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PROCEEDINGS  
OF THE  
SOCIETY FOR PSYCHICAL RESEARCH.

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VOLUME XV.  
(CONTAINING PARTS XXXVI-XL.)

1900-1901.

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*The responsibility for both the facts and the reasonings in papers published  
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PROCEEDINGS  
OF THE  
SOCIETY FOR PSYCHICAL RESEARCH,  
PART XXXVI.

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PROCEEDINGS OF GENERAL MEETINGS.

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The 100th General Meeting of the Society was held at the Westminster Town Hall on Friday, June 23rd, 1899, at 8.30 p.m.; DR. ABRAHAM WALLACE in the chair.

MR. F. W. H. MYERS read some cases of the Cure of Warts by Suggestion, contributed by Mr. C. P. Coghill; an article by DR. A. R. WALLACE on the "Clairvoyance of Alexis Didier," which has since appeared under the title of "Extract from J<sup>s</sup> E. de Mirville's 'Des Esprits et de leurs Manifestations Fluidiques'" in the *Proceedings*, Part XXXV.; a paper by MR. F. PODMORE on "Speaking with Tongues," and a short paper by himself on "Some points connected with Mrs. Piper's Trances."

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The 101st General Meeting was held in the same place on Friday, November 17th, 1899, at 4 p.m.; the President, SIR WILLIAM CROOKES, in the chair.

A paper on "The Fire-Walk" by MR. ANDREW LANG was read by MR. F. W. H. MYERS, and is printed below.

PROFESSOR W. F. BARRETT read "A further paper on the so-called Divining Rod," which it is hoped will appear in the next Part of the *Proceedings*.

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The 102nd General Meeting was held in the same place on Friday, December 15th, 1899, at 8.30 p.m.; PROFESSOR SIDGWICK in the chair.

Some extracts from a paper by PROFESSOR J. H. HYSLOP, entitled, "Experiments in illustration of the Trance Phenomena of Mrs. Piper," were read by PROFESSOR OLIVER LODGE.

## I.

## THE FIRE WALK.

BY ANDREW LANG.

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The subject of this paper has, perhaps, nothing to do with psychical research. The phenomena described are purely physical, nor is there, apparently, much reason to suppose that they have any cause in the human mind. But, in history, they have often been associated with anecdotes of miracles, and with persons who had credit as miracle-workers. These have handled or walked through fire without any lesion or cautery. Of course, it is conceivable, barely, that in certain abnormal states of mind, men might be insensible to the action of fire. But no such state of mind would prevent fire from doing its normal work on the body. Moreover, I shall show that fire failed to cauterise the naked feet of a normal British official, whose mental condition was much as usual.

The subject occurs everywhere in history, legend, folk-lore, law and early religion, and yet nobody has thought it worth while to collect the ancient reports and to compare them with well-authenticated modern examples. In Mr. Tylor's celebrated work, *Primitive Culture*, only one or two casual allusions are made to the theme. "They built the high places of Baal, in the valley of the son of Hinnom, to cause their sons and daughters to pass through to Moloch," that is, to pass through the fire, "whether in ritual or symbolical sacrifice" (*Primitive Culture*, Vol. II., p. 281). As a supposed rite of purification the ceremony is again touched upon lightly (Vol. II., p. 429). Again, "The ancient ceremony of passing through a fire, or leaping over burning brands, has been kept up vigorously in the British Isles" (Vol. I., p. 85), namely, at the midsummer ceremonies, when it is, or was, the custom to jump over, or run through, light fires. Nobody would guess that a rite of passing deliberately, and unscathed, through ovens or furnaces yet exists in Japan, Bulgaria, the Society Islands, Fiji, Southern India, Trinidad, the Straits Settlements, the Isle of Mauritius, and, no doubt, in other regions. We must distinguish between such sportive playing with fire as prevailed recently in these isles and the more serious Fire Ceremony of Central Australia, which tests endurance, on the one hand, and the apparent contravention of a natural law, on the other. Again, we must discount the popular reply that the hand can be rapidly plunged into molten metal and



withdrawn without injury, for we do not happen to be concerned with such a brief exposure to heat. Once more, the theory of the application of some unknown chemical substance must be rejected, because, as we shall prove, there are certainly cases in which nothing of the kind is done. Moreover, science is acquainted with no substance—alum or diluted sulphuric acid, or the like—which will produce the result of preventing cauterisation. Sir William Crookes, at least, is not familiar with any such resources of science. His evidence as to fire-handling by D. D. Home is familiar, and I understand that Mr. Podmore can only explain it away by a hypothesis of a trick played in a bad light, by means of an asbestos glove or some such transparent dodge (*Studies in Psychical Research*, pp. 58-59). Perhaps he adds a little “hallucination” on the part of the spectators. But asbestos and hallucination are out of the question in the cases which I am about to quote. Home was, or feigned to be, in a state of trance when he performed with fire. The seeress of Lourdes, Bernadette, was also in religious contemplation when she permitted the flame of a candle to play through her clasped fingers (which were unscathed) for a timed quarter of an hour.<sup>1</sup> Some Indian devotees, again, aver that they “meditate” on some divine being while passing over the glowing embers, and the Nistinares of Bulgaria, who dance in the fire, are described as being in a more or less abnormal mental condition. But even this condition is absent in the well-attested Raiatean and Fijian examples, in which, also, no kind of chemical preparation is employed. Finally, where savages are concerned, the hardness of the skins of their feet is dwelt upon. But, first, the sole of a boot would be scorched in the circumstances, while their feet are not affected; next, the savages’ feet were *not* leathery (so Dr. Hocken avers); thirdly, one of the Europeans who walked through the fire at Rarotonga declares that the soles of his own feet are peculiarly tender. Thus every known physical or conjectured psychical condition of immunity fails to meet the case, and we are left wholly without an ascertained, or a good conjectural, “reason why” for the phenomena.

I shall begin with the most recent and the most authenticated cases, and work back in time, and in civilisation. Mr. Tregear, the well-known lexicographer of the Maori and the allied Mangarova languages, lately sent me the twenty-ninth number of *The Journal of the Polynesian Society*, March, 1899, Wellington, N.Z. Professors Max Müller and Sayce are Honorary Members of the Society, which studies Polynesian languages, customs and conditions. Mr. Tregear attests the upright, truth-telling character of the British official, who

<sup>1</sup> Dr. Dozous timed the “miracle.” Boissarie, *Lourdes*, p. 49.

is the narrator of his own experiment. As the journal is not widely circulated in England, I quote the whole of the brief report.

TE UMU-TI,  
OR FIRE-WALKING CEREMONY.

BY COLONEL GUDGEON,  
BRITISH RESIDENT, RAROTONGA.

In this Journal, Vol. II., p. 105, Miss Teuira Henry describes this ceremony as practised in Raiatea, of the Society Group. We have lately received from Colonel Gudgeon the following account of his experiences in walking bare-footed across the glowing hot stones of a native oven, made in Rarotonga by a man from Raiatea. Since the date of the paper quoted, it has come to light that the Maoris of New Zealand were equally acquainted with this ceremony, which was performed by their ancestors. On reading Colonel Gudgeon's account to some old chiefs of the Urewera tribe, they expressed no surprise and said that their ancestors could also perform the ceremony, though it has long gone out of practice.—EDITORS.

I must tell you that I have seen and gone through the fire ceremony of the *Umu-Ti*.

The oven was lit at about dawn on the 20th of January, and I noticed that the stones were very large, as also were the logs that had been used in the oven for heating purposes.

About 2 p.m. we went to the oven and there found the *tohunga* (a Raiatea man) getting matters ready, and I told him that, as my feet were naturally tender, the stones should be levelled down a bit. He assented to this, and evidently he had intended to do so, for shortly after, the men with very long poles that had hooks, began to level the stones flat in the oven, which was some twelve feet in diameter. He then went with his disciple and pointed to two stones that were not hot, and instructed him the reason was that they had been taken from a *marae*, or sacred place.

He then unwound two bundles, which proved to be branches of a large-leaved *Ti* (or *Dracæna*) plucked, it is said, from two of these trees standing close together, and it is said that the initiated can on such occasions see the shadow of a woman with long hair, called *te varua kino* (evil spirit), standing between the trees. The right hand branch is the first plucked, and it is said that the branches bend down to be plucked.

So much for the Shamanism, and now for the facts.

The *tohunga* (priest) and his *tauira* (pupil) walked each to the oven, and then halting, the prophet spoke a few words, and then each struck the edge of the oven with the *ti* branches. This was three times repeated, and then they walked slowly and deliberately over the two fathoms of hot stones. When this was done, the *tohunga* came to us, and his disciple handed his *ti* branch to Mr. Goodwin, at whose place the ceremony came off, and they went through the ceremony. Then the *tohunga* said to Mr. Goodwin, "I hand my *mana* (power) over to you; lead your friends across." Now, there were four Europeans—Dr. W. Craig, Dr. George Craig, Mr. Goodwin, and

myself—and I can only say that we stepped out boldly. I got across unscathed, and only one of the party was badly burned; and he, it is said, was spoken to, but, like Lot's wife, looked behind him—a thing against all rules.

I can hardly give you my sensations, but I can say this:—that I knew quite well I was walking on red hot stones and could feel the heat, yet I was not burned. I felt something resembling slight electric shocks, both at the time and afterwards, but that is all. I do not know that I should recommend every one to try it. A man must have *mana* to do it; if he has not, it will be too late when he is on the hot stone of Tama-ahi-roa.

I cannot say that I should have performed this wizard trick had I not been one of the fathers of the Polynesian Society, and bound to support the superiority of the New Zealander all over Polynesia,—indeed all over the world. I would not have missed the performance for anything.

To show you the heat of the stones, quite half-an-hour afterwards some one remarked to the priest that the stones would not be hot enough to cook the *ti*. His only answer was to throw his green branch on the oven, and in a quarter of a minute it was blazing. As I have eaten a fair share of the *ti* cooked in the oven, I am in a position to say that it was hot enough to cook it well.

I walked with bare feet, and after we had done so, about 200 Maoris followed. No one, so far as I saw, went through with boots on. I did not walk quickly across the oven, but with deliberation, because I feared that I should tread on a sharp point of the stones and fall. My feet also were very tender. I did not mention the fact, but my impression as I crossed the oven was that the skin would all peel off my feet. Yet all I really felt when the task was accomplished was a tingling sensation not unlike slight electric shocks on the soles of my feet, and this continued for seven hours or more. The really funny thing is that, though the stones were hot enough an hour afterwards to burn up green branches of the *ti*, the very tender skin of my feet was not even hardened by the fire.

Many of the Maoris thought they were burned, but they were not, at any rate not severely.

Do not suppose that the man who directed this business was an old *tohunga*. He is a young man, but of the Raiatea family, who are hereditary fire-walkers.

I can only tell you it is *mana*—*mana tangata* and *mana atua*.

On this report a few remarks may be offered. (1) No preparation of any chemical, herbal, or other sort was applied to the Europeans, at least. (2) "The handing over the *mana*" (or power) was practised by Home, sometimes successfully (it is alleged) as when Mr. S. C. Hall's scalp and white locks were unharmed by a red-hot coal: sometimes unsuccessfully. A clergyman of my acquaintance still bears the blister caused when he accepted a red-hot coal from the hand of Home, as he informs me by letter. (3) The "walk" was shorter than seems common: only twelve feet, four paces. (4) A friend of Colonel Gudgeon's was badly burned, and the reason assigned was a good



folk-lore reason, since the days of Lot's wife, of Theocritus, and of Virgil: he looked behind. (5) The feeling as if of "slight electric shocks" is worthy of notice. (6) Colonel Gudgeon clearly believes that a man without *mana* had better not try, and by *mana*, here, he probably means "nerve." As we can hardly suppose, in spite of Home, that *mana*, in a supernormal sense, can be "handed over" by one man to another, Colonel Gudgeon's experience seems equally to baffle every theory of "how it is done." Perhaps we can all do it. Members of the Society may make their own experiments. Perhaps Colonel Gudgeon faced fire in a manner so unusual as a result of Dr. Hocken's description of the Fijian rite at Mbenga, an isle 20 miles south of Suva. This account was published in the *Transactions of the New Zealand Institute*, Vol. XXXI., 1898, having been read before the Otago Institute on May 10th, 1898, and is here reprinted in full as follows:—

#### AN ACCOUNT OF THE FIJI FIRE CEREMONY.

By DR. T. M. HOCKEN, F.L.S.

Amongst the many incidents witnessed during a recent visit to the tropical islands of Fiji, probably none exceeded in wonder and interest that of which I propose to give some account this evening, and to which may be applied the designation of "fire ceremony." It is called by the natives "*vilavilareco*." In this remarkable ceremony a number of almost nude Fijians walk quickly and unharmed across and among white-hot stones, which form the pavement of a huge native oven—termed "*loro*"—in which shortly afterwards are cooked the succulent sugary roots and pith of the *Cordyline terminalis*, one of the cabbage trees, known to the Maoris as the "*ti*," and to the Fijians as the "*masawe*." This wonderful power of fire-walking is now not only very rarely exercised, but, at least as regards Fiji, is confined to a small clan or family—the *Na Ivilankata*—resident on Bega (=Mbenga), an island of the group, lying somewhat south of Suva, and twenty miles from that capital.

A small remnant of the priestly order at Raiatea, one of the Society Islands, is yet able to utter the preparatory incantation, and afterwards to walk through the fire.

It exists also in other parts of the world, as in parts of India, the Straits Settlements, West India Islands, and elsewhere. Very interesting accounts of the ceremony as seen at Raiatea and at Mbenga are to be found in the second and third volumes of the *Journal of the Polynesian Society*, and in Basil Thomson's charming *South Sea Yarns*. These descriptions filled our small party of three—my wife, Dr. Colquhoun, and myself—with the desire to witness it for ourselves, and, if possible, to give some explanation of what was apparently an inexplicable mystery. Our desires were perfectly realised.

The Hon. Mr. A. M. Duncan, a member of the Legislative Council of Fiji, and agent at Suva of the Union Steamship Company, to whom I carried a letter of introduction from Mr. James Mills, the managing director of that company, was most courteous and obliging, and promised

his best efforts in the matter. His energy and ready response succeeded, with the result that a large party from Suva enjoyed such a day as each one must have marked with a red letter.

It was necessary to give the natives three days in which to make their preparations—constructing the oven and paving it with stones, which then required heating for thirty-six or forty-eight hours at least with fierce fires fed with logs and branches. They had also to gather their stores of food to form the foundation of the huge feast whose preparation was to succeed the mystic ceremony. During these three days we lost no opportunity of collecting from former witnesses of the ceremony whatever information or explanation they could afford, but with no very satisfactory result: the facts were undisputed, but the explanations quite insufficient. Some thought that the chief actors rubbed their bodies with a secret preparation which rendered them fireproof; others that life-long friction on the hard hot rocks, coral-reefs, and sands had so thickened and indurated the foot-sole that it could defy fire; but all agreed as to the *bona fides* of the exhibition. The incident recounted in the *Polynesian Journal* was also confirmed—where Lady Thurston threw her handkerchief upon the shoulder of one of the actors, and, though it remained there but a few seconds before being picked off by means of a long stick, it was greatly scorched.<sup>1</sup>

The story or legend attached to this weird gift of fire-walking was told us, with some variation, by two or three different people, and it is mainly as follows: A far-distant ancestor of the present inheritors of this power was walking one day when he espied an eel, which he caught, and was about to kill. The eel squeaked out, and said, “Oh! Tui Na Galita (= Eng-Galita), do not kill me; spare me. I am a god, and I will make you so strong in war that none shall withstand you.” “Oh, but,” replied Na Galita, “I am already stronger in war than any one else, and I fear no one.” “Well, then,” said the eel, “I will make your canoe the fastest to sail on these seas, and none shall come up with it.” “But,” replied Na Galita, “as it is, none can pass my canoe.” “Well, then,” rejoined the eel, “I will make you a great favourite among women, so that all will fall in love with you.” “Not so,” said Na Galita, “I have one wife, of whom I am very fond, and I desire no other.” The poor eel then made other offers, which were also rejected, and his chances of life were fading fast when he made a final effort. “Oh, Na Galita, if you will spare me, I will so cause it that you and your descendants shall henceforth walk through the *masawe* oven unharmed.” “Good,” said Na Galita, “now I will let you go.” This story varies somewhat from that told in the *Polynesian Journal*.<sup>2</sup>

The eventful morning was blazingly hot and brilliant, and the vivid-blue sky was without a cloud as we steamed down towards Mbenga in the s.s. “Hauroto.” Mr. Vaughan, an eminent inhabitant of Suva, who has charge of the Meteorological Department there, was of our party, and carried the thermometer. This was the most suitable for our purpose procurable; it was in a strong japanned-tin casing, and registered 400° Fahr. We had also three amateur photographers.

<sup>1</sup> I have not seen this account.—A. L.

<sup>2</sup> See also Mr. Thomson's *South Sea Yarns*.—A. L.

Owing to the numerous coral-reefs and shallows, we finally transhipped into the "Maori," a steamer of much less draught. Approaching the silent verdure-clad islet, with its narrow beach of white coral sand, we saw a thin blue haze of smoke curling above the lofty cocoanut trees at a little distance in the interior, which sufficiently localised the mysterious spot. We now took the ship's boat, and soon, stepping ashore, made our way through a narrow pathway in the dense bush until we came to an open space cleared from the forest, in the midst of which was the great *lovo*, or oven.

A remarkable and never-to-be-forgotten scene now presented itself. There were hundreds of Fijians, dressed according to the rules of nature and their own art—that is, they were lightly garlanded here and there with their fantastic *likulikus* of grass, ornamented with brilliant scarlet and yellow hibiscus flowers and streamers of the delicate ribbonwood. These hung in airy profusion from their necks and around their waists, showing off to advantage their lovely brown glossy skins. In addition, many wore clean white cotton *sulus*, or pendant loin-cloths. All were excited, moving hither and thither in wild confusion, and making the forest ring again with their noisy hilarity. Some climbed the lofty cocoa-palms, hand over hand, foot over foot, with all the dexterity of monkeys. The top reached, and shrouded amongst the feathery leaves, they poured down a shower of nuts for the refreshment of their guests.

The celerity with which they opened the nuts was something astonishing, and afforded an example, too, as to the mode of using stone implements. A stout, strong stick, 3 ft. long, and sharpened at both ends, was driven into the ground, and a few smart strokes upon it soon tore from the nut its outer thick covering. The upper part of the shell was then broken off by means of a long sharp-edged stone as cleanly and regularly as the lid of an egg is removed with a knife, and then was disclosed a pint of delicious milk, a most welcome beverage on that overpoweringly hot day.

The great oven lay before us, pouring forth its torrents of heat from huge embers which were still burning fiercely on the underlying stones. These were indeed melting moments for the spectators. The pitiless noon-tide sun, and the no less pitiless oven-heat, both pent up in the deep well-like forest clearing, reduced us to a state of solution from which there was no escape. Despite this the photographers took up their stations, and others of us proceeded to make our observations. The *lovo*, or oven, was circular, with a diameter of 25 ft. or 30 ft.; its greatest depth was perhaps 8 ft., its general shape that of a saucer, with sloping sides and a flattish bottom, the latter being filled with the white-hot stones. Near the margin of the oven, and on its windward side, the thermometer marked 114°.

Suddenly, and as if Pandemonium had been let loose, the air was filled with savage yells; a throng of natives surrounded the oven, and in a most ingenious and effective way proceeded to drag out the smouldering unburnt logs and cast them some distance away. Large loops of incombustible lianas attached to long poles were dexterously thrown over the burning trunks, much after the manner of the head-hunters of New Guinea when securing their human prey. A twist or two round of the loop securely entangled the logs, which were then dragged out by the united efforts of scores of natives, who all the while were shouting out some wild rhythmical



song. This accomplished, the stones at the bottom of the oven were disclosed, with here and there flame flickering and forking up through the interstices. The diameter of the area occupied by those stones was about 10 ft., but this was speedily increased to a spread of 15 ft. or more by a second ingenious method. The natives thrust their long poles, which were of the unconsumable wi-tree (*Spondias dulcis*), between the stones at intervals of perhaps 1 ft. A long rope-like liana—*wa*—previously placed underneath the poles, and 1 ft. or 2 ft. from their extremities, was now dragged by scores of lusty savages, with the effect of spreading and levelling the stones. This done, our thermometer was suspended by a simple device over the centre of the stones, and about 5 ft. or 6 ft. above them; but it had to be withdrawn almost immediately, as the solder began to melt and drop, and the instrument to be destroyed. It, however, registered 282° Fahr., and it is certain that had not this accident occurred, the range of 400° would have been exceeded, and the thermometer burst.

During all these wild scenes we had seen nothing of the main actors—of the descendants of Na Galita. Doubtless to give more impressive effect, they had been hiding in the forest depths until the signal should be given and their own supreme moment arrive. And now they came on, seven or eight in number, amidst the vociferous yells of those around. The margin reached, they steadily descended the oven slope in single file, and walked, as I think, leisurely, but, as others of our party think, quickly, across and around the stones, leaving the oven at the point of entrance. The leader, who was longest in the oven, was a second or two under half a minute therein. Almost immediately heaps of the soft and succulent leaves of the hibiscus, which had been gathered for the purpose, were thrown into the oven, which was thus immediately filled with clouds of hissing steam. Upon the leaves and within the steam the natives, who had returned, sat or stood pressing them down in preparation for cooking the various viands which were to afford them a sumptuous feast that evening or on the morrow.

But for us the most interesting part of the drama was over, and it only remained to review observations and draw conclusions. Just before the great event of the day, I gained permission to examine one or two of the fire-walkers prior to their descent into the oven. This was granted without the least hesitation by the principal native Magistrate of the Rewa district, N'Dabea by name, but generally known as Jonathan. This native is of great intelligence and influence, is a member of the Na Galita Clan, and has himself at various times walked through the fire. On this occasion he took no other part in the ceremony than that of watching or superintending it. The two men thus sent forward for examination disclosed no peculiar feature whatever. As to dress, they were slightly garlanded round the neck and the waist; the pulse was unaffected, and the skin, legs, and feet were free from any apparent application. I assured myself of this by touch, smell, and taste, not hesitating to apply my tongue as a corroborative. The foot-soles were comparatively soft and flexible—by no means leathery and insensible. Thus the two Suvan theories were disposed of. This careful examination was repeated immediately after egress from the oven, and with the same result. To use the language of Scripture, “No smell of fire had passed upon them.” No incantations or other religious ceremonial were

observed. Though these were formerly practised, they have gradually fallen into disuse since the introduction of Christianity. I did not succeed in procuring the old incantation formula; doubtless it was similar to that of the old Raiatean ceremony, which is given in the second volume of the *Polynesian Society's Journal*, p. 106.

Whilst walking through the fire, Dr. Colquhoun thought the countenances of the fire-walkers betrayed some anxiety. I saw none of this; nor was it apparent to me at either examination. The stones, which were basaltic, must have been white-hot, but due to the brilliance of the day this was not visible.

Various natives, being interrogated for an explanation, replied, with a shrug, "They can do this wonderful thing; we cannot. You have seen it; we have seen it." Whilst thus unable to suggest any explanation or theory, I am absolutely certain as to the truth of the facts and the *bona fides* of the actors. A feature is that, wherever this power is found, it is possessed by but a limited few. I was assured, too, that any person holding the hand of one of the fire-walkers could himself pass through the oven unharmed. This the natives positively assert.

My friend Mr. Walter Carew, for thirty years a Resident Commissioner and Stipendiary Magistrate in Fiji, has frequently conversed with Jonathan (referred to above), who, whilst withholding no explanation, can give none. He says, "I can do it, but I do not know how it is done"; and, further, that at the time he does not experience any heat or other sensation.

Does any psychical condition explain these facts, as suggested in Lang's *Modern Mythology*? I certainly did not observe any appearance of trance or other mental condition. In connection with this Mr. Carew thinks that intense faith is the explanation, and that if this were upset, the descendants of Na Galita would be no longer charmed. But it is difficult to see how any mental state can prevent the action of physical law. Hypnotism and anæsthetics may produce insensibility to pain, but do not interfere with the cautery.

Many of the so-called fire miracles are remarkable indeed, but are readily explained, and by no means come within the present category. Such, for instance, as plunging the hand, which is protected by the interposed film of perspiration assuming the globular state of water, into boiling lead. Similarly, many conjuring feats. At the beginning of this century an Italian—Lionetti—performed remarkable experiments—rubbed a bar of red-hot iron over his arms, legs, and hair, and held it between his teeth; he also drank boiling oil. Dr. Sementini, of Naples, carefully examined these experiments, and experimented himself until he surpassed the fire-proof qualities of his suggestor. He found that frequent friction with sulphurous acid rendered him insensible to red-hot iron; a solution of alum did the same. A layer of powdered sugar covered with soap made his tongue insensible to heat. In these and similar instances, however, an explanation, though probably not a very sufficient one, has been given, but in that forming the subject of this paper no solution has been offered. Lang's chapter on the "Fire Walk" should be consulted; his account of the Bulgarian Nistinares is

as wonderful and inexplicable as anything here recited. The whole subject requires thorough scientific examination.

My next case occurs among a civilised race, the Japanese, and is vouched for by Mr. Lafcadio Hearn, an American writer, whose book I have not at hand, and by Colonel Andrew Haggard (*The Field*, May 20th, 1899, p. 724). Colonel Haggard saw the fire-walk done in Tokio, on April 9th, 1899. The fire was six yards long by six feet wide. The rite was in honour of a mountain god. Ablutions in cold water were made by the performers, and Colonel Haggard was told by one young lady that she had not only done the fire-walk, but had been "able to sit for a long time, in winter, immersed in ice-cold water, without feeling the cold in the least." After some waving of wands and sprinkling of salt, people of all ages walked through, not glowing stones in this case, but "red-hot charcoal." "I examined their feet afterwards, they were quite soft, and not a trace of fire upon them." Colonel Haggard says that the rite is "a very unusual thing" in Japan; many of the Japanese living in Tokio had never heard of it before. Colonel Haggard was unable to get any clear answer as to why the rite is performed. The priest talked something about a good God who had power over the bad element of fire. It is not clear how, the rite being so unusual, two Japanese ladies told Colonel Haggard that they had "frequently gone through the fiery ordeal."

If any one is anxious to know the particulars of the rite as practised in the isle Mauritius, he may communicate with our police officials there, who annually superintend the performance. Coolies from southern India do just what is done by Japanese and Fijians. Our administration, however, does not permit women to pass through the fire.

After giving these recent examples in Mauritius, Japan, Rarotonga, and an isle of the Fijian group, I am obliged to fall back on the evidence already set forth in Chapter XII. of my book, *Modern Mythology* (1897). The Bulgarian practice I take from the *Recueil de Folk-lore, de Littérature et de Science*, edited by the Bulgarian Minister of Public Instruction, with the aid of Drs. Schischmanof (whom I know personally) and Mastov. In a private letter, Dr. Schischmanof hints at *extase religieuse*, as in the self-mutilations of Dervishes and Fakirs. Their performances are extraordinary enough, but there was no religious ecstasy in the little Japanese boy of six, whom Colonel Haggard saw pass through the fire, none in Colonel Gudgeon, none in the Fijians observed by Dr. Hocken. The fire-walkers in Bulgaria are called *Nistinares*, and the faculty is regarded as *hereditary*. We find the same opinion in Fiji, in ancient Italy, and in the Spain of the last century. In Spain the fire-walkers were employed to help to put



out fires. The story is given in the essay on the last Earl Marischal, in my *Companions of Pickle* (p. 24), and is derived from d'Alembert's account of the Earl: "There is a family or caste in Spain, who, from father to son, have the power of going into the flames without being burned, and who, by dint of charms permitted by the Inquisition, can extinguish fires." The Duchess of Medina Sidonia thought this a proof of the verity of the Catholic faith, and, wishing to convert the Earl, asked him to view the performance. But he insisted on lighting the fire himself, and to that the Spaniards would not consent, the Earl being a heretic.

To return to the Bulgarian Nistinares, they dance in the fire on May 21st, the feast of Ss. Helena and Constantine. Great fires of scores of cart-loads of dry wood are made. On the embers of these the Nistinares (who turn blue in the face) dance and utter prophecies, afterwards placing their feet in the muddy ground where libations of water have been poured forth. The report says nothing as to the state of their feet. The Nistinare begins to feel the effect of the fire after his face has resumed its wonted colour and expression.

As for India, I may cite Mr. Stokes, in *The Indian Antiquary*, (II., 190), Dr. Oppert, in his *Original Inhabitants of India*, (p. 480), and Mr. Crookes, in *Introduction to Popular Religion and Folk-lore in Northern India*, (p. 10). Mr. Stokes uses evidence from an inquest on a boy that fell into the fire and died of his injuries, at Periyângridi. The fire-pit was 27 ft. long by  $7\frac{1}{2}$  ft. broad, and a span in depth. Thirteen persons walked through. Mr. Stokes did not witness the performance (which is forbidden by our law), but explains that the fire "would hardly injure the tough skin of the sole of a labourer's feet." Yet it killed a boy!

The incredulous say that the fire-walkers smear their feet with oil from the fat of the green frog. Dr. Oppert admitting that "the heat is unbearable in the neighbourhood of the ditch," says that the walkers "as a rule do not do themselves much harm." This is vague. Equally vague is the reference to rumours about "a certain preservative ointment."

In Trinidad, British West Indies, Mr. Henry R. St. Clair, writing to me, describes (September 14th, 1896) the feat as performed by Indian coolie immigrants. He personally witnessed the rite, which was like that described to me by Mr. Stephen Ponder. In both cases, the performers were Klings. The case witnessed by Mr. Ponder took place in the Straits Settlements, Province Wellesley. The trench was about 20 yards long by 6 ft. wide and 2 ft. deep. A pyre of wood, 4 or 5 ft. high, was lighted at noon; by 4 p.m. it was a bed of red-hot embers. The men, who with long rakes smoothed the ashes, could not stand the heat "for more than a minute at a time." A little way from the end

of the trench was a hole full of water. Six coolies walked the whole length, and thence into the water. "Not one of them showed the least sign of injury." They had been prepared by a "devil-doctor," not a Brahmin. On a later occasion Mr. Ponder heard that one of them fell "and was terribly burnt."

In these cases, Trinidad (and Mauritius) and the Straits Settlements, the performers are South Indian coolies. In all cases there were multitudes of European spectators, except in Mauritius, where, I learn, Europeans usually take no interest in the doings of the heathen.

Turning to Tonga, we have the account of Miss Teuira Henry (*Polynesian Journal*, Vol. II., No. 2, pp. 105-108). The sister and sister's child of Miss Henry have walked over the red-hot stones, as in the Rarotonga and Fijian cases. The ovens are 30 ft. in diameter. The performance was photographed by Lieutenant Morné, of the French Navy, and the original photograph was sent to the Editor of the *Polynesian Journal*, with a copy from it by Mr. Barnfield, of Honolulu. The ceremony, preparatory to cooking the *ti* plant, is religious, and the archaic hymn sung is full of obsolete words. Mr. Hastwell, of San Francisco, published a tract, which I have not seen, on the Raiatean rite, witnessed by himself. The stones were "heated to a red and white heat." The natives "walked leisurely across" five times; "there was not even the smell of fire on their garments" (cited in the *Polynesian Journal*, Vol. II., No. 3). There is corroborative evidence from Mr. N. J. Tone, from Province Wellesley, Straits Settlements, in the *Polynesian Journal*, II., 3, 193. He did not see the rite, arriving too late, but he saw the fire-pit, and examined the naked feet of the walkers. They were uninjured. Mr. Tone's evidence is an extract from his diary.

As to Fiji there are various accounts. The best is that of Mr. Basil Thomson, son of the late Archbishop of York. Mr. Thomson was an official in Fiji, and is a well-known anthropologist. His sketch, in his *South Sea Yarns* (p. 195, *et seq.*), is too long for quotation. The rite is done yearly, before cooking the *masûve* (a dracæna) in the oven through which the clan Na Ivilankata walk. "The pit was filled with a white-hot mass, shooting out little tongues of white flame." "The bottom of the pit was covered with an even layer of hot stones . . . the tongues of flame played continually among them." The walkers planted "their feet squarely and firmly on each stone." Mr. Thomson closely examined the feet of four or five of the natives when they emerged. "They were cool and showed no trace of scorching, nor were their anklets of dried tree-fern burnt," though "dried tree-fern is as combustible as tinder." "The instep is covered with skin no thicker than our own, and we saw the men plant



their insteps fairly on the stone." A large stone was hooked out of the pit before the men entered, and one of the party dropped a pocket-handkerchief upon the stone "when the first man leapt into the oven and snatched what remained of it up as the last left the stones." Every fold that touched the stone was charred. Mr. Thomson kindly showed me the handkerchief. He also showed me a rather blurred photograph of the strange scene. It has been rudely reproduced in the *Folk Lore Journal*, September, 1895.

Such is part of the modern evidence; for the ancient, see *Æneid*, XI., 784, *et seq.*; Servius on the passage; Pliny, *Hist. Nat.*, VII., 2; Silius Italicus, V., 175. This evidence refers to the Hirpi, of Mount Soracte, a class exempted from military service by the Roman Government, because, as Virgil makes Aruns say, "strong in faith we walk through the midst of the fire, and press our footsteps in the glowing mass." The Hirpi, or Wolves, were probably a Totem group, like the Wolf Totem of Tonkaway Red Indians; they had, like the Tonkaway, a rite in which they were told to "behave like wolves." (Serv., *Æneid*, VII., 800). The goddess propitiated in their fire-walk was Feronia, a fire-goddess (Max Müller), or a lightning goddess (Kuhn), or a corn goddess (Mannhardt). Each of these scholars bases his opinion on etymology. You see that scholars differ!

I have merely given evidence for the antiquity, wide diffusion, and actual practice of this extraordinary rite. Neither physical nor anthropological science has even glanced at it (except in Dr. Hocken's case), perhaps because the facts are obviously impossible. I ought to make an exception for Sir William Crookes, but he, doubtless, was hallucinated, or gulled by the use of asbestos, or both. Perhaps Mr. Podmore can apply these explanations to the spectators whom I have cited. For my part, I remain without a theory, like all the European observers whom I have quoted. But, in my humble opinion, all the usual theories, whether of collective hallucination (photographic cameras being hallucinated), of psychical causes, of chemical application, of leathery skin on the soles of the feet, and so on, are inadequate. There remains "suggestion." Any hypnotist, with his patient's permission (in writing and witnessed) may try the experiment.

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Since this paper was written I have seen an article, "Les Dompteurs du Feu," on the same topic, by Dr. Th. Pascal (*Annales des Sciences Psychiques*, July—August, 1899). The first part of the essay is an extract from the *Revue Théosophique Française*. No date is given, but the rite described was viewed at Benares, on October 26th, 1898. I am unable to understand whether Dr. Pascal is himself the spectator,

and narrator, of the *Revue Théosophique*, or whether he quotes (he uses marks of quotation) some other writer. The phenomena were of the usual kind, and the writer, examining the feet of two of the performers, found the skin of the soles fine and intact. In four cases, in which the performers had entered the fire after the procession—with the Master of the Ceremonies and two excited persons, who split coconuts with swords—had gone, there were slight cauterisations, healed two days later. The author of this passage speaks of a Brahmin (apparently “The Master of the Ceremonies”) who observed to Mr. Govinda Das, “that the control of the fire was not so complete as usual, because the images of the sanctuary had been touched by Mahomedans and others in the crowd.”

The second case, not given with marks of quotation, occurred in the park of Maharajah Tagore, on December 7th, 1898. “A Frenchman, the son of Dr. Javal of Paris, was present.” The narrator, “*nous*,” was also present and went up, after the rite, to venture his hand in the furnace. He was warned that the Brahmin had left, ten minutes before, and that “the fire had recovered its activity.” The Maharajah, however, caused the ceremony to be repeated, and some minutes after, all was ready. The narrator then traversed the fire, barefoot, at *un petit trot*, “a little less than two paces a second.” As 100 yards can be run in ten seconds, this trot was remarkably slow. He felt in paces one and two a sensation of burning, in the five following paces a sensation of intense heat. There were three small brown marks on his feet, which formed blisters, but did not interfere with walking, and healed “in some days.” He now learned that the Brahmin’s *premier aide* did the ceremony not quite successfully. He is convinced that, but for the ceremony, he would have been seriously injured.

The third case was at Benares, in February, 1899. Three Hindoos collided and fell in : neither they nor their clothes were burned.

The author clearly regards the performers of the ceremony as able “to tame considerably the destructive energy of the fire.” This, of course, is the theory of the savage devotees. The ceremony was only a procession of sacred images carried in a glazed sanctuary, and words, not known to the spectator, were uttered by the Brahmin. Holy water was sprinkled, and a cocoa-nut was thrown into the oven. As has been said, incantations are pronounced in Fiji and elsewhere. To the anthropologist the close resemblance of the rites in Polynesia and India is an interesting circumstance.<sup>1</sup>

The subject seems to me to deserve further examination by medical experts.

<sup>1</sup> In the *World Wide Magazine* (December, 1899), a Japanese lady describes the performance witnessed by Colonel Haggard, already cited.



## II.

DISCUSSION OF THE TRANCE PHENOMENA OF  
MRS. PIPER.

## II.—BY MRS. HENRY SIDGWICK.

It is scarcely necessary to remind readers of the *Proceedings* of what the evidence concerning Mrs. Piper's supernormal powers consists; but as in this paper I propose to add to the discussion of it without adding to the evidence itself, it may be as well briefly to describe what my *data* are. The first paper about Mrs. Piper appeared in 1890, in *Proceedings*, Vol. VI. It is entitled, "A Record of Certain Phenomena of Trance" and consists of an introduction by Mr. Myers, an account of sittings with Mrs. Piper in England by Professor Oliver Lodge, an account of other sittings with her in England by Dr. Walter Leaf and a brief account of his own experiences with Mrs. Piper by Professor William James; the whole extending to 223 pages. The next contribution to the subject was "A Record of Certain Phenomena of Trance," by Dr. Richard Hodgson, which appeared in *Proceedings*, Vol. VIII., in 1892, and which is a paper of 168 pages, describing and discussing sittings with Mrs. Piper in America. This was followed in 1898 (*Proceedings*, Vol. XIII.), by "A Further Record of Certain Phenomena of Trance," by Dr. Hodgson, extending to 298 pages, and this again by a paper by Professor W. Romaine Newbold, entitled, "A Further Record of Certain Phenomena of Trance, Part II.," occupying 44 pages of *Proceedings*, Vol. XIV. To these may be added a "Discussion of the Trance Phenomena of Mrs. Piper," by Mr. F. Podmore, in the same volume, which,—though, like the present paper, it does not add to the evidence,—throws interesting light on it. Besides the above published papers, I have through the kindness of Dr. Hodgson had access to certain unpublished notes of sittings held previous to 1898, of which I have made some use in what follows. Further evidence has been accumulating since the publication of Professor Newbold's paper, and both Dr. Hodgson and Professor Hyslop have, I understand, important further light to throw on the subject; but of this I know nothing at present in detail.

So much for the *data*; I must add a few words as to the assumptions I shall make regarding the origin of the information given by Mrs. Piper. In the first place, I accept the conclusion arrived at, as Professor Newbold says, (*Proceedings*, Vol. XIV., p. 7), by "every one, so far as I know, who has studied the case at any length, that it



was not consciously got by Mrs. Piper during waking life and then fraudulently palmed off on the sitter as supernatural"; or, as Dr. Leaf puts it: (*Proceedings*, Vol. VI., p. 559) "as to the first and most obvious question, whether she consciously acquires knowledge with regard to her sitters, with the intention of deceiving, I may say most positively that I regard such a supposition as entirely untenable." Further, I do not believe that the information as a whole is drawn by normal means from the mind of the sitter; I agree with Dr. Leaf that "the more I consider the whole of the evidence, the more I remain convinced that it gives proof of a real supernormal power, subject, however, under conditions at which we can hardly even guess, to periods of temporary eclipse," (*Proceedings*, Vol. VI., p. 561); or, as Professor Lodge says, (*Proceedings*, Vol. VI., p. 443), "concerning the particular means by which she acquires the different kinds of information, . . . I can only say with certainty that it is by none of the ordinary methods known to Physical Science."

What explanation then can we propose for the phenomena? In discussing this question, I shall assume that telepathy—in the sense of the impression of one living mind by another otherwise than through the recognised channels of sense—may be taken as provisionally established by the evidence collected by our Society and other investigators. This being granted, I agree with Professor Lodge (*op. cit.*, p. 451) "that thought-transference is the most common-place explanation to which it is possible to appeal" in the case of Mrs. Piper. But in Dr. Hodgson's last paper he endeavoured to carry us a momentous step further. He concluded that the hypothesis that we are—in some instances, at least—in communication with the spirits of the dead through the entranced Mrs. Piper is now practically proved. This seems to me to be too tremendous a conclusion to base on observation of a single medium; even such prolonged observation as Mrs. Piper has submitted to. I fully admit, however, that there is very considerable evidence for it, and I propose to adopt it as a working hypothesis for the purposes of the present paper.

Accordingly, the question I wish to argue is a subsidiary one, which arises when this hypothesis is accepted. Granting communication from the dead, I wish to argue for Telepathy *versus* Possession as the mode of communication,—in an extended sense of the word Telepathy, which I must begin by explaining.

So far, though a good deal of evidence has been collected for the occurrence of telepathy between living minds both spontaneously and experimentally, it has not been of much use to formulate hypotheses as to what telepathic communication consists in and how it occurs, because we have not been able to get the phenomenon sufficiently under control to give us a chance of verifying hypotheses about it.

But it may safely, I think, be assumed that, granting the existence of telepathy, the explanation of it must lie in one of two lines. It may be a physical process—a communication between brain and brain analogous to light or electrical induction or gravitation. In this case it would reveal to us a hitherto unknown sense, an unknown power of the human body. Or it may be a spiritual process,—a communication between mind and mind in which the physical world is not concerned ; any cerebral change in the percipient being a consequence of it and not a part of its cause. In this case it would tend to establish scientifically the existence of mind apart from body, with the further chain of probabilities that the mind continues to exist when the body is dead, that it continues able to carry on telepathic communication with minds in the body, and, indeed, that telepathy is of the nature of the communication of disembodied spirits with each other. Conversely, if we could prove the existence of the dead and their power of communicating with the living, we should make it probable that telepathy between the living was a non-physical process.

This intimate connection between the possibility of communication with the dead and a theory of telepathy makes it, I think, convenient, when we are discussing the evidence for the former, to use the word *Telepathy* in a sense wide enough to include any communication between mind and mind otherwise than through the recognised channels of sense, whether the minds communicating be in the body or not, and it is in this sense that I here use it.

The thesis then that I wish to maintain is this : Granting, for the sake of argument, that the evidence concerning Mrs. Piper proves, not only that there is telepathic communication between her trance personality and living persons,<sup>1</sup> but that knowledge is in some way derived from those who are dead, we have still no sufficient reason to think that the intelligence actually communicating by voice or writing with the sitter is any other than Mrs. Piper herself. Call it Mrs. Piper's trance personality, dream personality, secondary self, subliminal self, or what you will, but it is still Mrs. Piper ;—Mrs. Piper in communication of a supernormal but partial and uncertain kind with other minds—minds of the living and of the dead—and conveying to the sitter by voice and writing the information thus obtained, mixed up with much that has been acquired or imagined in a normal way.

I am, of course, aware that, compared with the great question whether communication with the dead takes place at all, the question whether it reaches the sitter's waking consciousness through Mrs. Piper's intelligence or directly is comparatively a small one. Nevertheless, for those who answer the first question in the affirmative, it is

<sup>1</sup>See *Proceedings S.P.R.*, Vol. XIII., p. 373, for Dr. Hodgson's view as to this.

important to know as far as possible how the communication occurs, and for those to whom the first question is still an open one, the second may have an important bearing on its solution ; for the facility with which we accept new facts necessarily depends much on the coherence or incoherence of the theory into which they have to fit. At any rate, the two questions are distinct, and in considering the bewildering mass of Piper evidence it cannot but be advantageous to look at it from different points of view and to examine each question in detail, and as far as may be by itself ; and I think that Dr. Hodgson, in his able analysis of the Piper evidence, has hardly done justice to or discussed sufficiently the hypothesis that in all cases, and however true it may be that there is really communication between the living and the dead, the intelligence communicating directly with the sitter, through Mrs. Piper's organism, is Mrs. Piper.

The alternative hypothesis to which Dr. Hodgson inclines, and which, even when not explicitly stated, seems constantly to be assumed in his summary of the evidence for communication from the dead, is that spirits other than Mrs. Piper's use her organism when she is entranced, and that in particular the spirits of certain dead persons known to certain sitters speak to them directly with her voice or write for them directly with her hand. This is the hypothesis I wish to oppose. It is the obvious alternative to the hypothesis which I am about to advocate, again saying emphatically that I only hold it provisionally and not as a final conclusion. To pass a final judgment on the whole complicated subject of Mrs. Piper's trance utterances, to decide what we can regard as proved by them, would, I think, be premature ; and to pass judgment on parts of it without regard to the whole, and without further experiment with other persons more or less similarly endowed, would be rash.

The fact with which we have to start,—and which, *prima facie*, gives plausibility to the supposition that when Mrs. Piper is in trance the intelligence communicating through her is not her own,—is that it invariably says it is some one else, and frequently acts a part unlike Mrs. Piper's ordinary personality with very considerable consistency. My own experience has been solely with the Phinuit control, and those sittings in which I have taken part as sitter (as distinguished from reporter) have been bad—that is, have afforded no indication whatever of supernormal knowledge. Nevertheless I felt as others do, not only that the Phinuit personality did not confuse itself with Mrs. Piper's normal self, but that the sitter was under no temptation so to confuse it. Conversation with it differed from conversation with Mrs. Piper in her normal state as naturally as conversation with one acquaintance differs from conversation with another. I have no doubt that the case for regarding the communicating intelligence as an



independent individuality seems yet stronger when it represents itself as being an intimate deceased friend of the sitter's, sustains the part with vigour, and, besides showing knowledge which it is difficult to suppose to be derived from any one but the deceased, exhibits traits characteristic of him and recognises appropriately his friends and acquaintances. And the illusion is strengthened by the fact that two seeming individualities are capable of communicating simultaneously, one by the voice speaking and one by the hand writing.

It is, however, well known through experience that neither the assertions nor the beliefs of secondary personalities are in themselves worthy of credence, any more than one's dream impressions are; and, moreover, that—as may be seen in the case of some hypnotised persons—they can often by suggestion be made to act, and apparently believe themselves to be, any given character. The mere assumption and successful personation by the trance personality of a character different from Mrs. Piper's affords, therefore, no sufficient ground for believing that it is not herself. Nor does the possession by the trance personality and the normal Mrs. Piper of two more or less independent chains of memory prove anything, for a similar distinctness in chains of memory may easily be observed between the hypnotic trance personality and the waking personality of the same individual, and even between different stages of hypnotisation of the same individual. (See Mr. Gurney's papers on "The Stages of Hypnotism," *Proceedings*, Vol. II., "Stages of Hypnotic Memory," Vol. IV.; also *L'Automatisme Psychologique*, by Professor Pierre Janet.)

Nor again does the simultaneous existence of two apparently independent intelligences, one speaking and the other writing, prove anything, for this phenomenon too may be observed in good hypnotic subjects when the supposition that either intelligence was any other than the subject's own would be obviously absurd. (See Mr. Gurney's paper on "Peculiarities of Certain Post-Hypnotic States," *Proceedings*, Vol. IV., and also Professor Janet's book.)

The only thing that would prove the communicating intelligence not to be Mrs. Piper would be the possession of a stream of consciousness provably continuous with that of some other person and discontinuous with that of Mrs. Piper. Proof of this would be exceedingly hard to obtain, because if telepathy between the dead and the living be possible, we may suppose either active prompting of Mrs. Piper's trance personality by another spirit, or passive community of ideas between the trance personality, the sitter and the spirit of the deceased. Either might enable the trance personality to produce a semblance of possessing the stream of consciousness of the deceased within the limits of the communications that occur. The trance personality actually claims sometimes that it is prompted, as

above suggested, by another spirit. For instance, in a sitting of Dr. Thaw's (*Proceedings*, Vol. XIII., p. 567), when his friend Dr. H. was supposed to be communicating, not directly, but through Phinuit, a long speech was made so characteristic of Dr. H. that Dr. Thaw, wishing to know whether he or Phinuit was speaking, said, "Can you tell me anything about Dr. Phinuit?" The answer was, "I'm talking to you myself, you rascal; I'm talking for him." "Well," said Dr. Thaw, "you're trying to make us think he's talking," to which Phinuit replied, "I'm simply telling you what he says. I'm trying to imitate him."

It will be replied that though it may be almost impossible to produce clear proof for the world that the communicator is what he professes to be, yet in spite of this difficulty, practically adequate proof has been obtained in such a case as George Pelham's. For in this case there is so much that is characteristic in small ways, in manner, expressions used, etc., and in particular so unfailing a recognition of friends, and recognition of them in a manner appropriate to each, that for those who knew G. P. the conclusion is inevitable that no mere prompting by G. P., no combination of reading of G. P.'s and the sitter's minds by the trance personality could enable it to give so exact a representation of him. The communicator must be G. P. himself.

It is, no doubt, impossible for one who has not had the special experience on which subjective estimates of this kind are based to judge fully of their value; many small points involved can hardly be conveyed in words, and others are of too intimate a nature for the sitter to be willing to publish them. I think, however, that the importance of the evidence for direct communication afforded by such experiences may easily be over-estimated. I would point out, in the first place, that the representation given of the deceased person must from the nature of the case be of a very limited kind. For instance, when the *soi-disant* G. P. communicates, he usually does so in writing, the whole of Mrs. Piper's body except the hand being in an inert condition. At the best, therefore, he is limited to the words he writes and to the possibilities of dramatic representation of a person's whole *manière d'être* through the movements of the hand alone. The handwriting, it will be remembered, is not like that of the living G. P. The case is not materially improved when the communication is by spoken words, for gesture and expression and voice are still excluded from the likeness, "and the language shows a tendency to depart more or less from the language that would have been used by the communicator when living" (*Proceedings*, Vol. XIII., p. 332). A likeness under these circumstances must bear to a full dramatic representation of a person somewhat the relation that a clever pen and ink sketch done in a few strokes bears to a finished portrait in oils. The likeness

may be unmistakable in both ; but that in the sketch, being limited to a few salient points, requires much less minute knowledge of the subject to produce.

In the second place, the likeness of the trance personality to the person it purports to be does not strike every one equally. Thus Miss W. writes (*Proceedings*, Vol. VIII., p. 29): "The clearly marked personality of that friend, whom I will call T., is to me the most convincing proof of Mrs. P[iper]'s supernatural power, but it is a proof impossible to present to any one else." Yet her mother felt, after a sitting, that "it did not really seem like T." (*Proceedings*, Vol. VIII., p. 33.) And Mr. Marte, a friend of G. P.'s, formed, we are told, an opinion entirely unfavourable to Mrs. Piper, from which I infer that he did not think the personation of G. P. successful. (*Proceedings*, Vol. XIII., p. 429).

The personation, again, is sometimes very imperfect, even within the narrow limits possible. Thus, in Miss A. M. R.'s experience, the communicator purporting to be her friend seems to have presented a mixture of the characteristics of that friend and of Phinuit. She writes (*Proceedings*, Vol. VIII., p. 113):—

When my friend H. takes control of the medium, it seems to be quite a different personality [from Phinuit], although there is something in the voice or manner of speaking that is like Dr. P[hinuit]. The voice, however, is not nearly so loud. When I asked him once why this was, he told me that Dr. P[hinuit] was right by him, and that he could not stay a moment without his help. In a great many little ways he is quite like what my friend used to be when living, so much so that I am afraid it would take a great deal of explanation to make me believe that his identical self had not something to do with it, wholly apart from the medium's powers, or from anything that may be in my own mind concerning him.

One more thing about these personations of known individuals I may notice. They seldom, if ever, I think, profess to present themselves in *propria persona* at once. At their first appearance their communications purport to come through some one else ; or, as Dr. Hodgson expresses it, they "require some practice in handling the writing mechanism, and at first the assistance of G. P. or some other experienced amanuensis has usually been required." (*Proceedings*, Vol. XIII., p. 376.) Thus at first Phinuit speaks for G. P., G. P. writes for John Hart when he first communicates, and so forth. This is what we should expect to happen if Mrs. Piper's trance personality, with the aid of the sitters, and by means of knowledge accumulated normally and supernormally, gradually acquires a more or less coherent conception of the individual it ultimately takes itself to be. So, too, on this hypothesis, should we expect the gradual improvement in the personation which seems—in some cases, at least—to occur.



So far I have tried to show that the grounds on which the belief is based that the direct communicator is the person it alleges itself to be may easily be less cogent than at first sight appears, even when that person is known to the sitters, and is more or less successfully represented. I now go on to show that the communications afford in certain cases positive grounds for believing that the direct communicator is not the person it professes to be and probably believes itself to be, and, further, that it is Mrs. Piper's trance personality.

Inconsistencies, incoherences and contradictions in a communicator's account of himself, oblivion and error about things which it seems inconceivable that the real person should have forgotten or be mistaken about, and an intellectual standpoint inferior to his in life are, it will be admitted, good reasons for doubting the communicator's claim to identity. Suggestibility, a tendency to false excuses and prevarication, to claim powers which it does not possess, a tendency also to recur foolishly to things that have turned up at previous sittings but have lost their interest, are characteristics of an automatic personality. And that the automatic personality is here Mrs. Piper's is confirmed by certain intellectual limitations, an interest in certain kinds of trifles, and certain impressions as to what has occurred, which the communicator exhibits, and of which the origin is most easily found in Mrs. Piper's training and habits. I shall try to illustrate these points by examples.

Phinuit is, of course, a glaring instance of a communicator whose account of himself is inconsistent, incoherent and altogether impossible. Mr. Leaf puts the case briefly, as follows (*Proceedings*, Vol. VI., p. 560):—

That Dr. Phinuit is what he gives himself out, the spirit of a departed Marseillais physician, I may say at once that I do not see the least ground for believing. His own word does not, in view of his moral standard, apart from other considerations, carry even a presumption of veracity; nor has a single one of the numerous statements he had made as to his life on earth proved capable of verification. On the other side, his complete ignorance of French is a positive ground for disbelieving him, and one which he has never been able to explain.

The case against him is shown to be even stronger in the report made by Dr. Hodgson in 1892. (*Proceedings*, Vol. VIII., pp. 50–54.) Dr. Hodgson was not at that time inclined to believe Phinuit to be a "spirit," and if he is more inclined to think so now, it is because the G. P. control vouches for him in some sort.

The group of communicators whom Dr. Hodgson refers to in the last section of his last paper on Mrs. Piper (*Proceedings*, Vol. XIII., pp. 407–412)—the so-called "spirit guides" of Mr. Stainton Moses—again are apparently so hopelessly at sea about their alleged earth life, and so

ignorant about Mr. Moses himself and what they were supposed to have taught him, that I cannot see how it is possible to believe them to be what they represent themselves as being. Mr. Moses' "spirit guides," whether phases of Mr. Moses or not, wrote through his hand that they were the spirits of certain definite persons known to history. The names they gave have not, I believe, been published, and are known to very few persons. In Mr. Moses' writings they appear under the pseudonyms, Imperator, Mentor, Rector, Doctor. If, therefore, they could, through Mrs. Piper, give the unpublished names which Mr. Moses has left on record, it would be a very interesting piece of evidence. But though Mrs. Piper's hand has written various different names as being theirs, none are right; and not only are wrong names given, but they are supported for a time with insistence and detail, only to be rejected afterwards in favour of others equally wrong. The *soi-disant* Doctor, for instance,—in Dr. Hodgson's MS. records of sittings—claims to be Homer, who sang of Troy, and is indignant when it is doubted. Mentor, again, maintains, during more than one sitting, that he is Ulysses, says (Dec. 7th, 1896) that he "often, often" sees Telemachus, and that Penelope—though he does not apparently remember her name till Dr. Hodgson suggests it—is with him always. I do not see how elaborate false statements of this kind, inconsistent with the claims of the communicators to be Mr. Stainton Moses' "guides," are to be accounted for by any amount of difficulty experienced by them in using Mrs. Piper's organism as a writing machine.

The *soi-disant* guides of Mr. Moses are guaranteed by G. P. just as Phinuit is, and this Dr. Hodgson seems to regard as a reason for believing in their independent existence. But surely it may be equally well regarded as a reason for believing the G. P. control to be itself a phase of Mrs. Piper's trance personality. Moreover, believing in G. P.'s guarantee involves us in difficulties, for he is capable of corroborating false statements; *e.g.*, Doctor's statement that he was Homer. And further (on April 23rd, 1897), Rector warns Dr. Hodgson "not to rely too much upon the statements made as tests, so-called, by your friend George. He is too far away from your earth now to be clear in regard to tests, test-conditions, etc." If the guaranteed spirits throw doubt on the trustworthiness of the guarantors, where are we?

As an instance of ignorance and mistake about its own name in a personality professing to have been known to the sitter, I may quote a remark of "Mrs. M." in *Proceedings*, Vol. XIII., p. 348. She says:—

The aunt of my husband's who died in June, 1894, has often sent me, through Phinuit or my husband, some appropriate sounding message, and twice purported to write herself, but never seemed able to give her own name correctly; it is an unusual name, probably unique in this country, and I tried to get it written a number of times, but with no success.

If the writing intelligence was really the aunt, this forgetfulness of her name is odd, but it is still more odd that she should make incorrect guesses.

A curious instance, not yet published, I think, of false statement by the communicating intelligence occurred on one occasion (March 7th, 1894) when Dr. Hodgson had given the entranced Mrs. Piper a MS. of Dr. Wiltse to hold. Later in the sitting Dr. Wiltse himself was personated, the communicator stating in a confused way that he was dead and his body in the water. Dr. Wiltse is well known to readers of the *Proceedings* as having communicated several interesting experiences to the Society, which have been published. In reply to Dr. Hodgson's questions, one of these was described by the *soi-disant* Dr. Wiltse sufficiently for identification, leaving no doubt as to the person intended by the communicator. But all the time Dr. Wiltse was alive and well and going through no abnormal experience of any kind; nor was any knowledge shown by the communicating intelligence of his actual doings or surroundings. We may surely more easily attribute this personation to a freak of Mrs. Piper's trance personality than account for it in any other way.

Again, is it possible to suppose that Mr. Moses, who never was in Professor Sidgwick's house, should, writing with Mrs. Piper's hand, send a message, "when you see my friend Sidgwick, kindly ask him if he remembers the evening we spent together at his own home"?

Besides specific concrete statements, inconceivable as coming from the person the communicator professes to be, we find evidence in certain cases of a much lower intellectual standpoint than was his on earth. Take, for instance, Professor James' account of the "E. Control" (*Proceedings*, Vol. VI., p. 656):—

The first messages came through Phinuit about a year ago, when, after two years of non-intercourse with Mrs. Piper, she lunched one day at our house and gave my wife and myself a sitting afterwards. It was bad enough; and I confess that the human being in me was so much stronger than the man of science that I was too much disgusted with Phinuit's tiresome twaddle even to note it down. When later the phenomenon developed into pretended direct speech from E. himself, I regretted this, for a complete record would have been useful. I can now merely say that neither then, nor at any other time, was there, to my mind, the slightest inner verisimilitude in the personation. But the failure to produce a more plausible E. speaks directly in favour of the non-participation of the medium's *conscious* mind in the performance. She could so easily have coached herself to be more effective.<sup>1</sup>

Or, again, G. P., when he ventures on philosophical subjects, is apt to miss the point in a way which I understand the real G. P. would

<sup>1</sup> This remark is true also of the personations of the Stainton Moses' "guides."



not have done in this life. I know Dr. Hodgson does not altogether agree, but it seems to me impossible to read the following more detailed account of the sitting of which part is briefly described in *Proceedings*, Vol. XIII., p. 428, and believe that the alleged G. P. understood the questions or knew what he was talking about. No difficulties of communication or interruptions from the sitters will account for the absence of relevancy in his remarks.

M. : What do you think now about your argument on one and the many, one or many ?

G. P. : "I was not absolutely wrong, but still I can hold a little different argument now, John." . . .

M. : What do you think now about the multiplicity in the universe ?

G. P. : "I am convinced of the reality of the ego"

M. : He is getting off the track a little I guess.

G. P. : "which I want to explain after I answer your questions."

M. : What do you now think ?

G. P. : "My, the world will still go on multiplying and progressing and advancing in knowledge."

M. : What do you now think of the appearance of unity and the multifariousness of phenomena ? Is there really a unity, or is it all contributed by our intelligence ?

G. P. : "Oh, Marte, this is yes, there is a unity most certainly, help me a little please."

M. : You know, George, you thought that the appearance of unity was all due to our imagination ?

G. P. : "This light is weak I know, but I have answered your question on that, and told you I was wrong." . . .

M. : What do you think of cosmical weather, George ?

G. P. : "It is certainly caused by . . . [undecipherable] . . . [shaking hands with M. again.]

"that helps me ; ask your question again. . . ."

M. : What do you think about cosmical weather, Chauncey Wright's cosmical weather ?

G. P. : ". . . so my good friends can understand my answer. Marte, you know what I mean. Oh, you were right most . . ."

Mrs. Howard : Write again, most what ? Absolutely ?

G. P. : "Emphatically, it is *not atmospherical weather*."

Mrs. Howard : George, you are writing so fast ; now wait and see if Hodgson can read that.

G. P. : "Why don't you help me" . . .

R. H. : We can't read after "emphatically" ; that is all we can read.

G. P. : "Atmospheric is the word, now I."

M. : Well, now that you have got into that world, George, have you got any new light upon the character of natural law ? Do you now find that law is . . .

G. P. : "Yes, law is thought."

M. : Do you now find that law is permanent ?

G. P. : "Cause is thought."

M. : That doesn't answer.

G. P. : "Ask it."

M. : Is law permanent, or is it only transitory result ?

G. P. : "It is permanent"

M. : Then do you agree with Chauncey Wright ?

G. P. : "and everlasting."

M. : Then do you agree with Chauncey Wright ?

G. P. : "Most certainly on that point."

Mr. Howard : What do you think of his views on cosmical weather ?

G. P. : "He knows nothing, his theory is ludicrous."

M. : He just said he agreed with him. That was the point I was asking about, the permanency of law.

Other instances of communication below the education and intelligence of the supposed communicator will be found in a subsequent part of the argument.

I proceed now to the manifestation of characteristics which belong to automatism, beginning with suggestibility. The following is an instance from *Proceedings*, Vol. XIII., p. 346. Mrs. M. writes:—

On February 15th and March 7th, 1895, Miss Edmunds had two sittings for me . . . I asked Miss Edmunds to ask my husband to write his full name, which was not on my visiting card, and which she could not possibly be aware of. The name was at once correctly written, but for some reason Miss Edmunds did not think it was right, and asked if the writer was quite sure of it. The name was then differently written, though commencing with the same letter R (which was on my visiting card).

Having once been assumed, it becomes established, and at a sitting which Miss Edmunds had for Mrs. M., on January 25th of the following year (1896), the name was written again in the same way, though Miss Edmunds then knew it was wrong, and knew what the real name was. Now it is not likely that Mr. M., had he been in direct communication with Miss Edmunds, could have been induced by the mere expression of a doubt to substitute a name that was not his for the true one. But if we conceive Mrs. Piper, dreaming as it were, and in partial and fragmentary communication of a telepathic nature with other minds, such as appears to occur between percipient and agent in thought-transference experiments, the whole incident falls into line with previous experience.<sup>1</sup>

In this particular case we have the interesting phenomenon of the communicator being led by suggestion off the right track on to a wrong one. But any one who has had sittings with Mrs. Piper will

<sup>1</sup> It should be noted that Mrs. M. believes that her husband had actually communicated through Mrs. Piper. Her summing up of the impression the whole series of sittings made on her, given in *Proceedings*, Vol. XIII., p. 348, is interesting and instructive. It will be seen that the giving of a wrong name was combined with good dramatic impersonation.

remember how indications given by the sitter are followed by the communicating intelligence. Mr. Clarke, whose sittings are recorded in *Proceedings*, Vol. VI., pp. 569-581, notes an instance of such following of an intentionally false lead. Phinuit says (p. 574):—

“You are the fellow that has got the hole in the ground with the black things in it.” (Sitter: “Coals?”) “Of course, do you think I am a fool? There is something to come from that hole yet, because I see light beyond it.” [Note by the sitter:] “In the former sitting mention had been made of a town lot and buildings as ‘interest in the ground.’ The vagueness of this reference is here developed, by a question intended to be misleading, into definite error.”

Such incidents exhibit a form of the “fishery”—to use Professor Lodge’s word—so much practised by Phinuit, and which appears to many simply a fraudulent attempt to get from the sitter the information to be reproduced to him as supernormally acquired. But though there is no difficulty in supposing a trance personality guilty of such deliberate attempts to make its powers seem greater than they are, I think it probable that much of the apparent fishery is unconscious, and merely due to a dream-like readiness to take ideas indiscriminately, whencesoever they come, and weave them into a whole.

A disposition to assume powers not possessed and to resort to prevarication and false excuses to account for ignorance or failure are moral defects which are well known to be frequently exhibited in automatic writing, and which are abundantly exhibited in Mrs. Piper’s trance utterances, whether spoken or written. I have said moral defects, but perhaps it would be more correct to say defects partly moral, partly intellectual. The habit of attributing to reason actions and opinions really due to impulse or sentiment, and the consequent necessity of finding plausible reasons afterwards in order to maintain self-respect as a reasonable being, is common in human nature, and the man who does it usually thinks he has really been influenced by the reasons he alleges to himself and others. The apparently lower moral standpoint of the automatic personality may be at least partly due to abeyance of the critical faculty, which tends to render the powers assumed and the excuses made occasionally so absurd that it is difficult to believe the communicating intelligence to have taken itself in by them.

The kind of thing I refer to is well described by Dr. Leaf in the following passage (Vol. VI., p. 560):—

Phinuit, in fact, exhibits just the low moral tone which we so often find in table-talking, planchette, and other manifestations, as we now regard them, of the secondary self. He swears freely, and indulges in slang of the vulgar New England sort, in a way quite alien from the manners of Mrs. Piper. This is, of course, a trifle; but it is more serious when we find him continually making attempts to deceive. If he is not able to make a right



statement, he is always ready with a guess of more or less ingenuity to conceal his ignorance, or at least with some ambiguity or subterfuge which should make a show of turning the difficulty. Hardly a sitting passed without his making at least a few statements which were altogether wrong.<sup>1</sup>

As a particular instance of Phinuit's prevarication I may quote a passage in a discussion of him by Dr. Hodgson (Vol. VIII., p. 50):—

Concerning his inability to speak French, Phinuit's original explanation to me was that he had lived in Metz the latter part of his life, and there were many English there, so that he was compelled to speak English, and had forgotten his French. I replied that this explanation was very surprising, and that a much more plausible one would be that he was obliged to use the brain of the medium, and would therefore manifest no more familiarity with French than she possessed. This—trite enough—suggestion appeared to Phinuit also more plausible, since a few days later he offered it himself to another sitter as an explanation of his inability to sustain a conversation in French!

But no one denies that, whatever Phinuit may be, he possesses many of the characteristics of an automatic personality, and I may be asked to produce instances of prevarication in other personations. Here, then, is one perpetrated by a communicator professing to be Mr. Stainton Moses. At a sitting (recorded in MS.) on November 14th, 1896, he says: "Give my love to . . . Myers, Watts, Massey, Wedgwood, Bennett, and any other friends who may inquire for me." Dr. Hodgson naturally replies, "But Wedgwood is with you now, isn't he?" To which the communicator, with obvious insincerity, rejoins, "I intended this. He just mentioned to send his love also."

G. P. is the supposed communicator in the following case. He had been asked at a previous sitting for names of the members of a certain association. He says:—

I answered part of that question [the part he answered was correct], but did not give the names of the other two people, because it would be no test, because I told her [Mrs. Howard] the names of the other two in life, and as she knows them, if I was to give the names in her presence, they would say it was thought-transference. No, I shall reserve the two names to tell Hodgson some time when he is alone with me, because *he* does not know them. [All true.] (*Proceedings*, Vol. XIII., p. 303.)

Dr. Hodgson remarks on this, "Many will regard this as perhaps an excuse for ignorance, as names subsequently given were not correct." I confess to being one of the many, but I think it would be hard on G. P. to attribute the false excuse to him.

An experiment frequently tried in sittings with Mrs. Piper is to ask the "control" to visit distant people or places and report what is

(<sup>1</sup> Cf. also Mr. Myers' remarks on automatic personalities, *Proceedings*, Vol. VI., p. 440.)

seen. The communicator generally professes to be able to do this; but the profession is seldom justified by the results, though there are apparent exceptions. Now that Mrs. Piper's trance personality should imagine it has this power, and should accordingly dream that it visits the places—that, in fact, Mrs. Piper dreaming should imagine the whole thing—is natural enough; but it is, I think, going out of our way to complicate hypotheses by attributing the dream to G. P. or “George Eliot.” While the series of hitherto unpublished sittings to which I have more than once referred was going on, Mr. Myers was holding a series with another medium in England. It had been arranged between him and Dr. Hodgson that supernormal communication should, if possible, be established between the two. Mrs. Piper's “controls”—“George Eliot” especially—professed to visit Mr. Myers' circle and to give and receive messages. Not once was there a glimmer of truth in what was said. Mr. Myers wrote: “Yours up to April 23rd inclusive received. Not a vestige of truth; no messages received; whole description erroneous.” After hearing this, “George Eliot” continues to assert that she gave the messages, and saw the sitters, and when Dr. Hodgson asks, “How about the numerous messages that you brought back from Myers?” replies, “We would ask you to ask him to consider carefully what his thoughts were, that is, those not put into actual speech.” Observe that it is not merely the failures that I lay stress upon, but the reiterated false statements that things have been seen which have not been seen, and messages received that have not been received, or delivered that have not been delivered. We find (Vol. XIII., p. 345) Mrs. M.'s husband similarly professing to repeat messages, but always giving them quite wrong, except in one case, and in that case there had been no deliberate experiment. Of the same character are the incidents connected with Miss Hannah Wilde's letter, of which Professor James gives the following account in 1890 (Vol. VI., p. 657):—

Mrs. Blodgett, of Holyoke, Mass., and her sister, devised, before the latter died, what would have been a good test of actual spirit return. The sister, Miss H. W., wrote upon her death-bed a letter, sealed it, and gave it to Mrs. B. After her death no one living knew what words it contained. Mrs. B., not then knowing Mrs. Piper, entrusted to me the sealed letter, and asked me to give Mrs. Piper some articles of the deceased sister's personal apparel to help her to get at its contents. This commission I performed. Mrs. P. gave correctly the full name (which even I did not know) of the writer, and finally, after a delay and ceremony which occupied several weeks on Phinuit's part, dictated what purported to be a copy of the letter. This I compared with the original (of which Mrs. B. permitted me to break the seal); but the two letters had nothing in common, nor were any of the numerous domestic facts alluded to in the medium's letter acknowledged by Mrs. Blodgett to be correct. Mrs. Piper was equally

unsuccessful in two later attempts which she made to reproduce the contents of this document, although both times the revelation purported to come direct from its deceased writer.

This subject of Miss Hannah Wilde's letter continues to recur at sittings long after the above passage was written, and always with equal futility. It is an instance of the foolish return to a subject which has lost its interest, which we find in cases where co-ordination is lacking, as in automatic writing or second childhood. Dr. Hodgson notices this characteristic, and describes it as follows (Vol. XIII., p. 333):—

That the writing should frequently—especially towards the end of the sitting, as the communicator becomes exhausted and stupefied, and loses the power of inhibition—become extremely automatic and dreamy is to be expected; and questions put to the communicator in this state will be answered by the first vague associations that rise to the surface of the failing consciousness. Not only is this the case, but such irrelevant (as they may be) associations occasionally tend to recur at later periods, when the communicator lapses into the same stage of dreaminess, precisely as they might be expected to do in ourselves. For example, in the latter part of Professor Norton's sitting on May 26th, 1894, when Professor Norton put a question which G. P. living in full consciousness would have answered by a reference to an essay he wrote at Harvard on Jane Austen, G. P. replied by references to what he had written about Robert Noyes (pseudonym), the life of whom he had published not long before his death, and to articles in the *Sun*, a New York paper with which he was connected at the time of his death. Professor Norton then mentioned Jane Austen. On two or three later occasions, at the end of a sitting, when the writing became particularly dreamy, the name of Professor Norton appeared in connection with that of Jane Austen.

The difference between Dr. Hodgson's interpretation of this dream-like futility and my own is that he is disposed to attribute it to the difficulties of a spirit other than Mrs. Piper in using her organism, while to me it seems an indication that the communicators are no other than Mrs. Piper herself in trance.

