

## PROCEEDINGS OF THE GENERAL MEETING ON

*Friday, January 18, 1884.*

The sixth General Meeting of the Society was held at 11, Chandos Street, Cavendish Square, on Friday, January 18, 1884.

PROFESSOR HENRY SIDGWICK, PRESIDENT, IN THE CHAIR.

## IV.

## SECOND REPORT OF THE LITERARY COMMITTEE.

*Committee*:—W. F. BARRETT, F.R.S.E.; CHAS. C. MASSEY; REV. W. STANTON MOSES, M.A.; F. PODMORE, M.A.; and EDMUND GURNEY, M.A., and F. W. H. MYERS,\* M.A., *Hon. Secs.*

About a year has now elapsed since the first Report of the Literary Committee was presented to the Society. During that period the Committee has met on nearly 60 days, and its individual members have done much work for it between the meetings. The time seems now to have arrived when we may fitly give some account of the position in which our work at present stands.

First, as to the Society's Library. Many books of value have been presented to the Society, and a lady, who wishes to remain anonymous, has contributed £50 as the beginning of a Library Fund. A Library Sub-Committee has been appointed, and a catalogue of our collection, amounting at present to more than 300 volumes, has been published in Part IV. of our Proceedings.

It has been necessary so far to spend very little of the Society's own funds on books, and the books thus purchased have been almost all of them of a strictly scientific character. Among the books presented to us there is more variety; but on the whole, our collection contains a large proportion of rare and valuable works, which form a good investment, as for the most part they are already rising in price, and the growing interest in our subjects is likely to induce a further increment in their value. It will be seen that we are particularly strong in the older works on Mesmerism and Hypnotism, which contain many important observations, and are now very rare.

Our Library, as we conceive it, is primarily intended for serious

students of psychical topics. We wish to bring it to a point such that no one who is investigating these subjects with any thoroughness may be able to neglect the advantages which it offers. But we desire also to make it conveniently available for the less scientific reader, and we are gradually purchasing duplicates of such books as we find to be most often inquired for.

We shall be happy to receive suggestions as to books (especially foreign books and articles) which might advisably be purchased; and still more glad to receive gifts of such books or donations to the Library Fund.

To pass on to the more specific work of the Committee; the collection and examination of evidence.

A series of meetings of the Literary Committee, occupying the greater part of eight days, was held at the President's house, at Cambridge, August 20th-27th, 1883. The evidence bearing on Phantasms of the Living,—some 400 narratives which had been already printed on slips,—was then considered; and a resolution was passed, (and subsequently approved by the Council,) entrusting to Messrs. Gurney, Myers, and Podmore the composition and publication of a book to be entitled "Phantasms of the Living," to be published with the sanction of the Council, and as one of the publications of the Society.

The proposed scheme includes the apparitions at or about the moment of death, which are one of the most widely attested forms of such phenomena. The persons whose phantoms are discerned at this last hour may still rank among living men. But, even thus, our projected work is intended, as will be seen, to deal with a small part only of the field which we hope in time to traverse. That field is a dark territory, and full of pitfalls, and the safest way of moving across it is not to leap, but to crawl. We wish to make each foot of ground secure as we proceed, and never to lose our line of communication with familiar and established things.

Now our Society claims to have proved the reality of Thought-transference,—of the transmission of thoughts, feelings, and images from one mind to another by no recognised channel of sense. And here we find a starting-point from whence we can begin to consider *all* impressions which seem to be flashed from one human being to another,—*phantasms*, as we call them, in order to include under a term more general than *phantoms* impressions which may be not visual only, but auditory, tactile, or purely mental in character.

While we deal thus with living human beings, we feel that we have behind us a solid and increasing mass of experimental analogies, of which we shall make the fullest use, and which will supply us, as we venture to hope, with at any rate something nearer to a true philosophy of apparitions than it has till now been possible to frame.

Such is the task on which we are engaged ; and of which we hope in the course of a few months to present to the Society some considerable instalment. As for the book itself, we shall aim at its publication in the course of this year ; but the material grows on our hands, fresh problems present themselves, and fresh inquiries are undertaken, till the work which we once hoped to compress within one duodecimo volume seems now likely to extend itself over two volumes octavo.

In the meantime, the greater part of the narratives which form the substratum of the forthcoming book, are offered in printed slips to the inspection of Members and Associates in the Society's rooms. Those only are excepted which have been communicated under conditions which would make it a breach of confidence thus to submit them to view.

The cost of these slips, which will amount altogether to some hundreds of pounds, is defrayed by a member of the Society, who wishes to remain anonymous. Their convenience is manifest. They enable each member of the Committee to study each narrative, and to record his comment or criticism in the readiest way. It must, of course, be understood that the fact of printing or exhibiting these slips does not imply that any conclusion has been arrived at by the Committee as to the precise value of any one of them. They are at present lying open to discussion, and we should be very glad to receive any comments, additional particulars, parallel cases, &c., which any members of our Society who study them may be disposed to write on the slips, or to send us. Most of the slips already printed refer to phantasms of the living, but we are also collecting and printing similar narratives relating to phantasms of the dead, premonitions, and other subjects, for future consideration.

The labour involved in preparing these slips is very great. We calculate that over 10,000 letters have been written during 1883 in the course of the collection and verification of evidence. This may seem an extravagant number, and we trust that in future years the task will in some respects become an easier one. For the difficulty depends in great measure on the *novelty* of the inquiry, and consists in explaining to correspondents what is wanted, and in inducing them to hunt up dates, obtain corroborative signatures, &c., which in many cases they can easily do when once they have been led to perceive the all-importance of accuracy and precision in narratives of this kind. Much time and trouble have been expended on persuading correspondents to allow the publication of their names. We ourselves have, of course, the names of all our correspondents ; but we naturally think it right to place the names before the public whenever we are permitted to do so. Some cases, of course, there are where we should not think of requesting such permission ; cases where some painful or intimate circumstance

connected with the narrative makes anonymity plainly desirable. But it is not in these cases that our difficulties lie, but often in cases where the story is one which our correspondent would be willing enough to tell at any dinner-table. For the fact is, that persons unaccustomed to seeing themselves in print suffer still in many cases from a kind of unreasoning timidity which makes them dread unknown consequences if they allow the public eye to rest on their own name printed in a book, or on the name of their grandfather, or their mother-in-law, or the husband of a former cook. Our correspondents' letters, their very prejudices, are sacred in our eyes, or we could give some amusing instances of this coy reluctance in cases where no human being could derive any annoyance if the whole story were in the newspapers at once.

Much good has been done in this direction by the publication in the *Fortnightly Review* of our first Report, containing a number of signed narratives of apparitions. Not one of these narratives has been shaken, and not one of the writers has complained of any ridicule, notoriety, or other inconvenience as following on the publication of his name and address in this widely circulated periodical. But a wide change in this respect can only be induced by gentle and gradual persuasion;—we must not associate *Psychical Research* in the public mind with brusque interrogation or rash intrusion into the sacred places of memory.

Again, a large part of this correspondence is of a nature which cannot make much show in any book or report. It has consisted in the practice of the somewhat difficult art of courteous and unobtrusive cross-examination;—in endeavours to elucidate points where it seems to us that there is a risk that our correspondents' memory may have been dim or incorrect. We desire, of course, not only that all our narratives shall be the *bond-fide* depositions of trustworthy persons—(of that we think we can make sure)—but also that there shall be as few as possible of those little slips and inaccuracies into which the most trustworthy person sometimes falls, and which if subsequently discovered are represented (sometimes very unfairly) as vitiating the whole account.

And if to any of our correspondents we have seemed needlessly pertinacious, irritatingly pedantic in our constant requests to them to look out contemporary documents, to procure original signatures, to get the corroboration of a second person, which often seems so needless to a narrator, conscious that he is telling the exact truth;—in short, to attend to minutiae on which we sometimes dwell so much that we may perhaps seem to have overlooked the main point of the narrative altogether;—if we have seemed to inflict on our most useful informants in this way trouble which might have been spared, we would ask their pardon, and we would assure them that the result of this minute verifi-

cation is by no means, as a rule, to throw doubt upon the narratives sent us, but much oftener to strengthen them in unexpected ways, and especially to exhibit those *undesigned coincidences* with other accounts of the same kind, which help to show that we have got hold of a plexus of coherent phenomena, and not a mere bundle of isolated and individual fantasies.

It is, however, an unfortunate peculiarity of the human race that they are very unwilling to answer many letters of inquiry on the same subject ; and as we have no means except persuasion of getting any answers from anyone at all, it may be supposed that to bring any given narrative up to the required pitch of accuracy and completeness, may sometimes be a very tedious matter. But the very slowness and tediousness of the inquiry is in itself a safeguard, and correspondents who see how attentively we ponder over every expression which they use, feel themselves thus invited to a careful precision of statement which in many cases is really remarkable. A critic of the rough-and-ready school, indeed, will sometimes say to us, "Get your witnesses together, *put them on oath*, and then I will believe them," but until he offers to arm us with the powers of the law, "to swear witnesses and call for documents," we shall permit ourselves to smile at his somewhat crude conception of the way in which his fellow creatures would be likely to behave when approached as he proposes to approach them.

But although we do not present ourselves to our correspondents with a Testament ready for them to kiss, and an oath to administer, we do think it important to secure *personal interviews* with them (at considerable cost of time and trouble to ourselves, and sometimes, we fear, to them), whenever we can arrange such an interview without its being felt as an intrusion. It has been suggested by one or two critics that this personal acquaintance is valueless in those cases where it has merely been acquired for the purpose of the inquiry. We may reply that a juryman's acquaintance with a witness is generally acquired in the process of the inquiry to which that witness's evidence relates ; but that nevertheless he would be but an indifferent judge who should consider it unimportant whether the jury saw the witnesses or not. The fact, of course, is, that in judging of men and women an interview of an hour or two, though it is not so good as a lifelong intimacy, is incomparably better than nothing. It increases the sense of responsibility and gives the opportunity for much individual detail, which may have been thought too trivial for a formal correspondence.

On the whole, the trouble which we have taken in calling on our correspondents has resulted in great satisfaction to our own minds. We have found them decidedly not lacking in common-sense and accuracy of mind ; by no means ignorant of the possibility of mere morbid

hallucinations ;—and, in short, distinguished from the mass of mankind not at all by being more imaginative or more hysterical, but rather by taking somewhat more of interest than other people in the scientific acceptance of phenomena which they happen to have themselves observed.

We are not to be understood as disparaging persons whose nervous organisations are abnormally sensitive, or as expressing distrust of experiences which differ widely from those of ordinary mankind. We believe that from some *exceptional*, even from some *morbid* organisations, psychical science has much to learn. Some such abnormal natures we are very glad to have encountered ; and for certain experiments it is much to be desired that we should find more “sensitives” of every kind. But we are merely stating a simple matter of fact when we say that the correspondents who have seen the apparitions which we have so far collected, have, for the most part, been persons of a perfectly healthy and normal type. We make it a rule (for instance) to ask each of them whether they have ever experienced anything in the nature of a hallucination besides the special case which they recount, and in hardly any instances have they ever seen or heard anything else of the kind at all.

But this brings us to a point at which we shall have to ask of all our Members to render us all the assistance that they can. For it is not enough, we think, for us to ask our correspondents who tell us of dreams that “come true,” and apparitions that coincided with deaths, how many dreams of the same sort they have had which have *not* “come true,” and how many apparitions they have seen which have coincided with nothing at all. This is necessary, but this is not enough. What we should *like*,—it is of course impossible,—would be to get a regular *census* of death-dreams and hallucinations, so as first of all to know how many people in England had ever had hauntingly-vidid dreams of the deaths of relations, and how many people in England had ever seen a human figure in the room when nobody was there. Then, next, we should like to know how many of these hauntingly-vidid dreams had “come true,” and how many of these so-called hallucinations had been *truth-telling* impressions, that is, had coincided with the death or other sudden shock of the person whose image was seen. For, when speaking precisely, we must give the name of *hallucinations* to *all* such images seen where no one is standing in bodily presence :—though we may distinguish those hallucinations into the *deceptive* or *morbid* hallucinations which are merely a symptom of diseased eyes or brain, and the *truth-telling*, or, as we may call them, *veridical* hallucinations which do, in fact, coincide with some crisis in the life of the person whose image is seen, and which, therefore, as we contend, though they are in one sense hallucinations, yet have some reality be-

hind them—some origin exterior to the eyes and brain of the person who sees them.\*

Well, if we knew how many hallucinations of both kinds were seen by sane persons in England in a year, and how many of those hallucinations were *truth-telling*, then we should be able to calculate in a very definite way whether chance alone would explain all the coincidences,—whether, in fact, it was by mere accident that hallucinations ever told the truth. We cannot, of course, make such a census complete, or such a calculation definite. But, if only enough people will help us, we ought to be able to get at facts enough practically to put the matter beyond controversy. Provisionally, we think ourselves justified in proceeding on the assumption that hallucinations of the kind with which we are mainly concerned—that is, solitary hallucinations occurring only once in a lifetime, and not attributable to any known morbid condition of the percipient's organism—are very rare phenomena: that they do not occur, let us say, to one person in a hundred. This assumption is confirmed by the best estimate of common experience that we were able to make without formal statistical investigation: and it is at least implied in all that has been published on the subject of hallucination by medical writers. Still it is an assumption which may always be disputed, so long as it does not rest on a basis of carefully collected facts. Let us consider how to ask for the statistics required in as precise a manner as possible. First, as regards dreams. Certain dreams are sent to us which have turned out true, and it is argued that the dream, which *coincided* with the event, was in some way *caused* by the event. Well, let us consider what is the kind of dream which has a *primâ facie* look of being something more than mere ordinary nonsense or nightmare?

If I dream that a friend is dead, about whom I am in no way anxious at the time, and if that dream is unusually vivid, and so haunting that it continues to distress me not only while I lie in bed but for an hour or more after I have got up; and if it afterwards turns out that my friend *did* die that night, then there is some *primâ facie* reason for me to say: "I *felt* that this was no *ordinary* dream; and now it is *proved* that it was no ordinary dream, for it has come true." If, however, it can be shown that most other people habitually have dreams just as vivid and haunting as this of mine, which nevertheless do *not* come true at all, then I shall probably have to admit that *my* dream came true by mere chance, and was no proof of any subtle sympathy between my dying friend and myself.

The question about dreams, then, which we want asked of as many people as possible is this: "Can you recall that you have ever, in the course of the last 10 years, had a dream of the death of some person

\* On this subject see the interesting "Thoughts on Apparitions," by the Bishop of Carlisle, in the *Contemporary Review* for January, 1884.

known to you (about whom you were not specially anxious at the time), which you marked as an exceptionally vivid one, and of which the distressing impression lasted for as long as an hour after you rose in the morning?" And similarly as regards hallucinations. In order to find out how common mere morbid hallucinations are we have asked the question: "Have you ever, when in good health, and completely awake, had a vivid impression of seeing, or being touched by, a human being, or of hearing a voice or sound which suggested a human presence, when no one was there?"

The reception which these questions have met with illustrates the dangers of what may be called *over-scrupulosity* in controversy. Of course, we should have been pleased to get as many *Noes* as possible in reply to these questions; for if (as we are inclined to think is the case) only a *very small* percentage of people ever do have vivid death-dreams or morbid hallucinations of any kind, then, of course, our cases of coincidence of dream or apparition with a friend's death are all the more valuable. But we were so anxious not to give any indication that we welcomed the answer *No*, that we worded our questions in such a way that (as it turns out) most people thought that the only answer we cared about was *Yes*. Our critics ridiculed us for eagerly collecting every kind of dream or hallucination that we could lay our hands on, and swallowing them all whole. And our friends did not take the trouble to tell us that they had had *no* hallucinations. They gave as an excuse for not answering our questions that they could only have answered *No*, which was the very answer that would have been most welcome, and they only wrote to us when they *had* some morbid hallucination to tell us of. We are thus in danger of getting results which are of no value as statistics, because the ordinary mass of mankind—who have *not* had hallucinations or death-dreams—think that we do not care for their answers. We must say more plainly, then, that we care for *all* answers,—and for *Noes* fully as much as for *Yeses*,—and we must earnestly ask each of our Members to take the trouble to collect 25 or 50 or 100 answers for us. Let them simply send in the number of *Noes* or *Yeses* on a postcard, as the circular instructs them; and then if there are any cases in any way interesting amongst the number, let them send also on a separate piece of paper an account of such cases, as full as they are willing to make it.

This is a kind of help which we can hardly ask from casual acquaintances—for it does, no doubt, involve some little trouble—but we think that we are justified in urging the members of our own body to take this amount of trouble for the ends which we all have in view. It is an eminently good opportunity for the Society to justify its own existence; for the results, the importance of which we trust we have made clear,



can only be obtained by the active co-operation of a large body of people, and, but for some such association as ours, might remain for ever unsought and unknown. If all our members, or even a quarter of them, would help as actively as some have already done, we should have good hope of getting our questions answered, as we desire, by 50,000 persons.

And, in truth, the need of a census of this kind (we must repeat) is conspicuous from a scientific point of view. The medical authors who have written on hallucinations are necessarily extremely vague as to their *frequency*, and for the most part content themselves with repeating a certain group of striking cases, without much attempt to determine whether these are truly typical in character. No real attempt at collecting statistics has been made, and for our purpose statistics of hallucination are of the highest importance; and the best chance of obtaining these is to make an effort so decided as to attract for the moment at least the attention of large masses of people, and to induce them to answer the easily-answered questions which are here proposed. We hope that something of the *collector's* spirit may be aroused in many minds for the service of the inquiry. The collection of perfectly futile objects for the mere collection's sake, which amused our grandfathers and grandmothers, has sunk now into the diversion of children. But the instinct remains in their elders, and still prompts to many a collection of butterflies or flowers, useless except as teaching accurate discrimination to the collector. We would ask that some of this energy may be transferred to a field where there is still a harvest more ample than the labourers can gather in; namely, the collection of definite psychological facts.

