

JOURNAL

OF THE

SOCIETY FOR PSYCHICAL RESEARCH.

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NEW ASSOCIATES.

- BALL, THOMAS STANLEY, Spring Bank, Fulwood, near Preston.
- CHURCH, MRS. MARY M., Ashland, Schuylkill Co., Pennsylvania, U.S.A.
- ELLIS, MRS. ROBERT, 40, Keppel-street, Russell-square, London, W.C.
- FOX, ST. GEORGE LANE, National Liberal Club, London, S.W.
- FREEMAN, MISS ADELAIDE C., Holcombe Cottage, Westcott, Dorking.
- KUHLENBECK, DR. LUDWIG, Osnabrück, Germany.
- TINCKLER, MRS., 496, Oxford-street, London, W.
- TEVEZ, VICTOR P., Llandaff House, South-road, Clapham Park, London, S.W.

MEETING OF COUNCIL.

At a Meeting of the Council held on the 17th of June, the following Members were present:—Professor H. Sidgwick (in the chair), Professor W. F. Barrett, Messrs. Edmund Gurney, F. W. H. Myers, Frank Podmore, and H. Arthur Smith.

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

Eight new Associates, whose names and addresses are given above, were elected.

It was agreed that two Members, Mr. Oscar Browning and Miss A. M. M. Hogg, should, at their request, continue as Associates.

The decease of Mrs. Georgiana B. Kirby, of California, an Associate of the Society, was recorded with regret

It was agreed that the names of five Members and Associates, whose subscriptions had remained for some time unpaid, or whose present addresses could not be ascertained, should be struck off the List.

Cash accounts for the months of April and May were presented in the usual form.

It was resolved that the issue of the *Journal* should be suspended during the months of August and September.

The next Meeting of the Council was fixed for Friday, the 29th of July.

GENERAL MEETING.

A General Meeting was held on Friday evening, June 17th, at the rooms of the Society of British Artists, Suffolk-street, Pall Mall. Professor Barrett took the chair.

Mr. F. W. H. Myers read a paper on "Active and Passive Automatism—the Dæmon of Socrates," of which the following is an abstract :—

The subject of Automatism widens as we look into it ; and in order to understand the automatic *writing* which was our first subject of inquiry, we have to take account of analogies of many kinds. In our last paper we discussed some pathological cases which showed a tendency to the formation of a separate mesmeric chain, or secondary personality, resembling the *soi-disant* personalities which communicate through planchette. To-day I wish briefly to review some other modes of automatic self-manifestation, *message-bearing* or *annunciative* like planchette, but some of them active in ways other than writing, and some of them apparently *passive*,—or, as I prefer to put it, messages by one stratum of the self to another through sensory rather than through motor channels.

We will take first a mode of automatism closely analogous to writing, which can be easily practised, and which has been often misunderstood. I mean table-tilting ; the attainment of a message by tilts of a table (not *raps* proper, which are a different thing), so managed as to spell out the letters of the alphabet. This is the simplest form of verbal message ; as employed, for instance, by prisoners in contiguous cells. These tilts, we may for the present assume, are due to unconscious muscular pressure on the part of the sitter or sitters ; but the messages thus obtained afford a curious inlet into the chaos of fragmentary cerebration which is always proceeding within us. [Instances given, and co-operation in experiment earnestly asked for.] Another form of active automatism is automatic utterance or trance-speaking. But besides these automatisms which run parallel to the active or motor side of verbalisation—*i.e.*, to word-writing and word-uttering—there are automatisms which run parallel to the sensory side of verbalisation—to word-hearing and word-seeing. It is true that messages thus conveyed to the conscious self are not usually classed as automatisms, but as hallucinations ; yet they are fundamentally the same phenomenon. They represent messages travelling upwards, so to speak, from unconscious to conscious strata of the self, thus effecting their purpose by the externalisation of sensory images, rather than by muscular movement. Where these messages come up from what may be termed the dream-stratum, as in dreams or madness, the close intertexture of sensory and motor elements is obvious enough. Our dreams are a jumble of incipient vision, incipient audition, incipient speech. The

madman sometimes vociferates incoherently himself, sometimes listens to the imaginary utterances of persecutors or angels. Again when the messages travel up from the *hypnotic* stratum, sensory and motor effects are practically interchangeable. When we inspire a post-hypnotic suggestion it is equally easy to make our subject speak or write, or to make him hear or see hallucinatory words.

Going deeper down to the *telepathic* stratum of our being, we find that telepathic messages (as the cases cited in *Phantasms of the Living* show) sometimes work themselves out in a *motor* form—prompting to writing, speech, or movement of a more general kind; and sometimes in a *sensory* form, as hallucinations of sight, hearing, or touch. Following these analyses, it is natural to inquire whether any phenomenon—automatic in the sense of not being consciously originated—exists on the sensory side in parallelism with the phenomena of automatic writing and speech. It is plain that an automatic activity of the *word-seeing* centre (if more than a mere delusive hallucination) would be a kind of *clairvoyance*. An automatic activity of the *word-hearing* centres (again assuming that something deeper than mere insane delusion is involved) would result in what are called *monitory voices*—a more or less completely externalised audition, whose substance is at least coherent, and perhaps superior in sagacity to the subject's conscious trains of thought.

The Dæmon of Socrates affords a conspicuous historical instance of such monition;—difficult to explain except on the view that a profounder stratum of the sage himself was thus communicating with the superficial or conscious stratum, in messages indicating at least a deeper insight—perhaps a wider purview—than the conscious Socrates could attain.

The voices which inspired Joan of Arc were of the same kind. Here, too, we have a message penetrating from the subconscious to the conscious strata of the personality, and acting as a stimulus to draw out from the organism its maximum of force. Nor need such an interpretation be in reality less encouraging than was the reference of the voice to a divine power by Socrates, or to saints and angels by Joan. If there be something deep within us which prompts to noble action, even when that action manifestly leads to death, we may conjecture that *that* cannot be an evil to which we are thus authoritatively invited.