So far the qualities I have wished to call attention to in the communications—indications of suggestibility and so forth—are such as might occur in any automatic writing. But it somewhat strengthens the case to note that there are many things in the communications which are natural enough if we suppose them to come from Mrs. Piper, but decidedly less so if attributed to the supposed communicator. I think it is to Mrs. Piper's own interest in such trifles that we may trace the frequent recurrence in the sittings of domestic details—new hats, lost pocket-handkerchiefs, etc.—and to the customs of Mrs. Piper's social circle that we may trace such a false statement by the supposed G. P. as the following:—In answer to the question, "Can't you tell us something . . . your mother has



done?" he replies, "I saw her brush my clothes and put them away. I was by her side when she did it." As a matter of fact, G. P.'s clothes were brushed and put away after his death, as Mrs. Pelham wrote, not by herself, but by "the man who had valeted George." G. P. was not likely to suppose that his mother would personally brush his clothes, or to be interested in the matter at all; but in Mrs. Piper's mind the idea would easily associate itself with the ideas of his death and his mother.

Phinuit's intellectual attainments were notoriously limited to Mrs. Piper's. We have already referred to his ignorance of French, and again Dr. C. W. F. (*Proceedings*, Vol. VIII., p. 51) questions whether Phinuit's medical knowledge extends "beyond what Mrs. Piper has read in 'Domestic Medicine.' He always gives the common or vulgar names of the medicines he describes, and never their botanical ones." Similar limitations are conspicuously shown in references to history, literature, and science. We may take the statement by the alleged Sir Walter Scott that there are monkeys in the sun as an instance. This is mentioned by Professor W. R. Newbold (*Proceedings*, Vol. XIV., p. 48) in connection, as the reader will remember, with a very curious and interesting incident indicating thought-transference from a distance between himself and Dr. Hodgson and Mrs. Piper, which I need not enter into here.

The most glaring instances of what I mean naturally occur in the course of non-evidential parts of Mrs. Piper's trance utterances, which have not generally been published. I will give some from the MS. records of unpublished sittings. Here is a biological statement made on January 27th, 1897. Emperor says, the "interior," the "spirit," comes in "in the bird and the monkey," not in the horse or the dog; for "the brain of each is of a lower quality, and has practically no mind." All birds and monkeys, he says, have "interiors," but no other animal except man. The following again are specimens of chemistry and physics. Rector says, on February 1st, 1897, that "the spiritual ether is not composed of oxygen and hydrogen . . . it is composed of the finer tissues termed luminiferous ether." And Emperor tells Dr. Hodgson on February 5th, 1897, that "the etherial or spiritual body is composed of air, light, and a large amount of hydrogen" . . . "heat, light, air, hydrogen." Later in the sitting he says "it is known to us as ether," and further "the body of the medium becomes filled with luminiferous ether . . . until it is so distended that it cannot contain any more." Dr. Hodgson naturally expostulates about some of these statements, and Emperor accordingly modifies his views as the sitting proceeds, and finally arrives at the assertion that "the ether body is a subtle and exquisite matter known to us as vacuum . . . not on the

same plane as hydrogen at all." And we are reduced to a still more hopeless state of confusion between mind and matter when we are told next day that "vacium" is "known as love." Confusion in all this there undoubtedly is, but it is, I think, clearly the confusion of a person talking about what he does not understand, not the confusion of a person who knows, but has a difficulty in expressing himself.

And the same is true of the communicator's views on literary and historical matters. For instance, Mentor, on December 7th, 1896, when professing to be Ulysses, is asked by Dr. Hodgson after Achilles. He says "the murderer." *Dr. Hodgson*: "He slew Hector." *Mentor*: "Yes," and later he explains "His intentions were not evil, it was an insane impulse." On another occasion (June 4th, 1897) "George Eliot" describes her meeting with Bacon, who says, "You have questioned my reality. Question it no more. I am Shakespeare." On March 30th, 1897, again, "George Eliot" met "several distinguished-looking gentlemen,—Shelley and Chaucer, the original and only writer of 'Canterbury Tales,' then Burns" . . . "I also saw the original Adam Bede" . . . "also Homer."

The same want of knowledge is shown in the statements of Mrs. Piper's "Imperator" about Melchisedek—such as that "just before Israel was sent into bondage" Melchisedek "departed and returned to God" (on February 22nd, 1897), or (at the same sitting), that "about the time of his [Melchisedek's] ascending there came the tribe known as the Jews." Again (February 23rd), an interval is assumed between the days of Melchisedek and of Abraham during which "none better, purer, more godlike reigned than he." After this Dr. Hodgson gave a brief account of the blessing of Abraham by Melchisedek, and the next day (February 24th), when taxed with having spoken of the days between Melchisedek and Abraham, Imperator says, "That is wrongly registered; there was no time between." Again, on March 10th, "Imperator" says that Melchisedek reigned spiritually all through the days of "Abraham, Moses, Elijah and Elisha down to the days of King David." Now Mr. Stainton Moses' "Imperator" said a good deal about Melchisedek which has been published in *Spirit Teachings*, and which, though there is no ground for supposing it to be all true, is at any rate sensible and coherent, and such as we should expect from an educated man. A comparison of the account given by Mrs. Piper's Imperator and Mr. Moses' Imperator reveals some faint resemblances between the two—just enough to suggest that Mrs. Piper had seen the passages in *Spirit Teachings*, as was doubtless the case, since Dr. Hodgson left a copy with her on January 27th. The reproduction, if reproduction it can be called, is certainly not what one would expect from a person who had got up what had been said in order to reproduce it. But on the other hand, the remarks I have

quoted and referred to will hardly, I think, seem to any one to be those of a person who understands what he is talking about and is only hindered from making it clear by a difficulty in manipulating the machinery of communication. They are statements which can only be explained by supposing absence of knowledge.

The illustrations I have given are, I think, sufficient, or perhaps my readers may think more than sufficient. It only remains briefly to recapitulate the argument. We have presented to us in Mrs. Piper's trance a number of *soi-disant* different personalities—some of them known to have lived and to be now dead, some of them living, some probably imaginary. The statements and the intellectual calibre of many of them are utterly inconsistent with their claims, and even in the best personations there are lapses which cannot easily be explained if we are in direct communication with the professed communicator. These lapses and limitations, and other characteristics of the communications, are just such as have been frequently observed in secondary personalities, and in particular correspond to what we should expect to find in Mrs. Piper's secondary personality under the suggestive influence of the conditions of the sittings. I am, therefore, driven to the conclusion that Mrs. Piper's trance-intelligence has a strong tendency to unconscious dramatic personation; and is continually dreaming itself to be a number of different persons under the influence of suggestion (including self suggestion) somewhat as an ordinary hypnotic subject can be made by suggestion to assume different characters with startling dramatic effect. At the same time—as we have assumed throughout—along with the limitations there are fragments of knowledge exhibited by the trance personality in some sittings, which it is very difficult to suppose to have been acquired by Mrs. Piper in any normal way. A large proportion of these fragments of knowledge are in the minds of the sitters, some are in the minds of distant living persons, and a few were, so far as we can tell, known only to the dead. And further the selection and grouping of some of these fragments, even when they might have been learnt from living minds, is, as Mr. Hodgson has pointed out, sometimes most easily explained by supposing that the dead are partially responsible for it. Now, if we may assume that telepathy is possible between living minds and between the minds of the living and of the dead, and that the process in the two cases is similar, we find that our facts arrange themselves in a comparatively simple way. Mrs. Piper, we suppose, has in trance an unusually developed telepathic faculty. As in telepathic experiments with hypnotised subjects, ideas come to her in more or less dream-like sequence without her being generally able to trace their origin, which is sometimes telepathic and more often not.



It is introducing a complication almost gratuitously to suppose that communication with the dead is different in kind from telepathic communication with the living, and that, in order to communicate, the dead must turn out Mrs. Piper's spirit and themselves take its place in her organism,—their want of practice in using it being the cause of the confusion and imperfection of the communication. Confusion and imperfection we certainly find; but we always have found them in thought-transference experiments, and we do not try to account for this by supposing that the agent's spirit makes direct use of the percipient's nervous system to express through it the numbers or diagrams to be transferred and, owing to want of the same practice in using the percipient's brain that he has in using his own, often gives the wrong number! Why, if the communication occurs at all, it should be so fragmentary and irregular, we cannot tell. Where the failure in the transmission occurs we do not know. It may well be in the organism;—it is conceivable that potentially and subliminally all minds are in telepathic communication, though the results emerge into consciousness obscurely and incompletely, and often not at all. But to suppose the organism to be the cause of the failure is a very different thing from the ponderous hypothesis that another spirit uses that organism, and uses it imperfectly.

It would not, however, be fair to call the hypothesis of spirit possession quite gratuitous, for there is a difficulty in the telepathic hypothesis to which I have not yet referred. It is that agents differ. Some persons in the body seem capable of acting as agents when others are not, and agents in the spirit similarly vary; but while we can suppose that the difficulty in the case of living agents is physical and due to the agent's organism, we cannot thus account for it in the case of the dead. It is probable enough, however, that the difficulties may sometimes be psychical, especially as this would be analogous to our experiences in daily life, when the difficulties human beings have in making themselves understood by one another are by no means only physical.

But there is another factor playing an important part in the Piper sittings which has hardly, I think, received the consideration it deserves, and to which these differences in the veridicality of the communications are evidently at least partly due, namely, the sitter. The success of sitters in obtaining interesting communications varies, as we know, enormously, and there are persons who sit with Mrs. Piper—have even sat often with Mrs. Piper—and though they receive plenty of communications, receive none which there is the slightest reason to suppose supernormal at all. It is, I think, the case that the sitters who obtain most evidence of supernormal communication of some kind are those whose sittings afford evidence of supernormal

communication of all kinds, both of thought-transference from the sitter and of communication with the dead or with distant living minds. If it were only a question of information derived by Mrs. Piper telepathically from the sitter, it would be easy to attribute the failure of an unsuccessful sitter to his being a bad agent; but a good sitter seems in some way to make the process of transmission easier, even when he does not seem to be the source of the information.

I do not think Dr. Hodgson has attempted to explain this. He, of course, recognises the fact, and says, indeed (*Proceedings* XIII., p. 371), "There are certain persons who appear to be almost entirely unsuccessful as regards communications from their 'deceased' friends, and indeed at 'sittings' altogether, and from causes which apparently inhere in themselves." But these causes appear to Dr. Hodgson—judging from what he says further on in the same paragraph—to be such things as want of attention or of desire to succeed; or, again, to quote his words, "It may even be that the state of mind of some persons is actually repellent to the efforts which their 'deceased' friends make to communicate." Such causes as these, no doubt, may operate, but there is something more. I speak as one who has uniformly failed, whether as agent or percipient in thought-transference experiments, or as sitter with Mrs. Piper and other mediums, and I am sure that the cause is not want of sympathy or desire to succeed, or belief that success is possible. After years of failure the inevitable absence of hope may have a damping effect, but this will not account for failure when hope is still fresh. I think it is certain that the sitter does not serve only as an attraction to his deceased friends, but that,—granting, as I am doing throughout, the genuineness of Mrs. Piper's power,—there are subliminal qualities in the sitters which make the exercise of this power possible with some and not with others. Now this is not, I think, consistent with the hypothesis that the communicator in the other world uses Mrs. Piper's organism directly; for if so, why should it be more difficult for him to speak or write for A than for B? We require a hypothesis which allows for all three minds—the minds of the deceased friend, of Mrs. Piper, and of the sitter—being subliminally concerned in the result.

In attempting to frame such a hypothesis, two things should, I think, be borne in mind. First, I cannot but assume that, whatever other sources of information Mrs. Piper may draw from, she does sometimes draw telepathically from the sitter's mind. Evidence excluding other possible sources of information is, almost necessarily, rare, but it is afforded when mistaken ideas of the sitters are reproduced. As instances of this I may refer to an experience of Professor Newbold's, given in *Proceedings*, Vol. XIV., pp. 9, 24; also to an attempt of G. P. to describe the contents of a letter presented by

Mrs. Howard, when what G. P. said corresponded not with the actual letter, but with one which Mrs. Howard mistakenly thought she had presented (*Proceedings*, Vol. XIII., p. 320).

Secondly, our other investigations have produced some evidence, though not a great deal, pointing to the possibility of telepathic communication between the dead and living persons in a normal condition (not entranced)—evidence of the same kind, though much less in amount and in cogency, as that which apparitions at the moment of death afford of telepathic communication between the dying and their living friends. I do not propose to go into this here, and will only refer the reader to a paper by Mr. Gurney and Mr. Myers on "Apparitions occurring soon after Death," in *Proceedings*, Vol. V., and to Chapter XVII. of the "Report on the Census of Hallucinations," *Proceedings*, Vol. X. I think now, as I and the other writers of the Report thought then, that the amount of evidence is not in itself sufficient to constitute anything like a conclusive case for *post mortem* agency. Still it should not be ignored.

Now, obviously, if we once admit that telepathic impacts sometimes reach the normal consciousness of percipients through hallucinations, there is no need to suppose that those which do so are all the telepathic communications which occur. An unknown proportion—perhaps a very large proportion—may, for aught we know, never rise above the subliminal consciousness; just as ideas suggested to a hypnotised person may remain entirely unknown to him when he is awake, or may in some cases be brought to his waking knowledge by his own automatic action or by a suggested hallucination.

If, then, it be granted that we are not without reason for thinking that Mrs. Piper may be in telepathic communication with the sitter, and that the sitter may, without knowing it, be in telepathic relation with his dead friends; and also—what seems to be certain—that the sitter plays some important part in bringing about the results obtained through Mrs. Piper, is it not conceivable that his function is, to some extent, that of a channel of communication? The departed spirit communicates, we may suppose, telepathically directly with his friend the sitter, but this communication is entirely subliminal, and the sitter is not normally conscious of it at all. Mrs. Piper in trance receives telepathically impressions from some sitters which do rise, partially at least, to her trance consciousness, and are then given back by her to the sitter through the ordinary channels of sense in speech or writing.

Mrs. Piper in trance plays, on this hypothesis, a part analogous to that of a bad mirror reflecting very imperfectly the contents of the sitter's subliminal consciousness, coloured and distorted by the contents of her own. She does for the sitter, in a different way, exactly what a veridical sensory hallucination does. Such a hallucination—



say, an apparition at the moment of death—is generally an embodiment of many associated ideas from the percipient's own mind combined with what we may suppose to be the immediate result of the telepathic communication from the dying person. The percipient will see his friend most likely in clothes of a familiar aspect and with a familiar expression, in a position fitting in with his own physical surroundings; all these adjuncts to the idea of the friend being generally, it would seem, due to the percipient's own associations with the friend. Similarly we may suppose that Mrs. Piper derives from the sitter impressions subliminally received by the latter from the dead, combined with ideas previously in the sitter's mind, and reproduces these further complicated by her own ideas. Sometimes—indeed generally—there would be no evidence of any communication from the deceased, the sitter's own ideas being, so far as appeared, alone reflected. Further, it would not be at all inconsistent with the hypothesis that Mrs. Piper should herself, as well as the sitter, receive in some instances direct communications from the dead. One result, we may observe, would follow if the process resembles that which I suggest. Mrs. Piper's trance personality would in the course of a series of sittings gradually acquire as part of its own stock of ideas a definite conception of the deceased agent, and as this happened, it would become more and more difficult to distinguish veridical communications from those for which Mrs. Piper alone is responsible.

Any hypothesis must, with the very small amount of knowledge we at present have, be exceedingly tentative, and the one I have ventured to suggest, if true at all, can only be a very rough approximation to the truth. The mere attempt to state it, probably gives a misleading definiteness to things of which we have only a very dim perception. But I know of no other hypothesis which seems to me to fit at all even the very limited number of facts which we can regard as provisionally established, or legitimately assumed to be so for the purposes of the present discussion; and I have ventured to bring it forward in the hope that it may suggest lines of observation and experiment which may throw light on the very obscure and difficult questions involved in Mrs. Piper's phenomena.

## III.

DISCUSSION OF THE TRANCE PHENOMENA OF  
MRS. PIPER.

## III.—REFLECTIONS ON MRS. PIPER AND TELEPATHY.

BY ANDREW LANG.

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In Dr. Hodgson's "Further Record of Observations of Certain Phenomena of Trance" (*Proceedings*, Part XXXIII., pp. 284-413), he promises to consider objections to his provisional hypothesis. Such objections I venture to produce. In 1898 his hypothesis was that "the chief communicators" (with Mrs. Piper) "are veritably the personages that they claim to be" (p. 406). This means, of course, that, in Dr. Hodgson's opinion, Mrs. Piper is actually "possessed" (in the old phrase) by the spirits of the dead. This is indeed a "far-going" theory. But if we reject it, and also reject the idea that Mrs. Piper is a conscious impostor, and in collusion, for example, with such people as "Mrs. Howard," we must try to produce some other hypothesis. The hypothesis of telepathy from the living has been advanced, and has been met by Dr. Hodgson with arguments of considerable force. Though I have no theory to offer myself, perhaps I may be allowed to make a few remarks on the problem; especially as regards the telepathic explanation. When I published *The Making of Religion* in 1898, one of my purposes was to set forth alleged savage parallels to the "psychical phenomena" recorded by the Society as occurring among civilised peoples, European and American. Among such phenomena occurs the "possession" of a human "medium" by a god or a ghost. When the medium at Delphi, or in modern Burmah, China, Polynesia, savage North America, New Zealand, or Africa, becomes convulsive, and afterwards utters prophecies or other statements about what cannot normally be known to him or her, the pre-scientific theory was that the organisation of the medium is being used by a god or spirit. But this "spiritistic" theory has been employed by mankind to explain almost every kind of obscure phenomenon, and has in every field been rejected by science; apparently with justice. When the theory is now revived by Dr. Hodgson to account for the singular case of Mrs. Piper, one cannot but regard it, *a priori*, with extreme

suspicion. There are other grounds for distrust. Thus Dr. Phinuit, the *soi-disant* spirit of a French doctor, is notoriously ignorant of French, and not one of his statements about his earthly life admits of verification. He is, or was, not only vulgar beyond belief, but also tricky, evasive, false, dishonest. Thus Professor Peirce (May 1st, 1892) says, after a "sitting," "Phinuit seemed to me to be constantly groping after indications from me to correct and direct his intelligence, and in some cases he seemed to be so directed." The "sitting" is "a struggle for knowledge to which the sitter contributes."

I know how much some Psychological Researchers dislike evidence from anthropology, but a Zulu sitting with a possessed woman could not be better described than in Professor Peirce's words. The sitters are expected to give indications by beating the ground when the seeress is right, and the seeress, like Mrs. Piper, constantly asks tentative questions. "Who's Herbert?" "I get a Maria in your surroundings?" with a pause of interrogation. This is Mrs. Piper's method: compare Callaway, *Religion of the Amazulu*, pp. 361-374. As to the evidence for the Zulu method, it is explicitly set forth in Bishop Callaway's Zulu texts and translations; or see *Making of Religion*, pp. 152, 153. The coincidence of Zulu and Piperian methods is exact, as far as the "struggle for knowledge to which the sitter contributes" is concerned. Professor Trowbridge, again, was "struck by a sort of insane cunning in the groping of the woman after something intangible" (*Proceedings*, Part XXXIII., p. 526). Professor Norton thought Mrs. Piper's conditions "analogous to those of an ill person dreaming a suggested dream, in which trains of dreams to which the dreamer has been accustomed are modified by the special conditions of the moment." He made no doubt of Mrs. Piper's good faith, but what is the good faith of "an ill person" in a dream modified by the sitters?

Thus both Phinuit and Mrs. Piper's performances in general have produced the worst impression on various and most intelligent observers. Professor Macalister says: "I let her see a blot of ink on my fingers, and she said that I was a writer." This implies, of course, that, through her apparent trance, Mrs. Piper's normal self was on this occasion wide awake and eager to catch at any knowledge that could be normally acquired. Even if Mr. Macalister had been happier in later sittings that blot remains indelible. Mr. Macalister "thought the whole performance an imposture and a poor one." Later, an American author, cited as Mr. Marte, who knew "G. P.," "formed an opinion entirely unfavourable to Mrs. Piper." Dr. Weir Mitchell is a thoroughly qualified observer as a physician and a man of the world, and he says that had he not heard Professor James's statements, he would have come to the conclusion "that the



whole thing was a fraud and a very stupid one." Later sittings, of course, might have altered that opinion. (*Proceedings*, Vol. VI., pp. 605-606; Part XXXIII., pp. 429-483). Mr. Barkworth, as far as his own experiences went, set down any successes to muscle-reading.

I might quote many such results, but what chiefly confirmed my own suspicions were the so-called communications, through Mrs. Piper, of Professor Oliver Lodge's deceased uncle. I have not space for a detailed comparison of what Mrs. Piper said with what was said by a surviving brother of Mr. Lodge's uncle as to some incidents of their distant boyhood. But lately I went through the two accounts with a member of the Society, certainly not prejudiced against Mrs. Piper, and I think he admitted that in only one case (the possession by the uncle of a dried snake skin) did the revelation of the dead really harmonise with the recollections of the living kinsman of Mr. Lodge. Of course, my memory may exaggerate. The events, moreover (though they did not really occur at all as the dead uncle described them), were exactly such as do arise in the experiences of almost every human boy. Readers can examine critically the two statements, and decide for themselves (*Proceedings*, Vol. VI., pp. 465-557). For these and other reasons I conceived that Phinuit, at all events, was only a morbid secondary personality of Mrs. Piper's. Now such personalities, or moods, or whatever we call them, are notoriously apt to be dishonest, detestable, and the very reverse of the patient's normal character. The old English proverb says "Ale is another man"—that is, the alcoholic mood differs from the normal personalty. The character of the dipsomaniac (say, Prince Charles Edward) is the precise contrary of the victim's original character. To take a more recent instance. The *Dundee Advertiser* lately (December, 1899) published the sad story of a local artisan. He got "mad drunk," doubtless on some infamously adulterated whisky, went home and beat his mother to death. He then strayed out, fell into some water, and on emerging knew what he had done. He at once gave himself up to the police. Whisky was "another man," and a much worse man.

Worse, too, than the normal self of the patient is, as a rule, the non-alcoholic "secondary personality;" the "preposterous scoundrel," as Professor Shaler calls Phinuit. In "unfortunate subjects of abnormal conditions of the brain," says Mr. Huxley, "the disturbance of the sensory and intellectual faculties is not unfrequently accompanied by a perturbation of the moral nature, which may manifest itself in a most astonishing love of lying for its own sake." He cites a case of a soldier whose left parietal bone was fractured by a bullet. In his normal life he is honest, intelligent, and kindly. In his abnormal intervals "he is an inveterate thief,"

and all his proceedings present "a close parallel to the phenomena of somnambulism and of mesmerism," including examples of strange lucidity. Mr. Huxley cites "De l'Automatisme de la Mémoire," by Dr. Mesnet, in *L'Union Médicale*, July 21st, 23rd, 1874. His own text is in *Culture and Science*, 223-231. The whole story is most curious, and worthy of attention.

Now not only is, or was, Phinuit dishonest and tricky, but even "G. P." frequently gives absolutely false information when asked about what is occurring at a distance. He says, for instance, that his father is writing a letter (an easy guess) when he is not. The late Mr. Moses, when invited to give the real names of Imperator and Co. (his "controls") gives wrong names. The attempts of G. P. to translate Πάτερ ἡμῶν ὁ ἐν τοῖς οὐρανοῖς (Part XXXIV., p. 46) read as if part of Mrs. Piper's mind was guessing on the lines suggested by *Pater* (*pater noster*). The false information is given with perfect aplomb, and is "not right, though natural enough," i.e., a deliberate guess (Part XXXIII., pp. 345, 346, 347). The communicator is "Mr. H."

We are told that subtle traits of the dead are recognised by their friends in these communications. Thus a husband recognised familiar phrases of his dead wife's, such as, "Well, I should say I had!" "Don't I?" "Well, I guess!" (*Proceedings*, Part XXXIII., p. 504). But surely these phrases are excessively common in the conversations of American women, if we may judge by the dialogue of American novels. On the other hand, deliberate evasiveness and calm falsehood were not the traits of G. P. and the rest when alive. Therefore I would attribute *these* traits, at least, to Mrs. Piper's "preposterous scoundrel" of a secondary personality.

That spirits, in the next life, making use of Mrs. Piper's brains, nerves, voice, and hands, should be confused, is intelligible. But why should they be impudently mendacious, absurdly ignorant, and furtively evasive; fluent in twaddle, and "groping" when a simple question as to something familiar to them when alive is asked, that is, in many cases?

If Mr. H., or Phinuit, or G. P. were honourable spirits, they would say that they don't know, when they don't know. They would not give false information, "natural enough" (easily guessed), but totally wrong. Such proceedings, on the other hand, *are* characteristic of a secondary personality, which is occasionally supplied with right facts by guess, muscle-reading, or even telepathy. Dr. Hodgson has ably argued that the failures and confusions "fit the claim" of spirits, and do not fit the theory of telepathy (Part XXXIII., pp. 391-396). But the points on which I have dwelt do assuredly harmonise with the notorious character of the secondary personalities of people honest in

their normal state. And these points do not fit the character of honourable spirits, eminently the reverse. Mr. Newbold insists on this, in the case of Mr. Moses (Part XXXIV., p. 41). Indeed Mr. Podmore, with his views of the performances of the incarnate Mr. Moses (Part XXXIV., pp. 52-53) must regret that Mr. Moses and his crew of "controls" have invaded Mrs. Piper.

In reviewing my book, *The Making of Religion* (Part XXXIV., p. 131) Mr. Podmore censures my remarks on Mrs. Piper (p. 151). Dr. Hodgson's report (Part XXXIII.) came out, as I said, while my book was passing through the press, and I had neither space nor time for criticism. I merely said that the report did not win me to Dr. Hodgson's opinion, namely, that the "chief communicators" were spirits of the dead, and that I was sure my own dead friends would steer clear of Mrs. Piper. I added that her interest for me was "purely anthropological." If *she* deals with the dead, so, apparently, do similar savage mediums. If the pretence is rejected in their case, it will need a great deal of evidence to prove an exception for Mrs. Piper.

Another example of this argument occurs. Speaking for the "spirit" theory, Dr. Hodgson says (Part XXXIII., p. 396) that the best results are got by treating the communicators as if they really *were* spirits. "We should soothe him . . . express sympathy," and so on. The Chinese know this, and, in using their peculiar planchette, they adulate the communicators, and set out for them wine, fruit and cakes. Even English planchettists (not believing in spirits) address the "communicators" with flattery, and think that the results are improved. Would Dr. Hodgson argue that they and the Chinese are dealing with real spirits? The argument seems as good in one case as in the other,—that is, granting the improvement in results.

So far, then, I think that the "communicators," even the best, do show notes of the dishonest secondary personality of Mrs. Piper. G. P., to be sure, recognises Phinuit as a real entity, a spiritual entity, and surely G. P. should know! But, if so, how can we account for Phinuit's inability to prove that he ever existed on earth, and for his ignorance of French? Dr. Hodgson says that many things are inexplicable by his theory of spirits, just as many "biological incidents" have not been fully explained by the theory of evolution. (Part XXXIII., p. 406). But the things not accounted for by the theory of spirits are obviously of a *morally* suspicious nature, and include a very low moral element. This is not, I am apt to think, the case with the "biological incidents" unexplained; perhaps it could not be. Still, the spiritual *supercheries*, after the fullest allowance for the confusion of a spirit dealing with Mrs. Piper's body, *donnent furieusement à*



*peruser.* Thus it is odd that when asked to describe distant actions by Mrs. Howard, G. P. should go wrong by a day, while he is quite awake to our mere human time when communicating in ordinary sittings (Part XXXIII., pp. 304–308). Perhaps he only succeeds, as is suggested, when people are thinking “strongly and emotionally” of him. (Let us remember this.) Still, why confuse the time? The volubility of G. P. in platitude contrasts again, as I have said, unpleasantly with his accustomed hesitation when asked for a humble test of his identity.

There is another point on which I venture to think that the wonted precision and fulness of Dr. Hodgson’s notes are absent. We need to know, in each case, whether the sitter did, or did not, hold Mrs. Piper’s hand, and we also need to know how long the sitting was in point of time. We must have information as to the chances of muscle-reading.

Very singular are the possibilities of muscle-reading, of which I give an example. Experiments were recently made on a blind, deaf mute, a girl, to see whether she was utterly without hearing. She looked pleased when music was played, annoyed or alarmed when a hideous din was produced. But it was observed that she was then holding the hand of a lady, her instructress. The lady being placed at some distance, no sound that could be made affected the expression of the child’s face. No doubt her sense of touch was exceedingly keen, but so may be Mrs. Piper’s, when entranced and in contact with the hand of her “sitter.”<sup>1</sup>

Though Mr. Podmore accuses me of “liking my psychical bric-à-brac restored and decorated,” whereas Mrs. Piper’s bric-à-brac is “battered and worm-eaten,” I do not feel guilty! Even in a book written only for entertainment (*Book of Dreams and Ghosts*), I investigated historical cases by the aid of MSS. and rare books, and Celtic living tradition. Overcoming the inexpressible tediousness and distastefulness of the task, I have, in fact, several times read carefully through most of the Piper records. I must now confess that, taking the G. P. notes alone, I think there is *prima facie* reason for further inquiry. Dr. Hodgson

<sup>1</sup> The case referred to is that of a deaf, dumb, and blind American child, Helen Keller. See *Mind*, XIV., p. 306. We read, “Whereas Miss Sullivan” (the lady who instructed the child) “had been disposed to credit her with some inexplicable mental faculty, it now appears, on closer observation, that her power of divining the thoughts of those she is with may be wholly explained by her acute perception of their muscular vibrations.” I think I have rather overstated the case, because I originally took it at second-hand from the conversation of a psychological friend, who later supplied the exact reference. This is instructive, but it remains true that the child showed signs of appreciating sounds when she held Miss Sullivan’s hand; and not when Miss Sullivan placed her hands on a table. If Miss Keller is alive and well, experiments *à la* Piper might be made with her, if she entertains no objection.

shows, moreover, that written reports cannot convey the same impression as is produced by years of personal experience; and there must be strong evidence in what overpowers *his* scepticism. Again, we are not permitted to know, as he knows, the most telling evidence, and that must be allowed for in our conclusions.

I do not think that the normal Mrs. Piper makes private inquiries. That hypothesis I gladly discard. In the case of G. P. we learn (Part XXXIII., p. 295) that he died by an accident, in New York, in February, 1892. Mrs. Piper was then in New York, and probably read about the affair (G. P. being, apparently, a more or less notable person), in the newspapers. She could, if she chose, make inquiries; she might hear gossip (though I lay no stress on this), and G. P. did not begin to possess her till several weeks later. But I do not suppose that Mrs. Piper heard of Mr. Hart's studs (once G. P.'s), or of "Katherine," and of circumstances connected with her which Mr. Hart did not know that he knew, and probably never had known. I would incline to explain these things (Part XXXIII., pp. 297-298) by muscle-reading (if Mr. Hart held Mrs. Piper's hand, about which nothing is said), or by telepathy. In M. Bourget's case, his hand was in that of Mrs. Piper, and she took a very long time to give him information which he certainly thought surprising.

Probably Dr. Hodgson ought to record the time occupied in a conversation which may look very short in print, and certainly we should know whether the sitter held Mrs. Piper's hand or not. These facts Dr. Hodgson must have known in M. Bourget's case; my recollection is that M. Bourget told me that he held Mrs. Piper's hand, and that the sitting was fairly long. He remained, even so, unable to explain the results, as he has said in print (*Figaro*, January 14th, 1895; *Proceedings*, Part XXXIII., pp. 495-499).

I recognise the importance of the case of Mr. Hart, Katherine, and G. P.'s studs, and of similar examples.

We now come to the Howards, who did not see Mrs. Piper till about three weeks after Mr. Hart. Here, again, I do not know if these most intimate acquaintances of G. P. held Mrs. Piper's hand during the sittings, nor is the time occupied by the conversations given. These details ought, I venture to think, to be recorded. On the hypothesis of Mrs. Howard's collusion, almost all the mystery vanishes. I believe in no such matter (knowing, as I do, nothing about Mrs. Howard). In no such matter do *I* believe, but it is, to the sceptic, a theory more acceptable than the theory of possession by the soul of a dead man. But, in the case of collusion, Mrs. Howard (Part XXXIII., p. 306) would have told Mrs. Piper what she was going to do on April 28th (when Mrs. Howard was to have a sitting), not what she did on April 27th. It was what she did on April 27th, that G. P. described

as events of April 28th. Dr. Hodgson, in a similar case, suggests that Phinuit was "getting glimpses into the mind of Mrs. Holmes, was reading off some of her past experiences" (Part XXXIV., p. 307). So Mrs. Piper *may* have read in Mrs. Howard's mind, on the 27th, and transferred the results to the 28th, time being perhaps rather mixed to her secondary self. Mrs. Piper's "secondary personality" was as likely to do all this as G. P. was; and *he* is rather less of a *vera causa*. Phinuit, by the way (Part XXXIII., p. 314) does not seem to have known that G. P. knew Greek, and, indeed, in the next world he knows very little.

In all cases, knowledge by G. P. of what occurs here, at a distance from his sitter, seems to be "obtained indirectly and telepathically through the minds of living people, rather than by a direct visual perception, such as we enjoy" (Dr. Hodgson, Part XXXIII., p. 315). If this be true, why suppose a surviving G. P. at all? Mrs. Piper's own "secondary personality" may be able to do all that G. P. (on the animistic hypothesis) is supposed to do,—for example, could reel off, by aid of telepathy, the long private communications to Mr. Howard who was present (Part XXXIII., p. 322). G. P. was strongest with his most intimate friends, and I admit that one does not see why this should be so, if G. P. is only a function of Mrs. Piper's secondary personality. That fact, certainly, seems to be against the hypothesis of mere telepathy between the living. We can scarcely argue that G. P.'s friends only happened to succeed best with Mrs. Piper.

This brings us to the question whether Mrs. Piper's remarkable, though direly interrupted, successes, say with G. P., are due to information from the spirit of that gentleman, or to telepathy on the side of Mrs. Piper, tapping, as it were, the minds not only of living persons beside her, and often known to her, but also of living persons distant, and unknown to her, though known to people in her room. To this obviously wild hypothesis we are almost driven, if we reject the theory of fraud, and the theory of communicating spirits of the dead.

The hypothesis of fraud rests on a *vera causa*; thus we might say that Robert Houdin was bribed to give a false testimonial to the clairvoyance of Alexis Didier. On David Hume's theory, a long set of impostures is the most legitimate explanation of Mrs. Piper's successes. For reasons given by Dr. Hodgson, I cannot accept the theory of imposture by Mrs. Piper, in her normal state. For one thing she could not afford the expense of private inquiries, which would more than swallow up the profits.

Turning, then, to telepathy, Dr. Hodgson argues that there are *limitations*, which gradually disappear, in the news given by persons *recently* dead, and there are limitations at first sittings. (Part



XXXIII., p. 392.) Moreover, good telepathic living agents (as they seem to be) fail constantly with certain dead "communicators." But G. P. was very lucid at first, though recently dead, and dead by an accident. "First sittings," "first communicators," again, may be confused, but the confusion may be due to the living "agents"; in fact, as far as I see, the failure may always be due to living agents, dealing with another living agent, Mrs. Piper. We know so very little about telepathy that we can hardly argue on this side of the matter at all. Possibly I fail to grasp Dr. Hodgson's full meaning in pages 391-393. "That memories of little children recently dead should have a special telepathic agency" seems extremely natural, just as G. P. is said to succeed most when people are thinking "strongly and emotionally" of him. People *do* think "strongly and emotionally" of dead children, and, perhaps, that kind of thought of the living sitter is most readily caught by Mrs. Piper. Just so, if persons who died as children forget childish things, as we are told, that may be because the survivors, after many years, have themselves forgotten them (though the subliminal self never forgets), and cannot wire them to Mrs. Piper by telepathy or muscle-reading. Regarding the effect as if of real traits of the personality of the dead, produced on the living who knew them, it is evidence, I fear, only for the survivors themselves. On the other hand, it is not *like* the late G. P., it was no trait of his, to give false information with *aplomb*, nor was it like the late Mr. H. to repeat, as true, what was only a "natural" guess. Thus the sitter who knew the dead, may think he recognises personal traits. But this may be due to his own memory of these traits, which are telepathically reproduced by Mrs. Piper, or due to self-delusion, and certainly other very important moral traits of the dead are often reversed, and unrecognisable.

We are dealing here with a most imperfectly known agency, telepathy; with a better-known agency, the secondary personality; and with another wholly unknown agency, spirits of the dead. The preference for any of these Laputan alternatives is apt to be decided by personal bias. But, to a faint extent, telepathy has the advantage of being a *vera causa*. The advocates of telepathy, attempting to explain Mrs. Piper's successes, may fall back, as Dr. Hodgson says, on "the hypothesis of telepathy from the living, that the subliminal consciousness of the sitters, and *also of distant living persons*, might be drawn upon by the living." Thus, Mr. Pelham is doing something, in Washington, with a photograph of his son, G. P., and G. P. reports this, at Boston, through Mrs. Piper. The explanation (apart from guess or collusion) will be that Mrs. Piper got at the subliminal consciousness of the remote Mr. Pelham, and so on in other similar cases. Such a telepathic explanation is "to the Greeks foolishness."

Dr. Hodgson (Part XXXIII., p. 393) cites Mr. Oliver Lodge thus: "It ought to be constantly borne in mind that this kind of thought-transference, without consciously active agency, has never been experimentally proved." How can you experiment consciously in the unconscious? But Dr. Hodgson adds that the successes and failures do not show any direct relation to the conscious minds of the sitters. Clairvoyance of Mrs. Piper's, or communication between her and the subliminal self of Mr. Pelham, would cover the case of the photograph at Washington, without calling up the ghost of G. P. But, suppose G. P. communicates to Mrs. Piper what is unknown to the sitter, but known to A. or B. at a distance, who *are* known to the sitter. Can telepathy be forced to cover *that*?

Dr. Hodgson puts the case thus: "We must then make the arbitrary suppositions that Mrs. Piper's percipient personality gets into relation with the minds of distant living persons, (1) who are intimate friends of the sitters, at the time of the sitting" (as in the case of Mr. Hart, who knew the Howards), "and (2) who are scarcely known, or not at all known, to the sitter," but, in one of two cases at least, *were* known to Dr. Hodgson, who was present (Part XXXIII., p. 394, p. 372). All this would imply "a selective and discriminative capacity in Mrs. Piper's percipient personality" (as in the personality of whoever inflicts a telepathic hallucination on a person at a distance), and, widening this theory to cover all cases, "we reach a conception which goes as far as the 'spirit' hypothesis itself."

The conception does go as far,—but not in the same direction. It stretches a more or less accepted hypothesis,—telepathy; it does not introduce an old but discredited hypothesis, the direct action of the dead.

Now the stretching of the telepathic hypothesis was almost forced on me (if I was to have any hypothesis) during Miss Angus's experiments with a glass ball. I presume that these experiments were "experimental," in Mr. Lodge's sense of the word, but I am not certain. (*Making of Religion*, pp. 94–112.) There was in these experiments, apparently, "a selective and discriminative capacity in 'Miss Angus's' percipient personality." But there was no room for the theory of the spirits of the dead, for all concerned were alive. To be sure the Polynesians explain all water-gazing by a theory of spirits, but Dr. Hodgson will not agree with the Polynesians (Ellis, *Polynesian Researches*, II., p. 240).

Again and again Miss Angus, sitting with man or woman, described acquaintances of theirs, but not of hers, in situations not known to the sitters, but proved to be true to fact. Now the "far-going" hypothesis of direct clairvoyance was here excluded (in most cases, not all) by conditions of time. In one instance Miss Angus described

doings from three weeks to a fortnight old, of people in India, people whom she had never seen or heard of, but who were known to her "sitter." Her account, given on a Saturday, was corroborated by a letter from India which arrived next day, Sunday. In another case she described (about 10 p.m.) what a lady, not known to her, but the daughter of a matron present (who was *not* the sitter), had been doing about 4 p.m. on the same day. What the person seen was doing was *not* a thing familiar, for I asked that question. Again, "sitting" with one lady, Miss Angus described a singular set of scenes much in the mind, not of her "sitter," but of a very unsympathetic stranger, who was reading a book at the other end of the room. I have tried every hypothesis, normal and not so normal, to account for these and analogous performances of Miss Angus. There was, in the Indian and other cases, no physical possibility of collusion; chance coincidence did not seem adequate; ghosts were out of the question, so was direct clairvoyance. That Miss Angus (who, by the way, was in the most normal and wide-awake condition) had got into touch with the Absolute, and was making discriminating selections from the stores of Omniscience, did not seem likely, because her crystal pictures appeared to be directed by the mind of a person present, not always the sitter. Nothing remained for the speculative theoriser but the idea of cross currents of telepathy, between Miss Angus, a casual stranger, the sitters, and people far away, known to the sitters or the stranger, but unknown to Miss Angus. Unpublished examples of these things went on the same lines. Miss Angus picked up facts, unknown to the sitters, about people known to them but not to her.

Now, suppose that Miss Angus, instead of dealing with living people, by way of visions, had dealt by way of voice, or automatic handwriting, and had introduced a dead "communicator." Then she would have been on a par with Mrs. Piper, yet with no aid from the dead. Her cases do not differ from Mrs. Piper's cases, except in copiousness, and in the circumstance that her condition was normal, and that she was new to all such exercises. Of course, like Mrs. Piper, she had failures. I asked her to try to see the room of a person known to me by correspondence only, a person whom I never met (it was a room in Africa, though of course I did not say so), and she failed. It was trying her rather high. We did not seek to improve the result by exclaiming "Dear Tom, Dick, or Harry, in Bengal, Edinburgh, or the Soudan, or the Red Sea, *do* try to appear more frequently in the glass," as Dr. Hodgson addresses the dead "communicators." We could not do that, because the essence of the game lay in Miss Angus's ignorance about Dick, Harry, and Tom, who were kept private in the mind of the sitter. Otherwise the performances of Mrs. Piper and Miss Angus



were on a par (except for the deadness of the persons concerned), granting the difference of the methods of crystal-gazing, on one hand, and of trance-speaking, or automatic writing, on the other.

Not to rely solely on Miss Angus, I take another instance. My friend Mr. Lesley is known to the world as a man of business, a golfer, and a composer. He can see crystal-pictures, but (like most of my acquaintances who possess the faculty, including my cook) has hardly any interest in the practice. One day Mr. Lesley and I had been talking about a lady, unknown to him, but known to me, though I had never seen her house. Mr. Lesley began to look into a glass water-jug, and described what he saw, the interior of the hall of a house, with a good deal of detail. Neither of us recognised the house. I happened later to tell this to the lady of whom we had been talking; she said, "Why, that is *my* house," and, on visiting it, I found that in all respects it answered to Mr. Lesley's description. It may be a common type of hall, but I do not remember having seen one like it elsewhere, nor did Mr. Lesley know any such place.

Now, suppose that the lady who occupied the house had been dead. And suppose that, instead of looking at a glass water-jug, Mr. Lesley had gone into a trance, and announced that the dead lady was speaking with his voice. Suppose that when asked for a test she had described the hall in her house (which was unknown to me and Mr. Lesley) with certain curious details. Would not Dr. Hodgson argue that this might be better explained by the hypothesis of communication through her spirit than by telepathy between Mr. Lesley and anybody not present who knew the house? Yet, as its owner was and is alive, the theory of a spirit is wholly impossible, and if not telepathy *à trois*, then some other non-spiritualist theory must account for the facts, as for the facts in Miss Angus's cases.

Miss Angus's successes may not be due to cross-telepathy, nor may Mr. Lesley's success; very likely that is the wrong explanation. But of all known "supernormal" explanations, that alone is *viable*, in these instances, and it is not, I think, incapable of application to Mrs. Piper's cases. Of course I do not reject the explanation by spirits, in Mrs. Piper's case; I only state the objections which occur to me, combined with the fact that Mrs. Piper is saturated with the animistic hypothesis, and has a dishonest secondary personality, if not dozens of such personalities. In Miss Angus's performances Mr. Podmore suggests (Part XXXIV., p. 130) my own provisional guess of telepathy *à trois*. It is a guess, even a wild Laputan conjecture. But we are here concerned with Laputan themes and speculations; like Mr. Darwin, we are making "fools' experiments." Dr. Hodgson's hypothesis may be right; but, in this region of dreams, we ought to hold very lightly by all hypotheses; and, surely, we ought not to

argue from one of them in favour of that old belief, the posthumous existence of the human spirit, and its power of communicating with the living, through a living organism. This is to base faith on a conjecture about conjectures. Moreover, in ordinary normal material, such as philological or anthropological speculation, we often see how science overshoots her mark, remaining for a generation in sure confidence about a theory which the next generation explodes. We ought not to let our psychical theories affect our practical beliefs. To do that may be to prepare for ourselves, or for our successors, a cruel disappointment.

My objections are tendered with the consciousness that Mrs. Piper's communications, to Mr. Howard, for example, are far more copious and minute than the visions of the crystal ball gazer. But the two sets of automatisms have common features in the way of picking up knowledge, not, in many cases, possessed by the sitter, though familiar to some other living person. And as spirits can (I suppose) have nothing to do with this in the crystal cases, they *may* have nothing to do with it in that of Mrs. Piper. Moreover, the visions in the glass ball did not play tricks, or give false information. You, the sitter, might think of Tom or Dick, and nothing might appear at all in the glass, or something quite unknown to everybody might appear. But Dick or Tom were not seen in situations which they never occupied, or in actions which they never attempted, except perhaps in two cases, apparently suggested by the expectations of the sitters. Possibly these cases of cross-telepathy may be to the point. On Dr. Hodgson's side, Mrs. Piper's re-creation of a personality, such as G. P., for example, is, at his best, seems indeed a very difficult argument for an objector to meet. Moreover, the conceivable "form of consciousness" of Mrs. Piper, "like a dome of many-coloured glass," with colours corresponding, more or less, to many personalities of the dead, is indeed "very much an unknown quantity." Alas, the whole problem is a series of unknown quantities! We remain none the less grateful to Dr. Hodgson for such disinterested labour in a region which Professor Shaler, most naturally, found uninteresting, because "I don't see how I can exclude the hypothesis of fraud." Frauds by secondary personalities, I fear, can hardly be excluded. We are like Porphyry, so many centuries ago, in his wrestlings with "lying spirits." But, unlike Porphyry, Dr. Hodgson has not been discouraged.

Since writing this paper I have been allowed to see in proof Mrs. Henry Sidgwick's "Discussion of the Trance Phenomena of Mrs. Piper."<sup>1</sup> We agree as to the notes of the "secondary personality" of Mrs. Piper among her professed communications from

<sup>1</sup> That is, the paper printed above, immediately preceding Mr. Lang's.—ED.

the dead. We agree in not accepting the savage theory of "possession," at least at present. But granting telepathy *à trois* (which is a great deal to grant), I see no reason for the hypothesis that Mrs. Piper ever receives telepathic communications from the dead. Has she ever communicated a single thing that was known to a dead person, but to no living man or woman? Such are my doubts; my bias is a desire not to believe that the dead are in any way mixed up with sittings at so many dollars. It would not surprise me if some normal explanation (at which I cannot guess) were finally to be found for even the most puzzling of all these phenomena. I am not quite such a failure as Mrs. Sidgwick is personally as an intermediary in these experiments. Twice my thought has (apparently, and in the absence of any other hypothesis known to me) been "picked up" by an experimenter, and in neither case was it my surface thought. These things bias one in favour of the belief that there is something here into which it may not be waste of time to inquire.



## IV.

ON SOME PHILOSOPHIC ASSUMPTIONS IN THE  
INVESTIGATION OF THE PROBLEM OF A FUTURE LIFE.

BY F. C. S. SCHILLER.

There is always a flavour of impertinence about the intervention of a philosopher in a subject of scientific research. He cannot, as such, make original contributions to the facts, and when he makes an attempt to criticise the contributions of others, he is terribly prone to do so from the *a priori* basis of some far-fetched cosmic theory which nobody else in the world besides himself believes in or even understands, and so becomes comic instead of cosmic. If, again, he contents himself with ponderously pondering on the accepted facts of a science he becomes a bore, consuming time and getting into the way of more practical workers.

It seems clear, therefore, that the usefulness of a philosopher is very limited. It is undeniable only in cases where he is needed to clear out of the way other philosophers who have become obstreperous and obstructive; but for the moment there did not seem to be anybody in sight whom the spirit had moved to vilipend Psychological Research *a priori*, and who, therefore, required to have the error of his ways brought home to him in technical language.

Hence I was somewhat at a loss to see why the Council of the S.P.R. should have deemed it expedient to request me (at very short notice) to write a paper, the purely theoretical character and hurried composition of which demand, I feel, the most abject apology. Indeed, I could not have been induced to say anything at all, but for the fact that philosophy seems to me to have also a function, not yet mentioned, which may enable it to be scientifically suggestive and serviceable, at all events at a certain stage in the development of a science.

The function in question is that of discussing the working methods of a science, of exhibiting their full scope and logical implications and connections, and considering the merits of the alternative ways of treating the subject. Such a critical *methodology* of a science is necessarily dull, but, perhaps, on that account, all the better adapted for philosophic discourse. And in view of the intellectual myopia which scientific specialism engenders, there are, perhaps, few things more salutary, as an unpleasant medicine is salutary, than for a

science to become conscious of the working assumptions, or, if you will permit me so to call them, the methodological postulates, on which it proceeds.

In the case of Psychological Research, in particular, the discussion of such methodological assumptions seems to be more novel, easier and more useful than in disciplines which have reached a more assured position among the sciences. It is likely to be more novel, because of the novelty of the whole subject. It is likely to be easier to dissect out and contemplate in abstraction the methodological assumptions of an inchoate and infant science, because its organism is not so strongly knit and the flesh of fact does not so closely shroud the bone of method by which it is supported; it is still in a low stage of organisation in which the whole may be taken to pieces and put together without much injury to the vitality of its parts. An advanced science on the other hand is far more difficult to handle: it imposes on the philosophic critic by its very mass of coherent and consistent interpretation; it appeals to him by its noble record of service to the human race; it crushes him by the sheer weight of immemorial authority. In its facts and theories have long been welded together into so indissoluble a union that the former can no longer be questioned, while the latter have for the most part risen to the dignity of indispensable 'necessary truths' implied in the very nature of the human mind and underlying the whole structure of human knowledge. We gain little help therefore from the assumptions of sciences like mathematics and mechanics in considering what assumptions should be made in a new subject like Psychological Research; we learn little about the making of a science from sciences which can neither be unmade nor remade, and in whose case it requires a considerable effort of philosophic thought to realise the methodological character of their fundamental postulates. More might perhaps be learnt from the assumptions of *parvenu* sciences which have but recently obtained full recognition, but for the fact that a critical dissection of their methods is decidedly dangerous. For the "*arbor scientiae*" seems in their case to have developed a symbiotic arrangement greatly resembling that whereby certain trees protect themselves; just as any attack on the latter is ferociously resented by a host of ants which the tree provides with food and shelter, so any interference with such a science is sure to draw down upon the mildest critic the onslaught of an infuriated professor who lives upon the science. In Psychological Research, on the other hand, no such danger is to be apprehended; we have not yet developed any professionals whose mission it is, as William James has wittily remarked,<sup>1</sup> to kill out the layman's general interest in the

<sup>1</sup> *Human Immortality, init.*

subject, and hence the philosopher may proceed at his leisure to observe how the science is made and to try instructive experiments with its working methods, without fear of offending vested interests.

Again a philosophic discussion of possible methods is likely to be more useful in Psychical Research because such methods are still plastic cartilage, as it were, which has not yet grown into rigid bone, and may be moulded into a variety of forms. Hence by reflecting betimes upon the advantages of alternative methods, the philosopher may flatter himself that he can be of real service in guiding the course of investigation, or at least in helping it to avoid certain pitfalls. Not, of course, that even here he would be wise to presume to lay down the law *a priori* as to the actual working and merits of the various methods; he should content himself with expounding the logical characteristics which sound methods in Psychical Research must possess, and explaining why exactly they must possess them.

I do not propose, however, on this occasion to discuss the methodological value of the assumptions made in Psychical Research generally, but only in so far as they affect the question of a future life. The reasons for this are obvious. The possibility of a future life provides the motive force in our inquiries. It is true that the S.P.R. is unique in aiming to solve this problem in a scientific way, but though we are scientific, we may yet be honest—in avowing the existence of a practical motive. If attacked on this score, let us meet our critics with the doctrine that in this respect at least we are *not* unique, inasmuch as in the end all true science is inspired by practical motives, and that it is the fear, no less than the hope, of a future life that renders its possibility so urgent a subject for scientific consideration. Moreover, just now the evidence in connection with Mrs. Piper's trances seems to have brought this possibility well above the horizon of the S.P.R., while at the same time much confusion and prejudice still seem to prevail about it which philosophic criticism may help to dissipate.

I may begin by passing over with a merely formal mention the assumptions which are required for every scientific investigation. As a matter of course we must assume that the phenomena under investigation are knowable and rational in the sense of being amenable to determinable laws. The need for this assumption is so plain that *a priori* attacks on Psychical Research on the score of undermining the fundamental principle of all scientific research can hardly be put down to anything but voluntary or involuntary ignorance of the grossest kind.

Next we must enunciate a methodological axiom with which at first sight few will be disposed to quarrel, viz., that we must proceed to



the unknown from what is known to us. The remark is Aristotle's,<sup>1</sup> and I may be suspected of quoting it merely because Oxonians can but rarely resist the temptation of quoting Aristotle. But in reality it is not such a truism as it appears, at least in the meaning I propose to put upon it. It means in this connection that, both psychologically and logically, we must interpret any supposed future life by the knowledge we have acquired of our present life. It is a methodological necessity, in other words, that we must project this world into the next, if ever we purpose scientifically to know it. Our assumption may be wrong in the sense that it may be wrecked on barrier reefs of impenetrable fact—possibly it will be—but, right or wrong, we can work with no other at the outset. As we go on we shall no doubt detect the initial crudities of our assumptions, and correct them as our knowledge grows. But whatever differences we may discover between the two worlds must rest upon the postulate of a fundamental identity, in default of which our reason would be merely paralysed. From a complete otherness of the other world nothing would follow; a future life in which everything was utterly different would mean nothing to us, and in proportion as the difference grows the practical efficacy and theoretical knowableness of the conception diminish.

Now this, I venture to think, is a philosophic result of considerable practical importance.

(1) It goes a long way towards explaining the anomaly of the feebleness of most people's religious beliefs about the future life. For the heavens and hells of the various religions, in spite of their pretensions to evoke forces which should utterly dwarf the three-score years and ten of our mortal life, are found in practice to constitute motives so weak that they are continually routed and set aside by the trivial temptations of the moment. The reason is that they have ordinarily been conceived as differing too radically from the known conditions of life to excite the same serious belief, to require the same matter-of-fact forethought as, *e.g.*, next year's crops or to-morrow's money market. And so the belief in a future life, even where it has not been degraded into a merely verbal assent to a traditional formula, has commonly lacked that intimacy of association with the ordinary concerns of life which is needed to render it psychologically efficacious as a motive to action.

(2) Again, it turns out that the spiritists were by no means wrong in principle when they proceeded to construe the future life, of which they believed themselves to possess cogent evidence, very much on the lines of our earthly life. Their constructions may in detail be as crude and absurd as their adversaries allege—I am neither familiar enough

<sup>1</sup> Eth. Nich. i, 3, 5.

with the literature to discuss this point nor convinced that they are—but it is a mistaken prejudice to reject such accounts *a priori* as too trivial or undignified to be ascribed to the inhabitants of another world. Owing, no doubt, to the unduly tragic view we have come to take of death, the prejudice that the decease of Brown, Jones and Robinson must instantly transmute them into beings of superhuman powers and tastes, and transport them into regions where they are initiated into the uttermost ecstasies and agonies of the scheme of things, has become almost universal. Indeed, I have often been amused to see how strongly this notion influences people who are really entire disbelievers in the possibility of any future life; while scorning everything ‘supernatural,’ they reject the spiritist’s version thereof as *not supernatural enough*, because they are quite sure that if there were a future life at all, it would have to be as full of angels and demons as what they would call ‘the traditional mythologies.’ In a more respectable form the same feeling shows itself in the large number of persons who refuse to accept the evidence, *e.g.*, in the Piper case, because they do not like the sort of life to which it seems to point. This may seem a somewhat naïve *ignoratio elenchi*, but the psychical researcher can hardly afford to smile at it, for he is continually having it impressed upon him how very serious are the obstacles which prejudices of this sort form to the discovery and recognition of the facts, and how manifestly the ‘will to believe’ is the *ratio cognoscendi* of truth. Hence a systematic challenge of the whole assumption that another world must be as different as is conceivable (or rather inconceivable) from this, may serve to clear the atmosphere.

And inasmuch as the groundlessness of a false assumption is never revealed more clearly than by a request for the reasons on which it rests, I should like, for my own part, to add to the general challenge a particular request, asking philosophers to show cause why a hypothetical ‘other’ world must necessarily be conceived as out of time and out of space. The conviction that this must be so underlies, I am sure, much of the high philosophic scorn of empirical spiritism and popular theology, but I do not think it would be easy to support it by a valid and cogent philosophic argument. For so long as temporality and spatiality form indispensable characteristics of the only world we experience, the presumption surely is that they will pervade also any other, until at least a definite method has been suggested whereby they may be transcended.

(3) Thirdly, it must be recognised that the methodological principle of interpreting the unknown by the known tells strongly in favour of the simpler, and *primâ facie* easier, theory of the agency of personal spirits as against the more complex and unfamiliar notions of an impersonal clairvoyance, or subliminal consciousness, or non-human

modes of cognition by gods, devils or cosmic principles of a more or less unknowable kind. I am very far from thinking that we should in such matters hastily commit ourselves to the interpretation which *prima facie* seems the most plausible, or, indeed, to any definitive theory whatsoever, and I should be sorry to see the ingenious attempts to provide a non-spiritistic explanation of the phenomena in question prematurely abandoned—if only on account of their excellence as mental gymnastics—but I cannot admit that such attempts are one whit less anthropomorphic in principle than the ‘spiritist’ hypothesis (they only stray further from their human model), while I cannot help admitting that methodologically they are more cumbrous and so considerably inferior.

Admitting, therefore, that as a working theory the hypothesis of the persistence after death of what we call the human personality possesses considerable advantages over rival theories, let us inquire further by what methods, resting on what postulates, that theory may be verified.

(1) We may rule out once more the notion that such a future life is essentially supernatural in character. This notion has been a favourite with believers, but it is easily turned into a terrible weapon in the hands of their adversaries. For the supernatural is, as such, incapable of investigation, and belief in it must be mere faith, exposed to every doubt and jeer, if, indeed, it can be even that, seeing that a real faith must be nourished by at least partial and prospective verification in fact. Hence the answer to this notion is simply this: that if the future life be really ‘supernatural’ in the sense of having no connection of any sort with nature, there could not possibly be any evidence of it, and it would have to be for us non-existent; while if there be evidence of it, this would *ipso facto* include it in the widest conception of nature, and render the nature of the connection between this world and the next a legitimate subject for scientific research. If, therefore, the connection be rare and precarious, the reason cannot possibly be that from time to time some audacious spirit has impiously achieved the impossible by breaking through the natural order; it must be in the peculiarities of the natural order itself. Or, to sum up in a single phrase a discussion which would long have become needless but for the persistence of attempts to dispose of an inconvenient investigation of facts by logical quibbles about words, if ‘supernature’ is to be retained, it must not be in the sense of something alien and hostile to ‘nature,’ but strictly as meaning a higher department or aspect of nature itself.

(2) We must suppose a certain continuity of psychological constitution in the human spirit throughout every phase of its existence. This does not imply that death may not be a great event, involving a



great gain (or loss) in the intensity and extent of consciousness and memory ; it asserts only that if we are to have knowledge of a future life at all, we must assume that the general characteristics of mental life will persist. Without this there could be no proof of 'spirit-identity' ; without 'spirit-identity' there could be no proof of a future life. Unfortunately, however, this assumption of ours would lead us to expect that the proof of 'spirit identity' would be difficult. For it is psychologically far more probable that the moral character and the feelings would traverse the shock and change of death unshaken, than that little bits of knowledge about terrestrial affairs would persist in equal measure. Yet it is these latter that afford the best tests of 'spirit-identity.'

(3) As we must try to explain all the facts by principles already known to be valid, we must account for the remarkable dissociation between this world and the next by the principle of psychological continuity. That such dissociation must exist will hardly be denied by any one who has realised how very rare an experience a 'ghost' is, even with the most expert of ghost seers and in its most favoured haunts. But it would seem that if the departed still retained their personality and psychical continuity, 'ghosts' ought to be more plentiful than blackberries, and unhedged by that divinity which makes people so reluctant to make a clean breast of their ghost stories. *Prima facie*, therefore, it requires explanation that in spite of psychic continuity so much dissociation should prevail.

Nevertheless it may, I think, be shown that the assumption of psychical continuity would be quite compatible with the prevalence of an almost complete dissociation between this world and the next. For any great event tends to dissociate us from our past, and this would apply *a fortiori* to an event like death, which *ex hypothesi* launches us into a new world. A new world, moreover, would engross us not only by its novelty, but also by the practical need of accommodating ourselves to new conditions of existence. Hence the psychological conditions for great concern about the world we had left behind us would hardly be present. This argument, moreover, could be considerably strengthened by psychological observations with regard to the interest which is taken in the affairs of our world by the aged. For it would be unlikely that an interest which had already grown faint should effectively maintain itself amid the distractions of a new life.

And even if the desire to communicate were felt, it could hardly be assumed that the knowledge and power to do so would at once be at the disposal of the new-comer, who, for aught we know, might find that, as upon his entry upon this scene, a period of helplessness and dependence analogous to infancy had to be passed through.

It would seem probable, therefore, that to render communication effective, quite as systematic and sustained an effort would be needed on the other side as is being made by the S.P.R. on this, while the self-regarding motives for making it would be indefinitely less potent. For while each of us *ought to have*<sup>1</sup> a strong personal interest in determining what his prospects may be after death, no such case could be made out for a retrospective interest of the departed in our world. And in their world the prevalent sentiment might esteem it better to leave us in our present doubt and discourage attempts to pry into the possibilities of communication with another world still more strongly and sincerely than the majority of us do now. This, indeed, will appear the more probable when we remember that, according to the principle of psychic continuity, the *same* people will be making the same sentiment in both cases. Nevertheless, it is conceivable that some day a fortunate coincidence of the efforts of an infinitesimal minority on both sides should succeed in establishing spirit-identity and forcing upon the reluctant masses of men the scientific fact of a future life which they did not in the least desire to have so established. Even then, however, we should still be very far from any definite and detailed knowledge of the nature of the future life in itself, the difficulties of transmitting which would increase enormously in proportion as the dissociation between the two spheres of existence became greater.

Thus the general upshot of our discussion so far would be that a future life which was accessible to scientific methods of proof would necessarily appear to be of a somewhat homely and humdrum character. Broadly speaking, our conceptions of it would rest on the assumption of social and psychic continuity, and they would tend to suppose that the reward and punishment of the soul consisted mainly in its continuing to be itself, with its true nature revealed more and more clearly to itself and others. Hence there would be but little room for epic flights of a lurid imagination, and those who hanker after the ecstasies of the blessed and the torments of the damned would have to go, as before, to the preachers and the poets. We may, however, trust these latter to work up a more copious material into pictures quite as edifying and thrilling as those of Homer, Dante and Milton.

I have assumed hitherto, without a hint of doubt, the general possibility of the conception of a future life. But, after all, this also is an assumption, of a very vital character, and one which has been strongly impugned on *a priori* grounds. I shall devote, therefore, my

<sup>1</sup> I emphasise the 'ought,' for, as a matter of empirical fact, the present number of those who are *scientifically* interested in the question to the extent of a guinea per annum appears to be about 1500 !

concluding remarks to disposing of such philosophic attempts at an *a priori* suppression of the question and to stating some of the philosophic considerations which lead me to think the conception of a future life a valid and non-contradictory one, whether or not we can find empirical evidence of its actual existence. On the first point I may be brief: I should not deny that it is possible to devise metaphysical systems which will render the persistence of the individual consciousness improbable and even impossible, and which consequently close the question to all who really believe them. Personally, I believe those systems to be demonstrably wrong, but it is enough for our purpose that they should be gratuitous, and that we may, at least equally well, adopt metaphysical views which leave the question open, or even lead us to regard a future life as *a priori* probable enough, and needing only verification *a posteriori*.

Hence, speaking for myself—and in so personal a matter one must speak for oneself if one wants to speak to the point—I cannot at all appreciate the enormous antecedent difficulty which so many philosophers seem to feel about the conception of a future life. Even its most difficult implications, like, *e.g.*, the transition from one world to another, seem to become quite easy, if we start from the proper philosophic basis. Let us, for instance, assume—as I think we must do in any case—the philosophic position of an idealistic experientialism. I use this clumsy phrase to designate the view that ‘the world’ is primarily ‘my experience,’ *plus* (secondarily) the supplementings of that experience which its nature renders it necessary to assume, such as, *e.g.*, other persons and a ‘real’ material world. In that case the world, in which we suppose ourselves to be, is, and always remains, relative to the experience which we seek to interpret by it, and if that experience were to change, so necessarily would our ‘real’ world. Its reality was guaranteed to it, so long as it did its work and explained our experience; it is abrogated so soon as it ceases to do so. Hence we may conceive ourselves as passing through any number of worlds, separated from each other by (partial) discontinuities in our experience, each of which would be perfectly real while it lasted, and yet would have to be declared unreal from a higher and clearer point of view.

Nor would this conception remain an empty form, which we could not find anything in our experience to illustrate. I venture to affirm that we are all of us perfectly familiar with what it feels like to pass from one world into another. When we fall asleep and *dream*, we pass into a new world, the space, time, persons, and laws of which, *though they persist in principle*, have no very obvious connection with the corresponding characteristics of our waking life. It is true that the reality of each dream-world is very precarious: it is dissolved by every



clumsy interruption from a more 'real' world, in the *ex post facto* judgment of which the dream-world is fleeting, chaotic, and unmanageable. Hence the consensus of common sense declares dream experiences to be unreal—though, it may be noted, it has taken men a long time to arrive at this conclusion and to disabuse themselves of the notion that after all there must be a literally veridical and inspired meaning in all their experiences. What has not been realised with equal clearness—probably because the observation seemed to have no direct practical bearing—is that the existence of unreal worlds of dream-experience casts an indelible slur on the claim of our present waking life to absolute reality. What has happened once may happen again, and when we wake to another world our terrestrial life may appear as grotesque a parody, as misleading a distortion, of true reality as the most preposterous of dreams.

Now this, which may be regarded as an awakening of the soul from the point of view of the subject of the experience, is at the same time the dissolution of his dream-world and life. That is, from the point of view of the beings that interacted with him in that world and life—supposing, of course, that his 'unreal' experience was, as even dreams are, veridical to some extent—it is his withdrawal, his *death*. Hence the "paradoxes about death" which I have already had the honour of propounding to the members of the S.P.R.,<sup>1</sup> and to which I am now trying to supply a sort of commentary. "No man ever yet perished without annihilating also the world in which he lived," *i.e.*, the world of *his* experience, or as we may perhaps say with still more accuracy, the objective world, in so far as it was assumed to explain *his* experience. Moreover "no man ever yet saw another die, but if he had he would have witnessed his own annihilation": inasmuch as he could never see the other's self and so *a fortiori* could not observe its destruction; what he saw was the 'death' of a 'body' which was merely a phenomenon in his own world of experience. But if, *per impossibile*, he could have witnessed the destruction of the subject of a world of experience, his own destruction, as a phenomenon in such a world, would have been included in the catastrophe. Thus both these paradoxes are designed to bring out the essential and incurable philosophical ambiguity of 'death.'<sup>2</sup> Death is not the same thing for him who experiences and for him who witnesses it. It forms the limiting case which involves the breakdown of the great social convention, whereby we postulate (for practical purposes) a common world which is experienced by us all.<sup>3</sup> Even

<sup>1</sup> In the S.P.R. *Journal* for March, 1898, Vol. VIII., p. 204.

<sup>2</sup> Cf. also *Riddles of the Sphinx*, Ch. XI., §8.

<sup>3</sup> "The world is the greatest of all conventions, but all are unconventional enough to leave it."—S.P.R. *Journal*, March, 1898.

during life that convention is maintained only at the cost of excluding from reality all such experiences as are personal, or divergent, or incapable of forming a basis for common action. At death it breaks down altogether, and the long suppressed divergence between the world of 'my' experience and the 'objective' world, which is nobody's experience but is supposed to account for everybody's, dominates the situation.

When a man dies his relation to the common world apparently ceases,<sup>1</sup> but for what reason we cannot say. It may be that he has ceased to be; it may also be that he has ceased to interact with us—until we also have followed his example. Similarly when we witness a death, all that we can safely and scientifically say is that a peculiar feature in our experience which impelled us to assume a self-conscious spirit, analogous to our own, in order to account for the behaviour of the complex of phenomena we called the body of our fellow man, has undergone a change such that the behaviour of his 'body' no longer warrants the inference of the presence of his 'spirit.' Again, the reason may be that the spirit is destroyed, or that it has ceased to animate the 'body.' Thus it would seem as though all that could be affirmed for certain about death was that it was a disruption of the common world in which spirits acted together; what else or what more it was would remain in doubt—the spirit may have perished or it may have 'passed away.'

Thus, so far as philosophy can determine, it would seem as if the chances of destruction and survival were exactly equal and that we were doomed to doubt for ever. Nevertheless, considerations may be adduced which must add decisively to the weight of the latter alternative. For it should be noted that the two alternatives are not equally well situated with respect to empirical evidence. No conceivable empirical evidence can suffice to establish the destruction of the soul at death, because none can even be relevant to the issue. For it can only concern appearances in the common world of the survivors, it can only prove that the rupture of connection with it at death is utter and entire. But that is not enough. A scientific proof, therefore, of the annihilation of the soul is rigorously impossible. On the other hand, there is no such intrinsic impossibility about a scientific proof of the persistence of consciousness through death; there is, in fact, no particular difficulty about conceiving empirical evidence sufficient to establish this doctrine with as high a degree of certainty as we have for any of our beliefs as to matters of fact. The whole difficulty consists in getting the evidence. If we had

<sup>1</sup> "To die is to cut off our connection with our friends; but do they cut us, or we them, or both, or neither?"—*Ibid.*, p. 205.

succeeded, all we should need to do would be to modify our original assumption that death meant an *absolute* rupture of relations, an utter dissolution of the common world. We should have to say instead, that death altered the mode of communication of spirit with spirit, rendering it different and difficult without interrupting it altogether. But, properly interpreted and manipulated, the common world would persist through death. What exactly would be the nature of the common world, thus extended to include a life after death, philosophy could not, of course, forecast; that would remain a question for positive research to determine.

Hence it is manifest that I have reached a point at which the philosopher must modestly stand aside; he can reassure the psychical researcher that no *a priori* impossibilities block his way and that no authentic fact can be too anomalous to be explained; he can clear his eyes of the prejudices which obstruct a clear vision of the goal, but its actual achievement must be the reward of the courage and endurance of the empirical researcher who fights his way to it step by step through thickets of complex and perplexing fact, and barbed-wire entanglements of hostile human prejudice.



## SUPPLEMENT.

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### I.

#### A STUDY IN SPIRITISTIC HALLUCINATIONS.

BY PROFESSOR HARLOW GALE.

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Dr. E. S. was a physician in St. Paul, Minn. with a special reputation for success in diphtheria, a graduate in 1872 of Trinity College, Hartford, and of Rush Medical College, Chicago ; an unusually vigorous, decisive, and intelligent man. He was a disbeliever in religion and somewhat offensively so. He became associated with people below his own intellectual and social grade, but not to the vicious extent which he was subsequently fond of exaggerating. When thus about 45 years of age, with grey hair and beard, and still a bachelor, he experienced in 1894 a sudden and dramatic conversion to "Christian Spiritualism." He then gave up all practice and interest in his profession and in the real estate business to which he had chiefly given his time for several years, and devoted himself with intense single-mindedness and enthusiasm solely to the propagation of this faith. With a small independent income, he did not make it a money business nor himself a professional, but in shabby clothes and with the barest necessities of life he gathered a little group of converts about him. His time was occupied in visiting these friends, studying his Bible and reading, and holding meetings three times a week in his room in a business block.

In about two years he came into communication<sup>1</sup> with Dr. Hodgson, to whom he described himself and his work in the following characteristic letter :—

329½, Jackson Street, St. Paul, Minn., *May 7th*, 1896.

(Dr.) (I presume you do not care for title) RICHARD HODGSON,  
5, Boylston Place, Boston, Mass.

My dear Doctor :—Your esteemed favour of the 5th inst. duly received. In reply will try to be brief. In the first place I wish to avoid all unnecessary notoriety. It will be impossible to condense more than two years' extensive experiences, and the accounts of hundreds of meetings, into a single letter. Suffice it to say I am a regular physician, graduated in 1876, at Rush

<sup>1</sup> This first communication was a postal card as follows :—

329½, Jackson-street, St. Paul, Minn., *April 30th*, 1896.