We want, then, the questions in the circular answered as widely as possible. And we want also, as much as ever, well-attested narratives of all kinds bearing on matters within our Society's scope. We find sometimes that someone who could give us a well-attested account of some death-wraith or similar phenomenon, refrains from doing so because, as he says, "You must have quite enough of such accounts at this stage of the inquiry to establish the fact in question." But in reality we can never have too many of such narratives. And we can never have *enough*, so long as a well-attested fact of this kind is still spoken of as "contradictory to experience"; moreover, what we want to do is not to *establish* facts merely, but to *explain* them; and in order to make even the first approach to explanation in a subject so strange and obscure we want the widest possible range of narratives to collate and compare. Each narrative, besides its *evidential* interest as strengthening the credibility of similar narratives, has its *theoretical* interest in enabling us to trace with greater clearness the links of connection which correlate one such account with another; the significant features which

must be the basis of a true classification. To illustrate our meaning by a single point: Every detail as to the clothing in which any apparition is seen is of value, as indicating whether the phantasmal picture may be more probably referred to the percipient's mind, or to the agent's, or to some further source. We do not say that definite laws can be laid down on such a point; but we do say that this is the way in which the laws of such apparitions are to be sought for,—by the collation of every scrap of evidence which bears on some perfectly definite problem, which must necessarily have a solution expressible in terms of our existing knowledge, if only that solution can be reached.

This is merely one instance of the numberless questions which need the collection and collation of great masses of evidence before any really permanent or scientific treatment of them is possible.

And those who aid us may feel that they are lending a hand to no slight or trivial undertaking. The very violence of the attacks which a certain school of thinkers have made on our work may be fairly taken as unconscious evidence of the importance and novelty of the results towards which our researches point. The school to which we refer is that which insists, in tones louder sometimes and more combative than the passionless air of Science is willing to echo or convey, that all inquiries into man's psychical nature, all inquiries which regard him as possibly more than a portion of organised matter, are no longer open, but closed, and closed against his aspirations for ever. For ourselves, we are far indeed from retaliating either the contempt or the animosity which this able group of thinkers often express for us. Many of us have thought at one time as they think now; and have learnt to honour the courage which can face the worst, and which scorns to feed with vain hopes the creatures of a day. If we disagree with this school—as we do fundamentally—it is not that we dispute their ability or their patience in dealing with the facts on which they have concentrated their attention. It is simply that we have taken pains to collect and master an immense mass of evidence, which they have not yet included within the scope of their inquiries.

They cannot complain of us for dogmatising, for we do not dogmatise. They cannot complain of us for invoking sentiment in aid of argument; for we have refrained, with a persistency which has perplexed and disappointed some of our supporters, from anything which could possibly be construed into an *ad captandum* appeal to the friends of religion. All that can be complained of is that we are ready to take facts as they come. But facts are facts, whatever view of the Universe they make for. It is no blame to the materialist if the Universe is a soulless interaction of atoms, and life a paltry misery, closed in the grave. But neither is it any blame to *us* if the Universe is the field of spiritual energies, and if “highcapacious powers” do in truth “lie folded up in man.”

## APPENDIX.

We give here one of our cases where the percipient is one who has never experienced any other hallucination of the senses, and where the coincidence of time is very close. It is from the Rev. Robert Bee, 1A, Colin Street, Wigan (curate of St. George's, Wigan):—

On December 18th, 1873, I left my house in Lincolnshire to visit my wife's parents, then and now residing in Lord Street, Southport. Both my parents were, to all appearance, in good health when I started. The next day after my arrival was spent in leisurely observation of the manifold attractions of this fashionable seaside resort. I spent the evening in company with my wife in the bay-windowed drawing-room upstairs, which fronts the main street of the town. I proposed a game at chess, and we got out the board and began to play. Perhaps half-an-hour had been thus occupied by us, during which I had made several foolish mistakes. A deep melancholy was oppressing me. At length I remarked: "It is no use my trying to play; I cannot for the life think about what I am doing. Shall we shut it up and resume our talk? I feel literally wretched."

"Just as you like," said my wife, and the board was at once put aside.

This was about half-past 7 o'clock; and after a few minutes' desultory conversation, my wife suddenly remarked: "I feel very dull to-night. I think I will go downstairs to mamma for a few minutes."

Soon after my wife's departure, I rose from my chair, and walked in the direction of the drawing-room door. Here I paused for a moment, and then passed out to the landing of the stairs.

It was then exactly 10 minutes to 8 o'clock. I stood for a moment upon the landing, and a lady, dressed as if she were going on a business errand, came out, apparently, from an adjoining bedroom, and passed close by me. I did not distinctly see her features, nor do I remember what it was that I said to her.

The form passed down the narrow, winding stairs, and at the same instant my wife came up again, so that she must have passed close to the stranger—in fact, to all appearance, brushed against her.

I exclaimed, almost immediately, "Who is the lady, Polly, that you passed just now, coming up?"

Never can I forget, or account for my wife's answer. "I passed nobody," she said.

"Nonsense," I replied. "You met a lady just now, dressed for a walk. She came out of the little bedroom. I spoke to her. She must be a visitor staying with your mother. She has gone out, no doubt, at the front door."

"It is impossible," said my wife. "There is not any company in the house. They all left nearly a week ago. There is no one, in fact, at all indoors but ourselves and mamma."

"Strange," I said; "I am certain that I saw and spoke to a lady, just before you came upstairs, and I saw her distinctly pass you, so that it seems incredible that you did not perceive her."

My wife positively asserted that the thing was impossible. We went downstairs together, and I related the story to my wife's mother, who was busy with her household duties. She confirmed her daughter's previous statement; there was no one in the house but ourselves.

The next morning, early, a telegram reached me from Lincolnshire; it was from my elder sister, and announced the afflicting intelligence that "our dear mother had passed suddenly away the night before; and that we (*i.e.*, myself and wife) were to return home to Gainsborough by the next train."

I could scarcely realise the fact for a moment, and I forbear to attempt any description of my subsequent feelings. I have the consolation of knowing that my mother was prepared for her end, a satisfaction to us all in our bereavement. The doctor said it was heart disease, which in a few minutes had caused her death.

After giving some details of his arrival at home, and of the kindness of friends, Mr. Bee continues :—

When all was over, and Christmas Day had arrived, I ventured to ask my brother the exact moment of our mother's death.

"Well, father was out," he said, "at the school-room, and I did not see her alive. Julia was just in time to see her breathe her last. It was, as nearly as I can recollect, 10 minutes to 8 o'clock."

I looked to my wife for a moment, and then said : "Then I saw her in Southport, and can now account, unaccountably, for my impressions."

Before the said 19th of December I was utterly careless of these things ; I had given little or no attention to spiritual apparitions or impressions. I was utterly careless of these things.

In answer to inquiries Mr. Bee adds :—

My mother died in her dress and boots ; she was taken ill in the street, and had to be taken to a neighbour's house in Gainsborough a few paces from her own house. The figure resembled my mother exactly as to size, dress, and appearance but it did not recall her to my mind at the time. The light was not so dim that if my mother had actually passed me in flesh and blood, I should not have recognised her.

In answer to the question whether this is the only case of hallucination that he has experienced, Mr. Bee answers "Yes."

He further adds :—

The gas light over the head of the stairway shone within a frosted globe and was probably not turned on *fully*.

The fact is, that there was ample light to see the figure in, but just as the face might have been turned to me, or was turned to me, I could not, or did not, clearly discern it. Many, many times, my regret and disappointment when I recall this fact have been deeply felt.

Mrs. Bee writes to us as follows :—

If anything I can say to you will be of any use, I will willingly give my testimony to all my husband has said. I remember perfectly 10 years ago my visit to my mother's, and my husband's unaccountable restlessness on the particular evening mentioned, also Mr. Bee asking me, after I had been downstairs, if I had met a lady on the stairs. I said "No, I do not think there is any one in the house but us." Mr. Bee then said, "Well, a lady has passed me just now on the landing ; she came out of the small bedroom and went downstairs ; she was dressed in a black bonnet and shawl." I said, "Nonsense, you must be mistaken." He said, "I am certain I am not, and I can assure you I feel very queer." I then went to ask mamma if there was anyone in the house, and she said no, only ourselves ; still Mr. Bee insisted someone had passed him on the landing, although we tried to reason him out of it.

In the morning, while we were in bed, we received a telegram stating that Mrs. Bee had died suddenly the night before. I said at once, "Robert, that was your mother you saw last night." He said it was. When we got to Gainsborough we asked what time she died ; we were told about 10 minutes to 8, which was the exact time ; also that she was taken suddenly ill in the street (wearing at the time a black bonnet and shawl) and died in 10 minutes.—MARY ANN BEE.

Now when the apparition is, like Mr. Bee's, a quite unique experience in the percipient's life, and where its coincidence with a death is as close as this, the improbability of a mere chance coincidence is so enormous that it is hard to express it with anything like accuracy in actual figures.

We will, however, roughly show the lines on which such a problem would need to be worked.

Let us say that the percipient has had only 30 years of intelligent life, and that during those 30 years he has slept for nine hours a day. He has then had  $15 \times 30 \times 365 = 164,250$  hours of waking intelligent life. Now, in this space of time he has had *one* apparition, which resembled his mother, and was within a few minutes of her death. Now if we say only that it was within the quarter of an hour in which death occurred, there are  $164,250 \text{ hours} = 657,000$  quarter-hours of life in which that apparition might have come; so that the chance of its coming in the right quarter-hour was  $\frac{1}{657,000}$  (keeping to round numbers). But this is on the supposition that the only apparition which could possibly appear to him would be his mother's, or at least one which resembled his mother in general aspect as nearly as the phantom which he actually saw.

Now visual hallucinations known to be morbid are by no means limited to the human shape; they are grotesquely varied in ways too numerous to mention. But, for the sake of argument, we are willing to assume (it is a monstrous assumption) that *one-tenth* of all phantoms accidentally caused are likely to resemble a lady in a black bonnet and shawl. The chance, then, that an apparition of the right class (the old lady class) would show itself in the right quarter-hour (the quarter-hour of actual decease) will be  $\frac{1}{6,570,000}$   $\times \frac{1}{657,000 \times 10}$ ; or the chances *against* this happening to any given person will be over  $6\frac{1}{2}$  millions to one. That is to say, in order to explain satisfactorily Mr. Bee's experience on the supposition of accidental coincidence, it ought to be shown that there are something like  $6\frac{1}{2}$  million adult Englishmen now living who have had one single visual hallucination of a very distinct sort in the course of their lives.

Even if Mr. Bee's case stood alone, this result would be somewhat surprising. Of course it does not stand alone; and if we take from our store, say only nine more cases of equally close coincidence attending an apparition seen by an English adult now living, and not otherwise subject to hallucinations,—say, for instance, the cases on slips 55, 92, 229, 352, 382, 602, 627, 628, 685,—we increase the improbability tenfold, and require that 65,000,000 English adults now living should have had a single distinct visual hallucination, to make it probable that in these ten cases the coincidence would have accidentally occurred as it did.

## V.

## NOTE ON THE EXISTENCE OF A "MAGNETIC SENSE."

BY PROFESSOR W. F. BARRETT.

The experience of those who have worked with large magnets and of men of science generally, is entirely opposed to the notion that magnetism can be felt or can exert any good or ill effect on the human body or other organism. Nevertheless, the statements of Reichenbach, Gregory, Ashburner, and others seem to indicate that upon certain sensitive "subjects" a magnet can produce a distinct and powerful influence. The investigation of this question properly belongs to the "Reichenbach" Committee, and has been referred to in their first report; the object of the present note is merely to detail a few independent experiments which I have recently made, whilst Mr. G. A. Smith was in Dublin.

When a powerful electro-magnet was employed, Mr. Smith stated that he distinctly felt an unpleasant sensation when he approached the magnetic poles. The effect seemed to be most powerful when his temple was nearly touching one pole; except on one day, when suffering from facial neuralgia, and then he appeared much more sensitive in the face than in the forehead. The peculiar and unpleasant sensation produced by the magnet he describes as slowly rising to a maximum in 15 or 20 seconds after the current has been sent round the coils of the electro-magnet. In like manner the effect seemed to die down slowly after the contact was broken. Unknown to Mr. Smith, the circuit was closed and opened several times, and the magnetism correspondingly evolved or dissipated, the result being that there was a fairly accurate correspondence between the physical and the psychical effect. The faint molecular crepitation which accompanies the magnetisation of iron, and can be heard when the ear is very near the magnet, is, however, very apt to mislead the imagination. To avoid this, Mr. Smith was placed at a distance where this faint sound could not be heard, and he was then requested to walk up to the electro-magnet, and, judging only from his sensations, to state if the current were "on" or "off." The experiment was made 12 times successively, and he was correct in 10 out of the 12 trials. Assuming the fact of Thought-transference, the imagination might, it is true, have been misled; but as I did not find Mr. Smith able at other times to respond to my unexpressed will, it is hardly probable that my knowledge as to when the current was on or off would suffice to explain his

success. Of course it would be possible for a trickster, using a concealed compass-needle, to be able to impose on a careless experimenter; but care was taken, and I have not the least reason to doubt the *bona fides* of Mr. Smith. Obviously the foregoing observation, which I have thought it better to put on record, is of little value unless corroborated by a far more extensive series of experiments, conducted with the most stringent precautions to avoid the creation of illusory effects.

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APPENDIX, added March 12th.

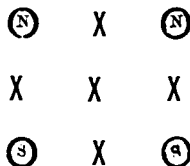
Sir William Thomson has published in *Nature* for March 6th, 1884, the report of an address on the "Six Gateways of Knowledge," delivered by him in Birmingham. In this address Sir William remarks, "If there is not a distinct magnetic sense, I say it is a very great wonder that there is not," and he then refers to an experiment made to test this point in the following words:—

"This experiment was carried out in a most powerful manner by Lord Lindsay (now Lord Crawford), assisted by Mr. Cromwell F. Varley. Both of those eminent men desired to investigate the phenomena of mesmerism, which had been called animal magnetism; and they very earnestly set to work to make a real physical experiment. They asked themselves, Is it conceivable that, if a piece of copper can scarcely move through the air between the poles of an electro-magnet, a human being or other living creature placed there would experience no effect? Lord Lindsay got an enormous electro-magnet made, so large that the head of any person wishing to try the experiment could get well between the poles, in a region of excessively powerful magnetic force. What was the result of the experiment? If I were to say *nothing!* I should do it scant justice. The result was marvellous, and the marvel is that nothing was perceived. Your head, in a space through which a piece of copper falls as if through mud, perceives nothing. I say this is a very great wonder; but I do not admit, I do not feel, that the investigation of the subject is completed. I cannot think that the quality of matter in space which produces such a prodigious effect upon a piece of metal can be absolutely without any—it is certainly not without any—effect whatever on the matter of a living body; and that it can be absolutely without any perceptible effect whatever on the matter of a living body placed there seems to me not proved even yet, although nothing has been found. It is so marvellous that there should be no effect at all, that I do believe and feel that the experiment is worth repeating; and that it is worth examining, whether or not an exceedingly powerful magnetic force has any perceptible effect upon a living vegetable or animal body. I spoke then of a seventh sense. I think it just possible that there may be a magnetic sense. I think it possible that an exceedingly powerful magnetic effect may produce a sensation that we cannot compare with heat or force or any other sensation."\*

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\* Upon this point a medical authority, Dr. W. H. Stone, published the following remarks four years ago:—"It appears that a similar question was long ago asked of Faraday, when he was engaged on his diamagnetic discoveries by means of the

For some time prior to Sir William's lecture experiments have been in progress in my laboratory, the object of which is to ascertain if any special and perceptible effect is produced upon any living organism by a powerful magnetic field. Large helices have been made to encircle the head and limbs, and through them a strong electric current can be sent; so far nothing has been felt in my own case from the intense field of magnetic force thus generated. I have also had the opportunity of trying the effect of the enormously powerful magnetism generated in one of the larger sized "Brush" dynamo-electric machines. The machine was dismantled, its revolving armature being removed, and the massive pole pieces attached to the field magnets of the machine adjusted so that they clasped the head on either side. A current of great strength, generated by a large Siemens dynamo, was then sent through the magnets, but neither in my own case, nor in several others who tried was anything felt that could be attributed to a "magnetic sense." The circles indicate the poles of the pair of magnets and the crosses the successive positions in which the head was placed.



large magnet belonging to the Royal Institution. He stated that he had never been able to trace the slightest physiological effect, whether sensorial, motor, or hypnotic, on himself when using the strongest magnetic fields then attainable. It is true that the discovery of dynamo-magnetic machines has placed at our disposal currents of vastly greater electro-motive force and quantity than were then available. With these the subject should, if at all, be patiently investigated. One or two rather trivial facts, such as the inability of many persons of nervous temperament, of whom the writer is one, to sleep soundly in the north and south position, and the singular vitality of magnetic treatment from very early times down to the present, are hardly strong enough to countervail the negative evidence here adduced. Still the subject, not being in direct contravention of any known scientific facts, deserves further investigation in a fair and dispassionate spirit. It seems *primâ facie* improbable that so important a factor in cosmogony should be otiose and inefficacious in its highest development—human life." Baron Reichenbach, as is well known, made numerous experiments on the effect of the position of the bed on persons of sensitive temperament. He found that invariably the most pleasant position and the most refreshing sleep was obtained when the head was to the north and the feet to the south. These results were obtained with persons he terms "sick sensitives," but a much larger number of carefully conducted observations on these and others are needful before any conclusions of the least weight can safely be drawn, as it is most important, but most difficult, to exclude the effect of the imagination.