A short discussion followed the reading of this paper. Mr. Lane Fox insisted that a true conception of the Self could not be reached except by the aid of Oriental philosophy. Major Taylor referred to the difficulty of supposing that a message tilted out by a table at which several persons were sitting was a reflection of the unconscious action

of some one of them alone. Mr. Lockhart reported a case where the movements of the table appeared to be such as unconscious pressure could not have caused. Mr. Myers, in reply to Mr. Lane Fox, pointed out that, whatever truths might be in Oriental philosophy, the *raison d'être* of the S.P.R. consisted in the application, as far as possible, of the scientific method to problems hitherto left to metaphysical or religious speculation. He admitted the difficulty urged by Major Taylor, and advised careful experiment as to the effect on the automatic answers of the presence or absence of each presumed automatist in turn. In reply to Mr. Lockhart, he urged the use of some simple contrivance which should make it mechanically impossible to communicate movement by mere pressure,—as by protecting a tongue of wood by a cardboard penthouse, on which the hands might be laid. Until movements could be induced either without contact, or with contact so applied as to be demonstrably unable to produce the movement, the mere impression of the automatist that the movement was independent of his pressure could not be held conclusive.

The Chairman reinforced the appeal for further experiment.

The Meeting then assumed a conversational character.

CASES RECEIVED BY THE LITERARY COMMITTEE.

G.—179

From Mrs. Morris, of Pentrabach, Trecastle, Breconshire.

This account originally appeared in *Rifts in the Veil* (p. 101), published in 1878, by Mr. W. H. Harrison. It is there reported, from memory apparently, by Miss Theobald. The account has been sent to Mrs. Morris, who accepts it as correct. The account has also been read through and signed by Miss E. B. B. (the sister who was present at the scene last described), who refuses, however, to write an independent account, or to allow her name to be given.

From *Rifts in the Veil*, p. 101 (published 1878).

One cold winter's night I awoke, and to my great surprise I found there was bright firelight in the room. I sat up in bed and noticed that the ordinary grate was not to be seen, but in its place appeared an old-fashioned open hearth upon which was blazing a splendid fire, the light of which filled the room and had woke me up. I saw a small strip of carpet laid down in front of the fire, but there was no fender. When we went to bed there had been a large fender, but *no carpet* and no fire.

As I looked with astonishment, I particularly remarked a bright pair of brass fire-dogs, with very curious and pretty twisted fireirons resting upon them. By the side of the fire was a beautifully-carved oak arm-chair, made with a square seat, the point of which was in front, and a rounded back. It was such a chair as was used 200 years ago. In this chair was sitting an old man; he was resting his elbow on the arm of the chair, and with his hand supporting his head; he was looking directly towards me, with an intent, sad gaze.

He was dressed in the style of the olden times—200 years ago—with knee breeches and stockings. I noticed, curiously, the flicker of the fire, as it was reflected in his bright knee and shoe buckles.

I woke my sister, who was sleeping with me, saying, “Do you not see that old man sitting by the fire?” She sat up by my side, but saw nothing, and advised me to “Go to sleep,” advice she acted upon herself, but I lay down and shut my eyes for a time, then sat up, and again saw the scene that I have described, and watched it for some little time, for I was not in the least frightened, not even at the sight of the old man, and I often wish I had spoken to him. At last I lay down and went to sleep. On awaking in the morning, my sister asked me what I had been talking about in the night, fully admitting that when I awoke her I was myself most fully awake, and not in a dreaming condition. We had been living in the house about two months when this occurred, and we found that it was known throughout the town to be haunted. We lived there nearly two years, and during the whole time were annoyed by mysterious knockings and noises, but the “White Lady” did not show herself until just as we were leaving. My father and mother had already returned home, sending me, with my younger sister and a young housemaid, to finish the packing up. On the Saturday evening my sister and I went out, leaving the servant to cord some boxes, and put the rooms in order; we did not return till past ten o’clock, when, to our surprise, we found the servant sitting in the hall with the front-door open. She began to cry on seeing us, saying she had been much frightened. She told us that after we had gone out, and she had changed her dress, as she was coming out of her room, which opened on to the front staircase, she thought she saw me coming upstairs, only I had changed my dress, and had on a long white one; she exclaimed, “Oh! Miss A., you are never going out, just now, in your best white dress?”

By the time she had said this the figure was close up to her; then she saw it was a woman, dressed in a long trailing gown of some white material, but she could not distinguish any face. The figure stopped when quite close to her, and suddenly she thought what it really was—the ghost!—upon which, with a scream, she sprang over the flowing train, ran down into the hall, and had been sitting by the open door ever since. She had seen the figure walk into the drawing-room.

The girl was so much alarmed that I told her she could make up a bed for herself in the room that I, with my sister, was occupying. It was the bedroom where I had seen the old man by the fire. That night passed quietly, but the next night a strange thing happened. We were very late; it was past twelve before we all three retired to our room. You will understand that there was no one else in the house but our three selves. As the door would not latch securely, I placed before it, to keep it shut, a chair, with a heap of things upon it. The servant and my sister were in bed. I was standing by the dressing-table, when suddenly the door was pushed open so violently that the chair was thrown out into the middle of the room. I turned round sharply, and there saw, standing in the doorway, the tall figure of a woman in a long white dress, such as had been described by the servant. The sudden opening of the door had so terrified both the servant

and my sister that I was compelled to give my attention to calming both of them down. I did not tell them what I had seen, as I would not frighten them more. I should add that when the figure went away, the door was drawn to again.

Some few minutes passed before I had quieted my sister. I then lighted a night-light, and put out the candle preparatory to getting into bed myself. To my surprise I saw, when the room was thus darkened, that there was a bright seam of light all round the door, which would not close tightly. I went and opened the door, and found the whole passage illuminated by this white light, as light as day, but I saw no more of the figure. This frightened me dreadfully, but I could only jump into bed and feel glad it was our last night in that house.

I should say that for many years that room had been nailed up as unfit for occupation, on account of the haunting; it had not been very long unfastened when we went to stay there.

E. B. B.

Mrs. Morris adds :—

Pentrebach, Trecastle, near Brecon, South Wales.

August 31st, 1884.

I have carefully read over the account you forward me of my singular experiences at Lyme Regis, and shall proceed to answer your questions as fully as is in my power.

As to the duration of the apparition, as far as I can remember from ten minutes to a quarter of an hour. I certainly saw the whole scene as described during the time my sister sat up by my side.

I fear the servant cannot be traced, but will make the attempt. She left our service six or seven years ago.

I found a curious account of the house in an old history of Dorset, but have never heard any person describe appearances in the house. Callers used to ask if we knew "Judge Jeffreys" had stayed in the house, and if we had ever heard it was supposed to be haunted, &c.