My dear Sir,—You, if still interested in your great work for your Society, and care for them, will—D.V.—receive some interesting communications, upon my assurance of such being welcome from me, and of more definite knowledge of your address.  
Yours,

Dr. E. S.

Medical College ; am a graduate of an Eastern Institution ; am now forty odd years of age ; brought up a Christian, but from certain disbeliefs and teachings of the so-called churches, I became, if not an infidel—I think, while by no means an atheist, I was an infidel in belief, and certainly in practice and life—but heard voices once and a while, and saw clairvoyantly, but did not understand these manifestations, [nor] try to bother with them, and while they lasted —“ annoyed me ” as I used to think—I fancied I was on the verge of insanity : finally I attended a spiritualist meeting with much cowardice and shamefacedness, saw what they called, and what I now know were materialisations, and could get when alone table rappings, table-tippings, and see clairvoyantly. But the table rappings and table-tippings did not continue long. I finally met a business-man, who was a friend, and he reluctantly told me he had seen Our Saviour many times, and that he saw and communicated with his late wife by asking God, Our Father, in the name of Jesus Christ, if he might be permitted to do so. His late wife was a devout Christian. I shook hands with him and said I would try it that night. I wanted to see my mother whose body had been buried many years before. I prayed The Father, in the name of Jesus Christ, if it was in accordance with His Will, not to her injury, peace or discomfort, and for my good, that I might see my mother. I was all alone in my then bed-room, doors locked. I saw her immediately, fully recognised her and was very thankful for the vision. Then I asked God in Christ Jesus’ name to let me know the truth as He willed, notwithstanding my former infidelity, so-called science, lies of priests, preachers, or myself, or any other sources of errors. I began to see more clairvoyantly ; in daylight, and to write automatically, and these matters were often (or rather never found not to be true) proven true, with reference to past events or future events. Never once untrue ; often true. I began holding private meetings with my clairvoyant friend, who had for over twenty years, at different times, seen Jesus Christ. At first we did not see Him ; but our own friends, and many of the departed members of our families came, and gradually brought, introduced, and made us acquainted with persons of hundreds, nay, thousands of years previously existent in the flesh. They talked, my friend could hear and distinguish their different voices, etc. Finally the Master came, and has come very often to our meetings now for over six months. He has cured one man who tells me he has been “doctored ” for over ten years for progressive locomotor-ataxia, and by many physicians. I have taken the Master by His substantial Spiritual Hand, hear Him talk to me often, and hear many others. Now the Master has now been seen by twelve different people ; some have seen Him many times. We are informed comparatively few will see Him before He comes at the beginning of the reign of a thousand years, which coming, we believe and have been informed, would be a spiritual coming, at least, to the hearts and minds of human beings. We use no curtain. The Master forbids it. We gather to worship Him, have seen Him accompanied by the Blessed Virgin, Mary Magdalene, Martha, Lazarus, all the Apostles and many, many others. Several times by angels (who never were in the flesh), His messengers. The man whom he cured was cured instantly, nothing was said by the Master about curing him. When the man went away from the meeting he could walk as well as ever, and had no pain.

Four besides himself were at the meeting ; the cured man and myself only saw the Master. The Master spoke briefly to me ; some time to this man, who said he heard Him, saw Him, and felt Him, and His hands. I saw and heard Him. The four present (besides the sick man) felt the sick man, who while mentally happy, was wet and cold. A Dr. Dunlap has seen The Master and others, —Abraham Lincoln one of them—at our meetings. He only saw the Master once, and briefly. Some of us have seen the Master very many times, and for—sometimes—not only half-an-hour, but even an hour, or more, at the time. One Jewish gentleman has seen him when alone, once in a town-place of business— in Minn., recently while in New Haven, Conn., and yesterday while with me. I and two others have seen the Master in the clouds of Heaven (seemingly to me, very near me) ; and not to me for a few minutes, but for a long while, He followed me. It was a glorious sight. He has unlocked doors for me. He reveals himself not to all whom I wish. My wishes seem to have no control over who may see Him, or what shall occur, or whom He may bring with Him. Many self-righteous, of course, call me insane. I *rejoice*, remembering His words, but am sorry for their ignorance. Subliminal consciousness, etc., and such theories I am fully aware of. I can only say they do not work with me, or any one else so far as I can judge, at our meetings. Now I have only given you a very brief outline of what would require volumes to tell, in order, and with details. Perfect sequence of occurrences has not been followed. Tried to get man to write out an account of his sickness, and cure ; but cannot prevail upon him so far. He will tell those who do not approach him “*with a jostling air*” generally. He is more than ordinarily intelligent. Was not what is called “a religious man” previous to my talking to him about Christian Spiritualism. I feel the spirits, have heard them sing wonderful music—two electric lights burning at the time,—had them take hold of me *forcibly indeed* ; this and singing when *alone* ; not expecting anything of the kind. Have seen the Dear Lord and heard Him speak, while He manifested Himself both as the Saviour, The Infant Saviour, The Father and The Holy Spirit. As the Saviour He comes in a brown robe, as The Father in a wonderful blue robe, as The Holy Spirit in still more wonderful and dazzling white robe. Often in all ways, with a golden crown upon, or high in the air above His head. I will enclose a card which explains itself. He often says “The time of my coming to the whole world is near at hand.” “To do what we can to prepare the world for His coming.” After speaking to me I generally ask Him to give some mark of approval or disapproval to what I say He has just said to me. He generally approves by *gently* bowing His Head, or that and moving the hands. Once He added to another person something additional. Excuse haste.—Yours, etc.,

E. S.<sup>1</sup>

<sup>1</sup>Two long letters of ten foolscap pages together were written to Mr. Hodgson on two successive days in August 1896, though Dr. S. apologised for his haste and having been too busy to write before. In these he described more in detail the materialisations of Christ to him, specially His clasping him tightly about the neck, his holding St. John upon his breast as the Master had done at the last supper, and enumerated other personages visible to him and some of his friends as Dante, Beatrice, Hypatia, Julius Cæsar, Pontius Pilate, Lincoln, Hancock, Garfield, Admiral Foote, Washington, Longfellow, etc. Each letter ended in an appeal for co-operative study



The enclosed card, which he distributed to enquiring friends, was as follows :—

“**BEHOLD, I COME QUICKLY.**”

Have you seen the Lord? If not, why not?

He says, there are two (2) necessary conditions. You must have His Commandments : You must, also, keep them.

“He that hath My Commandments, and keepeth them, he it is that loveth Me ; and he that loveth Me shall be loved of My Father ; and I will love him, and *will manifest Myself to him.*”—St. John, 14th Chapter, and 21st Verse.

Christ Jesus has been seen by several, now in the flesh, who believe in Him, and have submitted themselves to these conditions. —Christian Spiritualists.

“And My reward is with Me.” “To give every man according as his work shall be.”

[ON THE OTHER SIDE OF THE CARD.]

“God is a Spirit ; and they that worship Him must worship Him in spirit and in truth.”

“But be not ye called Rabbi ; for One is your Master, even Christ, and all ye are brethren.”

“Except ye be converted, and become as little children, ye shall not enter into The Kingdom of Heaven.”

“Whosoever therefore shall humble himself as this little child, the same is greatest in The Kingdom of Heaven.”

“And whoso shall receive one such little child in My Name receiveth Me.”

“Whosoever will save his life shall lose it ; but whosoever shall lose his life for My sake and the Gospel’s, the same shall save it.”

“What shall a man give in exchange for his soul?”

“I am The Good Shepherd, and know My sheep, and am known of Mine.”

“Except a man be born again, he cannot see The Kingdom of God.”

“I stand at the door and knock ; if any man hear My voice and open the door, I will come in to him, and will sup with him and he with Me.”

This letter was sent by Mr. Hodgson to me to interview Dr. S. I thus found him in June, 1896 ; and in a long interview, which he could with difficulty end, went over the above events in more detail. He invited me to

of his phenomena. “I cannot find people with brains, sufficient knowledge, truth, and leisure, and with the heart and disposition to investigate with me as I would, and do most earnestly wish these great and wonderful manifestations should be investigated. The so-called Christians are too much in the dark, too ignorant, or too indifferent. And the infidels do not believe in Jesus Christ’s divinity, and therefore they never can, even though able clairvoyants often, see Him at all, or even his more noted Saints.” “O for intelligent, pure-minded, pure devoted, capable, and disinterested fellow helpers and investigators ; who do not wish notoriety, nor seek selfish aims, and who could afford and would devote the time to obtaining the results.”

“GOD IS A SPIRIT.”

“I AM THE LIGHT OF THE WORLD.”

join his circle, gave me the names of ten persons who had seen Christ at his meetings, and kindly started me off alone on a trip to interview these persons. The following notes of a séance will give a fair sample of his methods and results. Though he usually held his meetings at his own room, he occasionally did at the homes of his friends, and this one was held at the house of Mrs. F. B., who had lately become a professional Clairvoyant and Healing Medium.

“633, Cedar-street, St. Paul, Sunday evening, *June 7th*, 1896.

“Thirteen persons present, including Mrs. Gale and myself. A covered lamp gave light enough for one to distinguish men from women in the circle, but not features. Dr. S. sat in an arm-chair about four feet from the ends of the circle in front of a pair of brown portières, covering a double door. He did not at any time appear to be in a trance condition, sat upright with his hands on the arms of the chair, and his outline at least was visible to us during the entire meeting. He offered prayer and there was frequent singing interspersed during the evening. Several times he urged the sitters who saw anything at all to speak of it, and he occasionally asked different persons if they did not see this or that. After some waiting and silence Dr. S. said, ‘The Master is here on either side.’ Mrs. B. said, ‘I see Him in a brown robe, He is blessing us.’ Dr. S.: ‘I’m so glad you said that, for I felt He was blessing us.’ Then Dr. S. gave a disconnected religious talk which he said the Master gave through him. He then announced General Grant as there for Capt. S., but Capt. S. did not see him, though he had seen him living. The General then purported to talk through Dr. S., referring to the ‘Little Vedas.’ Mrs. B. said she saw him in his uniform with bright buttons, and added that Henry Ward Beecher was there by Dr. S.’s. side and with a book in his hand. But Dr. S. said, ‘Yes, but I am not through with General Grant yet.’ Beecher, however, also gave a talk about the Virgin Mary, etc., in a disjointed way, quite unlike his natural flowing eloquence. Dr. S. said, ‘What is back of me on the curtain?’ Mrs. B. ‘A cross,’ and through suggestions from Dr. S. allowed it was white and about two feet high. Asked if there wasn’t something at the foot of it, Mrs. B. piped up, ‘Mary Magdalene.’ Discussion between them about the hair, which had ‘tawny lights in dark brown’ to Dr. S., but to Mrs. B. was ‘dark brown and shiny all around.’

“Dr. S. saw a bunch of flowers behind Dr. D. and said they were coming nearer him, whereat Mrs. B. saw a whole bushel full of flowers on the floor in front of Dr. S. Mrs. W., next to the place where the flowers were described, said she smelled them; Mrs. B. then also did and called them ‘white roses.’ One other lady thought that possibly she smelled them, but Dr. S. did not and no one else.

“The light was now nearly all put out, though patches of light came through the blinds on to the wall and curtain back of Dr. S., and other streaks under and through the cracks in the door behind him. Dr. S. then saw a high back chair in the corner of the room by Dr. D. and with peacock feathers about it. Mrs. B. then described an Indian man, large and massive, with pompous look, and called him Pocahontas. Dr. S. asked if he wanted to talk and Mrs. B. said, ‘Yes, he nods with his head.’ This she usually said for the advent or departure of each figure. The Indian then made

through Dr. S. a long tirade against FitzHugh Lee, (as they finally made out with the help of Capt. S.) for murdering an Indian and fleeing to Cuba. Capt. S. then suggested that the real Pocahontas was a girl, and a young man sifter sustained this fact. Dr. S. came to the rescue of Mrs. B. by saying he 'got it as a man,' etc. Much exhaustion came upon Dr. S. after this Pocahontas episode, which had lasted some time, and he asked Mrs. B. to describe St. Peter, who was there. So Mrs. B. said he had 'white hair, brown beard, shaggy eyebrows, somewhat bald, and high forehead'; Dr. S. explaining the brown beard through his growing younger in the spirit world! He made a speech through Dr. S. in which he promised to appear to us all in our homes, especially to Capt. and Mrs. S. An Indian girl, 'Red Clover,' was then announced, daughter of 'White Feather' and other pedigree. 'Dear Red Clover' was a great favourite, and on Dr. S.'s suggestion shook hands with Mrs. W., so that Mrs. W. exclaimed, 'Why, I believe I almost see her.' She also made a speech through Dr. S., who thanked her for having cured a sick woman lately through him. Dr. S. saw a lily over between Dr. D. and Mrs. Gale. Dr. D. had a niece named Lily, who was sick; but Dr. S. insisted this belonged to Mrs. G. Mrs. B. also saw it 'as tall as Mrs. G.' Then followed much discussion as to the symbolical meaning of this lily. It was decided that it was a message from some one, perhaps Mrs. G.'s mother. But, after much fishing about and finding from Dr. D.'s direct question that Mrs. G.'s mother was not in the spirit world, Mrs. B. echoed Dr. S.'s suggestion of its being from an aunt (which was possible). Then with relief Dr. S. laughed and said he saw a turnip with the lily. Mrs. B. allowed she saw something 'round, long, and red on top.' Then followed a long discussion in which the lily and turnip were finally interpreted to mean that there was a difference of religious opinion between Mrs. G. and the departed aunt, in which Mrs. G. must get rid of some Unitarian tendencies. (It came out in some later conversations that this association of Unitarianism with Mrs. G. had been gained through a newspaper notice of the singing of my cousin, Miss Gale, in the Unitarian Church in St. Paul.) Dr. S. then asked Mrs. G. if she was not connected with Universalists or Unitarians, but was disappointed to find her a heathen.

"The Master was again announced, Dr. S. explaining that he sometimes sees the figures behind him, 'through the back of his head.' He explained the Pocahontas mistake by her name being confused with the real one of Powhatan. He instanced another mistake which once occurred when a Mr. Taylor's father was given as dead, but when found living was given as 'spiritually dead,' adding 'S. could not help it—it upset S.'

"Dr. S. laughed again in apparent relief and said he saw a cow and a lot of calves there, but no explanations were offered. Coming back to Mrs. G., Dr. S. told her that if she 'only got rid of some little things and in fact of some big things, and kept at it, she would be able to see and develop.' Then followed a rambling talk about a Saracen, 'Pinoalto,' with some prophecy. Finally Dr. S. asked Mrs. B. if she saw something on his head, and she saw a wreath of flowers. The meaning of this was not enlarged upon. The meeting broke up and ended in a general conversation. Capt. S. asked Dr. S. if he should pay Mrs. B. anything and he replied, 'Just as



you please. It is not necessary, but, if you feel like it, it will help her out.' Whereat Capt. S. paid her one dollar, but no one else seemed to give anything."

During the evening, as always in his conversations, Dr. S. made frequent reference to his own disinterestedness, aversion to notoriety or even publicity, and showed a naïve pride in his position as a non-professional medium of such revelations to his grateful friends.

Aside from the alleged clairvoyant perceptions of sight, hearing, touch, and smell, described by Dr. S. and Mrs. B., the only other persons confessing any were the case of flower-odour by Mrs. W. and possibly another lady, the doubtful touch of a hand by Mrs. W. and a confession from Dr. D. — extorted after repeated urgent suggestions by Dr. S.—that he saw a light on the curtain which might possibly be a head. Mrs. Gale and I saw nothing except the streaks and patches of light which came through the blinds and cracks, and the changing coloured patches of the retina's own light projected on the wall or curtain. These latter we could of course see just as well with the eyes closed. The alleged perceptions then were not objective as in the ordinary materialisation séance, where every one present can see something in common, however much the objective sense perception may be variously enlarged by illusions.

In an interview three days after this meeting Dr. S. explained that in his experience so far the figures had maintained about the same degree of reality, i.e. "etherialisation," and only "materialisation" when seized by the hand. He considered the former the higher manifestation, though that began with him, and yet he desired somewhat,—and his followers still more—the latter form. To my queries, he had not made direct experiments of seeing the figures with the eyes closed, but believed he could and did, and described again seeing behind him "through the back of his head." About darkness, it only makes the colours brighter, otherwise he could see as well in good light, which they sometimes had in their meetings. Walking with me in bright daylight through the crowded business streets he would describe, in pauses between his greetings to his many acquaintances, the figures in the air before and behind us and which were accompanying him. In all these communications from the spirits Dr. S. showed great earnestness and sincerity, in spite of some levity and a good deal of common-placeness quite below the normal level of his intelligence and conversation.

"In the second meeting at which we were present on a Monday evening in June, 1896, in Dr. S.'s own room, there was more seriousness. Here Dr. S. sat nearer in our small circle of only eight persons and with a background of a large black cloth on the wall behind him. Though he turned off the electric light in his room, it was at no time completely dark, for various streaks and patches of light came through the blinds and transom. After the Master, as usual, appeared 'a little Italian girl playing a harp,' a Jewish girl for a Mr. Lazarus, Zacchæus for quite a stay, and then the Table of the Lord's Supper was described by Dr. S. as before us and administered spiritually, Mrs. B. indicating the position of the Master in his service around the circle. Then 'Sophie,' Mr. A.'s wife, was announced, accompanied by a historical Minnesota Indian squaw, 'Old Betz,' who brought frogs' legs to Mr. A. as she had formerly done in real life. A dove

also brought herbs in an envelope to Mr. A. from his wife. Mr. A. was greatly moved and welcomed her with great emotion, said she was with him 'every hour of the day and half of the night.' He kept asking Dr. S. if she did not say something more, and Dr. S. described her as looking like a beautiful young girl of fifteen, at which age Mr. A. had a photograph of her. Though Mr. A. did not see her, he said he felt her all down his arm and knew that she was there. Then Mrs. B. described, as standing behind me, a tall, thin-faced, black-coated man, who, with a note-book, was taking a record of the meeting. He could not reveal his identity till I became a believer, which would surely happen if I kept searching and came to know the truth.

"In this circle Mr. A. was the only one who saw anything outside of Dr. S. and Mrs. B. For though Mr. A. did not see his wife, he said he saw two figures described near Mr. Lazarus, the figure behind me, some flowers, and the 'Master as a child' back of Dr. S. At the beginning of the meeting, however, Dr. S. was disappointed that Mr. A. did not see and moved his seat to the middle of the circle. Dr. S. made great efforts in vain with Mr. Lazarus, who had formerly confessed to having seen something, and once had him come up and hold his (Dr. S.'s) hands. In addition to the stray lights entering the room and the retina's projected light I could also see after long fixation a light boundary contrast about Dr. S.'s head. On my describing this in response to Dr. S.'s frequent questioning of each of us, they took this as a great confession and were enthusiastic for my development, which Mrs. B. thought might possibly come in a year, or even in six months, though her own had required nine years. Dr. S.'s last words to us were characteristic of much talk during the evening as always,—'Don't think S. a saint—I wish he were one.'"

On again joining the circle ten months later, in April, 1897, I found much the same group of devoted friends and enthusiastic disciples in various grades of "development." The aged Dr. D. saw a great deal, and though "Sophie" did not come to Mr. A. he groaned in joy and said the meetings were getting better and better. Methuselah appeared opportunely for Dr. S. to ask if he really lived to be 969 years old, and on his nodding assent Dr. S. explained, with the confidence of authority, how some St. Paul clergymen had tried to bring his age within our historical limits by a new explanation of the ancient Hebrew system of reckoning time. A beautiful and intellectual Babylonian female figure was described by Dr. S. near me, and I was told to rise, wipe my hand, and she would take it. Dr. S. and Mrs. B. said she did, and my negative confession did not prevent their talking much about it. More theological metaphysics had grown up during the past year, "the infant word" *e.g.* being quite a puzzle to the newer disciples. On my student companion mildly suggesting that perhaps imagination had something to do with the appearances, Dr. S., was warmly indignant, gave him a long talk, seized his head violently with both hands and asked if that was imagination, for so they came to him.

During these meetings there was never any appearance of effort at, or opportunity for, deception; but in December, 1896, an interesting opportunity occurred to test Dr. S.'s faith and honesty. During a two-and-a-half hours'

interview I chanced to show him some alleged spirit photographs of Munler, Boston, which I had just got from one of the best known photographers in St. Paul, and though he had never seen any before he did not show much interest in them. But he immediately suggested that I wrap up some photographic plates in black paper, enclose them in a box, seal it securely, and the Master had given him to understand that if left with him eight hours he could perhaps get a photograph of the Master. So the next day when alone in a dark room of my laboratory, I wrapped two 4 by 5 plates in three layers of black paper and two layers of black cloth, tied them with a string and sealed the knots, packed these in a plate box, sealed its four corners, put this in the outside cover, sealed a string around the whole, took drawings of all these strings and seals, and took this package to him in the forenoon. I offered to come later in the day and develop the plates after the eight hour limit, but he now wanted to keep them over night. So two days later we met, went to the dark room of Zimmerman Bros., well known dealers in photograph materials, and developed the plates with hydrochinon. He had expressed much anxiety that the box should not go into any other person's hands and that I, with him alone present, should develop the plates. Nothing came upon the first plate with ten minutes' development, though we examined it frequently, and nothing on the second plate after fifteen minutes' development. Dr. S., standing by, asked many questions and seemed in ignorance of the process. He was very much disappointed, and said that the Master told him just then—as he stood in a listening attitude—that we must try it again and keep at it, for “according to your faith shall it be unto you.” Finally after the Hypo bath Dr. S. spied something on the first plate, an irregular patch about an inch in diameter, and said it was a face. The longer he looked at it, the more certain he became, till after a few moments he pointed out—on my asking—the nose, chin, beard, and long hair, as he had described them. On taking the plate out into the cellar for better light he called an electric light repairer, who, on the Doctor's suggestion, admitted it was a face. But on Mr. Zimmerman being appealed to, he said there was nothing, it looked like a plate which had not been exposed. And on the Doctor's calling his attention to this face in the corner, he wiped the plate on the back side and the “face” disappeared with the dirt and water. Somewhat chagrined at this, the Doctor hurriedly left, while I washed the plates, dried them, and still could find nothing on them. On taking all the wrappings with me and carefully examining them, as I had of course not been able to do when I opened the boxes and wrappings in his presence, I am very sure he had made no attempt to open the box when in his possession.

A month later I told him I was ready for another trial, as he had proposed. But his faith had somewhat weakened, for though he still expects some time to get these material proofs, he considered it for the present premature and a waste of time. I found later from Mr. Zimmerman that Dr. S. had visited him after our failure, and his weakening of faith was evidently due partly at least to Mr. Zimmerman's discouragement. However, toward the end of our interview he wanted me to bring some plates as before and sit with them in my lap during a meeting, so as not to allow them out of my possession. On my suggesting a camera as necessary, he thought that would almost surely be successful. But as the main point of testing any



disposition toward trickery had been tried, it did not seem worth while to continue these trials further.

As to any other physical or real evidence of the existence or presence of his spirits, Dr. S. could not offer anything of evidential value. He asserted that a door had been unlocked for him, showed me the door and explained the conditions, but they were too uncertain to amount to anything. His few alleged cases of the communication of past knowledge from the spirits were not even reputable cases for telepathy. And though his prophecies were a great source of pride to himself, they seemed to reveal nothing more than could easily be accounted for by chance and the reproduction of his knowledge as an omnivorous reader of newspapers. (One day before his departure I found him tearing up a great quantity of newspaper clippings which he had collected in a large ledger.) The following prophecy printed in the *St. Paul Dispatch*, dated and published August 12th, 1895, was characteristic of his religious life and dealt with current newspaper topics.

#### “THE BRIGHT FUTURE.

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*“Which the Spirits Predict for the Favored North-West.*

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#### “GREAT CROPS IN STORE.

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“FOR THE NEXT THREE YEARS THERE WILL BE BOUNTIFUL HARVESTS TO BE GARNERED IN THESE STATES—TWIN CITIES WILL BE UNITED IN THE NAME OF ST. PAUL—CANALS NOW PROJECTED WILL BE ACTUAL FACTS, AND THE GREAT METROPOLIS WILL TEEM WITH THE COMMERCE OF A WORLD.

“As long ago as last February the *Dispatch* printed a crop prediction, furnished “by spirits” through the medium of a favored individual, which has been verified with wonderful accuracy of detail. The spirits now furnish another, a little more sweeping in its scope, which is appended:

“The first week in February, 1895, a Christian spirit was permitted by The Master, to impart to certain people, assembled in The Name of the Master, and worshipping Him, certain very important revelations about certain coming events, which prophecies have all been verified by the events having taken place in the meantime. These prophecies given in February, 1895, related to the great and bountiful crops, which would be given, and given to the people of the Northwest by the Master this present year.

“Several persons can testify that very early in the season the February prophecies were made known to them.

“There have lately been given more prophecies of still greater events and importance to the people, particularly of the Northwest. They have been given by the same Christian spirit, speaking in the name and by the power of the Master, Christ Jesus, on Aug. 7, 1895. These prophecies, no one, not disposed to believe, need believe. The persons to whom they have been given do believe, and are thoroughly convinced that events related therein to come, will come, for they have proven these Christian spirits many times, and in many matters of even far greater importance than those

relating to material prosperity ; that is, the Divine Government of the entire universe by God, our Heavenly Father, by the Divine Son, Jesus Christ, Our Redeemer, the Master, and through the Master, the Holy Ghost, the Spirit of Truth, the Comforter, the Holy Spirit of God, working on all good spirits, whether in the spirit world, or still in the flesh. The prophecies made Aug. 7, 1895, are briefly as follows :

“First—That the present crops in the three states of Minnesota, North and South Dakota, all these prophecies are primarily related to the states mentioned, are greater than any estimates yet made, and supposed to be reliable by the general public. The crops for the next four years following will be bountiful in these same three states, while crops in certain other parts of the world during part of the same period will be less than the average crops during ordinary years, excepting Illinois and Indiana, during the next two years will bear the average crops. In other Northwestern regions the crops will be also fair, but not so abundant as in these three states.

“Thirdly—That a great waterway will be made within the next ten years, and great ocean ships will land where Minneapolis now is.

“Fourthly—The next great change will be that the Twin Cities will become one great city, and that (greatly enlarged), the united cities will be called St. Paul, which will extend many more miles farther eastward and a great many more miles westward ; that a great many towns will spring up along the waterway from the lakes to the City of St. Paul.

“Fifthly—Other great events will be the great manufactories for manufacturing and producing by electrical processes, iron and steel, which will directly employ large numbers of men, and especially benefit millions of people in the states of Minnesota, North and South Dakota. That the State of Minnesota will increase in population very many hundreds of thousands ; and that most of the now waste lands will be cultivated ; that Minnesota will become one of the favored fruit growing sections, and that the climate during the winter will become much warmer ; that great and large boats will float in the Mississippi river, to what is now called Minneapolis, from the South and from the Atlantic, through the Great Lakes to the then City of St. Paul ; that ships from the South and ships from the North will be locked up and down at the then City of St. Paul and other points necessary to the successful operation of this great waterway.

“There will be a great many large manufactories in successful operation in the then City of St. Paul, particularly in rolling mills, and iron and steel manufacture.

“These prophecies are of the true Christian spirits, who come in the name of the Master, Jesus Christ, and in and by Him only.

“TRUE CHRISTIAN SPIRITS.”

Over a year after these prophecies I heard him dispute very dogmatically about them with a well-known business man to whom he was formerly family physician. He once wanted me to see our largest miller to tell him about some kind of a dredge which could dig the river channel very easily, giving me exact details and figures as it had all been given him by the spirits.

After his conversion he despised his former profession and especially the medicine, against which he could hardly say enough. But he seems gradually to have come to heal through the alleged mediumship of spirits. After one or more cases of partial relief to deafness, his first and still his greatest case occurred in the spring of 1896 and is referred to in his letter above. Mr. A., having been a railway conductor for fifteen years, became afflicted with locomotor ataxia until he could only walk with difficulty by the aid of a cane. He consulted the president of Rush Medical College, Chicago, who gave him something for partial relief, but could not offer him any hope of cure. Having left the railway and become the proprietor of a small hotel in St. Paul, and with his lameness now about four years old, he attended one of Dr. S's meetings. There he saw the Master approach him with outstretched hands, felt the hands on his head, heard Him say something ending with "rise and walk," at which he rose and walked off easily. During this laying on of His hands he "felt as if burning up," in which condition Dr. S. had all the ten persons present touch Mr. A. Though he is now slow in his speech and movements, he has had no relapse, and as I have seen him several times during the past two years and since shortly after his cure I can testify that he walks without any dragging of the foot. He became an enthusiastic Christian as well as Spiritualist, by far the most consistent and devoted of Dr. S's followers, and has "never ceased to thank the good Lord daily for this miracle."

At meetings since this Dr. S. would occasionally describe the "Master's laying on of hands" for some one's betterment. He said that an Indian spirit also had effected several cures at patients' homes. These healings increased until during his last year in St. Paul they occupied much of his time. He had moved from his private room to an obscure office, and though primarily for better quarters for his tri-weekly meetings, yet with his medical library again set up and his plain door sign of Dr. E. S., he was in a better situation for his spirit treatment. Thus one day in January, 1898, I found a Mrs. W. there, who, after her testimony as to the spirits she had seen, described the stomach trouble for which she had gotten relief from Dr. S. as from no one else, though she had travelled about considerably. By this time, however, he had returned somewhat to his regular medicine, for in addition to the spirit treatment he also gave her a medical prescription. As he impressively bade her good-bye, never expecting to see her again on earth, she was greatly moved and could not sufficiently express her gratitude to him for her bodily relief and religious awakening. Two of his later patients I also interviewed. Mrs. Johnson, 106, East Fairfield Avenue, West St. Paul, though she came to the door on crutches, claimed that she had been greatly helped by Dr. S. in her long-standing rheumatism. She too had the greatest gratitude for this relief and said he had stimulated her to have more will power and courage. Frau Haase, on the River Flats, had been a general invalid for seven years, especially with pains in her back. Without any medicine Dr. S. had made her a new woman, and, though she had some returns of pain, can now will them away herself. Similar cures she now makes for her neighbours. Her gratitude to Dr. S. was unbounded, and she showed a painful anxiety for his safety since his departure. Almost all his patients were women.



He had frequently spoken of his stay in St. Paul as being uncertain, said that he might leave at any time, and showed many signs of unrest. Finally in January, 1898, I found him really packing up to leave. Mingled with his customary paradoxical and pseudo-humility talk, he gave as one of his reasons for leaving that all his followers were too ignorant and he himself the only scholar. He seemed discouraged at the slowness of his propagandism. Also he complained to me for the first time of lack of financial support. The aged Dr. D., a retired physician, he abused roundly as "too stingy to come" to him more, though he never demanded money from him or any one. Mr. A. was the only one who had given him money at his meetings, about two or three dollars all together, and, as he himself said, "had been repaid a dozen-fold." His patients gave him money or not, spontaneously, according to their means. Some, as Mrs. J., insisted on paying him every visit; others, as a working girl, Miss C., paid for the medicine, in case he added any to the spirit treatment. Others, as Frau H., could and did give him nothing. But he felt imposed upon by some who could afford to give, but who did not. Thus he was "not making his office rent," and in vigorously tearing up his great collection of newspaper scraps, letters, and writings, he showed his righteous and violent indignation. And it seems from his business agent, a well-known lawyer, that there was much justification for this feeling; for in trying to help financially some of his followers, as well as owing to depressed business conditions, he had gotten his property so tied up and his income therefrom so reduced that he had to accept whatever any one offered him. And though his habits of living were the simplest and cheapest, yet he is described as being to some of his followers "generous to a fault." He would tell no one but his agent where he was going, and though his friends inferred to the Klondike, he has been reported as in Seattle, Wash.<sup>1</sup> His absence was to be for an indefinite time and very doubtful if he ever returned. Thus in Lohengrin mystery and with evident consciousness of a mission to perform, he left St. Paul in February, 1898, for some place "where he would be very busy."

To judge of the nature of Dr. S.'s own spirits, it will be of much value to first go over the testimony of his friends, as I gathered it in interviews and notes at the time. Mrs. A. J. C., a widow about sixty, mother of one of the highest legal officers of the state, had in June, 1896, seen Christ three times in Dr. S.'s presence, once in his room and twice in different houses of his friends. The figure appeared in vapoury grayish outline, as if about one hundred yards off. It had a smooth face, hair parted in the middle, a crook in the hand, was always accompanied by two female figures, one being Mary the Mother, the other was Dr. S.'s mother. They always appeared just back of Dr. S. Though the room was dark, she could see the faces of those present. Dr. S. had at the outset described to Mrs. C. and her friend Mrs. P. how the Master would come, and she saw it substantially as he had described, though Mrs. P. did not. The figures appeared suddenly and did

<sup>1</sup> Prompted by a newspaper notice of Mr. Hodgson's article on Mrs. Piper, Dr. S. wrote him from Chicago in September, 1898, again urging as in the above letters an investigation of his phenomena. He was then engaged in commercial life.

not form gradually. She saw them, however, also with her eyes closed and her hands held before her eyes, trying this experiment several times. She had no impression that it was the Master beyond the resemblance of the figure to the conventional pictures of him and Dr. S.'s word. Since these meetings she has often tried in the twilight to call up voluntarily these figures, but not succeeded. However, when lonely in her own room and longing for her mother, she can suddenly see her in the same vapoury way, but, when anxiously trying to do so, cannot see her. Dr. S. had originally invited her in the hope of her seeing her husband, but this she has not succeeded in doing. Messages, however, of no special purport were given by Dr. S. as coming from the late Mr. C. Mrs. C. doubts genuine materialisation, but is investigating spiritualism in accordance with a promise to her husband when on his death-bed. She believes that as a girl she had clairvoyant power, saw occasional figures all through her life and described a persistent one which she saw constantly during the months preceding the birth of one of her sons and which she believes marked the whole unusual appearance of this son. Since starting her investigation of spiritualism, the frequency and variety of these figures have greatly increased, till now, (August, 1898), she has seen "hundreds." She still, however, much to her sorrow, has never seen her late husband. She thinks Dr. S. a very pure and high-minded man, but eccentric and in danger of being unbalanced.

Mr. A., who experienced the dramatic cure of locomotor ataxia, has seen "lots of figures, as many as ten about Dr. S. at once," and still (August, 1898) sees his wife, mother, and the Master in his hotel; but he thinks he has seen only one real materialisation and that was once when his mother put her arm about him. All the others he described as more spirit-like, and yet opaque enough that he cannot see objects through them.

Miss McC., about thirty years of age, in the office of a large business house, had seen Christ alone and with a friend of hers before she knew of Dr. S. She has since then seen him at Dr. S.'s two or three times and also when alone. Thus she thought that Dr. S. claimed too much in his special privilege as the medium of Christ's appearance. As a consumptive she was having treatment from Mrs. B., the professional friend of Dr. S., "to see if the spirits were of any value" for her sickness.

Captain E. S., real estate agent, is a spiritualist through his wife, who is an amateur medium. They both have been twice to Dr. S.'s meetings, but neither saw anything, "although Mrs. S.'s mediumship is strong." A professional medium, Mrs. H., whom they once took with them, claimed to see all that Dr. S. did. With a high regard for Dr. S., Captain S., however, thinks him in danger of being unbalanced by exclusive devotion to his work.

T. B., a student friend of mine, and now a physician, visited Dr. S. one afternoon in June, 1896, was warmly welcomed, and entertained by a long talk. As a sample, apparently, of his power, Dr. S. volunteered a description of B.'s grandmother, as he saw her playing the piano; but, on learning that she never saw a piano, he symbolised it as joy at B.'s getting on the right track. In the evening B. attended one of the regular meetings. The usual appearances of the Master were described, perfume smelled, Dr. D. saw something, Mr. A. got excited and saw everything, Miss McC. saw something. But B. neither smelled nor saw anything, except the flickering

lights on the wall which came through the transom or window. Dr. S. took much trouble and evident pride in following B. up with long letters and invitations for more visits and meetings, and in encouragement for his study of these all-important phenomena.

F. F. W., a German-American lawyer, about fifty, was introduced to me with much explanation at lunch one day by Dr. S., and, on walking back to Dr. S.'s office together, he urged and insisted that Mr. W. should tell me what he had seen. But Mr. W. had to mildly protest that he himself had really seen nothing as yet, but was merely developing, although Dr. S. had told me that Mr. W. had seen General Grant; but Mr. W. admitted that Dr. S. had correctly told him about his having met General Grant in the south part of Milwaukee, soon after the war. In a subsequent talk with me alone, Mr. W. described the varying patches of form and colour which he had seen at the meetings, and which were evidently the retina's own light. Mr. W.'s wife, however, sees clairvoyantly, and was successful in the first meeting reported above. Though Mr. W. has no confidence in professional mediums, he believes Dr. S. to be perfectly sincere and genuine.

Frau H., one of his patients, a middle-aged woman, besides seeing the Master at Dr. S.'s, described in detail her seeing Him in her own house. But she has not seen many others of the biblical characters.

W. B., lawyer and business agent of Dr. S.'s, has attended three or four meetings, and the first time he saw nothing. But the second time, on fixating with effort and extreme tension, he once or twice saw what they called the "face transfiguration," once to that of General Grant, although, he allowed, the whiskers were not different from Dr. S.'s. He is sure that once, anyway, he recognised a transfigured face before any one else described or named it, though he is not sure whether he first spoke of it. The transfigured face was very fleeting. Having been warned by his wife against being completely hypnotised, he did not make the same effort to see at the next meeting, and then saw nothing. Mr. B. thinks the Doctor has telepathic powers from his own account, and believes him thoroughly honest and sincere.

There is only one witness who seems to me of no evidential worth, and that is the professional, Mrs. B., who was only occasionally present at the meetings, though she happened to be at the three which I attended. Her ready and cheerful manner of echoing or anticipating all the usual round of figures described by Dr. S., the ridiculous mistakes she made (as, for example, in the Pocahontas matter), the business way in which she advertised herself for patients and encouraged every novice to sit for the development of his natural mediumistic powers, the transparent superficiality with which she once announced her starting a Bible class at her home for weekly study—all these things were highly suspicious of her genuineness, and quite opposed in general to the Doctor's traits. So I doubt very much whether she really saw anything, and I believe she deceived him. For, to my expressed objections to her in my last interview with him in January, 1898, he was not concerned, and believed her to the end his good friend, though he included her among his ignorant followers. But Mr. A. tells me that Dr. S. warned him and others "not to take too much stock in what she said." Mr. A. himself had become



suspicious of her, as having too much imagination, for he thought it was strange that she always saw what he did, but that he by no means always saw what she described. Another reason of distrust which he gave was quite characteristic of such a mental composition, viz., that Mrs. B. had told him that his wife had not appeared to him for some time because she had been sick, but when she did again appear, and he asked her if the sickness was the reason, she denied it, and said she had been off on the Master's business. There is a possibility, of course, that she may have been in collusion with Dr. S., supposing him to have been consciously deceiving.

When then we come to the question of Dr. S.'s own genuineness, two components of his character must be specially considered. First, he had undergone an intellectual change which so disturbed his mental equilibrium that he was carried largely, if not altogether, over that vague and broad transition towards the category of the insane. When a man with the scientific education of a physician is suddenly "converted" in middle life to the literal belief in biblical theology adopted by Dr. S.; or when, in other words, a mature mind abandons reasoning by facts and induction for the theological deduction from authority and for poetical analogies, that is a regression to unreasonableness. And Dr. S.'s unreasoning in this sphere made him dogmatic and imperious in other things. He frequently ridiculed my trying to get at the "chemical basis," as he expressed it, of his spirits, and said I could never see till I joined him in first believing. On my seriously trying to explain my efforts to find a psychological basis for his spirits, he would cut me short authoritatively, saying he knew all those things and that they had nothing whatever to do with his Christian Spiritualism. And yet I could not find that he had seriously studied or considered any natural explanation of his phenomena. His intense emotional concentration in this subject so contracted his intellectual life that he had no longer any interest in the overwhelming mass of the world outside his narrow theological limits.

Secondly, this intellectual concentration of attention and interest was accompanied by an abnormal heightening of the feeling-component in his character. For his intense pride in his mission showed itself constantly in many forms. It often became laughable under the cover of his continual pseudo-humility talk,—“don't think S. a saint,” “it's no credit of S.'s,” “S. is a mere nothing.” Though he did not proselytize to any one's annoyance, yet his greatest happiness was to get some one to whom he could “talk spirits;” and any new possible convert was followed up tenaciously with talks, visits, introductions and letters. He never associated himself with any religious or spiritualistic church or organisation, but evidently had more pride and pleasure in forming and being the privileged head of his little circle of followers. When I asked him about other spiritualists and especially what he thought of the materialisation phenomena, then being given at our neighbouring N. W. Spiritualistic Camp Association, he would only say, “God bless them—perhaps they have something;” but he never seemed to have had there or elsewhere any experience of the usual clairvoyant phenomena and cabinet materialisation. He emphasised the fact that he was a *Christian* Spiritualist, in that he was a Christian first of all and then by logical necessity a Spiritualist, which he feared was not the

order or relative importance with most Spiritualists. He explained the rare appearances of Christ and other biblical characters as due to the lower plane of the ordinary mediums, and thus by inference, the special communion of which he was the instrument was a Divine mark as surely as that of Parsifal's and Sir Galahad's. And yet the number of his followers who had seen the Master—and this was the test of his disciples—only grew from twelve in June 1896 to about thirty at the beginning of 1897, and about fifty in 1898, at the end of his four years' activity in this new faith in St. Paul. So in this case then we find the delusion of grandeur, which is so commonly a characteristic in the unbalanced feeling-life. The pecuniary motive of the professional was absent, and the only apparent motive was the altogether disproportionate satisfaction which he got in this obscure life as the centre of his heterogeneous group of Christian Spiritualists.

With these abnormal intellectual and emotional characteristics, then, I think it most probable that Dr. S.'s spirits were for the most part real to him and not assumed. It seems rather probable that with his anxiety for success and converts he may have unconsciously come to add merely verbal details or even whole descriptions to his apparently real sense perceptions.<sup>1</sup> But that the Master and a large number of figures were honestly real to him, I believe, from his self-sacrifice for his cause, sincerity, and lack of sufficient motive for complete deception, whether conscious or unconscious. But the only visual objective reality corresponding to the spirits seemed to be the projected lights of the retina, stray patches and streaks of light entering the room, and perhaps some boundary-contrasts and after-images. For the hearing, touch and smell perceptions of the spirits and spiritual objects, I have no evidence of any objective reality. Some of the cutaneous or muscle sensations constantly present with attention and some singing in the ears may have been the nucleus of the touches and voices. Whether, then, these alleged clairvoyant etherialisations and materialisations should be catalogued under illusions or hallucinations depends on whether there were any objective perceptions and point of attachment. This objective element being apparently at most so inconsiderable, these phenomena of Dr. S.'s should more properly be classed as hallucinations or in part as pseudo-hallucinations.<sup>2</sup> This is probably the more justifiable from Dr. S.'s own statement in his letter above that before his conversion he "heard voices once in a while and saw clairvoyantly, but did not understand these manifestations," and was annoyed by them till he fancied himself on the verge of insanity. With this exaggerated or even pathological tendency to hallucinations came the suggestion from his old German friend of seeing Christ. The mental changes wrought by his conversion were exactly fitted to develop this hallucination and the others would develop most naturally by association, intense attention, and hallucinatory projection.

This process can be traced in his followers even more clearly. The most instructive witness was Mrs. C., with a hallucinatory predisposition from girlhood, and who only needed the emotional loneliness of her widowhood

<sup>1</sup> Compare the case of the Creery Sisters: *Proceedings* of the S.P.R., Vol. V., p. 269.

<sup>2</sup> See "Report of the Census of Hallucinations" in *Proceedings* S.P.R., Vol. X., p. 76. Also James's *Psychology*, II., p. 115.

and the directing suggestion of Dr. S. to immediately see the Master. Her experiments with closed eyes show all her figures to be no more than hallucinations. Similar is the case of the hotel-keeper, Mr. A., in his loneliness for his wife, and perhaps some predisposition to hallucination as shown by his nervous degeneration in locomotor ataxia. With these preparations, the confident assurances of Dr. S., the apparently miraculous relief of his lameness by suggestion, and his enthusiastic acceptance of Dr. S.'s religion, all aided in giving a hallucinatory objectivity to his ideas and feelings; while for other followers who needed more training in attention and emotion, Dr. S.'s method was well fitted. The long waiting and expectation in darkness, the enviable descriptions of successful "seeing," the frequent encouragement and urging for the sitters to describe anything they saw, the personal interrogation whether they did not see this or that—all this aroused partial or occasional success.

This case of spiritistic hallucinations is of special interest, it seems to me, as being so comparatively simple, so free from the usual confusion of accompanying fraud, unconscious deception, genuine physical phenomena and telepathy, automatic writing, etc. Thus it affords a clear view of the process of the development of spiritistic hallucinations, helping to fill an evidential gap in the "Report on the Census of Hallucinations," where one reported case gave rise to the remark that "it is possible that habitual attendance at spiritualistic séances may lead in some persons to the seeing of apparitions and hearing of voices." (*Proceedings of the S.P.R.*, Vol. X., p. 176.) Mr. Gurney, from his study of the earlier Census, had also thought that "it must be admitted as possible that mere verbal suggestion may act similarly [*i.e.*, to hypnotic collective hallucinations] on certain minds at certain times, without the preliminary of any definite hypnotic process." (*Phantasms of the Living*, 1886, Vol. II., pp. 187-8.)

In helping to reduce the separation between suggestion in the conditions of hypnosis and of normal life the case under consideration is also valuable. For the late progress in the understanding and explanation of hypnotic phenomena has been by freeing them from the marvellous and abnormal, and by connecting them with the better known facts of our normal mental life.

But it is in the help it affords in explaining some parts of the usual medley of mediumistic phenomena that this case is more immediately valuable. For with some basis of genuineness, the usual phenomena of any medium or group of spiritualists are a bewildering conglomerate, and yet an apparently homogeneous whole, because they are usually all explained on one hypothesis. But, more and more, in all psychological phenomena it is becoming necessary first to separate the phenomena into as many categories as possible and then seek for the explanation of each class. Throughout my acquaintance with this case I have had continually in mind its possible application to the clearing up of such enigmas as the experiences of W. Stainton Moses and the high testimony on record to the marvels of D. D. Home.

There was much in common as to characteristics and methods between Dr. S. and Mr. Moses. There were the same impatience, obstinacy,



irritability, lack of humility and stubbornness of mind. ("The Experiences of W. Stainton Moses," by F. W. H. Myers, *Proceedings S.P.R.*, Vol. IX., pp. 247, 252, 293). Dr. S.'s professional education was more scientific, but his social environment was not so high. There was, however, on the whole a more serious religious purpose and level in Dr. S.'s phenomena, and a more devoted self-sacrifice to his mission. (*ibid.*, pp. 258 and 334) Both were amateur mediums, giving their manifestations to only a small circle of friends, Mr. Moses being the more exclusive. And while avoiding notoriety, both were willing their experiences should be used as testimony and aided in their publication.<sup>1</sup> The fundamental motive of both seemed to be the satisfaction of authority and leadership. The intellectual character of the witnesses to Mr. Moses' phenomena were higher than Dr. S.'s, but there were similar emotional conditions of admiration, unsuspiciousness and expectation.

When one now reviews Mr. Moses' phenomena to separate any possible class of hallucinations, it is of course impossible to draw any clear-cut and certain lines. Unfortunately we have not a sufficiently full report of the methods and conversations during the séances to judge of the amount of verbal suggestion. But it seems to have been quite sufficient (as in *ibid.*, p. 322, in Dr. Speer's Report) for the training of expectation until, after repeated séances, the sitters knew by association what to expect from any name, sound, light, odour, or touch. Some of the visual phenomena seem undoubtedly hallucinatory. Mr. Moses tells us that the most successful large luminous appearances, as well as the most perfect musical sounds, occurred when he was in a state of unconscious trance (*ibid.*, p. 268 and 276), and thus we must usually rely directly on the testimony of the sitters. The luminous haze and globes, together with their movements described by Dr. Thompson (*ibid.*, p. 274), seem quite possible as hallucinations, and the similar ones in Dr. Speer's statement (p. 275) seem to have been held by a probably hallucinatory hand and drapery, even if the lights were objective. Here the process of verbal suggestion is well described, when the "controlling spirit informed me that he would endeavour to place a light in the medium's hand. Almost immediately a light came. 'Now I will show you my hand.' A large, very bright light then came up, and inside of it appeared the materialised hand of the spirit. He told me that the folds which I saw around the light were drapery, and to prove it he brought the light and passed the drapery over the back of my hand several times."

One more evident case of hallucination was in Mr. Moses' "vision" where "Dr. and Mrs. Speer saw misty light but nothing more" (p. 293). A moving column of light, seven feet high, with a large glowing mass of light, the cross behind the medium's head, and the cluster of oblong lights, (p. 297) may have been partly, if not wholly, hallucinatory. In this séance, as in most, the description is not full enough for us to be sure that objective stray lights were not present as the basis of illusions. Dr. Speer is said in Mr. Moses' account to have first seen the light, and Mrs. Speer's testimony

<sup>1</sup> Dr. S.'s case was thus more similar to Mr. Moses' than the case of Mr. Z., reported by Professor Sidgwick in his paper on "Disinterested Deception" in the *Journal S.P.R.*, Vol. VI., p. 274; for Mr. Z.'s case lacked the religious and publicity motives.

to it is wanting, though she was present. Four months later Dr. Speer recalls seeing "the hand and arm." (p. 306.) Seven months after these other phenomena began, the first human figure was recorded by Mrs. Speer. "We saw a form surrounded with light, standing between myself and the medium." (p. 302.) But it is not mentioned either in Mr. Moses' or Dr. Speer's notes. Again four months later Mr. Moses recorded: "Dr. and Mrs. Speer and I saw a figure carrying it [scent] apparently. I also saw a figure in the middle of the table when the lyre was sounded there." (p. 312.) But neither Dr. nor Mrs. Speer testified to the former figure, and Mr. Harrison, who was present, is not included in Mr. Moses' account as seeing the figure.

Dr. Speer alone then notes the beginning of numerous lights:—"Between thirty and forty spirit lights appeared, many of them as large as large oranges. They began near the floor, then ascended, and at last by request came within our circle, so that we might have touched them. Many of the lights were amorphous, or rather, formless. They succeeded each other with great rapidity," (p. 313.) At the next séance he notes similar lights emerging from the aperture of the curtain of a cabinet which had been fitted up, and in which Mr. Moses sat. With such a convenience it seems more probable that these lights were produced objectively by the medium, as the lights are said to have cast reflections upon objects, and some were so large and bright as to show the whole of the lintels and door posts. (p. 313.) Under these cabinet conditions at the following séance Mr. Harrison seems, from Dr. Speer's notes, to have seen these lights. The medium having suggested going upstairs to the cabinet, and trying for lights, Dr. Speer notes that "they came almost immediately, of various shapes and sizes, some very large." He gives rough drawings of their shapes and continues, "they were all of a pale bluish green, or rather greenish blue, tint. Many of them had a perfectly distinct hard outline and bright nucleus. Mr. Harrison says that such lights could by no possibility be imitated [Mr. H. was an electrician], and imagines that they were produced by hands holding crystals with drapery falling on them." (p. 314.) The shapes and colours of these lights are thoroughly characteristic of the projections of the retina's own light. It is suspicious of the trustworthiness of the medium's account that Mr. Harrison's imagined explanation of the light by the hand and drapery in Dr. Speer's notes is given in Mr. Moses' account as a described fact. Again Dr. Speer notes "a large number of splendid lights, apparently held by hands, were seen in the cabinet. I myself saw an arm apparently holding one of these lights." (p. 317.) But no testimony is given to these by Mrs. Speer and Dr. Thompson, who were present. Another similar case two days afterwards occurred at which Mr. Percival joined the family group.

Again, sitting with Dr. Speer alone, Mr. Moses himself records having "witnessed the formation of some eight or nine very beautiful spirit lights. They formed quite close to me and near my left hand, about a foot from the floor, floating upwards till they reached the level of the table and became visible to Dr. Speer. They were expressly made at my side, instead of, as usual, at my back, so that I might see them. They seemed to develop from a very bright speck, about the size of a pea, until they attained the size of a

soda water tumbler, and showed a soft luminosity like pale moonlight. They seemed to be covered with drapery and to be held by a hand. They faded slowly out, remaining visible about thirty or forty seconds, or perhaps a minute. The largest would be about eight inches long." (p. 319.) As Mr. Moses did not himself usually see anything, this is an interesting description. For, as far as it is genuine, it is characteristically hallucinatory. Dr. Speer's corroboration to this séance is not recorded ; but the next day, during their trip together in Ireland, he records that "fifteen great lights showed themselves, varying from the size of an orange to that of a shaddock." (p. 321.) The next day Dr. Speer recorded, in a longer account of the usual lights and hands, that the control speaking through Mr. Moses "explained the process of making the lights, and told me that the folds I saw around them hanging down were really drapery, which he would prove to me. He then brought the light close to my hand, and brushed it backwards and forwards with as distinct a materialised drapery as can well be conceived. This feat he repeated first on one hand and then on the other, five or six times, at the same time directing my attention to the character of the light and the tangibility of the drapery." (p. 323.) This is an excellent example again of the suggestion process, though a later direction to lean forward and close his eyes till told to open them seems to complicate any explanation with a probable element of trickery. The more permanent visibility of a cylindrical light is recorded later ; forty-five minutes being given by Dr. Speer and five minutes by Mr. Moses, who contrasts this with the other lights which "have faded very rapidly, lasting from ten seconds to, perhaps, a minute." (pp. 325-6.) This discrepancy of its permanency is apparently a mistake of Dr. Speer's or a misprint, for later he records the same light "for some minutes." (p. 328.)


Mrs. Speer's account is again given at another séance where she saw "two or three very large lights. One must have measured with its drapery quite two feet in height ; he [the control] brought it close to my face, and brushed my hands many times with the drapery ; he also touched my hand, and his hand felt as human as my own. The lights were not good, as Mr. S. M. was ill, and the atmosphere was not right, and this interfered with their development. The medium then said he saw a fresh spirit standing by Catherine." (p. 329). Though Mrs. Speer did not see this figure she noted that on joining the medium and Dr. Speer and "sitting down, I saw a bright figure standing behind Mr. S. M." (p. 329.) Neither of these figures was seen by Dr. Speer, who only records : "Three splendid lights appeared ; an arm was seen [not mentioned as seen by Mrs. Speer or Mr. Moses] ; drapery repeatedly thrown over us." (p. 329.) Mr. Moses' own explanation of these lights is interesting. In this séance he recorded "a number of lights, some of which were comparative failures, the nucleus only being visible. This nucleus Mentor has explained that he brings with him, and in unfavourable conditions he is not able to surround it with the luminosity which he gets from the circle." (p. 328.) At a séance three weeks later he recorded : "When we went up into the cabinet upstairs, Mentor made some very clear lights, one of which endured over four minutes. I had been very anxious to try the duration of the light, because an imitation of such lights is made by phosphorised oil ; but lights so made are of very



brief duration. I believe a favourable trial would show that Mentor's light would last seven or eight minutes." (p. 331.)

In the published notes from the second and subsequent years' experiences (*Proceedings S.P.R.*, Vol. XI., pp. 24 to 114), we will not follow the lights more in detail. Beyond the light reported by Dr. Speer (*ibid.* p. 31), which "remained visible for upwards of forty minutes" and the Prophet's little light, which Mr. Moses records as giving answers through flashes during a long conversation with him (p. 38), and which so seem more objective than hallucinatory, there is nothing new as compared with the first year's lights. In only four cases is there the evidence of one of the sitters besides Mr. Moses. But a very suggestive feature in these later experiences is the increased frequency of "visions" and human figures described by Mr. Moses. Only three figures were reported in the first year, one by Mrs. Speer (IX. p. 302), and two by Mr. Moses (pp. 312 and 329), and on the hallucination theory we should expect that after the practice and suggestion-training of the first months there would be an increase of the more complicated and complete figures over the simpler lights with their partially visible hands, arms, and drapery.

Mr. Moses recorded that he saw "a veiled figure standing by Dr. S. Mrs. S. could see the light, but could not distinguish the figure." But later in the same séance he was, "as is frequently the case, clairvoyant, and described the face of the Emperor, his waxed imperial and moustache, his impassive marble face, and wound up by saying he was a 'regular Mephistopheles.' The form was just in the same place and apparently could not come within the circle." (XI., p. 33). Dr. Speer records this of the "clairvoyant member of the circle," (p. 34), but not of himself.

Now occurs the first record of one of Mr. Moses' curious visions which he had experienced twice before. (See pp. 36, 37). The next day also, "on coming out of the trance, I saw and described the spirit as one of commanding mien, with a short robe girt round the waist, and a crown with a cross  in the centre, just as the Prophet has a star. In his hand he held a long wand, terminating in a cross. He said nothing, and gradually faded from view." (p. 37). Again a few days later Mr. Moses was clairvoyant and described his grandmother again. "She had a child with her. I also described the Prophet and Theophilus, and several spirits who were apparently at the ceiling over the table, and were showering down scent" (p. 38) and he "described clairvoyantly the Prophet and the Seer together—the latter a young and very beautiful spirit, the Prophet dignified and aged. Also Grocyn and a tall spirit by Dr. Speer whom I did not know. There was a deal of light in the room." (p. 40.) This latter condition Mrs. Speer described as the room being "filled with spirit light, large masses floating about in every direction." (p. 40.) An interesting record in the hallucinatory objectivity of Mr. Moses' progress now occurred. "In the midst of the séance, when perfectly clear of influence, I saw Theophilus and the Prophet. They were as clear and palpable to the eye as human beings would be in a strong light. Placing my hand over my eyes made no difference, but turning away I could see them no longer. This experiment I repeated several times. I also saw the little Catherine. This is the first time I remember being so clairvoyant in a séance without being previously entranced."

(p. 43.) Five more figures are recorded, either by Mr. Moses or Mrs. Speer, as seen by Mr. Moses alone. (pp. 45, 46, 48 and 58.) "Once two very clearly defined figures were near Dr. Thompson, visible to both of us," (*i.e.* to Mr. M. and Dr. T.'s son), (p. 52) but the latter's evidence is wanting. The last recorded event of this class is another vision of Mr. Moses' (see pp. 59 and 60).

These records extend over the first two and a half years of Mr. Moses' phenomena. Of the later experiences of the last three years the son of Dr. Speer, Mr. Charlton T. Speer, has given a very suggestive summary. "Numerous lights were generally visible to all the sitters. The lights were of two different kinds—objective and subjective; the former were usually like small illuminated globes, which shone brightly and steadily, often moved rapidly about the room and were visible to *all* the sitters. A curious fact in connection with *these* lights always struck me, *viz.*, that looking on to the top of the table one could see a light slowly ascending from the floor and to all appearances passing out *through* the top of the table, the table itself apparently not affording any obstacle to one's view of the light. It is a little difficult to explain exactly what I mean, but had the top of the table been composed of plain glass, the effect of the ascending light as it appeared to one's organs of vision, would have been pretty much the same as it was seen through the solid mahogany. Even then, to make the parallel complete, it would be necessary to have a hole in the glass top of the table, through which the light could emerge. The subjective lights were described as being large masses of luminous vapour, floating around the room and assuming a variety of shapes. However, Dr. Speer and myself being of entirely unmediumistic temperaments, we were only able to see the *objective* lights, but Mr. Stainton Moses, Mrs. Speer, and other occasional sitters frequently saw and described those which were merely *subjective*. Another curious point in relation to the objective lights was that, however brightly they might shine, they never—unlike an ordinary lamp—threw any radiance round them or illuminated the smallest portion of the surrounding darkness when it *was* dark—in the slightest degree." (*Proceedings IX.*, pp. 345-6.) It seems questionable whether the objective lights here described were not partly illusion, if not hallucination. The latter point of the non-radiance of the objective light is contradictory to several of Dr. Speer's notes, as above quoted. (See *Proceedings IX.*, p. 313.)

On collecting together the phenomena of hearing, there is much more difficulty in separating the probably hallucinatory from the real sounds. But with an undoubted large basis of real sound to be accounted for, there is besides, in the details of localisation, quantity and quality of sound, much which can most probably be explained as illusion. For in localisation, *e.g.*, we know experimentally how helpless one is in distinguishing sound as front or back and as to distance.

In my own experience in a semi-professional group of spiritualists, some raps, which I made on the legs of my chair in the middle of the circle, were localised by one of the circle as coming from a bric-à-brac cabinet high up on the wall, about eight feet to my left, and this was acquiesced in by others. Again, Mrs. Gale and I, at a materialisation séance, had localised the sounds of a mouth organ as coming from the cabinet about ten feet in

front of us, until Mrs. Gale stood at the cabinet curtain, when it seemed to be about four feet to her right; and only considerably later did we discover that it came from a sitter immediately in front of us.

The "fairy bells," described by Mr. Moses and Mrs. Speer, show well these possibilities of illusion, if not of hallucination. Mrs. Speer records: "I then heard a sweet tinkling sound like a clear bell close to my ear; it moved and played in front of the medium, who was still deeply entranced, and afterwards it played over the middle of the table, when Dr. S. heard it for the first time. It was a striking manifestation; the notes were so clear, sweet, and low—we named it 'fairy bells.'" And again: "Before meeting this evening we heard the 'fairy bells' playing in different parts of the garden, where we were walking; at times they sounded far off, seemingly playing at the top of some high elm trees, music and stars mingling together, then they would approach nearer to us, eventually following us into the séance room, which opened on to the lawn. After we were seated the music still lingered with us, playing in the corners of the room, and then over the table round which we were sitting." (XI, pp. 56 and 57.)

Clairaudience is only reported by Mr. Moses himself in his group; and, like the clairvoyance, coming more often, if not altogether, after the first year. He reports a long conversation with a figure, "John C." (XI, p. 45), and "Judge Edmunds" (p. 49). Mr. Moses gives this suggestive description:—"The peculiarity of the séance was that when I could hear the sound no one else could, and *vice versâ*. I heard by clairaudience and not by natural hearing, being very deaf with my cold. On request I was enabled to hear the sound with my natural ear, but I described it long before it was heard by the others, and heard it frequently when they did not. At the same time I was unconscious of sounds apparently made on the table under my nose." (p. 50.)

In the sense of smell the phenomena were decidedly material, as judged by Mr. Percival's account (IX., pp. 272, 273), and Dr. Speer's: "Scent thrown in great quantities—so much was thrown at Mrs. S.'s eye as to produce excruciating agony and congestion of the eye, with almost blindness for twenty-four hours" (p. 328); and after a "large quantity of Sp. Pulegii . . . the dining-room cloth and my own nether habiliments had to be exposed to view in the back garden; and on the following morning our dining-room floor and passage had to be freely fumigated with pastilles." (p. 339).

But if the phenomena of smell were probably not hallucinatory, those of touch, again, probably were. The touch of drapery on Dr. Speer's hand (pp. 275, 323) was promised by the medium as proof, and thus acted as a most natural preparatory suggestion. As in the other senses, Mr. Moses' own experience here too was most real. He records:—"The most curious part of the séance was that in which an elaborate attempt was made to relieve my bronchitis. When we made the usual break, after sitting for about forty minutes, I was perfectly conscious of two materialised hands making passes over my head, throat, chest and back. . . . The sensation of the hands was perfectly distinct and unmistakable." (XI, p. 49.)

In reviewing these experiences of Stainton Moses for a probable element of hallucination, I am, of course, aware that any reasonable deduction to



be made on account of this explanation from the unprecedented variety and great mass of his spiritistic phenomena (see IX., p. 336) will be comparatively small. The overwhelming mass of the phenomena and all the greatest marvels still remain unsolved. But from these I think another indeterminable amount must be subtracted. For with this hallucinatory tendency in the simplest phenomena, it seems probable that both Mr. Moses' and his witnesses' accounts of the physical phenomena are unconsciously exaggerated and distorted; and this to a degree beyond the ordinary possibilities of mal-observation, owing to these special mental characteristics and to training conducive to hallucinations and illusions. (See Davey and Hodgson on the "Possibilities of Mal-Observation": *Proceedings S.P.R.*, Vol. IV., 381, Vol. VIII., 252.) Though, as has been said, different persons will make various estimates of the amount of Mr. Moses' experiences which can reasonably be accounted for by hallucinations and illusions, yet it seems to me Dr. S.'s case has been helpful in giving us materials for some explanatory reduction of these puzzling spiritistic experiences. How the larger mass still remaining can be explained is a separate question, which, though out of the bounds of this paper, I cannot but think can be similarly analysed as due to automatic writing, some small residuum of perhaps genuine physical phenomena and telepathy, and more or less unconscious and disinterested deception prompted by the pleasure of leadership and authority. (See Mr. Podmore's treatment of Mr. Moses' case in his *Studies in Psychical Research*, 1897, pp. 115 to 119.) Some such analysis of these alleged spiritistic phenomena seems to me much more reasonable than Mr. Myers's disjunctive that "the permanent fraud of the whole group, or the substantial accuracy of all the records, are the only hypotheses which seem to me capable of covering the facts," (*Proceedings*, IX., p. 254), and later, having excluded the former alternative, he concludes:—"I therefore regard the reported phenomena as having occurred in a genuinely supernormal manner." (*Proceedings*, Vol. XI., p. 25.)

The allowance for hallucination in the high testimony to the phenomena of D. D. Home has been suggested and followed out in some detail in Mr. Podmore's *Studies in Psychical Research* (pp. 121-124).

NOTE.—For the study of hallucinations the greatest work by far is the "Report on the Census of Hallucinations," by Professor and Mrs. Sidgwick and their Committee in the *Proceedings* of the Society for Psychical Research, Vol. X., 1894, pp. 25-422. Chapters IV.-XI. deal with hallucinations in general, apart from their connection with the evidence for telepathy. See also report on the earlier census and treatment of it, by E. Gurney in *Phantasms of the Living*, 1886, Vol. I., pp. 457-518.

As a compendium of great value see Parish: *Hallucinations and Illusions*, London, 1897. For a shorter treatment see James: *Psychology*, 1890, Vol. II., pp. 114-131.

In connection with the insane, see Ziehen: *Psychiatrie*, 1897, pp. 20-43.

## II.

## A CASE OF ALLEGED LOSS OF PERSONAL IDENTITY.

The following account is an abridged report of an alleged loss of personal identity, and presents many features difficult of interpretation, and not without interest. It describes the history of a Mr. Hall, discovered at Mankato, Minnesota, at which place he declared that he could not recall his past life. Upon further investigation this past life was traced, and many conspicuous fraudulent facts ascertained in connection with it. The man had an obvious reason for desiring to wreck his connection with the past: and an attempt to evade the consequences of his acts, ingenious, but not unparalleled, might appear at first sight the simplest explanation. Yet those who have come into personal contact with the man decline to accept this plausible hypothesis. The case seems more probably one of those obscure cases on the borderland of abnormal personality—cases where it is so difficult to ascertain how much is responsible conscious decision, and how much uncontrollable epileptic changes.

On the 14th of October, 1896, says Professor Harlow Gale :—

A gentlemanly and intelligent Englishman about 50 years of age walked into Mankato, Minnesota, and registered at the City Hotel as "John Harrison, St. Paul." The expenses of lodging and breakfast having exhausted his money, he left the hotel the next morning, and wandered about town all that day and the following night. In the morning he returned faint and weary to the hotel, told the clerk he had no money, and offered his umbrella as security for breakfast. He then inquired for the Episcopal Church, and thus found his way to the Rector of the church, Rev. George H. Davis, to whom he said he could not recall his past life. He knew he was a churchman, showed an Episcopal Prayer Book, recalled having presented some girls from his Sunday School class for confirmation at some Easter two or three years ago; had been in the habit of acting as lay-reader somewhere, and had perhaps been in the furniture business. He knew, however, that his name was not Harrison, though he could not recall his real name. But beyond these facts he said he remembered nothing; could not tell whence he came into Mankato (except that he walked by a bluff and across a bridge into town). His consecutive memory broke off at that point two days before. Having asked to be directed to a physician, Mr. Davis took him to Dr. J. W. Andrews, corner of Cherry and 2nd-streets, to whom alone he told the same story. He was then further sure that his wife and children were dead; that he had lost his valise and pocket-book, t did not know where or when. He thought he had left home several

weeks ago; had had means; had been in Winnipeg; was engaged somewhere in business for himself, and that this was the first attack of the kind he had had. He said he never tasted liquor, and was in apparent good health. So he was taken to the hospital to await the probable recovery of his lost memories. A week after his admission, hearing of the cyclone at St. Louis, he said he recalled something how that injury looked. Acting on this suggestion, Prof. C. F. Koehler, Psychologist at the State Normal School at Mankato, gave him a railroad map. This he scanned till he came upon the Mobile and Ohio railroad, which seemed familiar to him. Following this along he came to Fruitdale, Ala., where he then recalled having been in the lumber business. After this experience he called on Mr. Davis, and seemed very happy at being on the track of his identity. Shortly before this he had, after some apparent effort, recalled the name of Blanchard, Chamber of Commerce, Chicago, with whom he had been in business. Through Dr. Andrews he had written him, telling him of his loss of identity, and asking for help. Mr. B. replied that he thought he recognised the man from Dr. Andrews' description, and would come from Chicago to see him, whereat he was happily excited. On November 5th Mr. Blanchard came, was gladly received by his former business associate, who now recalled the saw-mill and most of the people in his employ. On Mr. B.'s telling him his name was John Harcastle Hall, he then for the first time recalled that it was. He could not remember when he left Fruitdale, or Mobile, but did recall being in Chicago in June. Only pleasant recollections seemed to return to Hall as he chatted and chuckled along with his former business friend.

Mr. B. now accused him in severe terms of dishonesty, and threatened him with arrest. But, to "liar, forger, embezzler, defaulter," etc., Hall only listened in a dazed way, and with a surprised and hurt expression. He knew nothing of the \$400 drawn in Mobile, nor of the \$300 borrowed there, nor could he account for some \$2,300 sent, more than to think he must have paid it out. He asked why he should have been so anxious to send for Mr. B. if such things had occurred, and he was liable to be prosecuted.

Mr. Blanchard, later in the day, gave an account of his connection with J. H. Hall. It appeared that he had arrived at Fruitdale, Alabama, penniless; he started a project of a saw-mill there, securing local support; had called on Mr. Blanchard, presenting unexceptional references, none of them, however, extending into the immediately preceding seven years, and had finally induced him to provide capital for the saw-mill. This Hall superintended, sending monthly reports to Blanchard at Chicago, the accounts being altogether fraudulent, Hall intercepting all the money instead of paying the employés.

"Hall was industrious to a high degree, feverishly active, the first at the mill in the morning and the last to leave, always going here and there, but accomplishing nothing. He was not a good business man, yet the business would have been successful had he acted squarely. He was a master of lying and forged letters, and carried on a systematic course of deception. He did petty thieving when he could have gotten more from me. He forged a letter purporting to be written by one Slater to himself, saying that the writer had \$1,500 to invest, and would give it to Hall to use in the business if he (Slater) could have a position. I was led to believe that this



was done, and that Hall had put this money into the business ; yet I afterwards learned that the letter was a forgery and not attempted to be an imitation of Slater's handwriting. Slater was a common deck hand on a boat and had no money, but Hall gave him a good position to keep up the deception, but did not pay him anything. Hall put no money into the business.

"Hall's non-payment of his men and the storekeepers began to make them clamorous for money, and he came back two weeks later and wanted money. I had none then, but told him to go back to the State line and I would telegraph him \$400 then. This was done. Hall got on the train for Fruitdale, and when the train arrived there the labourers were on the platform, demanding their money. The foreman rushed in and stated that the men were highly excited and that they must be paid. Hall coolly took from his pocket a big roll of bills and told the foreman that he had more than enough money to pay every man. He then took from another pocket a big package of what purported to be bills and handed it to the foreman, saying that there was the exact amount to pay the men. The foreman took the package and got off just as the train was pulling out. He found on examination that the package was merely paper cut up and shaved down to the size of bills and wrapped in paper.

"Hall went on to Mobile that night and borrowed \$300 from a friend and then disappeared, but has never been seen or heard of since, until I met him to-day. He wrote to me on June 17th on stationery of a prominent Mobile hotel where he was not stopping, saying that he was feeling sick that day, and that 'the mental strain and worry of the last 14 days has almost broken me up.' This was the day that he disappeared and the last that I ever heard from him. He got about \$10,000 of my property on a tissue of falsehoods.

"Hall was the reverse in his habits of what a defaulter usually is. He was frugal and stingy, and his habits were correct, and he never tasted liquor. At Chicago, if I did not invite him to my house, he would go to a twenty cent lodging house and a ten cent lunch counter. He does not act differently now from what he did when I knew him before. There is the same timid hesitancy in his bearing. Although he was a systematic embezzler, I shall not have him arrested or prosecute him, as I do not care to be revengeful, and he has no means that I know of.

"His is a most curious case. I don't think that he was crazy during the six months that he was in business with me, and I am at a loss to know whether he is feigning his present condition. If he is, which I think probable, he may have sent for me on account of being oppressed by the fear of arrest. He can remember the good that he has done and forget the evil, for he seems not to remember his bad deeds in Alabama."—*Mankato Free Press*.