But whilst the results were negative in this case, and doubtless would generally be found so, yet Charcot has shown that in certain cases of hysterical patients a transference of sensation from one side of the body to the other is produced by magnetism. An apparent confirmation of these results was obtained by Dr. Stone in the case of a patient of his at St. Thomas' Hospital. The case is recorded in the Reports of St. Thomas' Hospital, Vol. X., 1880, and the following is the part relating to the specific effect of magnetism:—

JULY 1ST.—Dr. Stone, thinking it probable that repeated examination by this time might have put the patient on her guard, and roused an inclination to simulate, described minutely the effects obtained by M. Charcot with magnets in her presence, intimating his expectation that the anæsthesia would be transferred. This was the first hint she had received, as all clinical observations had hitherto been made out of the ward, and the students had been enjoined strict silence in her presence. A magnet of eight large steel plates was placed in the bed beside the sensitive leg, and there left. It was, however, *non-magnetised*, and really only an inert piece of steel. On the following morning she answered clearly and with great simplicity "that it had done nothing at all; she did not know it was there." It was then taken away on some pretext, strongly magnetised by means of a powerful electro-magnet, and returned to the ward apparently unchanged on the 3rd.

7TH.—On its being similarly applied, after magnetisation, the anæsthesia disappeared from the affected leg, but no alteration occurred in the other.

The chief points of interest of the case are, first, that the age, character, and simplicity of the patient precluded any suspicion of imposture; secondly, that singular precautions were from the first adopted to preclude the possibility of unconscious deception from the effect of "expectant attention;" thirdly, the marked and unequivocal effect of magnetism.

This case offers the most trustworthy evidence which has hitherto come under my personal observation of the influence of the magnetic field on ordinary sensation. It is materially strengthened, moreover, by an interesting communication by Dr. Julius Dreschfeld, in the *British Medical Journal* of August 14th, in which he shows the efficacy of the same great physical power in non-hysterical cases. Another valuable contribution to the physiology of the subject is contained in the *Journal of Anatomy and Physiology* for January, 1879, from the pen of Professor M'Kendrick, in which he demonstrates the fact that "a portion of nerve stretched between the poles of an electro-magnet, so as to touch each, will not excite contractions in a muscle when touched by a copper wire during the passage of the current through the wire of the electro-magnet."

It is to be regretted that neither of these last writers affords any quantitative clue to the intensity of the magnetic field employed. Judging from the proved phenomena of diamagnetism and those of the action on light, the field should be of considerable strength, but whether it acts most when diffused or when concentrated requires further investigation. The unilaterality of nervous functional conditions certainly points to the hypothesis already named of equilibrium more or less stable between the two halves

of the nervous system, like that of a duplexed cable. This is also confirmed in a physical point of view by the slow oscillations of transferred sensibility, which closely resemble those of a metronome pendulum, in which the inert mass is large and the unequilibrated moving force proportionately small. A far less magnetic movement would also be competent to disturb such a system than one in more stable surroundings.

For farther experiments I have constructed a large electro-magnet wound with a helix of iron instead of copper wire. It develops a singularly spacious and regular series of curves even with a battery of moderate intensity. I propose also to excite it by means of a fine gramme machine which has recently come into my possession. Whether the results ultimately prove positive or negative, it is clear that a sufficient *prima facie* case is made out for further investigation.\*

Although the magnet did appear to exert "a marked and unequivocal effect" on Dr. Stone's hystero-epileptic patient, yet it is obvious that a larger number of experiments, conducted with stringent precautions, are necessary before any general induction can safely be drawn.

\* Dr. Stone informs me that the trials he made with this magnet upon himself and other healthy individuals yielded no evidence of any sensory effect peculiar to magnetism. It is chiefly with this electro-magnet, through Dr. Stone's kindness, that the experiments of the Reichenbach Committee have been made.

## VI

## THE STAGES OF HYPNOTISM.

BY EDMUND GURNEY.

One of the first things which strikes the student of what is often loosely called the "hypnotic" or "mesmeric" state is this—that even the simplest manifestations of hypnotism are wont to present not *one* state but *two*, distinguished from one another by very marked characteristics. But the distinction, as usually drawn, has been of a very rough kind. As a rule, it has been noticed that good "subjects" first get into a state in which many of them show great acuteness of sensibility, and in which all of them can be made to do or imagine very odd things; and that from this they gradually merge into a state of profound sleep or even coma. Such were the stages as originally observed by Braid, and subsequent investigation has done little to define them further. I am going to point out what appear to me grave deficiencies in this distinction of states, and to attempt to draw a much more precise one. But, before doing so, it is necessary to refer to another mode of distinction, which, if not cleared out of the way, might greatly confuse us. In discussions on hypnotism we continually find three *types* of condition recognised—the *cataleptic*, the *lethargic*, and the *somnambolic*: the cataleptic being the condition where the limbs will remain in any position in which they may be placed, without effort on the "subject's" part; the lethargic being the condition where the muscles are relaxed, but abnormally liable to contractions and spasms under gentle stimulation; the somnambolic being the condition in which the "subject" exhibits the singular eccentricities of conduct associated with public entertainments. But though these are all real and important conditions, they do not the least represent distinct *states of the individual*, or distinct *stages of hypnotism*. For the peculiarities of muscular condition may quite well *co-exist* with that peculiar mental state which is described as somnambolic. The cataleptic state, moreover, does not belong to normal hypnotism at all, but is a decidedly exceptional phenomenon, and can only be considered otherwise by confusing it with the mere ordinary *rigidity* of the limbs; and again the effects of muscular irritability may be locally produced, by hypnotic processes, in various parts of the body, while the "subject" remains in his normal waking state. On the whole, then, this mixing up of physical and mental and of constant and occasional characteristics, in a list which professes to sum up the *fundamental forms of hypnotism*, seems extremely misleading; and in what

follows I shall speak only of cases where the *mind* of the "subject" is to some extent affected, and shall base my distinctions primarily on the constant features which that mental affection displays.

If, then, the "subject"—the conscious individual and not merely a part of his body—has succumbed to the hypnotic influence, if he has passed out of his normal waking state and crossed the threshold of trance, then, before he reaches the profound sleep in which his mental condition is a mere blank, there lie before him two and only two markedly distinct states or stages, each of which however may present within itself a very large amount of variety. We may conveniently designate them as the *alert* state or stage and the *deep* state or stage. These states are, I believe, produced or producible in the case of every "subject" who is sufficiently susceptible to hypnotism to be able finally to be put to sleep by it; but the question will very naturally occur how, if that is so, there can ever have been a doubt about it. How is it that the character, and even the very existence, of the two states has escaped general recognition? The answer is broadly this,—that, in the first place, each state admits of many degrees, and the characteristics of either of them may be only very slightly or only very transiently presented; and in the second place, unless special means are adopted, it is very easy to mistake the alert state for normal waking, and the deep state for sleep. This will become clearer when the states themselves have been further described.

To begin, then, with the *alert* state. This is the state in which a "subject" is when, after the usual preliminary period of gazing fixedly at some object held near the eye, or of having passes made over the upper part of his person, and after the usual involuntary closure of his eyes, the strain on his eyelids is released by a few touches and words, and he is restored to what may look quite like his natural waking condition. Sometimes, it is true, the difference is very marked, and he will sit with a vacant air, irresponsive to every voice except that of the operator, and clearly not in possession of his ordinary faculties. He may be made to perform imitative actions and to obey commands in a mechanical way; but his consciousness may be at a very low ebb, or (as some have held) may have lapsed altogether. But even these cases will exhibit two characteristics of the alert stage which are also characteristics of normal waking:—the "subject's" eyes will be open and capable of seeing; and he will (almost invariably) prove sensitive to pain if he be pinched or pricked. Very often, however, the resemblance to normal wakefulness is far closer than this; for the "subject" will be found to converse with perfect comprehension, memory, and even humour. Where then does the essential distinction of his state lie?

The main point which can be observed at the moment is that, though perfectly capable of sustaining a conversation, he does not *originate*

remarks. If not spoken to he will sit quiet, and, if simply asked what he is thinking about, he will almost always answer "Nothing." Perhaps it may be said that even this condition of passive vacancy is after all not so very different from that in which a large number of our fellow-creatures spend a large portion of their waking hours. If this be allowed, then we shall have to seek the essential difference of the hypnotic condition, not in any feature which it immediately presents, but wholly in two *possibilities* attaching to it, either of which demands appropriate treatment to become a reality. In the first place, if the "subject" be left completely to himself, he will rapidly sink into the *deeper state*, and thence into hypnotic sleep, in either of which he will prove insensitive to any moderate amount of torture. Clearly the condition which leads rapidly and naturally on to further conditions of this sort is not a *normal* one: it can never be said of a person in a *normal* state, however sleepy he may be, that in two or three minutes he will be in a condition when pins may be run into him, or the severest pinches applied, without awaking him or evoking any signs of distress. The passage into these deeper conditions, it should be observed, is often so rapid that the fact of their being reached *through the alert stage* may be wholly unnoticed. The hypnotising process may carry a sensitive "subject" in a minute or less from a condition of normal waking into hypnotic sleep; and in such a case the "alert" period has been represented only by the few seconds before his eyes closed. If he had been taken in hand during those few seconds, and had been talked to or kept employed, this passage into the deeper state would have been prevented; but if he is allowed to follow the natural course without interference, he will simply be seen to go to sleep, and he must be awakened by the operator before any phenomena can be exhibited. This *liability to lapse*, then, is one distinguishing characteristic of the alert state.

It is characterised, in the second place, by the possibility of obtaining, while it lasts, certain special phenomena of an active sort. The "subject" can be made to do, and to continue doing, any action which the operator commands, although he may be perfectly conscious of making a fool of himself, and may strongly desire to resist the command. He can also be put under the influence of delusions—can have his senses deceived, so that he mistakes salt for sugar, ammonia for eau de Cologne; or can even be made to believe that he is in some distant place, or that his identity is changed.\* These are the common platform-phenomena;

\* It should be remarked that even here the necessity remains of stimulating the "subject" from time to time, to keep him going. When he is under a delusion, he will sometimes give long connected accounts, in great part often fictitious, as if he were following the course of a dream; but though very slight questions and comments will be enough to make him proceed, he will not do so if left to sustain a complete monologue.

and as the very object of hypnotising a "subject" at all is usually to procure some of them, and the possibility of procuring them is thus practically certain to be tested, it might seem that the recognition of the abnormality of his state was in this case, at any rate, assured. But though this may be so during the *course* of the manifestations, it is not unfrequently otherwise at their *close*; and a "subject" who has been allowed, perhaps, to lapse into sleep, while others are being operated on, is often roused and even sent away, without its being observed that he has been roused not into a state of normal wakefulness, but only into the alert stage of hypnotism. To all appearance he is quite himself; and, the performance being over, it does not occur to anyone to try whether he is still at the mercy of commands and delusions; while the general stir and commotion prevent him from lapsing again into sleep. So he goes off home, acting and answering in a quite natural manner, till the effect wears gradually off; or, as more often happens, he continues to feel drowsy and headachy, goes to bed, and wakes up in his usual condition next morning.

Passing now to the *deep* stage, we find that this in turn is liable to be confounded with a contiguous condition, namely, the genuine hypnotic sleep into which it tends to merge. It resembles that condition in the fact that the eyelids are closed; that, if one of them be forcibly raised, the eyeball is found to be rolled upwards; in the general insensibility to pain and to ordinary modes of stimulation. And there exists here precisely the same chance as we noted in the former case, that the particular stage will escape detection. If the "subject" be left to himself, he will have no opportunity to manifest its characteristics, but, passing rapidly through the period during which these might be evoked, will soon lose consciousness and individuality in profound slumber. With some "subjects," moreover, the invasion of mental torpor is so rapid that it might be hard to fix and retain them in the genuine deep stage, even if the proper means were adopted. But many others, if taken in time, after their eyes are closed and they have become insensible to pain, but before sleep has intervened, will prove quite capable of rational conversation; they are mentally awake, even when their bodies are almost past movement, and when even a simple command is obeyed in the most languid and imperfect manner. The state is, however, harder to sustain at an even level than the alert one, owing to a stronger and more continuous tendency to lapse into a deeper condition. In the alert state the "subject" can usually be kept going for an indefinite time: in the deep state he usually shows an increasing dislike to being questioned or meddled with.

Enough, perhaps, has been said to show how the two stages of hypnotism may be distinguished from normal waking on the one hand and from blank slumber on the other. But the marks which have been

so far given as distinguishing the two stages *from one another* are by no means equally constant and precise. The closure of the eyes, the insensibility to pain, the disinclination, amounting sometimes almost to inability, to move, are all in a general way characteristic of the deep stage; and to them may be added a diminution of the irritability of the conjunctiva and of the susceptibility of the pupil to light, with irresponsiveness to any voice but that of the operator.\* But of these characteristics the only one which is invariable is the bodily torpidity. Closure of the eyes is nearly invariable; but I myself have seen two well-marked instances where the eyes remained wide open throughout the period which, as judged by all other indications, was certainly the deep stage. The irritability of the eye is sometimes only slightly affected. Insensibility to pain, though usual, is liable to still more frequent exceptions, as also is the irresponsiveness to the address of persons other than the operator. Nor are these normal features of the deep stage, even when present, entirely distinctive; as more than one of them are often also present in the alert. The irritability of the eye may suffer marked diminution at the very beginning of the hypnotising process.† Again, not only is insensibility of a *local* sort in the alert stage one of the commonest phenomena of public exhibitions, when an arm or a leg is stiffened, and submitted to the audience to stick pins into or otherwise maltreat; but even *general* insensibility may supervene while the "subject" is still open-eyed and capable of actively responding to suggestions and commands. This is especially the case if his attention is strongly directed in some particular channel; for instance, a "subject" who normally is sensitive in the alert stage, if while in the deep stage he be ordered to do some particular thing when he wakes, and be then roused into the alert stage, will often prove insensitive *till the thing is done*. Similarly I have known a "subject," who was quite sensitive before, become insensitive after, the communication of some strong impression. Being told that his sweetheart was drowned, he expressed the resolution not to survive her; and, while in this heroic mood, he bore the severest and most continuous

\* How little even the most elementary distinctions between the two states have been realised by high authorities, a couple of specimens from Heidenhain may show. "The pupil of a hypnotised person contracts energetically when light falls upon the eye." It is impossible that this sweeping assertion could have been made, had Heidenhain examined eyes in any but the alert state. "Hypnotised persons never fall down." This statement, it is true, he qualifies in a note by saying that he has seen *one* person fall down. The fact, of course, is that in an enormous majority of cases (I myself have never seen an exception) the "subject," if standing, falls down a very few seconds after closing his eyes and lapsing into the deep state.

† See (in addition to the observations of Braid, Tamburini, and Seppelli), the case recorded by Mr. Stanley Hall in *Mind*, XXX., p. 177.

pinching without a sign of sensation. In such cases the nervous energy is all, so to speak, concentrated into a single channel, in the performance of the task or the contemplation of the catastrophe, and there is none left to feel with—just as it has happened to soldiers in the excitement of battle to be temporarily unconscious of wounds. Insensibility to pain may also be present in the alert state, if the “subject” is put under the influence of such a delusion as would naturally involve it, *e.g.*, if he is made to believe he is a statue; and I have known insensibility, which originated in this way, *continue* for some little time after the delusion was withdrawn, and another substituted—the phenomenon being the more remarkable in that the “subject,” who had been receiving the most savage nipping with total indifference, actually shed tears at the memory of a *fictitious* nipping, which he was told had been administered to him on the previous day. Closure of the eyes, again, may be present in a state which in respect of every other symptom is the alert one: indeed, unless the spasm which brings down the eyelid during hypnotisation be relieved by some distinct local process, the eye might naturally remain closed as much during the alert stage as during the deep. Nor can we even account the bodily activity which may be evoked during the alert stage, in contrast with the torpidity of the deep one, as a really sharp or essential distinction. The activity, it will be remembered, always has to be evoked *ab extra*, and often much against the “subject’s” inclination. If left to himself, he will sit as passive in the former state as in the latter, and the one will insensibly merge into the other. The difference here, then, might seem to be one rather of degree than of kind; and the two states to be merely the less and the more advanced stages on the path to complete torpor. I may add that the advance of *mental* decline, where it can be marked, seems to be of the same graduated kind. Thus a “subject” who, first in the alert and then in the deep state, is assured that he is going to be hanged next morning, will succumb to the idea with about equal readiness in both cases; and the only observable difference will be that in the deep state he does not seem equally to realise its gravity. A “subject,” for instance, with whom I tried this particular experiment and who was rendered decidedly grave when in the alert state, was chiefly occupied, in the deep state, with the half-jocular invention of dodges to avoid pain. But, where the whole mental condition is so abnormal it is hard to regard such slight differences in the power of judgement as important or distinctive, or to regard them as other than steps in a single process of mental unhingeing.

The question then presents itself: Is there any distinction of *kind* between the two states—any single test by which we can make sure in which of them the “subject” is—any sort of phenomena, capable of constant reproduction, which will draw a clear line between them, and



not merely represent a gradual and continuous decline of hypnotic waking into hypnotic sleep? I believe that there *is* such a distinction; and that the phenomena needed to establish it are to be found in the domain of *memory*. And as memory will afford the means not only for distinguishing the one stage of hypnotism from the other, but also further for distinguishing the "hypnotic state" as a whole from the normal one, I may attempt to make my rapid sketch embrace all its various conditions, so far as I have been able to observe them.

First, then, as to the "subject's" memory, when completely awakened from the alert state, of what has taken place during that state. The degree of it varies with the number of times that he has been under the hypnotic influence.