This, I should explain, was not until after the appearance of the old man, which took place very shortly after we went there.

This will answer also your eighth question, as numbers of people had made these remarks half laughing. The landlady was extremely angry at our mentioning these things, and threatened to summons the servant if it was repeated, or indeed anyone that interfered with the letting of her house, which was my reason for not wishing the name of the house to be published.

You are quite welcome to use my name, should you ever see fit to publish this account.

MARY ETHEL MORRIS.

Mrs. Morris writes again :—

September 9th, 1884.

I am enclosing my sister's written account of my curious experience at Lyme Regis in 1875. I will copy what she says about the dates, as I thought she has a clearer idea of them than I have.

"I am amused at your vague idea of the time we went to L——. We went there on the 16th of December, 1874, one year after papa was taken ill, which, as you remember, was the 16th of December, 1873. Probably I have more reason for remembering all that happened along there than you

have. We went to the Christmas dance, and it must have been early in February that 'one Jones' appeared to you, as — came down for a week from the 21st of January, and it was not until after he had left, but it was not long after.

"You returned to Brooke Lodge in September, 1876, the 21st or 22nd. It was, I know, a day or two after the fair, so that Mary Ann's experience must have come off on the night of the 18th or 19th. I can, I expect, get Mary Ann's address, if you like."

From Miss Lucia B., sister of Mrs. Morris.

Topsham, South Devon, *September 4th, 1884.*

Although I have constantly heard of strange things happening in the house in which we lived for nearly two years, I myself never heard or saw anything beyond the slamming of doors at night, doors which we knew to be locked. With respect to what my sister saw one cold night early in February, 1875, I perfectly recollect her awaking me and saying she saw an old man sitting in front of the fire-place, in which she asserted there was a large fire. I myself saw only that it was very dark, but I am fully convinced that my sister was quite awake and not dreaming at the time, also that she saw all that she has stated at that time, and also the later appearance.

LUCIA C. B.

Mrs. Morris writes again :—

September 17th, 1884.

You will by now have received my letter enclosing my sister's statement and Miss Theobald's account of my experiences at L—. Her account is much in the form I gave it her, and I did not see in it anything that required altering.

I enclose a short account of the noises I spoke of, and of my own personal experiences in different ways. I have often spoken of them among friends, and certainly have never, to my knowledge, been the subject of hallucinations in any way (*i.e.*, except such as have been veridical or doubtful.—F. P.).

During our stay of nearly two years' duration in the house before spoken of at L—, we were constantly hearing odd noises, generally at night, or at evening. A door leading to a cupboard in the drawing-room (which was directly under my bedroom) would sometimes bang loudly, although I and my sister, who shared my room, had carefully locked it before retiring for the night. This was of frequent occurrence. Also the sound of a heavy tread coming upstairs towards our room door, which sounded like the tread of a person coming slowly upstairs and wearing heavy boots, and always pausing when apparently close to the top. This sound was of a less frequent occurrence, and we sometimes thought might have been caused by rats. The house being old, rats abounded. On one occasion, when sitting up with my father, both he and I and the servant, who had gone that instant to a bedroom opposite for a few hours' sleep, heard a most piercing and awful scream; so terrible was it that my father started up with the exclamation, "Good God! What's that?" I sprang from my chair, and Charlotte, the servant, came to the door in horror to know if I had "heard that." She had gone to my mother's room, who was quietly asleep; and on going from room to room we found every member of the family asleep. The scream

lasted some seconds and seemed to fill the air of the whole house. It was beyond description.

October 22nd, 1884.

As to the disturbances at L—, I only remember hearing the sound of the door we had locked bang loudly. It was, to the best of my remembrance, always locked in the morning.

I must tell you, as I should have done at first, that I am one of four sisters, and that it was my sister Lucia who was with me at the time I saw the old man, but my sister Edith at the time the figure of the woman came and the room door was opened. I shall be writing to her this week and will ask her to tell me what she remembers of the event.

CORRESPONDENCE.

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

SIR,—In Part XI. of the Society's *Proceedings*, the paper describing how Mr. Eglinton was detected by Professor Carvill Lewis and others in the performance of fraudulent slate-writing, was introduced by two accounts of what were considered by the witnesses to be genuine and conclusive sésances ; and I was asked to annotate these accounts with the object of showing why we regarded them as inconclusive. I observe, in *Light* for June 18th, that "M.A. (Oxon.)," the writer of one of these accounts, thinks the character of my notes "extremely captious." I mention this, not as complaining of the expression, which I regard as very natural considering that he states that he has "not had an opportunity of studying with care" the experiments of Professor Carvill Lewis and Mr. Davey ; my desire is rather to emphasise—what others besides "M.A. (Oxon.)" may possibly have failed to catch—the intimate connection between my notes and these experiments. For the novelty and interest of Mr. Davey's investigations lie precisely in the proof they afford that the kind of suggestions of mal-observation and lapse of memory which I have made in the case of "M.A. (Oxon.)" and others, are not captious or over-refined. We can now refer to a large accumulation of evidence, proving that mal-observation and lapse of memory of the kind and degree required do actually occur, and so frequently that we cannot fairly accuse the witnesses of unusual or culpable carelessness.

Take an instance :—"M.A. (Oxon.)" thinks it absurd to suppose that at his sésance Mr. Eglinton himself read the number of the cheque enclosed in the locked slate. Why is it absurd, when we find a more surprising conjuring performance of Mr. Davey's thus described (see *Proceedings*, XI., p. 471) :—

"The next thing he showed me was a slate which locked up with a patent lever lock. After I had washed the slate, he asked me to write down on the inside any question I liked, then put a piece of chalk in, lock it up, and put the key in my pocket. The question I asked was, 'What kind of weather shall we have to-morrow ?' He was out of the room while I wrote it down, and it was locked up by the time he came back ; he then placed it on the table, the gas being alight at the time, we joined hands and put them on the top of the slate. After a little I again heard writing, and when I opened it there was the answer, in red chalk, each side of the slate : 'Ask the

clerk of the weather.' It had been written with the piece of chalk I had put in. I am quite certain the slate had not been opened after I had locked it up."

Why could not Mr. Eglinton read and write down the number of the cheque in a locked slate under the observation of "M.A. (Oxon.)," if Mr. Davey could read and answer a question in a locked slate under similar observation?