In a final private conversation with Hall the following day before returning to Chicago, Mr. B. talked very kindly, offering to forget the past and to furnish Hall with money to go wherever he wished to make a fresh start if he would only tell Mr. B. what he really knew. At this Hall broke down in tears and sobs, but could recollect nothing more and could not accept the offered assistance.

On the other hand, a Mr. W. P. Stuart, who had known Hall in Fruitdale, wrote on November 4th to the effect that he was convinced of Hall's honesty, and that his affairs, although involved, would have been by no means hopeless but for his flight. With regard to the question of Hall's mental condition, a letter was received from a clergyman who had known him in Dallas, Tex., and who spoke of him as having been temporarily insane. In response to enquiries there came the following letter :—

“ State of New York—Manhattan State Hospital,

“ Ward's Island, New York City,

“ March 22nd, 1898.

“ A. E. Macdonald, M.D.,

“ General Superintendent.

“ Male Department,

“ Percy Bryant, M.D.,

“ Medical Superintendent.

“ DEAR SIR,—I beg to say, in reply to your inquiries, that on the 27th of August, 1895, a patient named John Harrison, *alias* Hall, was admitted to this hospital. The patient was discharged before I assumed charge here as Superintendent, and therefore I am unable to give you any information derived from personal observation. He was discharged November 12th of the same year, and sent to Dallas, Texas, where his parents lived. [?] It appears from the records that correspondence had been had with Rev. Edward Wickens, of Dallas.

“ The records further show that the patient, upon admission here, was dull, depressed, and could give little information of himself, except to state that he had been in New York City a few days. It also appears that he was much confused. When released he had recovered from his depression and physical inactivity.—Very truly yours,

“ PERCY BRYANT,

“ Medical Superintendent.

“ Harlow Gale, Esq.,

“ Instructor, Univ. of Minnesota,

“ Minneapolis, Minn.”

This former condition, resembling his present, except with only a few days' loss of memory, occurred then shortly before the starting of the Fruitdale lumber scheme in which he interested Mr. Blanchard.

It appeared later, from articles in St. Paul papers, that in 1893 Hall had been obliged to leave Seattle, where he had had the management of a large box factory. But no serious dishonesty had been alleged against him there. His career in Seattle was terminated by the discovery that he had undergone a term of imprisonment in St. Quentin, California. Of the particulars of this affair in California very little seems to be known. From Seattle Hall went to Dallas; then followed his stay at the N.Y. State Insane Hospital, and some time after that the Fruitdale venture. An attempt was made to obtain from him some connected account of his career by means of hypnotism, but with very little success.

Finding no employment in Mankato, he went to Minneapolis in December, 1896. There, while seeking some opening in business, he took a prominent

part in church work, and won the good opinion of his neighbours. But in the autumn of 1897 he disappeared once more, leaving behind him no few unpaid bills and an impaired reputation. He was in Cincinnati during the winter of 1897-8, and since then nothing more has been heard of him.

After examining Hall's career, Professor Gale concludes as follows :—

When one now looks back at this strange mental composition to analyse and classify it, there appear to me two important characteristics connecting him over toward the insane :—

1. A delusion of grandeur ("Grössewahn"), as shown in his restless activity and pleasure in these series of large schemes started on such small foundations. His absorption in them was intense, leaving him no interest for other things except his church. In my interviews with him he could not get away from talk about himself ; some brain micro-photographs, which I, for this purpose, showed and explained to him, had no interest. In his church, even, he pushed himself forward everywhere in an unusually commanding way.

2. The common delusion of persecution was an easier explanation of his series of failures than the reasonable and evident lack of foresight, capital, and honesty. But no such reasoning would affect him. The reversed judgment of his friends, who came to know him thoroughly, was explained, too, in this way.

With these two characteristics (which are only exaggerations of the normal mental components of pleasure in activity and success and of suspicion) is specially connected the question of his consciousness of, and responsibility for, his dishonest practices, however great or small they really have been. That he is not a deliberate and conscious knave or even deceiver seems to me reasonably sure. And as, therefore, the ordinary motives which influence the average mental complex do not come into his consciousness with inhibitory or motor force, he is not responsible as is the average man. His case is of much interest as an example of the many components of knowledge, feeling, and motor reaction which constitute the manifold complex "mind," and the limitless variety of these combinations, with the wide transition region between sanity and insanity. In its non-responsibility the case illustrates the modern "necessity" of willing as a reign of law in mental life as against the old metaphysical "freedom." His would be a case for restraint and not punishment.

As to the genuineness of his loss of personal identity, it seems to me, in some degree at least, genuine, and perhaps entirely so. His unstable, nervous temperament, with its excessive activity and disposition to worry and suspicion, were favourable to such a catastrophe. For the first collapse in August, 1895, though only of short duration, there seems no motive for deception, except to gain a retreat and rest. And if the first one was genuine, the still greater stress of circumstances, which culminated in June, 1896, was enough to cause a greater mental rupture. However, the suspicious limits of his recovered memories remind one of the often wonderfully ingenious reasoning and cunning self-control of the insane, and give much evidence to the probability that with some real loss of



memories he consciously or unconsciously—as it seems to me more likely—used his more or less mental break to cover his dishonesties.

I am fortunate to be able to append the following examination of Hall by Dr. H. A. Tomlinson, superintendent of the St. Peter State Hospital for the Insane, and gratified at the approach of my conclusion to his judgment founded on such greater knowledge and experience.

*March 23rd, 1898.*

. . . “The man was undoubtedly an epileptic, the muscular outbreak being absent and replaced by the establishment of so-called double consciousness. This was evidenced by his conduct and history, both as given by himself and learned from others afterwards. I did not know anything about Mr. Hall’s ever having been in a hospital for the insane, but should not be surprised to learn that he had been in several, for that is the usual history of such cases. I did not make any notes of my examination of this man, and my recollection is hazy as to details. I remember, however, having noticed several of the stigmata of degeneracy, and among them some asymmetry in arterial distribution and location. Besides, the radial artery on one side was quite small, while on the other it was disproportionately large. Several similar cases to this one have been reported, and one I knew myself much more peculiar than this one. There is no doubt but that this man was markedly defective morally, and traded on his peculiar mental condition, of which he was more conscious than he was willing to admit or recognise. These cases seem very clear to me, presenting only exaggerations of those characteristics which make the confidence man and dead-beat conspicuous, the difference being that one acts with definite purpose and from preconceived plans, while the other is the creature of environment, apparently acting automatically upon the suggestions of those around them.”

In the prosecution and preparation of this study I wish to express my sincere thanks for the kindness and aid of Rev. George H. Davis, Dr. J. W. Andrews, Prof. C. F. Koehler, Mr. E. F. Searing, of the *Mankato Free Press*, and Dr. H. A. Tomlinson.

## III.

## NOTE BY MR. F. C. S. SCHILLER

ON HIS REVIEW OF PROF. MÜNSTERBERG'S "PSYCHOLOGY AND MYSTICISM"  
(in *Proceedings S.P.R.*, Part XXXV., p. 348).

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I am informed that I have given great offence to Professor Münsterberg by quoting incompletely (in the last number of these *Proceedings*, p. 358) a passage from the article on "Psychology and Mysticism," which I criticised and which he has since republished in a book entitled *Psychology and Life*. I feel, therefore, that I owe Professor Münsterberg the reparation of a full quotation and the readers of the *Proceedings* an explanation of how the omission came about.

Professor Münsterberg's full context runs as follows (*Atlantic Monthly*, p. 78; *Psychology and Life*, p. 260): "Why do I avoid these séances? It is not because I am afraid that they would shake my theoretical views and convince me of mysticism, but because I consider it undignified to visit such performances, *as one attends a variety show, for amusement only, without attempting to explain them*, and because I know that I should be the last man to see through the scheme and discover the trick."

The clause which I have italicised was omitted by me (with the due asterisks) simply because it seemed to me to convey the unjust and offensive innuendo, which recurs with such deplorable frequency in his article, viz., that no one can be supposed to take an honest, serious and scientific interest in the topics of Psychological Research. Now I had already protested against this assumption on p. 350 of my article, and so did not think it necessary to draw attention to a passage whose only novelty seemed to consist in the indication it seemed to afford of a habit of attending variety shows on the part of Professor Münsterberg. Of course if I could have anticipated that this omission would really prevent Professor Münsterberg from defending himself against the somewhat damaging charge of having recklessly misconstrued the whole position of the S.P.R., I should have been delighted to quote him at any length.

As it is, I regret rather that I quoted so much and did not omit the first clause also. For, I may point out, it was not really and seriously relevant to my general argument, or even to the immediate discussion, which at that point consisted mainly of "chaff" about the "surpassing innocence of his character" to which Professor Münsterberg had so ingenuously confessed.

On the whole, therefore, I may assure the readers of *Proceedings* that my position with regard to the passage complained of is thoroughly defensible, though I forbear to show this more fully lest I should continue to give offence to Professor Münsterberg.

With regard to that gentleman himself, I can only say that if he feels hurt I am sorry, but that he himself provoked the tone of my criticism by

the tone of his article. The S.P.R. has always welcomed criticism ; it has endured much that was not always inspired by that fulness of knowledge and accuracy of statement which are so conducive to the effectiveness of a criticism. But the time has come when it need no longer offer both cheeks to any smiter who chooses to arm himself, not with reason, but merely with the professorial handle to his name. Nevertheless, I believe I am expressing only the general sentiment of the S.P.R. in saying that, as soon as there is manifested on Professor Münsterberg's side any disposition to drop the misplaced and irritating tone of professorial superiority and *a priori* dogmatism, there will be no persistence in the *persiflage* which was needed to defend the interests of the S.P.R. from being crushed by the weight of authority in the eyes of the general public. And as soon as it is admitted by Professor Münsterberg that on our side also there are men who are honestly desirous of truth and fact, we shall readily respond by admitting that truth and fact are far more likely to be elicited by co-operation for truly scientific purposes and by truly scientific methods, than by recrimination. And even if co-operation should prove impracticable, the field of psychology is surely wide enough to admit of the toleration of those who, in a truly scientific spirit, though perhaps with more than the usual scientific boldness, have ventured to penetrate into a vast and intricate mass of residual phenomena.



## III.

## REVIEWS.

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*The Alleged Haunting of B—— House, including a journal kept during the tenancy of Colonel Le Mesurier Taylor.* Edited by A. GOODRICH-FREER (Miss X.), and JOHN, MARQUESS OF BUTE, K.T. (London: George Redway. 1899.)

This is the house of which Mr. Myers wrote to the *Times* in June, 1897: "I visited B——, representing that Society [the S.P.R.] . . . and decided that there was no such evidence as could justify us in giving the results of the inquiry a place in our *Proceedings*."

From the evidential standpoint Mr. Myers' verdict was no doubt right; but nevertheless the Editors, who prefix the above quotation as a motto to their book, are in my judgment fully justified in publishing the result of that inquiry. Of the hundreds of accounts of haunted houses contributed during the last seventeen years to the Society's archives, we have very few—probably not more than four or five—in which the evidence is based, to any considerable extent, on contemporary documents, and any addition to this small number is therefore very welcome.<sup>1</sup> And B—— House is a fairly typical case. The Editors bring forward what appears to be ample proof that the house had the reputation of being haunted for many years prior to their visit, though the manifestations on which that reputation rested would seem to have consisted mainly of inexplicable sounds; there is but little first-hand evidence for visual phantasms prior to 1897, and where first-hand documents are quoted, they do not always bear a date (see *e.g.*, pp. 13 and 17). But the phenomena during Colonel Taylor's tenancy were surprisingly numerous and varied, and were recorded day by day and with all needful fulness.

For all that, the journal is quite unimpressive. Partly, no doubt, this very want of impressiveness is a tribute to the excellence of the record; if the ghostly sights and sounds in other haunted houses had been as faithfully set down day by day, they might be found to make even duller reading.

But there are other reasons why this laborious journal of "phenomena" fails to impress. The inexplicable incidents recorded consist mainly of sounds, occurring most frequently at night, but not seldom also in the day-time, which are described by numerous witnesses as being unusually striking, distinct, and varied in character. No experiment, however, was made to determine the actual source of the sounds. A phonograph is mentioned

<sup>1</sup> One such case is recorded in *Proceedings*, VIII., p. 331, another in *Proceedings*, VI., p. 236. One or two other cases will be found in the *Journal*.

in the early part of the book, but it was never employed. The house, as the Editors inform us, stands in a well-known seismic area; but a seismometer was actually introduced only on the last night of the tenancy, when the noises had already ceased to be heard. But apart from the use of instruments requiring, probably, expert handling, there is much that might have been done and was not done. The bedrooms, or some of them, were fitted with fixed basins, with pipes communicating with the outer air; and the whole house was furnished with a system of hot water pipes, which were in working order during part of the period under consideration. Some experiment might surely have been made to determine whether these metal pipes, contracting or expanding under changes of temperature, had anything to do with the sounds heard. Miss Freer and her coadjutor do not appear even to have determined whether or not the sounds which they described day after day were objective. Professor Lodge thinks that they were, but "not impressive," and "not distinct enough to be evidential" (pp. 192-200). Colonel Taylor writes, "I heard many noises in the night during my stay at B—, but they were of much the same sort I have been accustomed to hear at a similar time in other houses. I think that some of our witnesses may have given them undue prominence under the influence of their own expectancy. The clairvoyant visions of 'Ishbel' in the grounds are not of great evidential value for the scientific world in general, and I think that any amount of 'voices' could be read into the noises of the running stream, near where she is seen, by those who wished to hear. Still there are some objective noises which cannot be easily accounted for in an ordinary way" (p. 153).

But, after all, the main reason why the recital of these various manifestations fails to impress us has still to be told. It was Miss Freer who first saw a ghostly figure; it was again Miss Freer who first heard ghostly noises, and throughout these records it is Miss Freer who is most frequently and most conspicuously favoured with "phenomena." Miss Freer has shown that she knows how to observe clearly and how to record accurately, but, for all that, her testimony in a matter of this kind carries very little weight.

Of course I intend nothing in derogation of Miss Freer's moral and intellectual qualifications. Miss Freer's testimony to ghostly sights and sounds fails to impress because she is liable, in a quite unusual degree, to hallucinatory experience, and because many of her hallucinations, as she has herself shown in her article on Crystal Vision (*Proceedings* V., pp. 486 *et seq.*), are purely subjective, being merely the recrudescence of previous sensory or intellectual experiences. Miss Freer is not, of course, responsible for this mental idiosyncrasy, but, obviously, it seriously impairs the value of her testimony to experiences which owe their interest, for most readers, solely to the presumption that they may not be of a purely hallucinatory order, but may possibly have their origin in telepathic or even extra-human sources. But we are entitled to ask why Miss Freer does not apply to her visions in "haunted" houses the same canons which she has applied with conspicuous success to her visions in the crystal. When, to take a well-known case, Miss Freer saw in the crystal a vision of a sea-shore succeeded by the vision of a huge mouse (*Proceedings*

V., p. 512), she found the explanation of the incongruous spectacle in the revival of subconscious images called up in her mind a few days before by two lines of poetry :—

“ Only the sea intoning,  
Only the wainscoat mouse.”

Now consider the following experience in B— House. “In the afternoon (February 6th, 1897) an experiment was made with the apparatus known as a Ouija board, and this, as is very often the case, resolved itself, after a time, into automatic writing. There is in the library a portrait of a very handsome woman, to which no name is attached, but which shows the costume of the last century. Her name was asked, and the word ‘Ishbel’ was given several times. . . . In reply to questions as to what could be done that was of use and interest, the writers were told to go at dusk, and in silence, to the glen in the avenue, and this, rightly or wrongly, some of those present identified with what had been called Scamp’s Copse” (*i.e.*, a small fir plantation, at which the dog had twice already barked uneasily). They went to the copse accordingly, and Miss Freer saw there, in the dusk, the figure of a nun. Upon this Miss Freer builds a theory of the identity of the nun with the Ishbel named by Ouija ; but as the figure seen did not correspond with the portrait already referred to, she finds the original of her vision in one Isabella (Gaelic, Ishbel), a sister of a former owner of the house, who had actually taken the veil. Again, since the figure of the nun as seen did not apparently tally with the figure of the lady whom it was assumed to represent, the theory is still further elaborated by the assumption that the figure seen by Miss Freer reflected the (hypothetical) mental picture of the real nun formed in the mind of a former inmate of the house, who had never seen the lady in the flesh ! (p. 226, etc.) For myself, I can more easily believe that the nun, like the mouse, had her birth in the percipient’s imagination.

Whatever view we take of the matter, it is a point of some interest that two witnesses—one other lady and one man, the Rev. Mr. Q.,—after they had heard what Miss Freer had seen, saw the nun in her company ; and that several persons, when the burn was running, heard the nun and her companion conversing. The only other experience of evidential importance was the vision of a crucifix, seen independently by the Rev. Mr. Q., Miss Freer, and a gentleman who had stayed in the house some years previously. The account of the last named, however, is given some years after the incident, and after he had heard of the later experiences.

It should be added that Miss Freer saw one or two other human figures, and dogs, about the house at various times ; that a servant also saw a figure, and that a visitor, Miss Langton, had a dream in which she saw the figure of a man, afterward recognised from her description. Of the pulling off of bedclothes, the feeling of chill, the sensation of a presence, and other vague feelings of the kind, it is hardly necessary to speak.

The phenomena ceased after a service performed by a Bishop on the morning of May 6th.

FRANK PODMORE.



*Neurypnology: or, The Rationale of Nervous Sleep considered in relation to Animal Magnetism or Mesmerism, and illustrated by Numerous Cases of its successful Application in the Relief and Cure of Disease.* By JAMES BRAID, M.R.C.S., C.M.W.S., etc. A New Edition, edited with an Introduction Biographical and Bibliographical embodying the Author's later Views and further Evidence on the Subject. By ARTHUR EDWARD WAITE. (London: George Redway; 1899.)

The above work comprises: (1) a biographical introduction, which gives a short account of Braid's life, and a more extended one of his writings; (2) a reproduction of the original edition of *Neurypnology*, which forms the greater bulk of the volume; (3) an appendix of editorial notes chiefly drawn from Braid's later works; and (4) a bibliography of Braid's writings.

According to Mr. Waite little is known about Braid in this country, and there is no trustworthy bibliography of his writings. His later works, rare and almost unknown, have been traced with difficulty. Mr. Waite, however, claims to have been successful in his researches, and presents, as their result, a work on Braid which he asserts to be substantially complete.

Mr. Waite has undoubtedly rendered a service to hypnotic literature, but he has not made the most of the material collected, and his work is neither as complete nor as accurate as he believes it to be. The following are a few, amongst many, objections which might be urged against it. Thus, we are told that the discovery of hypnotism alone lifted Braid out of the groove of the ordinary practitioner. This is incorrect. Before he discovered hypnotism, Braid had operated on 262 cases of Talipes, 700 cases of Strabismus, and 23 cases of Spinal Curvature, and his reputation as a surgeon attracted patients from all parts of the country.

Although *Neurypnology* is historically interesting, it must not be forgotten that it was written almost immediately after Braid commenced his hypnotic work, and that later his views underwent a complete change. The French translation of *Neurypnology* is more valuable than Mr. Waite's reprint, as in an appendix of 36 pages it gives a full translation of Braid's last manuscript, containing a summary of his more matured theories. This is barely referred to by Mr. Waite.

Some passages show a somewhat unusual want of care in compilation. Thus (pages 317 to 319) a case of torticollis cured by suggestion is recorded at length, and we are told that a year later there had been no relapse. The case is repeated (pages 343 to 344) along with the fresh statement that "whether the cure was permanent does not appear from the narrative."

The bibliography, to which Mr. Waite attaches great importance, only imperfectly reproduces those I myself published at earlier dates (*Brain*, Part LXXIII., Spring, 1896; *Proceedings of the Society for Psychical Research*, Part XXX., June, 1896; *Revue de l'Hypnotisme*, June, 1898). In seven of the works referred to by Mr. Waite, the title is wrong or incomplete; in twenty-one, quoted from medical journals, neither volume nor page is given. The only new material is two unimportant letters in reply to one from "James Simpson, Esq., Advocate," erroneously described by Mr. Waite as "James Singer," while in my bibliographies are to be found nine of Braid's writings which are omitted from Mr. Waite's.

Further, some of Braid's works, of which Mr. Waite has been unable to trace a single copy, have long been in my possession.

According to Mr. Waite, Braid's connection with the debated subject of phrenology terminated with the phreno-hypnotic experiments recorded in "*Neurypnology*." The subject, we are told, remains where he left it, while Dr. Foveau de Courmelle's verdict, viz., that the phenomena are as likely to be the result of real action as of unconscious suggestion, is accepted by the unprejudiced. Mr. Waite is ignorant of, or ignores, Braid's later experiments. From these Braid concluded that all phrenological phenomena were the result of direct or indirect suggestion, and arose either from a previous knowledge of phrenology, or from a system of training during hypnosis. They manifested themselves when points were touched which audible suggestion had taught the subjects to associate with particular ideas. Further, pressure upon the head was not necessary, and the associations could be established in the same artificial way between the phenomena and other parts of the body. Finally, the excitation of the muscles of expression was capable of arousing corresponding ideas in the subject's mind.

It is certainly surprising to be told at the present day that the phenomena of phrenology are as likely as not to be genuine.

Mr. Waite's account of Braid's views on mesmerism or animal magnetism demands serious notice. Formerly, according to him, it was generally but erroneously believed that Braid opposed animal magnetism; thanks, however, to French writers, this error has been rectified. Braid was the champion and witness of animal magnetism, and placed the central fact of mesmerism beyond all further dispute. In opposition to this, it is to be noted, and the point cannot be too strongly insisted upon, that the primary and cardinal fact upon which Braid founded all his theories of hypnotism was the purely subjective origin of its phenomena. Everything depended on the physical and mental condition of the patient; not on the volition or passes of the operator which threw out magnetism, or called into action some occult fluid or medium. Braid never swerved from this view. Later he repeated it more emphatically and demonstrated experimentally that all the phenomena attributed to magnets, metals, drugs in sealed tubes, could be equally well produced by direct or indirect suggestion when the exciting agents were absent. Thus, a wooden magnet acted like a real one when the subject was deceived as to its nature, whereas a genuine one produced nothing if the subject did not know it was there.

According to Mr. Waite, "The metallo-therapeutic theory first discovered by Burq and developed by Charcot, inaugurated a new era of hypnotism, and led to that long series of researches connected with the Salpêtrière School of Paris, which have done so much to obscure, if not indeed to tarnish, the rival lustre of the Nancy School of Liébault."

This view of metallo-therapeutics is many years too late. The views of Burq, which were shared by Elliotson and the mesmerists of his day, were successfully combatted by Braid. Charcot simply revived an old fallacy, which the Nancy School, following in Braid's footsteps, again overthrew. As far back as the International Congress of Psychology of 1892 the Charcot theories had practically ceased to excite scientific interest.

According to Mr. Waite, "It has been rightly observed that Braid, the discoverer, so to speak, of suggestion, by no means fully realised the possibilities of suggestion of the unconscious kind" Bernheim, and other writers unacquainted with Braid's later works, had previously made similar statements. It is difficult, however, to understand how Mr. Waite could commit a like error. Braid repeatedly, clearly and emphatically pointed out that unconscious suggestion accounted for all the phenomena which the mesmerists attributed to magnets, metals, and occult fluids. Sometimes, he said, the suggestions were given in their most obvious form by the operator verbally predicting in the subject's hearing the nature of the phenomena which ought to appear. At others they were given less directly by passes, manipulations, excitation of the muscles of expression, etc., but these owed their power, not to their physical influence, but to the ideas they excited in the subject's mind. When all forms of suggestion were rigorously excluded the mesmeric phenomena did not appear. This, Braid asserted, proved the influence was a mental one; were it electric, suggestions were as unnecessary in the case of the subject as in that of a galvanic battery.

Apparently Mr. Waite believes in animal magnetism, metallo-therapeutics and phrenology. All these things Braid successfully combatted, and to attribute to him any belief in their existence shows an absolute failure to grasp the spirit and significance of his teaching.

J. MILNE BRAMWELL.

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*My Experience of Hypnotic Suggestion as a Therapeutic Agent.* By HON. J. M. CREED, M.L.C., M.R.C.S. Eng., L.R.C.P. Edin., Sydney. (Sydney, 1899.)

This paper is reprinted from *The Australian Medical Gazette*, of January 20th, 1899, and was originally read before the N.S.W. Branch of the British Medical Association, on December 23rd, 1898. The author is a well-known medical man and a member of the Legislative Council of New South Wales, so his views demand attention. He has been using hypnotism in his practice for some years, and he recalls his experience in this pamphlet.

The interest of the paper is almost entirely medical, and, like Professor Bernheim of Nancy, the author hypnotises patients for all kinds of diseases, and finds suggestion relieves symptoms, even where it does not cure the disease. For instance, in a case of pleurisy, accompanied by much pain, he was able to give entire relief by suggestion, and it was only by the persistence of the physical signs that he was able to assure himself that the pathological condition still existed. As he points out, the control of pain is a very important matter in many cases, for pain lowers vitality, prevents sleep, and retards recovery. This is especially the case in surgery, where mental and bodily repose are greatly conducive to satisfactory and rapid repair. Dr. Creed is quite enthusiastic about the effect of suggestion in gastric ulcer. Here, he thinks, it acts by enabling the patient to take nourishment freely without pain, and so cures the ulcers by improving the state of the blood and raising the tone of the system.

On several occasions the patient has, in the absence of the surgeon, been able to control pain by inducing anæsthesia by a prearranged signal.



For instance, Mr. H. burnt his hand severely with molten and flaming "Chatterton's compound," the melting point of which is very much higher than that of sealing-wax. By placing the forefinger of his left hand to his lips he was able to inhibit the sensory nerves, and though the injury was followed by deep ulceration, he felt no pain while the wound was healing.

Pain, of course, serves a useful purpose, and is Nature's danger-signal to direct attention and care to the part. Without it, serious injury might be done without the subject being aware of it, as sometimes happens in hysterical and paralysed persons. But pain is nearly always in excess of requirements, and it is a great blessing to be able to control it.

Dr. Creed seems to have been very successful in asthma, and he mentions a curious experience in this disease. The patient was a clerk in a Government office, and had suffered from asthma for 14 years. Hypnotism greatly relieved her, and she only got attacks when she took cold. When away from Sydney, she used to write to the doctor and ask him to send directions by telegraph, and these suggestions always gave her relief. On one occasion he was away, and her friends arranged that a telegram should be sent as if from him in reply to her note. It failed to relieve her, as she said she knew somehow it was not genuine. On his return he sent another telegram, and that proved successful. It is difficult to believe that the curative agency was anything but expectant attention and suggestion in this case, and one would like to know if there was not something in the form of the first telegram or in the behaviour of her friends which aroused the patient's suspicions.

Dr. Creed has treated many bad cases of alcoholism with great success; and he says that no one who is a good hypnotic subject need suffer from sea-sickness.

Like all medical hypnotists, Dr. Creed finds his best subjects among the intellectual and healthy-minded classes, and he even says that he has generally found hysterical women are not hypnotisable. He is strongly of opinion that the dangers of hypnotism are overstated, and he contends that a hypnotised person cannot be made to do anything opposed to his waking conscience. There we must beg to differ from Dr. Creed, and we think it is much better not to minimise the dangers of hypnotism, but to look them in the face and take the proper precautions against them. Granted that a person of strong principle cannot be induced to commit a crime whilst in the hypnotic state, or by means of post-hypnotic suggestion; but we know that many people only refrain from doing evil from expediency or fear. These are the weak-kneed people who are often too easily influenced without hypnotism. By suggestion with hypnotism the power to influence them is greatly increased, and the danger of this fact is not decreased by ignoring it. The remedy is, of course, easy. Let hypnotism be used with the same precautions as surround the administration of chloroform or laughing gas.

There is one other feature of Dr. Creed's excellent paper which we think might be amended with advantage. It is too much a record of success, and though he disclaims considering hypnotism a universal panacea, we should like to be told what proportion of failures he has met with. However, in this respect Dr. Creed is in good professional company, for medical men who write on their special subject are notoriously optimistic.

It must be a matter of gratification to the Society for Psychical Research to see one of the subjects Edmund Gurney and its other founders did so much to bring to the front now occupying a recognised and honourable position as a therapeutic agent. This is shown by such papers as this by Dr. Creed, and is a sign of the times.

CHAS. LLOYD TUCKEY, M.D.

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*The Effects of Mind on Body as Evidenced by Faith Cures.* By HENRY H. GODDARD, A.M., Fellow in Psychology, Clark University. (*The American Journal of Psychology*, Vol. X., No. 3; April, 1899, pp. 431-502.)

The author here presents "a brief survey of the field, that portion of the data which is of most psychological interest, and some of the conclusions, from an extended study of mental therapeutics. We hope to present in a later publication an extended report of all the work referred to." The material was gathered through answers to a syllabus, personal interviews and investigations with various "healers" and their patients, and from the literature of the various sects,—for the author uses "Faith Cures" as comprehending the various forms of distinctive suggestion treatment. Christian Scientists are largely considered, including a sketch of their leader, Mrs. Eddy; "Divine Healers" are followed up in the large movement of Dr. Dowie, of Chicago, and the itinerant Schlatter; and "Mental Science" as the most reasonable form, through its founder, Dr. Quimby. The author's "result of this investigation, extending over more than two years, is the absolute conviction based upon experience, only one or two items of which we can give here, that the curative principle in every one of the forms is found in the influence of the mind of the patient on his body. In other words, however different the claims and the method, the explanation of all is the same." These religious forms of suggestion are then compared with cases in the practice of regular physicians and in hypnotism. After a review of the failures and limitations of the faith cures, the conclusion is that their "greatest field is in the realm of prevention" (p. 499), and that "suggestive therapeutics must take its place by the side of drugs rather than to assume to supersede them." (p. 488.) The concluding psychological discussion is extremely valuable. Thus the whole article is the most timely, interesting and able study that has appeared in this fruitful field since Dr. and Mr. Myers's *Mind-Cure, Faith-Cure, and the Miracles of Lourdes* (*Proceedings S.P.R.*, Vol. IX., p. 160), which the present study has unfortunately not mentioned, together with the several other studies on this subject in the *Proceedings S.P.R.* But a wide circle of students await Mr. Goddard's complete report with impatient eagerness.

H. GALE.

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*L'Année Psychologique, Cinquième Année.* (Paris, Schleicher Frères.)

*L'Année Psychologique* for 1899 is an even more striking monument of scientific diligence and fecundity than the previous volumes have been. The contributors of important articles, besides those by MM. Binet and

Henri, are perhaps this year more numerous than usual. But the amount of work done by these two *savants*,—the Editor and the *Secrétaire de Rédaction*,—in the way both of original experiment and of accurate and critical analysis of the work of others—seems rather to increase than to diminish. Most strongly must it be said that the volume is an indispensable aid to any student of Psychology.

Yet although this may be truly affirmed, it is only fair to add that in some cases the subjects of research seem scarcely of sufficient importance to occupy for long the attention of scientific minds. The elaborate Bibliography contains the titles of 2,558 books or articles connected with Psychology which appeared in 1898. This list represents a great mass of intellectual effort, from which, perhaps, fewer leading ideas have in fact emerged than might have been hoped in an age where really illuminating generalisations in science are wont to be so promptly pursued.

There are two papers, both of them from M. Binet's indefatigable pen, which bear on subjects which have been discussed in these *Proceedings*: one is on Suggestibility, one on "L'illusion de fausse reconnaissance," for which I have elsewhere suggested the name *Promnesia*. To speak frankly, both these papers seem to me to illustrate a tendency which runs through all this mass of work,—a tendency to treat the *easy* parts of the subject with completeness and mastery, and to ignore altogether those *difficult* parts which we in these *Proceedings* do make honest, if not always convincing, efforts to explain. Thus, the article on Suggestibility sets forth clearly and ingeniously a number of causes which predispose to the supraliminal reception of suggestion; but it scarcely even touches on the profounder puzzles of their subliminal reception,—apart from the conscious knowledge of the subject, and beyond the domain of his conscious will.

Characteristically enough, we find in the course of this article a long analysis of an everyday experiment of Professor Patrick's in automatic script, but of Mrs. Piper's trance-writings—from which that very paper of Professor Patrick's takes its rise—not a word. The easy thing is additionally explained; the difficult problem—Dr. Hodgson's Report—remains as item 1,249 in the Bibliography without further notice.

With the article on the *déjà vu*—paramnesia or promnesia—the case is somewhat similar. The conclusion reached on p. 742 is merely the obvious common-sense one which explains most of the recorded phenomena—but not all. "The memory which is renewed has made a part of another conscious state—call it A—at the moment when it was stored up in the mind, whereas the subject, when he recalls the memory, is in conscious state B. On account of the analogy between the two states, the earlier perception A becomes fused into the actual perception B;—the two syntheses are brought thus into a sort of collision. The Group A tends more or less to reconstitute itself, in spite of the antagonism of the Group B; there is a struggle, a competition, between two different conscious states, resulting in a distressing sense of duplication, of loss of personality. One feels oneself changed and no longer the same, because the forgotten Group A recovers activity and tries to supplant the actual Group B."

M. Binet does not claim originality, except of expression, for this explanation, which is, in fact, obviously sufficient for the commoner cases.



“We may lay aside,” he says, “the hypothesis of cerebral duality due to Wigan, and that of telepathy due to Lalande ;—this latter hypothesis, in fact, is so extraordinary (*tellement bizarre*) that one cannot understand how it can have been put forward in our times.” (Let readers who do not think it worth while to send us fresh evidence for telepathy, because they suppose that every one admits it, note that last phrase !)

Well, it may be that neither telepathy nor cerebral duality in any form is really needed here ;—but if M. Binet wishes to make certain of this, he should first manage to dispose in some way or other of some of the most remarkable of the cases which he himself cites. If, for example, J. H., the military doctor, did in truth repeat phrases from *Ferdinand le Noceur*, before they were uttered by the actors,—as Lalande tells us,—what is M. Binet’s explanation for *that* ?

I certainly cannot complain if M. Binet, with all he has to read, has not read pp. 340–347 of Vol. IX. of the S.P.R. *Proceedings*. Nor can I expect that anything which I have there advanced is at all a final solution of these puzzles. Nevertheless, our psychology is hardly yet so profound that it suffices to set forth with clarity ingenious deductions from what we already know. The history of far less complex studies should teach us that it is dangerous to assume too soon that any given science has for us no more surprises.

F. W. H. MYERS.



PROCEEDINGS  
OF THE  
SOCIETY FOR PSYCHICAL RESEARCH,  
PART XXXVII.

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PROCEEDINGS OF GENERAL MEETINGS.

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The 103rd General Meeting of the Society was held at the Westminster Town Hall, on Friday, January 26th, 1900, at 4 p.m.; SIR WILLIAM CROOKES in the chair.

MR. F. PODMORE read a paper on "Witchcraft and Poltergeists."

A paper by MR. F. C. S. SCHILLER, "On some Philosophic Assumptions in the Investigation of the Problem of a Future Life," was read by MR. J. G. SMITH. This paper was published in Part XXXVI. of the *Proceedings*.

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The 104th General Meeting was held in the same place on Friday, March 2nd, 1900, at 8.30 p.m.; PROFESSOR SIDGWICK in the chair.

MISS M. H. MASON gave an account of two cases of the cure of warts by suggestion.

Some extracts from part of PROFESSOR J. H. HYSLOP's Report of his sittings with Mrs. Piper were read by MR. F. PODMORE.

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The 105th General Meeting was held in the same place on Friday, May 18th, 1900, at 8.30 p.m.; the President, MR. F. W. H. MYERS, in the chair, delivered the Address which follows.



## PRESIDENTIAL ADDRESS

BY F. W. H. MYERS.

## ARGUMENT.

1. Psychical Research is no longer felt to need the recommendation of names independently eminent in other branches of study.

2. Yet if recognition is to be paid primarily to actual work performed for our Research, the name of Edmund Gurney must occur as that which all would fain have honoured;—the man whose attitude towards our Research was of the loftiest, the most unselfish kind; the man who felt most strongly the sheer moral need of discovering a future life, if the cruel injustices of this life are to be conceived as compatible with a First Cause worthy of love or worship.

3. For most men the quest of immortality will answer to a stronger element of personal desire;—although such desire need not imply bias in the estimation of evidence.

4. No attachment to Christian tradition, no recognition of the need and value of high intuitions, should blind us to the fact that only on truths scientifically demonstrated can a world-philosophy or world-religion be based.

5. Yet the facts proved by Science have not thus far been adequate to satisfy the spiritual needs of mankind. May not Science discover further facts which may at any rate *prove the preamble of all religions*?

6. Such is our attempt;—for we believe that by maintaining unity of method in our search for every form of truth we have the best chance of discovering new facts of deep spiritual importance.

7. And, in fact, this line of inquiry has already pointed us to a hidden, subliminal world within us,—and through *that* world to an unseen, but responsive, spiritual world without.

8. And if the messages from that unseen world be felt at present to be inadequate to our desires, yet our faith in the coherence and intelligibility of at least the material universe should lead us to trust that clarity and dignity cannot be permanently lacking in any system of communications which may proceed from the Universe of Spirit.

9. We seem, indeed, to be awakening into a new consciousness of the living solidarity of the human race, in this world and the

next, which will afford an adequate motive for utmost effort and highest hope.

10. But if we are thus to gain the advantage of scientific certainty for our deepest beliefs, we are bound in return to treat the scientific virtues as necessary to salvation.

11. The especial function of the Society for Psychical Research should be to insist upon this view, and to form an advisory centre for widespread investigation.

12. For aid in this task we can address our claim alike to the scientific and to the religious world; our wider Science, of which Religion is the subjective aspect, must come not to destroy, but to fulfil.

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1. When I heard, in absence from England, that the Council of this Society had done me the honour of electing me as its President for the current year, I felt that a certain definite stage in the Society's evolution had been reached at an earlier date than I should originally have expected.

My predecessors in this Chair, I need not say, have, without exception, been men of the highest distinction. The list has included men whose leadership would confer honour on any body of men whatever;—on such bodies, for instance, as the British Association or the House of Commons. We have been grateful to these eminent persons for lending the sanction of their names to our early beginnings. And we have other names in reserve of similar distinction;—destined, I hope, some day to adorn our list of Presidents. Yet for the current year the Council have preferred to choose a man who has little claim to such a distinction, beyond the fact that he has worked for the objects which our Society seeks, from days even before the Society's formation;—and that he is determined to go on thus working so long as his faculties may allow. So have our friends chosen; and if a man may speak thus of his own election, I think that the choice is appropriate enough. For the time has come when we may fairly indicate to the world that we believe our Society can stand on its own bottom; that it carries on a branch of scientific work which, although novel and tentative, is legitimate and honourable; and therefore that we do not need to put forward in its prominent positions only those names which have been made independently illustrious by good work of other kinds performed elsewhere. As representing the principle that the plain, unadorned Psychical Researcher is just as respectable in his own way as anybody else, I am proud indeed to see my humbler name inscribed after the names of Henry Sidgwick, Balfour Stewart, Arthur Balfour, William James, and William Crookes.

2. But here one thought must rise ;—must rise for all who knew the early days of this research, but most of all for me ;—Would that Edmund Gurney were standing where I stand now ! For us who knew him best the years since he left us have but served to illustrate his uniqueness and to deepen his memory ;—have made us feel how much of the humorous adventure, the sympathetic fellowship, the deep delight of this research of ours has with him passed irrevocably away. On the lighter side of things, we can never renew the intellectual enjoyment of those years of our small beginnings spent at his side ;—watching how his flashing irony, his fearless dialectic, dealt with the attacks which then poured in from every quarter ;—with the floundering platitudes of obscurantist orthodoxy, or with the smug sneers of popular science, belittling what it will not try to understand. On the graver side, we shall hardly see another example of just that attitude of mind with which Gurney entered on this research,—and which made for us so deep an element in his incomparable charm.

For in that many-chorded nature sympathy was the deepest strain ;—sympathy which flowed forth indeed to those he loved in such penetrating and intimate tenderness as few mortals have had the happiness to know,—but which expended itself more widely in a profound compassion for the multiform sorrows of men. And thus, as needs must happen in those responsive minds which hear, in the Apostle's words, the whole creation groaning and travailing until now, there came to him the conviction that the question of life after death was the only test which we could really apply to the existence of a Providence ;—nay, that it was no mere *articulus stantis aut cadentis Ecclesie*, but in sombre earnest, for all humankind, the *articulus stantis aut cadentis Dei*. Strangely enough, it was for others rather than for himself that Gurney desired this great possibility ; his own mournful and stoic temper dwelt little on any personal hope. But he felt that if the First Cause has summoned into life on earth, though it were but one single man alone, miserable amid all the happy ;—one single soul foredoomed to eternal protest and inescapable woe ;—then that First Cause is not a God to whom a good man can offer love, or a just man worship. Alas ! how many theologies does this clear moral axiom shrivel as with burning fire ! how many philosophies does it scatter to the winds !—philosophies of men walking delicately on wordy bridges across the grim abyss of things, —satisfied that the world is well enough, while round them wronged, degraded lives by millions are perishing in agony and for ever. It was in response to such easy optimism that Gurney's logic was the most intolerably trenchant, his sombre silence the fullest of sad scorn ;—for in truth this contented blindness of sealed spirits is in itself the vilest woe of man. *He* could not avert his eyes, and disport himself in a fool's Paradise. *He* could not weave a web of words, and stifle



in a philosopher's dream. Suffer me to apply to my friend for a moment even those lofty lines in which a great poet has invoked the greatest :—

“Thou that seest universal Nature moved by universal Mind ;  
Thou, majestic in thy sadness at the doubtful doom of humankind.”

3. It is well that this noble figure should stand at the entrance of our research ;—should show how unselfish may be the impulse which has prompted to eager labour,—eager even beyond the limit which physical powers allowed. But assuredly the mass of us Psychical Researchers have no need whatever of heroic virtue. We have enough and to spare of such motives as appeal to ordinary men. We have the stimulus of intellectual curiosity,—more richly satisfied, I think, in ours than in any other quest ;—and beyond this most of us, I think, have the healthful, primary desire for the prolongation—the endless prolongation—of life and happiness. I know, indeed, that for various reasons some men of strong and high nature, as well as many men of feebler nature, do fail to feel this desire ; but on the whole one must regard that form of *Welt-Schmerz* as but a passing mood of our race's immaturity,—as what physicians call a *neurosis of development* ;—one must admit that usually when a man cares little for existence this is because existence cares little for him, and that it has been doubt as to the value of life and love which has made the decadence of almost all civilisations. Life is the final aim of life ; the mission of the highest Teacher was that we might have it the more abundantly ; and the universe strives best towards its ultimate purpose through the normal, vigorous spirit to whom to live itself is joy.

The danger, then, for our research will lie not in lack but in excess of motive ; our minds may be biassed in their judgment of evidence by a deep instinctive desire. For my own part, I certainly cannot claim such impartiality as indifference might bring. From my earliest childhood—from my very first recollections—the desire for eternal life has immeasurably eclipsed for me every other wish or hope. Yet *desire* is not necessarily *bias* ; and my personal history has convinced myself—though I cannot claim that it shall convince others also—that my wishes do not strongly warp my judgment,—nay, that sometimes the very keenness of personal anxiety may make one afraid to believe, as readily as other men, that which one most longs for.

For when, after deriving much happiness from Christian faith, I felt myself forced by growing knowledge to recognise that the evidence for that culminant instance of spirit return was not adequate, as standing alone, to justify conviction, I did honestly surrender that great joy ; although its loss was more grievous to me than anything else which has happened to me in life.

Then with little hope—nay, almost with reluctant scorn—but with the feeling that no last and least chance of the great discovery should be thrown aside, I turned to such poor efforts at psychical research as were at that time possible; and now it is only after thirty years of such study as I have been able to give that I say to myself at last, *Habes totâ quod mente petisti*—"Thou hast what thine whole heart desired;"—that I recognise that for me this fresh evidence,—while raising that great historic incident of the Resurrection into new credibility,—has also filled me with a sense of insight and of thankfulness such as even my first ardent Christianity did not bestow.

Yet if I thus find the happiness which sprang from far-reaching Tradition and Intuition surpassed by the happiness which springs from a narrower, but a more stable range of demonstrated fact, I nevertheless speak in no spirit of reaction or of ingratitude towards traditions and intuitions which must yet, for many a century, be potent for the salvation of men. I by no means take for granted that any scientific inquiry, any induction from empirical facts, can afford to man his only or his deepest insight into the meaning of the Universe. I have no controversy with those who say that contemplation, revelation, ecstasy, may carry deep into certain hearts an even profounder truth. I recognise also that our Science is a conventional structure; that it rests on assumptions which we cannot fully prove; or which even indicate, by their apparent inconsistency, that they can be at best but narrow aspects of some underlying law imperfectly discerned. All this we may all admit; just as we admit the inadequacy, the conventionality, of human speech itself. Speech cannot match the meaning which looks in an hour of emotion from the eyes of a friend. But what we learn from that gaze is indefinable and incommunicable. Our race needed the spoken and written word, with all its baldness, if they were to understand each other and to grow to be men. So with Science as opposed to Intuition. Science forms a language common to all mankind; she can explain herself when she is misunderstood and right herself when she goes wrong; nor has humanity yet found, at any rate, since that great wedding between Reason and Experience, which immortalises the name of Galileo,—that the methods of Science, intelligently and honestly followed, have led us in the end astray.

It is only in the region of inquiry into a spiritual world—I mean a world of immaterial and yet individual realities—that these truisms are still in danger of being taken for paradoxes. At once the intimate interest and the extreme obscurity of that investigation have long prevented it from being kept fully and fairly in that scientific field where man's attempts at all other knowledge are now collected and appraised. In their rude beginnings, no doubt, Religion and Science

were indistinguishable. The savage observed such scanty facts as he could get at, and tried to shape both his practical and his spiritual life upon that observation. But his need of a theory of the unseen world (to put his vague hopes and terrors into our own phraseology) went far beyond what his scraps of experience could teach him. "What must I do to be saved?" was a question to which he could not find, yet would not wait for an answer. He fell into grotesque fancies, which his experience did not really support; and the divorce of Religion from Science at once began.

The spiritual need which thus acted on the savage continues to act on the civilised man. He too is impelled to build his faith on grounds outside his sphere of observation, to enlarge the safe, general, and permanent formula for religion in various more or less unsafe, specialised, and transitory ways. For it is—may one not say?—a safe, general, and permanent formula for religion if we regard it as man's normal subjective response to the sum of known cosmic phenomena taken as an intelligible whole. Under the title of Natural Religion this forms at least an element in all the higher forms of faith. Nevertheless it is felt to be inadequate; because the observable phenomena of the Universe, so far at least as they have yet been observed, have not been such as to evoke (save in some few minds) the full hope, the full devotion which our developed nature yearns to feel. To live by Natural Religion alone has been like living on turnips in the field. Most men demand their spiritual nutriment in a more assimilable form. The philosophical or the poetical contemplation of Nature has not satisfied them in the past; nor can they hope that the scientific contemplation of Nature will satisfy them any better now. They turn aside from the ambiguous pageant, the circumspect scrutiny; they specialise the name of Religion upon some clear, swift, extra-scientific knowledge as to the dealings of unseen Powers with mankind.

On such knowledge, or supposed knowledge, the peoples of East and West have stayed in many fashions their soul's desire; but, nevertheless, we all know too well that even yet there is no spiritual food attainable in the precise condition in which it will meet all healthy needs. We are all forced to feel that in the present divided and unstable condition of beliefs there is plausibility in the Agnostic's appeal to us to halt and mark time; in his insistence that we have not really evidence, up to modern standards, which can support any definite creed in matters remote from ordinary methods of proof. Some men, indeed, have ventured explicitly to reply that Christian Faith need not be founded on the same kind of demonstration as Science; that Tradition and Intuition can well supply her outward form and her inward glow. Urged among those who have much of consecrated tradition, of noble intuition in common, this high claim may seem convincing as the gaze



of a friend. But it has the inevitable weakness already indicated. Introduce other persons of different race but equally sincere, the Buddhist, the Parsee, the Jew—nay, the saint of science, like Darwin—and you can meet these men no longer on the ground of Christian Tradition or Intuition—you can meet them on the ground of Science alone. Thus even among spiritually-minded men we seem forced back into the view that Science can be the only world-philosophy or world-religion;—the only synthesis of the Universe which, however imperfect, is believed in *semper, ubique, et ab omnibus*, by all who can understand it.

5. This conclusion, however, as already implied, at present satisfies nobody. The Christian says that it is mere mockery to pretend that Science can be the base of Religion; for it tells us nothing of the spiritual world. “Naturally,” replies the Agnostic, hardening into Materialism; “since there is no spiritual world of which to tell.” “The Universe,” cried Clifford triumphantly, “is made of ether and atoms, and there is no room for ghosts.”

So soon, however, as the man of science takes this tone—so soon as he passes, so to say, from Huxley to Clifford—he loses his strong position, the Agnostic’s *raison d’être*. Clifford had not really turned over his atoms thoroughly enough to make sure that no ghost was hidden among them. As indisputably as any worshipper of Mumbo-Jumbo had that eager truth-lover framed an emotional synthesis which outran his Science.

Is, then, the passivity of pure Agnosticism the attitude with which we ought to be content? *Ignoramus et ignorabimus*—should this be the single clause of our creed? Surely that were too tame a surrender to the Sphinx and her riddle; which, in the old story, turned out after all to be rather easy to guess. Why should we not simply try to find out new facts here, as we have found out new facts everywhere else where we have looked for them? Just here we have not looked for them yet, because neither the priests of our religions nor their critics have till now been disposed for the quest. The priests have thought it safest to defend their own traditions, their own intuitions, without going afield in search of independent evidence of a spiritual world. Their assailants have kept their powder and shot for the orthodox ramparts, ignoring any isolated strongholds which formed no part of the main line of defence.

This search for new facts is precisely what our Society undertakes. Starting from various standpoints, we endeavour to carry the newer, the intellectual virtues into regions where dispassionate tranquillity has seldom yet been known. As compared with the claims of Theologians, we set before ourselves a humbler, yet a difficult task. We do not seek to shape the clauses of the great Act of Faith, but merely to prove its

preamble. *To prove the preamble of all religions*; to be able to say to theologian or to philosopher: "Thus and thus we demonstrate that a spiritual world exists—a world of independent and abiding realities, not a mere 'epiphenomenon' or transitory effect of the material world—a world of things, concrete and living, not a mere system of abstract ideas;—now, therefore, reason on that world or feel towards it as you will." This would indeed, in my view, be the weightiest service which any research could render to the deep disquiet of our time;—nay, to the *desiderium orbis catholici*, the world-old and world-wide desire.

6. First, then, we adopt the ancient belief—implied in all monotheistic religion, and conspicuously confirmed by the progress of modern science—that the world as a whole, spiritual and material together, has in some way a systematic unity; and on this we base the novel presumption that there should be a unity of method in the investigation of all fact. We hold therefore that the attitude, the habits of mind, the methods, by aid of which physical science has grown deep and wide, should be applied also to the spiritual world. We endeavour to approach the problems of that world by careful collection, scrutiny, testing, of particular facts; and we account no unexplained fact too trivial for our attention. Seeking knowledge before edification, we aim not at what we should most like to learn, but at what we have the best chance of learning; we dabble among beggarly elements; we begin at the beginning.

Into this frame of mind the long habit of our race in matters religious has made it difficult fully to enter. I have found it helpful to imagine what would be the procedure of some extraneous inquirer into the nature and fate of men—some inquirer exempt from their hopes, their fears, their presuppositions.

Let us suppose, then, that "a spectator of all time and all existence," a kind of minor Cosmotheorus, as Plato might call him, were speculating from the standpoint of this planet, as to what was likely to be the true position of the human race in the scheme of the Universe. Such an observer would be compelled to start from the facts before him. He would begin his investigation, therefore, not with God but with man. He would analyse the faculties of which he found man possessed, and would infer in what environment they were designed to operate—of what system, that is to say, of cosmic laws, expressing a special modification of the ultimate energy, the energy contained in the human race formed an integral element. His first discovery would be that the obvious material environment, which is all that most men know, does not exhaust the faculties nor cover the phenomena of human life. Most of man's senses, indeed, he could explain as concerned solely with matter. Sight he could not thus explain; and the study

of light would lead him to discover the etherial environment—a system of laws, that is to say, which, while fundamentally continuous with the laws of matter, does yet supply a new conception of the Cosmos, at once more generalised and more profound. But still the central problem of man's being would remain unsolved. Life and thought could not be referred to the working either of aggregated molecules or of etherial undulations. To explain Life by these two environments would be as impossible as it had been to explain Light by the material environment alone. Might there not be yet another environment—*metetherial*, spiritual, what you will? Was there any way of reducing this vast and vague problem of Life to manageable definiteness? Were there measurable traces of human faculty working in apparent independence of material or etherial law? Such traces, if he sought long enough, I maintain that he would assuredly find. He would find (as we have found) instances of teleesthesia, or perception beyond the sensory range; instances of telepathy, or direct communication from mind to mind;—nay, telepathic messages from the so-called dead—signs and apparitions by which minds discarnate impressed themselves upon minds still robed in flesh. How far the ether, in some of its unknown properties, may be concerned in these operations, our Cosmotheorus might be better able to guess than we. To him, perhaps, no environment would seem discontinuous with any other environment. But, at any rate, here would be definite traces of a new environment of Life and Thought; traces of the mutual action of minds, embodied and unembodied, in apparent independence of matter.

I must not here follow our imagined inquirer further; but surely we leave him launched upon a series of observations and experiments which have no inherent flaw in their basis, and no assignable limit to their scope.

7. I have dwelt at some length upon this line of argument, because I think that, in some form or other, it is our duty to have it always forthcoming, our duty to set it before the world in varying expression, until our age is really convinced that this great branch of knowledge, which deals with things unseen, can form no exception to those rules by which experience shows us that all valid knowledge has hitherto been won. So confident, indeed, do I feel in this gradual but certain method of approach—in this open, unfrequented way—that even if it had thus far failed to lead us to any discovery, I should feel bound to pursue it still. But it has not failed. This persistent analysis of unexplored faculty has revealed to us already far more than I, for one, had ever dared to hope. I may surely say with no more than the licensed exaggeration of epigram, that our method has revealed to us a hidden world within us, and that this hidden world within us has revealed to us an invisible world without.



Within each man, I say, there is a world of thought and of perception which lies outside the margin, beneath the threshold, deeper than the surface-tension of his conscious being.

“ We were the first that ever burst  
Into that silent sea.”

We at any rate were among the earliest to attempt to explore and map out that strange, submerged region—half lumber-room and half king’s-treasury—where amid things outworn lie things unborn, and possibilities of our unimaginable Future lurk among the exuviae of our immemorable Past. And yet in this confusion all is implicitly congruous and consecutive; each trace of faculty, whether it lie behind our actual stage of progress or before, belongs to a series of developments of personality whose terms have no assignable limit;—a series which carries us onward without a break, from dream and hallucination and bewilderment, up to the utterances of discarnate spirits and the visions of ecstasy.

For, in truth, from the mind’s inward vision we may learn more than from the seeing of the eye; from inward audition more than from the hearing of the ear. The *automatisms* which steal their way upward from hidden depths to manifestation amid man’s sensory perceptions and voluntary acts are found on analysis to contain elements of knowledge not attainable in any normal fashion. Such knowledge is shown in telepathic messages between living men, and in apparitions which tell of men dying, and in evidential messages from men whom we call dead. All this—in *Phantasms of the Living* and in fourteen volumes of *Proceedings*—I claim that we have adequately shown. And of late years we have advanced and consolidated these fragmentary and fugitive indications of the spirit’s survival by certain records of trance-phenomena and spirit-possession;—records as yet inchoate and imperfect, yet which must needs be faced and dealt with by all serious men.

8. But here I must needs stay a moment to prevent any misunderstanding. Throughout this address, of course, I am speaking for myself alone. I am not giving utterance to any collective view, but to my own view of the general drift and result of our collective action. But at this point I know that most even of those who may have gone with me thus far will,—and quite justifiably,—suspend their adhesion. Few even of my own colleagues have had full reason to believe that matter of real importance has yet been received from behind the veil, and in the world at large the general impression that even those messages which look evidentially as though they had come from discarnate spirits are yet practically futile and incoherent is strongly and naturally operative in checking public interest in what seems so strangely baffling a research.

I will not now protest, as I might protest, against the accuracy of this general impression of the actual facts. Accepting it for the sake of argument, I will confine myself to one simple line of *à priori* reasoning, which seems to me sufficient to show what, in the supposed case, is our plain, scientific duty. I say, then, that if once it be admitted,—as we are now assuming, for argument's sake, that it is admitted—that it is evidentially probable that some of these messages do indeed, in however indirect or confused a manner, emanate from an unseen world,—then it is a blasphemy against the faith of Science to doubt that they must ultimately prove to be of serious, of supreme importance.

The faith to which Science is sworn is a faith in the uniformity, the coherence, the intelligibility of, at any rate, the material universe. Science herself is but the practical development of this mighty postulate. And if any phenomenon on which she chances on her onward way seems arbitrary, or incoherent, or unintelligible, she does not therefore suppose that she has come upon an unravelled end in the texture of things ; but rather takes for granted that a rational answer to the new problem must somewhere exist,—an answer which will be all the more instructive because it will involve facts of which that first question must have failed to take due account.

This faith in the uniformity of material Nature formulates itself in two great dogmas—for such they are ; —the dogma of the Conservation of Matter, and the dogma of the Conservation of Energy. Of the Conservation of Matter, within earthly limits, we are fairly well assured ; but of the Conservation of Energy the proof is far less complete, simply because Energy is a conception which does not belong to the material world alone. Life is to us the most important of all forms of activity ;—of energy, I would say,—except that we cannot transform other energies into Life, nor measure in foot-pounds that directive force which has changed the face of the world. Life comes we know not whence ; it vanishes we know not whither ; it is interlocked with a moving system vaster than that we know. To grasp the whole of its manifestation we should have to follow it into an unseen world. Yet scientific faith bids us believe that there, too, there is continuity ; and that the past and the future of that force which we discern for a moment are still subject to universal Law.

Believing, then, that the whole Cosmos is such as to satisfy the claims of human Reason, we are irresistibly led to ask whether it satisfies other claims of our nature which are as imperious as Reason itself. Infinite Intelligence would see the Cosmos as infinitely intelligent ; but would infinite Goodness also see it as infinitely good ?

We know too well the standing difficulties in the way of such an assumption. They are that which we call Evil, and that which we

see as Death. Now as to Evil,—which for us here and now seems so ineffaceable a blot on the idea of Omnipotence,—we can perhaps nevertheless just conceive that for the Cosmotheorus all these defects and incompatibilities of human impulse and sensibility may seem as relatively infinitesimal in the unimaginable Sum of Things, as for us are the whirl and clashing of molecules in the dewdrop, which cannot mar for our vision its crystalline calm.

But *death*, as it presents itself to us, cannot be similarly explained away. If it be really, as it seems, a sheer truncation of moral progress, absolute alike for the individual and for the race,—then any human conception of a moral universe must simply be given up. We are shut in land-locked pools; why speak to us of an infinite sea?

What, then, should be the impulse, what the faith of Science, if she finds even the least reason to suspect that this truncation is in fact illusory; that on the moral side also there is conservation and persistence;—conservation not only of such ether-vortices as we assume to underlie our visible matter, but of the spiritual systems or syntheses which underlie the personalities of men?—persistence not only of crude transformable energies, but of those specific non-transformable energies which inform a Plato or a Newton, and which seem the only commensurate object towards which the whole process of evolution can tend? Surely in such a case, whatever dreaminess or confusion may mark the opening of intercourse with worlds indefinitely remote, Science should summon all her fundamental trust in the coherence, the intelligibility of things, to assure her that the dreaminess must pass and the confusion clear, and that the veriest rudiment of communication between world and world bears yet the promise of completing and consummating her own mighty dogmas,—of effecting a unification of the universe such as she has never ventured to hope till now? What are our petty human preconceptions worth in such a case as this? If it was absurd to refuse to listen to Kepler, because he bade the planets move in no perfect circles, but in undignified ellipses;—because he hastened and slackened from hour to hour what ought to be a heavenly body's ideal and unwavering speed;—is it not absurder still to refuse to listen to these voices from afar, because they come stammering and wandering as in a dream confusedly, instead of with a trumpet's call? because spirits that bend nigh to earth may undergo, perhaps, an earthly bewilderment, and suffer unknown limitations, and half remember us and half forget?

Nay! in the end it is not for us to choose;—we needs must join in this communion with what grace we may. We cannot, if we would, transform ourselves into the mere cynical spectators of an irrational universe. We are part and parcel of these incredible phenomena; our



own souls shall soon be feeling the same attraction, the same hesitancy, upon the further shore.

“ I am the doubter and the doubt,  
And I the song the Brahman sings.”

Let us do what we can, then, to dignify the situation. Let us try, then, whether a more serious response on our part may enable the senders of the messages to speak with clearer voice. To whose care indeed has such response been hitherto for the most part left? May not the instances where adequate precautions have been taken, adequate record made, be counted on the fingers of one hand? Might not our unseen correspondents turn the tables on us when we complain of their incapacity, and ask whether it was worth while to do better for the “domestic muffs” of Mme. Blavatsky’s far-famed *cénacle*, or for the sitters at the “materialisation séances” of the “Vampires of Onset”?

Assuredly we modern men have taken, in other quarters, more trouble than here is needed, with far less hope of reward. What has given its worth to the study of comparative religions except our steady effort to comprehend and to co-ordinate such childish and stammering utterances as have marked the rise in one nation after another of those spiritual needs and conceptions which make in the end the truest unity of the race of man? What should we have learnt from the Vedas, from the Book of the Dead—nay, from the Christian records themselves—had we approached those sacred texts in the spirit alternately of Simple Simon and of Voltaire?

9. The time, I think, is ripe for a generalisation wider than any which those ancient books contain. For just as a kind of spiritual fusion of Europe under Roman sway prepared the way for Christianity to become the European religion, so now also it seems to me that a growing conception of the unity, the solidarity, of the human race is preparing the way for a world-religion which expresses and rests upon that solidarity;—which conceives it in a fuller, more vital fashion than either Positivist or Catholic had ever dreamed. For the new conception is neither of benefactors dead and done for, inspiring us automatically from their dates in an almanac, nor of shadowy saints imagined to intercede for us at Tribunals more shadowy still;—but rather of a human unity,—close-linked beneath an unknown Sway,—wherein every man who hath been or now is makes a living element;—inalienably incorporate, and imperishably co-operant, and joint-inheritor of one infinite Hope.

Of course, I am not here supposing that any human gaze can pierce deeply into the world unseen. Such communion as we may hold with spirits in any degree comparable with ourselves must needs be on a level far beneath the lowest of “Thrones, Dominations, Princedoms,

Virtues, Powers";—nay, must be in the very vestibule and antechamber of the outermost of the courts of Heaven. These souls of ours are but *infantum animæ flentes in limine primo*;—the first and humblest conscious links in a wonderful order;—trembling still and half-bewildered at a future vaster than we know. I do not presume to forecast what we may come in time to learn; I only say that for the present hour there will be enough of motive to urge us to utmost effort to rise in the scale of being, if we can once be certain that such noble spirits as we have known by earthly intercourse or earthly record do still concern themselves with our progress, and still from their higher vantage-ground call to us that all is well.

Men objected of old to Copernicus that if our earth really swept round the sun in so vast an orbit, there should be an apparent displacement—a parallax—in the position of the fixed stars. Such parallax was long sought in vain; till at last advancing skill detected it in some few stars nearer than the rest; and our relation to these near luminaries proved to us our veritable voyage through the star-strewn deep. Perhaps in the spiritual world as well we have strained our gaze too exclusively on luminaries that are beyond the parallactic limit; and eyes turned steadily on some nearer brightness may teach us at last our kinship and community in the firmament of souls.

Not, then, with tears and lamentations should we think of the blessed dead. Rather we should rejoice with them in their enfranchisement, and know that they are still minded to keep us as sharers in their joy. It is they, not we, who are working now; they are more ready to hear than we to pray; they guide us as with a cloudy pillar, but it is kindling into steadfast fire.

Nay, it may be that our response, our devotion, is a needful element in their ascending joy; and God may have provided some better thing for us, that they without us should not be made perfect;—*ut non sine nobis consummarentur*.

10. To most of my hearers I doubt not that this forecast of a coming co-operation between incarnate and discarnate spirits will have seemed speculative and premature. My defence is that I believe that upon our own attitude towards these nascent communications their progress and development depend, so that we cannot too soon direct serious attention to the high responsibilities opening on our view. And now yet another practical question is ready, I think, for immediate discussion. All great changes in speculative belief must modify in some way man's immediate duty. In what way must our idea of duty be modified, be expanded, if a religion is offered to us which no longer depends on tradition and intuition only, but on reason also and on experiment; which is not locked away in an emotional compartment of

our being, nor adapted to the genius of special races alone, but is œcumenical as Science is œcumenical, is evolutionary as Science is evolutionary, and rests on a permanent and provable relationship of the whole spiritual to the whole material world?

No full answer to such a question can as yet be attempted or divined. But one point is clear;—and on that point it is already urgently necessary to insist. We must maintain, in old theological language, that *the intellectual virtues have now become necessary to salvation*. Curiosity, candour, care;—these are the intellectual virtues;—disinterested curiosity, unselfish candour, unremitting care. These virtues have grown up outside the ecclesiastical pale; Science, not Religion, has fostered them;—nay, Religion has held them scarcely consistent with that pious spirit which hopes to learn by humility and obedience the secrets of an unseen world. Here surely our new ideals suggest not opposition but fusion. To us as truly as to monk or anchorite the spiritual world is an intimate, an interpenetrating reality. But its very reality suggests the need of analysis, the risk of misinterpretation; the very fact that we have outgrown our sacerdotal swaddling-clothes bids us learn to walk warily among pitfalls which call for all the precautions which systematic reason can devise.

Upon a new scheme of beliefs, attractive to the popular mind as the scheme which I prefigure, a swarm of follies and credulities must inevitably perch and settle. Yet let those who mock at the weaknesses of “modern Spiritualism” ask themselves to what extent either orthodox religion or official science has been at pains to guard the popular mind against losing balance upon contact with new facts, profoundly but obscurely significant? Have the people’s religious instructors trained them to investigate for themselves? Have their scientific instructors condescended to investigate for them? Who should teach them to apply to their “inspirational speakers” any test more searching than they have been accustomed to apply to the sermons of priest or bishop? What scientific manual has told them enough of the hidden powers within them to prevent them from ascribing to spiritual agency whatever mental action their ordinary consciousness may fail to recognise as its own?

The rank and file of Spiritists have simply transferred to certain new dogmas—for most of which they at least have some comprehensible evidence—the uncritical faith which they were actually commended for bestowing on certain old dogmas,—for many of which the evidence was at least beyond their comprehension. In such a case ridicule is no remedy. The remedy lies, as I have said, in inculcating the intellectual virtues;—in teaching the mass of mankind that the maxims of the modern *savant* are at least as necessary to salvation as the maxims of the mediæval saint.



11. Now here, I take it, lies the special, the characteristic duty of the Society for Psychical Research. It is a duty far wider than the mere exposure of fraud ; far wider than the mere production of specimens of patient and intelligent investigation. Our duty is not the founding of a new sect, nor even the establishment of a new science, but is rather the expansion of Science herself until she can satisfy those questions which the human heart will rightly ask, but to which Religion alone has thus far attempted an answer. Or rather, this is the duty, the mission, of the coming century's leaders of spiritual thought. Our own more special duty is to offer through an age of transition more momentous than mankind has ever known, that help in steadying and stimulating psychical research all over the world which our collective experience should enable us richly to bestow. Such function *ought*, I say, to be ours indeed. We alone have taken the first steps to deserve it. I see our original programme completely justified. I see our *raison d'être* indisputably established. I see all things coming to pass as we foresaw. What I do *not* see, alas ! is an energy and capacity of our own, sufficient for our widening duty ;—enough of labourers for the vineyard so ripe for harvest. Speaking, if so I may, for the remnant of that small company of labourers of the first hour of the day, I must confess that our strength, at least, cannot suffice for the expanding task ;—nay, could not so suffice, even if Edmund Gurney were with us still ;—*non, si ipse meus nunc adforet Hector*. Other workers, good men and true, have joined themselves to us ;—but we have need of many more. We invite them from each department of science, from every school of thought. With equal confidence we appeal for co-operation to *savant* and to saint.

12. To the *savant* we point out that we are not trying to pick holes in the order of Nature, but rather, by the scrutiny of residual phenomena, to get nearer to the origin and operation of Nature's central mystery of Life. Men who realise that the etherial environment was discovered yesterday need not deem it impossible that a metetherial environment—yet another omnipresent system of cosmic law—should be discovered to-morrow. The only valid *à priori* presumption in the matter is the presumption that the Universe is infinite in an infinite number of ways.

To the Christian we can speak with a still more direct appeal. "You believe," I would say, "that a spiritual world exists, and that it acted on the material world two thousand years ago. Surely it is so acting still ! Nay, you believe that it *is* so acting still ; for you believe that prayer is heard and answered. To believe that prayer is heard is to believe in telepathy—in the direct influence of mind on mind. To believe that prayer is answered is to believe that unembodied spirit does actually modify (even if not storm-cloud or plague-germ) at least the

minds, and therefore the brains, of living men. From that belief the most advanced 'psychical' theories are easy corollaries. You may reply, indeed, that the Church, or the Bible, has told men all of the unseen world that they need to know, and that whatsoever is more than this cometh of evil. What say you to this argument when it is retorted on you by Omar with his Koran?"

But let us cease to speak as though the infinite Unseen World were a mere preserve or battle-ground of theologies. If every dogma ever promulgated from the Vatican were literal truth, Science would still affirm that scarcely anything of that world was known. If Religion be more than "the guess of a worm in the dust, and the shadow of its desire," it must be (I say once more) the spirit's normal answer to objective fact. The Cosmos is what it is, and Revelation can do no more than reveal it. Holiness itself must be the reflection of a reality behind the veil. If this be so, then Science has come not to destroy but to fulfil; Religion must needs evolve into Knowledge; for Religion can in no age admit an aim narrower than the prayer of Cleanthes,—the willing response of the soul to all she knows of cosmic law.

Out of the long Stone Age our race is awakening into consciousness of itself. We stand in the dawn of history. Behind us lies a vast and unrecorded waste—the mighty struggle *humanam condere gentem*. Since the times of that ignorance we have not yet gone far; a few thousand years, a few hundred thinkers, have barely started the human mind upon the great æons of its onward way. It is not yet the hour to sit down in our studies and try to eke out Tradition with Intuition—as one might be forced to do in a planet's senility, by the glimmer of a fading sun. *Daphni, quid antiquos signorum suspicis ortus?* The traditions, the intuitions of our race are themselves in their infancy; and before we abandon ourselves to brooding over them let us at least first try the upshot of a systematic search for actual facts. For what should hinder? If our inquiry lead us first through a jungle of fraud and folly, need that alarm us? As well might Columbus have yielded to the sailors' panic, when he was entangled in the Sargasso Sea. If our first clear facts about the Unseen World seem small and trivial, should that deter us from the quest? As well might Columbus have sailed home again, with America in the offing, on the ground that it was not worth while to discover a continent which manifested itself only by dead logs.

One final word to each main division of our critics;—to those first who have been disappointed so often that they refuse to listen to any further promise of news from the Unseen;—and then to those who relying on some grander revelation,—whether received from without them or from within,—disdain our slow collective process and

comminuted fragments of truth. I would remind the Agnostic that a pike was once kept in the same tank with a perch. There was at first a sheet of glass between them ; and the pike bruised his nose so often in snapping at the perch that in time he gave up that endeavour—as the Agnostic his endeavour after proof of a spiritual world—with a sigh or a sneer. Then silently the transparent screen was removed ; but now the pike was so convinced that his prey was unreachable that—like the Agnostic in presence of our new evidence—he continued simply to let the perch alone.

For those other men I will resort to a bold metonymy, and will speak of that great incurrent truth to which each man severally holds under the figure of the great stone at Ephesus which fell down from Jupiter. The faithful who proclaimed that wondrous fall were essentially in the right,—were far more in the right than the freethinkers who derided it. But whence and why that stone had truly fallen,—how vast the significance of that cosmic trajectory and rushing flame,—this could be known only when humble labourers had catalogued many a lesser congener of the mighty mass ; and had gathered the meteoric dust from the ocean's floor ; and had learnt that no field of heaven had been found so desolate as not to carry still the impress of ultimate energy and universal law.





PROCEEDINGS  
OF THE  
SOCIETY FOR PSYCHICAL RESEARCH,  
PART XXXVIII.

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PROCEEDINGS OF GENERAL MEETINGS.

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The 106th General Meeting of the Society was held at the Westminster Town Hall on Friday, June 22nd, 1900, at 4 p.m.; the President, MR. F. W. H. MYERS, in the chair.