(1) A "subject" who is quite fresh to hypnotism has frequently some remembrance, on waking, of *all* that he has gone through. Of such actions as are usually exhibited on platforms—imitative movements, sneezing, laughing, jumping and the like—his remembrance is distinct; and he perfectly recalls not only the actions but the feelings of passive acquiescence, or of surprise, or of repugnance, with which he performed them. A not uncommon description is that he felt as if he had *two selves*, one of which was looking on at the involuntary performances of the other, without thinking it worth while to interfere. He also perfectly remembers such simple mental operations as the effort to recall his name. Of performances which have involved more complex *mental ideas*, and where his mind has been at the mercy of some concrete form of delusion, his remembrance is dimmer. But still he will give some account of parts played by him in imaginary scenes, or even when under the impression that he was some one or something other than himself. It is probable that the delusive impression in such cases has not been quite complete. For instance, a "subject" who, when awakened to his normal state, remembered the fact of having been put to flight by a white ghost, described himself as having in a sort of way known that it was only a handkerchief which the operator was flourishing, and yet as unable to resist the ghostly terror.

(2) After a very short course of hypnotisation, these illusory changes of scene or of identity, and even the simple mental operation of trying to recall some familiar fact, are found to have left no trace on waking; but the "subject" can still perfectly recollect the imitative and other actions which he has performed *in propria persona*, and the sort of feelings which accompanied them. It will be observed how curiously fatal this fact is to Professor Heidenhain's theory that the actions of imitation, and of what he calls automatic "obedience," which a hypnotised person performs, are purely reflex and unconscious. For it is just of these actions that the clearest remembrance is retained; and in fact, if the "subject" has been made to perform a few of them after being

thrown into a light trance-condition, and is then brought back to the normal state, there has been absolutely no breach whatever in the continuous stream of his consciousness. He has passed through a strange experience, and that is all.

The memory of the events of the alert hypnotic state finds its precise parallel in certain cases of natural somnambulism—a condition in which all actions are of course performed *in propria persona*, and without any externally-induced illusion. Somnambulism is, as a rule, a decidedly deeper state than the lighter stage of hypnotism; and memory, on waking, of what has occurred in it is exceptional. I have, however, lately met with well-marked cases of it in two of my own acquaintance, who gave descriptions of their somnambulatory experiences very similar to those given by hypnotic “subjects.” Though exceptional such cases are probably not absolutely rare; and it is the more curious that Despine, Heidenhain, and others should have so hastily and sweepingly assumed that the function of true psychic memory is suspended in hypnotism.

(3) In my experience no true memory has ever been exhibited, on complete waking, of things which have been done or suffered in the *deep* state. Nor am I aware of any published record of such an event.\* The performance, at the appointed time, of *commands* impressed on the “subject” when in the deep state hours or even days before, is not a case in point. For though he feels impelled to do what he has been told to do, he has no recollection of the fact of having been so told, and is at a loss to comprehend his own impulse. This is merely an instance of the well-known phenomenon of cerebral (or as it has been called “organic”) memory; but is interesting as taking place when the person is otherwise in a completely normal state.

So much, then, for the conditions of the memory on complete waking; next as to its conditions in the hypnotic states themselves.

(1) Facts of the “subject’s” general knowledge, his address, business, recent employments, and so on, are remembered even in the deep state, if that state is sufficiently marked and prolonged for some amount of conversation to be sustained in it.

(2) With a favourable “subject,” something that has happened during one of the hypnotic states will often recur to the memory on the next occasion when that state is produced, though in the interval of normality—amounting, it may be, to several days and nights—which has intervened between the two occasions, it has been completely forgotten. (The thing, however, must not be an action performed *in propria persona*, for this—as we have seen above—would *not be forgotten* during the

\*Dr. Wyld, however, assures me that he has occasionally witnessed the phenomenon.

normal state; nor connected with a delusive impression, for this—as we shall see below—would *not be remembered* on the recurrence of the abnormal state.) This recurrence may be made a 'test of the extreme rapidity with which a "subject" will, in exceptional cases, pass from the alert hypnotic to the normal state—his attention being first arrested by a suitable incident, a few passes of the waking and "clearing" sort obliterating all knowledge of it, and then a few more of the opposite sort obliterating it back again to his mind, all within the space of a minute.

But the chief interest of this *induced* phenomenon of alternating memory lies in its resemblance to what occurs in *spontaneous* conditions. Even ordinary *dreaming* occasionally presents this feature in an embryonic form—a dream-scene or dream-incident being often more apt to recur in dream than in waking moments. Not that the mere recurrence could be taken necessarily to imply memory; but experiences are recorded in which the scene or incident, though one not associated with the waking life, is distinctly *recognised* as familiar when it recurs, which of course does imply a sort of memory. *Natural somnambulism* may seem to present more distinct resemblances; and certainly, if one judged from expressions used by the somnambulist, particular ideas which have made no part of the waking life are apt to recrudescence in the sleep-waking state. As a rule, however, it would be hard to represent this phenomenon as involving more than a mere re-awakening into activity of certain nervous tracts, which naturally manifest the fact of their activity by the same external results as on previous occasions. Even in the rarer cases, which more strongly suggest the recurrence of the past events as such, the presence of true psychic memory is more doubtful than in hypnotism, or at any rate is harder to substantiate; for the very tests which might substantiate it naturally tend to wake the somnambulist, and so to put an end to the condition. Perhaps, therefore, the clearest interest of the hypnotic alternations of memory is rather as illustrating the spontaneous alternations in cases of "*double consciousness*," where a single individual lives in turn two (or more) separate existences. There, as here, the transition may be almost instantaneous; and there, as here, while the memory of the *normal* state is continuous (its events being remembered even in the abnormal condition, just as we have seen that the events of ordinary life are remembered in either of the hypnotic states), the memory of the past events of any *abnormal* state lapses and recurs with the disappearance and reappearance of that state.

(3) If the phenomena mentioned under the last head are somewhat uncertain, it is otherwise when the condition intervening between two hypnotic states of the *same* kind is not normal wakefulness, but a hypnotic state of the *other* kind—*i.e.*, when a deep state intervenes between two alert, or an alert between two deep states. I have then found that (with certain well-marked exceptions to be mentioned here-

after) the ideas impressed in the one sort of state are invariably forgotten in the other, and are as invariably again remembered when the former state recurs. Thus the "subject," when in the alert state, is told something—some anecdote or piece of ordinary information—which we will call A. He is then thrown, or allowed to fall, into the deep state with closed eyes, and is asked, "What were you told just now?" He is quite unaware what is meant, nor will the broadest hints recall the missing idea. He is now told something else, which we will call B; and is then re-awakened into the alert state. Being asked the same question as before, he at once repeats not B but A, and it is impossible to evoke in him any memory of B. Thrown again into the deep state, he in a similar way recalls B, and A has once more vanished. Finally he is completely awakened, informed that two things have been told within the last five minutes, and offered £10 to say what either of them was—with a result entirely satisfactory to the experimenter. Occasionally I have succeeded in hitting a transitional moment at which *both* things were remembered; but it was a sort of knife-edge, and the slightest manipulation or pause tending to deepen the condition brought about the customary separation and oblivion of the thing told in the alert state.

The phenomena seem singularly constant. I have obtained them with a large number of "subjects," and with three operators in three different parts of England, two of whom certainly had no idea what result I was expecting. They represent, of course, that clear distinction of the two hypnotic stages—as something more than mere continuous degrees of a single trance-condition—to which I have been leading up; and the great rapidity of the transition, together with the sharpness of the results, seems to make them as satisfactory indications of that distinction as could well be imagined.

(4) To the rule thus established, there are certain definite exceptions. If the idea impressed in the alert state is a *delusion*, involving either a change of *scene* or a change of *identity*, it is not remembered in the usual way. A "subject" who has been made to believe himself elsewhere than in the room where he actually is, or to assume the part of another person or of an animal, and who while under this delusion has been carried into the deep state, on returning from that state proves almost always to be the natural self of the alert state, and refuses to believe that any idea of any sort has been impressed upon him. In the few cases within my experience where this has not happened, and where the delusory part was resumed when the alert state returned, it was noticeable that the bare *idea* seemed to revive first—very likely as the memory of the remark which had preceded and produced the illusion—and that then the illusion followed in the wake of the idea. Thus a boy who had been enacting the part of a fish on the floor, and who had been then thrown into the deep state and placed in a chair, was

brought back to the alert state by gentle upward passes. He sat for some seconds staring at the floor in a puzzled way, and then flung himself down and recommenced the fish-like movements. Waking to the alert state by any *sudden* means always ensured forgetfulness, carrying the "subject" at once over that low degree of the alert stage where recurrence of the delusion was possible. It may be added that though delusive ideas are thus forgotten, yet if the same delusion be again suggested in a general way, the *details* of the former one will be remembered. Thus a youth who had been impressed with the idea that he was a schoolboy attending Brighton College, and that his name was "Gerald Hamilton," completely forgot this change of identity when he returned to the alert state in which he had undergone it; but on being then again told that he was attending Brighton College, and asked his name, he gave it as Gerald Hamilton.

(5) The next case is stranger still, though quite as definite. If in the alert state any physical effect is produced by suggestion—*e.g.*, if the "subject" is made unable to flex his arm by being told that he cannot do so—a further very marked effect is produced on his mental powers and memory, although there is no special sign that his mind is preoccupied with attending to his bodily symptoms. A boy's arm was thus extended; he became unable to talk rationally; and being set to read aloud, he did so in a stupid and mechanical way, and could not recollect what he had read. He was now passed into the deep state, during which his arm dropped; and on being recalled from this state, was asked what he had been doing just before he went to sleep. He replied that he was holding his arm out; but both forgot and utterly denied the fact of the reading. Similarly the operations of opening a piano and picking out a tune on it, carried out by a "subject" while under an impression that he could not unclench one of his fists, were clean forgotten after an interval of the deep state.

(6) Any sort of argument or bothering has a singular effect in causing the "subject's" mind to drift into a deeper dream-like state. Thus, at the close of the experiment just mentioned, the "subject" was pressed for some time as to how his arm, which he remembered to have been stiff and extended, had dropped. While he was in a state of puzzle and worry, a sudden clap and call brought him instantly to a point at which the whole circumstance as to his arm was completely forgotten; but being allowed to lapse quietly, he again recalled it. There are thus sub-divisions of recollective power within the alert stage itself.

(7) We now come to an apparent exception of another sort. If the thing impressed on the "subject's" mind in the deep state is a *command*, which he is to execute "on waking," he will execute it as soon as he returns to the alert state; or, if allowed to work off his trance in natural sleep, he will usually perform the act on

normal waking; but if the act has been performed in the alert state, he will have no recollection of it when brought to his normal state.\* Such obedience is, however, no true exception to the rule that psychic memory of ideas does not extend from one state to another, any more than in the case above noticed where a "subject" obeys a command fixed for some distant hour: he feels an impulse but does not remember its source. A singular point in connection with this obedience is that it seems apt to fail in cases where a vivid and interesting idea is suggested at the same time as the command. Thus several "subjects" who were told in the deep state that a fire had broken out at home, and that they must go and help to put it out, on being recalled to the alert state sat without moving, and denied any impulse to do anything. The idea probably produced a strong mental picture, which, in disappearing with the change of state according to the rule above given, involved the further disappearance of the sense of obligation.

(8) Obedience also fails in the following case. If a command has been imposed in the deep state, and the "subject" is woken into the alert, but then, before he has time to perform it, is put under a delusion—this will *suspend* the performance of the act. Thus, a youth who had been told that he was to put on his hat and begin reading the newspaper, and had then been roused, was on the point of carrying out the command, when he was suddenly told he was a chicken. He instantly went down on the floor and began to cluck. He was then allowed to lapse into the deep state, and again brought out of it: he now at once performed the order. In this particular instance the order was not *remembered* in the second deep state, though carried out on emergence from it; the delusion had altogether obliterated it, as far as psychic memory was concerned. But this feature seems unsymmetrical, and was found not to be constant—the delusion as a rule having no effect beyond the particular sort of state in which it is induced.

Such in briefest outline is a sketch of the conditions of memory connected with hypnotism, so far as my own observation has gone. Brief as it is, it may perhaps suggest matter of reflection as to theories which assume hypnotism to be a state of mere unconscious automatism, on the ground that no true memory ever exists of what happens in it.

[All the experiments here mentioned were carried out as part of the work of the Mesmeric Committee of the S.P.R.]

\* After what has been said above, it will be readily understood that performance in the alert state of commands given in the deep, and remembrance of impressions from one alert state to another with a deep state between, are liable to be represented as performance or remembrance *on waking*—i.e., on complete waking into the normal state—owing to the ease with which the alert state may be confounded with the normal.

## VII.

REPORT ON WELLS SUNK AT LOCKING, SOMERSET, TO  
TEST THE ALLEGED POWER OF THE DIVINING ROD.

By W. J. SOLLAS, M.A., D.Sc., Fellow of St. John's College, Cambridge,  
Professor of Geology in the University of Dublin.

In response to an invitation I made one of a party of investigators, who on the 14th of May, 1883, proceeded to the little village of Locking, to conduct an inquiry into the alleged phenomena of "dowsing," "rhabdomancy," or divining for water. Our party consisted of Dr. Burder, the esteemed President of the Bristol Naturalist's Society, a lady who was believed to possess the power of divining, Mr. Pope, Mr. Pease, and others. I accompanied them as a professional geologist to determine the geological conditions under which the experiments were to be made.

At Locking we were introduced to the "dowser," Mr. Thomas Young, an aged and experienced man, who for 50 years had been engaged in finding underground water, and with admitted success. He showed us the whole *modus operandi* of his art; and we were told to particularly notice that, at the critical moment, the dowsing rod turned of itself, without any pressure being exerted by the fingers of the dowser; on this point, however, it was the general opinion that the dowser was mistaken. Dr. Burder and myself are positive that every time the rod turned we plainly discovered a corresponding movement in the fingers of the dowser. Not only Mr. Young, but Mr. Lawrence, a dowser of great local reputation, lay great stress on the fact, as they assert, that the movements of the rod are not governed by the movements of the fingers, and also that it turns independently of their volition, in fact, even in spite of their willing to the contrary. As to their volition of course I am unable to speak, but I am confident from what I observed, that the sole immediate cause for the turning of the rod is to be found in the muscular contraction of the hand of the operator.

We found Mr. Young very frank and communicative; one of his statements was to the effect that when the dowser walks backwards over the water he is in search of the rod turns downwards, while on walking forwards, it always turns upwards! A psychical fact so extraordinary that it seems worth while to put it on record. Mr. Young showed a close acquaintance with the ways and habits of underground

water. He told us that in the district about Locking it always flowed from east to west; moreover he actually showed us the natural spring from which the underground water issued which he had previously sunk upon at a point nearer its source, to obtain a supply for an adjacent farmhouse. With so much real knowledge the use of the "rod" seemed superfluous, so I ventured to inquire whether he would regard it as morally wrong to make use of his evidently practical knowledge to assist him in the discovery of springs. He replied, certainly not, but that "he would be nowhere without his rod."

We were now to put the dowser's skill to a practical test. A field on the alluvial plain, which extends from Locking to the Bristol Channel, was selected for the experiment, and on this the "dowser" was to indicate two spots, one beneath which water should be found, and another where it should not. Through these spots shafts were then to be sunk, and the prediction verified or not. In order to simplify the conditions of the experiment, Mr. Pease requested the dowser to operate blindfolded, but to this he absolutely refused to consent. This was very unfortunate, as the trial became one rather of the skill of the dowser than of the truth of dowsing: and I noticed during the trial that the dowser made very good use of his eyes. Looking about, he walked direct to one side of the field, without using his rod;—not wandering as one might have expected, rod in hand, hither and thither, in blind search for an indication. Arrived at the place he had chosen he used the rod over a line of march some 20 or 30 paces long, and as he did so the rod plunged vertically upwards at a spot where he then asserted water would be found. He repeated the traverse further down the field, in the same manner and with the same result. This he did again and again, and so produced the impression that a stream of water was flowing towards the sea beneath our feet, and along a line indicated by the successive points which he indicated by the rod. We therefore asked him to dowse along the length of the stream instead of finding points over it by traverses, and this he very willingly did, walking in an undulating manner over what he gave us to understand was the course of the stream. Finally, we requested him to make a fresh traverse and to indicate the spots where digging was to be commenced. He did so, and at our suggestion selected a place about 20 paces to the north of a ditch or "rhyn" which flows through the flat, as that where water would be found, and another 15ft. north of this, where he stated it would not.

We determined the position of these places in a way that left no possibility of changing them, and made arrangements for pits to be sunk by the dowser and his assistants. The supervision of the work as it proceeded was deputed to me.

Before proceeding further with an account of the experiment we



may consider the nature of the case we are about to try. It is the prediction of a practical man, skilled by experience, opposed to that drawn from the generalisations of science.

The dowser predicts that in one of the pits now to be sunk we shall find water, let us call that the + well; in the other we shall not, we will call that the — well. As a geologist I predict that we shall find water in both or neither. The dowser does not give reasons for his prediction; for mine they are the following.

The field we have selected for the experiments is part of the great alluvial flat which borders the eastern side of the Severn and Bristol Channel. Sinkings have frequently been made in various places through this flat, and I can therefore state beforehand that sand will certainly be met with in both wells, and will certainly furnish some water to both. Gravel may possibly be reached, and if in one, probably in both wells, and then both will yield water freely.

I now proceed to give an account of the observations made on the wells, as I visited them from time to time during their sinking.

May 22nd. Verified the positions of the wells. The + well had been sunk 10ft., and contained 6ft. of water; the — well had been sunk 5ft. and contained 1ft. of water.