Again, "M.A. (Oxon.);" challenges me to explain how Mr. Eglinton, at a séance in Russia, read the number of a bank-note enclosed in a sealed envelope. I do not think that I am bound to take up the challenge, since the only account of the incident to which "M.A. (Oxon.);" refers is Mr. Eglinton's own! But even if this account were confirmed by another witness of unquestioned probity, it would be sufficient to reply by another question. How did Mr. Davey read the number of the coin in the case described as follows (*Proceedings*, XI., p. 469) :—

"I took a coin from my pocket without looking at it, placed it in an envelope and sealed it up. I am certain that neither Mr. Davey nor myself knew anything about the coin. I then placed it in the book-slate together with a piece of pencil, closed it as previously and deposited it on the table; and having placed my hands with those of Mr. Davey on the upper surface of the slate, waited a short time. I then unlocked the slate as requested, and to my intense amazement I found the date of the coin written, by the side of the envelope containing it. The seal and envelope (which I have now) remained intact."

In the two cases I have quoted we know that mal-observation and lapse of memory must have occurred from the nature of the description, combined with Mr. Davey's assertion that the performance was mere conjuring. If independent evidence is desired we may turn to a sitting at which Mr. Hodgson was present. Take, for instance, the incident marked [c] at p. 427, too long to quote, but ending thus :—

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

And read Mr. Hodgson's comment at p. 488 :—

"This statement is erroneous. Mrs. Y. had not the slate under her eye the whole time, nor was it the case that either her daughter's hand or her own was placed upon it continuously."

I must further observe that, though anyone who reads carefully the accounts of Mr. Davey's performances and of Mr. Eglinton's will, I think, perceive a striking similarity between them, it is not in any way necessary for our argument to prove that they closely correspond. Our point is simply that no materially greater amount of mal-observation and lapse of memory need be assumed in order to explain any of Mr. Eglinton's performances as conjuring, than has been shown to have actually occurred in the case of Mr. Davey's sitters. All difficulty in supposing the whole of Mr. Eglinton's so-called mediumistic performances to be tricks is thus removed, while at the same time we have positive evidence that he does trick sometimes.

But "M.A. (Oxon.);" seems to think that it detracts from the value of Mr. Davey's experiments that they were not performed in the presence of himself or other leading Spiritualists whom he names. To this it seems to me sufficient to answer that we have really no reason to regard these persons as experts for the purpose of the present inquiry—that is, as having sufficient knowledge of both avowed conjuring and the performance of mediums to be able to compare the two. And of "M.A. (Oxon.);" himself we know that in 1877—five years after his Spiritualistic experiences began—he expressed the opinion that it was absurd to regard the entertainments of Dr. Lynn and Messrs. Maskelyne and Cooke as mere conjuring; while we have no evidence that he has given any attention to conjuring since then.

If, however, "M.A. (Oxon.);" really is an expert in distinguishing conjuring performances from mediumistic ones, I would suggest that he has now an opportunity of showing it by explaining exactly how, if the accounts of Mr. Davey's performances and Mr. Eglinton's were presented to him for the first time mixed up and so that he did not know which was which, he would distinguish the genuine from the spurious. If this cannot be done, does it not behove Spiritualists to leave Mr. Eglinton's evidence as at best inconclusive, and to seek for better?—I am, sir, yours faithfully,

ELEANOR MILDRED SIDGWICK.

P.S.—I may mention another point on which I cannot agree with "M. A. (Oxon.);" He suggests that Mr. Davey should take certain records of Mr. Eglinton's phenomena, and duplicate them in the presence of the observers who originally recorded them; and until this has been done, being unable to say "what similitude his tricks bear to the genuine thing," he thinks it needless to discuss the matter further. But apart from the improbability that Mr. Davey and Mr. Eglinton know all each other's tricks, it seems to me evident that for performer B to reproduce the illusions of performer A, under what the investigator, not the performer, supposes to be the same conditions, is not to do the same thing but something indefinitely harder. This is easy to illustrate. Suppose that the officer spoken of by Mr. Hodgson at p. 385 of *Proceedings*, XI., had required from another Indian juggler an exact repetition of the jumping coin trick as described by him. Is it not probable that, after the discussion which had taken place, he would have noticed that the second juggler took the coin out of his hand instead of letting him put it on the ground, and that in consequence—unless his *a priori* confidence that the trick must be the same overcame his confidence in his own observation—he would have said: "No, this won't do; the conditions are not the same; with the first juggler I placed the coin on the ground myself, and yet it jumped like the others."

MR. R. HODGSON'S CRITICISM OF AN EXPERIMENT IN PSYCHOGRAPHY.

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

SIR,—Being at present much and variously occupied, it is only within the last few hours that I have become aware of Mr. R. Hodgson's criticism, in the new number of our *Proceedings*, of a case adduced by me in a paper printed in *Proceedings*, Part X. I cannot allow that criticism to go

unanswered, and I must address my reply now to the comparatively limited public which has access to the *Journal*, as no opportunity was offered me (in accordance with a convenient, and now rather frequent practice,) of meeting Mr. Hodgson's remarks in the same number of the *Proceedings*.

Mr. Hodgson's first remark (p. 392) upon my account, which he quotes from *Light*, is that "the phenomenon was not a simple and isolated one," by which I suppose him to mean that other experiments at the same sitting had preceded it. That was so; but I am unable to see how this circumstance affects the "simplicity" of the phenomenon in question, as such, or its "isolation," considered as a distinct experiment. Mr. Hodgson probably means that the earlier use of slates might have afforded opportunities for preparation or confusion. And no doubt it might, but only on the supposition that my testimony fails to show that such preparation could not have been available, and that such confusion did not exist.

The next remarks, that the "phenomenon was not suggested," or previously prepared for by me, that the slates were Eglinton's, and that a pile of his slates were on the table, are all correct. It was expressly on these very accounts, and just because the case did *not* contain the additional elements of cogency so often found in evidence of psychography, that I selected it in my paper—"On the Possibilities of Mal-Observation"—as typically illustrative of "the extent of the claim I make for average powers of observation as against the possibilities of conjuring." (*Proceedings*, Part X., p. 87.) My contention was that average powers of observation can dispense with extra-precautionary conditions; it was not my object to adduce what is commonly regarded as the "best" evidence, but to draw the line of sufficiency. I do not object to Mr. Hodgson reminding his readers of these circumstances of the experiment, but I also wish to remind them that these circumstances belong to the statement of the issue, and not to its determination.