THE PRESIDENT read parts of the paper on "Pseudo-Possession" which is printed below.

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The 107th General Meeting was held in the same place on Monday, July 16th, 1900, at 8.30 p.m.; the PRESIDENT in the chair.

THE PRESIDENT read a paper on "Trance-Utterance and other Phenomena observed with Mrs. Thompson."

## I.

## ON THE SO-CALLED DIVINING ROD.

A PSYCHO-PHYSICAL RESEARCH ON A PECULIAR FACULTY ALLEGED TO EXIST IN  
CERTAIN PERSONS LOCALLY KNOWN AS DOWSERS :

BY W. F. BARRETT, F.R.S., ETC.,

*Professor of Experimental Physics in the Royal College of Science for Ireland.*

## BOOK II.

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“In the sciences, that also is looked upon as property which has been handed down or taught at the Universities. And if any one advances anything new, . . . people resist with all their might; they act as if they neither heard nor could comprehend; they speak of the new view with contempt, as if it were not worth the trouble of even so much as an investigation, or a regard; and thus a new truth may wait a long time before it can make its way.” (*Conversations of Goethe with Eckermann and Soret*; translated by J. Oxenford; Bohn's Standard Library, 1874, pp. 47-8.)

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## PART I.

## INTRODUCTORY.

After my previous lengthy paper “On the So-called Divining Rod,” published in Part XXXII. (Vol. XIII.) of our *Proceedings*, some apology is due for a second paper on this subject; the more so as the subject itself, to most intelligent people, appears a contemptible one and unworthy of prolonged and serious scientific inquiry. It certainly is not one I should have chosen for investigation. However, having been urged to undertake it, those who may have had the patience to read the mass of evidence given in my former paper will probably have come to the same conclusion as that to which I have been led,—in spite of very different preconceived ideas, —namely, that the whole subject is one eminently worthy of careful investigation; not only as a question of folk lore and of historical interest, but also because several problems of considerable psychological and also physiological interest appear to be involved. In addition to which the main problem before us is a settlement of the disputed point whether mere shrewdness, an “eye for the ground” and experience on the part of the so-called “diviner” afford a sufficient explanation of the success which he so often achieves.

It is true that the subject at first dispirits and repels the investigator from the quantity of rubbish that overlays it and the credulity and ignorance that surround it, and which characterise so many of the enthusiastic votaries and writers on the divining rod, both in ancient and modern times. But this is equally true of many other

obscure questions which this Society was founded to investigate, and it is our business to try and discover, with the divining rod of science, the treasure that is buried beneath *any* soil. As Sir John Herschel has truly said of scientific research in general:—"He that has seen obscurities, which appeared impenetrable, suddenly dispelled, and the most barren and unpromising fields of inquiry converted into rich and inexhaustible springs of knowledge on a simple change of our point of view, or by bringing to bear on them some principle which it never occurred before to try, will be the last to acquiesce in any dispiriting prospects."<sup>1</sup>

Before entering upon the discussion of the fresh evidence I have collected, it may be convenient to those who approach this subject for the first time if I briefly state by whom, and the purpose for which, the divining rod is at present employed, and how it comes to pass that what appears at first sight to be a mere relic of a superstitious age survives amid the scientific light of the present day. Readers of my previous report will, I hope, forgive the repetition.

There are in the country districts of England a certain class of persons who profess to be able to discover, without geological or local knowledge, the exact location of any underground "spring" or subterranean course of water supply.<sup>2</sup> For this purpose they usually, but not always, employ a forked or Y-shaped twig, generally of hazel or some pliant wood. Grasping the ends of the fork in a particular manner,—to which great importance is usually attached, albeit the mode of holding has varied in late years in an amusing manner,—the holder fixes his attention upon the slightly raised point of the twig, and thus becoming as far as possible oblivious to the world around, he traverses the ground. When supposed to be approaching the hidden source the twig is seen to quiver, and when the "diviner" believes he is vertically over the "spring," so vigorous is the motion of the twig that it forcibly strikes the holder's body, and, if short enough to escape the body, rotates rapidly, though the holder appears to be doing his utmost to restrain its motion; a fact that he will emphatically corroborate, and which seems to be undeniably proved by one limb of the twig being often snapped across, under the strain of the opposing forces. The holder of the twig generally appears much exhausted by the effort; in some cases he complains of sickness or giddiness,

<sup>1</sup> *Discourse on the Study of Natural Philosophy*, p. 8.

<sup>2</sup> I put the word spring in inverted commas, as this term is invariably employed by the "water-finder." As so used, it is, however, often a very misleading expression. In order to remove misconception on the part of non-geological readers, I have given the reason why this expression is usually incorrect in an Appendix on "Geological Views of the Distribution of Underground Water," which will be found in my previous Report. See also Appendix B., p. 340, in the present Report.



occasionally the pulse rises, he breaks out into a violent perspiration, and trembles all over. As a rule (but not always) he asserts that he experiences singular sensations, resembling muscular cramp, when he is over an underground water-course. These sensations are so well marked that in some cases he discards the use of the twig and trusts to his sensations alone.<sup>1</sup> If now the place be carefully noted beneath which the hidden water-course is asserted to exist, the probability is that, however often the spot be crossed by the holder of the twig, the same phenomena recur, even when his attention is distracted, or another and independent diviner employed. Further, if a well be dug at the spot so indicated, water will usually be found at no great depth below the surface. These water-finders, or diviners, are widely and regularly employed, not by ignorant folk, but by some of the most distinguished and clever people in England, by Cabinet Ministers and Judges,<sup>2</sup> by shrewd business men and large landowners—men not likely to waste their money on a silly superstition. In parts of the south-west of England, in Somersetshire particularly, these water-finders are held in high esteem, and are known as “dowsers,” and their rod as the “dowsing rod.”

This alleged power of finding underground water, when to an ordinary observer there appears nothing to betray the presence of a spring below, is not confined to a professional and paid class of men, but is found among amateurs, young and old, male and female, and in all classes of society. Nor is the dowser only met with in England; he is found to-day in some parts of France, Italy, Switzerland, Germany, Denmark, Scandinavia, and Finland, and flourishing in the United States, Canada, Australia, and doubtless in other places of which I have no direct evidence. Many instances of the wide distribution of the dowser were given in my former paper, and others will be given in the evidence to be cited later on.

In my previous paper I gave an account of fourteen contemporary *English* dowsers, all of whom make or add to their living by this means, and, in addition, evidence was given of the practical use of the rod by upwards of thirty amateurs of both sexes and ranging over every class in society, from elderly magistrates to young children. Since then several other dowsers, both professional and amateur, have come under my notice (see p. 234). I have not, however, found any Scotch or Irish professional dowsers, though these countries regularly invite the services of English dowsers. I should say there are at the present

<sup>1</sup> For a fuller account of the sensations alleged to occur in dowsing and the evidence thereon see the discussion of this subject in my previous paper, *Proceedings S.P.R.*, Part XXXII., Appendix D.

<sup>2</sup> For example, Lord Salisbury, Lord Lansdowne, and others; see also the testimony of the Rt. Hon. Sir Edward Fry, D.C.L., F.R.S., etc., p. 226.

time in England at least a score of these men engaged in the business of water finding, and a very profitable business some of them find it to be. Curiously enough I have come across no women professional dowzers, though this is contrary to what one might expect.

The etymology of the word *dowsing rod* is uncertain;<sup>1</sup> it is first mentioned by the philosopher Locke in 1691, who speaks of the "*deusing rod* or *virgula divina*." Locke was a native of Somersetshire, born under the shadow of the Mendips, and for upwards of two centuries the Mendips have been, and still are, the radiating point and stronghold of the dowser. The term "*dowsing rod*" is preferable to "*divining rod*," as the latter, or rather its Latin equivalent *virgula divina*, was used by Cicero, Tacitus and other writers of antiquity to denote a wholly different thing, namely, divination by rods or bits of stick; and no doubt a good deal of the prejudice which the term *divining rod* creates at the present day is due to the fact that it suggests to most people some form of rhabdomancy. The forked *divining rod*, or *virgula furcata*, our modern *dowsing rod*, is not much older than the age of printing, and was first described in one of the earliest treatises on mining, Agricola's *De re Metallica*, published in 1546; for its original use was in the search for metallic ores, and, thus used, it spread from South Germany to the South of France and thence over Europe, reaching England towards the end of the 16th century.<sup>2</sup> Not until the 17th century was it used in the search for underground water, but whether for ores or water, the business-like way in which it was employed<sup>3</sup> distinguished it from the ancient *virgula divina*, the efficacy of which was supposed to depend on the ceremonies and Kabbalistic words which accompanied its use. But among the superstitious and ignorant some of the practices of rhabdomancy clung to the new rod along with the old name; hence it is that in several descriptions of the *divining rod* from the 16th century down to the present time we find special importance

<sup>1</sup> See Appendix A to my former paper. With great deference I am inclined to believe the etymology suggested on p. 261 of that Appendix, viz., from the middle English *duſchen*, to strike, is after all the most likely: the objection raised to this by Professor Skeat on the ground of the pronunciation being *dowz* is much weakened if not destroyed by the fact that it is very frequently pronounced in other ways. Moreover I find that when the rod was first brought to England at the end of the 16th or beginning of the 17th century, it was called "the striking rod," *schlag-ruthe*, by the German miners who brought it over.

<sup>2</sup> In Germany the word *wünſchel ruthe* (wishing rod) was, and is still, used as the equivalent both of the old *divining rod*, *virgula divina*, and the more modern *dowsing rod*, the *virgula furcata*; though, as mentioned in the previous foot-note, the German miners of the 16th century called the latter *schlag-ruthe*. Several German writers, long before the age of printing, speak of the *wünſchel ruthe*, and a forked *wünſchel ruthe*, it is true, is referred to by one Nithart, a German writer of the 14th century, but in these early cases it was the ancient magic wand, or *virgula divina*.

<sup>3</sup> This is well seen in pictures from Agricola's work and from Sebastian Munster's *Cosmography*, 1550, one of which we give later on, p. 343.

attached to the day of the month the diviner was born, the particular kind of twig employed, the day or hour on which it was cut, or the way it turns for underground water or metallic ores, etc.

Here, for instance, is an illustration of the survival of this superstition, and also of the use of the rod in Jutland. In Kristensen's *Jyske Folkeminder* (Popular Traditions of Jutland), published at Kolding, 1888, Vol. IX., Section 822, under the heading, *Folketro und bonde regler* (Popular Beliefs and Farming Rules), the following passage occurs:—

It has been a general custom that, when a well was to be dug, word was sent for a "water-shower" (*vand-viser*), who went over the ground with a forked (*kløftet*) willow-wand. If the twig began to twist about in his hand, there was water near the surface, and digging could be begun with confidence.

Much the same procedure was followed to discover ore. A two-year-old willow-shoot was taken, which was forked in growth. If water was to be found it must be of a different age. One who can find ore may not be able to discover water : it depends upon what month they are born in. <sup>1</sup>

Those who have had experience of an investigation of this kind, where people are afraid of being laughed at for giving their testimony, will understand the labour of obtaining accurate evidence of eye-witnesses who are willing to append their names. The correspondence entailed in this inquiry has been endless ; not far short of 6,000 letters had to be written for the purpose of my previous report. Upwards of 200 cases of water-finding by dowzers in recent years have been investigated ; in each case the independent evidence of disinterested persons who had witnessed the experiments was sought. Generally speaking such evidence was obtained, the witnesses allowing their names and addresses to be given. Altogether, 152 cases of dowsing were cited in my previous paper ; of these 140 were successful,—that is, the predictions of the dowser were verified, a well was sunk on the spot and water found at the place indicated,—and 12 were failures.

As one is far less likely to hear of the failures than of the successes of amateurs, let us confine our attention only to the professional dowser. Omitting a remarkably successful series of cases by an American dowser, which Dr. Hodgson kindly investigated, 105 cases of British professional dowzers were given in my former paper ; of these 95 were successful and 10 were failures. That would make the record

<sup>1</sup> I am indebted to Mr. W. A. Craigie of Oxford for kindly searching Kristensen's works for me, and for the translation of the foregoing passage. Kristensen is an authority on Danish folk-lore, though his books are not to be found in the British Museum. The use of the divining rod in another part of Scandinavia is referred to in my previous paper, p. 245. Professor Lochman, of the University of Christiania, recently read a paper in that city in which he said his scepticism on the subject had been overcome by his own personal experience with the rod.



of failure less than 10 per cent., which is certainly remarkably small. I have endeavoured to find the percentage of failure which skilled geologists or other scientific experts have made when they are consulted as to water supply,—for failures, and sometimes very costly failures, they have,—but it is difficult to ascertain. I doubt if their percentage of failure is less than the average dowser's; probably it varies very much with the individual employed in each case.<sup>1</sup> As might be expected, some professional dowsers, especially those who are fond of advertising themselves in newspaper paragraphs, have a poor record, and some are unadulterated humbugs. These do their best to conceal their failures, and their success is probably no greater than would have been obtained had a well been sunk at haphazard without their aid. For in many districts of England, the water-bearing area, for shallow wells, lies over a large extent of country, and in such districts one cannot go far wrong in sinking anywhere.

I may remark here that, whilst the idea of the illiterate dowser concerning the distribution of underground water is often most grotesque, yet, if I may venture to say so, it is not impossible that the geologist has a good deal to learn on this subject. Although, as Mr. T. V. Holmes, F.G.S., remarks, many dowsers think water exists here and there beneath the surface of the ground, like a pot of buried coins, instead of, as is often the case, in sheets, wherever a permeable stratum, like sand or gravel, meets an impermeable one like clay or slate; yet on the other hand the wide practical experience of those dowsers who are well-sinkers has been too much overlooked by geologists.<sup>2</sup> Underground watercourses do exist, under certain geological conditions, and it is here, where an error can easily be made, that the true test of the dowser comes in. In deep artesian wells skilled geological knowledge is always necessary, but even this sometimes fails completely. Now it must be remembered that the professional dowser is sublimely ignorant of geology and has, in general, a contempt for science,—as a rule, he is an illiterate man. It was noticed more than a century ago, both in Germany, France, and England, that “peasants who do not puzzle their minds with doubts or reasonings” (I quote from Pryce’s famous mining work of 1778) are the most successful dowsers. This is true to-day. The well-known dowser, the late J. Mullins, was a working mason and well-sinker, and his success as a dowser in the discovery of underground water was really phenomenal; he rarely was at fault, and I think we may take it he was the most remarkable dowser this century has produced.

<sup>1</sup> The geologist, however, labours under restrictions which the dowser is free from; see on this Mr. Holmes’ letter, p. 140.

<sup>2</sup> In support of this view, see Dr. A. R. Wallace’s letter on p. 374.

Two cases of John Mullins' success were critically and fully investigated in my former paper—one at Horsham, on the estate of Mr., now Sir, Henry Harben, and the other at Waterford, for the large factory of Messrs. Richardson. In both of these, as might be expected, the best geological and engineering advice had been obtained prior to calling in the dowser. It was only when the resources of science were exhausted,—*over £1,000 having been spent in each case* in fruitless sinkings and borings in different places on the estate,—that Mullins was at last sent for, in 1889 to Waterford, and in 1893 to Horsham. He was an entire stranger to the neighbourhood in both cases, and was received, not unnaturally, with a good deal of suspicion. At Horsham he quickly pointed out two places where an abundant supply of water would be found within a moderate depth, and his predictions were verified to the letter. I have visited the spot and obtained the evidence of eye-witnesses, including Sir H. Harben, and at my request the place has been visited by competent geologists. The case was ably discussed from a geological point of view by Mr. T. V. Holmes, F.G.S., past President of the London Geologists' Association, and Mr. E. Westlake, F.G.S., in my previous Report, pp. 221–230 and 278–280.

The case of Waterford is still more remarkable ; I will not repeat the details, which will bear careful study, as it is, I think, one of the most remarkable cases in the whole voluminous literature of the rod. Suffice it to say, that the rock there is of a hard slaty nature called Ordovician, it is hidden beneath a surface bed of some 40 ft. of boulder clay and in the search for water various wells and borings had been fruitlessly made, one boring being over 1,000 ft. deep. Not far from this deep and useless boring, made under the best scientific advice, Mullins found the twig turn vigorously ; he traced what he said was a line of copious water supply—either a line of jointing or a “fault” in the hidden rock—and fixed on one point as being the best to sink the well, asserting water to the extent of 1,500 gallons an hour would be found at a depth of some 80 or 90 ft. I need not say that no one believed him, but in despair a boring was made at this spot, when suddenly, at the depth of 81 ft., water burst up the bore tube and rose half way to the surface. After pumping day and night, the supply could not be run dry ; it was measured and the yield was found to be 2,000 gallons per hour, which has been maintained with but slight fluctuations from 1889, when the experiment was made, to the present time, a period of eleven years. These statements have the advantage of being corroborated by eye-witnesses and by letters written at the time, not by gullible, ignorant people, but by educated men, including a well-known local geologist, Mr. Budd, who held as it were a watching brief against Mullins ; and by Mr. Kinahan, a

distinguished geologist of H.M. Irish Geological Survey, as well as by Mr. Richardson himself. In fact Mr. Kinahan (who, as one of the best Irish geological authorities, was consulted by Mr. Richardson), writes to me: "As far as the actual results went I failed, and the diviner 'wiped my eye.'" No surface observation nor experience, nor "an eye for the ground" can explain this Waterford case, nor some others I shall refer to in the sequel.

On the other hand, in the opinion of competent geologists who have kindly given me their aid in this inquiry, several cases where the dowser has had a striking success, and which at first sight appear to suggest some power transcending any recognised faculty, were found, upon geological observation on the spot, to be capable of explanation by the rapid detection of surface indications of underground water by the dowser. This will be seen from the useful geological notes Mr. T. V. Holmes, F.G.S., has kindly appended to some of the cases in the previous and also in the present Report. A fuller investigation of several cases was, at my request, undertaken by Mr. E. Westlake, F.G.S., who has made a special study of hydro-geology, and whose valuable co-operation throughout this inquiry I have already acknowledged. Mr. Westlake selected a group of cases, given in the previous Report, which *appeared* to be evidential of some special faculty possessed by the dowser, and visited each of the places; the result of his investigations is given in the able paper which forms an Appendix to the present Report, see p. 315. It will be observed that Mr. Westlake is of opinion that many of these cases can be explained without calling in the aid of any novel faculty or instinct on the part of the dowser—merely a sharp eye for the ground was required. At the same time we must remember it is much easier to prophesy *after* the event than before it, and it remains to be seen whether our geological friends would have thought the location of the site in these cases quite as obvious *before* the well had been sunk, or the dowser had visited the spot, as it seemed to be afterwards. However that may be, I gratefully accept the opinion of geological experts, and will here quote a letter which expresses Mr. Holmes' views on the subject.

28, Crooms Hill, Greenwich Park, London, S.E.,

*December 18th, 1899.*

I may add that I am attracted, not repelled, by the supernormal, for our senses must give us extremely limited perceptions of things as compared with the perceptions possible to more highly gifted beings. Hence, among ourselves, there is no improbability in the existence of powers of special perception in individual cases, even though the total range of perception may not be supernormal. Thus if the line A.B. represents the normal range of a sense, there may be abnormally gifted persons whose range is not



greater, but is more extensive at one end of the scale, if correspondingly less so at the other, like the line C.D.

A. ————— B.

C. ————— D.

However, it has always struck me that while stories of apparitions, &c., are treated with a curious ultra-scepticism in many cases, the dowser's claims to supernormal powers have been admitted with the most extraordinary alacrity.<sup>1</sup>

As the position of our *old* towns and villages testifies, the men who knew where water might be attained at a depth of from 5 to 50 ft. have always been with us. Geologists generally do not trouble themselves with water, as regards supply for dwellings, though anxious to know whether the superficial formations are deep sea, shallow sea, estuarine river, glacial, etc. And the cholera in the first half of the century so discredited water from superficial deposits that supplies from those sources have not been trusted, even when sufficiently good. The dowser's supply is voted excellent, and he is triumphant where a hydro-geologist would feel that he would be deservedly blamed for recommending it, should it become polluted and cause disease.

All I feel is that an eye for ground can do very much and can be immensely developed by experience. The dowser needs no geological knowledge beyond what he might have had a century ago. The most remarkable successes of dowsers are those in which sensitiveness to the proximity of water cannot have helped them, while their views as to its distribution seem fatal to the hypothesis that they are influenced by special sensitiveness. Somehow, the more one looks into cases, the more the supernormal recedes.

I quite agree with you that the twisting of the twig—though a most interesting phenomenon—has no [direct nor invariable] connection with the nearness of underground water.

T. V. HOLMES.

The dowser asserts that he does not concern himself with the nature of the ground, but is guided solely by the twisting of his forked twig or by some peculiar sensation he experiences. In this case one would have expected to find some *blind* dowsers, as the blind are peculiarly sensitive in some directions, but—though I know the extensive literature of the divining rod pretty thoroughly—I have not been able to find a single blind dowser in any country in the world during the four centuries the dowsing rod has been employed. It may be urged that blind people could not engage in the peregrinations required of a dowser, nor properly hold the forked twig if they had a guide. This may be so, but I do not think it is the true answer, for I can find no account of the sudden and involuntary twisting of the rod in the hands of any blind person under any

<sup>1</sup> By *some* persons, certainly not by geologists, nor by the Council of the S.P.R. nor by myself.—W.F.B.

conditions: blind people may not have been tried, and I intended, had time permitted, to have tested the inmates of a blind asylum. The true explanation, I believe, will be found in the fact that the rod only moves in the hands of a novice when he has *seen* it "work" in the hands of somebody else; it would no doubt move with many blind people if they had previously *felt* the hands and the twig of a dowser when the latter is twisting. In any case the absurdity of imagining there is any direct connection between underground water and the motion of the so-called divining rod is seen from the fact that the first use of the rod was for finding underground ores, then for finding buried treasure, then for tracing lost cattle or boundaries, then for a multitude of uses in the moral world (until its employment for this latter purpose was forbidden by the Inquisition in 1701), and at the present day dowsers profess to discover, not only underground water and mineral lodes, but hidden coins, water or gas pipes, etc., by the twisting of their rod. Obviously, therefore, the peculiar involuntary motion of the rod and what causes this motion is one thing, and the existence of an alleged dowsing, or water-finding, faculty is quite another.

#### OBJECT OF OUR INVESTIGATION.

This brings us to an important point in our investigation. We must know clearly *what* we are seeking for. Is it for the cause of the motion of the dowsing rod, or for a proof or disproof of an alleged "dowsing faculty," or for a particular explanation of that faculty, assuming it to exist? Now it will be found that nearly every scientific investigator of the divining rod during the last 100 years has set out with some theory of the action of the dowsing rod, which, when he has satisfactorily demolished, he has asserted the whole thing was a fraud. Some of the most famous names in French scientific history a century ago, when they showed that *electricity* afforded no explanation of the movement of the divining rod, asserted they had disproved the existence of the alleged faculty itself. Had they been versed in the history of the subject, they would have found that a century prior to their day equally eminent French *savants* asserted the same thing, because they had demonstrated the *devil* was not the cause of the twisting of the forked twig. And now, in our own day, no less eminent French, American, and English men of science assert,—electricity and the devil having been laid,—that *involuntary muscular action* on the part of the dowser finally disposes of the divining rod and the mystery of an alleged dowsing faculty. This, of course, is equally erroneous. Assuming this explanation to be established, all that it explains is the twisting of the rod. Here is an instance of this fallacy.

Mr Emerson, to whom I have already referred<sup>1</sup>, published in the *American Journal of Science* (*Silliman's Journal*) for 1821 a series of remarkable cases, showing how underground water had been found by dowsers where it had previously been sought for in vain. Five years later he was led to an ingenious theory of the motion of the rod as due to involuntary muscular action, an explanation we may readily admit: then after blindfolding a young boy, in whose hands the forked twig moved, albeit he had never been tested as a dowser, Mr. Emerson draws the following sweeping conclusions from the failure of some fallacious experiments which we have given later on, p. 212:—

The pretensions of diviners are worthless. The art of finding fountains and minerals with a succulent twig is a cheat upon those who practise it, an offence to reason and to common sense, an art abhorrent to the laws of nature, and deserving of universal reprobation.

I venture to say that the majority of scientific men who have read Emerson's papers will imagine he has proved his assertions, and that the question had been finally settled by him, just as in Europe it was considered that Chevreul in 1854 had settled the question by a similar explanation, forgetting that the astronomer Lalande had also given a similar explanation in 1782. What a hardy perennial this superficial and easy dismissal of the whole subject is may be seen in a recent letter in the scientific journal *Nature* (January 6th, 1898), which I have quoted elsewhere, p. 288. Here, once more, an American professor tells us "the whole secret of the divining rod" is to be found in the involuntary muscular action of the operator.

And so also with regard to blindfolding. If well-conducted experiments, with a good dowser, show that blindfolding prevents the dowser from fixing on the same spots that he did when he could see,—places where he alleged underground water to exist,—all we have proved is the extreme improbability of any direct influence of the water on the dowser, or any clairvoyant perception on his part. Failure when blindfolded does not prove the dowser to be a charlatan, but merely that a particular physical or psychical explanation is improbable. For the success of the dowser under normal conditions may still be due to some instinct or faculty, derived from a rapid but sub-conscious observation and interpretation of the surface indications of underground water or mineral lodes, or his success may be due merely to experience and shrewd observation, which, from habitude, has almost ceased to excite a conscious effort. As eyesight would be necessary in either of these cases, the dowsing faculty, if there be such, would in this event take its place along with other familiar illustrations of keenness of perception in men and animals.

<sup>1</sup> See previous Report, p. 44. Not Ralph Waldo Emerson, but a contemporary and namesake.



Let us therefore clearly recognise in our experiments what is the aim we have in view. A particular theory may be proved to be wrong, but the important point is to obtain *trustworthy* evidence of all the facts in each case, and when a sufficiently wide range of evidence is before us, to endeavour to arrive at some explanation. It is for this reason that I have been compelled to burden this and the previous Report with such an accumulation of cases, for the subject is full of difficulties from a scientific point of view, and any explanation we may give must be based upon an extensive survey, and is likely to be either a tentative or a complex one.

We may summarise the objects of our investigation as follows :—

I. Whether the alleged claim of the dowser to a special faculty, or even facility, in the discovery of underground water or mineral lodes has any basis in fact, and if so :—

(i.) Whether this is due merely to knowledge acquired by the dowser from experience and careful observation of the ground ; or

(ii.) Whether such information is derived by some instinctive and sub-conscious process of observation on the part of the dowser, or perhaps hyperæsthetic discernment of surface signs too faint to be perceived by the ordinary observer ; or

(iii.) Whether there is evidence of any supernormal perception by the dowser—that is, information derived subconsciously otherwise than through the ordinary channels of sense.

II. The evidence for and the explanation of : (i.) the sudden and often uncontrollable movement of the rod in the hands of the dowser ; (ii.) the apparent transmission of this power of motor-automatism from a sensitive to an insensitive person ; and (iii.) the singular *malaise* and convulsive spasm which is associated in so many dowsers with the involuntary motion of the rod.

III. The History and Bibliography of the subject. These have been prepared, but the matter is so voluminous that it must be published later on.

I propose to begin with No. 1—further tests as to the alleged claims of the dowser.

## PART II.

## EXPERIMENTS BY THE AUTHOR.

§ 1. *The Carrigoona Experiments.*

It has been suggested<sup>1</sup> that the most satisfactory test of the alleged claims of the dowser would be to carry out some experiments under my own personal supervision, boring or sinking wells in places where the dowser indicated water would or would not be found. As I have said elsewhere, this can only be a very restricted test, owing to the expense involved, and, unless care be taken, may be as inconclusive as were Professor Sollas's two test borings described in the previous Report, pp. 220 and 221. However, I determined to make the attempt, and have done so this spring and summer, with the results I will now give. In order that an impartial judgment may be arrived at by my readers a somewhat detailed account is, I fear, unavoidable.

Certain precautions were necessary if any satisfactory evidence one way or the other was to be obtained. (1) The place chosen must be one entirely unfamiliar to the dowser, and no opportunity must be allowed him of knowing beforehand where the experiment is to be made, otherwise the dowser might make a prior careful examination of a geological map of the district (assuming he had access to such maps and could understand them—a possible assumption); such examination would doubtless yield a good deal of valuable information to a clever man. (2) The district selected must be one geologically suitable, that is to say, not a large water-bearing or waterless area, the character of which he might discern at a glance, and where he could not go wrong anywhere in predicting water or no water. (3) The dowser should be taken direct to the place and not allowed to get any information from persons living in the neighbourhood as to the position of any existing springs or wells. (4) The persons who accompany the dowser should also be ignorant of the likeliest spots where water would or would not be found, lest they perchance consciously or unconsciously convey their knowledge to the dowser.

These conditions were all successfully met, the place I selected—a mountainous region in the co. Wicklow, four miles from Bray—admirably fulfilling the geological conditions required, and a region I believe no dowser had ever visited. I wrote to two or three of the best known English professional dowsers, asking them if they could come over, and as Mr. Stone replied he was shortly coming to Ireland, I arranged to see him. On his arrival he had no idea where the

<sup>1</sup> In a review of my previous Report in *Nature*, October 14th, 1897.

experiment was to be made, nor did I tell him, but took him straightway to the place. My friend Mr. B. St. G. Lefroy accompanied us to take notes as an independent careful observer, and the memorandum he has written will be given directly. It was Easter week, 1899, and the day was fine. Mr. Stone told me he had never been in co. Wicklow before, and I have no doubt that is quite true. Leaving our car in the Rocky Valley, we ascended the mountain road leading round a hill called Carrigoona, on the lower slopes of which there are patches of cultivated ground, but the quartzite rock of which the hill is composed juts out here and there, though the hillside is generally covered with two or three feet of alluvial soil, on which gorse and heather and bracken grow, clothing the mountain with a garment of beauty. The panorama from the mountain-side is also one of unsurpassed beauty, and stirred the emotions of my geological friends who afterwards visited the spot with me.

For the accompanying geological map of the district I am indebted to the kindness of H. M. Geological Survey. The field we



Scale: Six inches to the mile. Quartzite marked thus, x x x.

FIG. 1.

first went to is on the eastern side of Carrigoona and is marked E on the map. The upper part of the map is due north. A general





FIG. 2.  
LOOKING WEST.

view of the surrounding country is shown in the picture opposite, Fig. 2; this is taken from a rocky eminence on the south-eastern corner of the field E and is therefore looking nearly west: Carrigoona mountain is seen on the right hand side of the picture; only the summit of the quartzite knoll in the middle distance is visible when in the field, where the boring operations are seen going on as described subsequently.<sup>1</sup>

#### *Experiment I.*

Having secured the necessary permission, I took Mr. Stone first to the field, E, Fig. 1, rather over an acre in extent, and asked him to begin operations. He pulled out a slender forked twig from his pocket, and immediately walked round the field; at one spot the twig forcibly revolved, and he said we should find plenty of water there, less than 15 ft. deep. The spot was marked, and after traversing the field two or three times he asserted that water was flowing from north to south along a line or region he traced out, but that at the side of the field very little, if any, water would be found.<sup>2</sup> "Bore anywhere along this line," he said (see dotted line in Fig. 3), "and you will get plenty of water, but very little or none over there," *i.e.*, on the east side of the field. The places were marked and subsequently fixed by measurement, so that the marks could be removed. All round the field was a rough stone wall, overtopped to a height of 8 ft. or 9 ft. by a thick gorse hedge, so that it was impossible to see a plateau of rock that flanked the east and west sides of the field beyond the boundary wall; I mention this for a reason that will appear presently. The field was on the mountain side, it sloped downwards from north to south, was uniformly covered with grass, and had no trace of water anywhere on the surface.

#### *Experiment II.*

From this field we crossed the mountain and went to an enclosed piece of the commons on the western side of Carrigoona, marked W on the map, Fig. 1. Here, again, I asked Mr. Stone to select two test places. He walked round the field, but there was no motion of his forked twig. It so happened a friend had purchased this field in order to erect a summer cottage, and particularly wanted to find a spring. Noticing a patch of rank and very green grass I asked him to try that spot. "No good," he said; "it's no good boring anywhere in this field; there is little or no water." Pressed to try once more, he fixed on one spot as best to bore, but said only a little water, "of no use,"

<sup>1</sup> I have to thank Mr. Kendrew for the photograph of which Fig. 2 is a reproduction; unfortunately the reproduction does not do justice to the original photograph.

<sup>2</sup> The three dots on the field E, Fig. 1, indicate these places, and where the bore-holes were subsequently made. The dotted line across the map is supposed to indicate a line of "fault" in the rocks: but this is only a geological surmise.

would be found: this place was marked. It is the more northerly of the two dots marked in W, Fig. 1.

### Experiment III.

Three weeks later a country gentleman, Mr. J. H. Jones, of Mullinabro House, Waterford—who some years ago accidentally discovered that the rod moved in his hands, and had had some success in his own neighbourhood as an amateur dowser—kindly agreed, at

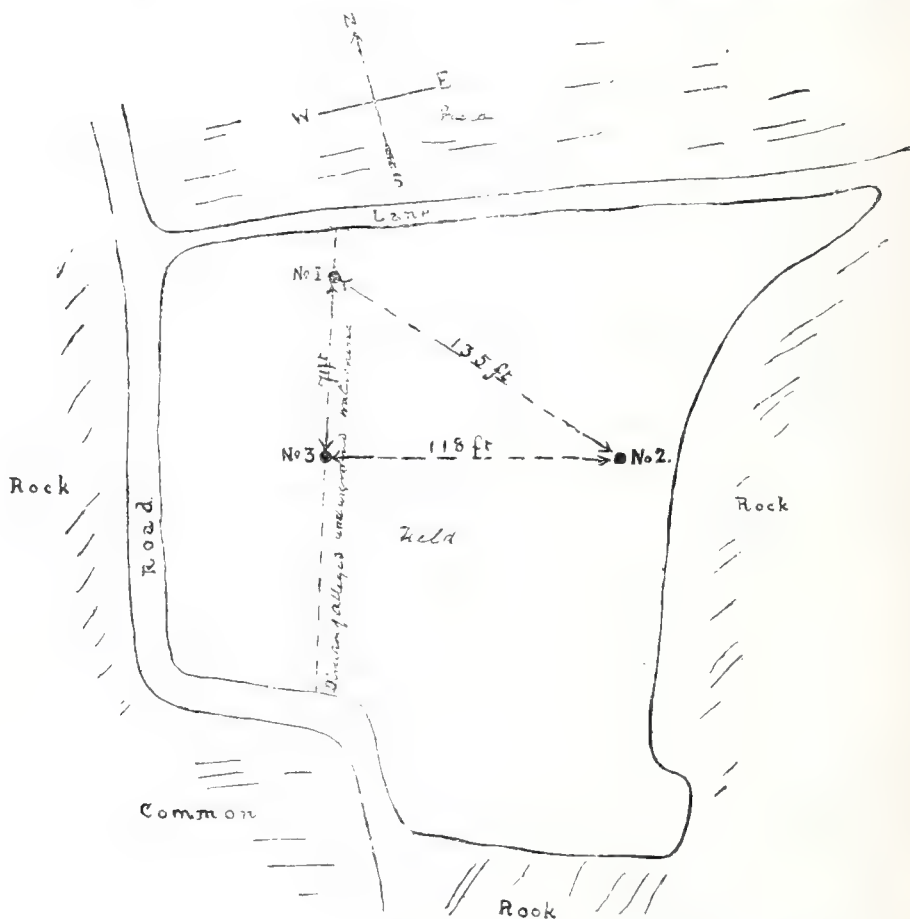


FIG. 3.

Scale 1 inch to 50 feet.

PLAN OF THE FIELD.

my request, to try the same fields. Mr. Jones had never been in that district before, and knew nothing of the places marked by Mr. Stone. These marks had been carefully removed prior to Mr. Jones' visit.

As before, Mr. Lefroy accompanied us, and we agreed to let Mr. Jones go round the fields alone, so that no hint could be derived from any involuntary indications on our part. Mr. Jones used a small, slender forked twig, which he held in the same way as the elder Mullins. We



went, first, to the larger field on the east of Carrigoona. On completing his perambulation of the field we examined the places marked by Mr. Jones. The spot where the twig moved most vigorously with him, and where he was confident we should find water, was not a foot distant from the place selected by Mr. Stone, as we found subsequently by measurement with the tape. This was at No. 1, in Fig. 3. Mr. Jones also asserted the watercourse ran from north to south in the field, in the same direction as marked out by Mr. Stone; but from the movement of the twig he asserted we should also find water a few feet on the east side of the place marked No. 1, but flowing towards Stone's line. We then crossed the mountain to the smaller field on the western side; after Mr. Jones had traversed it with his "rod" he said much as Mr. Stone did, that very little, if any, water would be found there.

#### *Borings.*

It was not until July, 1899, that the boring apparatus arrived. Mr. Stone had kindly placed it at my disposal, so that the only expense incurred was for labour and the services of his brother, Mr. E. Stone, who was a skilful working engineer. A four-inch bore-hole was made in each case; a hardened steel "jumper" and the usual boring tools being employed. Fig. 4, p. 146, shows the boring apparatus at work. Mr. E. Stone is standing on the left of the picture, and the men are boring with the jumper. The thick gorse hedge is seen round the field. From conversation with the farmer who had tilled the eastern field for many years, we expected to reach the bed rock some 6 ft. below the surface, and I anticipated a laborious and costly boring if we were to get anything better than surface water. As I had taken a farmhouse for the summer on the northern slope of Carrigoona, I was able daily to inspect the result of the boring operations. We commenced at the spot marked No. 1 in Fig. 3, and a foot below the surface struck a very hard dry clay; this went on for a depth of 8 ft., with slow progress, until suddenly, after two days' work, a bed of sand was encountered, through which the "jumper" sank, and water rushed up the bore-hole to within 4 ft. of the surface. After 4 ft. of sand a bed of gravel was reached, but the quantity of water was so great that the sides of the lower part of the bore-hole were constantly washed in, and we could not sink deeper until a lining tube was obtained. As this could only be procured in London, we proceeded to test the east side of the field, where Mr. Stone said little or no water would be got. Here No. 2 bore-hole was sunk, and nothing but a hard clay, with stones intermixed in one part, was met with till the rock was reached at a depth of 12 ft., when a little water was found between the rock and the impermeable stratum above it. I made them continue boring to a

depth of 1 ft. 6 in. into the solid quartzite rock, but no more water was obtained. We then began No. 3 bore-hole, on what Mr. Stone called the "water-line." The boring was similar in its results to No. 1, the water-bearing permeable stratum being struck 6 ft. below the surface,



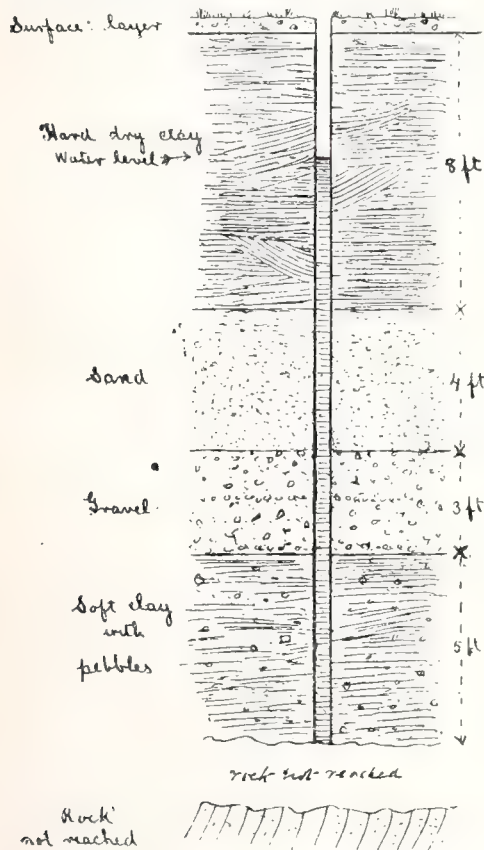
**FIG. 4.**

IN THE FIELD, LOOKING NORTH.

and abundant water rose in the bore-hole. The boring was continued when a bed of soft plastic clay was reached, and after this a hard clay mixed with pebbles. I was astonished at not reaching the rocky bed, and determined to continue the boring ; after we had gone to a depth

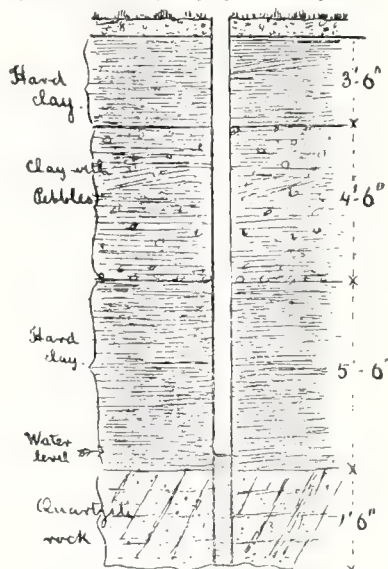
## No. 1 BORE-HOLE, 20 ft. deep.

Abundant Supply, rose to 4 ft. from Surface.



## No. 2 BORE-HOLE, 15 ft. deep.

Very little Water, 12 ft. from Surface.



## No. 3 BORE-HOLE, 22 ft. deep.

Abundant Water, rose to 5 ft. from Surface.

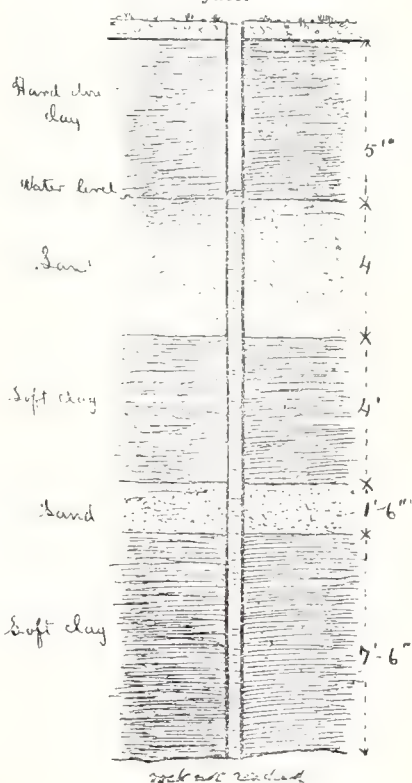


FIG. 5.

of 22 ft. we were still in the clay, and were so impeded by the constant washing in of the sandy stratum that we had to abandon the boring, the lining tubes not having arrived.

Fig. 5 shows sections of the different borings. The surface of bore No. 1 is 6 in. above the level of bore No. 2, and 3 ft. above the surface of bore No. 3, as will be seen subsequently in the cross section of the field, Figs. 8 and 9.

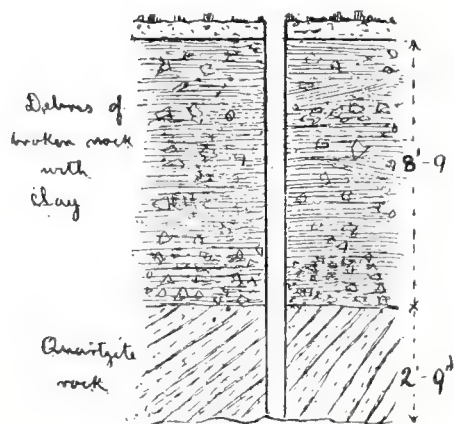
So far, then, Mr. Stone's prognostication had proved perfectly correct, and we now moved the boring apparatus to the field on the western side. Here I made them sink a bore-



hole at a spot that seemed to me most likely to yield water, where the ground appeared moister, and was covered by a patch of rank grass. The boring, No. 4, in Fig. 6, proved difficult and very tedious. Instead of clay a rocky *débris* mixed with clay was encountered below the surface-soil; at 9 ft. deep the bedrock was reached, and, thinking we might strike a fissure it was bored to a depth of nearly 3 ft., but not a drop of water was obtained. Then I had a bore-hole sunk at the spot fixed on by Mr. Stone as likely to yield a little water, but "no use," as he remarked, (this is No. 5 bore-hole); here, after the shingle, we

NO. 4 BORE-HOLE, 11 ft. 6 in. deep.

No Water.



NO. 5 BORE-HOLE, 16 ft. deep.

No Water.

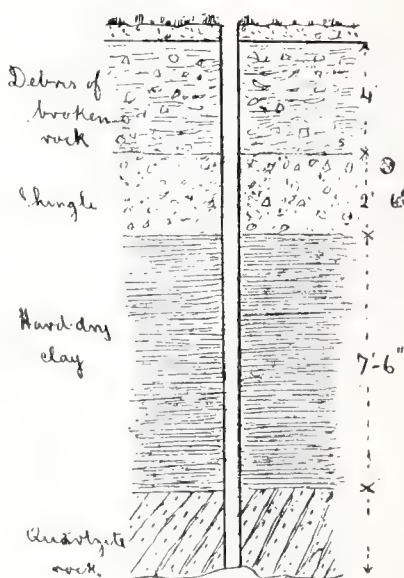


FIG. 6.

reached a hard dry clay, and continued boring through this until the rocky bed was encountered at a depth of 14 ft. from the surface, but no trace of water was found. This field slopes sharply down from east to west, and No. 5 bore-hole is at some 10 ft. lower level than No. 4, which is on the upper eastern side of the field. Finally, I had a small trial bore made at the lowest point in the field adjoining the mountain road, but only dry shingle, and no sign of water, was met with. Here, again, Stone's predictions were verified; there might have been a little water between the impermeable clay and the rocky bed when he made his trial at Easter, as it was then in the early spring, after rains.<sup>1</sup> The borings were all made in the long drought we had during the summer

<sup>1</sup> Subsequently, after the summer drought, Mr. Lefroy noticed a little water at the bottom of this bore-hole, see P.S. to his memorandum, p. 150.

of the present year; it was, therefore, all the more astonishing to find such a good supply of water in the No. 1 and No. 3 borings in the eastern field.

After the perforated lining tube had arrived we were able to go on boring in No. 1 hole, but found a second bed of clay below the gravel, and reached no rock, though we went down 20 ft. All the water was therefore supplied by the permeable layer between the two impermeable clays. In order to test the quantity, I procured a pump and 15 ft. of iron tubing, and found that No. 1 bore-hole yielded five gallons a minute, which, if the supply held, would be equivalent to over 7,000 gallons in the 24 hours. After pumping 10 minutes, however, the pump choked, and the water was then found to stand 8 ft. from the surface; it had been lowered about 4 ft. in the four-inch bore-hole. The water was flowing in all the time through the perforations in the lining tube, but not sufficiently rapidly to maintain a constant level, as the pump was a powerful one. The temperature of the water was some 20 deg. F. below that of the air. No. 2 bore-hole contained so little water that we were able, in a few minutes, almost to empty the bore-hole by ladling out the water with a small tin can. The pump had to be sent back to Dublin for repair, and another trial was postponed till early in November, as my geological friends thought a more crucial test would be afforded at that period of the year.

I will now give the account of Stone's visit, which Mr. Lefroy kindly sent to me.

*August, 1899.*

On April 2nd, 1899, Professor Barrett invited me to accompany him and Mr. Stone, an English "dowser," to a field in the Rocky Valley, co. Wicklow. We drove directly from Bray station to the field, which is enclosed by a high stone fence, partly covered by a gorse hedge. The configuration of the immediately adjacent land cannot be seen from the field, owing to its situation and the height of this fence and hedge. The area is between one and two acres in extent. Mr. Stone had no opportunity afforded him of examining the surroundings of the field except upon the side of approach, nor did he, I fully believe, receive any information on that subject, to which he made no reference in my hearing, and was apparently indifferent. I watched him closely, but saw no sign of attention on his part to surface indications, if any existed.

He traversed the field in various directions, holding a fork of the divining rod between the forefinger and thumb of each hand. His manner was that of easy confidence, and he readily maintained a conversation at the same time. At certain points the rod in his hands was violently twisted. One of these points, marked "No. 1" on the plan, Fig. 3, he stated to be that most suitable for boring purposes and he declared that a sufficient supply of water for domestic use would be found there at a very moderate depth. To the best of my recollection he said not more than 15 ft., but of this I am not now certain. The other points at which the rod was notably contorted

were in a line south from this, which he attributed to the existence of an underground watercourse. The direction of this is shown on the plan by the dotted line.

The positions of these points Professor Barrett and I measured at once and noted on a rough plan. The line of the asserted watercourse was marked by taking the distances from the side of the field. Except on that line, only very slight movements of the rod were anywhere observable, and an area in the eastern portion of the field was designated by Mr. Stone as practically waterless. The test boring subsequently made in this area is marked "No. 2" on the plan.

On April 22nd, I again visited the place with Professor Barrett and Mr. Jones, of Waterford, an amateur "dowser." The same conditions were repeated. He saw no more of the surrounding land than was strictly unavoidable and appeared to pay no attention to that or to any indications other than those furnished by the "rod." No marks had been left by which the places selected by Mr. Stone as yielding water could have been identified or guessed at. Mr. Jones walked over the field as Mr. Stone had done, the "rod" behaved similarly in his hands, and his results were practically the same. The difference of a foot or so in the "best" point selected may be reasonably attributed to his crossing the line of the watercourse referred to at a different point.

From this field, which is on the eastern side of Carrigoona Hill, we went, on the day first mentioned, to the western side, and Mr. Stone was asked to find a spring in a small field on that side. He walked over this field in the same manner with the rod, and said that no water, or so little as to be useless, would be found in it,—that there were traces of water at a certain spot, but that it would be "no good boring." Mr. Jones also went with us to this other field and he confirmed what Mr. Stone had said of there being little water to be found there.

B. ST. G. LEFROY.

P.S.—Since the borings have been made, I have measured and verified their positions with Professor Barrett, and on November 5th, 1899, I again visited Carrigoona, and made a careful examination of the borings. In the field on the eastern side, the one first tried by Mr. Stone, I found the water in boring marked No. 1 in the plan [see Fig. 3] standing 6 ft. from the ground level. The water in boring No. 3, on the line of the watercourse alleged by Mr. Stone to exist, stood 6 ft. 3 in. from the surface of the ground. The boring No. 2 was dry. In the field on the western slope of the hill I found the upper boring [No. 4] quite dry, and the lower boring [No. 5] had four or five inches of water in it [Fig. 6]. These results agree with the predictions made by Mr. Stone, and also by Mr. Jones, before the borings were made.

B. ST. G. L.

Although the excellent maps of the Irish Geological Survey had given me a general knowledge of the suitability of the locality chosen for the experiments before they were made, it seemed desirable that the opinion of some recognised geological authorities should be obtained. For this purpose I asked some of my geological friends, who were



intimately acquainted with the geology of that district, to visit the fields where the experiments were conducted, and I gratefully acknowledge their kindness in doing so and furnishing me with the accompanying valuable reports. It would have been fairer to the dowser had I asked the geologists to come *before* the borings were made, as the depth (which I informed them) to which borings Nos. 1 and 3 were made, and yet no rock reached, revealed the unsuspected fact that the field was on the site of an ancient and deep V-shaped depression or narrow valley between the rocks, probably an ancient river bed now filled with drift. The first report is from my friend and colleague Professor Grenville Cole, F.G.S., the professor of geology in the Royal College of Science, Dublin.

*August 20th, 1899.*

(i) The Rocky Valley is cut by denudation through the ancient series of shales and sandstones, altered to slates and quartzites, which occur in the Sugar Loaf area,<sup>1</sup> at Bray Head, and at Howth. Unless a fissure were fortunately struck, it would be very difficult to find water in this series. It is not a water-bearing series in itself, as is well known to residents at Howth, who are in the habit of collecting rain and surface-water.

But in the Rocky Valley area, considerable deposits of glacial drift occur, varying from clays to permeable sands, filling all the ancient hollows carved in the slates and quartzites. A trained eye readily picks out, by contour and the green or cultivated patches, the position of the sands and gravels where they abut on the older series.

The heights are formed of slate and quartzite, on which heather and some thin bogs accumulate.<sup>2</sup> The rainfall on these is considerable, and soaks off down into the old waterways, carved before glacial times, in the rocky bed. As these hollows are full of drift, the drift, where sand and gravel prevail, absorbs the water. The supply at any point, by tapping the drift, must depend greatly on the extent of the impervious gathering-ground round about, and would, in any case, probably diminish in autumn after a dry summer.

(ii) The field selected for boring in the Rocky Valley shows a marked green surface, with the hard quartzite sloping steeply down close at hand on either side. It falls towards the valley, and so, evidently, does the old rocky bed under the infilling of drift. Water would accumulate in the infilling, and would probably flow slowly down along the middle line of the old hollow. Its constancy may be doubted, but can be tested satisfactorily after the present dry season (August, 1899). A trained eye would certainly select the central line of the field, along the ancient stream-hollow, for boring.

GRENVILLE A. J. COLE.

<sup>1</sup> The "Sugar Loaf" is the miserable modern name given to one of the most beautiful and impressive mountains in the co. Wicklow, a mountain rising to nearly 1700 ft. high, and flanking one side of the Rocky Valley as Carrigoona does the other.—W. F. B.

<sup>2</sup> This refers to the Sugar Loaf, as there are no bogs on Carrigoona.—W. F. B.

In a subsequent letter Professor Cole says :—

I think a professional engineer would undoubtedly bore two or three holes along the line under which the two rock slopes at either side meet ; this line might or might not be along the lowest line of the hummocky surface of the field above, where the borings were made. I am clearly of opinion that a casual visitor who knew *anything* about general conditions of water supply would proceed a good distance from the obvious rock on either side before he predicted a fair water supply. But I also feel with you that the rapid survey and determination in two minutes or so [by the dowser] show either exceptional powers of observation or confidence in some power not possessed by ordinary engineers.



FIG. 7.  
LOOKING EAST.

With all deference to my friend Professor Cole, I am quite sure that the rocky contour immediately on each side of the field was *not* observed by Mr. Stone, nor could it be seen from the side that we approached. It requires a trained geological eye, after traversing the ground round about, to arrive at the conclusions stated by Professor Cole. I myself had been over the place dozens of times, having spent two summers in the neighbourhood, and I confess that before the borings were made there appeared to me no evidence that the “hard quartzite sloped steeply down close at hand on either side,” leaving a deep ravine filled in with glacial drift. As can be seen to some extent from the photograph reproduced in Fig. 7, to all appearance the local

opinion seemed probable enough—that the field was simply a layer of a few feet of clay over a rocky bed which was nearly level from east to west.<sup>1</sup>

It was desirable, in order to decide the question as to the exact levels of the respective bore-holes, that a careful survey of the field should be made. For this purpose my friend and former student, Mr. J. A. Cunningham, B.A., kindly assisted me in taking the levels shown in Figs. 8 and 9 on the next page. In these diagrams the vertical scale is exaggerated five times the horizontal, the surface depression shown in Fig. 8 not being perceptible in the field. These diagrams illustrate the probable geological sections of the field from bore-hole No. 1 to No. 2, nearly west and east, and also the section from north to south. It will be observed in Fig. 8 that the nearly dry bore-hole No. 2 is actually a little lower than the water-bearing hole No. 1. Possibly water might be found between Nos. 1 and 2; in fact, Mr. Jones predicted it would be a few feet to the east of No. 1. But this can only be settled by boring at intervals across from west to east; I hope to be able to do this eventually. As the bed rock was not reached in bore holes Nos. 1 and 3, its representation in both sections is in part imaginary, as is the lateral extent of the layer of sand in Fig. 8.

For the next memorandum I am greatly indebted to my friend Mr. J. R. Kilroe, of H.M. Geological Survey of Ireland. Mr. Kilroe, it will be remembered, gave me the benefit of his geological knowledge in the famous Waterford case described in the previous Report, and on the present occasion he at once responded to my request and, at considerable inconvenience, came from a distance on purpose to inspect the field.

Carrigoona Hill, rising to the moderate height of 802 ft. above the sea, commands one of the most attractive and varied panoramas in Wicklow. From its foot, eastward, spreads the park-like country containing and adjoining Kilruddery, the Earl of Meath's demesne, to the town of Bray and the sea beyond. North-westward the hill overlooks the Dargle Valley which separates it from the Dublin and Wicklow Mountains. Southward the eye is attracted by the valley of Killough, from which, on its east side, rises almost precipitously the Great Sugarloaf Mountain to a height of 1,659 ft.—this beautiful conical hill being separated from Carrigoona by the narrow, weird gorge known as the Rocky Valley. Further eastward the Little Sugarloaf Mountain bounds the landscape beyond the vale of Kilmacanoge.

The hills named above—Carrigoona, with the Great and Little Sugarloaf—constitute a group standing above the general level of the surrounding country, a circumstance attributable to the greater resistance offered to

<sup>1</sup> I am much indebted to Mr. Kendrew, of Dublin, for the photographs here reproduced. Mr. Kendrew happened to be taking some photographs in the neighbourhood whilst the boring was in progress.



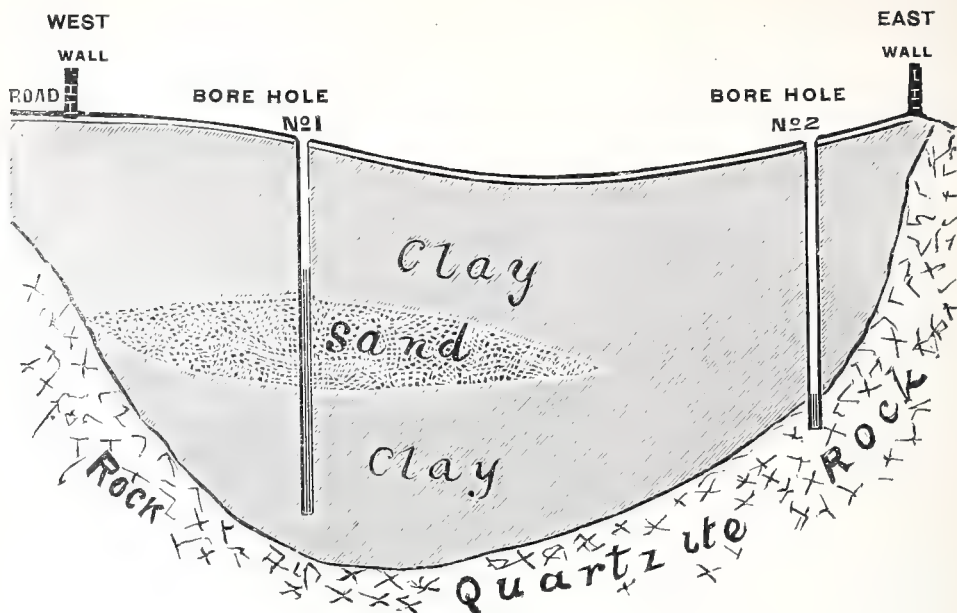


FIG. 8.

SECTION OF FIELD, EAST AND WEST.

Horizontal scale 1 in. = 60 ft.

Vertical scale, *exaggerated five times*, 1 in. = 12 ft.

SOUTH

NORTH

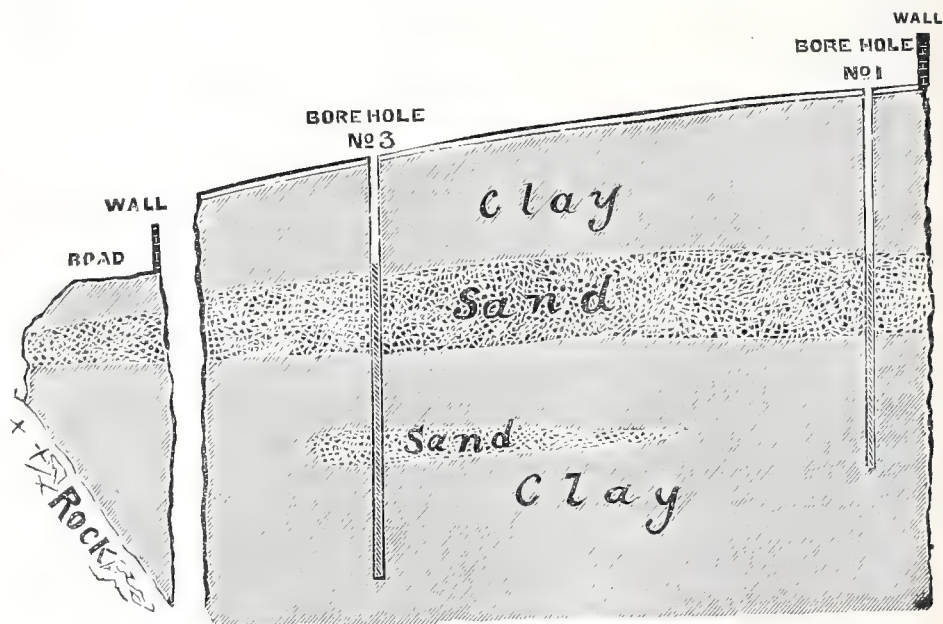


FIG. 9.

SECTION OF FIELD, NORTH AND SOUTH.

Horizontal scale 1 in. = 60 ft.

Vertical scale, *exaggerated five times*, 1 in. = 12 ft.

disintegration by quartzite, which forms the hills, than by the slate rock which prevails in the lower ground. Bands of slate also occur with the quartzite; and the results of unequal denudation are apparent in the uneven—in some places, rugged—aspect presented by the higher ground.

The rocky bed is largely covered with a mantle of glacial drift, which, though chiefly occurring in the low ground, is of fairly general distribution, resting in various places on the hill-sides, and partially filling hollows between crags. In these latter cases, the drift consists of local rock detritus, chiefly a mixture of sand, clay, and pebbles of slate and quartzite. Limestone pebbles also are interspersed in the drift which skirts the hill slopes; but few, if any, are to be found at higher elevations. The drift in the latter positions usually contain lenticular layers of sand and gravel, which form favourable reservoirs for underground water.

Alluding to the immediate topography of the ground experimented upon (E, Fig. 1), it is situated on the south-eastern slope of Carrigoona Hill, adjoining the Commons, that is, the heathery and rocky portion of the hill near its summit. Descending the slope, one observes crags of quartzite jutting up on either side of a hollow, which is partially filled with drift of the character described above. The south-eastern drainage of the hill would naturally in part flow along this hollow, and would rapidly disappear, were there no drift. The drift, however, intercepts the drainage, and would merely retard its flow were it quite porous in texture; but it is comparatively impervious, with the exception of the sand and gravel layers; and whatever drainage-water percolates through, reaches and is retained by the layers, which consequently are ready sources of supply when tapped. These are almost horizontal, probably dipping more or less from the sides towards the centre, and somewhat basin-shaped when spreading to any extent over the area occupied by the field. Hence, when such a stratum is pierced, the water rises to a height in the boring corresponding to its head in the basin-shaped layer—higher than the layer at its centre; and practically to the same height in borings which tap the same source—as in the case of the two successful borings with which we are concerned here.

Judging, then, from geological data, the nature of the ground would suggest as the most promising site for a water-boring, a point, say, midway between the quartzite crags—perhaps a little nearer to the western crag than the eastern, say 40 yds. from the former and 60 from the latter. The successful borings [Nos. 1 and 3] have, on the dowser's advice, been put down some 22 yds. from the west side of the field (lying immediately between the crags) and some 70 yds. from the east side.

It is very improbable—at least, very difficult to understand—that a merely casual visitor to the place, however shrewd, if not versed in geological facts, could divine the existence of water in the field by a cursory inspection, much less indicate with any accuracy the spot and depth at which it would be found. The success attending the experiment here is rendered the more striking by the dowser's additional prediction that water would not be found in the other field at the west side of Carrigoona (W, Fig. 1); though, to the ordinary observer, there would appear to be at least as much probability of finding a supply in this place as in the one above described.

J. R. KILROE.

Mr. Kilroe's opinion is all the more valuable as he is a recognised authority on the "drift," and it will be seen, from the last sentence in his interesting and full report, that he regards the dowser's prediction as very curious and remarkable, and, *a fortiori*, still more striking as having been given before any knowledge of the nature and slope of the underlying ground had been obtained.

For my own part, I do not think geological knowledge, nor even observation of the rocks of the neighbourhood, has anything to do with the dowser's success. If a plausible explanation is to be found, it is much more likely to be in the impression made upon the dowser by some surface indications of water. This impression may be an entirely subconscious one, and probably is more effective when it is so, and it ultimately evokes the involuntary motion given to the twig: for it certainly *is* involuntary—a purely reflex action—in a genuine dowser.

Although there were absolutely no indications of water in the experimental field, E, Fig. 1, yet on the way up to it there is a small spring (marked *S* in the map, Fig. 1) emerging from the rock, and flowing over the grass beside the road. This spring is not derived from the field, but comes from quite another direction, though a casual observer could not tell this.<sup>1</sup> A little beyond there is also evidence of some feeble springs.<sup>2</sup> Mr. Stone may have rapidly, and more or less unconsciously, perceived all that I have described, and when he traversed the field his subconscious self may have made good use of this information, and drawn a very happy but accidental conclusion therefrom. It could hardly have been his conscious self, for he was talking to me of other matters all the time, and appeared quite as astonished as I was when the twig forcibly leapt up and twisted round as he crossed the dotted line on Fig. 3. There was a barely perceptible contraction of the muscles of the hand when the twig thus vigorously turned; the movement is one that I never yet found any one able consciously to imitate without a good deal more visible muscular exertion. But that matter I will refer to again later on.

As regards Experiment II., where it will be remembered no water was found, there is in one corner of the field a hole some 6 ft. deep, with the débris from it lying about. This hole is quite dry, and, of course, the nature of the surface-soil can be seen from the sides of the

<sup>1</sup> There is a famous spring, known as Silverwell, that gushes out of the quartzite rock at the foot of the Great Sugarloaf Mountain, about a quarter of a mile further up the Rocky Valley; it is marked *S'* in Fig. 1.

<sup>2</sup> Which probably have their origin in the outcrop of the layer of sand shown in Fig. 9; but this is not obvious, as the turf at the spot is simply swampy, and a stretch of dry ground intervenes between this spot and the field.



hole and the débris, which consists of shingle and rocky débris. Mr. Stone passed sufficiently near the hole to have examined it if he wished to, but he seemed to pay not the least attention to it. Again, however, this dry hole may have been the means of setting his subconscious self to work, and have led to the result already described. It would, however, have been an unsafe induction from any conscious process of reasoning, for the boring No. 5 revealed quite a different material beneath the surface from that in the hole and in No. 4 boring, which was on the same level as the hole.

I have now placed on record all the information about these experiments which my geological friends and careful observation have enabled me to obtain, and I leave the matter to the judgment of each reader.

## § 2. *Experiments at Kingstown.*

### *Experiment IV.*

When Mr. Stone was in Ireland he came to Kingstown, and I asked him to see if he could find any underground water in the De Vesci grounds which are in front of my house. This place had already formed the subject of experiment, see Part XXXII., p. 175. Though I did not inform Mr. Stone of this fact, he might possibly have read of it in the previous Report, and hence I do not attach much, if any, value to this experiment, though, even assuming he had a vague recollection of the plan given in the former Report, it would have been of very little service to him. Referring to that plan (p. 175) the site of a disused well is marked: it is entirely concealed beneath the lawn, and only one or two persons know of its existence. The rod moved vigorously directly Mr. Stone passed over this spot, and he asserted a spring existed below, which he traced across the gravel path to the centre of the wood marked "shrubbery" on the plan; here, he declared, a plentiful supply of water would be found. To fix the place a snap-shot photograph was taken of Mr. Stone at this moment, see next page. Mr. J. H. Jones also tried the same place before he visited Carrigoona with me, and he, too, from the prompt motion of the "rod," fixed on a spot very close to that selected by Mr. Stears and Mr. Stone. There is no doubt water does exist below this spot, at no great depth; but whether it is generally diffused surface-water or not I have no means of knowing. There is a famous ever-flowing spring on a lower level not far from the grounds, but this was unknown to the dowzers. More to the point is the fact that by careful observation one can see a very slight circular depression on the evenly-covered sward, where the mouth of the well once existed. This possibly may have started a similar subconscious suggestion in each case; any

information or involuntary suggestion from myself or others was guarded against. In any case the experiment cannot be regarded as of much value, though the triple coincidence is certainly curious.<sup>1</sup>

*Experiment V.*

One other experiment I made with Mr. Stone. He told me that he had been able to detect buried water pipes or drains when water



FIG. 10.

SNAP-SHOT OF MR. STONE DOWSING.

<sup>1</sup> As these pages were passing through the press a curious incident occurred in the De Vesci grounds, which removes all doubt of there being a watercourse at one of the very spots marked by all three dowzers. Here, after long continued heavy rain, the earth suddenly fell in, to the consternation of the gardener who was driving a lawn-mower over the spot; a deep hole was left, on examination of which by myself and the gardener it was seen that an ancient-built watercourse (still carrying water) ran some 5 ft. below the surface, and had broken in. The "oldest inhabitant" knew nothing of this watercourse (for it was not a mere surface drain), and apparently led to the old well which is supposed to be there.

was flowing through them, and he at once consented to try the experiment in my presence. I therefore asked him to come into my garden, under the surface of which ran a water pipe, and tell me when the water was running or not. In the present case a water tap was turned on and off at the back of the house, completely out of ear-shot of Stone, who was in the front, and a system of signals was arranged so that I knew when the water was or was not running; the signals could not be seen by Stone, nor would he have known their meaning. As the house water supply comes through the buried garden pipe, when the tap was turned on at the back a flow of water was at once set up in the garden pipe. The result of this experiment showed that the number of times Stone proved to be right was very little different from the number given by pure chance coincidence. Hence this experiment, so far as it goes, was adverse to the dowser's belief that he could detect whenever water was running in a pipe.<sup>1</sup> The elder Mullins, now dead, was often successful, I am informed, in a similar experiment, though I do not know what precautions were taken; except in the case narrated by my friend Master Bruce, and given on p. 83 of the previous Report, and in the experiment by Mr. Finch-Hatton given in *Proceedings S.P.R.*, Vol. II., p. 102; but a good many instances of success, under strict conditions, would be required to be at all conclusive. A series of experiments of this kind were, however, tried a century ago, with the famous French dowser Bleton, and the results were certainly very remarkable (see p. 269).

This ended my experiments with Mr. Stone, and I have to thank him for his ready courtesy in submitting to any test I wished, not only without fee, but at some inconvenience to himself, as he put off an engagement in England in order to stay a night in Kingstown at my request. I intended to have tried blindfolding him and taking him over the same ground the following day, but neither he nor I could spare another day just then; he promised, however, to submit himself to that test when I or any friend could carry it out, either in England or over here. An account of a successful experiment of this kind with Mr. Stone is given in Part V., p. 210.

<sup>1</sup> The pipe was only a small leaden one, about  $\frac{3}{4}$  in. bore. The experiment is such a very simple one, and precautions to avoid misleading inferences can so easily be taken, that I hope some of our members may be induced to test any dowser in their neighbourhood who may be willing to try. I should be grateful for any information on this point. Care must, of course, be taken that no other taps are turned on in the house whilst the experiment is in progress, and that no sound of the running water, either in the pipe or from the tap, reaches the dowser.



## PART III.

CASES WHERE THE DOWSER FOUND A GOOD SUPPLY OF UNDERGROUND  
WATER NEAR TO A USELESS WELL OR BORING PREVIOUSLY  
SUNK TO AS GREAT OR GREATER DEPTH.

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No. 1. *The Shanklin Experiments.*

Without multiplying the evidence needlessly, I will now give a few more cases that have reached me where a useless well or boring having been sunk *without* the aid of the dowser, the latter found a good supply of underground water hard by, and at a less depth. Some forty cases of this kind were given in the previous Report; and although at first sight such cases appear to afford strong evidence of some supernormal, or at any rate some peculiar, faculty possessed by the dowser, a critical investigation on the spot reveals that this conclusion is unwarranted in many cases. Assuming that the dowser has a keen eye and considerable experience in his business, such successes could often be explained by shrewd guesses on his part. Most geologists who have so far overcome their prejudices as to believe the dowser may, to some extent, be an honest man, would explain *all* his successes in this way, attributing them to a conscious exercise of his power of observation. For my own part, I venture to disagree: the geologist speaks from his own experience, and rarely has he seen a dowser at work or investigated the evidence on his behalf. It is certainly not by a *conscious* exercise of his powers of observation that the dowser usually succeeds; sub-conscious observation and interpretation of surface indications there may be, and doubtless often is—we may call this instinct if we like. The question before us is, can we stretch this explanation to cover the whole ground?—for we are bound first to exhaust every known or possible explanation. To enable the reader to arrive at the geological evidence in the cases I am about to cite, I am greatly indebted to my geological friends, Mr. E. Westlake, F.G.S., and Mr. T. V. Holmes, F.G.S. The former has in several cases conducted a critical investigation on the spot, and the latter has in many cases appended valuable geological notes.

In the previous report (*Proceedings* S.P.R., Vol. XIII., p. 60) a brief account is given of the success of two amateur dowsers at Shanklin, in the Isle of Wight. Subsequently I visited the spot and obtained fuller particulars at first-hand. As the case seemed one eminently worthy of careful geological investigation, I requested Mr. Westlake, F.G.S., to make an independent examination and report.

Accordingly he did so, and the accompanying able report which he has drawn up is the result of his inquiries.

REPORT BY E. WESTLAKE, F.G.S.