June 1st. The + well was now 16ft. deep, and contained a good deal of water, eight or nine feet; water was still trickling in from the sides. As work had been suspended in this well, water had had time to accumulate. The — well was 20ft. deep, and contained 4½ft. of water. Water was trickling into it, but not so fast as into the other. The well-sinkers said that this 4½ft. was merely surface water. A bed of peat 14ft. from the surface had been struck in both wells.

June 15th. Visited the wells, but found that the work of sinking was suspended. The + well indeed had been abandoned owing to the caving in of the sides. The — well had been timbered down to 20ft.; it was 22ft. deep, and water was slowly dripping into it.

June 18th. Received a letter from Mr. Hellier, the bailiff of the estate, stating that the real depth of the + well was 17ft. 6in., and the depth of water in it 9ft. The — well 20ft. with 4ft. 6in. of water (these numbers are for Friday, June 15th).

June 24th. Commissioned my friend and pupil, Mr. F. P. Evans, to visit Locking. He reported that the water stood in both wells at precisely the same height, viz., 5ft. 4in. below the level of the field. So that the + well contained 12ft. 2in. of water; the — well 14ft. 8in. of water.

The reports from the 16th June to the 24th require some comment. On Thursday, the 14th, the men were sinking, consequently on that day the — well was dry; on the 15th, Friday, work having ceased for 24 hours, it contained 4ft. 6in. of water; on Saturday, the 24th (and

may be several days earlier), it contained 14ft. 8in. Thus the well which should not have furnished any water could not by any perversion of language be called "dry."

June 28th. Mr. Hellier writes, saying that the men have sunk through the clay and found black sand, in which they have sunk 8ft. On making a subsequent visit I found that this black sand was a fine arenaceous clay.

July 9th. Paid an unexpected visit to Locking; I was expected the previous Saturday; work had been suspended, but one of the sinkers, George Gale, was still there. He told me that they had been sinking on Saturday, the 7th, and had "kept the water down, for me to see everything!" On the 8th, Sunday, water, so he said, had accumulated to the extent of about 4ft.; on Monday afternoon, when I took measurements myself, there were 12ft. of water in it. So that 12ft. had accumulated in 48 hours. I made a thorough examination of the shaft, descending as far as the water, and examining the nature of the sides. The deposits passed through were as follows:—

Blue Silt	...	...	...	...	...	14ft.
Peat	...	...	...	...	...	1ft., or 1ft. 6in.
Blue Silt, becoming sandy lower down...						8ft.
Red Clay	...	...	...	...	...	5ft. or 6ft.
? Trias						

The sinker told me they had sunk to 29ft., in which case the red clay would have been penetrated to the depth of 6ft. If this red clay is Trias, then Trias must be beneath both wells, and the bottom rock being the same in each, the dowser must have calculated on the presence of gravel in the + well, and its absence in the — well, if he made his prediction on scientific grounds.

Water was streaming from the sandy sides of the — well just as I had seen it issuing from the sides of the + well on June 1st.

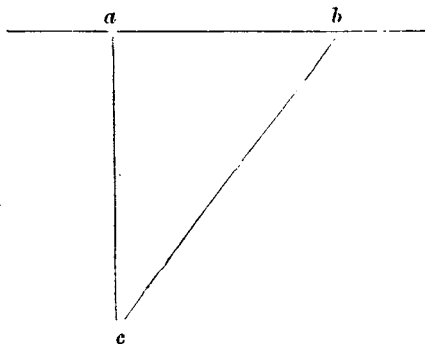
August. Mr. Metcalfe visited the wells for me and found the water standing at the same level in each.

This record of observations seems to me to completely verify the prediction made on the scientific side that the behaviour of the two pits with regard to furnishing water would in all probability be the same for each. The dowser attributed the water in the — well to an outflow from the +, but this is of course an excuse made after the trial had been completed, under conditions to which he had himself assented; and an excuse which is without any rational basis. For if the rock between the — and + wells is so porous that water can stream through it from one to the other (and the increase implies this) then the rock between the two, and necessarily

surrounding the two, will be water-bearing, and would furnish water to both, and thus would have supplied the — well if the + one had not existed.

The tenant who farms the estate said he had *seen* the water flowing from the + to — well! The dowser and his assistants confined themselves to asserting that it went into the — well from the side nearest the + well, as indeed, from my own independent examination, I can confirm it did; at the same time it trickled in from the other sides, that farthest off included, quite as freely.

To me, therefore, it appears that the case of the dowser has conspicuously failed, and I attribute it to the fact that the dowser's experience has throughout been gained and employed in the finding of *water*, not in seeking for dry places. As to his ability to discover underground water, these experiments simply confirm general testimony which is unanimously in favour of his powers. That these powers are totally independent of the use of the rod is, I think, proved by the following considerations. For the sake of argument, let us assume that the dowser is physically affected by the presence of water beneath his feet. The action between him and the water will take place in a straight line, drawn from him to it. On the analogy afforded by physical forces, the force of the action will vary directly as the mass of the acting bodies and inversely as the square of the distance. Let  $a$  be the + spot marked by the dowser, and  $b$  the — spot, distant from each other 15ft. Let  $c$  be the position of a stream of water, 20ft. vertically below  $a$



Join  $c$ ,  $b$ .

Then the action at  $a$  will be  $\frac{m}{a^2} = \frac{m}{400}$ , and at  $b$   $\frac{m}{b^2} = \frac{m}{576}$ .

So that if the action at  $a$  be represented by 1, at  $b$  it will be  $1 - \frac{11}{36}$ , or about  $\frac{2}{3}$ .

Now, if a force of 1 causes the rod violently to assume the upright

position, surely one of  $\frac{2}{3}$  should have some appreciable effect, whereas the rod shows no sign of agitation, remaining quite unconcerned, till  $a$  is reached.

Again, we have stated the case here far too favourably for the dowser, for the underground water is not all concentrated at  $c$ ; it is diffused throughout the alluvium, the underlying deposits are all moist, and the diffused water will act upon the dowser as well as that concentrated in one place. The action at  $a$ , therefore, is:—

1 + influence of the diffused water =  $X$ , and at  $b$ :  $\frac{2}{3}$  + influence of the diffused water =  $Y$ , and the difference between  $X$  and  $Y$  is much less than  $\frac{1}{3}$ .

Moreover, on many occasions, the underground spring exists at a much greater depth than 20ft., and yet affects the dowser when he stands vertically over it. Its influence in these cases must be often less than the  $\frac{2}{3}$  in our experiment, yet it makes itself decidedly felt.

The dowser is naturally possessed of much mother wit, and he has a large experience of the behaviour of underground water; but his wonderful rod has no more magical power than the gold-headed cane of the medical practitioner of the last century.

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NOTE BY EDWARD R. PEASE.

Whilst agreeing on other grounds with most of the conclusions which Professor Sollas sets forth in his very able report, I cannot feel his complete confidence in the conclusive nature of this particular experiment. In September I went again to Locking, and had both the wells emptied of water. The + well I found now to be a mere hole some 10ft. deep, whilst the — well was a carefully-timbered shaft 24ft. in depth. I made minute observations of the rate at which both wells filled with water, but during the course of them the sides of the + well caved in and filled it to the depth of 2ft. This almost destroyed the value of the measurements, and I was not able to repeat them. But in so far as they have any value, they seem to show that the + well filled more rapidly than the — well, although its depth was so much less. The tenant of the farm told me that the water in the + well was of better quality than that in the — well.

## VIII.

## THE DIVINING ROD.\*

BY EDWARD R. PEASE.

The Art of Divining has long been considered by men of science and by the general public as one of the Black Arts, which alone has survived in remote villages and amongst uneducated mining populations. Unlike the others, it is considered a harmless superstition that preys only upon men's pockets, and does not imperil their liberty or their lives. Therefore the strong arm of the law has neglected to extirpate it, and we are able to examine the only living example of a class of superstitions that flourished many centuries ago.

This is the commonly-held view of the matter. But a little investigation discloses the fact that belief in the power of the divining rod is by no means confined to remote villages and to ignorant persons. The evidence of its value is strong, and it comes from unexpected quarters.

The Divining or "Dowsing" Rod is a V shaped twig, commonly of hazel, from 1 to 3ft. in length, and from a quarter to half an inch in diameter. It is firmly grasped by the two ends, one in each hand, and the "dowser" walks carefully over the ground to be tried, holding the rod before him. When he comes upon a spring of water the rod moves as if of its own accord.

In England the rod is used for seeking for water chiefly in the south-western counties. It is employed also in the United States, and on the Riviera. In Cornwall and California it is used to discover veins of metallic ore, and in Pennsylvania to find mineral oil.

A rod is the most natural symbol of authority, and hence at all times and places, those who lay claim to occult powers are found to use some sort of magic wand.

But the divining rod which we are now considering, is a *forked rod*, and the fork is its essential feature. We must, therefore, disregard all

## \* LIST OF WORKS CONSULTED.

- "Jacob's Rod." Thomas Welton, London. 1870. (?)
- "Curious Myths of the Middle Ages." Baring Gould. 1872.
- "Myths and Mythmakers." Professor Fiske. Boston, 1873.
- "Proceedings Bristol Naturalists' Society." Paper by Messrs. Tawney and Pass, 1874.
- "The Divining Rod." Charles Latimer, Cleveland, U.S. 1876.
- "Psychological Review." Paper by Mrs. de Morgan. September, 1879.
- "Cornhill Magazine." January, 1883.
- "Nature." Paper by E. B. Tylor, May 17th, 1883.
- "The Divining Rod." By Rossiter W. Raymond, Ph.D. Read at Boston Meeting of the American Institute of Mining Engineers, February, 1883.

evidence of the use of a simple rod, whether by ancient peoples or by savages, and confine our attention to the comparatively modern custom of using a forked divining rod. A work on the art and science of water-finding, published in Orleans in 1569, contains no allusion to it, although in some places it must have been known at that date. Melancthon, who died in 1560, is one of the earliest writers on the divining rod, and he explains its power by the then common theory of affinities. Agricola, the great German mineralogist of the first half of the 16th century, also wrote on it at considerable length, and was by no means a believer in its efficacy.

From this time till the end of the 17th century, the rod was commonly employed for many purposes throughout the West of Europe, and the writings of philosophers and enthusiasts are numerous and bulky. Mr. Raymond, of New York, in his pamphlet, to which I shall refer more particularly hereafter, enumerates some 46 works, by nearly as many different writers, all published in the century and a-half preceding the year 1701.

During this period, especially towards the end of it, the foremost philosophers of the age, princes and noblemen, and Church dignitaries, debated, and experimented, and quarrelled over the existence or non-existence of this mysterious power, over the various explanations propounded to account for it, over the honesty or trickeries of celebrated diviners.

The Baron and Baroness Beausoleil were amongst the earliest devotees to its cause. The Baron was "undoubtedly, one of the foremost mine-engineers of his time." He was made comptroller-general of the mines of France, Hungary, the Papal States, and other countries, and he travelled all over Europe studying the subject, whilst the Baroness published books on the use of the rod. They obtained wealth as well as fame, but were accused of sorcery by their enemies, were imprisoned by Richelieu, and died in poverty about the year 1645.

Amongst other writers of the period Mr. Raymond mentions Robert Boyle, who in 1666 brought the subject before the Royal Society.

In 1692 comes the celebrated case of the murderers tracked and discovered by Jacques Aymar. The story is so well known that it is not necessary to dwell upon it here. It may be found in the *Cornhill Magazine* for January, 1883, and is fully discussed by Mr. Raymond in his pamphlet. Aymar attained great fame by his success, but he conspicuously failed when tested by Prince Condé, and was obliged to retire into private life.

In 1701 the use of the divining rod was condemned by the Inquisition, and for some time we hear but little about it. In 1780 a Dr. Thouvenel experimented with a diviner of Dauphiny, named Bleton,

and published a memoir in its favour, which created much sensation. Bleton was tested by other scientific men, but without success. The experiment of blindfolding him was repeatedly tried, and the indications of his rod were, under this condition, proved to be fallacious. Penet, who lived at the beginning of this century, was another celebrated diviner, who professed to discover coal and hidden metals. A commission of savans tested him for three days at Padua, but failed to obtain good evidence of his alleged powers, and afterwards he was detected whilst examining at night the enclosure where an experiment was to be made.

It would seem, therefore, that the claims of diviners were carefully investigated by competent men of science at intervals during the 17th and 18th centuries, and that they uniformly failed to elicit satisfactory results. The domain of science was then mapped out far more imperfectly than it is at present, and therefore alleged facts of an extraordinary nature were examined with an absence of prejudice which it is impossible now to obtain. We must therefore in my opinion, attach considerable weight to the negative results of these investigations.

During the last few years the art of divining has attracted fresh attention, and a number of articles and letters about it have appeared in various newspapers and periodicals.

A member of the Society for Psychical Research, Mr. E. Vaughan Jenkins, of Oheltenham, has made a most valuable collection of contemporary evidence, which is an unique and most important contribution to the literature of the subject, and which, with great generosity, he has placed at the disposal of the Society.

He has communicated with various professional diviners, and has obtained reports of their operations from landowners, architects, builders, and other persons, who are entitled to speak with authority. Concerning one of the best known of the professional diviners, John Mullins, of Collerne, Wilts, he has obtained twenty-two records of the successful location of wells, nearly all from persons of standing, chiefly owners of property in Gloucestershire and Lincolnshire. Twelve records refer to Mr. W. S. Lawrence, of Bristol, of whom I shall have occasion to speak later on; seven cases are recorded in which Stokes, a carpenter of Newbury, Berkshire, was the diviner; and seven others refer to Pavey of Cheddar, and one or two other persons. All these, it must be recollected, have been written and sent to Mr. Jenkins within the last 18 months, all are from persons actually cognizant of the facts, and generally present at the divining operation, and many of them are very careful and detailed statements of what took place. It must be remembered too that these were all collected by one gentleman, in the course of a few months, and are probably but samples of a vast mass of

testimony which a more prolonged and wider search would reveal. Several of them also refer to more than one case of successful dowsing.

In an appendix to this paper a complete list of these cases is given, and a few of them are printed in full.

Notwithstanding the general consensus of scientific opinion against dowsing up to this time, there seems to be a *prima facie* case for the necessity of further examination of the subject from the evidence collected by Mr. Vaughan Jenkins.

This evidence tends to show.

1. That the power of certain persons to find water when experts fail is widely believed in to this day, and is utilised by practical men to whom the finding of water is a business matter. This will be seen by a glance at the cases given in the Appendix. Many of these persons doubt the dowser's power to begin with, but are convinced of it by his success.

2. That this power does not depend on superior knowledge of the locality. See, especially, case 5, where Captain Smith says that "Mullins had not been in our neighbourhood before"; case 2, where Mr. Finch Hatton says, "We established on good authority that Mullins had no previous acquaintance with Haverholme"; case 4, where an old well was to be re-opened "but no one on the estate knew where it was, not having been opened for a number of years. But Stokes, with his divining rod, discovered the well (although a perfect stranger to the place), and it proved to be where he predicted, under the paving in the centre of the pathway." See also cases 1, 26, 29, 31, and 46.

3. That the power does not depend on geological or quasi-geological knowledge or instinct. See especially case 13, where Sir W. E. Welby-Gregory says, "To save time I took him (Mullins) to a spot from which he could see the contour of the hills (about a mile distant) from which most of our water comes, showed him what springs we knew of, and told him to choose for himself the best part to try for more. He unhesitatingly selected the upper part of a certain hill (which was afterwards pronounced by an eminent civil engineer to be unquestionably the most likely spot within sight to contain water), and thither we went. He tried the whole hillside over without finding a drop." Also in the same case 13, "The civil engineer before referred to assured me that from his knowledge of the geological formation of the country, he could say confidently that there was no chance of finding a water supply under my new house at a depth of less than 120 or 130 feet; and his opinion was confirmed by another geological authority who was then in the neighbourhood." Mullins had previously, on the site of the new house, "indicated two lines, about 30 yards apart, along which he said water was flowing. This was all he could find there." Ultimately Sir



W. E. Welby-Gregory "determined to disregard the geologist and have a well sunk on one of these lines," where water was found in due course.

4. That the power is not only shown in finding spots where water is, which might be explained by supposing that there was water everywhere in that region at that depth—but spots where it is not. See again case 13 where, as previously mentioned, a well was sunk to a depth of 25ft. on one of Mullins' lines. Subsequently a shaft for a lift was sunk "between the two lines . . . some 12 yards or 15 yards from either, and to a depth considerably greater (10ft. or 12ft.) than the well. As no water came into this, though the formation was precisely similar, and the well has not been affected by it, I am satisfied," Sir W. E. Welby-Gregory continues, "that, had I not employed Mullins, I might have sunk wells in any number to no purpose." See also case 31 where a well is sunk so that its side is below the point indicated and water comes in from that side. And case 41 where Mr. Adey says, "Stokes . . . told my foreman that it was of no use going on with that well . . . and told them where the spring was, and that it was near the surface. . . . I told him not to pay attention to such rubbish, and to continue sinking the well. . . . At the depth of 40ft. there was not the slightest appearance of coming to water. My men then threw out a hole where Stokes indicated, about 30ft. or 35ft. from the well, and at a depth of only 5ft. from the surface came upon a spring."

5. That dowzers believe themselves to possess some sort of occult power, although they can offer no explanation of it. It would appear, moreover, that they rarely make mistakes in locating wells. But some allowance must be made for the fact that most of our evidence has reached us from correspondents obtained through the diviners themselves. See, however, cases Nos. 3, 5, 7, 38, 39, 40, and 41.

We have next to consider the experiments recently made with diviners by myself and other members of the Society.