Mr. Hodgson adds:—"Mr. Massey's attention, moreover, seems to have been partly given to the temperature of Eglinton's hand." Certainly, when the change of temperature was observable I noticed it. I do not know whether Mr. S. J. Davey produces changes of his temperature at will, to distract the attention of his sitters. As I had stated "I was sure that success was near when I felt the coldness of the medium's hand," I think Mr. Hodgson might more reasonably have inferred (as the fact was) that my attention was stimulated by this expectation. But if he means that I was watching for this symptom instead of giving my whole attention to the slates and to Eglinton's behaviour, I can only say that the supposition seems to me as unwarrantable as it is certainly unfounded. The symptom, when it occurred, always came upon me as a surprise; the fall of temperature when I was holding the hand having in at least one instance with Eglinton, and in several with Slade, been so startlingly rapid as to be in itself phenomenal.

After this preliminary skirmishing, Mr. Hodgson comes to closer quarters with my evidence. And, first, he finds an ambiguity in my statement:—"Both slates were then, as I carefully assured myself, perfectly clean on both surfaces," and asks: "*When* did Mr. Massey assure himself, *before* or *after* Eglinton laid one slate upon the other?" I should have thought it unmistakably clear from my statement that my examination of the slates,

and the putting of them together by Eglinton, were parts of one uninterrupted transaction, and that therefore my examination must have *immediately anteceded* the act of joining the slates. That was the natural order, and the other would imply an original neglect to take an obvious and necessary precaution, and an after-thought and after-action to correct it. There is surely no "ambiguity" in not expressly excluding such a supposition. A sentence or two later, Mr. Hodgson "thinks" that "less violence" would be done by the natural interpretation of a report in which it would perhaps occur to very few to find an alternative reading. I appreciate the circumstances that, in Mr. Hodgson's view apparently, there might have been *less* liability to deception as to the state of the slates, if my examination had been *subsequent* to their junction by Eglinton; but he makes use of this inadmissible supposition for the double purpose of suggesting (1) that I may have failed in the particularity of statement I myself describe as essential ("Who, in this case," he says, "placed the slates together again? Mr. Massey or Mr. Eglinton? 'We must have particularity of statement,'" &c.); (2) that in one event, in that case, a possible opportunity for substitution would have arisen. The ambiguity is, I submit, entirely of Mr. Hodgson's own creation.

I come next to his foot-note (pp. 392-3). Having, in my paper, quoted with verbal accuracy my report in *Light*, and Mr. Roden Noel's endorsement of it, I subsequently, in the same paper, treated this endorsement ("every word of this account I am able to endorse") as an adoption by Mr. Noel of my report, as equally applicable to his own observations as to mine. Certainly Mr. Noel, who, perhaps I may say, wrote to me (as also in a letter which has been published in *Light*) in warm commendation of my paper, has taken no exception to a passage which, in Mr. Hodgson's opinion, goes beyond the authority of the original report and endorsement. Indeed, I cannot see what Mr. Noel's endorsement could have meant at all, if it did not confirm, from his own observation and memory, every statement which was obviously of evidential importance, and which did not appear from the account itself to be exclusively my own. Had Mr. Hodgson insisted that different witnesses should give independent accounts, not merely adopting the report of one, he would have put his objection in a right form, and I should have been the first to agree with him in principle, although for the purpose of my argument it was not *necessary* to show that the *other* witness had made exactly the same observations as my own, but only that his observations were not at all *opposed* to mine. But Mr. Hodgson's criticism on the point as it stands, that "there is nothing said in the original report about Mr. Noel's 'carefully assuring' himself," is an attempt to raise a doubt upon the scope and meaning of Mr. Noel's endorsement which is excluded on the face of the latter by the terms used.

Mr. Hodgson finds less apparent ambiguity in the expression—"which we *then* 'carefully assured' ourselves were both clean on both surfaces"—used in my argumentative recapitulation of the report, than in the expression of the report itself—"Both slates were then, as I carefully assured myself, perfectly clean on both surfaces." I confess I can with difficulty imagine anyone reading these two sentences as fairly admitting different meanings in

regard to the moment of the act of assurance. I do not wish to characterise any part of Mr. Hodgson's criticism, but I will allow myself to say that it does not err on the side of liberality, and that the objections we have been hitherto considering, though perhaps hardly describable as subtle, seem to require for their apprehension an effort of imagination.

Having brought himself to suppose that I meant the only thing I could naturally mean, viz., that my examination of the slates was "just before" Eglinton put them together, Mr. Hodgson is of opinion that in that case my observation may have been deceived, that there might have been one side of one slate which I never saw, or that another slate might have been substituted for one of the two slates, both sides of which I did see. As he rightly adds, I did not state that I took the two slates into my own hands, and he is also quite right not to suppose me at that time (or, I may add, now) "an expert in detecting sleight-of-hand manipulations of slates. And so much," he adds, "for the amount of mal-observation required."

Mr. Hodgson thus closes the case as to mal-observation with, as we see, a simple delivery of his opinion as to what was possible consistently with my statement, or rather, notwithstanding it. He allows my "then" to mean "just before" the act of joining the slates, though he emphasises the "before," and I insist on emphasising the "just." The form of my statement admits of no interval of time between my "assuring myself" as to the state of the slates and the physical act of joining them by Eglinton. I claim for my careful and deliberate statement its true and only possible meaning of actual immediacy. (We are now, it will be observed, on the point of observation, not of memory.) The second of Mr. Hodgson's alternative suppositions—substitution—either negatives this immediacy, postulating an unobserved interval, after I had "carefully assured" myself of the condition of the slates with a view to this very experiment, and before their junction, or suggests an effect of *instantaneous* sleight-of-hand, similar to that which notoriously defies detection in the case of playing cards, coins, and other small objects. The first of his suppositions casts doubt on the sufficiency of my examination of the slates actually used—of which presently. But first as to substitution, and the "sleight-of-hand" theory of that, *without* the appreciable interval which I say is excluded by my testimony, whatever that may be worth on this point. Now I will give up the whole question, as regards exclusion of conjuring in this particular case, if Mr. Hodgson will produce any conjurer in the world who will effect for me (who am, as said, no expert) such an unobserved instantaneous substitution of a slate of the described dimensions, who will, in short, do what the particular supposition we are considering suggests to have been done in this case. And I would pay any, and more than any sum which could reasonably be demanded for the single performance. (Of course, if Eglinton could do this, he would be an expert in sleight-of-hand of the very first order, and could easily find a market for the legitimate exercise of such skill.) I need here hardly point out, that when the question is of sleight-of-hand, properly so-called, it would be irrelevant to raise the difficulty we hear of in the case of Mr. Davey, that observation will be more suspicious and keener with an avowed conjurer than with one the nature of whose powers is regarded as problem-

atical. The three-card trick does not succeed by virtue of any such doubt in the spectator's mind, and Dr. Herschell, who I saw the other evening perform some amazingly clever tricks of this sort at an evening party at Mr. and Mrs. Eglinton's, explained after each how it was done, leaving us just as much perplexed at the next.