On visiting the Isle of Wight in 1897 at Professor Barrett's request, for the purpose of examining the cases at Wootton and Arreton, criticised in the columns of *Nature*, I took occasion to inquire into some experiments at Shanklin, cited on p. 60 of his former paper, which were exceptional as having been made by members of the Local Board, who were themselves the dowzers. I found that, though three or four eminent geologists had been previously called in, they had altogether failed to find water, while the three dowzers had been as uniformly successful. As the strata at the point in question are as well known to geologists as any in England, this result was rather unexpected, and I have therefore taken some pains to get at the circumstances. After visiting the place on four occasions, and through the kindness of Mr. Milman Brown, the Chairman of the District Council (who has revised this account), and others concerned, I am now able to give the main particulars, which to make intelligible I will preface by a few words on the geological situation.

The Shanklin reservoirs are in the Gault Clay a mile south of the town, at the sources of the small stream which issues in the Chine. The collecting ground is the southern mass of the Chalk and Upper Greensand, the water issuing at the base of the latter where it rests upon the Gault. The valley is a right-angle facing the north and east, due to the main joints in the Greensand, which run E. and W. and N. and S. The dip of the strata is 1 in 45 ( $1\frac{1}{4}^{\circ}$ ) south. Along the sides of the valley the Greensand has a local inward dip of from  $2^{\circ}$  to  $8^{\circ}$ , due to the slipping out of its base upon the Gault, forming fissures which increase in size as they descend. On account, however, of the tendency of the water to run southwards, the valley is unfavourably situated for springs, and on its sides one only has been utilised, viz., on the south side just below the top cottage, The Retreat; the remainder are in the copse at the head of the valley.

Greatwood Copse, the scene of the water-finding, occupies a semi-circular hollow a quarter of a mile wide at the angle of the valley. The effect of this hollowing is that the Greensand, which extends for a quarter of a mile to the north, obtains in the copse a slight southern face traversing the fissures aforesaid, and it is here that the springs take their rise. The well (to be described presently) which was sunk at a spot indicated by J. Mullins, the late famous dowser, is placed in this southern face—the very best place, geologically speaking, *i.e.*, on

the above theory that the main supply comes from the north-east corner of Shanklin Down. As the consumption of water in the town has been continually increasing (in 1898 it was about 50,000 gallons a day), while the supply has never been large (about 40,000 gallons in September, 1898), there have been continual efforts to augment it, which, as far as they relate to our inquiry, are as follows :—

The first wells in the copse, Mr. Brown informed me, were sunk in 1875 by the London engineers, Messrs. Quick. The east well, 150 yards west of the road, is 55 feet deep, and held (September, 1898) 9 feet of water; its cover is 411 feet above Ordnance Datum (*i.e.*, above sea-level). The west well, 20 yards further in, is  $420\frac{1}{2}$  feet O.D., and  $65\frac{1}{2}$  feet deep, with 10 feet of water. There is thus a difference of only 1 foot in the bottoms of the wells. They are connected by a sub-water heading so as to form virtually one well, and by a middle heading for syphoning purposes, the floor of which averages 383 feet O.D. They supply, I am informed by Mr. E. C. Cooper, the town surveyor, about 4,000 gallons a day. The water, when syphoned, stood (September, 1898) at 365 feet O.D. When not syphoned it stood (December, 1899) at 382 feet O.D.; in winter it rises till it overflows the outer sill of the middle heading at 388 feet O.D.

The upper heading, which is close by, was driven without expert advice above a wet place in the copse, and followed the water in a westerly direction for a distance of from 400 to 500 feet. This heading starts at a level of  $401\frac{1}{2}$  feet O.D., at a point 15 yards west of the middle heading and 18 feet above it. (See Fig. 12.)

About 1878 Mr. H. Bristow, F.R.S., F.G.S., of the Geological Survey, was consulted by the Board, and visited the spot. On his advice the upper heading was extended to a point about 900 feet west of the entrance directly under the old chalk pit on Shanklin Down, but without getting any appreciable addition to the flow of water. When I measured it in December, 1899, the quantity was 9,000 gallons a day, which, however, is below the average; the turncock, Mr. Whittington, said when the flow was good it was double this. That hardly any water was found in Bristow's extension is probably due, as pointed out by Mr. Topley, F.R.S., in a subsequent report, to its having been driven "at too high a level"; "water," he adds, "might be found by a well or boring at the far end, but would probably not rise into the heading."

In 1885 the Board consulted two other geologists, Mr. W. Topley, F.R.S., of the Geological Survey, whose report in May of that year I have just cited, and Mr. Mark Norman, author of a geological guide to the Isle of Wight, but their schemes of sinking to the Lower Greensand were not only costly but problematical.

In 1893, as a last resort, the Board called in Mr. John Mullins, the well-known Wiltshire dowser, who marked several spots in the field to the south of the reservoirs, but the conditions were too onerous for them to sink there. On their own land, however, by the S.W. corner of the upper reservoir, which was from 50 to 100 yards to the north of Mullins' marks, they sank a well 30 feet deep, but without getting water. Then they got Mr. Parsons, the Brading harbour-master, to bore at the bottom of the well



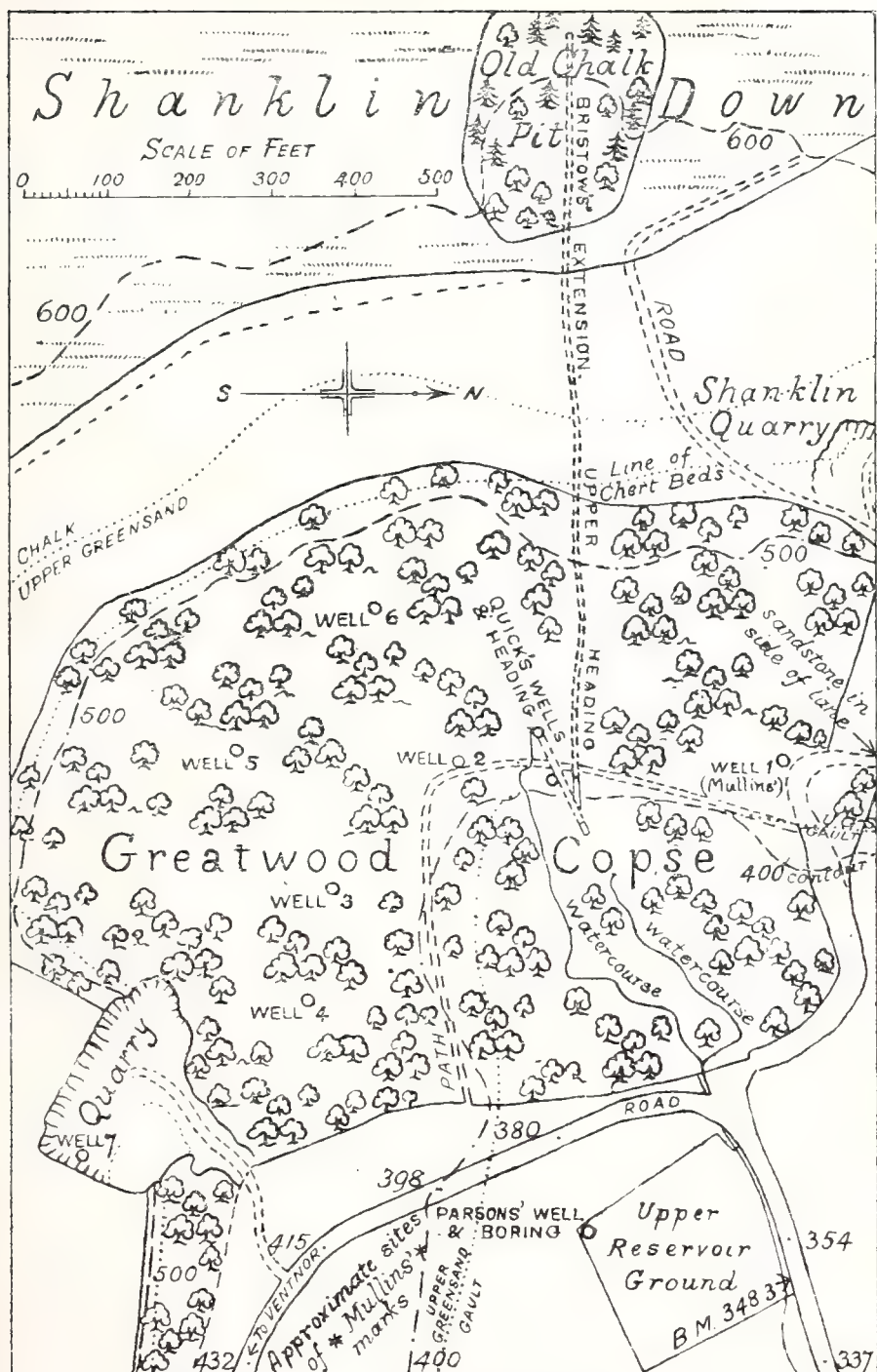


FIG. 11.

*From the 25 in. Ordnance Survey Map of the Isle of Wight, 1863, with additions from the Geological and local Surveys.*

The numbers, 337, etc., indicate height above sea level. Strong dotted lines are the 400, 500, and 600 ft. contours; faint ditto are geological boundaries. Asterisks are Mullins' marks as remembered. The watercourses are the natural drainage.

for 97 feet—*i.e.*, to 127 feet from the surface—but he got no water, only bad air, which suffocated him, his son, and his men, who were all found dead in the well one morning in July. Mr. Thom, of Manchester, continued the boring from 200 to 300 feet deeper into the Lower Greensand, but after diminishing the bore from 8 to 4 inches the Board refused to sanction a further reduction and the boring was abandoned. The borehole is now disconnected. The well has a supply from surface soakage of from 400 to 800 gallons a day, which is not used. I may remark on the foregoing that Mullins' marks, being on the outcrop of the Upper Greensand, offered a reasonable prospect for finding water, whereas the site chosen for the well at a level of 373 feet O.D., being on solid Gault clay, could have only a little from surface percolation.

In 1895 the Board consulted Mr. A. Strahan, F.G.S., of the Geological Survey (who had re-surveyed the district), on the prospect of obtaining a supply by a deep boring on the same site. In his report of September of that year he says, "I do not think there is a more favourable site, from a geological point of view, in the immediate neighbourhood." His plan, like that of Messrs. Topley and Norman, was to sink some 400 to 700 feet, and pump from the Ferruginous Sands below the sea-level; "but this," said Mr. Brown, "would have cost us a great deal, and he could not promise us anything certain when we got there."

Returning to Mullins' operations, it appears that after he came, early in 1893, two members of the Board, Messrs. Brown and Bailey, who had found the twig turn in their hands, started dowsing in the copse on their own account. They each found the twig turn at certain places nearly coincident; one of these places they found to be the same spot previously fixed upon by Mullins. At this spot (No. 1, see map, Fig. 11) they sunk a well 26 feet deep, and at two points of their own (Nos. 2 and 7) they sank wells 28 feet and 64 feet deep. Though No. 1 is only 100 yards to the north of the headings and No. 2 only 40 yards south, a good supply of water was found in the three wells. No. 1 supplied, at first, 7,000 gallons a day; No. 2, about 10,000 gallons. No. 7 required, at first, nine days to pump it out; at the present time, however, it only supplies about 2,000 gallons a day. In 1895, again following the twig's indications, they sank four more wells (Nos. 3, 4, 5 and 6), in *all* of which water was found. Nos. 3 and 4 have supplied from 1,500 to 2,000 gallons a day. No. 5, when dug, had 2 feet of water which ran freely, but has since failed. No. 6 (72 feet deep) has not been connected to the reservoir. These additional supplies, though quite inadequate for a growing town, had, said Mr. Brown, kept them going for the time being. The height of the ground at these wells (Nos. 1 to 7) I found, by levelling from St. John's Church, to be 415, 415, 413, 419½, 447½, 465 and 466 feet O.D. All the wells are now covered over, except No. 7, but I have ascertained their respective depths to be, approximately, 26, 28, 28, 30, 33, 72 and 64 feet; their bottoms are, therefore, approximately, 389, 387, 385, 389½, 414½, 393 and 402 feet O.D. Hence, except in No. 5, the water lies at levels between the middle and upper headings, showing the dowsed water to come from the same stratum as that in the other wells and headings. In No. 7 the water stands at 416½ feet O.D., so that the exhaustion of No. 5 is probably due to its not having been sunk deep enough.

The geological position of Parson's well is in the Gault clay, which is penetrated after the first 100 feet, the remainder of the boring being in the sandstones of the Lower Greensand (Neocomian). Quick's wells and the middle heading are in the passage-beds between the Gault and Upper Greensand, which pass into one another insensibly. The upper heading is mostly in the Upper Greensand, the base of which, according to the Geological Survey, is a little below the path in the copse, and coincides nearly with the 400 feet contour. The dowsed wells start in the Upper Greensand, and, except No. 5, penetrate to the passage-beds.

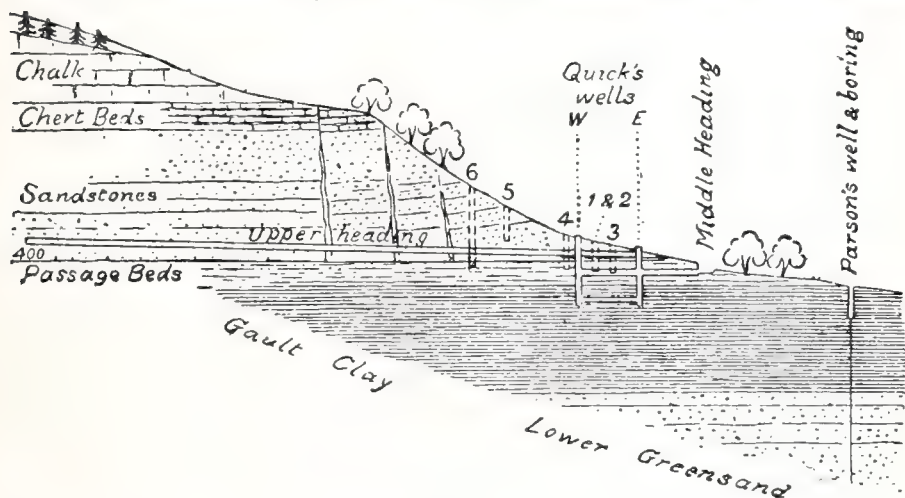


FIG. 12.

Diagrammatic Section through Greatwood Copse: vertical scale (exaggerated twice) 208 feet to one inch.

The dowsers' wells, shown by dotted lines, are numbered as in Fig. 11.

Parson's boring extends to double the depth shown in the above diagram.

Tabulating the results obtained by the professionals and dowsers respectively, we have:—

*Engineers' and Geologists' Results.*

			Supply when good.		Supply at worst.	
Quick's east well ...	...	55 ft.	} syphon,	4,000 gals. per day		
„ west „ ...	...	65 ft.				
Submerged heading ...	...	60 ft.				
Middle „ ...	...	120 ft.	} flow,	20,000	„	9,000 gals.
Upper „	about	900 ft.				
Parson's well and bor- ing, not connected	} „ 300 ft.					
Total 1,500 ft.				24,000 gals. <sup>1</sup>	„	9,000 „

<sup>1</sup> This estimate agrees, Mr. Brown tells me, with a measurement of 25,000 gallons made by the surveyor, Mr. Colenutt. Parson's well is surface water and not used.



*Dowsers' Results.*

		Supply when good.		Supply at worst.	
No.		gals. per day		gals.	
No. 1	well (Mullins's)	26 ft.	flow	7,000	
2	„ (B. and B.'s)	28 ft.	„	10,000	
3	„ „	28 ft.	„	2,000	} „ „ 0 „
5	„ „	33 ft.	„	1,000	
4	„ „	30 ft.	syphon	2,000	„ „ ?
6	„ „	72 ft.	not connected	?	„ „ ?
7	„ „	64 ft.	pump	2,000	„ „ ?
Total 281 ft.		24,000 gals.		„	1,250 „

The dowsers' first total includes the first flows (always exceptionally large) for Nos. 1 and 2; in the second total, which I took in December, 1899, after a very dry season, most of the wells had gone dry. Thus, for comparison with the engineers' total, the dowsers' first total is too large, and the second too small. To compare the water found by them respectively, we ought properly to have figures either for all the first flows, or for some time when the springs were high enough to get flows from all the wells. Mr. Cooper writes (July 11th, 1900): "I think if you put the value of the [dowsers'] wells at their best as a third the value of the headings, you will then be stating their [relative] value very fairly." According to this estimate, and taking the amount of excavation into account, the dowsers obtained nearly twice as much water per foot as the other water-finders. Of the success of the latter, five-sixths was due to empirical means, one-sixth to the engineers, and none to the geologists who went either above or below the proved water-bearing bed. The dowsers on the other hand went straight to the point by putting their wells into the water-bearing bed itself.

The relative professional failure, however, does not entitle the dowsers' results to pose as mysterious. The top of the Gault is typically water-bearing: the well-known landslip at East End, a mile or two distant, is due to the water in this very bed, the level of which in the copse was known from Quick's two wells. The probability, therefore, of *some* water at this horizon no geologist would doubt. What they did doubt was whether a useful quantity would be found in the copse by driving or sinking at haphazard: thus in his report Mr. Topley says, alluding to Mr. Bristow's attempt, "Galleries have been driven into the Upper Greensand for the purpose of intercepting springs . . . and I cannot recommend any further experiments in this direction." The subsequent experiments of the dowsers, introducing *ex hypothesi* a new method of discovery, do not throw light on the problem of haphazard sinking, except in so far as the doubtful utility of wells Nos. 5 and 6 tends to show that a useful quantity of water is *not* to be met with

everywhere. The results of the experiments may be considered under their (1) qualitative and (2) quantitative aspects :—

(1) The finding of some water in all the dowzers' wells if claimed as evidence for dowsing involves the assumption that water could *not* be found everywhere by sinking into the passage beds. Between this and the geological deduction that it probably *could*, there is no direct evidence to decide. Quick's wells, though supporting the geological view as far as they go, are close together, and inadequate for comparison with the dowzers' seven distributed over the copse.

(2) The 50 per cent. superiority of the seven dowsed wells assumes that Quick's wells represent the average of as many trials by ordinary methods ; granting this, the superiority seems too slight to exclude accident. In support of the dowzers' opinion that they had a faculty for pitching on the right places, Mr. Brown mentioned that in one or more wells they came on loose rubble like that filling the fissures in the adjoining quarries, which made him think they had hit on a "vent" (water-bearing fissure) : this was confirmed by Mr. Young, the sinker of wells Nos. 2 and 7. Failing an examination of the wells at the time, we can now only judge of the extent of this feature from its probable consequences in an increased supply, so that it does not add to the evidence. Hence the dowzers' success cannot I think be pressed in either aspect, for want of proper terms of comparison.

There remains the broad result that for equal amounts of work *the dowzers got twice as much water as the engineers, while the geologists got hardly any.* As regards surface signs of water in the copse I did not notice any in relation to the dowsed wells, and have given the dowzers the benefit of assuming there were none. But the absence of a reason for digging somewhere in particular may lead to digging nowhere, and in such cases the rod may succeed by supplying a motive for digging. The sceptic may urge, "the dowser turns the twig and fortune does the rest." We see, however, that geological explanations of dowzers' successes do not necessarily show that the geologists would have found the water themselves.—E.W.

#### No. 2. *The Errol Case.*

The following case is of interest, as it was first published in the columns of one of the leading engineering journals, *The Engineer*, for October 8th, 1897, after an editorial note on the Divining Rod. The writer of the accompanying letter to *The Engineer* is a member of a well-known engineering firm in Perthshire :—

Errol Works, Errol, Perthshire, *October 1st, 1897.*

Referring to your article on the above subject, as you say of yourself, "There was no one in the world more anxious to believe that these

divinations were hoaxes than I was." The worst of it is, they will keep on succeeding. I will give you my own experience. A gentleman in this parish—Mr. Clark, of Taybank—started above where he thought he would get water. He went down, I think, over 460 ft., and got none. He heard of a water-finder, now deceased—Mullins by name. Of course, he did not believe in him, but thought he would give him a trial. A lot of folks turned out to see him, amongst them the Rev. Robert Graham, LL.D. My father and I were also present. I was quite sure there was nothing in it. Mullins was first taken to the bore Mr. Clark had in progress. He was not told it was a failure. He was taken over it with his V-shaped hazel twig, and asked how much water would be got there. He said, "You may go down hundreds of feet, and you will not find any water." He—Mullins—discovered water not fifty yards from this bore. He indicated its course, and marked it by pegs driven into the ground. He told Mr. Clark if he went down 40 ft. to 50 ft. he would get so many gallons of water, and, if what he said was not true, he would pay for the digging of the well. Mr. Clark dug it, and got the supply named at the depth stated. Mullins held the ends of the V-shaped twig in his hands—an end in each hand—firmly. Whenever he came over running water the twig turned round and round, and, if held long enough, the twig twisted till it broke. When Mullins had the twig in his hands, he more than once allowed me and another party to take an end each, where the twig projected past his hands. We held on as hard as we could, and with the turning of the V part of the hazel twig, it was twisted till it hung by the bark. Yet another proof. I took the V-shaped wand in my hand, and passed it over running water without any result. Mullins laid his hands on my wrists, and grasped them firmly, when the twig instantly began to turn, and continued turning till he removed his hands. He never touched the twig while it was in my hands. Whatever folks may say to the contrary, there *is* something in it. "Facts are chiefs that winna ding, and darna be disputed."

DUNCAN A. MORTON.

In reply to my inquiries Mr. Morton writes :—

Errol Works, Errol, Perthshire, *October 11th, 1899.*

Yours of 9th inst. came duly to hand. I regret that I never replied to yours of October 14th, 1897. As I have not much time at my disposal, I will reply as briefly as I can to your queries. Before Mullins came, Mr. Clark had made a bore 430 ft. deep, and got little or no water, and that little he did get was bad and generally supposed to come from some drains or something of the kind. Mullins came and was taken over the bore with his rod, and said, "You can go down a thousand feet and you will not find any water there." He discovered a spring *at once* 100 yards from the bore at a depth of 40 ft.—an abundant supply of good water—first-rate water, indeed. Mr. Clark uses it yet, though there is a district supply now! Mullins said water would be got at a depth of 35 ft. to 40 ft., and if not, he would pay for the digging of the well.

I was as determined against the rod as any man alive till I saw it, but I could not shut my eyes to facts.



I took the V-shaped hazel wand in my hand, and passed over where Mullins said there was *running* water. The twig never moved, but whenever Mullins grasped my wrists the twig turned round and round till it broke and hung by the bark. I explained all this in my letter to the *Engineer*, October, 1897.

I saw Mr. Clark, of Taybank, last night, and he gave me the particulars. Mullins indicated the course of the running water, and we put in pegs, and they just looked like (in an uneven line) the wriggling of a stream, and came out right in line on the face of the rising ground with a drain which Mr. Clark had made, and which he said cut through a stream of water.

I shall be glad to reply to any further inquiries from you.

DUNCAN A. MORTON.

I have also heard much to the same effect from Mr. Clark, and, in a subsequent letter, Mr. Morton informs me he has, at my request, seen Mr. Clark again, and sends me the following additional particulars. The exact depth of the useless boring was 432 ft. This bore first went through a stiff blue clay or marl, turning to red when deeper; the water that was eventually obtained was insufficient and not good. Mullins' well was dug 40 ft. and then bored to a total depth of 80 ft., when the water rose to within 12 ft. of the surface; in spite of very dry weather and continual use it has never fallen below 17 ft. from the surface. It was the old Mullins, now dead, who was employed.

Mr. Holmes' geological comment on this case is as follows:—

Looking at the general geological map of Scotland by Geikie, I find Errol on the "Old Red Sandstone" Series. The beds pierced in the deep boring were, however, it is said, clay or marl. (It may be worth remarking that a name such as *Old Red Sandstone* only implies a series of rocks of a certain relative age in which red sandstone is the predominant rock.) Of course clay and marl are not likely to give a decent water supply at any depth. But close to Errol come the alluvial deposits of the Carse of Gowrie, and these (or any other alluvial deposits) would give a flat surface, contrasting in the most marked way with ground where Old Red Sandstone formed the surface beds, and might furnish a good supply of water at 35 ft. or less. I don't know the place, but the rough diagram below will explain the *general* difference. If the river Tay ran through soft clay, the slopes outside the river deposits would be more gentle in contour than would be the case with harder rocks, but the practised eye would hardly fail to distinguish between slope and flat whether on the Tay, the Trent or the Thames.

T. V. H.

The diagram showing the conjectured geological section, which Mr. Holmes kindly adds to his note, is not reproduced, as it is of course only a conjecture. I should be extremely obliged if any of my readers in Perthshire would make further geological inquiries into this case, as it is evidently one of considerable interest. It will be remembered that it was old John Mullins who was the dowser in the famous

Waterford case, which was so fully discussed in the previous Report. The useless bore in that case was over 1,000 ft., and there were absolutely *no* surface indications to guide him.

### No. 3. *The Lytes Cary (Somerset) Case.*

The following is a very interesting case. The dowser, in this instance, was an amateur, the late Mr. E. M. Hippisley, a leading surveyor and land agent in Wells, Somersetshire, who was singularly successful as an amateur dowser (see p. 216). In a paper on his own experiences in dowsing that he read at the Bishop's Palace, in Wells, in April, 1892, he stated :—

At Lytes Cary [near Kingsdon, Taunton] there is an interesting old English house with a chapel in good preservation, which, with some modern [farmhouse] buildings, enclose a large courtyard. I found [by dowsing] on the south side near the garden, indications of water, and had a well sunk 25 ft. deep, in which the water rises to within 13 ft. of the surface, and here a singular incident occurred. During the past year part of the paving on the opposite side of the yard gave way, when an old well was discovered 45 ft. in depth, but no water in it, and only 30 ft. from the new well.

Mr. Westlake kindly went over to Lytes Cary and made a careful examination of the surroundings, and of the geological conditions as well as the facts of the case. He reports to me as follows :—

*May, 1900.*

“Lytes Cary is in the southernmost bend of the river Cary, in Somersetshire. The farmhouse is in the occupation of Messrs. Porter and Porter, who aided my inquiries in every way. I was first shown the sites of the wells, both quite covered over and concealed; they are about 40 ft. apart. The old well was carefully built, and is 60 ft. deep (not 45 ft., as Mr. Hippisley thought) so I was informed by the estate builder, who had measured it himself. He told me also that Hippisley's well is 25 ft. deep, sunk in the marl all the way down, and the sides are strong enough to stand without masonry.<sup>1</sup> It still continues to give an abundant supply to the house, and contained 9 ft. of water when last opened. There are also three other wells on the N. and W. of the premises, about 190, 140, and 80 ft. distant from Hippisley's well; one of them, the furthest off, is 22 ft. deep, and yields a small supply of “suspicious” water; the second, which once contained a few feet of foul water, has been filled up; and the third was long ago abandoned and used as a cesspool.

“The farm premises are all on level ground. The strata at the farm, according to the geological survey, are Lower Lias, which, as

<sup>1</sup> This does not necessarily show the strata were different.

seen in quarries about a mile to the north, consists of the usual thin limestones with partings of marl, and has little permeability except in the vertical joints. The beds at the farm appear to underlie the above, but there are no surface indications, geological or otherwise, to show that water is more likely to be found at one spot than another. Had the existence of the old well been known a geologist would have been chiefly concerned to *avoid* its proximity. In finding water so near it Mr. Hippisley certainly achieved the improbable, though, as agent for the estate, he was thoroughly acquainted with the place, except the existence of the old dry well, which had been lost sight of by all. A characteristic of the Lower Lias is to hold water in unsuspected fissures. This places the hydro-geologist at a disadvantage, and renders this formation a suitable ground for dowsing, or chance discovery, as the case may be. I have referred to other cases of the dowser's success in the Lower Lias in my Report, p. 315.—E. W."

#### No. 4. *The Sherburn-in-Elmet Case.*

I heard that Mr. Stone had been engaged by the owner of some property at Sherburn-in-Elmet, Yorkshire, to find a water supply where a well, over 150 ft. deep, already existed, but had run dry, and that Stone had predicted by the "rod" that an abundant supply would be found less than 50 ft. from the old well at a depth of 60 ft., and that, upon boring, Stone's prediction was verified and a good supply obtained. I wrote to the owner of the property, Mr. Lolley, for an exact statement of the facts, and had the following reply:—

Northfield House, Lovell Road, Leeds, *December 6th*, 1899.

It is quite true we had a well 70 ft. deep, and had bored about 90 or 100 ft. from the bottom of the well without finding water. The well had been in use about fifteen years, and always had a good supply until an artesian well was sunk in the low-lying part of Sherburn, after which we seemed to lose the water.

Seeing Mr. Stone's testimonials, I applied to him. Mr. Stone visited Sherburn and said that there was a good supply of water some 60 ft. deep beneath a place he indicated, about 15 yds. from the old well, and on the boring being carried out the water *was* found at 62 ft. deep.

WALTER LOLLEY.

This case is of interest, and I should be glad of some further information concerning it, especially of the geological character of the locality. Sherburn-in-Elmet is midway between Leeds and York, and Mr. T. V. Holmes, F.G.S., informs me it lies near the bottom of the slope of the Magnesian Limestone, which here dips down to the Old Red Sandstone that overlies it in the valley. I wrote to



Mr. Lolley to ask if he had kept a record of the strata bored through, and he replies :—

Leeds, December 28th, 1899.

The borehole at Sherburn was through limestone and clay, the rock is the "Magnesian" limestone, and my property is on the eastern slope of this; in fact, the lower part of the village further east is on the alluvial clay of the Vale of York. The water was found in a layer of gravel and clay in the borehole after getting *through* the limestone.

WALTER LOLLEY.

#### No. 5. *The Tiddington House (Oxford) Case.*

The following letter appeared in the *Farmer and Chamber of Agriculture Journal* for October 18th, 1897. The case is interesting as the artesian well borers were attempting to bore through the Oxford clay, whilst the dowser, as in many other cases, evidently found a permeable surface stratum, of sand or gravel, above the clay.

Tiddington House, Oxford, October, 1897.

Being without water for my garden and stable, except what was fetched by water cart some distance day by day and caught from rainfall, and this altogether unsatisfactory, I experienced a great difficulty in getting a proper water supply, and eventually decided to employ a firm of artesian well borers to bore for water for my use in 1883. A spot was decided upon, and a boring put down to a depth of 312 ft., which cost me over £300, and no water whatever was obtained; all I have for my money is the piece of parchment containing the sections of the boring and a few samples of the formation. . . .

I engaged Mr. Tompkins to search my property for a water supply . . . and instructed him to carefully test my stable yard, as this was the site I was most anxious to find water on. After carefully searching this property he reported that no water existed there, and that it would be simply useless to attempt to obtain water here. I then informed him of what I had previously done, and the amount of money I had uselessly spent. Thus the water-finder proved correct in his first test.

A move was then made into the gardens, and, after going over them, the water-finder reported a stratum of water was running across the corner of the garden, and was an overflow from a spring above. He traced up this stratum of water. The twig he was using was a white thorn cut in the shape of a V, which he held by the two prongs apex downwards. As he walked over this stratum of water, the twig kept rising in front of him until he came to the head of the spring<sup>1</sup> in the upper part of the paddock above, when it turned completely over and over, and so strong was the upward movement of the rod that it bent considerably in its frantic endeavour to turn over, and on being held firmly by the water-finder, it broke off in his hand. Naturally, we were all surprised and astonished at such a remarkable occurrence.

<sup>1</sup> This is an absurd but favourite expression of Mr. Tompkins, who is under the delusion that underground water originates in a reservoir, and that the higher up you trace the "spring" the more copious the supply.—W. F. B.

I instructed Mr. Tompkins to prepare me an estimate to sink a well four feet diameter in the clear, and to supply and erect a wind engine and storage tank. The well was put in hand, and at a depth of 30 ft. water was struck. The well was steined up before sinking deeper, and a strong pump put down to keep the water under, and before sinking many feet it became necessary to put on seven men to keep the water going. From the inrush of water the bottom men were working up to their knees in water, and the moment the pump and buckets stopped they were in danger of being drowned out. The work proceeded under these circumstances for a week or so, until it became useless to continue the work owing to the abundance of water; 10,000 gallons per day being the yield of the spring at a depth of 37 ft., and this site is at a higher elevation than my stable yard. I cannot speak too highly of Mr. Tompkins' practical success, both as regards his finding water and the satisfactory way in which he has carried out the work.

G. W. BENNETT.

In reply to my inquiries Mr. Bennett confirms the statements in his published letter and adds:—

Tiddington House, Oxford, *October 9th*, 1899.

My well has never gone dry, neither this summer nor last, though it gets low generally about November or December. The only drawback to the windmill is, I find, we have not always enough wind to pump, and sometimes we are four or five days without wind for pumping for drinking purposes. There are [not] and never have been any wells near where Mr. Tompkins found the water with the divining rod.

G. W. BENNETT.

Mr. Holmes' comment on this case is as follows:—

Oxford stands upon "Oxford Clay," the thickness of which varies from 300 ft. to 600 ft. (Woodward). But over the greater part of the site the Oxford clay is covered by gravel, which is usually from 8 ft. to 20 ft. thick. And gravel on a higher level is often found to cap the plateaux of the district. I do not know whether Tiddington House is *in* or *near* Oxford. The unsatisfactory boring of 312 ft. was evidently in the Oxford Clay. But the satisfactory supply was evidently from sand and gravel lying above the Oxford Clay. But the gravel on which Oxford stands is not that deposited by the rivers Isis and Cherwell, but is of the higher level and older date. Where in Oxford and its suburbs, between the Isis and Cherwell, the ground is covered by a thick mass of superficial sands, gravels, etc., there the ground is likely to be higher than where the Oxford Clay is bare. And the contours of the ground will tell at once to the practised eye where there is bare clay and where there is a gravel-capping above it, or river-gravel at a lower level. Of course, if, in this case, the successful sinking had been in the gravel of the Cherwell or Isis, and the house on the Oxford Clay, it would have been at a lower level than the house. It appears, however, to have been at a higher level than the house—or at least the stable yard. Gravel seems to have the same way of capping the highest ground in the Oxford Clay country that it has in the London Clay districts.

T. V. H.

Mr. Holmes' geological diagnosis of this case is confirmed by the following letter, which I received in reply to my inquiry as to any surface indications, and as to the nature of the boring and also the distance apart of the useless boring and Tompkins' well. The sand or gravel bed was evidently thicker than intimated by Mr. Holmes, and no doubt the rush of water came when the well-sinkers got close to the underlying impermeable clay. All the same, this does not explain *how* Tompkins hit on the right spot, for "the practised eye" of the well-sinkers who put down the useless bore, and that of Mr. Bennett's gardener, failed to discover the right spot.

Tiddington House, Oxford, *January 3rd, 1900.*

The Tompkins well is on rather a higher elevation than where we bored in 1883, and is quite 150 yards from that boring. The Oxford clay is certainly capped by sandy grit or gravel. We first took off about 3 or 4 ft. of turf mould; then went into a red sand which continued some 25 or 30 ft., when the water came in so fast they could not go on digging the well. To the ordinary eye, no one could possibly have said where to hit upon the spring. Mr. Tompkins went backwards and forwards in a zigzag course with the twig till he came to what he calls "the fountain head," where he cut a sod out of the turf to show the well-sinkers where to begin to dig the well.

I should say we are about 40 ft. above the level of the river Thames, which connects with the Isis at Dorchester, Oxon., and there it is called "Thamisis" or Thames. We are nine miles from Oxford, and nearly two from the river Thames.

G. W. BENNETT.

#### No. 6. *The Chelford (Cheshire) Case.*

The following letter appeared in the *Manchester City News* for October 23rd, 1897. The writer is the head of a well-known firm of chartered accountants in Manchester.

Bella Vista, Heaton Moor.

Having noticed the great amount of controversy that has of late been manifested, together with the number of articles that have appeared in various contemporaries, on the so-called divining rod as a means of discovering water supplies, I venture to give you an account of my recent experience with a water-finder and his work.

Having had occasion to obtain a new water supply for a house in Cheshire, the opinion of a firm of artesian well borers was obtained. They recommended that at a certain spot an artesian well should be sunk, with the idea of tapping a spring of water, and at the same time to have the advantage of an artesian well to ensure absolute purity. Accordingly a contract was obtained and a well [boring] made, with the hope of finding a supply of water at a reasonable depth; and after several months' work, entailing considerable expense, boring was carried to a depth of about 480 ft. But the long looked for supply of water was not obtained, and after trying for some time to get a supply of water from this deep well, I found that it was almost useless, and



in the end had to be abandoned as a failure. I then, for the first time, sought the advice of a firm of water-finders who use the divining rod, and having made an appointment with Messrs. John Mullins and Sons, Colerne, Box, Wiltshire, I met a member of their firm on the estate.

In the simplest way possible he began to prospect the grounds for what he called a spring, or water-bearing strata. Carrying in his hands a V-shaped hazel twig, holding the point downwards, with one fork in each hand, he began to walk across the lawn, and after walking a distance of about twenty yards he suddenly stopped, with the twig automatically twisting in his hands. He declared that here a spring existed at a depth from the surface of not more than 40 ft. To convince me that he could not govern the action of the twig in his hands, he requested me to grip the ends of the twig, and so try to keep it from turning, but in trying to do so I was quite powerless. After trying for some minutes to gauge the volume of the spring, which he professed to do by the action of the twig, he then gave what he considered to be the approximate yield of water per day, and proceeded to trace the direction in which the spring was running. Having gone into the kitchen garden, he said the spring was only 10 ft. from the bore which had previously been made to so great a depth.

In order to prove that he had faith in his own method, he asked to be allowed to undertake the necessary work, and to obtain the supply of water which he had predicted, at the same time undertaking that if his predictions were not substantiated, the work done by him would not be charged for. A contract was entered into on these terms. As a result, the well has been made, and, at a depth of 36 ft., a good supply of water has been found. The water is of excellent quality.

I venture to trouble you with these facts, because I think that this method of discovering springs of water deserves to be better known.

CHARLES WILLIAM NASMITH.

I wrote to Mr. Nasmith for some additional particulars and received the following reply :—

69, Princess Street, Manchester, *June 6th*, 1898.

In reply to yours of the 31st ult. *re* the water supply at Mereleigh, Chelford, I have very little to say beyond that which I gave in my letter addressed to the *City News* of October 22nd, except that Mullins was quite a stranger in the neighbourhood, and had no knowledge of the subsoils. He first found the spring of water on the lawn, and traced its course from the lawn to the kitchen garden, and the site he selected for the well was 10 ft. from the deep bore, and at this site the well has been made, with the result given in my letter, and the supply is still satisfactory.

The subsoil where the spring was found was sand, with about 13 ft. of clay on the surface and clay below the level of the spring, but it appears that in the old deep bore it was all clayey.

No other authority was previously consulted beyond the firm who did the boring. They, I believe, worked by the geological chart of the neighbourhood.

CHARLES WILLIAM NASMITH.

Mr. Nasmith also sent me a plan of the estate with position of the useless boring and Mullins well, but this adds nothing to the information given in his letter. In reply to Mr. Nasmith, the artesian well borers, Messrs. Timmins, wrote to the local papers to say that the well sunk by Mullins merely yielded surface water and was therefore unsafe to use for drinking, that the surface soil was sand and an old disused well sunk in this sand existed close by. Mr. Nasmith, however, maintains that the Mullins well does *not* yield surface water, but is sunk below an impermeable stratum of clay. I submitted the whole correspondence to Mr. T. V. Holmes, who writes in reply as follows:—

28, Crooms Hill, Greenwich Park,

London, S.E., *June 11th, 1898.*

As to the discrepancy between the accounts of Timmins and Nasmith as to the strata at Chelford, I can offer only the following remarks:—My experience of the sections obtained from well-sinkers and borers is that they are extremely careful as to details, and go into more detail than a geologist usually would. The well-sinker will not mistake darker-coloured sand, which owes its special hue to saturation with water, for clay.

Then, if we suppose that there were *locally* 13 ft. of clay at the surface, the water found in both old and new wells evidently came from the same source, the lower part of the sand; and if the occasional surface clay did not protect the old well from surface pollution, it would not protect the new one.

As regards the deep boring, the new red marl would be mainly what might be termed clayey, and its top would probably be not many feet below the level of the water in the sand.

I enclose a rough diagram to illustrate the general nature of things. The alternatives as regards water in the district are evidently that you either get it from the surface sand and gravel, or from the new red sandstone hundreds of feet below. Any surface clay (supposing it not to be *made* ground; see subsoils memoir), being of no practical importance.

T. V. HOLMES.

It is clear, therefore, from Mr. Holmes' letter that he regards the clay as occurring in patches on the surface and affording no real protection from possible pollution from surface drainage. In reply to my further inquiries Mr. Nasmith informs me that the old condemned well was 18 feet deep, the useless boring is 480 feet deep, whilst the new well, the supply from which he informs me is still "very satisfactory," is 36 feet deep. Chelford is near Alderley Edge in Cheshire.

Shallow wells, certainly, often yield safe potable water, as is seen from the following letter which I received from the Rev. J. G. Geare in reply to my inquiries:—

Farnham Rectory, Bishops Stortford, *May 26th, 1897.*

In reply I beg to say that Mullins, junr., found on my rectory of three acres a splendid spring of good water with his divining twig on September

28th, 1893. He found water at less than 20 ft. depth. A boring was made and a well (brick) was sunk to 15 ft. depth. The water was analysed, and found good, and since then I have had a bountiful and unfailing supply through a rotary pump action.

I may add that Mullins was always correct as to depth to sink a well and the best place to bore. His was a first visit, and no test previous to his had been made.

J. G. GEARE, Rector.

Upon which Mr. Holmes remarks :—

Whether the water from surface beds is wholesome or not depends largely on the density of the population around any given well. Nobody can get water *absolutely pure*. But the poor geologist would hardly venture to recommend surface supplies, for finding which the dowser is hailed as a supernormally gifted benefactor.

T. V. H.

#### No. 7. *The Welwyn Case.*

The writer of the next letter is known to Mr. F. W. H. Myers, who kindly sent me the letter.

The Node, Welwyn, Herts, *October 30th, 1899.*

I sent for a water diviner last summer, as I wanted advice about the water supply here, our well not being satisfactory. When the diviner came he at once found out the direction of the spring supplying my well and advised me to make a heading in a certain direction which he marked, and which I afterwards carried out, 150 ft. below the surface. The well-sinkers found, before they had carried this heading 6 ft., the inrush of water was so great they had to stop, in spite of a steam engine which kept going night and day with powerful pumps.

After trying every device for keeping water out of an accumulator house, which always seemed to be rising, in spite of a concrete floor 6 in. thick, the same diviner told me exactly where a spring caused the trouble. We tapped it from the outside, and now the place is perfectly dry.

I held one side of the forked rod myself and the "diviner" the other, and when we came to water [alleged underground water] the strain was so great on my fingers I was obliged to ask him to stop. From the position of the rod it was absolutely impossible for him to produce the pressure, which increased with the strength of the stream.

MONTAGUE PRICE.

The last paragraph of this interesting letter I will refer to again when dealing with the motion of the rod. In reply to my inquiries as to the name of the dowser, the geological strata, and whether any surface indications or other wells existed to guide the dowser's choice, Mr. Price replies as follows :—

The Node, Welwyn, Herts, *November 7th, 1899.*

Anthony, of Huntingdon, was the "diviner" we employed here last June. Nobody knew where the spring came from supplying the well.



When the "diviner" arrived, being Saturday afternoon, the workpeople had gone and I certainly did not know where the water came from. I was with him all the time he was here, and he did not go down the well, which is over 150 ft. deep.

There is no well anywhere near the existing one, and, so far as I know, there was no surface indication to guide the diviner. In fact, he found the spring on adjoining property, 50 yards from the well. We drove the heading in the well (already existing) 150 ft. below the surface in pure chalk. Other springs he found on my property, but I had no occasion to try them.

MONTAGUE PRICE.

Mr. Holmes explains this case as follows :—

In this case the supply from a well in the chalk 150 ft. deep being unsatisfactory, the dowser simply recommended a heading. This is a common and well-known contrivance in such cases. Water in the chalk, the great water-bearing formation of S.E. England, circulates by the lines of bedding and jointing, especially the latter. Where the lines of jointing are feebly developed the supply of water from the chalk is usually feeble—the permanent saturation level being of course reached. The usual plan then is to drive a heading or headings in order to tap additional joint lines ; one direction being about as good as another.

T. V. H.

Mr. Holmes' comment on the geological aspect of this case is that the dowser simply advised doing what a skilled engineer or geologist would recommend. But it must be remembered on the other hand that the dowser probably knew nothing of geology or engineering ; I doubt if he even knew that the well was in the chalk, as he came from Huntingdon. Assuming, however, he knew all this, and was a geologist in the bargain, how did he find out the direction in which to make the heading which reached so large a supply "before 6 ft." had been driven ? The Richmond headings<sup>1</sup> were driven great distances in the chalk under the advice of the best geologists and engineers, and yet to a large extent failed to accomplish their purpose. Was it by a mere bit of luck that here, as in many other cases, the dowser succeeded ?

No. 8.

The next is a letter addressed to Mr. Stone, the dowser, from a Mr. Stevenitt.

Minting, Horncastle, Lincolnshire, *February 7th*, 1898.

I beg to inform you that I dug a well at the second place marked by you, viz., about 20 yds. from the stockyard in grass field, and found water. I dug a well 42 ft. deep, so that I should have a good supply for the yard. We have plenty for ourselves and several neighbours, who fetch it regularly

<sup>1</sup> See previous Report, Vol. XIII., p. 195.

for drinking purposes. I feel sorry I did not know of you sooner ; it would have saved me something like £200 in boring 290 ft., where I only found salt water at 135 ft. deep.

T. STEVENITT.

I wrote to Mr. Stevenitt, who confirms the above statement and tells me the useless bore is only 45 yards from the well. He was for twenty-five years Superintendent of Police in the County Constabulary. As regards this case Mr. Holmes remarks :—

In Lincolnshire the gratitude for good water at 42 ft. on the part of Mr. Stevenitt is most intelligible. May it never be polluted by soakings from the stockyard to any injurious degree.

T. V. H.

No. 9.

The next letter is from the Managing Director of the Bermaline Maltings, Haddington, where Mr. Gataker was the dowser employed.

We may state that, previous to our communicating with Mr. Gataker, we had decided to put down an artesian bore at a different part of our ground. On arrival Mr. Gataker started over the ground at a fair speed with the palms of his hands towards the earth. After proceeding some distance he was able to locate a spring at the end of our new maltings. He then proceeded over a strawberry field belonging to us, and at about 70 yards from where he located the first spring he located another. He guaranteed that from either of those springs we would get a supply of about 20,000 gallons per day at a depth of from 100 ft. to 150 ft. We put down a 4 in. bore at the first spring, and are pleased to say that at a depth of 102 ft. we are getting a supply of about 100,000 gallons per day, and the water is coming up with great force. We believe it will rise to about 40 ft. above the surface. We tested it at 13 ft., when it overflowed with a strong pressure.

We challenged the editor and readers of an Edinburgh evening newspaper, who seem to think that water was to be found in any part of our ground, to put down a similar bore half-way between the two springs located by Mr. Gataker in our ground, and if the same quantity of water was found at the same depth we would pay the expense of putting down the bore, and if not found, the party challenging was to pay the expense ; but up to now the challenge has not been taken up by any one.

It might interest your readers to know the strata the bore went through, and we have much pleasure in enclosing you a copy of the journal, from which you will see that it was after piercing 14 in. of the fourth layer of sandstone that the water was struck, and as soon as the crust of this layer was broken, the water came away with a rushing noise.

MONTGOMERIE AND COMPANY (LIMITED),

JOHN MONTGOMERIE,

Managing Director.

It is to be regretted this challenge was not accepted, as it would have been a very crucial test, though, from geological considerations, we are disposed to think the newspaper would have won. Mr. Montgomerie, however, in his letter to the Edinburgh newspaper, states his reasons for believing that water would not be found "at any part of the valley of the Tyne." He says:—

Bermaline Maltings, Haddington, *January 14th, 1899.*

During the excavations for the new bridge here, there was dug out a hole 40 ft. square by 13 ft. deep alongside our mill dam, with only about 3 ft. of an embankment between the water and the hole. We naturally expected that this hole would be filled up with water rising through the gravel bed; but to our surprise there was not a drop of water came up. Any water that found its way into the working came in at the north-east corner—that is, the corner furthest from the river.

There is a firm in East Lothian, not far from Haddington, who put down a bore without calling in the aid of a "water-finder." They have, I believe, sunk it to a depth of 660 ft., and, failing to find water, have had to abandon the bore.

I should be glad of any further information regarding the deep bore-hole here mentioned, as neighbouring artesian wells do not usually vary like this.

In reply to my inquiries Mr. Montgomerie states that his successful borehole is about 100 ft. above the sea level; he sends a "journal of the bore," showing the strata passed through. First, 20 ft. of sand and gravel, then 6 ft. of boulder clay, followed by 19 ft. of fire clay, after that 45 ft. of sandstone and what are called "faikes" in alternate layers, then 8 ft. of marl, and, finally, sandstone, the depth of the bore being nearly 103 ft.

#### No. 10. *The Aspley Heath (Bedford) Case.*

The next case is also one where a deep, unsuccessful bore had been made prior to the dowser's visit. The account here given was published in the *Midland Counties Herald* for April 7th, 1898.

A Bedfordshire gentleman, Mr. Plater, wanted water for his house, so got a firm to sink a well 60 ft. deep, this being the depth at which it was expected water would be found. However, none was found, and then sinking was continued to 100 ft. deep, but still without success. A bore was then put down at the bottom of this well a further 66 ft. deep, making a total depth of 166 ft., but still no water. At Mr. Plater's request a water expert visited the place, and said it was useless to continue working at that spot, for no water would be found; but he felt absolutely confident that by sinking a well at a spot only 39 ft. from there, there ought to be a plentiful supply of water at about 100 ft. or so. Mr. Plater acted on his advice, and at 116 ft. an abundant supply was found. The old well, although so close, is said to be still as dry as a bone.



I wrote to Mr. Plater, enclosing the above report, and obtained the following reply :—

Silver Birch, Aspley Heath, Woburn Sands, R.S.O.,

*October 16th, 1899.*

I return the press cutting which was enclosed with your letter to me of the 9th inst. It is correct. In reply to your three questions :—

1. As stated in the newspaper cutting, an unsuccessful attempt was made to a depth of 166 ft., at which we had bored several feet into Oxford clay.

2. No further useless wells or borings were made near the successful well.

3. There is an ample supply of water obtained from the well sunk on the spot indicated by Mr. Gataker.

I may state that my property at Aspley Heath, Bedfordshire, where the wells were sunk in 1896, is about 500 ft. above sea level.

Mr. Gataker stated that water would be found on the spot indicated, but not under 100 ft. from the surface.

ARTHUR C. PLATER.

Mr. Holmes remarks : “The boring in Oxford clay was unsuccessful, and Mr. Gataker was successful. The interesting point is the statement in the newspaper cutting that the useless well was only 39 ft. from the good one. Mr. Plater, however, does not seem to confirm this statement.—T. V. H.”

I wrote again to Mr. Plater with regard to this latter point and also to inquire whether he was present when the dowser visited the spot, and whether he could tell me the strata bored through in his new well. Mr. Plater replies as follows :—

Silver Birch, Aspley Heath, Woburn Sands, R.S.O.,

*December 14th, 1899.*

In reply to your letter the distance between the two wells, about 39 ft., is quite correct. The new well is 116 ft. deep. I was present when Mr. L. Gataker expressed his opinion that water would be found at a depth of about 100 ft. I have lost my record of the strata through which we bored in the old well, but to a depth of about 90 ft. sand, with occasional layers of ironstone, formed the principal feature ; at about 130 ft. we bored through 6 ft. of fuller's earth, and after more sand we bored into the clay several feet, and then ceased work. I think the water in the new well is on a bed of fuller's earth. The level of the mouth of the new well is about 14 ft. lower than that of the old. The latter is now used as a rainwater tank.

ARTHUR C. PLATER.

Though I am not a geologist, the explanation of this case appears to me simple. The bed of fuller's earth found at a depth of 130 ft. in the old well evidently forms the bottom of the new well, which is

116 ft. deep and 14 ft. lower level = 130 ft. As fuller's earth (a silicious clay) is a nearly impermeable water bed, when this was pierced, as in the old well, the water would escape into the sand below the bed. If the record of the old boring was shown to Mr Gataker, or known to him, the explanation of his successful prediction is also easy ; though I have no doubt it would be rejected by all concerned, as dowzers trust more to themselves than to any information given them.

It will be seen from the notes which Mr. Holmes and Mr. Westlake have so kindly added to many of these cases, that a geological explanation of *why* the dowser found water in the places he indicated is clear enough. The important point is, would these places have been selected by a shrewd observer, or even by a skilled geologist, *before* the dowser had visited the spot? In some cases this might, as in No. 10, have been so ; on the other hand a considerable body of evidence exists to show that the dowser, though ignorant of geology and an entire stranger to the locality, *succeeded*, where expert geological or engineering advice, aided by careful local observation, completely *failed*. Those who have taken the trouble to read the previous Report will recall several cases of this kind, notably the Waterford case on p. 106 *et seq.*, the Horsham case, p. 117 *et seq.*, the Uppingham case, p. 127 *et seq.*, Sir Welby Gregory's case, p. 96, etc.

Then again, if merely shrewd observation and considerable experience as well-sinkers afford a sufficient explanation of the dowser's success, how are we to explain the achievements of young dowzers and other complete novices? Take the case of the French charity boy Bleton recorded in Part X, p. 257, or, referring to the previous Report, take the cases of the eleven-year-old Cornish lad given on p. 36, Vol. XIII., or the young daughter of an estate agent given on p. 33 *et seq.*, or the daughter of an English clergyman on p. 38, or the young son of another clergyman on p. 39, etc., etc. Then, too, we have other amateur dowzers of all ages and in all ranks of life, whose successes in the discovery of underground water certainly appear at first sight to be greater than mere observation or good luck would account for. Numerous instances of this were given in the previous Report, and a few additional cases of successful amateur dowzers, such as his Honour Sir Richard Harington, Bart., are quoted later on.

One of the four methods suggested by me to test the pretensions of the dowser (see p. 251 of last Report) is "separate and entirely independent examination of the same ground by different dowzers." The evidence on behalf of this test I will now proceed to cite.

## PART IV.

EXPERIMENTS WITH TWO OR MORE DOWSERS INDEPENDENTLY TRIED  
ON THE SAME GROUND.No. 1. *The Cheltenham Experiments.*

I am indebted to our fellow-worker, Lieut.-Colonel Le M. Taylor, of Cheltenham, for the following interesting report of his own and other experiments at Cheltenham. It appears that in 1896 the Directors of the Cheltenham Steam Laundry, anxious to obtain a supply of water for the laundry, at Colonel Taylor's suggestion and by arrangement with him, employed two dowsers, independently of each other, to locate the best site whereon to sink a well. Colonel Taylor arranged first to take Mr. Tompkins to the laundry, and after he had fixed on the place or places where water would be found, the laundry directors were to employ a second dowser and the results were to be compared. The following is an abridged account of the proceedings given in a local newspaper:—

A “diviner” went over the ground a week or so ago in company with several of the directors. He subsequently wrote a report which, according to an arrangement unknown to the diviner himself, the directors sealed and kept strictly secret. As business folk, the directors were not going to trust to the decision of one expert, and, therefore, another was engaged to go over the ground on Wednesday in company with other directors, who did not know what the result of the former experiment was, in order that there might be no unconscious collusion or “thought reading.” Mr. Chesterman, of Bath, the chief huntsman on this occasion, seemed to think that his task was none too easy, as it is not always that a reliable supply of water can be found in a small field of clay soil, such as that at the side of the laundry to which his prospecting was restricted. Taking one of his small, slender twigs, he held it in front of him with his arms lowered and one end of it in each hand, so that the angle of the fork pointed very slightly downwards, about three feet from the ground. Thus he commenced to walk slowly in a straight line across the field. Suddenly the twig gave a turn in the operator's hands, began to revolve, and continued to do so while he remained within the area in which he experienced the “shock” of water. The two directors attempted to stay the revolving motion of the twig while the operator carried it over the affected area, but though each seized one end of it, they could not do so, the ends of the twig, in fact, slightly lacerating their fingers as it turned in resistance to all the pressure they could employ. Similar results were obtained with the wire. Then Dr. Cardew walked across the affected spot with the twig in his hands, but the simple apparatus remained quite stationary until Mr. Chesterman placed his hands on the doctor's wrists, when it began to revolve.—*Cheltenham Echo*, December 10th, 1896.



How far the sites fixed on by the two dowzers coincided will be seen from Colonel Taylor's report and drawings, which I now append.

#### LIEUT.-COLONEL TAYLOR'S REPORT.

On December 8th, 1896, I was informed that Mr. Chesterman, of Bath, had been engaged by the Directors of the Hatherley Laundry to "douse" for water on their property, and that he was to arrive the next day. I wrote him a note and asked him to call on me when he had finished, which he kindly did. On his arrival I showed him my plan of the laundry ground and asked him to point out where he had found water. The dotted lines on the plan, Fig. 13, show the places indicated by him, and these agree also with the information I subsequently got from Mr. Wilkins, one of the directors, who accompanied Mr. Chesterman during the experiment. Mr. Wilkins also told me that Chesterman had found indications of water about the place I have marked E, but as it was not a strong stream, no more was done about

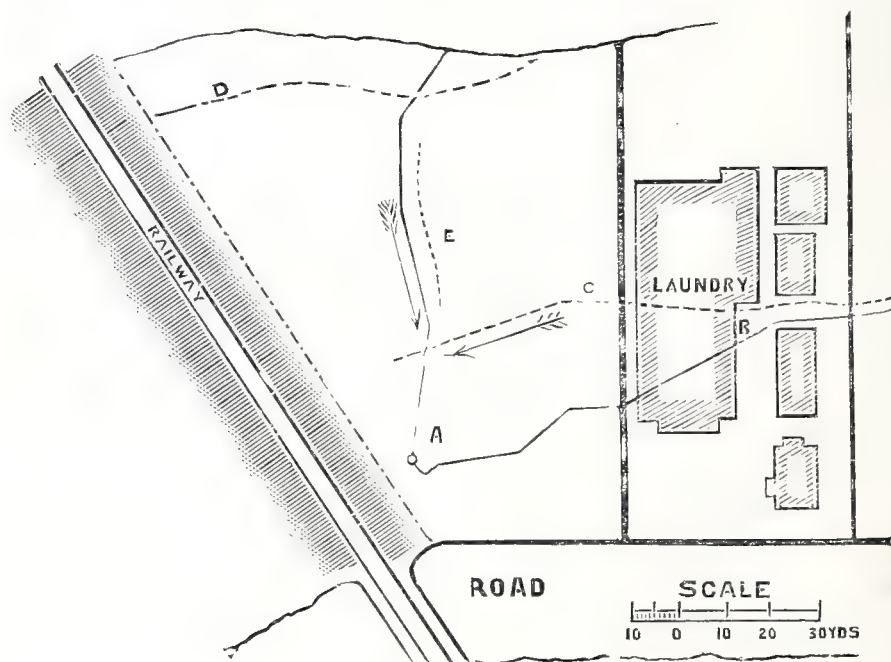


FIG. 13.

Rough plan of Hatherley Laundry, where water-finding experiments were tried, November and December, 1896.

Tompkins on November 25th traced the streams marked in continuous lines and recommended Well at A, estimated at 80 ft. to yield 3,000 gals. in 24 hours, and also one at B, but not so strongly.

Chesterman on December 9th traced streams marked in dotted lines and recommended Well at B, 108 ft., to yield 3,600 gals. in 24 hours, or Well at C, 90 ft., to yield the same quantity, or one at D, 85, to yield, however, less.

it. Mr. Chesterman has been for years in the Indian D.P.W., and therefore quite understands a plan. No one who was at the former experiment at Hatherley was present on December 9th, and Mr. Wilkins did not open the envelope containing the record of what Mr. Tompkins had done till the trial was over. Mr. Chesterman was not told that another man had gone over the ground, though, as he told me, he suspected it after he had completed his search from being asked what he would say if another finder had indicated other places.

All parts of the field near the laundry seem equally open to their choice, but it might be said that a desire to make things pleasant for the Laundry Company may have influenced each of them to locate water near the engine house, well knowing that water would most likely be found everywhere.<sup>1</sup>

Previous to Mr. Chesterman's visit Mr. Tompkins had been to my place to conduct the experiments on finding coins already sent you,<sup>2</sup>

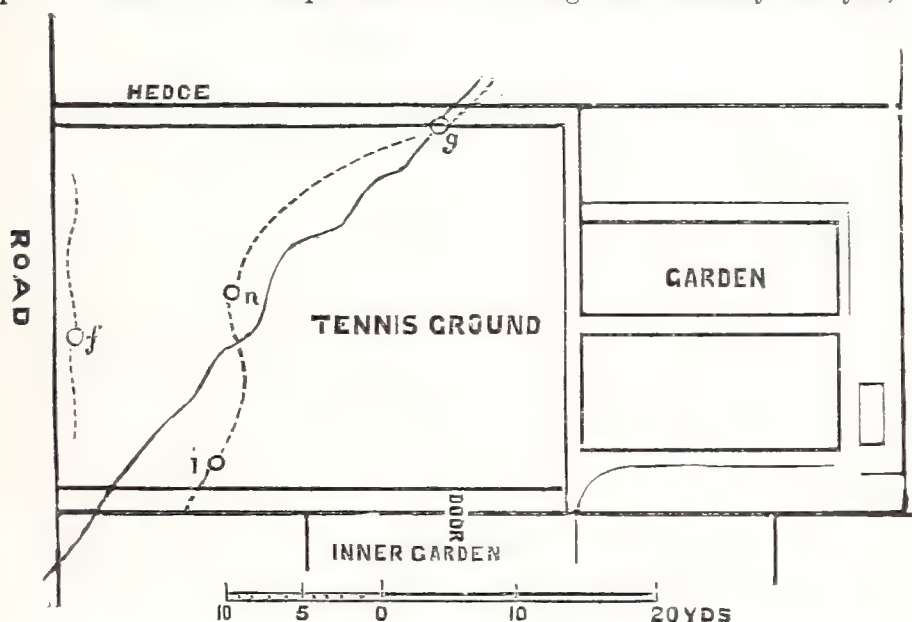


FIG. 14.

Rough plan of Garden behind Colonel Taylor's house.

The continuous line indicates the stream found by Mr. Tompkins, November 23rd ; the dotted line those found by Mr. Chesterman, December 9th.

<sup>1</sup> Colonel Taylor sends a geological map of a section of the district, taken from Murchison's *Geology of Cheltenham*. Sand occurs in patches up to 30 feet in depth, over the lias formation, which latter is probably some 300 or 400 feet thick at Cheltenham ; shallow wells sunk in the sand would yield surface water, and wells sunk 70 to 130 feet about Cheltenham frequently yield water, which, however, is more or less saline. But the laundry experiment is, if anything, adverse to the dowser. —W. F. B.

<sup>2</sup> These experiments will be given in a subsequent paper. —W. F. B.

and I had also asked Mr. Tompkins to indicate any place in my garden where underground water would be found. He traced the line of an underground stream and I marked it with pegs (about four inches long) driven in quite to their head; one spot *g*, Fig. 14, he specially noted.

I told Mr. Chesterman about my experiments with Mr. Tompkins and asked him if he would go into my garden and try to locate a stream found by the latter. He kindly consented. It was quite dark when we went out, but it made no difference; in a short time he found water at *f*, Fig. 14, and said the stream ran parallel to the road. He then, passing up by the hedge, found the rod turn at the point *g*, when he said he was crossing a stream. I put down my hand to mark the place with a peg when it came into contact with the end of the peg I had previously put in to mark the spot Tompkins had selected. It was much too dark for either of us to see the pegs even if we had searched for them. Chesterman then walked down the centre and afterwards the inner edge of the tennis ground and crossed the stream at *n* and *i* respectively. The dotted line on the plan of my garden will fairly represent the flow of water underground as indicated by Mr. Chesterman.

How far, from these experiments, it may be considered that the subterranean streams influenced these two men I cannot say; I think, however, something more than chance must have dictated their choice of ground.

Mr. Tompkins follows his stream when he has once struck it, the "wand" pointing the way when the stream takes a turn, and he estimates the distance underground of the water and the yield by the position assumed by the rod and his feelings generally.

When Mr. Chesterman discovers a stream he does not attempt to follow it, but steps back, and then again approaching it at right angles marks the spot where he first feels the indication of water. He then goes some paces forward so as to cross the stream, turns round, and again approaches it, marking the spot where the water is again first felt. By dividing the distance between his two marks he gets a point directly over the water, and by a scale, which he has worked out from experience, he judges the distance below at which the water is flowing by the distance between one of the points where he first feels the water and the point immediately over it.

For example, when the horizontal distance is 15 ft. the water will be 20 ft. below, and when the distance on the ground is 5 ft. 3 in. the stream should be 85 ft under the surface.<sup>1</sup>

<sup>1</sup> Colonel Taylor here gives a plan of Chesterman's method, but it is hardly necessary to reproduce it, as it is purely imaginary and exactly opposite to the system described by Mr. Emerson in the *American Journal of Science* and followed by some dowsers in England.—W.F.B.



Every dowser, however, uses the method which he feels answers best; a water-finder whom I saw at work two or three years ago found the distance of the water underground by raising his rod above his head and then lowering it slowly till he felt "first contact" of influence and then he calculated by a scale he had made.

December 13th, 1896.

LE M. TAYLOR.

On inquiry I learn from Colonel Taylor that the directors of the laundry have not sunk a well, after all, at the place indicated by the dowsers, as they feared, Colonel Taylor says, "that the water, when got, might turn out to be 'Monkey Brand.'"

### No. 2. *The Westbury-sub-Mendip Experiments.*

Among the numerous letters and evidence of various kinds relating to the divining rod, which Mr. Vaughan Jenkins (to whose painstaking zeal this inquiry owes so much) has kindly sent me, there is a good deal of correspondence relating to a dowser named Thomas Pavey, of Cheddar. I will only refer to one case where the late Mr. Nalder, of Westbury-sub-Mendip, Wells, Somersetshire, employed Pavey and another dowser quite independently of each other to go over his ground at different times. I condense what Mr. Nalder says in a couple of letters to Mr. V. Jenkins written so far back as October and November, 1882:—

"I got the two men [Mr. Nalder writes] at a month's interval apart—quite unknown to each other or of what the other had done—to test my field for water, and though there was nothing to guide them, the rod moved within two yards of the same spot in each case; moreover they each traced the same direction as the line of the underground watercourse. In other parts of the field the twig would not work (or only very slightly), but it moved strongly where they asserted the 'spring' existed. I had a well sunk at this place and came upon a splendid water supply at 39 ft. Below the surface soil we passed through a bed of gravel, 2 to 2½ ft. deep, then came upon a *very hard* dry substance called by the well sinker 'wark.' The water rose in the well up to within 2 ft. of the top and remains so. The well-sinker told me he had sunk five wells at different places where Mr. Pavey found the twig moved strongly, and in each of the cases he had found abundance of water."

In a subsequent letter Mr. Nalder writes:—

Westbury-sub-Mendip, Wells, Somerset, *November 21st, 1882.*

I was at Cheddar this morning speaking to my butcher, Mr. John Branch, and he reminded me of a circumstance I had quite forgotten. It seems he came over to Westbury to see me shortly after Mr. Pavey had

marked out the spot for my well, and as Mr. Branch said he could "work the twig," he cut a forked twig from the hedge, and went to the *same spot marked out by Mr. Pavey*, and of which there was not the least sign whatever, as I had not begun the well. So this made *three* persons who had found the spring by the twig.

FRANK J. NALDER.

There was some correspondence on this case in the *London Times* during October, 1882. The former owner of Mr. Nalder's place pooh-poohed the thing and said the dowzers were only joking, to which Mr. Nalder properly says: "If so, it was a capital joke for me."

### No. 3. *The Thomastown Experiments.*

It has been already mentioned in the Carrigoona experiments that a friend living near Waterford, Mr. J. H. Jones, was an amateur dowser. Mr. Jones tells me that, having seen Mullins locate the site for a well in his neighbourhood, which turned out remarkably successful, he was astonished to find the rod also moved in his own hands and he writes to me as follows:—

Mullinabro, Waterford, *November 29th, 1897.*

I began experimenting with the rod as a sceptic—and thoroughly prejudiced against it, and thinking that its action in a diviner's hands was a mere trick or sleight of hand—but I am now convinced that the thing is genuine, and that the rod is moved in consequence of some action or influence produced in persons susceptible when near or over subterranean water.

J. H. JONES.

It will be remembered that Mr. Jones, in our Carrigoona experiments, independently pointed out the same places that Mr. Stone indicated as water-bearing or waterless. Mr. Jones has had several experiences of this kind, and a recent one is worth referring to. Water was much needed for a new convent, in Thomastown, co. Kilkenny, and, after some unsuccessful attempts to obtain a supply had been made, an English dowser was sent for; he pointed out a certain region in the convent grounds where a supply of underground water existed, estimating the depth at about 80 feet; an unlikely place it seemed, as the convent field was at some altitude. Mr. Jones was asked to try his hand independently, knowing nothing of the place pointed out by the English dowser and having nothing to guide him. The result was that he fixed on the same place, and I learn that any possible telepathic influence was also out of the question. Accordingly a well six feet in diameter was sunk at the spot indicated as best by both dowzers. After passing through a layer of surface soil and rocky débris, the solid rock was encountered at the depth of 15 ft. from the surface, and (at a cost of several hundred pounds)

the well was sunk in a hard micaceous quartzite. The Administrator of the parish writes as follows to Mr. Jones :—

Thomastown, Co. Kilkenny, *September 21st, 1899.*

I am happy to say that we have got water on the exact spot pointed out by you and Mr. Wills [the English dowser]. We are now 70 ft. deep, and since we reached 60 ft. water has come—so far, however, in small quantities insufficient for our needs; we are therefore continuing the boring. The well-sinkers declare they never encountered a harder stone. It is getting soft at one side, and sand is coming up with the water, a good sign. All the springs are very low now, and we expect a good increase of water during the winter.

JOHN ROE, Adm.

P.S.—I should mention the water is increasing every day for the past week.

In reply to my inquiry the Rev. J. Roe writes :—

Thomastown, co. Kilkenny, *December 8th, 1899.*

In your letter of the 9th ult., you have asked me three questions : 1st, “Is water difficult to find in Thomastown, etc.?” I may state that water is not very difficult to find here, but as the convent is situated in a rather elevated position, we anticipated considerable difficulty in finding water. The site of the house is really charming, but the great drawback was water; in fact, some wise heads told us we should never get it. However, we were determined to try our luck, and as the tradesmen were putting the finishing touches on the building, we began sinking operations. The spot selected at first was about 12 yds. from the kitchen door. The men worked on for about 15 ft. when we came upon the rock. We were very doubtful at first about our success, and having come upon rock so soon we nearly lost courage, and dreaded the great expense necessary even in trying to find it. I consulted Mr. Hynes, architect, Cork, and he advised me to bring over Mr. Gataker or his man. At first I laughed at the idea, and though I had heard and read a little about the divining rod, I was most sceptical as to its results. After some persuasion I communicated with Mr. Gataker and he sent over his partner, Mr. Wills, at a cost of of £10. When he arrived he looked at the site already selected, and after some evolutions of his rod said he should *abandon* it, as there was only a very, very small ripple, and at great depth. He then went through the whole field with his rod and marked out two or three places where an abundant supply of water could be obtained, but selected a rather elevated spot in preference to the others. He said we would most certainly get water at about 80 ft. and so many gallons per hour. By a most singular coincidence Mr. Jones came on the scene accompanied by a mutual friend, Mr. O'Connell (engineer), Kilkenny, and this brings me to your second question. We invited Mr. Jones to have a try with his rod, and he pointed out the *exact spot*, and traced out the *water in the same line* as Mr. Wills had done. They were quite *independent* of one another, and Mr. Jones knew nothing about the coming of the English dowser, or what he had already done. Mr. Jones could not tell the depth, nor the quantity, etc., but he was certain there was a strong current of water. I noticed in the operations when they came over the places where water could be found, Mr. Wills' rod *jumped up* and Mr. Jones's *down*.



Third question. We have now gone down about 75 ft. and the men have ceased to sink any deeper, as we consider we have got a sufficient supply of water for the convent. To-day the pumping apparatus, etc., has arrived from some English firm, and I expect it will be in working order by the first of next month. I need not tell you how difficult it was to bore through the rock and how expensive it has been upon us. The architect was anxious that we should go deeper, but the men found it very difficult to carry on the blasting operations, because of the constant flow of water. I must ask you to excuse me for the long delay in writing this, but I have been very busy. I may add that I am now convinced that the divining rod is no sham, but genuine, and I cannot explain its influence on some susceptible people when they come over or near water.

JOHN ROE, Adm.