Mr. W. S. Lawrence, of Elton House, Bishopston, Bristol, a retired stone merchant, was known by my father for many years as a Poor Law Guardian, and on December 26th, 1882, he came over to try some experiments with me in my father's grounds. He is an elderly man, of much intelligence, and is widely known for his power of dowzing, which he still occasionally practises. He uses a piece of watch-spring, about a foot long, in preference to the hazel-rod.

He first walked about the garden and fields and located numerous springs. We then blindfolded him and took him over the same ground, but the springs were so plentiful that it was not easy to ascertain whether he found them again. Once or twice, however, we clearly observed that the rod did *not* move with the dowser blindfold in spots where, just previously, he had located springs.

We next tested him with metal, hidden under plates. Mr. Lawrence had not tried this for some years, and was doubtful of success. But at the first attempt, he found, amongst seven plates, the two under which metal objects were placed. On this occasion some of the numerous spectators knew which were the right plates. In a second attempt, when none of those present knew which were the right plates, he succeeded in finding one object, but was at fault in regard to the other. In a third attempt he wholly failed.

A lady present was thought to possess some power, and a metal object was hidden for her under one amongst eight plates. The rod moved over two plates, and both being taken up, metal objects were found under both of them, the second having been added by one of those present, unknown to the dowser and all others. This circumstance appeared to be very remarkable, but as the lady in question has totally failed in her later experiments, this success must be regarded as accidental, or possibly it may be explained by Thought-transference.

The result of these experiments as a whole was, therefore, by no means conclusive, as the existence of the supposed springs was not tested by sinking wells, either on this occasion or after the experiments about to be recorded.

In March, 1883, I received from Mr. Jenkins an introduction to Mrs. B., of Clifton, a lady who is known to be an amateur dowser.

Mr. Podmore and I called upon her, and we then learned that several years ago she found out that she possessed the power of divining for water, in connection with the well-known case of discovery of water by Mr. Lawrence, at Lawrence Weston. She also uses a steel watch-spring.

The following report was drawn up by us at the time:—At our request Mrs. B. tried to discover money hidden under plates, by means of her steel spring. She stated that she had not attempted this before, and that she "*did not believe in it.*" In the first trial coins under two plates out of five were correctly discovered. In this case all present but Mrs. B. knew which were the right plates. In two subsequent attempts in another room, when no one present knew the right plates, she failed partially in one case and completely in the other.

We drove to Cote Bank, and in the first place tried over the same ground (a gravel path) that Mr. Lawrence had tried in experiments previously recorded. Mrs. B. located a spring on a spot which Lawrence passed over, and several yards further on Mrs. B. passed over a spot where Mr. L. thought he detected a very strong spring. A dozen yards farther on Mrs. B. found a spring in about the same position as a continuation of the last-mentioned spring was located by Mr. L. We told Mrs. B. of the strong spring detected by Mr. L. and passed by her, and on her return she confirmed Mr. L.'s discovery, though locating the

spring a yard or two away. We then blindfolded Mrs. B. and led her over the same ground, when the results were decidedly different, the steel watch-spring moving in several cases where previously it had been still, and *vice versâ*. But, owing to the limited space and peculiar varieties of level, the result was not satisfactory in either way.

We then asked Mrs. B. to test carefully a long level gravel walk, and we marked several springs which she detected upon it. She went over the ground a second time, and did *not* very certainly confirm previous discoveries. We then blindfolded her, and led her over the same ground. The watch-spring now moved in four or five spots, only one of which approximately coincided with a previously marked spring. Mrs. B. stated that the springs were very slight, the rod movements vague and unsatisfactory.

We next tried over a place where we told Mrs. B. that an underground current of water existed. This she asserted to flow about a yard from the spot where both the appearance of the land and other circumstances led us to believe the drain actually was. She maintained her opinion after several trials with closed eyes. She traced the drain (which carries the purest spring water) for 100 yards, but the conformation of the ground left little choice of directions.

We then tried coins under plates. We selected a spot in the drawing-room clear of springs, and placed four plates on a wooden table, which we moved round whilst Mrs. B. sat in a chair. One of the party only hid the metal (a large heap of silver, and a brass  $\frac{1}{2}$  lb. letter weight), and then left the room. The conditions were therefore carefully arranged. Mrs. B. selected two out of four plates, because the watch-spring moved freely over them, whilst it remained still over the others. These plates over which the rod moved were the two covering *nothing*. The other two covered the metals.

Mrs. B. then dowsed over the house, and detected correctly a very strong spring in a passage, which we told her led over a well. We went into the cellar, and here, over the same spring, close to the pump, the rod did not move the first time she passed over it. She detected springs in several places where Lawrence had not, and *vice versâ*.

This concluded our experiments with Mrs. B.

The next experiment was on a larger scale.

About this time Mr. Pope, of Clifton, had a well successfully located for him at a farm near Locking, in Somerset, by a dowser named Thomas Young. He sent us a report of the case and of some experiments made with the rod, and offered us permission to dig a trial well at the same place. Some members of the Society generously provided funds, and in May a party of us went down, including Professor W. J. Sollas, then of Bristol, now Professor of Geology at

the University of Dublin. He has presented us with the report which precedes this paper.

I must now proceed to the second part of the inquiry, by examining more particularly the methods and claims of diviners. My object is to ascertain on what points the theory and practice of the majority of diviners is agreed, and whether any physical or psychical cause or causes can be suggested which might furnish an explanation of the phenomenon.

As previously stated, the orthodox rod is a hazel twig shaped like a **V**, and from 1 to 3ft. in length. But many other materials are used; a piece of steel watch-spring is employed in Somersetshire; and in other places a whale-bone instrument with a terminal chamber full of mercury, or full of a mysterious unknown compound; or ordinary forked rods made of any sort of wood, green or dry, of iron or brass wire, or, as one authority says, "of any other supple and solid matters." Occasionally the genuine dowsing-rod seems to be unforked, and we hear of cases in which no rod at all is used. When a piece of watch-spring is used, it is held bent into a bow, which acts in the same manner as a forked twig. The rod is grasped by both hands, in various ways, which however have this in common, that the dowser holds it in a position of extreme tension, and at the same time of unstable equilibrium. Consequently a very slight variation in the pressure produces forcible and even violent motion, apparently quite unconnected with any action of the dowser. The dowser, thus holding his rod, walks slowly across the ground to be tried, and when he comes over the spring of water, or vein of metal, the rod begins to move. How it moves seems to depend upon the locality and the practice of the dowser. Sometimes it is held horizontally, and jerks violently up, as if repelled by the water spring; sometimes the water appears to attract the rod, and it moves downwards; sometimes it moves in a complete circle, approaching and then receding from the water.

The powers of the rod are a question in much dispute. It has been used to discover many things, viz. :—

Water in general (as in buried tubs): spring water, as opposed to surface water, when both are in buckets: water springs [even beneath the sea]: running water, as distinguished from all other water. Any metals, metallic ores, or compounds, and pure metals as opposed to alloys: coal, mineral oil, ochre, gypsum, red chalk, sulphur, &c. : lost boundaries of estates: Protestants; murderers, thieves, and other lesser criminals: and in fact, to quote the author of "Jacob's Rod," it can discover "many hidden things about which one is often much troubled, but few persons know the way to find them."

The popular theory is that there is some unknown force acting directly between the hidden thing and the rod. Such a notion need not, however, be seriously discussed here. It is impossible to imagine any

direct interaction between rods (which may be composed of wood, metal, or whalebone), and hidden things, which may be water, metals, metallic compounds, mineral oils, &c.

Moreover, this alleged occult force in some places attracts the rod to the object sought, whilst in other localities it would seem to have a repulsive effect, and again, in other cases, it produces a rotatory movement. This variation occurs even in the same locality, and with one diviner. It seems obviously highly improbable that any specific force can exert such varied movements.

The next theory to be examined is that the rod is moved by the diviner's muscles, and is merely an index of the effect of some subtle force which emanates from the water or metal, and acts on the diviner himself.

It is a moot point whether the diviner feels any sensations when the rod is working. Most witnesses assert that they have none whatever, whilst Mullins, and one or two others, state that they experience a thrill, or vague sensation, when they come upon the water.

Diviners always tell us that they are certain they are not moving the rod, and even that they attempt to restrain its action. No doubt, they are perfectly honest. But anyone who has had the smallest experience in psychical research is aware that such statements have no value whatever. We know by experience what care and study are required to discover whether our own hands are conspiring to deceive our intellects. We are not, therefore, surprised to find that honest dowzers are easily deceived by the unconscious actions of their own muscles.

It is commonly asserted that the rod cannot be made to turn, except in the hands of a diviner. This is simply a mistake. Anybody can work a divining rod, and can produce all the usual motions by means of muscular contractions, so slight as to be scarcely apparent to a careless observer.

Moreover, how can believers in the occult rod-moving force explain the curious experiment previously recorded, in which the steel spring moved strongly when held over two plates covering nothing, and was motionless over two others covering masses of metal? In this case it seems clear that the rod movements must have been caused by the unconscious volition of the diviner; and in this case above all others, the honesty of the diviner is beyond dispute.

The theory that a peculiar physical constitution is an important factor in the diviner's art receives much support from the fact that only a few persons are able successfully to use the rod. Mr. Baring Gould, however, tells us that on one occasion he saw watching the diviner a

crowd of villagers who each in turn tried the rod over the hidden spring; no one of them could restrain its movements, and they were somewhat surprised that Mr. Gould, when his turn came, succeeded in keeping it motionless. This would seem to be an exceptional case, and we shall now proceed to examine the phenomenon in its simplest form. We find that spring water and metals are alleged to have a certain obscure effect on certain exceptional persons, and that this physical disturbance of their tissues causes slight muscular contractions, resulting in movements of a rod held in their hands.

As regards pure metals the evidence is entirely experimental. Somebody hides a heap of coins, and the dowser proceeds to discover them as a proof of his power.

Unconscious indications given by the spectators would afford a simple explanation of this class of phenomenon; and in cases where these indications are excluded, the diviner may be assisted by the newly discovered faculty of Thought-transference. In harmony with this suggestion, we found in our own experiments that when those present knew the chosen plate, the diviner was often successful, whilst, when no one present knew it, the diviner generally failed. Messrs. Pass and Tawney tried experiments of this sort in Somersetshire, an account of which they read to the Bristol Naturalists' Society in 1874. In five trials, in each case amongst seven or eight objects, the diviner was successful three times in all. Messrs. Pass and Tawney attempt to explain away this decidedly remarkable result; but their success in so doing seems to me by no means so remarkable.

In a similar manner we may explain how it is that Mr. Stokes, of Newbury, can detect between two buckets of water, which of them is surface and which spring water, although older diviners assert that when the rod moves they "do not know if it is a spring or stagnant water." ("Jacob's Rod," p. 24.)

Next turning to metallic ores, I have been unable to obtain evidence beyond the general statement that the rod is commonly used in Cornwall for mining purposes. Mr. Raymond, in his paper, gives many instances of its complete failure in America, and of money lost by mining companies through their belief in its efficacy.

Finally we come to the use of the rod for the discovery of water springs. The evidence for this, as shown in the accompanying abstract, is exceedingly abundant and strong. There are plenty of diviners of good repute now living who without doubt use the rod, and find water. But the value of this evidence wholly depends on the abundance of water springs below the surface of the earth. If water is commonly to be found wherever one digs, but little of the evidence before us is of much value. This question can only be answered decisively by sinking a number of experimental wells such as those at Locking. This, how-

ever, is a very costly operation, and I fear the experiment is not very likely to be made.

Mr. Raymond's summary of the evidence is as follows: After stating that the application of the rod to the discovery of metals, coal, buried treasure, oil, &c., is shown to be chimerical, he proceeds in a passage of which the following is the substance:—

“The case is somewhat different with the discovery of springs and (since ore-deposits always have been and often still are the channels of springs) of ore-deposits.

Here we have much stronger and more abundant evidence in favour of the rod, and here, in my judgment, there is a residuum of scientific value after making all necessary deductions for exaggeration, self-deception, and fraud. There is undoubtedly a practical science of discovering metal-deposits and springs. Everybody knows that Indians in our desert West can find water where most white men cannot, and the experienced frontiersman has learned the art from the savages. The superficial signs of water springs are often subtle, and in settled districts they are little studied by the inhabitants. It is not necessary that a farmer who will want to locate a well once in his lifetime should know the signs of water as a ranchman must know them. Hence men who are keen observers get impressions amounting to a local science of the “lay” of the rocks, the difference in surface vegetation, &c., &c. The unconscious judgment of such an expert may decide upon a given spot, as he walks over it.

This brings me to the final inquiry whether there may not be, apart from unconscious skill or judgment, a purely physical effect produced by a subterranean spring upon a person walking over it. The effects of moisture and temperature upon the nerves, are very striking, and here, I think, is a matter which writers on the Divining Rod have generally overlooked.”

With this suggested explanation of the possible residuum of fact, Mr. Raymond closes his most valuable paper, and a summary of the evidence here presented would seem to point to a similar conclusion.

If we looked only at the history of Divining, we should dismiss it at once as a superstition. And, again, if we confine our attention to test experiments, we find nothing that merits serious discussion.

But the evidence for the success of dowsing as a practical art is very strong, and there seems to be an unexplained residuum when all possible deductions are made for accident, for local knowledge, and for inaccurate observations.

Mr. Raymond's hypothesis of the effects of moisture and temperature is not a very satisfactory one, but no better has yet been suggested.

## APPENDIX I.

## ABSTRACT OF EVIDENCE

*Collected by Mr. E. Vaughan Jenkins, of Cheltenham.**Diviner—John Mullins, of Collerne, Wilts.*

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
1	H. D. Shrines, Claverton Manor, Bath	Somerset	1852 and since	Mullins, a stranger in the place, located several wells successfully, and pointed out a spot where a spring was known to be; details of other cases. Archdeacon Earle was a witness.
2	W. J. Brown, Hazlebury House, Box	Somerset	1872	Details of two good cases; only well near, 180ft. deep; found a disused well which was covered over; found sovereigns hidden under stones.
3	Col. Wilson, Sleaford	Lincolnshire		Employed M. "a good deal;" sunk six or seven wells by his advice, all successfully. Wells found in dry country where large sums had been spent in boring for water without success.
4	J. H. Vessey, Welton Manor, Louth	Lincolnshire	1876	Well located correctly, and depth fairly given.
5	Capt. H. Smith, J.P., Folkingham	Lincolnshire		Given in full in Appendix II.
6	R. H. C. Nevile, Wellington Hall, Grantham	Lincolnshire		Water found near trial well 10ft. deep. Spring at a higher level and only 5ft. down; other striking details.
7	A. Taylor, Hay Hill Dairy, Bath	Somerset	17 years ago up till now	Has known him 17 years, and has employed him often, with most satisfactory results.
8	G. W. Johnson, Steward to Lord Lindsay, Stamford	Lincolnshire	1883, &c.	Details of several cases of success; says that local knowledge is needed to judge depth of spring. An emphatic testimony.
9	Geo. Hancock, Corsham, Wilts	Wilts		Given in full in Appendix II.



NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
10	Geo. Cooling and Sons, Seed Warehouse, Bath	Somerset		Given in full in Appendix II.
11	E. Issott, Ardwick Brewery	Manchester		Abundant water at depth predicted. Ground near had not been tried for water.
12	Hon. E. A. Pelham, Grantham	Lincolnshire	1877	Strong spring found at depth predicted.
13	Sir W.E. Welby-Gregory, Bart., M.P., Grantham	Lincolnshire	1877	Given in full in Appendix II.
14	F. T. Mott, F.R.G.S., Birstal Hill, Leicester	Leicester	1882	Given in full in Appendix II.
15	J. Thompson, Steward to the Duke of Beaufort, Badminton	Glo'ster	1877	Water scarce. Strong spring found deeper than predicted. Mr. T. was then not a believer in the Divining Rod.
16	G. Anst, Agent to Taylor Trustees, Bath		1878	Well found at about predicted depth, "where it is difficult to obtain good water."
17	P. Penchin, The Brewery Box	Wiltshire		Two cases described.
18	J. Copley, Melton Mow- bray	Lincolnshire		Found a small spring at 7ft. in a locality where water is scarce; had previously sunk 75ft. in vain.
19	J. R. West, Bath	Somerset	1877	Found spring for a brewery
20	J. H. Vesey, Malvern		1883	Spring found at right depth (letter addressed to Mulline)
21	The Hon. M. E. G. Finch Hatton, M.P., Sleaford	Lincolnshire		Given in full in Appendix II.
22	Benj. Perry, Bristol	Glo'ster	1873	Springs found in two cases by watch-spring bending down. Detailed account.

Diviner—W. S. Lawrence, of Bristol.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
23	Samuel Lang, 3, Bath Parade, Bristol	Glo'stershire		Well dug in vain at cost of £100; spring located 20ft. away; tapped by "culvert." Details given. A remarkable case.
24	J. Parsons, Accountant, Bristol	Glo'stershire	1882	Water found for Great Western Electric Light Company.
25	J. G. Davey, M.D., and Ass. S.P.R.	Somerset	1865	Old well at Northwood Asylum insufficient; new spring found and culvert driven to it. Careful and detailed account.
26	W. H. Cowlin, Building Superintendent, Eagle Land Co.	Somerset-shire		Detailed account of discovery contrary to local expectation. Clear and good.
27	Bristol Portand Channel Dock Company	Glo'stershire	1883	Letter from Dockmaster and Hydraulic Foreman; accurate details; water discovered at 70ft.
28	T. Stone, Bristol	Glo'stershire		Well dug with moderate success.
29	Mrs. Hare, Newton Abbot	Devon		Spring found, where local men considered sinking hopeless; Lawrence a stranger. Good case.
30	R. Harvey, Ashton Gate Brewery	Somerset	1873	Spring found for the Company. (Mereweather also present as dowsar.)
31	Stephens and Bastow, Bristol; S. Martell, their Foreman; H. J. Shaw, Architect, London	Sussex	1879	Given in full in Appendix II.
32	Col. Blount, Dorchester	Dorset		Strong spring found.
33	J. H. Lockley, Lewins Meade Brewery, Bristol	Somerset		Valuable spring found for the Company; also a well pointed out which was known only to writer.
34	H. Crisp, Architect; R. Butterworth; Lawrence; Mereweather; Mrs. Bengough, &c., &c.	Glo'stershire	A few years ago	Given in full in Appendix II.