The other sub-alternative of Mr. Hodgson's second suggestion supposes an interval during which substitution was effected, not by what is strictly and accurately describable as "sleight-of-hand," but by a dexterity which must have been conditional on an abeyance of our observation. I am hypothetically admitted to have really examined the four surfaces of the slates presented for my inspection, and then it is supposed, contrary to testimony as clear on this point as I could make it, that these slates were not put together by Eglinton immediately, but that he was able to produce and substitute for one of them another slate before my eyes, I being innocently unconscious—it is not yet a question of subsequent recollection—of this happening. Those who have read my paper on "The Possibilities of Mal-Observation" will, perhaps, remember that I by no means contended that such a lapse of observation could not occur in general, but maintained that the possibility or impossibility of its occurrence depended entirely on the witness's ignorance or knowledge that he was at a critical moment of the experiment, and of the particular danger to be guarded against to exclude deception. Nor should I claim for quite inexperienced observers, unless exceptionally quick-witted, a full appreciation of the important moments for observation, without having had occasion to consider these by the light of earlier experiences, and of the doubts thus suggested. I cannot, indeed, admit that on the very earliest occasions—now 12 years ago—of my own investigation of psychography, I was not as fully alive to the danger of substitution as I am at this moment, or that I was ever so illogical as not to have distinctly before my mind the fact, that if at a given moment it was important I should ascertain slate or slates to be clean, it was equally important that from that moment onward I should beware of a surreptitious change, and of every movement which might facilitate it. The witness must, however, as I also insisted in my paper, make it clear from his report that he appreciated important possibilities at the time, and I do not claim for experience any presumptions which would amount to an exemption from this rule. Mr. Hodgson does not suggest that my report insufficiently shows this, nor do I see how I could have made it more evident that substitution and insufficient examination were the dangers which, at the moments of liability to them, I had especially in view. But if this is granted, the supposition that I nevertheless failed to observe (covering compendiously with the word "then") an interval, and an action in that interval, between my examination of the slates and their junction, is one to which I will only reply by an appeal to public candour, if not to Mr. Hodgson's own.

Next, as to the question of due examination of the slates. Mr. Hodgson considers that my statement, "both slates were then, as I carefully assured myself, perfectly clean on both surfaces," does not sufficiently show that I may not have been deceived on this point. I must again remark that it is a question of my appreciation, at the time, of the importance of ascertaining

this fact, of whether my testimony sufficiently showed this appreciation, and also a sense of the importance of a distinct examination of each surface.

It would, of course, not require an extremely dexterous conjurer to present one side twice for examination by an observer who was not awake to the fact that this was a critical moment at which it might probably be a conjurer's object to deceive him. Hence my mention in my report of my "carefully" assuring myself, and my particularity of statement as regards "both" surfaces of "both" slates. The fact that a slate is free from a great quantity of writing (such as we afterwards discovered) does not require "careful" ascertainment if there is no danger of illusion, against which the form of my specification all through the sentence was obviously intended to suggest my precaution. It was just on this account that I contrasted it in my paper last year with another form of statement by another witness ("We examined the slates and satisfied ourselves that they were clean") which I regarded as insufficiently showing apprehension of this very liability. "You can only ascertain that a slate is clean," I said, "by successive examination of both its surfaces, the evidence of which must, in the reasonable intendment of the witness's language, exclude all possibility of deceptive manipulation while the surfaces seemed to be displayed." I certainly cannot undertake, at this distance of time, to say positively *how* my examination was conducted, whether by taking the slates in my own hand, or by seeing them turned slowly and deliberately round, one immediately after the other, immediately before they were put together; but I can most positively say that not only in 1884, but 12 years ago, I was just as alive to the possibilities of "hocus-pocus" in such a case as I am now.* But what is more to the present purpose, I contend that the language of my report, in its "reasonable intendment," should sufficiently satisfy a fairly critical reader that this was a point present to my mind. I repeat my challenge to any conjurer to succeed with an observer prepared for the possibility in question at the right time, and I maintain with undiminished confidence that testimony fairly raising the inference that the witness was so prepared is testimony which excludes the possibility of the illusion having been induced, just in proportion to the certainty of the inference from the language used. And this is the main thing; for, of course, whether I expressed myself on this point with sufficient clearness in my report is of quite secondary importance, as judgment against me herein would at most show me to have been not perfectly fortunate in my selection of a case to exemplify my general position as to the value of observation and testimony. The position itself would remain untouched. And in speaking further on of my "large trust in human observation," Mr. Hodgson seems to betray failure to apprehend the condition by which in my paper I so carefully limited this trust, which conditions being fulfilled, however, distrust of human observation strikes at the very root of all human testimony whatever.