In a concluding letter, Father Roe, in reply to my question why he did not in the first instance consult a geologist, writes as follows :—

Thomastown, co. Kilkenny, *December 11th, 1899.*

It would have been wiser to have consulted a geologist before beginning the sinking operations, but I was obliged to act very economically. I did not mean by "boring" a small borehole, for the well is 6 ft. in diameter at least from top to bottom; I only used the word as a pumping phrase. I did not intend that my letters should be published, as they are not very fit, but if you think they can be of any service to you in your valuable researches, you have my permission to use them.

JOHN ROE, Adm.

#### No. 4. *The Claverton Manor Experiments.*

In the previous Report (p. 95) a case is quoted from the *Proceedings of the Bath Field Club* where two dowzers independently fixed on the same spots in an estate belonging to the Deputy-Lieutenant of Gloucestershire, Mr. H. D. Shrine, J.P. As one of these dowzers was an amateur, and an Archdeacon, the case is above all suspicion of collusion, and through the kindness of Mr. Vaughan Jenkins I obtained the following fuller account of the experiment contained in a letter written to Mr. V. Jenkins by Mr. Shrine, who was an eye-witness. The date is somewhat remote, but the facts are not disputed.

Claverton Manor, near Bath, *December 20th, 1882.*

In reply to your questions relating to my experience of the use of the divining rod for the discovery of water, the facts are these :—About twenty years ago, intending to build a cottage residence on the top of Warleigh Hill, adjoining the hamlet of Conkwell, I employed a man named John Mullins, a reputed spring-finder and well-sinker living at Colerne, Wilts., to find water, which he did by means of a forked twig of hazel or thorn. He was, so far as I know, an entire stranger to the place, and could not have known anything about the springs. I took him to the top of the wood in a flat field, the subsoil of which for a great depth was known to be oolite rock. He pointed

out by means of his forked twig where water was, in his opinion, to be found under the surface, but at what depth he did not pretend to say. In every instance where he said he found water, there was to my knowledge a spring low down in the wood in a line with the spot indicated.

He afterwards made some attempts to find water on the lawn in the front of the house, and traced a spring upward to a spot on the grass plot in front of the greenhouse where, some years before, in moving a large arbutus tree we had found a spring of water. To test the man's water-finding powers more closely, before I allowed him to sink the well, I invited my friend and neighbour Mr. Earle—late Rector of Monkton Farleigh, and now Archdeacon of Totnes, who had the gift himself, and who had found several wells by this process—to meet Mullins in the field near Conkwell. He did so, and though unaware of what places Mullins had selected, they both agreed in a very remarkable manner in the spots where the hidden springs were; the forked twig turned with him at the same places as with Mullins. On this conjunction of their discoveries I decided to sink a well, and at a little over 80 ft. deep we came to a bed of yellow clay and found water. The bed was, however, too thin, and the men sank the well to over 100 ft. deep, when they came to a blue marl or lias, and we have a fair supply of water about 4 ft. deep.

Since then and quite recently Mullins has found for me several springs in Claverton and sunk two wells, in one of which the water was found not more than 6 ft. below the surface, where we had no idea there was any. In each instance of his finding by the twig a spring on the hill, he traced it down to an existing spring unknown previously to myself. He also found for us the course of several drains, the lines of which had been forgotten by the workpeople, and in every case was right and saved us much trouble in digging.

It certainly appears to me that Mullins is himself a believer in the divining rod, and if it is a mere trick or deception, it is strange that he could not communicate it to his own son who works with him. The twig on the approach to a spring curved upwards in his hands without the least appearance of action on his part.

HENRY D. SHRINE.

#### No. 5. *The Pontyberim Experiments.*

Another case where two dowzers, in this case both were amateurs, independently fixed on the same spot is given me by Mr. J. F. Young, of Llanelly. A friend of Mr. Young, a contractor, was building some cottages at Pontyberim, a mining village noted for its anthracite coal, eight miles from Llanelly. Wishing to select a site for a well, the contractor asked Mr. Young (who is well known in the locality as an amateur dowser) to come over and fix the best spot. Mr. Young tells me he had never been to the place before, and on arrival, after casting about with his twig, he fixed on a certain line along which he asserted underground water would be found. Having done this, he begged the contractor, Mr. Williams, to try the rod himself. This he did, and

to his surprise Mr. Williams found the rod moved vigorously, and apparently spontaneously at the same place found by Mr. Young. Then Mr. Young went out of sight of Mr. Williams, and having traced the course of the water-bearing fissure, as Mr. Young believed it to be, he returned and asked Mr. Williams to do the same. Aware of the influence of unconscious guidance by hand or look, Mr. Young informs me that he took special care to avoid this, and kept out of sight whilst Mr. Williams tried. On subsequently comparing notes, the same line was found to have been traced by both. Mr. Williams' sister, Mrs. Rees, to whom the cottages belong, was present, and adds her testimony. I have received independent reports from both Mr. Young and Mr. Williams. Here is the account given by the latter; after relating what I have stated above, Mr. Williams says:—

Pontyberim, *October 20th, 1899.*

Mr. Young then asked me to stop where I was, while he went to the side of the building in order to trace the course of the underground water. In a short time he returned and begged me to try if I could find it. I did so, and had not gone far before the curious "electrified" sensation again returned, and the action of the twig could be seen moving over a certain spot, which proved to be the same as that located by Mr. Young.

But still I was in doubt as to the probability of the existence of water there until a couple months after that, and in the worst of the last dry season, I resolved to put it to the test, and commenced sinking a well at that spot. On reaching a depth of 14ft. from the surface, I had the gratification of striking on a strong spring of clear and beautiful water, rising since to 5ft. and 6ft. in the well and so it now continues, a result of much importance to my sister, Mrs. Rees.

JOHN WILLIAMS.

I have read the above account of the finding of the spring on my premises, and the account is true in every particular.

MARTHA REES.

#### No. 6. *The Mayfield Experiments.*

I am indebted to Dr. Leadam for kindly sending me the following detailed account, accompanied with a map, of his experiments with two dowsers, whom he tested, independently of each other, on his estate in Sussex. Mayfield and Stone Mill are both on the "Hastings Beds," a series of sands and sandstones alternating with clays; see Topley's memoir on the *Geology of the Weald*.

167, Gloucester Terrace, Hyde Park, W., *December 7th, 1899.*

I have a small property near Mayfield, in Sussex, a hilly district principally soft sandstone formation with hard sandstone strata, as at Tunbridge Wells.

In 1895, as I wanted a further water supply, I sent for Mr. Gataker; he stated he had never been in the place before, and I accompanied him



throughout the experiment. As my primary object was to have an increased convenient supply for the farm buildings adjoining Stone Mill, I took him over there, and he soon marked a spot, C, as promising a good supply at 70 ft. depth. He then indicated spots D and E, giving from 50 to 70 ft. as the probable depth; a smaller but satisfactory supply at F, but, as he remarked, it was altogether out of the way of the farm.

As I also wanted a supply for the small house at the Stock-yards, I then took him round there, and he pitched upon A as a spot for a good supply at 80 ft. I then told him that I believed there must be water somewhere near the surface, as an underground dairy could not be kept satisfactorily dry, so he walked round the house, and outside the garden gate, about 30 yds. from the house, he marked a place, B, where he said we should find a sufficient supply for that small house at 12 ft. from the surface. I may say that the pond drawn near there is a surface pond and nearly dry in summer.

I determined to try first at B, and set an ordinary well sinker to dig a well; exactly at 12 ft. he came upon water in a stratum of blue marl. I had this dug down to 20 ft., brought the pipe for a house pump into it, and it has been in constant use ever since, and stood steadily at about 12 ft. from the surface during the late dry summer; though neighbours were allowed to use the well, the water did not sink more than 1 or  $1\frac{1}{2}$  ft.

Being well satisfied with this, I then set the same men to dig a well at E, and, after digging through varying strata, the man below, whilst digging in rather firm rock at 61 ft., suddenly called out to be hauled up as the water rushed up in volume at his feet. This flow was estimated at 300 gallons per hour, but there is a stratum of broken rock and rubbish about  $1\frac{1}{2}$  ft. above, through which the water flows away; for that reason and its distance from the house, and a suspicion that the broken stratum in some way rendered the water unsatisfactory, I have not taken it into use, and my farm people have continued till now to supply themselves from the small stream shown in the map.

Having determined this summer that I would dig or bore somewhere nearer the farmhouse, I thought, before doing so, that I would have another diviner over to see, as a matter of interest, to what extent his opinions agreed with Mr. Gataker's. You were kind enough, in answer to my inquiry, to recommend Mr. Stone, and he came in August. He also had never been in the place before, and I met him on arrival and accompanied him round.

I took him first to the field near some ponds on the map; the ponds were certainly hidden by trees, and are in a dip in the ground so that Mr. Stone could not have noticed them. He marked a spot at G, then at H, and traced that flow back towards G, all at about 70 ft. depth. Also at I, and at K, and at L, a large flow, "bubbling up," as he expressed it, at about 50 ft. depth, and flowing in both directions from there. L is about the highest point in the field ( $\Delta$  having been an ordnance survey station). He then marked M exactly where Mr. Gataker had marked C four years before, and N, O and P, which he considered one stream at about 60 or 70 ft. depth or rather less, and when I afterwards told him of Mr. Gataker's markings he said he thought his own places better, and that they were branches of the same supply. Similarly he marked Q and R, which appeared

to be the stream which Gataker had marked at F on its way to the meadow at the bottom, and I was already aware that somewhere about there a good supply was coming down to the stream and percolating into it.

Mr. Stone recommended boring at P, and I accordingly allowed him to send his foreman and boring tools to do it. They went through 5 or 6 ft. of mould, then about 30 ft. of sand rock, unfortunately a very hard seam, then about 3 ft. of blue clay. At 42 ft. 6 in. they again got into sand rock, and in that at 47 ft. reached water, which rose about 6 in. up the bore hole. It remains at about that level and is estimated at 200 gallons per hour, but I have not yet got a pump fitted. Before writing to me Mr. Stone advised that the boring should be carried deeper (I think there was no justification

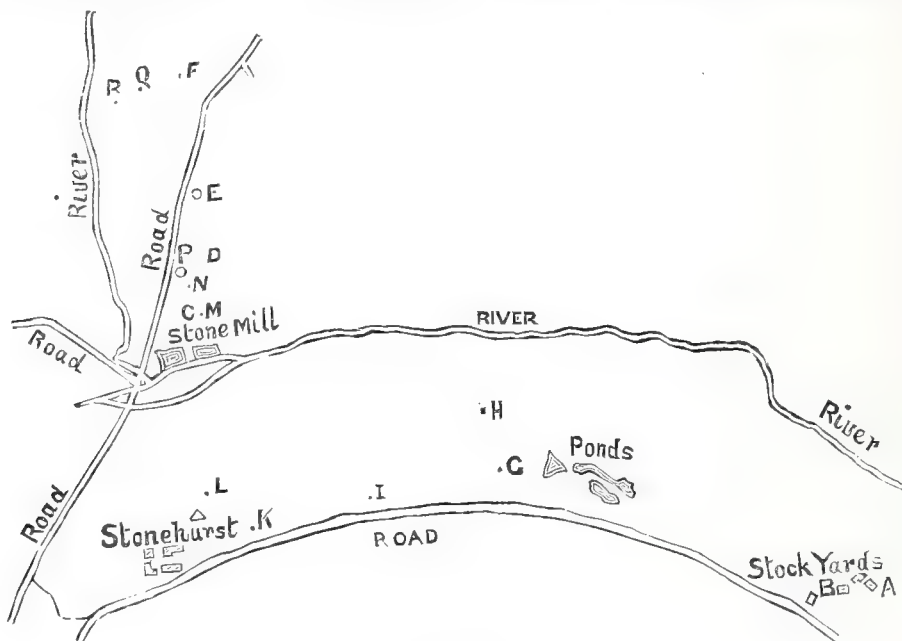


FIG. 15.

A to F positions marked by Mr. Gataker.

G to R " " " " Stone.

Wells sunk at B, E and P.

Scale—About 1 in. to 850 ft., approx. 6 in. to the mile.

for this, as I considered it simple speculation and stopped it), his view being that by going deeper we should strike a stronger spring and perhaps have the water fill the bore hole to the surface. The boring showed that the water-bearing rock (or sandstone) reached to 58 ft. 6 in., then blue clay to 61 ft., rock to 65 ft., and blue clay again to 69 ft. We therefore have, in my opinion, run a great risk of losing the water altogether. The layer of clay from 40 ft. to 42 ft. 6 in. was peculiar, almost black, and I am sending you by parcel post a sample of it, and if there is any peculiarity about it, I should like to know.

I will report later to you the working of the supply when it is fixed.

W. W. LEADAM.

P.S.—With two streams of water on the map you may perhaps wonder at the need for boring, so I must explain that in the summer time the mill stream is so small that it takes three days to refill the millponds after use, and the water is very irony. The other stream is only just enough in summer to supply my house by a hydraulic ram. It is good water, and appears in summer entirely supplied by percolation from the water stratum indicated by the diviner at F, Q and R.

The foregoing case is instructive, not from the coincidence of the spots C and M chosen by Gataker and Stone independently, a coincidence that was possibly accidental, but from the illustration it affords of the dowser's notion of underground water. With the utmost assurance, wherever he goes (if his rod turns) he proceeds to trace out narrow streams of water flowing underground, just as streams flow on the surface. Now in the foregoing case the geology of the district is perfectly well known and underground streams, such as are imagined by the dowser, are out of the question.

Mr. Holmes makes the following remarks on the foregoing case:—

Mayfield and Stone Mill are not on the same subdivision of the Hastings Beds, but they agree in being both on sandy, not clayey beds. Topley gives the details of a quarry showing 36 ft. of stone “at the curve of the main road half a mile West of Mayfield.” (p. 77.) He also mentions some fine natural exposures of sandstone rocks (between Mayfield and Stone Mill) as “continued down the valley towards Stone Mill.” Thus the dowser thereabouts would have not only the evidence of hard rock interbedded with clay or other soft stuff, deducible from the contours of the ground, but the actual sight of the sandstone. I note that Dr. Leadam told the dowser that there must be water somewhere near the surface (par. 3), as an underground dairy could not be kept dry, near B, and that then the dowser decided that there was water, at B, 12 ft. from the surface—a very simple deduction therefrom.

Granting the dowser's honest faith in himself and his rod, it is easy to understand how he might say to himself when in a farm yard, or other enclosure near a house,—“That would be a convenient spot for a pump or well, if there is a stream of water there.” Any other dowser would probably take the same view and hence both would try that spot in the first place and give it an unconscious preference.

T. V. H.

Other cases of two or more dowsers tried on the same ground independently of each other are given both in the present Report (see, *e.g.*, p. 203); and in the previous Report eight or nine such cases were referred to and some particulars given, see Vol. XIII., pp. 32, 40, 47, 70, 81, 89, 95, and 210. All of these were successful, but in many of these cases the exact conditions of the test or of the ground are insufficiently described. There is, therefore, a considerable body of evidence of a certain value, but of what value it is hard to say, existing upon this instructive point.



## PART V.

EXPERIMENTS IN BLINDFOLDING THE DOWSER.

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In the previous Report, pages 175 and 205, I described some experiments made by myself on carefully blindfolding two dowsers and noting whether they were able, by the movement of the rod, to indicate the same spots when blindfolded as when not blindfolded. If there be, as all dowsers and most of their supporters believe, a subtle influence exerted by underground running water upon the dowser, then blindfolding should make no difference. The same remark applies if the dowser be psychically sensitive, possessing, *e.g.*, some kind of clairvoyance,—provided always the dowser is at his ease, *i.e.*, if he joins heartily in the experiment.

In all experimental work it is essential for success that the instruments you employ are suitable for the purpose in view and also in good order. Having selected the proper instruments, the mental atmosphere in *physical* experiments does not affect the results, as we are dealing with non-living matter, but the physical conditions are all important. In experiments with living beings, the physical surroundings (except in so far as they produce discomfort) are unimportant, but, as we are now dealing with living instruments, our experiments are sure to fail or be inconclusive if the instrumental appliances we have to employ are out of order; and they are very apt to be deranged by a sudden change in the psychical conditions. Modern physical and chemical research has taught us the profound influence exerted by an imperceptible difference in the materials we are dealing with; the tendency of all psychical research is to reveal that an influence quite as profound is exerted by an imperceptible difference in the mental apparatus we employ. Though this may seem quite reasonable and obvious to most of us, it has taken physicists 200 years to learn the former truth, and we should not therefore be surprised if those to whom psychical research is new overlook, or even ridicule, the latter.

It may be remembered that some years ago Professor Ray Lankester, F.R.S., published an account of some experiments he had made in blindfolding a dowser, a lad named Rodwell, and a good deal of public interest was excited by what Professor Lankester and many others considered to be his complete exposure of the trickery of dowsers in general and of this lad in particular. Now, whilst I have no wish to be an apologist for this dowser, as similar experiments which I made with him (p. 205 of the last Report) were inconclusive, and in one instance adverse, yet it must be remembered

that Professor Ray Lankester, though a distinguished naturalist, is not an equally distinguished experimentalist. His scepticism regarding phenomena that do not lie within the range of his experience might almost be termed ferocious, and, on the occasion referred to, it can hardly be said he dealt with the living instrument—the dowser—as gently as he would have dealt with his microscope. Upon this point I may perhaps quote a sentence from a letter which I received some time ago from Mr. T. V. Holmes, F.G.S., who is quite as keen a critic as Professor Lankester, but somewhat better informed in all that relates to the subject of the present research. Mr. Holmes writes:—

With regard to the experiments of Professor Ray Lankester on the alleged power of Rodwell, I am very decidedly of opinion that no result of any value could be obtained unless the boy was quite at his ease, and that he would require much more tact and geniality to put him at his ease than a man would. Now the impostor, or semi-impostor, shows his hand to the genial man, and is rigidly on his guard in the presence of the ungenial, as though “sweetness and light” never co-existed in the same person.

This is a sagacious observation, and bears out what I have already said with regard to those who are engaged in any experimental work connected with our Society.

#### No. 1. *Experiments by the Author.*

Returning to my own experiments, described in the previous Report, those narrated on p. 175 were made at Kingstown with Mr. Stears, who readily consented to be blindfolded, but who only approximately fixed on the same spots when he could *not* see as when he could see. The other experiments were made at Wimbledon (p. 205) with the lad Rodwell, who strongly objected to be blindfolded, for the reasons there given, but who at last consented: in two experiments he was completely wrong and in a third he was fairly correct. It may be urged that, Rodwell being blindfolded against his will, the experiment was not a fair one, as he was not at his ease; this I quite admit. Hence I can only regret that, taken as a whole, my own experiments were, as I have said, inconclusive.

#### No. 2. *Mr. Pease's Experiments.*

Mr. E. R. Pease made two experiments with a blindfolded dowser, which are described in his report on the divining rod, published in Vol. II. of our *Proceedings*, p. 85. These, like my own experiments, were inconclusive, and even had the dowser been successful, the result would have been of little value, as it is stated the dowser was “led over the same ground” in both cases. Unconscious muscular suggestion might well have vitiated the experiment.

No. 3. *Experiments by Lord Winchilsea and others.*

But there is a good deal of evidence by other experimenters on the blindfolding of dowzers. Eight cases in addition to my own experiments were given in the previous Report. Some of these are with amateur dowzers, as Mr. Golding, of Bocking, Essex, p. 28 (in all cases pages refer to the previous Report, *Proceedings S.P.R.*, Vol. XIII.); and Colonel Aldworth, D.L., of co. Cork (p. 31), and also a young farmer he mentions; another case is given on p. 40. A careful experiment made by Mr. J. E. Marshall, of Leeds, with the dowser Adams, is described on p. 48; an experiment with Lawrence, on p. 70; and with the late J. Mullins, on p. 95; an experiment made by Sir Welby Gregory, also on Mullins, on p. 97; and one by the late Earl of Winchilsea (then the Hon. Finch Hatton, M.P.) also on Mullins (same page). Here Earl Winchilsea states:—

Mullins was then blindfolded, and though led round by a different route, again fixed on the same spot. At first he 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.

This experiment is more fully described in *Proceedings S.P.R.*, Vol. II., p. 102. In this case it was the discovery of a pipe of running water, but the success of the dowser may have been due to involuntary muscular suggestion, as Mr. Finch Hatton states he “led” the dowser. The same explanation may also account for the success that is asserted in several of the other cases mentioned in the previous Report, though unfortunately the particulars respecting them are too meagre to be of much value, except perhaps in the case given by Mr. Marshall.

I will now quote some further cases of blindfolding which have since reached me. The first of these is a very interesting case, and here again J. Mullins is the dowser. The introductory part of this case is also of interest, and might have been added to the cases given in Part III.

No. 4. *Mr. Hoskyns' Experiments.*

In the *Somerset and Dorset Notes and Queries* for June, 1897, the Editor publishes a letter from a Mr. H. W. Hoskyns, written to him on January 2nd, 1889. Mr. Hoskyns, whose letter, together with information he has subsequently sent me, I abridge, says:—

Having an impure supply of water on his estate, North Perrott Manor, Crewkerne, Somersetshire, he sent for the late John Mullins, the well-known dowser, who, by means of the rod, fixed upon the best site for sinking a well, estimating the water to be about 25 ft. below the surface. The well was sunk, solid rock was soon encountered, and after 25 ft. below the surface had been reached, and a considerable thickness of rock pierced, still no water was found. Suddenly, however, when 25½ ft. had been



reached, an inrush of water took place, so rapid that the men had to be hurriedly withdrawn from the well, and the water rose  $23\frac{1}{2}$  ft., or 2 ft. from the surface, and has given a constant supply ever since, even in the driest summers. An old well near contained water that was so polluted that it could not be used, though the well had been cleaned out several times. The spring found by Mullins has given a constant supply of *pure* water. Not far off, Mullins indicated the site of a second well, which proved equally successful. Mr. Hoskyns adds that one of his labourers found he also had the power of using the rod, a fact corroborated by the Editor, the Rev. C. H. Mayo, M.A., who visited the spot.<sup>1</sup>

There is an interesting sequel to this. In consequence of Mr. A. Lang's notes on my previous Report, in *Longman's Magazine*, Mr. Hoskyns wrote to Mr. Lang upon the subject, and the latter forwarded me the letter. Mr. Hoskyns writes :—

North Perrott Manor, Crewkerne, *December, 1897.*

With regard to the tests suggested by Professor Barrett in order to ascertain the reality of a dowsing faculty,<sup>2</sup> the following appears to satisfy Nos. 1 and 2, *i.e.*, separate examinations by different dowsers independently of each other, and blindfolding the dowser. Mullins having on his visit here, in 1888, given several (half-a-dozen or so) indications of water in the lane by the two cottages, I had pegs put in on the strip of grass land opposite these indications, and thrust in very low, so as only just to be seen when looked for, and not showing above the grass, so that the cottage children should not pull them up or displace them. Some rather long time after, when I had made up my mind to go to the further expense of making a draw-well (not a deep one) for the cottages, having meanwhile heard that a labourer on my estate, one George Elson, had the dowsing power, I went with him myself, had him bandaged *tightly and deeply, well over the nose*, and in addition made a steady man keep his hands over the bandage, whilst Elson walked down the lane in question, holding the twig. At each halt that he made, the twig turning downwards for water, I looked for Mullins' peg, but the grass had grown over them and they were all quite undiscernible. So I made Elson and his guide (for being completely blinded, he stumbled occasionally) scrape a mark on the road surface with their boots. When we had come to the end of the lane, I returned with these men and one or two others

<sup>1</sup> My best thanks are due to Mr. Hoskyns for giving every facility to Mr. Westlake, F.G.S., who, at my request, kindly visited the spot and made a careful geological examination and report on this case. Mr. Westlake reports that the old foul well was probably between 20 and 30 ft. deep. Mullins' well, about 18 yds. distant, is sunk in the Inferior Oolite, and at a depth of  $25\frac{1}{2}$  ft. water rushed rapidly in through a joint in the rock. This well gives a splendid pure supply, even when the river is nearly dry; the water is probably dammed up by a downthrow of Fuller's-earth, which passes close to the farm. The second well indicated by Mullins is 50 yards further west, is only 18 ft. deep, and does not give so continuous a supply. All three wells are at nearly the same surface level.—W. F. B.

<sup>2</sup> See p. 251 of the previous Report.

(Elson being then unblinded) and we all made close search for the Mullins' pegs, *every one of which, without exception*, proved to be *immediately* opposite one of Elson's boot-marks on the road. This uneducated labourer could have had no geological, or even rule-of-thumb, knowledge of water-bearing strata, for the place was quite strange to him.

The only objection, so far as I can see, that can be raised to this extremely interesting test is that the guide, who kept his hand on the bandage over the labourer's eyes, might have known where the pegs were placed, and unconsciously tended to arrest the dowser at the right spots, in fact, a case of "muscle-reading," as in the so-called willing-game. This is likely enough if the pegs were *discernible*, but Mr. Hoskyns distinctly says they were not, and as some time had elapsed since Mullins visited the place, no one seems to have remembered the exact spots where the pegs had been put. I have had some correspondence with Mr. Hoskyns about this interesting experiment. He informs me that Elson's guide knew nothing whatever about Mullins' pegs; involuntary guidance is therefore excluded.

#### No 5. *Mr. Westlake's Experiments.*

I asked Mr. Westlake, who was living near Crewkerne, to repeat, with Mr. Hoskyns' permission, the blindfolding experiment with Elson. Mr. Westlake reports:—

On April 24th, 1900, I went with Mr. Hoskyns and his agent, Mr. Slade, to Pipplepen farm. I walked down the same lane with Elson to a little stream at the bottom, a distance of 1,050 ft., in the course of which his twig turned seven times. I then carefully blindfolded him, and on retracing our steps, the twig turned ten times, five of which corresponded nearly or quite with his first marks, as under:—

Elson's first marks when not blindfolded.	His second series when blindfolded.	Difference.
206 feet 2 inches	(1) 206 feet 8 inches	+ 6 inches
406 feet 6 inches	(2) 326 feet 7 inches	
	(3) 429 feet 5 inches	+ 3 feet
	(4) 564 feet 3 inches	
648 feet	(5) 651 feet	
	(6) 680 feet 6 inches	
	(7) 742 feet 9 inches	
762 feet		+ 5 feet
872 feet	(8) 877 feet	
931 feet	(9) 934 feet	+ 3 feet
985 feet	(10) 985 feet	none

Elson was not held, and was only told (to prevent accidents) to keep to the right or the left. The results may have been somewhat impaired, first, by the steward, Mr. Slade, who accompanied us, having touched Elson with a stick (with a view to guiding him straight) at the points 1 and 5, immediately after which the twig turned; and, second, there was more or less conversation, at Nos. 8, 9, and 10, Slade informing Elson that he had passed the cottages (a landmark between 7 and 8), that the point 8 he had just indicated was the well (by the roadside), and so on.

In view of such sources of error, I think Mr. Hoskyns' experiment much better than mine, but, as I understand that some of those who took part in it *had* seen Mullins' pegs when they were put in, one cannot feel certain that the experiment was an absolutely conclusive one.

In the afternoon, I made a further trial with Elson in an orchard at North Perrott, where he had previously indicated two springs. Slade remained at a distance. Elson having found the points, I blindfolded him and, starting him from a little distance and directing him by voice, I got him to cross his marks. He thereupon re-found the spots *three times*, his marks at one of them being only a yard apart. It is possible he may have been able to reckon the distance; and I noticed also towards the end of the experiment that the bandage had slipped a little, and I could see the upper corner of his left eye, so that he may have been guided by the trees. Thus the result was probably inconclusive.

Elson's hands and arms are very muscular; he uses stout hazel forks and grips them very hard, and they turn downwards with corresponding strength, usually breaking, sometimes at the fork, but generally on one side; he then takes a fresh grip nearer the fork, and so on, till the stick may be used up to within an inch or so of the fork. When it turned over the stream his forearm muscles were strongly contracted. He says it usually makes his left thumb numb, and that he feels the effects in his biceps the day after. Mr. Slade said Elson had found water for many of the neighbours, and had never had a failure. The farmer also told me Elson had predicted water at another point half a mile to the east at 30 ft., and that it was found at 27 ft., and rose to within 5 or 6 ft. of the surface.—E. W.

#### No. 6. *Mr. Denison's Experiments.*

For the next very interesting letter I am indebted to my friend, Mr. B. St. G. Lefroy, who informed me that a Canadian scientific friend of his—Mr. Denison, of the Toronto Meteorological Observatory—had been making some experiments in blindfolding an amateur dowser. At my request Mr. Denison kindly sent the following account:—

Denison Square, Toronto.

With regard to the experiments with the "divining rod," I made a full note of them at the time and copy the following from my note-book:—*June 19th*, 1898. Rode over to Mr. Harris' residence at Clarkson, 18 miles west of Toronto. At dinner Mr. H. told the following story. "Last year my well near the house became dry, I sent for a well-digger to increase depth.



Before doing so he asked permission to explore with a hazel wand to ascertain if another spring would be found deeper before going to expense of digging. He said he had discovered a stream which would flow into the well from west to east, a few feet below present level. The well was then deepened, and, when down 3 ft., water *did* flow in from the west, so rapidly it was difficult to pump fast enough to enable men to lay the bricks. Ever since there has been a good supply of fine water. It was at this time I found the hazel wand would turn in my hands also. The stick was Y-shaped, and held firmly by both hands with thumbs turned outwards."

After dinner I got Mr. Harris to cut a forked plum stick about 20 in. long—he had used a hazel before and felt sure plum would not work ; however he grasped the plum switch firmly, as explained, holding it vertically before him. As he approached the well, the stick began to turn down in jerks until when over the stream to west of the well the rod turned so much that the bark was twisted near his hands. Mr. Harris is a powerful man and endeavoured to hold the rod in its original position. I then got him to explore other parts of the lawn and at 100 ft. from latter the rod again turned down. I then *blindfolded* him and allowed a sceptic in the party to turn him round about several times and then lead him in different directions; but when he came over the first and second spots, where the rod turned before, it again twisted down. He was once more turned about and asked to walk as requested *not* in contact with any one, but the same results occurred. He then tried an ordinary willow, and also a lilac twig, with good results. When a *dry* stick was used he obtained no action. Finally, I bent a galvanised wire thus :— When Mr. Harris held this like the twig, it not only moved downwards but twisted round to such an extent as to form a loop thus :— which assumed its natural shape as he moved away from stream.

I also got him to follow a stream for several hundred feet. Mrs. Harris tried without any result, but when Mr. H. grasped her wrists as she approached the spring, the rod turned forcibly. One daughter, aged 15, had also the power. When Mr. H. held the rod and Miss H. grasped his wrists, there would be a momentary increase in the vigour of the rod twisting, and when the daughter held the rod and her father grasped her wrists, a similar action occurred.

Out of the twelve persons who tried the above experiments, two had the power well developed, two slightly, while the remaining eight almost *nil*. When the weaker members used a rod over 4 ft. long, their slight muscular action was clearly shown by the far end of rod turning down. When Mr. H. held the rod exactly perpendicularly, it sometimes turned inwards until it pressed heavily against his chest. At the end of the experiments Mr. Harris' hands were considerably blistered.

I will try to follow up these experiments.

F. NAPIER DENISON.

It is to be hoped that Mr. Denison will do so, as the letter shows him to be a very careful observer, and I am greatly obliged to him for the trouble he has taken in the matter. It will be observed that

Mr. Denison distinctly states that in his second experiment the blind-folded dowser was *not* in contact with any one, and yet was equally successful. This is therefore an important and excellent experiment, and more evidence of this kind is much to be desired.

In these experiments with about 2 in 12 persons, or say 16 per cent., the divining rod “worked,” *i.e.*, the forked twig suddenly revolved independently of volition of the holder. I should say this is not an improbable percentage, although in 1810, Professor Sementini (the Regius professor of chemistry in the University of Naples)—in an essay he published entitled, *Pensieri e Sperimenti sulla Bacchetta Divinatoria*, asserts that with five out of six persons on the average he found the “rod” would move, that is over 80 per cent. This large percentage was doubtless due to the position of unstable equilibrium in which the rod was held, though it is possible this class of automatic action may be more common in some countries than others. Another *savant*, Amoretti (see p. 246), who had a wide experience, states that he found about 20 per cent. of those he tried were susceptible; this fairly agrees with my own estimate above. If any of my readers who are interested in the subject will try, and get their friends to try, the “rod,” or *autoscope*, as I have suggested it should be called, and let me know with what success, I should be grateful. The method of holding the rod is fully described in Appendix C to the last Report.

It will be noticed that Mr. Denison says a *dry* stick would not work, but any forked green twig would do and also wire. This is the experience of many others who can use the rod and who usually attribute it to the dry stick being a non-conductor of electricity; it is very improbable that electricity has anything to do with the matter; the reason is doubtless that the green twig is more supple and elastic, for whalebone is used by some dowsers in preference to a twig.

#### No. 7.—*Dr. Thouvenel's Experiments.*

The next evidence is a remarkable statement made a century ago by a distinguished and learned French physician—Dr. Thouvenel.

This statement refers to the young French dowser, Bleton; the evidence relating to Bleton's powers as a water-finder is given in considerable detail subsequently, pp. 257 *et seq.* Here I need only say that Dr. Thouvenel, having heard of Bleton's alleged powers, determined to test them thoroughly from a scientific standpoint. He did so, and published the results in a work called *Mémoire physique et médical, montrant des rapports entre les phénomènes de la baguette divinatoire, du Magnétisme et de l'Electricité*. Paris, 1781. A second memoir was published later, and though his theory of the divining

rod was, I believe, mistaken, that does not affect the evidence I will now cite. On pp. 77-80 of his first *mémoire* Thouvenel gives a summary of the tests he made with Bleton in Lorraine. The following is a translation of the passage :—

I took the precaution to repeat several times all the experiments just described in detail, after having carefully *blindfolded* Bleton ; in addition his arms were sometimes fastened behind his back leaving his forearms only just sufficiently free to hold the *baguette* at the extremity of his fingers, sometimes even confining these in order to hinder, if possible, all mechanical movement. These precautions were not taken for my own satisfaction, as I was already entirely convinced.

I conducted Bleton to places which he had never seen ; I took him towards springs of which I knew, but which he could not know of, at other times over ground where neither of us knew what might be found. Whenever he experienced his peculiar sensations—and the *baguette* had repeatedly turned at the same spot,—I then led him far away, bringing him back by quite different roads, still with his eyes bandaged. . . . When he had followed the course of an underground spring—sometimes for more than a quarter of a mile, across mountains, rock, or forests, and indicated on the way numerous sub-divisions of the same spring—I made him return. He then re-conducted me himself, though still blindfolded, only supported by one arm, to the point from which we set out, without straying a single step from the line previously traced and marked by pegs, which were often hidden beneath the surface. He re-found all the subterranean rivulets already pointed out, and followed exactly the sinuosities of the underground stream. It frequently happened that we came across springs whose course was interrupted by walls, terraces, or wide ditches, so that in order to enable Bleton to overcome these obstacles, I had to procure ladders, or take long détours, or in some way get him conveyed across ; nevertheless, although blindfolded, he soon regained the course without the aid of his eyes. Sometimes in order to try and deceive him, if his senses were concerned, I placed false marks as if to indicate a spring ; sometimes after he had followed a spring across several fields, I moved the pegs some feet away without his knowledge. Nevertheless, he was never led astray and always rectified such errors. In fine I tried all sorts of ways to deceive him, and I can testify that *in more than six hundred trials*, I did not succeed in doing so one single time.

M. Jadelot, the Professor of Medicine in Nancy, Thouvenel states, was a witness of, and co-operated in, all his experiments, which extended over a space of two months, and was no less struck than he was with the strength and importance of the evidence obtained.

In addition to the foregoing we have successful blindfolding tests made with Bleton by a small committee of *savants* quoted in No. 15 of Part X., p. 269. What are we to say to the above evidence ? It is true that Dr. Thouvenel mentions that he “led” Bleton when blindfolded, and therefore involuntary muscular suggestion may possibly account for most of the success of Bleton, but the results appear too



remarkable and too uniformly successful to be wholly explained in this way ; nor is it clear that Bleton was always touched by Dr. Thouvenel, who, it must be remembered, was a man of scientific training and habit of mind. Moreover, if the blindfolded dowser succeeds, as in Nos. 4, 5, and 6, when involuntary muscular action appears an inadequate cause, another explanation must be sought, and this explanation may, if established, be the true cause of success in some of the other cases where muscular guidance is possible.

No dowser of whom we have any knowledge in the present century seems to have rivalled Bleton, though it is much to be regretted no scientific man ever took the trouble carefully to test the remarkable powers of the late Mr. W. Lawrence, of Bristol, and Mr. J. Mullins, of Chippenham—names that will be familiar to readers of my previous Report. It will be noticed that Bleton, like Stone and other dowsers, mentions the peculiar sensations he experienced when over underground water ; this may be a delusion, but the coincidence is remarkable, as it is wholly improbable the English dowser ever heard of Bleton, or had obtained the idea from him.

As already remarked, I do not know of any *blind* dowsers ; if such exist, it would be most interesting to ascertain what degree of success they achieve. Of course, if there be any supernormal power possessed by a dowser, it is improbable that blindness would be prejudicial to its existence.

I will here add two or three cases which, though startling at first sight, *may* be capable of explanation by muscular guidance, and therefore I will not weary the reader with multiplying other instances of this kind that have reached me.

#### No. 8. *Judge Spinks' Experiments.*

A striking case of the success of an amateur dowser when blindfolded is narrated by a judge in British Columbia ; this was also given in the previous Report, p. 27. His Honour Judge Spinks writes to Mr. F. W. H. Myers as follows :—

Vernon, Okanagan, B.C., *February 27th, 1893.*

The rod works in my hands. I was rather sceptical, and thought that my own mind might work in some unknown manner on the rod and cause it to turn down where I fancied there ought to be water. I was *blindfolded* and led about with the wand, for about an hour at least, until I could not hold the wand upright without great pain. Each time the wand dipped, a peg was driven into the ground to mark the spot. I was walked in all directions, and passed over the same ground again and again, but in no instance did the rod fail to dip when it came to a peg. I have sunk two wells on the credit of the wand, and in both instances have found water, in both these instances contrary to the advice of the well-sinking experts. The power appears to increase rapidly with use. When experimenting with the

rod over a water hose, I had the water turned on and off several times, and could distinctly *feel* the jar that one hears in such cases.

WARD SPINKS.

In a subsequent letter Judge Spinks gives some additional evidence. A third well was sunk upon the indications afforded by the rod and also proved successful; between two of the wells the rod would not move; there the foundations of his house were subsequently dug and proved dry, though "only a few feet from being immediately between wells 2 and 3." I wrote recently to Judge Spinks to inquire whether the pegs were put in *before* he was blindfolded and if some one *led* him all the time he was unable to see. To this I received the following reply:—

Rossland Club, Rossland, B.C., *January 12th, 1900.*

It is some years since Mr. Streatfield and I experimented with the "wand," and my memory may be at fault. As well as I can remember, the facts were as follows:—I found several places where the "wand" dipped and marked them by driving pegs into the ground. Afterwards Mr. Streatfield led me blindfolded over the ground and the "wand" moved over every peg, I believe; I then led him and the "wand" dipped in his hands as it did in mine; he was blindfolded also. We led each other by the arm, so there *was* contact. Of late years I have been travelling too much to make experiments, but I tried the "wand" last Fall, and was surprised to find that it did not work with me as it use to.

WARD SPINKS.

It will thus be seen that in this case the amateur dowser was touched, or led, by a friend who could see, and hence the evidential value of the case is destroyed, as unconscious muscular guidance might account for the success achieved when blindfolded.

#### No. 9. *Mr. Withnell's Experiments.*

The next case relates to the blindfolding of Mr. W. Stone, evidence of whose success as a dowser was given in the last Report and also in the preceding pages. The following Report is abridged from *Morton's Lincolnshire Almanac* for 1899:—

Some two years ago the late Mr. J. B. Dunham, of the firm of Messrs. Dunham and Sons, Horncastle, commissioned Mr. Stone to find a supply of water at his farm, High Toynton, near Horncastle, where it was needed badly. On reaching the farm Mr. J. B. Dunham declared his intention of putting the dowser to a severe test. With a V-shaped hazel twig cut from a hedge close by, the diviner went over the ground in question, a field located on an incline some 200 yds. from the main road, and soon indicated a spot where he averred a plentiful supply of pure water would be found at a depth of about 25 ft. As he passed and repassed over this particular spot, the dowsing twig, held in the manner before described, turned and bent completely over towards him. The two spectators, watching every movement, candidly informed the diviner that they believed the turning of the

twig was due to some sharp movement on the part of the holder, and Mr. Stone readily acceded to the request that each in turn should be allowed to hold the mystic twig, as he himself held it, over the same place. But do as they would, the twig never moved in the slightest with either. Mr. Stone then suffered himself to be completely blindfolded and led away from the spot which he had indicated as the place where water would be found. His two guides, holding his arms (in the hands of which the twig was loosely held), led him about in a circle and then in a zig-zag fashion, managing by devious ways to lead him over the located spot, previously marked by a stake, and each time as the diviner crossed it the twig curled as before. At the same time those on each side of him distinctly felt a twitching in the diviner's arms, such as might be caused by an electric shock. When the coverings from his eyes were removed, Mr. Stone remarked that he knew from the sensations he experienced that he had crossed the place he had pointed out two or three times. Our previous doubts were removed; and more particularly was this the case when a short time afterwards a well was sunk on the site, and a most excellent spring of pure water found at a depth of 24 ft. In September last the writer journeyed to the same *locale* and found that the supply of water was as good and as strong as ever, being even more than was required on the farm.

Having ascertained that the writer of the above report was a journalist named Mr. F. Withnell, who took an active part in the experiment, I wrote to Mr. Withnell to inquire whether he had blindfolded Mr. Stone effectively, and received the following reply:—

*News Office, Horncastle, Lincolnshire, September 15th, 1899.*

The Divining Rod was a thing unknown to me before I came into Lincolnshire. Therefore, as a journalist (of fifteen years' standing, and a member and officer of the branch of the Institute of Journalists for ten years), combined with a scepticism born of a rough life and much travel abroad, I determined to investigate the matter for myself. I will vouch for the particulars contained in the report, and if necessary can give you the names and addresses of other persons (men of standing and experience) who have put Mr. Stone, in his water finding, through a similar ordeal, and assure me they are, as the result, fully convinced of Mr. Stone's power of finding water, and even gauging the depth at which it is to be found in apparently the driest of places.

I blindfolded Mr. Stone personally; closely folding a silk handkerchief and tying it tightly over his eyes, afterwards pulling a close texture bag, made of canvas, over his head and twisting the mouth of it round his neck. There was no mistake, therefore, that the blindfolding was thorough, and that he was totally unable to see downwards. Mr. Stone assured me, and those who were with me, that he had never before been over the ground, and that it was purely on account of the hazel twig turning over in his hands that he was able to say that water would be found at that spot. I was thoroughly satisfied with the experiment and said so, and my companions also expressed themselves in similar terms.

I have been with Mr. Stone on other water-finding excursions since then, and I have never known him fail in his findings or be wrong in his



calculations as to the depth, which he approximately estimated in each case. He informs me that he has had failures, but these are of rare occurrence and separated by long intervals. May I add that I have only been brought into contact with Mr. Stone professionally. He resides at Old Bolingbroke, which is nine miles distant from Horncastle, where I live.

FRED. C. WITHNELL.

As Mr. Withnell knew the spot fixed on by Mr. Stone, and it was "marked by a stake," it is quite possible to explain Mr. Stone's success in this blindfolding experiment by some involuntary muscular action on the part of "his two guides holding his arms;" albeit both Mr. Withnell and Mr. Stone have written to me subsequently, stating that no guidance was possible, as touching was excluded. This may have been another trial at the same place. Here is Mr. Withnell's letter:—

27, Bridge Street, Horncastle, Lincolnshire, *June 8th, 1900.*

As regards my account of the blindfolding of Mr. Stone, I certainly wish it to be understood that Mr. Stone proceeded *alone and untouched* after being blindfolded when searching for water on Mr. Dunham's farm at High Tointon; the only place where I personally have submitted him to a test. After Mr. Stone was blindfolded, I and the late Mr. Beavis Dunham simply led him away from some paraphernalia that was on the ground and against which he might have stumbled, and then, releasing our hold, we allowed him to take whatever course he chose to take, assuring him, on his own request, that we would carefully watch him and give him a warning shout if in his peregrinations he appeared likely to blunder against the hedge. As he walked about free from obstruction, we neither touched him nor called out to him. I have only this week verified this by conversation with two others who were on the ground at the time, viz.: Mr. Dunham's foreman and another.

FRED. C. WITHNELL.

#### No. 10. *Mr. Emerson's Experiments.*

So far I have quoted the dowser's success when blindfolded, but there are cases of *failure*. I have mentioned one, when I tested Rodwell at Wimbledon in 1890. Professor Ray Lankester's test of the same lad is a well-known instance of failure, and the following case of failure is so little known that I refer to it at some length, as it appeared in the leading scientific organ in America, the *American Journal of Science*, now called *Silliman's Journal*, and doubtless had much influence on scientific opinion in America. The investigator is the Rev. Ralph Emerson, who five years previously had published a paper in the same journal on some successful experiments with the divining rod. Mr. Emerson's paper is so very long that I am compelled to summarise what he says with regard to the blindfolding experiment.

In 1821 he found at a farm in Ohio that a successful well had been sunk at a spot indicated by a "diviner," where no sign of water previously existed.

One of the farmer's sons, a lad of 12, discovered the rod "worked" in his hands, and it was this young lad Mr. Emerson tested. In the first instance the young dowser, with his eyes open, professed to trace the course of an underground vein of water, the position being marked by Mr. Emerson; the boy was then blindfolded, led a short distance away from the spot and told again to trace the course of the water. He did so and the rod moving, he declared he was over the spot; this turned out to be perfectly correct. But on a second and a third attempt he failed, and afterwards "incessantly," so that, Mr. Emerson adds, "there could be no mistake, the illusion of all attraction underground vanished at once. The motion of the rod remained, but it must be accounted for some other way." Mr. Emerson then shows how the motion of the rod may be caused by a very slight muscular action on the part of the dowser;—this part of his paper is worth notice and is given subsequently (p. 287). He explains the success of the blindfolded lad on the first occasion by his probably keeping count of the steps he took, but that his incessant failures afterwards were due to his losing count, and hence Mr. Emerson concludes that the boy was a "young fox" and "the pretensions of the diviner are worthless."<sup>1</sup>

A singularly illogical and unscientific conclusion, but one very characteristic of the numerous "exposures" with which the history of this subject abounds. The surprising thing is that a precisely opposite conclusion was not arrived at, *viz.*, that the lad was not *right* every time, and that Mr. Emerson did not as triumphantly assert that he had conclusively established the truth of a mysterious influence exerted by the "veins of water" on the boy, and so "set the question for ever at rest" on behalf of the dowser. For, on reading the account carefully, it seems that Mr. Emerson in every case *was holding the blindfolded boy*: "I took him lightly by the elbow"; "I guided him back"; "I led the lad to the next spot and he missed it again"; "I led him to and fro, but he failed incessantly," etc. Now, if the boy had been a "young fox," he might with perfect ease have been right every time, simply by allowing himself to be guided by the muscular indications which Mr. Emerson might have given involuntarily whenever he crossed the right track, just as successful finds are made in the "willing game." The whole series of experiments made by Mr. Emerson are absolutely valueless, and hence his conclusion is equally so. There is, further, no evidence to show that this young boy was a "dowser" at all; not only, therefore, was the experimenter unskilled, but the instrument he employed was probably defective, and a confusion of thought existed in the experimenter as to the object *sought for* (see p. 141).

## PART VI.

## DETAILS OF A FEW FURTHER CASES OF AMATEUR DOWSERS.

Those who may have read the previous Report will remember the numerous cases of amateur dowsers that are there cited, particulars being given of upwards of twenty-five individuals of both sexes, and in all ranks of life, who had discovered by accident that the forked twig twisted round in their hands, and had used its indications with some success in the discovery of underground water. It may be of interest if I now give somewhat fuller details of a few of the additional cases of amateur dowsers that have reached me.

No. 1. *Sir R. Harington, Bart.*

Among notable persons who have found that they could successfully use the divining rod, and who have made use of its indications with advantage, may be mentioned his Honour, Sir Richard Harington, Bart., Chairman of Quarter Sessions for Herefordshire. In the *County Council Times*, for July 17th, 1897, Sir R. Harington published the following interesting letter of his experience with the rod :—

Whitbourne Court, Worcester, *July 11th, 1897.*

I discovered, quite by accident, and at the age of fifty-seven, that I possessed this power [of using the divining rod]. I was building a new lodge to this place, and, desiring to find a water supply, I sent (more from curiosity than from any belief in its efficacy) for the gardener of a neighbour, who was said to be able to use the divining rod. He came. I watched his proceedings, and then requested him to allow me to try the rod myself. I did so, and, to my great surprise, found that with me it assumed the vertical position in the same places in which it had done so with him. He discovered water, but the place which he assigned to it turning out inconvenient, I proceeded to investigate on my own account. I had in the meantime been told the method employed to estimate the depth of the water beneath the surface. I found indications of water at a depth which I estimated at about 17 ft. to 20ft. I caused a well to be sunk, and a small supply was found at about 18 ft. This, however, was at the beginning of a long drought, and late in the summer the supply failed. I tried again at the same place, when my rod told me there was water about 10 ft. deeper. I had the well deepened, and found more, fortunately just within the 28 ft. limit, and there has since (this was five years ago) been a sufficient and unfailing supply for the use of the lodge.

The most practical use I have since turned my power to is the discovery of leaks in a large artificial piece of water round my house. This for some distance runs parallel to the course of a brook at a much lower level, a road



passing along the top of the embankment, the slope of which is covered with wood and undergrowth. Occasionally a rat or rabbit makes a hole, which draws off the water, and is difficult to find by ordinary search. I always find it easily with the divining rod.

I have never been at the trouble to ascertain how many kinds of wood are serviceable, but the peach and Spanish chestnut will serve with me as well as hazel.

As may be supposed, I am not constantly searching for convenient places to dig wells: but I occasionally use the rod for the purpose of showing friends how it acts, and they generally try it themselves. I very rarely find any one who possesses the power which I do alone, but almost invariably when the person who cannot use it alone holds one end of the fork and I the other, it works: in some cases more strongly than with myself alone. But I do not find that I have the power claimed by Mr. Gataker of estimating the volume of the water. The effect on me is the same, whether it is a mere trickle or a copious current.

That the power exists, and that it depends on some natural cause connected with the physical idiosyncrasy of the individual possessing it, the operation of which is not yet understood, I cannot, after the evidence of my own senses, doubt. To stigmatise the action of those who do possess it as charlatanry appears to me a specimen of that arrogant ignorance which despises what it cannot understand.

I may add that the extent of the power seems to vary in individuals. For example, I cannot calculate the volume; my original instructor could not calculate either depth or volume, and, although the rod usually acts when held by another person and myself as above described, one of my daughters appears to have a kind of neutralising power, and when she holds one end of the fork and I the other, it will not act at all.

If the Local Government Board official, who, I understand, threatens Mr. Gataker and his fellows with prosecutions under the Vagrant Act, will do me the honour of calling here at any time when I am at home, I shall be happy to try the experiment with him, and let him try for himself—first, whether he possesses the power alone, and, secondly, if not, whether the rod acts with him jointly with me. His scepticism will not affect its action. The power is obviously purely physical.

RICHARD HARINGTON,

Chairman of Quarter Sessions for Herefordshire.

In reply to my inquiries Sir Richard Harington was good enough to write me at length confirming the facts mentioned in his published letter, and he also states that, on the other hand, the rod seemed to give him a wrong indication on one occasion.

In a chalk soil in Essex two or three years ago it indicated water on a friend's property near the surface. My friend dug down to the depth guessed, but found none, and I dissuaded them from incurring further expenses, lest I should be mistaken.

The principal practical use to which I have put the power is to fix the locality of leaks in the bank of an artificial piece of water, and in

determining the course of underground drains and leaks therefrom. For these purposes I have found the power practically useful.

Sir Richard Harington in reply to my question as to whether he could give me any further particulars of the use of his power, or the dates, etc., at which he found the leaks in the bank, writes as follows ; after giving some details which are contained in his published letter, he adds :—

Whitbourne Court, Worcester, *October 29th, 1899.*

It has occasionally happened that leakage into the brook has taken place through rat holes and the like, the locality of which my servants have been unable to discover. When this has been the case, I have used the divining rod, which has always told me correctly where the leak was. I cannot give dates, but the event has occurred on two or three occasions during the last ten years.

The piece of water in question is supplied at one end from the above-mentioned brook by means of a culvert of some length.

It appearing that there was waste of water in this culvert, I tried the divining rod, walking parallel to it. It told me, as the fact turned out to be, that the leaks were numerous, so many, indeed, that it was useless to repair the culvert, inside which I had to lay glazed pipes. This took place in the year 1898.

RICHARD HARINGTON, BART.

The use of the “rod” to find a hidden leak in a culvert or reservoir has been successfully tried by others ; see, *e.g.*, p. 41 of my previous Report, where the amateur dowsers, Messrs. Young and Robertson, independently fixed on the same spot. Mr. Westlake, F.G.S., has recently informed me of another case, where the dowser, R. Pavey, of Cheddar, did the same thing at the moat round the Bishop’s Palace at Wells. An engineer was first employed, and had spent £20 in trying to find the leak, but failed. Pavey, though a stranger, found the leak at once, and it was then stopped at a cost of a few shillings. Mr. Westlake visited the place and ascertained the facts ; he states the engineer was much impressed by Pavey’s success, as he, after careful and costly examination of the place, had failed. Experiments of this kind are worth careful repetition under strict scientific supervision.

## No. 2. *Mr. E. Hippisley.*

One of the most successful amateur dowsers of whom I have heard was the late Mr. Edwin Hippisley, a leading man in Wells, Somerset. Mr. Hippisley was a member of the Institute of Surveyors, and his firm, Hippisley and Sons, are the principal surveyors and land agents in Wells. At a meeting of the Wells Archæological Society, held at the Bishop’s Palace, in Wells, in April, 1892, Mr. Hippisley read a paper on his experiences with the dowsing rod. A

verbatim report of that paper appeared in the *Wells Journal* for April 21st, 1892, together with an interesting discussion that followed, in which the Bishop of Wells, who was in the chair, the Dean, and others took part. Mr. Hippisley's paper is too long to quote, but the following are some abridged extracts from it. Though his theory (a common one) is improbable, his facts are of considerable interest.

In using the rod I always find the point turn upwards to a nearly vertical position when approaching an underground spring, and it reverses its action after the spring is passed over: upon turning round and walking over the same spot the same thing occurs. The only theory I can form is that those persons who have the "gift," as it is called, are more susceptible to electrical action, water being a good conductor of electricity, and that the rod is moved owing to some sensation in, or electric influence on, the body of the operator.

Mr. Hippisley then proceeds to give details of numerous instances of his discovery of underground water by means of the rod, which proved of great practical value.

For instance, at the Lodge to the Cemetery at Portway, at the Sanatorium near Axbridge, at cottages near East Horrington, at Burcot, at Wookey Hole, and at two or three other estates in Somerset. At Cotford Farm, near Taunton, a well was being sunk for the Western Somerset Asylum, and after 50 ft. had been sunk, Mr. Hippisley, by means of the rod, directed the men to drive a heading west of the well, when a largely increased supply of water was obtained. Another and remarkable case at Lytes Cary I have already given on a previous page (see p. 174.)

No. 3. *Mr. J. H. Jones.*

The next case is that of a country gentleman to whom I have already referred, and who is well-known to me, Mr. J. H. Jones, of Mullinabro, near Waterford. In answer to my request for a brief account of his experience, Mr. Jones writes as follows:—

Waterford, *January 3rd, 1900.*

Some few years ago, a friend and neighbour of mine, after trying in vain to obtain an unpolluted water supply for his house, decided on enlisting the services of Mr. Mullins, the dowser. I was asked to witness the experiments and was glad to do so.

Mr. Mullins, on his arrival in Waterford, per steamer, drove rapidly to his client's house and in a few minutes indicated an underground water supply near the house quite unconnected, as he alleged, with the flow to a polluted pump-well in the stable yard; he pointed out a favourable spot for sinking, where subsequently a splendid supply of pure water *was* found at about 30 ft. deep. On the evening of Mullins's visit, I, for curiosity, cut a forked rod similar to the one he used, and experimenting in a field at home,



found, to my surprise, that at one place the rod turned vigorously in my hands. Walking onwards in a zig-zag direction (similar to the way Mr. Mullins proceeded in tracing an underground flow of water), I traced the line of flow detected by me until I found its outcrop in a never-failing spring at some distance from the place where I first found the rod to turn. The direction of the flow to this spring greatly surprised me, for I had hitherto imagined that the spring was fed from a lake, the water from which, at about half-a-mile from the spring, disappeared under the face of a limestone cliff, emerging in a short distance. Here, I imagine, there could be no "sub-conscious" influence derived from my knowledge that a spring existed some distance from where I first felt the influence of the rod, inasmuch as the line of flow traced out by me by its means was in a direction nearly at right angles to my preconceived notion of the direction of the supposed feeder of the spring.

The ensuing evening I tested myself where Mr. Mullins had found the line of flow for my friend, and I found I could trace it exactly as he had done. Here it might be said I was unconsciously influenced by Mr. Mullins's action, but, as I traced the flow for some fields beyond where he stopped and where neither of us had any idea the line ran, the theory of the influence of his findings on me seems to break down.

The above experiments converted me (in spite of my determination at first to doubt the *bona fides* of the dowser's indications) from being a prejudiced sceptic to being a confirmed believer in the genuineness of the dowser's claims to point out underground flows of water.

What is the mystic influence emanating from water, and why it should produce the effect I have described is a problem for scientists to solve.

It is marvellous to find a flow of water, as I have proved, exercising a strong influence through 70 to 80 ft. of a dense quartzite rock.

The influence is in my case independent of contact with the ground. It effects me though insulated by standing upon a support resting on glass. I can feel the influence of underground water in a house as readily in the topmost storey as on the ground floor. I can also feel it—apparently as strongly as when standing on the ground,—when I am riding, driving, or travelling by rail, and this not over predetermined places, but over untried ground.

Mr. Jones then goes on to describe how he endeavoured to verify the accuracy of the indications given by the rod by going over ground that had been previously traversed by other dowsers *before* he was acquainted with the places they had fixed upon as water bearing; the result, he says, convinced him that some peculiar effect was produced upon him by underground water, an effect that caused a certain spasmodic muscular contraction in his arms and hands, thus producing an involuntary movement of the rod. Mr. Jones states that on approaching the spring, the point of the rod begins to creep upward and is more or less violently jerked into a vertical position when directly over the water; the rod he recognises as merely an index of some peculiar impression produced upon himself.

No. 4. *Mr. J. D. Enys, F.G.S.*

In the preceding Report (Vol. XIII., p. 7) it was mentioned that the President of the Royal Geological Society of Cornwall, Mr. J. D. Enys, F.G.S., was an amateur dowser. In a recent letter to me Mr. Enys says :—

Enys, Penryn, Cornwall, *May 6th, 1900.*

In answer to your questions I may say (1) that only a slight feeling is produced in me when the rod commences to move over underground water, but a distinct feeling of relief when I let it go. (2) In a place I indicated (which place was also chosen by the dowser who showed me how to use the rod) an abundant supply of water was reached at 96 ft., which has stood the test of the late dry season.

The clerk of my Parish Council here (St. Gluvias), Mr. J. Lowry, tried the rod, at my request, over a place where I knew a spring existed ; suddenly I heard him cry out, with genuine surprise, "It is alive, sir ; it is alive." This *exactly* describes the sensation when the rod moves. I am unable, however, to form any opinion of scientific value on the action of the rod.

JOHN D. ENYS.

The interest of this letter is in the fact that not only is Mr. Enys a gentleman of high standing in his county, but he is also an able geologist. I asked Mr. Enys if he would try some blindfolding experiments, with precautions to avoid unconscious muscular guidance and other possible sources of error, but he has not as yet had time to do so. I hope he will pursue the subject.

No. 5. *Mr. J. F. Young.*

In the previous Report, I referred on page 40, to two amateur dowsers, Mr. J. F. Young and Mr. Robertson, of Llanelly, S. Wales, who have written a little book on the divining rod. Mr. Young has had an extensive correspondence with me, and has also in person related to me the numerous experiments he has made with the rod in the attempt to arrive at some solution of its perplexing movements. I have been much struck with Mr. Young's enthusiasm, intelligence and scientific spirit, and it may be interesting to put on record the following account which Mr. Young has sent me of his own experience. The account is of greater interest from the fact that Mr. Young was unaware, until he subsequently read my previous Report, that the divining rod, like planchette, was an *autoscope*, and therefore its movements were profoundly influenced by any predetermined idea or suggestion. Mr. Young's experience is very similar to that of some early writers on the rod, such as Zeidler, who in his *Pantomysterium* gives an account of the manifold objects which excite the motion of

the rod, and hence suggested the title of his book.<sup>1</sup> Mr. Young writes to me as follows:—

Llanelly, South Wales, *July 3rd, 1897.*

I will, with your permission, place before you some of my personal experiences with the “rod,” trusting you will understand I am only a struggling tradesman, with but little learning and very little spare time, but with an earnest desire to search for the truth, and with that object I have been for years an ardent reader of mental physiology, and psychological books of all kinds.

I soon became convinced that the movement of the rod was due to an involuntary action on the part of the “diviner.” To prove this I have watched carefully my own hands (and others’) and in every case when the subject sought for has been found, I observe a difference in the relation of one hand to the other; there is in my own case a slight advance of the right hand. In other cases with larger rods, I note the arms at the elbows, which are usually fixed to the side, give greater indications of a closer grip, and the hands are clenched together; others watching have confirmed my views.

What causes this involuntary movement of the muscles I cannot say, but I will give a series of cases where it is produced and where I have used the rod successfully.

*Water-finding* was the first which engaged my attention; finding the rod turn in my hand, I tried every available means of proving the reality of a water-finding faculty. At last I was convinced of its truth and its usefulness to mankind. Electricity is generally put forward to explain mysteries we know nothing of, and I fell into this same trap. I set to work, and made many electrical appliances (which gave me great pleasure, as it revealed what I previously knew but little of) to test the matter, both in relation to the soil, the atmosphere and my own personality, and at one time I thought I had it within my grasp, but alas, with more knowledge, events transpired which toppled my electrical theory over. I left electricity for the study of “Odic Force,” Animal Magnetism, Telepathy, Clairvoyance, Automatism, etc., and latterly the Spiritualist hypothesis, including in its wide range our “subliminal consciousness,” and I look upon this last as one that meets the questions at all, or nearly all points; and here I may add in opposition to some arguments which have been put forth by some, it seems to me, taken on the lines of this latter theory, it becomes a proper subject for investigation by the S.P.R., as it is a question of the study of human personality and not of the rod.

I take it that the different sensations and experiences of different water-finders is a matter of idiosyncrasy, as in my own person these vary according to conditions, from a strong convulsive feeling at one time, another a state of ecstasy, at another a peculiar, unpleasant feeling at the pit of stomach, and latterly a quick shock, darting, as it were, through the nervous system

<sup>1</sup> I have added this quaint book of Zeidler’s to our S.P.R. Library. Its title is *Pantomysterium oder das Neue vom Jahre in der Wüandschelruthe, etc.*, Magdeburg, 1700. It has eleven plates and thirty-one figures of different kinds of “divining rods,” made of every kind of forked article, such as tongs, snuffers, etc. Later on, I will make further reference to this book, an excellent abstract of which has been made for me by Miss Stokes.



at the moment of the perception. This sensation belongs especially to Class G of experiments (see below).

In a number of the experiments the rod seems indispensable, but even then merely as an *indicator*, its motion being a notification to my brain that the influence has taken place, which otherwise would not have reached my consciousness, and for these especially delicate tests I use the form of instrument enclosed. It is a small aluminium fork, light and delicate to the touch, and, being nearly void of elasticity, no tension can be put on. The instrument I hold lightly between finger and thumb of each hand, and when it acts, it either slowly *creeps* up or suddenly twists at the moment of the impression.

The experiments for convenience of reference I will classify, commencing with *Class A*, which includes all experiments with water, the feet in every case coming directly or indirectly in contact with the soil, the latter through the medium of a horse, or some form of conveyance, or *anything* that touches *terra firma*. The so-called insulation by bottles and other non-conductors, or even rubber boots, may be traced in their several effects to imagination on the part of the water-finder.

I have made some very critical tests in this direction, and have found all so-called insulation a failure.

I may here remark that in all investigation in this matter (with a *good sensitive*) that suggestion, *thought* or *spoken*, of the agent plays a very important part.

*Class B* includes the finding of metals of all kinds, either hidden, buried, or lying on the surface. A coin or two may be (by another person) hidden with some opaque covering. The hands of the sensitive, with or without the rod, being merely passed above the table, its whereabouts is at once localised and the finger goes down straight upon the coin. Telepathy cannot explain this, as it can be so arranged that no one can know where the coins lie. With the case of coins I must not omit to include the singular fact that where metal has lain, it leaves an influence for many hours, which can in the early hours be detected as well as the coin. Or, on a country road, say, but seldom used, I can locate the track of the wheel and the spots where the horse has trod. A wonderful variety of experiments can be made under this group.

*Class C*.—The finding of heat rays that have fallen on earth, stone, wood, etc. ; of a shadow or line of light, etc.

*Class D*.—The localising of a given spot, say a square inch, on which sound has been projected closely ; this vibration soon passes away, and cannot be done unless tried *at once*.

*Class E*.—Currents of electricity in a wire ; but this is difficult to prove, as the influence might be due to the metal and not electricity, as in the case of water flowing through *metal pipes*.

*Class F*.—The finding of a locality where friction has been made, say rubbing a floor, carpet, etc., if dry. On a dry walk it is successful ; on a wet one, not so.

*Class G*.—Under this heading the experiments are so numerous that my naming two or three will give you an idea how easy it would be to extend

them ; the phenomena taking place here are really the same that underlie the whole of the phenomena connected with the divining rod faculty.

Mrs. Young (my wife) whom I shall hereafter call the *agent*, and *myself* the *subject*.

*Expt. 1.*—Picture to yourself Mrs. Y. sitting at one end of a room ; a certain line is indicated on which I shall walk down the room *from* her and with my back toward her ; she has a watch before her, and at a given moment, only known to herself, wills me to stop ; that moment the rod moves and I stop. Sometimes when in good form I can do so minus the rod.

*Expt. 2.*—Mrs. Y. in same position fixes mentally, unknown to me (my back always toward her) on a sprig, flower or pattern on the carpet in a given line. I start on the quest ; as soon as my foot “ touches the spot,” the rod moves.

*Expt. 3.*—I now walk along near the wall. Mrs. Y. has fixed on a spray, flower, picture, bracket, vase or some such *small* object, say the height of my shoulder or head. As soon as I get opposite the signal comes. Many of these are conducted by the agent in one room and the subject in another. You will observe here also there cannot be any bodily contact.

*Expt. 4.*—Cut, say, a dozen pieces of paper all alike, an inch square, mark *one* only, as you like, say pencil initials or a number. After stirring and mixing up, place all on floor in different parts, not knowing the marked one. The subject walks about, and ultimately finds the marked paper. In this way you cut out thought-reading or telepathy.

*Expt. 5.*—A hair from head or beard is placed on the floor, the subject, of course, being absent. The agent leaves the room, accompanied with a stranger (who is not in the secret) to see fair play ; or the subject may be blindfolded or walks backward. As soon as he reaches the spot his rod moves. I won't trouble you with more, as you can picture to your mind's eye scores of similar experiments, those above narrated, with others quite as extraordinary, all indicating an intelligence—call it what you will—*not under our control*. It is almost needless to add that times occur with me and also with the agent when this cannot be done, and there are times when the power, whatever it may be, appears to be overflowing. It is, however, exhausting, and, after half-a-dozen experiments, a rest is required. I will do my best to carry out any other experiments you may suggest, and hope what I have recorded may be of some use.

The foregoing experiments must simply be taken for what they are worth ; they are not quoted as of evidential value, for we do not know, *e.g.*, how far any involuntary and unintentional signs from the “ agent ” may have reached the “ subject ” of these experiments. The detection of *points de repère* by hypnotised subjects, usually attributed to hyperæsthesia, resembles Mr. Young's detection of the track of a horse, etc., by the use of rod, and I am inclined to think the explanation in both cases is similar. I have given this letter at length, as it demonstrates the all-important part played by any suggestion or idea in producing the involuntary muscular action that causes the twisting

of the rod. It further shows how absurd is the dowser's notion that underground water *must* be indicated when the forked twig twists round in his hands.

No. 6. *Mr. R. G. D. Tosswill.*

Through watching Mr. Young's use of the forked twig in Devonshire, a gentleman at Budleigh Salterton, Mr. Tosswill, found the twig also moved when he held it. He soon became an enthusiastic amateur dowser, and sent me the following account of his experience after reading my previous Report:—

Budleigh Salterton, Devon, *October 13th, 1897.*

The first time that I saw the "hazel rod" used in seeking for water was on August 11th, 1897.

My friend who used it was searching for the spot underneath which a small stream of water was flowing through the adit of an old disused copper mine. He asked me to walk alongside him and closely observe the movements of the rod as he passed over the water. I did so, and soon observed the top of the rod quiver, and then move slowly forward and outwards away from his body, until the point of the rod pointed downwards to the ground at his feet.

He then asked me to try it, and walk over the same course that he had taken, holding the rod in precisely the same manner. I did so, holding the rod perpendicularly, with the end of either branch laid on the palm of each hand, with fingers and thumbs tightly closed, and back of the hands downwards.

On advancing, I fully expected to find that if the rod moved at all in my hands, it would do so in precisely the same manner as it did with my friend. To my astonishment, however, I found that on coming to the same spot, the rod quivered slightly, and then moved towards my own body, *inwards*, and then downwards, pointing to the ground. This was exactly in the opposite direction to which it had previously moved when in my friend's hands.

Since then I have used the hazel rod or some other "autoscope" nearly every day in seeking and tracing underground springs, water pipes, drains, hidden coins, etc. Hazel, willow, beech, thorn, elm and honeysuckle twigs, as also copper and other wire rods or forks that I have used, holding them perpendicularly either in the closed hands or between the thumbs and forefingers, move always in the same direction, *i.e.*, inwards and towards my body, and then downwards pointing to the ground.

ROBERT G. D. TOSSWILL.

No. 7. *Mr. G. H. Ward-Humphreys.*

In the *Cheltenham Examiner* for March 24th, 1897, Mr. G. H. Ward-Humphreys gives a lengthy report of his first experience with the divining rod, which affords a good illustration of the genesis of an amateur dowser and the amazement—shared by educated as well as illiterate persons—excited by the apparently automatic motion of the twig.



The night before, Mr. Gataker had expressed the belief that I was likely to possess the power of "divination"; and he was now very anxious for me to try my luck at once. He cut several twigs and armed me with one, and then, taking me down to the side of the lake where water already found was pouring in, he suggested I should walk across the covered-in stream. This I did three or four times in succession, holding the twig, but I was conscious of nothing, nor did I see any movement of the twig. He then, however, laid his hands upon the back of mine, and he walking backwards in front of me, we together crossed over the flowing water. Immediately the V-shaped twig turned completely round in my grip. Perhaps it is unnecessary to say anything about the astonishment with which I regarded this extraordinary movement of the inanimate twig. But what happened afterwards was even more remarkable: for, on going round to unbroken ground upon the other side of the lake, at one spot I felt the twig, which I was carrying with its point downward, so turn in my hand that the point was raised through an angle of 90 deg. Inquiries elicited the fact that this spot was exactly over a stream which had been found twelve months ago. But even now I was not convinced: though I could not believe that the others were deceiving me, I thought it possible that I might be deceiving myself. So I suggested that I might be *blindfolded*, led some distance from the spot where I was then standing, and then should be guided in such a direction that I must cross the line of the same stream. This was done, and I started my walk at a distance which I afterwards found to be about 60 yds. away. Walking then in the direction I have indicated, I suddenly became aware that the twig was commencing to move. This tendency became stronger at every step. I cast to the right and to the left, and finally guided by the stronger movements of the twig, walked some 15 yds. to the left of my original path and then stopped, the twig having now moved through an angle of 180 deg. My companions, who had hitherto been silent, were greatly amused at the sceptic [the writer]—who found, when able to see, that he had stopped about 3 ft. away from a peg which had been put in to mark the site of one of Mr. Gataker's springs! After this, scepticism vanished.

It will be observed that Mr. Ward-Humphreys states that when blindfolded he was "led," so that the explanation of his success in that case may be due to the involuntary guidance exercised by those who were touching him.

