be made against mediums without evidence independent of the marvellous nature of the alleged phenomena; and that on the other hand, when there is evidence of imposture it should be made widely known. If there have ever been genuine physical mediumistic phenomena, Spiritualists have done immense injury to their cause by hushing up cases of exposed deception, and thus, as well as by lax methods of investigation, encouraging its repetition.

I may, in conclusion, remark that I have read with care the evidence brought forward by Mr. Eglinton about his slate-writing. None of it appears to me to differ in essential characteristics from that which was printed in the *Journal* for June; and no more than that, therefore, does it exclude the hypothesis of conjuring.

ELEANOR MILDRED SIDGWICK.

CORRESPONDENCE.

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

SIR,—As “M.A. (Oxon.)” has publicly, though quite courteously, questioned the accuracy of a statement which I made in a foot-note to my paper on the Physical Phenomena of Spiritualism (*Proceedings* X. p. 66), to the effect that he asserted the entertainment of Maskelyne and Cook to be mediumistic, I think I ought to quote the passage on which I founded the statement. It is in a letter to the *Medium and Daybreak* of August 24th, 1877, which is headed “Conjuring, *plus* Mediumistic Phenomena,” and signed “M.A. (Oxon.),” and it runs as follows:—“I am glad to see that Mr. Alfred Wallace agrees, after seeing Lynn’s medium, with the substance of my letter in your issue of July 6th. Given mediumship and

shamelessness enough so to prostitute it, and conjuring can, no doubt, be made sufficiently bewildering. It is sheer nonsense to treat such performances as Maskelyne's, Lynn's, and some that have been shown at the Crystal Palace, as 'common conjuring.' Mr. Wallace positively says 'If you think it is all juggling, point out exactly where the difference lies between it and mediumistic phenomena.'"—I am, &c.,

ELEANOR MILDRED SIDGWICK.

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

DEAR SIR,—Though you do not choose to admit a letter of mine to the *Journal* because of its previous appearance* in *Light*, yet, perhaps, you will publish the following few sentences from it with regard to the criticism of séance reports in your last issue:—"I might more or less enlighten Mr. Hodgson upon several of the points he has touched upon in my own notes of séances. But I wish only to say in this matter that imperfections"—such as of coincidental observation—"in any report do not necessarily invalidate the facts related. If two or three newspaper reporters write an account of a street accident, their respective columns will probably show some discrepancies, arising from varied insight, position, &c., though the public will not gather therefrom that the street accident was probably a street play." . . . In case my reference as above to Mr. Hodgson's enlightenment is thought to be too general, I add one explanation only, and this with regard to the incident of Mr. Eglinton's copy of a drawing on a transparent, child's, or toy slate. Mr. Hodgson seems to have overlooked the fact that any kind of drawing, under the circumstances, was much more than the ordinary writing. He may think that, with practice and legerdemain, he could in time himself write, say some chance trisyllable, on a slate under the table without an observer's notice—a matter involving no "direct vision," i.e., involving no sight of the slate by the medium whilst writing; but he must acknowledge that it would be—with the same conditions of table and slate—a different thing, on the immediate suggestion of two close observers, to recopy or "trace," unnoticed, in red chalk, the first syllable of the word just fortuitously written. To trace over any few lines or figure, it would be necessary that for a few seconds the medium's eyes be directed both to the position of the syllable on the slate and to the point of his pencil. If the result, as in Mr. Eglinton's case, was the obtaining (not attaining as misprinted) of a portion of a drawing, "*roughly but correctly made*," without Mr. Smith's and my own knowledge, any further strong assertion remains justified.

I am, Sir, yours obediently,

J. MURRAY TEMPLETON.

39, Rue Gabrielle, Montmartre, Paris.

7th November, 1886.

* I had already explained to Mr. Templeton, *à propos* of a previous letter, that I thought it best to adhere to the ordinary rule in such matters, and not to print, as correspondence addressed to the Editor of this *Journal*, letters which had been previously published elsewhere. On his sending me the letter to which he now refers, I reminded him of the rule, but added that he of course had a right of reply to any remarks relating to himself and his evidence which had appeared in these pages.—Ed.

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

DEAR SIR,—As an Associate of the Society for Psychical Research I should much like to help on its investigations, for which I have, I think, some slight aptitude. but am withheld from doing so by the following considerations, which I should be glad to see discussed by other members of the Society for Psychical Research:—

Experiments in telepathy, automatic writing, &c., all require the will of the percipient to be, so to speak, annulled for the time, and the mind left blank, so that it may be invaded either by the influence of the mind of the agent, or by the sub-conscious memory, personality, or whatever it may be, of the percipient himself.

Now, though we have had no irrefragable *evidence* that intelligences other than human, or rather other than those of living people, can invade the mind voluntarily left blank in this way, yet it seems to me that it would not be wise to leave this possibility out of account, in venturing into such an unknown region. We should not think it wise to allow ourselves to be mesmerised by persons of known bad character: can we be *sure*, that by making our minds blank and abnegating our wills, we do not lay ourselves open to be affected by disembodied spirits, whom we should not at all have wished to influence us in life? Some who believe in such influences more than we see at present reason to do, hold the theory that steady *activity* of will is a defence against them, while *passivity* of will invites them: and there seems something rational in inclining to act upon this hypothesis in our present condition of ignorance; just as if being shipwrecked in the dark upon an unknown island, of which we were ignorant whether it was desert or inhabited by friendly or hostile tribes, we should be wise to put ourselves on the defensive.

The hypothesis that such influences, if they exist, would not necessarily make themselves known to our consciousness, is, I think, borne out by Mr. Myers' experiments in hypnotic suggestion, where the suggestion made from without appeared to the person under experiment to be from within. His experiments also seem to show that even in this case a sub-conscious resistance is almost invariably found to any practically undesirable suggestion made by the experiments, when the subject is a rational and self-controlled person. Is it impossible that some of the impulses which we take for granted come from our hereditary kinship with animals should really be thus impressed upon us from without?—Yours faithfully,

November 7th, 1886.

M. B.

SUPPLEMENTARY LIBRARY CATALOGUE.

The following additions have been made during the last three months.

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- [R] JUSSIEU (A. L. de) Rapport de l'un des commissaires chargés par le Roi de l'examen du Magnétisme Animal*Paris*, 1784
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- REICHENBACH (Dr. K. Freiherr von) Odische Begebenheiten zu Berlin in den Jahren 1861 und 1862.....*Berlin*, 1862
- Die Odische Lohe und einige Bewegungserscheinungen*Vienna*, 1867
- SCHAUENBURG (Dr. Carl H.) Tischrücken und Tischklopfen eine Thatsache*Düsseldorf*, 1853
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- Die Naturgeschichte der Gespenster*Weimar*, 1863
- WUNDT (Professor W.) Der Spiritismus. Eine sogenannte wissenschaftliche Frage*Leipzig*, 1879
- ZÖLLNER (Professor Friedrich) Zur Aufklärung des deutschen Volkes über Inhalt und Aufgabe der wissenschaftlichen Abhandlungen von Friedrich Zöllner.....*Leipzig*, 1880
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* Presented by Mrs. F. A. Moulton. ‡ Presented by the Publisher.

§ Presented by the Author.