Without discussing Mr. Hodgson's note at p. 393 (as to whether I had

* In part proof of this, I may say that during the Slade prosecution in 1876 I expressly recognised the reasonableness of Professor Lankester's suggestion as to possibilities of unobserved slate-reversal. See p. 20 of my preface to the English edition of Zöllner's *Transcendental Physics*.

not misplaced my *feeling* of assurance or the *process* by which I had assured myself, the precise meaning of which I confess myself unable to understand, I pass to the second division of his criticism, which concerns lapse and illusion of *memory*. Somewhere, I think, in the articles of Mr. Hodgson in the *Journal* last autumn, there is a remark that I neglected this question in my paper. Fortunately my answer to this reproach will cover much of the present ground. If my critic had not failed, as above remarked, to appreciate the conditions and limitations under which I maintained *observation* to be reliable, he could hardly have failed to see that they are the very same conditions which guarantee us against lapse or illusion of *memory* within the period during which an honest witness would profess to state his impressions from memory at all. That is to say, particulars which have been antecedently recognised as of vital importance, and to which, consequently, attention has been specially directed, are necessarily stamped upon memory as the most evidentially important facts. Now if a report written, we will say (as in my case), a few hours after the occurrence, emphasizes these particulars by definite specification (or by any form of words really equivalent to that) : if the report even, as it were, labours the point (as when I said, "forthwith, and without any previous dealing"), showing as evidently as language can the great importance which the witness attached to it *at the time of writing*, then, I say, criticism of such testimony has only two alternative judgments upon it : either that that sense of importance, evident in the report, was mentally present at the right time during the occurrence recorded, or that the witness, if in fact inaccurate, has been consciously "improving" the record of memory, and is to that extent a *dishonest* witness. When Mr. Hodgson suggests that my own statement, that Eglinton "forthwith, and without any previous dealing with them, presented one end of the two slates, held together by himself at the other end, for me to hold with my left hand, on which he placed his own right," is a statement insufficient to assure a critic of my testimony that I really *remembered* the immediacy of the sequence of acts, I can only say (putting aside any question of my veracity) that a critic so doubting would fail to appreciate the evidence afforded by my language of my having attached at the right time great *importance* to this very fact of immediacy. And so Mr. Hodgson's further supposition of an apparent "accident" (such as a pencil dropping out, with the consequent proceedings on Eglinton's part, &c.) intervening and being *forgotten* by me, is equally inconsistent with the sense of the importance of this moment of the experiment evinced by my testimony. I say this without any regard to the testimony being my own ; but, in fact, Mr. Hodgson's suppositions belong to my own earliest mental equipment as an investigator of this phenomenon. And it would indeed be wonderful if expedients so simple as that supposed had not long ago been perfectly familiar to my mind as possibilities, after all my investigation, thinking, reading, and talking—often with the most acute sceptics—on the subject. This is a remark applicable to experienced investigators in general. But new comers never credit their predecessors with this sort of intelligence, and my patience has often before been exercised by suggestions which assume my simplicity. No investigator can do more by his testimony to exclude such suppositions as those which

Mr. Hodgson considers "not improbable" in my case, than make the aim and direction of his vigilance apparent, raising a necessary inference that any "trivial incident" or "interruption" crossing the line of that aim and direction would have excited his instant and jealous attention. He cannot keep saying this, that, and the other thing did *not* occur, excluding them by an express process of exhaustion. Mr. Hodgson would not, I presume, deny that he has approached the consideration of my testimony controversially, with a foregone conclusion, and finding that careless and inaccurate statements are in fact frequently made on points of evidential importance—which I never doubted—he neglects the criteria by which we are able to distinguish, in the case of an honest witness, between statements originating in genuine and exact memory, and other statements which may or may not be thus derived, and which are fairly exposed to adverse suppositions.

I must add some words on the trick-slate hypothesis. Mr. Hodgson seems to assume that, because I did not add this suggestion to the fraudulent alternatives mentioned in an appendix to my report, I had not then heard of trick-slates, or at least was wholly ignorant of their contrivances. This ancient explanation was familiar to me years, probably, before Mr. Hodgson ever heard of psychography. It was necessarily pressed upon my attention during the progress of the Slade prosecution, when every conceivable way of accounting by fraud for the various psychographic effects was put forward. Trick-slates were shown to me, others described to me. But, of course, Mr. Hodgson would say that that is to no purpose, if I did not happen to get hold of the right one applicable to my case with Eglinton. When, however, he suggests, nay, treats it as evident, that this hypothesis was an after-thought last year, when I came to write my paper for our Society, because I did not mention it in my report, he seems to forget that I may have considered it to be already excluded by my examination of the slates. The two alternatives I dealt with—substitution and suppressed writing—and which I even described as the only conceivable alternatives—other than unverity—"to occult agency," were such alternatives as I supposed might present themselves to some minds as consistent with the evidences of my senses. But I rather believe the truth to be that this explanation had become so unpractical to me by free and frequent handling of all Mr. Eglinton's visible slates—his non-production of any other bringing us back again to the point of immediate observation—and by the notorious facility afforded to all his visitors in this respect, that my *logic* did here fail me, and that my hastily-written *appendix* to the report—after I had received that back from Mr. Noel and before posting it on to *Light*—was not well-considered. But when I was writing my paper last year, it occurred to me that as I had in fact carried the slate away with me (though chiefly then with a view to the hypothesis of chemical writing), and that under a condition I *did* distinctly remember, and which was *habitual* with me in such cases (never, that is, to let a slate I intended to take away be for a moment removed from my own custody or sight), I might as well state that fact for its additional value as excluding *one* theory of trick-apparatus. And whether Mr. Hodgson will credit the statement or not, I do most unhesitatingly declare that my memory *was* sufficiently clear and definite two years

later (and is *still*) for what I said in my paper on this point,* just as it is at this moment sufficiently clear and definite as to *all* essential particulars of that séance with Slade, *twelve years ago*, when the fallen chair was picked up and placed at my side, at my sudden request, and at a distance of five measured feet with a clear space from Slade, while I was watching that chair and that space intently.† And I do, indeed, differ very widely from Mr. Hodgson on the psychology of memory, if he holds this to be impossible, or even in the least improbable, when a deep impression has been made upon a consciousness intensely fixed and interested in the evidential details.

Mr. Hodgson's other supposition—that a false flap over the writing had been transferred to the *other* slate during the closure of the two, though of course not excluded by my custody of the inscribed slate, would not admit of prior examination of both slates in the hands of an investigator, as the flap must lie loosely over the writing concealed by it, if it is to fall free into its fitting in the other slate at the required moment. But as I did not positively state at the time, and certainly cannot now, that I did examine the slates in this way, when I “carefully assured” myself that they were clean, anyone who likes to suppose that Eglinton would run the risk of such an examination being required of slates, one of which had writing concealed by a loose flap, may congratulate Mr. Hodgson on the success of his criticism. I have had to examine it at a length which must appear inordinate; but it is easier to raise objections to evidence of this kind with brevity than to deal with them succinctly, especially when the principles on which testimony should be considered come necessarily into question.