*Diviner*—W. Stokes, of Newbury, Berks.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
35	Canon Portal, Newbury	Berks		Strong spring found.
36	Capt. Ward, Newbury	Berks		Spring found.
37	H. Taylor, Oare Hermitage, Newbury	Berks		Detailed account of experiments.
38	W. Chatters, Sanderford Priory, Newbury	Berks		Eight or nine times and no failure.
39	W. Church, Builder, Newbury	Berks		Never known failure. Gives case of discovery 2ft. from vain boring, 35ft. deep. Has seen Stokes distinguish between a bucket of spring water and one of stagnant water.
40	Charles Adey, Marlborough	Wiltshire	1880	Successful find. Diviner not in his own locality.
41	W. G. Adey, Newbury	Berks		Given in full in Appendix II.

*Diviner*—Charles Cross, coachman, of Hallatrow, near Bristol.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
42	T. Killen, Farrington Gurney	Somerset		Spring found at 30ft.
43	M. J. Williams, Paulton	Somerset		Spring found at 37ft.
44	G. Thresher, Frome	Somerset		Spring found.

*Diviner*—Thos. Pavey, of Cheddar, Somerset.

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
45	T. K. Taplin, F. J. Nalder, Westbury-sub-Mendip	Somerset		Disputed case, in newspaper correspondence. Mr. Nalder had a well found by three independent dowzers and considers it clear and valuable. A former owner pooh-poohs the matter.
46	Alfred Smith, Pershore	Worcestershire	1883	A good case; Pavey went, on Mr. Vaughan Jenkins' recommendation, to a new district; water found in an unlikely place.

*Diviner—George Lockyer.*

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
47	E. V. Jenkins, Cheltenham	Monmouthshire	1852	Given in full in Appendix II.

*Diviner not known.*

NO.	NAME AND ADDRESS OF CORRESPONDENT.	LOCALITY.	DATE.	ABSTRACT OF CASE.
48	Angus and Co., Contractors, Australia	Dorset	1874	Given in full in Appendix II.

## APPENDIX II.

## SELECTED CASES

*From Mr. E. Vaughan Jenkins' Collection.*

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No. 5.

FROM CAPTAIN HENRY SMITH, Horbling, Folkingham, Lincolnshire.

*December 26th, 1882.*

It will perhaps be the best answer to your letter respecting John Mullins (the water-finder as he is called here) if I tell you as well as I can what took place when he was at my place. My object in sending for him was to obtain his advice as to the best place to bore for water on some of my land, and when he came he said he must get a twig. I told him I did not believe in his conjuring tricks, I only wanted his advice, and he then said he was no conjurer, but was not the slightest use without his tools, so I stood by whilst he cut a couple of white thorn twigs. My nephew went with me, and we each carried a thistle spud. In crossing the first field I said "I have water here, so need not trouble you," but Mullins said he might as well carry the twig in his fingers, and whilst we were talking about the stock in the field he stopped suddenly and said we were crossing water. This I knew was the case, although there was then no appearance of any. The next field was the one I especially sent for him for, as I had dug a pond and bored about 40ft., but found no water. I took him to the side of the field above the pond, and after casting about for some time he said decidedly there was no water there, and proposed trying the other side, where he said there was plenty. We marked the place, and I have since bored there and found an abundant supply of water. I then took him across two other fields, and in each he found what he called a weak spring not worth boring for. We were at that time walking a little distance from him on each side, and marked the ground where he said he was over water, I believe unknown to him. On our return I said, "Now Mullins, I shall blindfold you and see if you can find those places again." He agreed to hold his head back and keep his eyes firmly closed, and as far as we could see he did so, and in each field stopped at exactly the same places we had marked, and the marks on the turf were so slight and from 50 to 100 feet from him on each side that I do not believe he could have seen them if he had opened his eyes.

We then came home to my lawn, and I showed him a beautiful spring, and also informed him what quarter we supposed the water came from (north); he tried all over the north side, and said most positively there was no water there, and then proceeded to the south side, and seemed almost convulsed. He said it was the strongest spring he had ever felt. I and my nephew took hold of the twig on each side below his fingers, and still it twisted up. I have bored there and found abundance of water. I may say here that Mullins had not been in our neighbourhood before, and distinctly asserted the water bearing strata lay very different from that we had always before supposed. We are on clay between the fen and limestone, and the land rises steadily from us until we reach the highest

point on the Great Northern Railway (Great Ponton), and my theory was that the rainfall there percolated the limestone and under the clay strata which is continued from thence to us. This Mullins quite denied, and our borings have proved the correctness of his statement so far as this, that there is abundance of water in the direction pointed out by him, but there may also be water where he says there is none; this has not been tested.

I did not look upon Mullins as a charlatan, but suspected he did a good deal by guess, and must confess to being unable to prove this.

That he could find the same indications of water blindfold quite puzzled me and my nephew, too, who is a thoroughly practical man.

That the twig was acted upon when Mullins had no control over its movements, we were also both satisfied. Mullins was very confident both in his assertion there was water or otherwise, and certainly did not adopt the usual rôle of a charlatan of falling in with the views of those who paid him.

I have recommended him to some first-class business men, and all tell me they are satisfied he is no impostor. I may add that, as a magistrate, I have frequently been told that I ought not to countenance such a man, but I generally take my own line, and try to arrive at some conclusion. In this matter I can only say I do not understand it at all, but give you the facts as near as I can, and shall be glad to hear of any further steps taken to explain this mystery.

#### No. 9.

From GEO. HANCOCK, "Fair View," Corsham, Wilts.

*January 2nd, 1883.*

Our testing of John Mullins' water finding powers has been of a negative rather than of a positive character. We have lately sunk a number of shafts with a view to mining operations, and our desire has been to select spots where there was no water to hinder us in our sinkings.

Mr. Mullins has gone over the ground with me and pointed out such spots. He has always been careful to explain "that we may, in a wet time, be troubled with surface water, but that we should find no springs." In every case he has been right. But in a shaft which we are now sinking we have come to a fissure about 40 ft. from the surface, and through this fissure there comes so much water that I have to-day been obliged to arrange a pumping apparatus in order that the men may be able to proceed with the work.

The ground in the neighbourhood of this shaft Mr. Mullins reported to be full of springs, and sinking our shaft below the level of the springs and breaking into the fissure may have diverted the water from its natural course. In that case there may not have been, when Mr. Mullins went over the ground, any water where now it is coming into our shaft.

Mullins generally forms an opinion—and a roughly correct one—as to the depth of the water below the surface, but he lays no claim to accuracy on this point. He says that a weak spring near the surface exerts the same degree of force upon the twig as a strong spring far down.

I have observed that after he has been walking about for some hours he gets physically exhausted, and, if I mistake not, somewhat irritable. He has told me that when he passes over a strong spring and the twig turns quickly, a thrill runs through his entire system, and that if he follows the water finding for several days consecutively, he is unable to sleep at night.

From what I have seen and heard of Mullins, I believe him to be a perfectly honest and reliable man. It is certain that he finds hidden springs and streams of water by the means above described, but he himself is ignorant of the natural law—and I make no doubt it is a natural law—by virtue of whose operation there is an affinity between him and the stick, and between the stick and running water, when the stick is in his hands.

## No. 10.

From GEO. COOLING AND SON, The Seed Warehouse, Broad Street, Bath.

*January 6th, 1883.*

We had very strong proof of the efficacy of the divining rod through Mullins, of Colerne. We had gone to very considerable expense in excavating to secure a supply of water for our greenhouses at the nursery, and were about giving up the matter in despair when we were recommended to send for Mullins, which we did, although with a great deal of incredulity. He, however, pointed out a spot where he said at a distance of 10 or 12 feet we should get a good spring, and within that depth we found water, and have had a constant supply running through all our houses since.

## No. 13.

From SIR W. E. WELBY-GREGORY, Bart., M.P., Denton Manor, Grantham.

*January 28th, 1883.*

In the spring of 1877 I was preparing to build a large country house on a new site, which I had selected on account, among other reasons, of its dryness. This site was on a large plateau of red loamy soil, resting on a bed of solid rock\* several feet thick, in which there was no indication of water; and it was a problem where the water supply was to come from, as there was none apparent which could be brought to the house without great expense. While puzzling over this I heard that John Mullins, of Colerne, had been employed to find water at various places in the neighbourhood with very remarkable success; that he had been tested in almost every conceivable way, and that not only country gentlemen and farmers, but plumbers in the neighbouring towns had frequently called in his aid, purely as a matter of

\* The rock I believe is marlstone, and the thickness of the upper bed has been since ascertained to be between seven and eight feet.

business, and I was at last induced to send for him. When he came, I asked him whether he required a twig of any particular wood, and he replied that almost any wood would do, except lance-wood and box-wood, which were too stiff. He then cut for himself from the nearest convenient tree, which I think was a sycamore, a forked twig in the shape of a Y of a foot or 18 inches long. I set him to work for a few minutes at first on my lawn, sending him over parts where I knew there was a current of water at a depth of a few feet, though none was visible or audible on the surface. He slowly quartered his ground like a pointer, bending forward and holding the twig about the level of his knees, point downwards, and tightly grasped an inch or two from the extremities of the prongs of the fork, with, if I recollect rightly, the thumb and fourth and little fingers of each hand below, and the fore and middle fingers above the twig. Whenever he crossed the water, of which I have spoken (a drain), the twig turned upwards in his hands, though he apparently resisted its doing so to the utmost; and the same thing happened at other parts of the lawn, where, though we did not know of any runlet, it was very likely that one existed. I then took him to the kitchen garden, where we had always been much in want of water; and presently the twig stopped him at a spot apparently as dry as the rest, and with nothing distinctive about it. I asked him what amount he supposed there was, whether running or stagnant, and at what depth. He answered that he was certain there was water, and that it was running, for no amount of stagnant water had the smallest effect upon him; but that he could only guess at the amount and the depth from the force with which the twig turned up. From his experience he should say that it was a stream not thicker than a walking stick, and at a depth of from 20 to 30 feet. He traversed the rest of the kitchen garden without finding any more, and I next took him to the site of the new house. Here he soon indicated two lines, about 30 yards apart, along which he said water was flowing in somewhat greater volume than the rill he had found in the kitchen garden, and at a depth, he thought, of from 30 to 40 feet. This was all he could find there, and as this seemed hardly likely to be sufficient for my requirements, I felt that it must be looked for at a greater distance. To save time, I took him to a spot from which he could see the contour of the hills (about a mile distant from the house) from which most of our water comes, showed him what spring we knew of, and told him to choose for himself the best part to try for more. He unhesitatingly selected the upper part of a certain hill (which was afterwards pronounced by an eminent civil engineer to be unquestionably the most likely spot within sight to contain water), and thither we went. He tried the whole of that hillside over, without finding a drop, and we afterwards took another hillside with no better success. It was now late, and Mullins was evidently becoming exhausted. I felt that I had seen enough to convince me that he was no impostor, and that whatever discoveries he made were due to some force over which he had no control, not to any power he had acquired by experience or observation of making good guesses at where water was likely to be; but I was not satisfied that he had found anything for me which promised to be of any practical use; so I dismissed him to tea in the housekeeper's room. After tea several of the servants whose curiosity was excited got him to exhibit his



art to them, and tried whether they themselves had any power with the twig. My gardener, Joseph Towers, found that it worked in his hands nearly as strongly as in those of Mullins himself. I took no further action then in the matter; but described what I had seen to the civil engineer before referred to, who, though by no means altogether pooh-poohing it, assured me that from his knowledge of the geological formation of the country, he could say confidently there was no chance of finding a water supply under my new house at a depth of less than 120 or 130 feet; and his opinion was confirmed by another geological authority who was then in the neighbourhood. So I virtually gave up all hope of deriving any benefit from Mullins' assertions. Some weeks later, however, my gardener came to me and said that he was in great want of water in the kitchen garden; that he had tried repeatedly with the twig over the place where Mullins indicated a rivulet; that it invariably turned up at the spot, and that he was quite convinced there was water there. If I would allow him, he could sink a well with the garden labourers, so as not to involve any additional expense. I consented to this; the well was sunk to a depth of nearly 20ft., when water poured into it freely, and it has supplied a long range of hothouses ever since.

I had previously decided upon bringing down the main supply of water to the new house in pipes, by gravitation, from a considerable distance; but as this was a heavy work, necessarily taking much time, the question of a supply during building had become urgent, and on the strength of what had occurred in the kitchen garden, I set Towers to traverse the new site, where the lines of water indicated by Mullins had been marked by pegs 60 yards or 70 yards apart, and just visible above the grass. These lines Towers and his twig emphatically confirmed, and I proceeded to test him. I had the projecting extremities of the prongs of the twig held tight by pincers, so that there could be no voluntary action on Towers' part when crossing the marked lines. Despite of this, the point of the twig twisted itself upwards, till the bark was wrinkled and almost split, while the strain and pressure upon the muscles of the man's hands were most apparent. I then blindfolded him, and turned him loose. The result was precisely the same. Whenever he crossed Mullins' lines, up went the twig. The presumption now appeared to be so strong in favour of the twig that I determined to disregard the geologists, and have a well sunk on one of the lines. This was done; at the depth of about 28ft. the water rushed in, and rose till it stood about 8ft. deep, at which it now remains, having, in the meantime, fully supplied all the requirements during building the house, which were probably not less than 1,000 gallons a day for three years or more, and since that having acted as an ample reserve to the house for all purposes whenever the distant supply for any reason has had to be shut off. I may add that I have since had occasion to sink a shaft for a lift between the two lines indicated by Mullins, some 12 yards or 15 yards from either, and to a depth considerably greater (10ft. or 12ft.) than the well. As no water came into this, though the formation was precisely similar, and the well has not been affected by it, I am satisfied that, had I not employed Mullins, I might have sunk wells in any number to no purpose under my house, unless I had happened to hit upon the rills indicated by him with such perfect precision.

## No. 14.

From F. T. Morr, F.R.G.S., Birstal Hill, Leicester.

*Written for the British Association Meeting at Southampton, 1882.*

I happen to be the owner of two acres of land on the slope of a hill in Charnwood Forest, Leicestershire, 600ft. above sea level. The hill consists of some ancient metamorphic slate, of which the geological horizon has not been definitely determined. On this land there is no spring water. Springs exist in several directions at distances of from half-a-mile to a mile, but on the other hand several wells have been sunk at similar distances in other directions but no water found. In one case the boring was carried to about 100ft., in a rich valley, but the search for water was unsuccessful. About a month ago I heard accidentally, but from what appeared to be a disinterested source, that a man was living near Chippenham, in Wiltshire, who had a peculiar method of discovering springs. As a good water supply would double the value of my land, I made some inquiries and received from a brewer at Manchester, and from a large agriculturist at Grantham, such strong testimony as to the ability of the person referred to that I thought it worth while to send for him and to let him try his skill. He came accordingly, a fine, sturdy, intelligent artisan, with no scientific knowledge, but a frank, manly address, and not the slightest appearance of a charlatan. He said, "If there is a spring on your land I think I can find it, but my method is a mystery to myself; I am always in doubt whether it will be successful, but hitherto it has never failed, and I have performed the operation hundreds of times." I took him to the land, and on the way he stopped at a hawthorn hedge and cut out a branch. This he trimmed up until he had got two twigs about the thickness of a cedar pencil, united at one end so as to take the form of the letter V with something like a right angle at the base, and each of the arms rather more than a foot long. On reaching the ground he took one of the arms in each hand, holding his extemporised implement before him with the angle downwards, and he walked across the field in a direction at right angles to the slope of the land. When he came to a certain spot I saw the angle of the stick turn upwards suddenly and forcibly. He stopped at this point and desired me to drive in a stake. Then he went about 20 yards further down the field and crossed it again in the same manner. This time, however, I accompanied him step by step, holding his hands firmly grasped in mine and watching carefully for any movement of his muscles. Again the angle of the stick leaped up, twisting round within the grasp of his fingers, and another stake was driven in. A third time he crossed the field, still lower down, while I held the two arms which projected beyond his closed hands, nipping them with my full strength and watching his hands at the same time. Yet again the stick turned up as before, twisting round within my fingers as well as his, and a third stake was put down. This process was gone through several times more, with like results, and we then found that the stakes stood in a diagonal line across the middle of the field. "Now," said he, "if you sink a well anywhere on that line you will find water, and judging from the force with which the stick moved, and my previous experiences, I should say that you will have to sink at least 30ft., and possibly as much as 40ft. or 45ft." As the line thus marked

out was not conveniently situated I desired him to try the upper part of the land, which he did, but without result. Another short diagonal line was, however, found at the bottom corner.

Not placing much reliance upon what looked like a sort of necromancy, I yet resolved that it should be tested, and just before I left home for Southampton I sent a gang of well-sinkers to the spot. Yesterday morning I received the following telegram: "Well, 28ft., sandy bottom, water 2ft. deep." I may say that to the depth of 20ft. the soil was dry, and I had received notice two days previously that on reaching 21ft. it became moist for the first time.

These are the facts, what is the interpretation of them? It may be said that the finding of water was purely accidental, and that a well sunk in any part of the field would have been equally successful. But there is no proof of this, and the experience of other persons at Manchester and Grantham, and the extraordinary behaviour of the stick have still to be accounted for.