But I am quite sensible that behind all apparent criticism lies the *argumentum baculum* of Mr. S. J. Davey and his reports. I am not dismayed by the alleged inconsistency of the latter with a thesis of my last year's paper—the reliability of well-discriminated statements of sense-perception. I do not think that inconsistency has been established, and disinclined as I am to divert myself from more interesting pursuits to an uncongenial inquiry, I may hereafter, circumstances permitting, ask leave to institute in the *Journal* a more curious investigation of Mr. Davey's case than has yet been undertaken.—Your obedient servant,

C. C. MASSEY.

* Although it would *not* serve me now as to some previous details—such as the mode of my examination of the slates. This is no doubt partly owing to the fact that my carrying away the slate kept alive in my mind the precaution which made that act of evidential value.

† This is a piece of evidence—recorded at the time—which has never yet, as far as I am aware, been dealt with by any critic. But I maintain that for a Society like ours to ignore its bearing upon the not more inexplicable phenomenon of psychography, and the evidence for that, when this evidence is largely prejudiced by allegations against the character of mediums like Slade and Eglinton, can be satisfactory to no candid mind. What degree of mal-observation, of memory-illusion, would Mr. Hodgson or other leading sceptics of our Society be content to suppose in this case of the chair, looking at the fact that the report was written from notes taken almost immediately after the sitting?

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

SIR,—Mr. Downing's letters in the March and May numbers of the *Journal* induce me to think that a brief record of my experiences in thought-reading may be of use to those who are investigating the subject. I may state at the outset that, except on one occasion when I tried figure drawing without any result, my only experiments have been in naming cards.* If I had found that any progress seemed to be made by practice, I should have carried these much further than I have done; but comparatively few though they have been, they seem to me to point to certain conclusions which I will proceed to mention.

In the first place success has almost invariably been in reverse ratio to the expectation of it, strange to say, thus my most successful attempts have almost always been the first, when neither I nor my friends, the different operators, had any idea that the experiments would succeed; and in the only case of a first essay where I felt confident of success, the operator being a sister with whom I am particularly *en rapport*, I failed utterly to obtain any result. Then it has almost always happened that on the second trial, after a successful first attempt, and when both of us anticipated improved results, we met with almost entire failure. Quite recently, after frequent fruitless experiments with the sister above-mentioned, I obtained some fair results by trying contact, but very unexpectedly as I have never found contact produce any effect in other cases. To give an idea of what I call success, I may mention that allowing myself two guesses—if I may use a word which is slightly misleading—at each card, I have named as many as seven right out of a dozen, of which three were named the first guess; and have frequently scored about half that number. In fact, if I am successful at all I usually get three or four correct out of 12. Experience has shown me that it is no use going on after a dozen.

Sitting opposite the operator with my eyes closed, I usually *see* the card, as if it were placed at the back of and slightly above my eyes, and this vision, as a rule, occurs *instantly* if at all. The method I have adopted is that the operator cuts the pack and looks at the bottom card, at the same time saying "Yes" as a signal, so that I am able to know when the appearance is instantaneous.

I must, however, admit that on the occasion of less successful experiments I have often failed to "see" any card, and then a vague impression of several cards is produced, as to which I cannot say whether it is visual or mental, and then I feel as if I were blindly guessing—quite a different sensation to the former, even if the result prove correct. In these cases, waiting a long time—two or three minutes—is no use. Either several cards suggest themselves, none producing any vivid impression, or the field remains perfectly blank. This is what has always happened after about a dozen cards have been selected.

The conclusion I have formed, rightly or wrongly, from the above and similar facts is that the process depends not on any effort of will on the side of either operator or patient, but in the case of the former on the power of

* The writer has sent his results, which are decidedly striking. They will be presented shortly (it is hoped) in conjunction with other records.—ED.

concentrating thought so as to produce a clear image to her mind, and, in the case of the patient, on the power of excluding other thoughts, so as to leave his mind open to impression. If, therefore, either party be tired, the experiments fail according to the degree in which she or he is unable to produce suitable conditions.

It may be of use if I chronicle one or two further incidents. I adopted the above-mentioned method of selecting a card, as I twice found that when the operator (a different person on each occasion) had chosen a card from the pack facing her, I guessed a card which she had momentarily intended to choose, but had changed her mind. This seems to conflict very strongly with Mr. Downing's theory, and only partially to agree with mine; for there was certainly no concentration of thought on that card, still less any expectation or intention that I should guess it. The one she did select I neither "saw" nor thought of at all.

I have tried experiments with 10 ladies and one gentleman, and have succeeded well with two (one of whom is a sister of mine, but not the one previously mentioned), and indifferently with two others. Also on one occasion I guessed two out of three cards when two ladies were acting as joint operators who had each failed singly. In this case none of us had any expectation of success, and I was unfortunately unable either to prolong or repeat the experiment; it is the only instance in which I have had any result with joint operators.

I have tried on several occasions the rôle of operator, but without result. It should, however, be mentioned that the most successful operator I ever experimented with had been in the habit of acting patient to her brother, though only guessing the suits, and it was their success which prompted my attempt. Miss — began as patient with me, but failed entirely to "see" anything, whereupon we reversed the parts, with great success. In that case, however, the second experiment proved almost a complete failure!—I am, yours obediently,

H. G. R.

SUPPLEMENTARY LIBRARY CATALOGUE.

The following additions have been made since the last list (Journal for May):—

- JOHNSON (Franklin, D.D.) *The New Psychic Studies in their Relation to Christian Thought*.....*New York*, 1887
- DICHAS (Albert-Germain) *Étude de la Mémoire dans ses rapports avec le Sommeil Hypnotique*.....*Bordeaux*, 1886
- ENCÉPHALE L', *Journal des Maladies Mentales et Nerveuses*. Vol. vii. No. 3.*Paris*, 1887
- FONTAN (Dr. J.) et SÉGARD (Dr. Ch.) *Éléments de Médecine Suggestive**Paris*, 1887
- RAMBOSSON (J.) *Phénomènes Nerveux, Intellectuels et Moraux, leur Transmission par Contagion*.....*Paris*, 1883
- LOMBROSO (Professor Cesare) *Studi sull'Ipnotismo**Rome*, 1886
- MORSELLI (Professor Enrico) *Il Magnetismo Animale, la Fascinazione e gli Stati Ipnotici*.....*Turin*, 1886