Of course, my own unsupported testimony as to the behaviour of the stick will not, and *ought* not to be taken as absolute proof of my statement. There were in fact three other witnesses, but I cannot produce them, at Southampton. Can any physical cause be suggested which could in any way be supposed to produce such behaviour in a forked stick? It had much the appearance of the deflection of the index in a galvanometer. If this man had a particularly sensitive constitution, and if an underground water-stream running for centuries in one direction could set up some kind of electric current, might he not actually become a galvanometer giving visible indications of that current?

Note.—The well was sunk to the depth of 31ft. and contained always 6ft. of good water.

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No. 21.

From the HON. M. E. G. FINCH HATTON, M.P., 23, Ennismore Gardens, S.W.

*February 29th, 1884.*

Hearing that J. Mullins possessed the power of finding springs of water, by means of a forked twig held between his hands, I desired him to come over to Haverholme Priory, near Sleaford, where I live.

This is what happened. First he cut a forked twig from a living tree, and held it between his hands, the centre point downwards and the two ends protruding between the fingers of each hand.

He then stooped forward and walked over the ground to be tried. Suddenly he would stop and the central point would revolve in a half circle until it pointed the reverse way. This he stated to be owing to the presence of a subterranean spring, and further that by the strength of the movement he could gauge the approximate depth.

My brother, Hon. Harold Finch Hatton, and I each took hold of one of the ends, protruding as stated above, and held them fast while the phenomenon took place, to make sure that it was not caused by a movement, voluntary or otherwise, of the man's own hand or fingers. The tendency to

twist itself, on the twig's part, was so great that, on our holding firmly on to the ends, the twig split and finally broke off.

The same thing occurred when standing on a bridge over a running stream.

Stagnant water, he states, has no effect on the twig.

Though now convinced ourselves of the man's good faith, and of the automatic—or rather independent—action of the twig, we resolved to test Mullins in various ways, of which the following is an account.

*First.*—We established it on good authority that Mullins had no previous acquaintance with Haverholme.

1. I took him on to the grass in front of the house, across which the water-supply pipe passed. There was no indication of its presence on the surface, nor did I previously mention its existence to Mullins; on crossing it, the twig moved in the manner described, and he could trace the water to right and left by its means, along the path actually taken by the pipe.

2. On our way to the kitchen garden Mullins discovered a spring on the open lawn, whose existence was unknown to me, it had been closed in so long, but was subsequently attested by an old labourer on the place who remembered it as a well, and had seen it bricked in many years before.

3. On reaching the kitchen garden I knew that a lead pipe, leading water to a tap outside the wall, crossed the gravel path at a certain spot. On crossing it the twig made no sign. I was astonished at first, till I remembered what Mullins had said about stagnant water, and that the tap was *not running*.

I sent to have it turned on, re-conducted Mullins over the ground, when the twig immediately indicated the spot.

When Mullins had passed on, I carefully marked the exact spot indicated by the twig. When he had left the garden, I said, "Now, Mullins, may we blindfold you and let you try?" He said, "Oh yes, if you don't lead me into a pond or anything of that sort." We promised.

Several sceptical persons were present who took care the blindfolding was thoroughly done.

I then re-conducted him, blindfold, to the marked spot by a different route, leaving the tap running, with the result that the stick indicated with mathematic exactness the same spot. At first he slightly overran it a foot or so, and then felt round, as it were, and seemed to be led back into the exact centre of influence by the twig.

All present considered the trial entirely conclusive of two things.

First, of the man's perfect good faith.

Secondly, that the effect produced on the twig emanated from an agency outside of himself, and appeared due to the presence of running water.

My brother, Mr. Harold Finch Hatton, is present as I write, and confirms what I say.

It is true that one of the Misses Wordsworth tried the twig, and was surprised to find that an influence of a similar nature, though not so strong, was imparted to it.

I merely give facts, without attempting to explain them.

No doubt it will be long before they are generally admitted—probably not until scientific (?) men cease to gratify their vanity by denying the existence of everything they cannot explain or account for fully.

It appears to me to be due to some occult form of magnetism, requiring a high conductive power in the operator. Mullins says that he feels something akin to an electric shock each time, and that his arms ache all night after many experiments.

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No. 31.

FROM STEPHENS AND BARSTOW, Bristol Steam Joinery Works.

25th March, 1883.

In the year 1879 we were building a house at Horsham, in Sussex, for Mr. J. Renton, of Guildford.

There were one or two wells on the property, but little or no water could be obtained from either (in fact water was very scarce indeed in the neighbourhood generally), and it was thought desirable to sink another well.

As we had known Mr. Lawrence for many years, and knew that he professed to be able to discover water, we communicated with and arranged with him to go to Horsham, hoping that if he should point out a place where we might sink, that the well would be a greater success than those already there.

Mr. Lawrence proceeded to the place, but we cannot, from personal observation, give you an account of what took place, as no member of the firm was able to accompany him (we may here state that neither of us have ever seen him use the "rod"), but we enclose you a letter from the man who was foreman upon the works at the time of Mr. Lawrence's visit (he is still in our employ, and we wrote to him upon receiving your letter), in which he fully describes how Mr. L. proceeded.

You will see by Mr. Martell's letter that he was in every way successful.

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FROM S. MARTELL, Foreman.

March 26th, 1883.

He selected one, a hazel twig, and walked about with it. On coming to a certain place he said, "Here is the spring of water," and the twig was violently jumping up and down, and he marked the place which was to be the centre of the well. He then tried the steel spring with the same result. On commencing to dig the well I was obliged to move a little on one side, as one of the new drains was in the way, and the centre of the well, as marked by him, became the *side*. On sinking down about 40ft. the spring burst out as large as a hammer handle in the *very spot* that he marked for the centre.

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FROM HENRY SHAW, Architect, 52, New Broad Street, E.C., and 39, Clapton Square, E.

March 28th, 1883.

With regard to the "Divining Rod," in 1878 I erected a house at Rudgwick for J. T. Renton, Esq. ; the site was on high ground, and the subsoil of stiff clay. During the progress of the works the contractor proceeded to

form a well close to the new building, and had bored a very considerable depth without coming to water. Messrs. Stephens and Bastow, of Bristol, were the contractors, and they advised that Mr. Lawrence should be employed to detect a spring. I had heard of him before, and was greatly interested as to his mode of procedure. He was shown where we had already bored. [Then follows a description of the process.] He said we should find water, the position being only some few yards from the original boring. We proceeded to sink at the place indicated by Mr. Lawrence, and within some 14 yards from the surface we came upon a good spring of water, and which has, I believe, continued to supply the house from that time to this. I may state that there was an old well some 100 yards from the new building, but Mr. Lawrence was not informed of this to my knowledge.

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No. 34.

From MRS. BENGOUGH, 5, Apsley Road, Clifton, Bristol.

March 7th, 1883.

I discovered I could use the "Divining Rod" in this way. A professional water-finder in this neighbourhood called on me on some business, and happened to say he was going that afternoon to find water at my cousin's new house at Henbury. A large well had been dug to a considerable depth without any sign of water, and as the house was nearly completed the matter was very important. I asked how he was going to find water, and he took from his pocket a watch-spring, and, going on to the lawn, showed me how he used it. I then tried it, and found it worked quite as much in my hands as in his. After he left I determined to go over to my cousin's house and try if I could find the water before he came over in the afternoon, so I bought a watch-spring and drove over, and tried it (before many witnesses) close to the well. I found it was most agitated at a spot about two yards from the well, and from this spot I traced a line across the garden to a field beyond, by the agitation of the watch-spring. My friends marked the place, and then we waited for the water-finder. He came and fixed exactly on my spot.

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From H. J. CRISP, Architect, Bristol.

March 24th, 1883.

Prior to sinking the well, we consulted a geologist as to the probability of finding water, and at what depth. He informed us that it was not likely that water would be found until we had sunk through the bed of the mountain limestone existing there, and which was about 150ft. deep, when we should come on a bed of clay and find an abundant supply of water. This applied to the district where this stone exists, and therefore the well could be sunk in any part with the same chances of finding water. We accordingly sunk a shaft to a depth of 150ft. through the rock, and then bored 10ft., but found no water. This may perhaps be accounted for to some extent, as by ill luck we came in contact with some faults (or saddle-backs as they are

sometimes termed) in the rock. We then called in Mr. Lawrence with his rod ; he held it over the mouth of the well and it was motionless, and Mr. Lawrence stated it was no use sinking any deeper there ; he then walked in a spiral line round the well, and when at a distance of about 20ft., the rod moved vigorously, but nowhere else near the well. With his advice we drove a level heading from the shaft at a depth of about 100ft. from the surface, towards the spot indicated by the rod, and after proceeding about 30ft. the water suddenly flowed in at the end of the heading, and the men had to leave the heading at once and get to the surface. Since this there has been a good and regular supply of water.

[These accounts are confirmed by Mr. Butterworth, the owner of the house, Mr. Lawrence and others. There is some slight discrepancy in the measurements, which, however, does not materially affect the case.]

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No. 41.

From W. G. ADEY, Builder and Timber Merchant, West Mills, Newbury.  
*April 19th, 1883.*

William Stokes has been in my employ as a carpenter and wheelwright from the year 1865, in which year I built some stables and chaise houses for the Rev. M. J. Ridley, of East Woodhay, and Stokes was on the works as a carpenter ; and, while it was in hand, Mr. Ridley wished the well that supplied the house to be opened and cleared out, but no one on the estate knew where it was, not having been opened for a number of years ; but Stokes, with his divining rod, discovered the well (although a perfect stranger to the place), and it proved to be where he predicted, under the paving in the centre of the pathway.

Altogether Stokes has been employed by me in that capacity as "water-finder" or "prophet," as he is called, in, probably, 18 or 20 different places, and I cannot say that he has failed on any one occasion, and I must confess that no one made greater ridicule of his abilities in that direction than I did, but was quite converted and made a true believer by the following circumstance.

In the year 1872, I was employed to build a mansion in this neighbourhood, and was naturally desirous to have the well as near to the scullery as possible, and directed my men to sink the well accordingly at the N.W. angle of the building ; but after they had sunk the well a few feet, Stokes went up, unknowing to me, and told my foreman that it was of no use going on with that well as we should not get water, and told them where the spring was, viz., in the N.E. corner, and that it was near the surface. My foreman asked me what he should do in the matter, and I told him not to pay any attention to such rubbish, and continue sinking the well. We did so, and at a depth of nearly 40ft. there was not the slightest appearance of coming to water. My men then threw out a hole where Stokes indicated, about 30 or 35ft. from the well, and at a depth of only 5ft. from the surface came upon a spring which kept the bricklayers and plasterers supplied all through the job, and has been used for the supply of the house to this day.

On another occasion I deviated very slightly from the course of the spring as indicated by Stokes and had to sink another well where he directed.

I could give you a list of several wells sunk under his direction, but I believe you have written to, and had replies from, several of my employers.

One bucket filled with spring water and another with rain water, placed side by side, and he will tell you, when blindfolded, which is the spring water and which rain water.

He is an abstainer, and a highly nervous, sensitive man, and I am now as great a believer in his powers as I was formerly a disbeliever.

He is going in the country to-morrow to advise in the sinking of a well on the hill for a new house I am commencing.

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No. 47.

FROM E. VAUGHAN JENKINS.

October 7th, 1882.

About 30 years ago I purchased a plot of land on a hill slope two acres in extent whereon to erect a residence of considerable value. It formed part of an estate laid out for building purposes in a suburb to Newport, Monmouthshire. The absence of waterworks necessitated the holder of each plot who intended building thereon to sink a well for his water supply. Having chosen the site for my residence, the architect fixed upon the most convenient spot for the first requisite—the well.

After the well-sinkers had reached a depth of 51ft. they decided, from the nature of the strata, &c., that it would be perfectly useless to proceed further with the sinking, as the search for water in that direction would be sure to end in failure.

A consultation of all the "knowing ones" in the matter was therefore held, with the result that, owing to the peculiar dip of the land and for various other reasons, "they did not consider there was the least possible chance of water being obtained on the plot of land anywhere." In this dilemma the foreman of the masons, a native of Devon or Cornwall—I forget which—exclaimed, "Why don't you try the divining rod? In the part of the country I come from no one would think of sinking a well without the guidance of the rod." Although quite incredulous, I replied that I should only be but too glad to have it tried if he knew any one who could use it. Upon which he said his little boy, 11 years old, possessed the power in a remarkable degree, and that if water was to be obtained on the plot he would pledge his character that his boy would find it. The lad, an honest, innocent, and nice-looking little fellow, being sent for and informed what was required of him, immediately repaired to a neighbouring hedge, and returned with a rod of blackthorn or hazel—I think the former—about 2ft. 3in. in length and of the thickness of telegraph wire. Then placing the ends of the rod between the thumb and forefinger of each hand, bending it slightly and holding it before him at a short distance from the ground, he started on his expedition, I and others following him and watching every movement closely. After going up and down, crossing and re-crossing the ground several times, but never on the same lines, the lad stopped, and, to our great surprise, we saw



the rod exhibit signs of motion, the fingers and thumbs being perfectly motionless. The motion or trembling of the rod increasing it slowly began to revolve, then at an accelerated pace, fairly twisting itself to such an extent that the lad, although he tried his best to retain it, was obliged to let it go, and it fled to some distance. The experiment being thus far successful, coupled with the respectability of the parents, the lad's transparent innocence, and the father's positive assurance that the operation might be immediately commenced with the certainty of success, the next day saw the well-sinkers in full swing on the spot indicated, and on reaching the depth of 48ft. they had the gratification of striking on a strong spring of pure and beautiful water coming in so fast as to cause them to make a hurried exit, and in a few hours the well contained a depth of 10ft. of water, rising since occasionally to 15ft., and so it now continues. The father stated that when he was a boy he possessed the same power, but entirely lost it at 16 years of age. I send you this incident for what it may be worth. To myself personally its results were most important, as it changed the position of my residence and secured me an exhaustless supply of beautiful water. I was then, and I am now, fully convinced of the total absence of any deceit or collusion and of the full integrity of the whole transaction, no fee or reward being asked for or expected, and I therefore cannot avoid entertaining the opinion that there must be "something in it," that something being dependent upon some peculiar magnetic or other condition of the human agent employed, and it may yet form one of the grand discoveries of this or some future age.

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No. 48.

FROM TOPHAM ANGUS AND Co., Contractors.

*December 28th, 1882.*

In the summer of 1874, we had instructions from the Somerset and Dorset Railway Company to sink a well at Shepton Mallet Station. We sunk to a depth of 103ft., and got no traces of water. Our chief agent sent over from Bath a man who could find water with the "Divining Rod." On arrival he went down the well, and after sundry manœuvring with the rod, he said "You will never come upon water here." He came up. After walking round the well a few feet, he came upon water which he said was boiling 40ft. below the surface. He recommended a small heading to be driven, about 40ft. from where he was standing. And he said, "I do not think the miners will have time to finish timbering before the rush of water will be upon them." Naturally the Cornwall miners, who had sunk the well, and myself thought it all humbug, but we found his statement true, for in less than 24 hours we had 63ft. of water, and we got a 10-horse power engine brought on the ground, and we could not reduce the water more than two feet in the 24 hours.

## ANNUAL BUSINESS MEETING.

The annual business meeting of the members of the Society was held at 11, Chandos Street, Cavendish Square, London, on the 18th of January. The President, Professor H. Sidgwick, was in the chair.

The balance-sheet for 1883 (which has been circulated among all the Members of the Society) was placed before the meeting. It showed that although the receipts from subscriptions had greatly increased since the preceding year, they had, even with the amount received from the sale of the Proceedings, fallen short of the expenditure by over £300. The President said that this deficiency had been fully met by the donations which had been given in aid of the Society's work. Looking at the fact of the rapid growth of the Society, which had increased its numbers from 150 at the commencement of last year to over 300 at the present time, and was still steadily increasing, he thought that this discrepancy between the income proper of the Society and its expenditure need not cause the Members any concern. He thought that, at this early stage in the development of the Society, it was far more important to make the work effective than to balance subscriptions and expenditure. Accordingly he contemplated with perfect equanimity the prospect that, although there would be a further large increase in the amount received from subscriptions, the Society would again this year have partially to subsist upon "charity." Indeed, he had the pleasure of announcing three or four additions to the Research Fund since the beginning of the year.

The following alterations in the constitution and rules, which had been agreed to by the Council during the past year, were submitted to the meeting and adopted :—

That in Rule 3 the words—"Provided that no President shall hold the office for more than three years consecutively"—be omitted.

That a new Rule, to follow Rule 8, be adopted as follows :—"The Council shall have power to elect as Corresponding Members, who shall be on the same footing as Honorary Members, persons able and willing to forward the objects of the Society."

That the following clause be introduced as the second sentence in Rule 17 :—"The names of persons for the first time proposed to be co-opted on the Council shall be brought forward at one meeting of the Council, and shall be sent round to all Members of Council previous to its next meeting, when the voting shall be by ballot, and a unanimous vote of those present shall be requisite to carry the election."

That in Rule 22 the third clause stand thus :—"In all meetings of the Council four shall be a quorum"—instead of as at present—"In all meetings of the Council five shall be a quorum."

The following gentlemen had been nominated by the Council to fill the vacancies caused by the resignation of two Members during the year, and by the retirement of four others under Rule 18 :—

EDMUND GURNEY	FRANK PODMORE	HENRY A. SMITH
EDWARD R. PEASE	E. DAWSON ROGERS	PROF. BALFOUR STEWART

No notice having been given of any other nominations, these gentlemen were declared elected.

On the motion of the Hon. Percy Wyndham, M.P., Mr. Morell Theobald, F.C.A., was elected, in accordance with Rule 28, as auditor for 1884, on behalf of the Members.