#### No. 8. *Mr. G. F. Attree.*

In the *Estates Gazette* for February 3rd, 1900, Mr. G. F. Attree, an auctioneer and estate agent of Brighton, gives a lengthy account of his experiences as an amateur dowser. He points out how prejudiced he was against the whole subject until, after watching a dowser at work, he discovered to his surprise that the twig moved in his own hands, twisting round whenever he came to the places marked by the dowser. He goes on to say:—

The operator holds the two ends of the twig so that the point of the V is at a right angle with his body and the thumb of each hand is in contact

with one end of the twig. In this position he walks over the land where he wishes to ascertain what springs exist. On coming over running water (no matter how deep it may be from the surface) the operator feels a tingling sensation in his hands and arms, and the twig immediately twists over until its point has completed more than half a circle.

During the year 1898 I spent much of my little spare time with the twig, frequently testing springs which I could prove through existing wells, and by this means, and by measuring the width of the influence on the surface of the ground, became able to gauge accurately the depth and strength of a number of springs. . . . I cannot say why so few persons possess the power, nor what the current is that passes from running water through the body of the operator, causing the twig to twist. All I have been able to ascertain is, that it is not an ordinary electrical current, for insulation by glass or indiarubber, which will stop the one, has no effect upon that which is brought into force in water-finding.

Mr. Attree is an enthusiastic believer in the "rod," and objects to the term "divining-rod" and "water-diviner," as he affirms there is no guessing or divining or pretending practised by an honest water-finder, the motion of the rod being due to some cause beyond the control of the operator: he states that he has successfully made use of the indications of the rod in finding "springs" for the benefit of his friends and neighbours. Several of these instances are given by Mr. Attree, and I have recently received some interesting additional evidence of Mr. Attree's faculty as an amateur dowser.

The interest of this case is the additional illustration it affords (1) of the involuntary muscular action that causes the motion of the twig; (2) of the accompanying peculiar sensation affirmed by so many dowsers; (3) of insulation not affecting the motion; (4) of the implicit belief of the operator that whenever the rod twists the presence of underground water must be indicated; and (5) of the readiness with which incredulity becomes credulity when something (such as the twisting of the rod) happens contrary to the sceptic's expectation; the dowser's erroneous views about "springs" and "head of the spring" being forthwith accepted without examination.

## PART VII.

## MISCELLANEOUS CASES.

I will here add a few stray cases that do not come under the previous classification. The following was kindly sent me by the Right Hon. Sir Edward Fry, D.C.L., F.R.S., etc. To those who imagine that dowzers are only employed by the credulous and ignorant, this testimony, from one of the highest judicial authorities in our country, who is also a man of science, is a sufficient reply.

Failand House, Bristol, *October 13th, 1898.*

I have sunk in four places for water near here. The first time was some twenty-five years ago at Failand House Farm. There I was guided by Mr. Lawrence, a well-known dowser, who lived somewhere in Gloucestershire. I sank for water and got some, but subsequently, under Mr. Merryweather's advice,<sup>1</sup> I deepened the well, and have now a very good supply, which has shown no signs of failure even during the prolonged drought of this year.

I sank a well about 20 ft. in Faulkner's garden—on my own selection of the spot—got no water, and gave up the pursuit.

About six years ago I sank a well near this house, and got water at, I think, 36 ft. This now supplies this house and the garden, stables, etc., etc. The site was chosen by a dowser, who came over with my architect, Mr. Price, of Weston-super-Mare.

I was not here either when Lawrence dowsed for the farm well, or Mr. Price's friend for the well near this house, and know nothing in particular about their proceedings.

I consulted Mr. Merryweather same years ago about sinking a well near this house, and he dowsed and thought he could find a line of water, but I did not act on that suggestion.

Last year, wanting to get a water supply for a farm (Lower Failand Farm—Parson's) I again consulted Mr. Merryweather. He came with his two sons, to one of whom he attributes the power of dowsing, and I went over the ground with them, and he ultimately advised me to drive a tunnel into the hill. He did this work for me, and found water, though it has failed during the long drought. I am not sure whether there is not a leak in the tunnel.

There is no doubt a good deal of water in the hill on which all the land in question lies, but how far it is to be found everywhere I cannot say. I rather incline to the belief that the old red sandstone, which is the rock, is traversed by faults or lines of weakness, and that down these the water passes.

<sup>1</sup> Mr. Merryweather is another dowser. See previous Report, p. 72. He died recently.



In the search for water of last year Mr. Merryweather certainly did not rely on the watch spring alone.<sup>1</sup> He considered also indications of water on the surface (from the character of the herbage) and the presence of small springs or droppings of water in the rocky sides of the lane, from which we drove the tunnel. He has been a well-sinker for years before he took to dowsing, and would, I am sure, scout the notion that he should neglect other indications of water. I may add that I have known him for many years—that I have employed him not only as before mentioned, but on other matters relating to drains, etc., and that I believe him to be a thoroughly honest man.

EDWARD FRY.

The fact mentioned by Sir Edward Fry that Merryweather is in part intentionally guided by surface indications is very interesting. This, however, is not usually the case with other dowzers, so far as I know. The value of surface indications is referred to more fully in another part of this paper. Sir Edward Fry also sends me the following extract from a volume called *Memorials: Part II., Personal and Political*, by the Earl of Selborne, Vol. II., p. 384–5.

In a letter to his daughter, the Countess of Waldegrave, under date September 25th, 1893, Lord Selborne writes, after referring to the effects of the drought:—"Willie" (*i.e.*, the present Earl of Selborne) "brought a water-finder from Wiltshire to see if he could find near the reservoir any underground water which could be used for increasing the supply there, and he did find it as he *said*, but till this is verified by sinking to the necessary depth I shall suspend my belief, though the same man has been successful in finding it at Hatfield and for Lord Jersey." To this a note is added, "The divining rod proved successful."

There are, I believe, no professional dowzers in *Ireland*, though English dowzers are in considerable request in certain parts of the country, and there are a few amateur dowzers. The following case of an amateur dowser, who is a farmer in the co. Tyrone, has lately reached me through a friend:—

Mullanteau, Stewartstown, *October 17th, 1899.*

The water-finder, William M'Crea, who lives near Stewartstown, was here to-day, and found two springs for us. It is really wonderful the way the rod turns in his hands; it broke nearly in two, though it was a strong piece of ash. He says he sleeps badly the night after he does much of it, and at the time it feels as if the sinews of his arms were being torn out. He first tried the rod about twenty years ago, when an uncle of his came from America, who had made a good deal of money there by finding springs, so he tried if he had the power, and found he had. He can tell how far the spring is down by counting the feet from where the rod begins to tremble

<sup>1</sup> Mr. Merryweather, like the late Mr. Lawrence, always preferred to use a long piece of flexible steel rather than a forked twig as an indicator, or "autoscope."—W. F. B.

until it turns over,—double that distance is generally right, but he won't engage that, though he says in most of the wells sunk he has been right to a foot or two. He has found a fine spring lately for the creamery at Moy, and it has done well ever since. He can tell of no failure, so far, that he has had. He is a man about fifty years of age, and has a good farm near this, so he does not do it for money. We will begin, I hope, to dig our well soon, and I will let you know how it gets on. The spring, he asserts, begins on a hill, and continues all down the field and up to the house, I suppose 400 yards or more, and that it could be tapped anywhere we choose.

C. M. KENNEDY.

I have also received other cases where McCrea was successful, but he dislikes using the rod, as he says "it seems to take such a lot out of me."

Through the kindness of friends I have received some particulars of the successful use of the rod for finding underground water by one or two other amateur dowzers in Ireland, such as the late Mr. Thos. Purvis, of Wexford, of whom Mr. J. Haughton, of Ferns, co. Wexford, informs me that Mr. Purvis enabled his family to obtain a splendid supply of water on their premises, "as abundant to-day as forty years ago when the well was sunk."

Colonel King-Harman, of New Castle, Ballymahon, Ireland, is another Irish amateur dowser, and in reply to my inquiries told me that a former steward of his had been a successful dowser in America; Colonel King-Harman having kindly obtained the man's address, I wrote to him and had the following reply in answer to my questions:—

Whitfield Farm, Kilmeaden, Co. Waterford, *October 30th, 1899.*

My experience of the divining rod was in Manitoba, Canada. There the rod proved successful every time but once, and I have every confidence would have done so then, but we struck on a vein of sand, and the well would not hold out.

I dug one well where other twelve were dry, and this one had abundance [of water]; it was finished by Government. Other six wells I dug myself out there, on the condition that if no water, no wage. All that I had to guide me was the rod, and in all those places water was scarce.

I cannot give the addresses of all I dug for, but by writing to Mr. Angus, postmaster, Logoch, Manitoba (for whom I dug the first well), you can have the names of all the places.

I have tried the rod in Co. Waterford for Major Chavasse on very dry hilly ground; he has not yet dug.

I have no special feeling in my hands or body when the rod turns.

GEORGE MITCHELL.

In *Bibby's Quarterly* (a farming journal) for November, 1897, is an illustrated account of a Cornish dowser named George Williams, who

lives at Falmouth. It seems he accidentally discovered that the rod moved in his hands, and was successful in tracing a lode by its means. Subsequently he used it for water, and several cases of his success are quoted. I wrote to the author of the article for further particulars, and had the following reply :—

Rosvean, Falmouth, Cornwall, *January 26th, 1898.*

George Williams has recently found a spring for me in the country near Falmouth. It was not where I had hoped or wanted, but his hazel rod pointed where the spring was. I was present while he walked all over the field. The work, he says, is always very exhausting to him. The spring, a fine jet of water in the rock, was found 28 feet below the surface. No one present knew anything about a watercourse there, and there was nothing to suggest it.

The spring at the vicarage (built on the hill at Budock) was 70 feet below the surface ; it was also found by Williams.

SUSAN E. GAY.

In a subsequent letter Miss Gay tells me that in sinking her well, a solid slaty rock was encountered and dynamite had to be used ; the water, when reached at a depth of 28 ft. was so abundant that “even through the drought of last summer it was never below 6 or 7 ft.”

I visited Falmouth and saw the place where Williams found a water supply for Miss Gay. It certainly seemed an unlikely spot, on dry rocky ground between Budock and Penjerrick : the geological formation is a clay slate (Devonian), near its junction with the granite ; as Mr. Westlake remarks, “water tends to run off the granite into the fissures of these slaty rocks, affording an excellent testing ground for the dowser” ; here and there the water makes its way to the surface as seen by certain “springs” marked on the map not far distant. I had an interview with Williams ; he is a working man, a mason, and, like other dowsers, has the most child-like faith in the indications of his “rod” : he showed me several testimonials from gentlemen in Cornwall who spoke highly of his services in finding water on their estates. But I had no opportunity of making a personal investigation of any of these cases. Miss Gay states that Williams has been engaged by the Great Western Railway Company to “dowse” at Truro for water and that he has worked with success for the Ecclesiastical Commissioners on various occasions.

Among the dowsers with whom I have had some correspondence is Mr. H. A. Canning, who is a sanitary and hydraulic engineer at Market Lavington, Wiltshire. Mr. Canning tells me that seeing a dowser at work some twenty-five years ago he tried his hand, and discovered that the forked twig “worked” equally well with him. He encloses me a list of names of gentlemen for whom he has been successful in finding water by the indication of the twig. In one case



the twig moved at a spot between two wells 100 ft. apart, which gave little or no water; on sinking at the spot indicated, an abundant supply was obtained. This may be so, but I have not verified the statements. Mr. Canning, in a letter to the *Standard* in January, 1889, states that upon finding the rod suddenly move at a certain spot he was then blindfolded, and again on traversing the same spot the rod moved. He adds there was an unmistakable but indescribable feeling upon approaching the spot beneath which underground water existed, a sensation that could not possibly be avoided, even though an effort was made to do so. It is certainly a remarkable circumstance, whether this sensation be illusory or not, that for 200 years the great majority of dowzers in all parts of the world have quite independently asserted the same thing, and as a rule believed that this peculiar sensation, or *malaise*, existed only in their own case.

An interesting account of an East Anglian dowser, Mr. Child, recently appeared in the *Eastern Daily Press*. Mr. Child, who was formerly in business in Norwich, now lives near Hadleigh in Suffolk, and, like the majority of dowzers, was originally a Somersetshire man. When living near Wells in Somerset he saw a dowser at work, and was greatly astonished to find the rod twisted round in his own hand at the same places. Several instances of his success are given in the newspaper report referred to, but these I have not attempted to verify.

I have already mentioned a Cheddar dowser named Thomas Pavey (p. 191).<sup>1</sup> The son, Rowland Pavey, of Cheddar Mill, is also a dowser, but has lately abandoned the use of the forked twig, and trusts to what he calls a "New Science" in his water-finding expeditions. What this mysterious science is he does not tell the public. I have had some correspondence with Pavey, and his letters are full of rhodomontade. Though he will not tell me what his wonderful secret is, he assures me it is "a science of sciences, and to keep it secret is keeping hid a field of wonderful discoveries that cannot be exhausted by scientists, as it is, I believe, the outcome of a higher state of civilisation." (!) In a printed circular he sends me he states that, though his science is in a crude state, yet the "science of geology will be surpassed, the uplifting of mountains and what lies hidden far beneath will be understood. There will be no more secrets in water-finding, and the human body will be understood as it never was before," beside a multitude of other revelations, which he enumerates, ranging over the universe! In spite of all this nonsense, the Rev. H. H. Streeten, who writes to

<sup>1</sup> F. H. Pavey, p. 234, another Cheddar dowser, is a connection of these Paveys.

me from Easton Vicarage, near Wells, tells me, in answer to my inquiries, that "Pavey certainly has a remarkable knowledge of the earth. *How* he has acquired this knowledge is a different question, but in localities he has never before visited he will in a short time tell you where the springs of water are, and generally he will say at what depth water is to be found underground. He is a quiet, nice-minded man." Mr. Westlake, who has visited Pavey, says he is a genuine enthusiast, with a firm belief in his "gift." The following report of an eye-witness describes how Pavey usually goes to work in determining the depth at which water would be found underground. In this particular case—

He knelt down on one knee with his head resting on one hand, and gazed downwards for about half-a-minute, afterwards giving it as his opinion that it would be necessary to go down approximately 60 ft. As he predicted, a good spring was struck within a few inches of the depth mentioned. The supply is plentiful, as is evidenced by the fact that, after it had been left during the night, three hours were occupied in getting sufficient water out of the well to enable a commencement to be made in lining it with brick.

There is, however, no evidence to show he was not aware of the depth of other local wells.

However striking some of these cases of the success of the dowser may appear to the lay mind, to an expert field geologist, who knows the district, the explanation is often simple enough. Here, for example, is a case in point. Mrs. Hollands, of Dene Park, Tonbridge, kindly sent me an account of the success which attended the dowser (Mr. Gataker) at her place, but informed me they had not made any previous attempts to find water, and adds :—

Dene Park, Tonbridge, *October 17th, 1899.*

We dug in *two* places, and found water about the depth he [the dowser] told us—fifteen to twenty-five feet. Two of these wells are now most valuable, in these long seasons of drought. Without them we should not have enough water to supply the house at all.

MINNIE HOLLANDS.

Mr. Holmes, F.G.S., remarks on this case :—

At Dene Park, near Tonbridge, a gravel patch capping the Weald Clay seems to have been tapped. I learn from Topley's Geological Survey Memoir on the Weald (1875) that there are many outlying patches of gravel at high levels in that district. Dene Park is rather more than two miles north of Tonbridge. Gravel patches are mentioned (p. 185) as existing near Starve Crow Farm (about half-a-mile south-east of the house at Dene Park) and east of Little Park (about a quarter of a mile north of Dene Park). The dowser would need simply a good eye for contours, as in the Oxford case.

T. V. H.

In addition to the cases which I have already cited, I have before me a number of other illustrations of the dowser's success in finding water, and very often correctly estimating the depth at which it would be found. In these cases, however, the replies I have received to my inquiries have shown that no previous attempt to obtain a water supply had been made, and several state that water would probably have been found without the dowser's aid, only they wanted to "make sure" before sinking a well. This correspondence has also shown how misleading are many of the reports appearing in some of the provincial newspapers. These reports doubtless give the dowser's own version of the matter. To quote a typical instance, the *Bath Chronicle*, which appears to be Mr. Gataker's favourite organ, contains two misleading statements in its issue of October 22nd, 1896. One is as follows :—

Another striking testimony to the ability of Mr. Leicester Gataker, the water expert, of Bath, has just come to hand. In the face of much scepticism he recently undertook to supply Mr. Smith Barry, M.P., Cordangan Manor, near Tipperary, [with water] for the purposes of a dairy, on special terms of "no water, no pay." He foreshadowed 300 gallons an hour at from 25 to 35 ft. deep. On this he at once started his men to carry out the sinking of the well. At a depth of 32 ft. a supply of 400 gallons has been obtained as a result.

I wrote to the Right Hon. Smith Barry for information, and his estate agent (Mr. G. C. Townsend) replies that "the results of Mr. Gataker's work here have not been sufficiently successful to justify me in giving you the information you ask."

The other statement, a few lines above in the same paper, is as follows :—

Mr. G. Marshall, of Sarnesfield Court, Herefordshire, was getting pretty well tired of well-sinking at the time he engaged the services of Mr. Leicester Gataker, but his latest move has been most successful.

In reply to my inquiries Mr. Marshall states that he sunk *no wells* before Mr. Gataker's visit, but simply made "catch-pits" to collect surface water. In further correspondence Mr. Marshall informs me :—

Heralds' College, London, E.C., *October 16th, 1899.*

In reply to your questions—

1 and 2 Before Mr. Gataker found water for me at Sarnesfield I had built a large tank and attempted to collect surface water, which failed me in a dry season.

3. Mr. Gataker came and indicated two spots within a short distance of my tank, telling the number of feet to sink the wells and the amount of water I should find. His estimate was perfectly correct, and since I have had the wells sunk I have had a most satisfactory supply. The late dry summer has been a severe test, but there has been a full supply of water,



and I have no reason to believe there will ever be otherwise. It is curious how many people appear to have the power of water finding did they know how to use it. Several of us tried when Mr. Gataker was at Sarnesfield, and when he placed a twig in their hands it moved, but with myself, although Mr. Gataker gripped me firmly by the wrists, there was no movement.

GEORGE W. MARSHALL.

Sarnesfield is near Weobley, about ten miles north-west of Hereford. I asked Mr. Marshall what was the need of a dowser, as probably surface water would be found anywhere in shallow wells at Sarnesfield. He replies :—

Heralds' College, London, E.C., *October 19th, 1899.*

Of course, on a soil like ours at Sarnesfield, which is on stone, you can get water anywhere when you get down to the rock ; but the point which shows Gataker's skill is that being told whereabouts (a mile and a half from my house) I wanted water found, he fixed the spots to sink the wells, and told accurately the depth to go and the quantity of water which would be found.

GEORGE W. MARSHALL.

Mr. Holmes remarks :—

The Gataker cases are an excellent illustration—as you remark—of the deceitfulness of newspaper reports. Mr. Marshall's second letter reduces very considerably the atmosphere of wonder enveloping the first.

T. V. H.

Much of the wonder which the dowser creates, in the unscientific mind, arises from the notion which he invariably fosters that underground water is found in narrow streams or “springs,” and that without his aid in hitting upon the exact spot where to sink a well, the chances are you would have missed the water altogether. It is true that in *certain* districts the geological formation causes underground water to be found in fissures or narrow streams, and there, as I have said before, the real test of the dowser is to be found. Such a formation prevails in the county of Somerset, as Mr. Westlake has shown in his instructive Appendix (p. 315). In the next Part, I have cited some failures of dowsers and the probable percentage of successes they obtain. It must, however, be clearly understood that neither in this Report, nor in the previous one, have I selected favourable cases and omitted others which were adverse. Every case that has reached me has, as far as possible, been investigated, either by correspondence with the person who employed the dowser, or by a visit to the spot by my geological friends.

## PART VIII.

PROFESSIONAL DOWSERS IN ENGLAND AND WALES AND THEIR  
PROBABLE PERCENTAGE OF FAILURES.§ 1. *List of Dowsers and Hints as to their Employment.*

It may be convenient in concluding this part of our work, to append a list of professional dowsers in England and Wales. The stronghold of the dowser is to be found in those districts where the geological formation renders it difficult to discern underground sources of water, such, for example, as in the Lower Lias and amid the limestone regions of Somerset and Gloucestershire and in the South of France. In and around Cheddar there are numerous professional dowsers; to some of these I have already referred. With the help of Mr. Westlake I have drawn up the following list of professional dowsers now living in England and Wales. No doubt there are several others of whom I have not heard, and there are, of course, in addition numbers of amateur dowsers, some of whom have been remarkably successful. It will be noticed that even in this imperfect list no less than eighteen professional dowsers are found in Somersetshire, more than half the total; and several who now live elsewhere, such as Mr. Child and Mr. Stears, originally came from Somersetshire.

*Dowsers in Somersetshire:—*

- J. Blake, Hill End, Winscombe.
- H. Chesterman, Anglo Terrace, Bath.
- C. Cross, Hallatrow Heath, near Radstock.
- Thomas Day, Shipham (miner).
- Thomas Foord, Shipham (labourer).
- L. A. Gataker, Weston-super-Mare.
- J. J. Green, Cheddar.
- Thomas Hawker, Somerton (farmer).
- W. Hills, Wells.
- C. Hole, Keward, Wells (carter).
- William Kerslake, Wells (tailor).
- W. Mereweather (jun.), Bedminster, Bristol.
- Rowland Pavey, Cheddar (miller).
- F. H. Pavey, Cheddar (plumber).
- C. Sims, Pilton.

Thomas Thomas, Worle, Weston-super-Mare.

H. Williams, Pilton.

W. R. Wyburn,<sup>1</sup> Woolavington, near Bridgwater (farmer).

*Dowser in Gloucestershire :—*

D. Lacey, Yate.

*Dowsers in Wiltshire :—*

H. A. Canning, Market Lavington (sanitary engineer).

H. W. Mullins, Colerne, Box.

B. Tompkins, Chippenham.

*Dowsers in Dorset and Cornwall :—*

W. J. Mitchell, Cerne Abbas (sanitary engineer).

G. A. Williams, Merrill Place, Falmouth.

*Dowsers in Wales :—*

T. Heighway, Llandrindod Wells.

W. Hoskins, Uplands, Swansea.

R. W. Robertson, Llanelly.

E. Rothwell, Cardiff.

*Dowsers in East Anglia :—*

R. Anthony, Yelling, Hunts (farmer).

H. Bacon, Newport, Essex.

J. Blanchard, Wisbech, Cambs.

S. T. Child, Capel Mills, Hadleigh, Suffolk.

W. Stone, Bolingbroke Hall, Spilsby, Lincolnshire.

*Dowsers in Yorkshire :—*

F. Rodwell, Wensley.

J. Stears, Hessle (near Hull).

It may not be out of place in this connection to give one or two practical hints to those who intend to employ a dowser, or “diviner,” as they usually like to be called.

(1) Ascertain the geological conditions of the place where a water-supply is wanted : in numerous cases where a water-finder is employed his advice is wholly unnecessary (see Appendix A. and also B.).

<sup>1</sup> Since the above list was printed I have been informed that Mr. Wyburn (a member of the Society of Friends) takes no fee, therefore he can hardly be reckoned among professional dowsers. The list of Somerset dowsers might no doubt be easily extended by further inquiry, *e.g.*, George Elson, labourer at North Perroth, p. 205.



(2) If a large supply is wanted and a deep artesian well or boring to be made eschew all dowzers and consult a good hydro-geologist.<sup>1</sup>

(3) If only a moderate supply is required,—especially in certain formations where geology is of little help,—select a couple of dowzers of good repute, and let each give his opinion unknown to and independently of the other.<sup>2</sup>

(4) As the best dowzers I know have been more or less illiterate men, their loss of time would be amply repaid by offering them a moderate fee and travelling expenses.

(5) If the water-finder is willing to sink or bore on the faith of his opinion, obtain his estimate, and if found to be reasonable, offer him so much per cent. extra on the condition of “no water no pay,” taking care to have a written agreement as to the limit of expense involved.<sup>3</sup> What this extra percentage should be depends on the geological conditions. Taking the country indiscriminately, 15 per cent. would probably be reasonable, but if “fissure water” (see p. 341) predominates, and moreover, if geological advice has already been sought and on trial has proved a failure, an extra rate of 25 per cent. is not unreasonable, if a certain supply be stipulated for.

## § 2. *Recent Cases of Complete or Partial Failure in Water-Finding by English Dowzers.*

Though nearly every professional dowser thinks he can be no more mistaken in trusting to the twisting of his rod for the indication of underground water, than a magnet can be mistaken in its selective attraction of iron, yet it is hardly necessary to say the dowser is by no means free from mistakes. He has his failures, sometimes costly failures to those who employ him. Several of his failures I detailed

<sup>1</sup> Mr. Westlake, F.G.S., in reference to this adds the following note:—“Even where deep wells are necessary, experience has shown it is sometimes advantageous to supplement geological advice by calling in a dowser, for, though a definite water level may exist, the *quantity* found at a given spot is often a matter of chance: such a case I saw recently near Newbury, where the dowser, Tompkins, had found, at a depth of over 100 ft. in the chalk, so large a supply that steam pumping-gear had to be used, whereas in two or three other wells in the neighbourhood, with water at about the same hydrostatic level, the quantity was much less. It is difficult, however, to say how far chance coincidence may account for such lucky hits.”

<sup>2</sup> On the question of independent examination by different dowzers, see Part IV., and also the last sentence but one in Professor de Mortillet's letter, p. 244.

<sup>3</sup> The reason for this will be seen in the next few pages. The dowser will usually make a rough estimate of the quantity of water likely to be found and the depth at which it exists; with some dowzers this estimate is often wonderfully close, with others it is as often wrong. It is desirable, therefore, to bear this in mind and also that the quantity of water yielded by all shallow wells varies largely with the season of the year.

in the previous Report, and I will here cite a few more that I have since met with ; others of course exist that I have not heard of. But whilst I have been obliged to omit numerous instances of the dowser's success which were of no evidential value, I have not omitted any of their failures which were verified upon inquiry ; meaning by failure, complete disappointment of the expectations raised by the water-finder.

As I have quoted an instance of Mr. Gataker's success at Aspley Heath, p. 185, here is an instance of his failure in the same neighbourhood. It is contained in a letter to Mr. Myers from a friend of his—the daughter of a former Professor at Cambridge—who writes most indignantly, and states that Mr. Gataker's predictions were not in any way fulfilled. The place is at Woburn Sands, and it seems that before Gataker's visit a well had been dug about 70 ft. deep in the Oxford clay without finding water. When Mr. Gataker came, he fixed upon a spot where he said a large water supply would be obtained—at what depth is not stated. A well was dug and only a little water obtained, even when headings were driven radiating from the sides of the well.

The writer of the letter suggests that the investigation of every dowser should be confined to the question of whether “he *ever* made a mistake, for if water were not found in the place and at the depth predicted by the diviner, I should consider him an impostor.” I am afraid this “short way” with dowsers would not carry the investigation very far. The writer concludes by expressing a wish to write to the *Spectator* “on the wickedness of diviners and the extraordinary folly and credulousness of educated persons in believing in them.”

Other dowsers besides Mr. Gataker have their share of failures, in spite of all their attempts to explain them away. Here is a case of the discomfiture of another dowser, related in detail in the *Daily Mail* for December 28th, 1897. It seems that a member of the Legislative Council of Jamaica, whilst on a visit to England, engaged the services of a “diviner” to locate the site for wells in a district in Jamaica which had suffered greatly from the need of a water supply. Handsome terms were offered by five parochial boards in Jamaica to the water-finder, who on his arrival in their neighbourhood was received with enthusiasm, and very soon, by means of his rod, pointed out several places where water would be found.

At Mandeville, a charming village in the hills, he pegged out part of the course of a subterranean stream and retired to lunch at a neighbouring hotel. In his absence some wags removed his pegs and lined out a totally different course. On his return the diviner took up the new direction and continued it for over a hundred yards more, and did not discover his mistake until one of his admirers pointed out the deception. But no borings for water had yet been made, and the authoritative statements of the diviner were unchallenged. At length the necessary implements

were obtained, and an attempt was made at several points to reach the water so confidently indicated. At one point where water was predicted as at a depth of 40 ft., a boring of 150 ft. was made, with the only result that no water could be found, the machinery eventually broke down, and after going down about 200 ft., the borer could not be extracted. The same ridiculous result occurred in other places, and up to the present at none of the points indicated by the diviner has water been found.

Like all other newspaper reports, this needs confirmation, but it is no doubt fairly correct. In 1897, Mr. Stears informed me he had entered into a contract to dowse for water in Jamaica, and he no doubt was the unfortunate dowser employed. Nor is this the only costly failure he, as well as some other dowsers, have had.

In the previous Report (p. 173) I gave some details of a failure on the part of Mr. Stears which led the Parish Council of Bardney, Lincolnshire, to a very heavy and useless expenditure. At a spot indicated by the dowser they bored to a great depth and found no water. I have since heard from Mr. Hind, the Clerk to the District Council, who informs me they continued boring to a depth of 450 ft., at a cost of £300, and found no water. The bore-hole then caved in. Bardney is about ten miles east of Lincoln.

As I have pointed out again and again, whatever success attends the dowser is mainly due to his discovery of a *moderate water supply yielded by comparatively shallow borings or wells*, i.e., from 15 to 50 ft. deep (see previous Report, *Proceedings S.P.R.*, Vol. XIII., pp. 235, 240, etc.). Hence it is a great pity that District and County Councils, when they want a large water supply, do not consult a good hydro-geologist. Mr. C. E. de Rance, F.G.S., one of the best authorities on water supply, published in 1891 an admirable paper on *The Underground Waters of Lincolnshire*, which I recommend to the Bardney District Council. They will find in it a record of deep borings not far from their own town.

Another case, more or less a failure, in which Mr. Stone was the dowser, has reached me from Messrs. Attenborough and Timms, of Northampton. They had originally sunk and bored to a depth of 70 ft. at Dassett Sidings, near Burton (between Fenny Compton and Kington railway stations), and found no water. Mr. Stone was then engaged, and with his rod fixed on a place 125 yds. distant where he stated water would be found: "a sufficient supply, he thought, would be reached at 90 ft., but the exact depth he could not say." Here Messrs. Attenborough bored to a depth of over 100 ft.; near the surface, as Stone had said, a little water was met with, but on testing with an artesian pump at the greater depth, "the result was unsuccessful," and in a recent letter to me (June, 1900) Messrs. Attenborough state: "We spent a great deal more money than we



were given to understand would be necessary, and, seeing no prospect of success, stopped the work after boring to over 100 ft."

Many of the professional dowzers undertake the work of sinking or boring for water at the spot they have selected, and, as in the above case, frequent disputes arise from this cause, the dowser wanting to go on boring, at his customer's expense, when he fails to obtain an adequate supply of water at or beyond the depth he estimated. It is necessary, therefore, to warn all those who intend employing a dowser as well-sinker to make a definite arrangement beforehand, such as I have suggested on p. 236.

Mr. Westlake, in the course of his investigations in the south-west of England, heard of some half-dozen cases of expensive failures on the part of the dowzers there employed; particulars of these cases and the names of the dowzers have not yet reached me. Mr. Gordon Harris, A.M. Inst. C.E., in a useful paper on "Water Supply to Country Mansions," read before the United Service Institution on December 4th, 1899, after referring to dowsing for water and my previous Report on this subject, mentions a case where a dowser predicted a water supply at 160 ft. depth on an estate in Hertfordshire. Mr. Harris, being consulted, showed that as the London clay had here to be pierced to reach the chalk, no water could be found under 200 ft.; a boring was made at the spot and geology triumphed.

Mr. T. V. Holmes, F.G.S., in his excellent paper on the Divining Rod published in the *Journal of the Anthropological Institute* for November, 1897, to which I have already referred, mentions three or four cases where dowzers had partially or completely failed; two of these I referred to in the previous Report (p. 236),—(1) one was when Mr. Tompkins was employed at Porthcawl in Glamorganshire; the geological formation at this place is, I find, some 15 to 20 ft. of red marl and shale, resting on a deep bed of Triassic Conglomerate; (2) the other case was at Ampthill in Bedfordshire, where 10 to 20 ft. of the lower Greensand rest on a bed of Jurassic clay over 500 ft. thick. In both these cases, as might be expected geologically, a small supply of water was found not far below the surface, and no increased supply after boring to a much greater depth.

These two partial failures are of considerable public interest, as the dowzers were in each case engaged by the respective District Councils, and the costs incurred were disallowed by the auditors to the Local Government Board. In reporting the case the local newspapers state that :—

Mr. W. A. Casson, the Local Government Board auditor, in giving his decision, quoted Professor Barrett, who had written a monograph "On the So-called Divining Rod," which was published in the *Proceedings of the Society for Psychical Research*. Though apparently strongly biassed in

favour of diviners, Professor Barrett admitted that their general ideas of water were absurd, as they imagined springs to exist like buried treasure, located to an area of a few square inches, or as underground rivers which they professed to trace within an inch on either side. He (Mr. Casson) regarded divination as a survival from times when magic and witchcraft were generally believed in.

Like many others, Mr. Casson was misled by the name divining rod; he should, however, be more careful in his official utterances; if he had read my report, he could hardly have made the foolish statement that I was "strongly biassed"; a judge or a jury are not said to be biassed when they arrive at a decision after patiently hearing the evidence on both sides that has been brought before them.

The reason assigned by the auditors for their decision was that the diviners made a pretence of claiming some supernatural power in finding underground water (this is a mistake; no dowzers claim such power; "abnormal" is meant), and it was stated the judges had laid down that

"the pretence of a power, whether moral, physical, or supernatural, with intent to obtain money, was sufficient to constitute an offence within the meaning of the law."

The fees charged by the dowzers were therefore illegal, as they could not be recovered in a court of law. In addition, the Porthcawl auditor surcharged the District Council with the expenditure on the boring, amounting to over £500.<sup>1</sup> Throughout England and Wales the public interest excited in the matter was great. I received nearly 100 cuttings from various newspapers relating to the subject; the editors for the most part lamenting the benighted and superstitious practices of the Porthcawl and Ampthill councillors. "It is impossible," said the *Leeds Mercury* in reference to this case, "to plumb the depths of human credulity."<sup>2</sup> But the surcharged Councils appealed, and ultimately, in answer to a question in the House of Commons, on August 7th, 1899, the President of the Local Government Board, the Right Hon. J. Chaplin, upset many beliefs by stating that the appeals were allowed and the auditors' decisions reversed, "the Board having been advised by their own legal adviser that the reasons assigned by the auditors for making the disallowances did not support the action they took in point of law." Hence, after

<sup>1</sup> Subsequently, the auditor must have amended his decision in respect to this, for according to Mr. Chaplin's statement in the House of Commons, the only sums disallowed were the small amounts for the diviner's fees. The best accounts of the Porthcawl case will be found in the lengthy reports in the *Cardiff Western Mail* for July 20th, 1897, *et seq.* The Ampthill case will be found fully reported in the *Bedford Times* for June 5th, 1897, *et seq.*

<sup>2</sup> A few exceptions to this general outcry were to be found in some journals, notably *Truth*, the *Evening Standard*, and the *Local Government Journal*.

this, Urban and Rural District Councils are left free to employ any competent water-finder they choose to select, whatever method he may employ. This is surely a wise decision, founded, I expect, less upon legal views than upon the experience which Mr. Chaplin and other members of the Government have had of the usefulness of dowzers on their own estates.

### § 3. *Percentage of Failures.*

This leads on to the question of the percentage of *failures* of different dowzers. I have tried to obtain from some of the professional "water-finders" a record of all the places they have visited in the course of say twelve months, so that by inquiry at each place I might ascertain the number of successes and of failures. But it is impossible to know if the record sent is a complete one. I have received from two dowzers a long list of persons and places, and have written to each person named with the result that all these cases were more or less successful, but the failures are just the ones that are not likely to be sent. Mr. Gataker frankly says in a letter to me that he has had occasional failures, which he cannot account for except by his state of health. The dowser very often explains away a failure by saying the depth is merely greater and the quantity of water less than he predicted, but all depends upon how much greater depth or how much less water. It would be much better if each dowser would keep a faithful record of all his engagements, and then take the public into his confidence, and give the results of his work and references to each of his employers. This, I suppose, we can hardly expect in the present state of human nature.

An estimate of the number of failures on the part of professional dowzers may perhaps be obtained from the extra charge some of them make when sinking wells on the "no water, no pay" system. That is to say, they agree to sink a well for a certain sum at the spot they have selected by the rod, and if water is *not* found at the place they predicted, and in about the quantity they estimated, then no charge is made; the loss falls on the dowser. If however, all turns out satisfactorily, a somewhat higher charge is made for sinking the well than an ordinary well-sinker would charge, or if no guarantee be given by the dowser. This extra charge varies with different dowzers, and is of course their insurance against failure; it expresses, however, something *more* than the percentage of absolute failures, for water may be found, but the supply may be insufficient, or the depth may be greater than was allowed for in the contract.

I have inquired what this extra charge is, and, as might be expected, it varies. Mr. Gataker makes an additional charge of 25 per cent. on the "no water no pay" system; a similar additional rate is, I



understand, charged by Mr. Chesterman ;<sup>1</sup> Mr. Tompkins in reply to my inquiry, says: "25 to 30 per cent. extra would cover the risk"; Mr. Stone says he does not work on that system, but would be willing to undertake work on the terms of only charging out of pocket expenses, in addition to his fee, if water is not found.<sup>2</sup> The late John Mullins (see p. 75 of my previous Report) made no extra charge, and (as a rule) was willing to forego all payments if after sinking the well a sufficient supply of water was not found. His sons, who carry on the business, and one of whom is a dowser (see p. 98 of last Report), inform me "we do not generally make any extra charge for giving a guarantee of finding water ; on three occasions only we charged 15 per cent. extra on the contract for making the well." They enclose me copies of some of their contracts for large amounts ; several of these, I notice, contain the clause "in the event of not obtaining a supply of 1,000 gallons per day of twenty-four hours, we will make no charge for the work done." But a thousand gallons per day is a very small supply for expensive operations, and contaminated surface water might easily yield this amount.

I am inclined to think we may take from 10 to 15 per cent. as the average percentage of failures which occur with most English dowsers of to-day, allowing a larger percentage for partial failures, meaning by this that the quantity of water estimated and the depth at which it is found have not realised the estimate formed by the dowser. This latter, however, though of great practical importance, is foreign to our inquiry, for what we want to know is whether, after making every allowance for shrewdness of eye, chance coincidence, and local or geological knowledge, the dowser has any instinctive or supernormal power of discovering the presence of underground water. The answer to this question is to be found by a study of the cases cited in this and the previous Report, and is discussed in a subsequent Part (p. 303). Here I will only add that the reader who has followed me will have seen how absurd is the dowser's notion that the mere twisting of his forked twig is an infallible indication of underground water. As I have already shown (Vol. XIII., p. 253), the movement of the twig is due to a sub-conscious *suggestion*, and this may arise from many causes besides the assumed influence of underground water on the dowser.

<sup>1</sup> In a recent contract between the District Council at East Knoyle and the dowser, Chesterman, so many gallons per twenty-four hours were stipulated for, and a certain depth not to be exceeded. The contract was taken at ordinary rates, Chesterman to receive 25 per cent. extra if he succeeds and to pay all if he fails: as the District Council have, I understand, already lost £150 over a well sunk upon geological advice, they have made a good bargain.

<sup>2</sup> This is a most unsatisfactory arrangement, for the dowser, being the contractor, makes himself the judge of what is an adequate supply of water and what the depth of the boring should be. I have heard some complaints of Mr. Stone in this respect.

## PART IX.

CONTINENTAL, COLONIAL, AND CALIFORNIAN CASES OF THE USE OF THE  
ROD IN THE SEARCH FOR UNDERGROUND WATER.§ I. *Continental Dowzers.*

In the previous Report, on p. 23, I mentioned that a French writer had published in 1850 a little book called *Histoire de l'Hydrosophie et de la Baguette Divinatoire*, and I spoke of the author, who was in early life a successful and, I believe, professional dowser, as "a M. Mortillet." I was unaware at the time that the author was the same person as Professor Gabriel de Mortillet, who has a European reputation, and is one of the most distinguished anthropologists in France. I therefore owe an apology to Professor de Mortillet, and expressed this in sending him a copy of the Report. It is often remarked that it would be of great value if a competent scientific man were also a dowser, and could give us his personal experience. Here we have a case in point, and to this may be added the case of Mr. Enys, F.G.S., the President of the Royal Geological Society of Cornwall, a letter from whom is quoted on p. 219. Professor de Mortillet's little book is not now accessible; the author, in reply to my inquiries, writes to me as follows:—

St. Germain-en-Laye, près Paris, 18 Décembre, 1897.

Je regrette beaucoup de ne pas pouvoir vous envoyer mon *Histoire de l'Hydrosophie*. Je n'en possède pas même un exemplaire chez moi. Je vais m'informer s'il en existe encore. Elle a été publiée à Chambéry; si j'en retrouve, vous la recevrez.

Permettez-moi de vous faire de sincères compliments sur votre *On the So-called Divining Rod*. Je n'ai fait que parcourir ce mémoire, mais je compte bien l'étudier avec tout le soin qu'il mérite.

Je mets à la poste, à votre adresse, en même temps que cette carte, un numéro de *L'Intermédiaire de l'Afas*,<sup>1</sup> contenant, page 23, le résumé de mon opinion sur la Baguette et le Pendule. Je vais annoncer votre mémoire dans un prochain numéro de *L'Intermédiaire de l'Afas*.

Agreez, cher Professeur, mes remerciements et l'assurance de mon haute considération.

G. DE MORTILLET.

<sup>1</sup> The word *Afas* is taken from the first letters of the Association Française pour l'Avancement des Sciences; this excellent journal is regularly issued as an organ of "Notes and Queries" to the members of the Association.—W. F. B.

As the summary Professor de Mortillet gives of his views on the *baguette* is of general interest, I append the following translation of the communication he refers to in *L'Afas*, February, 1896:—

The name “hydroscope” has been given to those persons who experience certain special sensations on passing over subterranean watercourses. Sometimes, though very rarely, the sensation is sufficiently strong for the subject to be directly conscious of it. In most cases hydrosopes are compelled to use an instrument in order to recognise the fact. The instrument employed is either a pendulum (*pendule*) or a rod. The pendulum is carried in the hand, so that the least trembling of the arm sets it in motion. As to the rod, it is a twig slightly bent, which the operator rests upon the index fingers of each hand so as to balance it. The least motion of the fingers towards or away from each other is then sufficient to displace the centre of gravity, which, passing from the centre of the rod towards the ends, and from the ends towards the centre, naturally causes the rod to turn. But this is not the true divining rod. This last is a stem, dividing into two branches nearly of equal size, and forming as acute an angle as possible. The end of a branch is grasped in each hand, and it is bent back in such a manner as to be in tension (resisting the pressure of the hand). These recurved and bent branches endeavour to spring back to their original straight line, which causes their point of union to move. To use this rod, an equilibrium as unstable as possible is set up, so that it may be disturbed by the least nervous jar, which will cause the instrument to move. Such are the principles governing the pendulum and the rod. The nature of the wood is a matter of indifference. Hazel is preferred, because it forks more frequently, and the rods are more flexible than is the case with other trees.

Pendulums and rods move over certain points in the hands of persons who are especially sensitive. These points are supposed to be over underground watercourses. As a matter of fact, this is generally, but not always, the case. I have seen important works undertaken merely upon indications furnished by hydroscopy, with full success. But, at other times, I have seen it fail.

This much, however, is certain, which I can affirm as the result of experience, that the point chosen by one diviner will also be chosen by others brought from a distance, and completely ignorant of the preceding experiments. A real phenomenon to study does therefore exist.

What has discredited this phenomenon is the attempt to employ it in moral matters, such as the search for robbers, assassins, etc.

I would call special attention to the penultimate paragraph, as expressing the opinion of a distinguished man of science who knows what he is talking about.

It will be noticed that Professor de Mortillet refers to the use of a *pendule* as well as a *baguette*.<sup>1</sup> The former is not, I believe, anywhere

<sup>1</sup> I have referred to the so-called *pendule explorateur* in the previous Report, p. 10. It consists simply of a ring or little ball, suspended by a thread which is held between the fingers, and, thus held, the involuntary motion of the hand sets it



employed by dowzers in England, but it is in use in some parts of America, and is described in an article in an American journal, the *Democratic Review* for 1850. The writer calls himself "An Old Rodsman," and states that some diviners in searching for metals use a forked twig, or two pieces of whalebone, and others a "small metallic ball suspended by a horse-hair or silk thread. As the operator approaches the hidden mine, the ball deviates more or less from the perpendicular, and this points out the proximity of the object sought." A very similar *pendule* is referred to in the next case, as employed by some French *sourciers*, or water-finders. These *sourciers*, I am informed by those in the South of France to whom I have written, are to be found in Dauphiné, Savoie, and Beaujolais, but in smaller numbers than formerly. A friend in France writes as follows :—

Lyons, *July 2nd*, 1898.

M. Raoul de Cazenove (a relative of Dean Cazenove) lately built himself a château, and after all was finished, found he had everything heart could desire except water. He knew of an old farmer reputed as a "sourcier," and went to him. He consented to come, but as he expressed it, "for love, not for money, as I never intended doing it again ; it exhausts me so terribly." He came, found, and was exhausted, so much so that they had much ado to fit him for returning home.

I wrote to M. de Cazenove, who kindly sent me the following particulars :—

Château du Solier, La Salle, Gard, *July 14th*, 1898.

I am happy to send you the few particulars I possess on the subject. Answering your two questions, I may say, 1st, since thirty years or so, I have had on many occasions to ask the "sourciers'" services. I have asked their help fourteen times ; their researches have been successful in nine cases. Our country is very irregular, abrupt and rocky ; so no wells have been bored. Water is to be found in digging horizontal galleries ; most of these go from twenty to one hundred metres [about 66 to 330 ft.] ; many are bored in granite ; some others in "calcaire" [limestone].

2nd. When I have observed the "sourcier" at work he feels a particular sensation, or *malaise*, which increases as he approaches the hidden water. Some use a rod ; many of them a heavy watch, hanging by its chain ; some of them don't use any instrument. We have a lot of "sourciers" in the country (mountains of the Cevennes) ; some of them are amateurs, many are professionals. Their estimation of the depth and volume of water is quite empirical ; hence mistakes and frequent deceptions result. Sometimes their

oscillating and appears to endow the *pendule* with a sort of intelligent activity. A considerable literature has arisen over the *pendule*, a toy which afresh excites the astonishment of succeeding generations. It is really a simple form of "autoscope," and can claim a remote antiquity, divination by an oscillating ring or *pendule* having been used by the ancients ; it was employed by Hilarius in the 4th century to ascertain who would be the successor to Valens ; Chevreul, in his *Baguette Divinatoire*, pp. 132 *et seq.*, gives the whole passage.

long experience furnishes them with sufficient indications, but they have no rules ; only personal and nervous feeling.

Many professional "sourciers" exist in France in various parts. I remain at your disposition for further inquiries.

R. DE CAZENOVE.

*Continental "Hydrosopes" in the Early Part of the Century.*

One of the most successful water-finders in the early part of the 19th century was the French Abbé Paramelle; he did not employ a rod, but trusted to his instinct, guided by a knowledge of hydro-geology which he had gained through careful observation. I have devoted an Appendix to Paramelle (see p. 352), as his case forms an instructive contribution to our research.

The subject of Continental dowzers would be incomplete without reference to the writings of Thouvenel, Amoretti, Count Tristan and Baron Du Prel. Dr. Thouvenel I have already referred to, and his experiments with the dowser Bleton are given in the next Part. Thouvenel, towards the close of the 18th century, went to Italy and there experimented with another dowser, or "hydroscope," a lad named Pennet,<sup>1</sup> who professed to find buried metals and coal, as well as underground water. Some of the results were so surprising that the great naturalist, Spallanzani, was much impressed, but as Pennet was, according to a letter published by Biot,<sup>2</sup> subsequently found tricking at Florence, Spallanzani concluded he also had been tricked and withdrew his former opinion. I do not know whether Biot had reliable evidence for his statement, for we all know that with many sceptics any evidence is thought good enough, if it serves to discredit phenomena new to science.

Amoretti, who was an Italian *savant* of some note, published various papers on his experiments with the Divining Rod, chiefly with two dowzers, one the lad Pennet, and the other, a boy, Anfossi.<sup>3</sup> In Vol. XIX. of the *Opuscoli Scelti*, Amoretti, in a letter to Fortis,

<sup>1</sup> See an article by Dr. Thouvenel in *Mélanges d'histoire naturelle de physique*, etc., Vol. III., Paris, 1807.

<sup>2</sup> Reprinted in 1858 in his *Mélanges scientifiques et littéraires*, tom. II., p. 80. In the Bibliography of the Divining Rod, which Mr. Westlake has with such immense industry prepared, numbers of papers in French, Italian, and German will be found between 1800 and 1810, arising out of Dr. Thouvenel's investigations.

<sup>3</sup> Amoretti's papers on the divining rod, in the British Museum, are mainly found in the *Soc. Ital. delle Scienze* for 1813 and 1816, and earlier papers in the *Opuscoli Scelti sulle Scienze* from 1798 to 1804. The later papers were published separately under the title of *Della Rabbomanzia ossia Elettrometria animale*. Part I. of this book was translated into German, and a paper on the same subject by J. W. Ritter added, with plates showing various rods held both by hands and feet. Some of Amoretti's papers were also translated into French and published in Fortis' *Mémoires*.

dated December, 1796, states : (1) That an Augustinian friar had, by means of the rod, found an underground spring, which had since supplied the country house of Signor Dolbecchi ; (2) that one Sanzio, a surgeon, found his pulse rose twelve to fifteen beats faster per minute when standing over subterranean water ; (3) that an insensitive person holding a divining rod became sensitive and the rod moved, when his hands were held by a sensitive [*i.e.*, a dowser] ; (4) particulars are given of a sensitive (a boy) who could locate underground water by his feelings, without using a rod : Pannet and Anfossi (other sensitives) also said the same. Amoretti, like Professor Sementini, Ritter, and others, believed in the close connection of electricity with the movement of the rod. The Count de Tristan was likewise fully convinced of the electric origin of the dowsing faculty ; his knowledge of physics was, however, very slight. In 1826 he published the result of numerous experiments with the divining rod in the valley of the Loire in a book called *Recherches sur quelques effluves terrestres*. An outline of these experiments is given by Dr. Mayo, F.R.S., in his excellent book, *Truths Contained in Popular Superstitions*, and by Chevreul in his *Baguette Divinatoire*, pp. 122 *et seq.*

The influence of "suggestion," whether conscious or unconscious, was not considered nor guarded against by any of these experimenters, nor was the effect of involuntary muscular action sufficiently recognised till later. Hence Amoretti, Ritter, Tristan, and very often Thouvenel, were led to attribute to animal electricity phenomena that were mainly due to suggestion. Thousands of experiments have been made and volumes written on the divining rod and the *pendule* (or magic pendulum), all of which are valueless owing to the non-recognition of this source of error ; I will, however, return to this later on.

In the autobiography of that learned and singular man, Zschokke, an account is given on p. 143, of several dowsers (or Rhabdomantins, as Zschokke calls them) whom he had met in Switzerland ; they are to be found, he states, in almost every canton. Dr. Ebel, of Zurich, a well-known geologist, sent Zschokke one of these dowsers to test ; he adds that the Abbot of St. Urban, in Lucerne, was a successful dowser. Zschokke was at first very incredulous, but his incredulity disappeared after the numerous tests to which he subjected a young female dowser.

#### *The German Dowser, Beraz.*

Du Prel, who is best known by his suggestive work, *Die Philosophie der Mystik* (admirably translated into English by Mr. C. C. Massey), published a short essay on the Divining Rod (*Die Wünschelruthe*) in



1890. In this essay Du Prel gives an account of Beraz, a notable German dowser. The following is a translation of this part :—

[In 1888] a friend in the South asked me to call upon Beraz, the spring-finder (who is since dead, but who lived at that time in Munich), and to make arrangements with him. It was necessary to find springs in a newly laid out health resort, which was unprovided with a water supply. My friend gave me (the quite unnecessary) advice, not to consult any learned professors, etc., on the subject of Beraz. I visited Beraz, and while waiting for his appearance, noticed—amongst others—the portrait of a gentleman hanging in the room. This gentleman, Beraz informed me, was his maternal grandfather, Professor Ritter; I naturally asked whether he were the author of the *Siderismus*, and was answered in the affirmative.<sup>1</sup> The testimonials that Beraz received are sufficient testimony to his powers. As the subject is one of great interest to the public generally, and to land-proprietors especially, I give a few dates of the finding of springs, with the places at which they were found, and addresses at which any one interested may obtain full proof for the asking :—The Capuchin Monastery on St. Nicholas-berg in the Würzburg (1877); the Corporation of Gart, near Traunstein (1876); the Institute of Young Ladies at Altötting (1882); Narr's Brewery in Zirndorf, near Nürnberg (1875), the Community of Algrund, near Meran (1882).

I have conversed with a person who witnessed one of Beraz's experiments on an occasion when he announced that there was a spring at a depth of 80 ft., and the spring was found at 83 ft. I do not doubt but that Beraz was sometimes unsuccessful, but, on the other hand, I am sure that many an enlightened corporation has lavished great sums on bringing water to their towns, which they might have obtained much more reasonably by employing a spring-finder.

In the London *Times* for February 11th, 1885, I find the following account of water-finding by Beraz in Bavaria :—

The *Allgemeine Zeitung* gives some interesting particulars of remarkable success in indicating the presence of water springs on the part of a man named Beraz, who seems to be a recognised authority in such matters. The scene of his performances was in the Bavarian highlands, at a height of more than 1,300 ft. above the level of the sea. The commune of Rothenberg, near Hirschhorn, suffered greatly from want of water, and invited Beraz last autumn to endeavour to find some source of supply for them. He inspected the locality one afternoon in presence of the public authorities and a reporter of the *Allgemeine Zeitung*, and announced that water was to be found in certain spots at depths which he stated. The first spot was in the lower village, and he gave the likely depth at between 62 ft. and 72 ft., adding that the volume of water which the spring would give would be of about the diameter of an inch and a quarter. After incessant labour for four weeks,

<sup>1</sup> Ritter's *Sidcrismus*, Du Prel remarks in another place, had lain uncut in the Public Library of Munich from 1808 to 1887, *apropos* of which he adds very truly "the scepticism of many is merely ignorance." Ritter conducted some investigations on the divining rod in Munich in 1806, with an Italian named Campetti.—W. F. B.

consisting mainly of rock blasting, the workmen came on a copious spring of water at a depth of almost 67 ft. What he declared about a water source for the upper village was very singular. He pointed to a spot where he said three watercourses lay perpendicularly under one another, and running in parallel courses. The first would be found at a depth of between  $22\frac{1}{2}$  ft. and 26 ft. of about the size of a wheaten straw, and running in the direction from south-east to north-west. The second lay about 42 ft. deep, was of about the size of a thick quill, and ran in the same direction. The third, he said, lay at a depth of about 56 ft., running in the same direction, and as large as a man's little finger. The actual results were as follows :—The first watercourse was struck at a depth of  $27\frac{1}{2}$  ft., running in the direction indicated, and having a diameter of one-fifth of an inch. The workmen came on the second at a depth of  $42\frac{2}{3}$  ft. ; it had a diameter of  $\frac{2}{3}$ ths of an inch. The third was found at  $62\frac{1}{2}$  ft. below the surface, and having a diameter of  $\frac{2}{3}$ ths of an inch—all three running in the direction Beraz had indicated.

The statements made by the reporter, who says that he was an eye-witness, are so circumstantial that they have the appearance of being trustworthy, but it is to be regretted they were not the subject of careful inquiry and corroboration at the time ; the more so as Rothenburg is in the very centre of German scientific culture. If any readers of this Report, who may be visiting Bavaria, can give me additional information on this case, I should be grateful.<sup>1</sup> Assuming the facts to be as stated, they are of the greatest interest and theoretic importance, for this case must be classed along with the Waterford case and a few others of a similar type.

In Nasse's *Zeitschrift für Psychische Aertze* for 1821, Professor D'Outrepont, Ph.D., of Würzburg, gives some interesting facts concerning a dowser at Malmedy (a town on the Belgian frontier) with whom he was acquainted. Mr. E. T. Bennett has translated Dr D'Outrepont's paper, and published an abridgement of it in the *S. P. R. Journal* for June, 1899, p. 83. It is therefore accessible for those readers who wish for further information.

## § 2. Californian Dowzers.

Amid the mass of correspondence which has reached me, often from distant parts, I have had several letters from California, where it appears there are several professional ore-finders and water-finders, or "water witches," as they are often locally called.

<sup>1</sup> I have to thank the Editor of the *Times* for sending me the editorial address of the *Allgemeine Zeitung*, to which I wrote in the hope that the reporter referred to might still be on the staff of that paper and able to give me some additional confirmation of his statements. Unfortunately, however, I learnt in reply that this was not the case ; nor could the Editor of the *Allgemeine Zeitung* ascertain who the reporter was ; Beraz, he informs me, is dead (Du Prel had stated this).

One correspondent, Mr. B. F. Dixon, of Escondido, California, the head of a firm of orange growers, writes as follows :—

Escondido, California, *November 22nd*, 1897.

Having read a review of your treatise on the Divining Rod in the papers here, the following facts may interest you.

In the fall of 1865 an acquaintance in Kansas had engaged a person to locate a well on his place. The man held a forked peach twig, which moved strongly at a certain spot. Upon my laughing at such utter nonsense, the man asked me to try. Treating it as a joke, I did so, holding the forked twig as I saw it held by the water-finder. To my astonishment, at the spot where the water-finder had marked, I felt what seemed like a slight electric shock and immediately the forked twig turned violently in my hands. I held the stick as tight as I could to prevent it moving, but it twisted right round to the ground. I was dumbfounded and the joke was turned against me. A well was sunk at the spot and a good supply of running water obtained.

When it became known that I was a "water-witch," my services were sought for and I have since located a large number of wells, without making any charge. I have been surprised to find how accurate the indications of the twig have turned out ; even in the depth I have rarely been far out !<sup>1</sup>

Mr. Dixon states he could give me many instances of his success when previous attempts to find water had failed. He quotes one case of his in considerable detail, which I will abridge. An acquaintance of his in Kansas had spent large sums in sinking several wells on his 40 acre farm, but had got no water, though some of the wells were 70 ft. deep. At last he begged Mr. Dixon to come over and try his hand. Mr. Dixon went and prospected with his peach twig ; he was surprised to find the twig move strongly at one spot, indicating a plentiful water supply, which he estimated at only 18 ft. deep. It suited the owner's purpose better to sink a well a few feet from this spot and at a depth of 22 ft. water was found streaming in from the direction indicated by Mr. Dixon. As the well caved in shortly after, the owner decided to sink a walled-in well at the very spot marked by the twig, and at less than 20 ft. from the surface came upon so abundant a supply of water that it has served the whole farm ever since. I have no reason to doubt the accuracy of Mr. Dixon's statements, but it is desirable to have them confirmed by independent observers, and therefore I should be grateful to any of our friends in California who may live near Escondido if they would kindly send me some further particulars.

Concerning one Californian dowser, a Captain Godfrey, I have received an enormous mass of correspondence, forwarded to me by

<sup>1</sup> Mr. Dixon does not say *how* he ascertains the depth by the motion of the twig.



Dr. Hodgson and various other friends. Some of the evidence is remarkable, and I hope to find time to give an abridgement of it at a future time. Captain Godfrey declares he is so powerfully affected by the neighbourhood of underground veins of ore or running water that he is made quite ill. He attributes it all to electricity (!) and his wife writes voluminous letters, asking for scientific enquiry. This I have attempted to obtain, but Captain Godfrey declines to submit to any tests. Dr. Hodgson has taken much trouble over this case, and referred me to one of the members of the S. P. R. in California, Mr. Radcliffe-Whitehead, who had recently visited Captain Godfrey. I wrote to Mr. Whitehead, and had the following reply :—

P.O. Box 144, Santa Barbara, California, March 6th, 1898.

I called on Captain Godfrey in Pasadena, where he lives, two months ago. He is a fine old man, and from what I have seen and heard I should judge him to be above all suspicion of fraud. From the statements made to the Society and similar ones made to me, I have no doubt that he has some curious faculty, in fact, is a *sensitive*.

The difficulty of going further with this case lies thus. Captain Godfrey says :—

1. "I don't care a rap for the scientific aspect of my case."
2. "I suffer physically when I experience the sensations given me by underground streams of water or veins of metal."
3. "I don't care to make experiments, nor will I seek for water for a fee. I will only do so if I am to have a share in the 'development' (as we call it here) of said water."

Now I, as a developer of water on my own land,—I am making two water tunnels at this time,—should be willing enough to employ Captain Godfrey to "locate" water for me for a fee, but I am not disposed to agree to his terms, though I have complete faith in his honesty.

I will write to him shortly and try to induce him to change his mind, but as he has lately refused a neighbour of mine who made a similar proposal, I confess I have not much hope. However *nous verrons*. I will report.

RALPH RADCLIFFE-WHITEHEAD.

In a subsequent letter Mr. Radcliffe-Whitehead informs me he has failed to induce Godfrey to change his mind, and mentions another Californian dowser he knows, and is about to employ. Another one of our Californian members, to whom I had written about Captain Godfrey, after undertaking to do what he could, writes as follows :—

2,727, Ellendale-place, Los Angeles, California, U.S.A.,

February 17th, 1898.

My own experiments have been with a man who is a little ashamed of his power, and does not attempt to explain it. He is honest beyond question, and has submitted to such complete tests—being tested when blindfolded ;

finding buried waterpipes ; and by the actual discovery of water in places where it was least to be expected—that I am reluctantly forced to believe that some such power exists, especially as a complete test satisfied me that it was not a case of mind-reading.

I have no knowledge of divining for water without a rod, but one of my farmers, who is an honest and clear-headed fellow, tells me that in his Iowa home they found all their wells by aid of a man who almost had convulsions when passing over running water. He gives some curious cases of wells so found, after attempts to get water in the same farms without his aid had failed, and one case where the man had a fit at a house-warming, and declared that the trouble came from a spring under the house. On digging, a powerful and valuable spring was discovered near the surface. If you value such evidence, I shall be glad to get the exact facts and the man's name for you.

WALTER NORDHOFF.

Various instances of the use of the rod for finding underground water in other parts of the United States and in Canada were given in the previous Report on pages 27, 44, 59, 62, and 215-220.

### § 3. *South African Dowisers.*

The use of the divining rod has even extended to Bulawayo. In an article in that able English journal, the *Surveyor*, which refers to the "cold scrutiny" and "careful dissecting" to which the diviner has been subjected in our previous Report, the following instance of the use of the rod in Bulawayo is quoted from a correspondent to a South African newspaper :—

Having sunk on the spot located through the medium of the divining rod, I have, at a depth of 30 ft., struck the water ; whereas my neighbour, at considerable expense, two years ago sank, on the same line and only 30 yards away, to a depth of 45 ft. without any result whatever. I am now convinced of the great expense which the rod may save in sinking for water.

This must be taken for what it is worth, as no names are given. In South African journals for August, 1897, particulars are given of a clerical water-finder.

"It will be remembered," says the *Grahamstown Journal* "that some months ago the Rev. Father Marconnes, of St. Aidan's College, who is an adept in the use of the divining rod, made certain predictions as to the finding of water in certain spots, which he pointed out. The Government steam drill, under Mr. J. L. Caithness, has now found water just outside the Chronic Sick confines at Cape Corps Camp on the Commonage. The water was found at a spot pointed out by the diviner. It was first tapped at 21 ft. by an excavation Mr. Caithness was making to get his bore loose, and at 37 ft. a supply of quite 6,000 gallons per diem was tapped. The water is good and fresh. Again, at the St. Andrew's College, where Father

Marconnes pointed out a spot, and said there was water at 40 ft., the precious fluid has been tapped exactly at 40 ft., and in good quantity."

Father Marconnes writes in a subsequent number of the *Grahams-town Journal* that he prefers to call his divining rod a water compass, or hydro-magnet, as it is a "scientific instrument," and clearly indicates that electricity is the true cause of its action; he does not, however, give any evidence in support of his belief. I do not quote this case as of any evidential value, for I have not been able to verify it, and moreover there is nothing to indicate that water would not have been found without Father Marconnes' aid, but I give it simply to indicate the widespread use of the rod.

As is well known, the divining rod was frequently used during the 17th century for tracking suspected persons, and for other purposes in the "moral world." Among the native races in South Africa the rod appears to be still in use for a like purpose. Though outside the range of the present Report, the following incident in connection with the siege of Mafeking has attracted so much attention that I will briefly refer to it here. A Press Association telegram appeared in the English newspapers of April 18th, 1900, to this effect:—

*April 5th.*

Lieut. Frank Smitheman, the renowned Matabele scout, got through the Boer lines, and arrived here yesterday, carrying an Imperial Government despatch for Col. Baden-Powell. He had an adventurous journey, spending two nights in coming from Ramathlabama on foot.

Lieut. Smitheman, who knows every inch of the country, was accompanied by a clever native runner, who uses a divining rod to ascertain the position of the enemy. This rod he handles after the fashion of the Wiltshire water-finder, thoroughly believing in its efficiency.

Lieut. Smitheman's feat is one of the utmost importance. Only two whites have entered the town since the beginning of the investment.

Two centuries ago (in 1692) the famous dowser, Jacques Aymar, became notorious by his discovery (through following the indications of the rod) of the murderer of a poor wine-seller in Lyons, and in 1703 he was employed to identify the Camisards by the same means. Since then (owing chiefly to the rod being condemned for any use in the "moral world," see p. 258) its employment for criminal purposes has disappeared in Europe. Inquiry may prove that the South African report is not to be credited, or the "diviner" worthless, but the case of Jacques Aymar is as well attested as any fact in history.<sup>1</sup> I have in my possession a mass of original documents, published at the time, which place the really extraordinary character of Aymar's discovery of

<sup>1</sup> The best modern account of this story is given in Figuier's *Histoire du Merveilleux*, Vol. II., Chap. 5.



the murderer of the Lyons wine-seller beyond doubt. Nor is there any improbability in the circumstance, if we here regard the dowser as a bloodhound following a trail, which his sub-conscious self picks up and indicates through the involuntary motion of the rod he carries.

#### § 4. *Australian Dowsters.*

In the previous Report (Vol. XIII., pp. 45-47) I gave an account of four successful amateur dowsters in Queensland and New South Wales. Several other cases of Australian dowsters have since reached me. Two cases are of such interest that I will briefly refer to them, as in both these cases the dowsters were gentlemen of good standing and intelligence.

(1) The following account has been sent me by Mr. Vaughan Jenkins; it was published in the *Sydney Stock Journal* and in the *Armidale Chronicle* (a New South Wales paper) for April 14th, 1900. The report is too long to quote, and I give a summary of its essential parts.

The President of the Armidale Cattle Show, Mr. F. J. White (a well-known man), was greatly in need of a water supply on his estate, Saumarez. For twenty-four years that his family have lived there they have had to cart water from a distance, as did their predecessor, Mr. Thomas, attempts to find water on the place having failed. Eventually Mr. White sunk a deep well, 120 ft. deep, through the solid rock, but no water was reached. A friend who was visiting the place happened to be an amateur dowser, and, seeing the useless well, offered to try his luck with the divining rod. He did so, and the rod turned vigorously at a certain spot. Here another well was sunk, and at 35 ft. water was struck, it rose to 20 ft. from the surface, and now supplies the place and the gardens. Mr. White then tried the rod himself and found to his surprise it turned in his hands: trusting solely to its guidance, he has since sunk six more wells, and found water in every spot so indicated. He is now so firmly convinced of the value of the rod that he has recently offered to pay the cost of digging any well if its indications turn out wrong. The writer of the article went as a sceptic to investigate and report, but his amused incredulity became astonishment when he found the twig turned in his hands also. He remarks: "I don't believe in that twig business, but the facts are against me. Will somebody explain?"

The foregoing newspaper report must be taken for what it is worth, as I have not had time to verify it by independent inquiries.

(2) The other case I have received through the kindness of a West Australian friend, Mr. E. T. Scammell, 37, St. Mary Axe, London, E.C. The dowser in this case was his friend, the late Mr. E. H. Derrington, for many years an Australian journalist of high

standing and unimpeachable integrity. At my request Mr. Scammell obtained a lengthy narrative, accompanied by the corroborative testimony of independent witnesses, from Mr. Derrington. This account is of interest from the fact that Mr. Derrington became a very successful dowser for mineral lodes, particulars of which will be given in that part of our work. Mr. Derrington writes:—

Coolgardie, W. Australia, *May 24th, 1898.*

Newspaper editorship during many years predisposed me to regard with strong scepticism all fads and faddists. I, with courtesy, declined even to witness experiments with the so-called “divining rod”—a name as absurd and untrue as any I have ever heard, since there is no divination in the whole process. Ignorance and credulity may still employ it, but no educated man would employ it. But, some fourteen years ago, there occurred in the colony I have made my home for now nearly half a century, one of the periodical droughts for which unfortunately South Australia has become noted. The misery and ruin in the out-lying agricultural districts were forced upon my attention, and then the pretensions of the water-finders came to my mind, and I resolved upon an impartial investigation, remembering what a priceless boon would be conferred if hidden supplies of water could be discovered in the waterless districts. I applied to an old German of most respectable antecedents and strong common-sense, who was renowned as a successful exponent of the art. Without giving me any illustration, he at once placed in my hand a V-shaped eucalyptus twig, and whilst he remained at the entrance-gate, requested me to cross from one side to the other through his garden. I did so, and midway was astonished by the twig (held by the points in the forks of the thumbs, the fingers being closed over it in the palms of the hands) suddenly up-rising and standing perpendicularly, *entirely without my volition*, and falling back into its horizontal position when I had passed one particular spot. I repeated the experiment a number of times with the same result, only that I noticed a singular sensation as of an electric current through the spine whenever the twig moved as described. My friend watching then showed me that I had stood over a concealed water-channel fed by a spring from the upper part of his garden, and complimented me upon being the most sensitive water-finder he had ever met. This was my first acquaintance with this unknown faculty. I laid aside scepticism and eagerly made use of this method to assist the distressed farmers with whom I came into contact.

I have since made very numerous experiments, with a view to an accurate estimate of the cause and effect; and I have submitted to a variety of crude tests, many of which were designed less for the discovery of a scientific basis than to throw ridicule upon an obscure phenomenon. Pseudo-scientists are prone to sneer at what they cannot understand, but I hold there is in ascertained results sufficient ground to justify the hope that, at some not far distant future, a possibility of effecting economic results of incalculable value in the discovery both of underground water and ore by means of the so-called divining rod.

As to the nature of the influence which moves the rod, I am inclined to believe that in certain sensitives there may possibly be set up a current or

influence by the electrical currents in the underground water or metallic veins. For I discovered subsequently my "galvanometer," the rod, moved whenever I was over metallic veins or even metals on the surface of the ground.

E. H. DERRINGTON.

In the foregoing narrative no evidence is detailed to show that Mr. Derrington's predictions were verified by digging or boring, nor that he succeeded when other means had failed; he adduces, however, some striking evidence of his success in the discovery of mineral lodes, but this part of his letter I will give in another place. His electrical theory sounds a plausible one; it is much the same as Dr. Thouvenel's a century ago, but is unsupported by any scientific evidence and will be referred to later on. I may add that in the recent obituary notices of Mr. Derrington in the South Australian newspapers, a high tribute is paid to him; he was not a man likely to be led astray by hasty inferences as to the value of the indications afforded by the divining rod.

I now propose to give a somewhat detailed account of that remarkable French peasant lad, Bleton, whose performances as a water-finder Dr. Thouvenel has made famous.



## PART X.

## BLETON, THE NOTABLE FRENCH DOWSER OF THE LAST CENTURY.

Barthélmy Bleton, to whom reference has already been made (on p. 20 *et seq.* of the previous Report) was born at Bouvantes in Dauphiny, somewhere between the years 1740 and 1750.<sup>1</sup> He was the son of a poor peasant, was brought up by charity in one of the Carthusian monasteries of Dauphiny, and became a herdsman. An accidental circumstance seems to have led to the discovery of Bleton's peculiar faculty. Here is the account which I find given by one of Dr. Thouvenel's correspondents, who writes from Dijon on April 14th, 1781, and who adds he has certain proofs of all that he has stated.<sup>2</sup> The following extract is translated from the original letter :—

Bleton, when seven years of age, had carried dinner to some workmen ; he sat down on a stone, when a fever or faintness seized him ; the workmen having brought him to their side, the faintness ceased, but each time he returned to the stone, he suffered again. This was told to the Prior of the Chartreuse, who wished to see it for himself. Being thus convinced of the fact, he had the ground under the stone dug up ; there they found a spring, which, I am told, is still in use to turn a mill.

A similar account is given by another contemporary quoted by Figuier, and by the writer in the *Monthly Review* for 1782, who quotes from a French pamphlet, the writer of which states the circumstance is "confirmed by many local witnesses." Some confirmation of the foregoing story is gained from a remark made by the Prior "de la Chartreuse de Lyon" in a document testifying to Bleton's extraordinary faculty (quoted by Thouvenel) that Bleton "was quite as well able to detect underground water when he was seven years old as he is now." (March 3rd, 1781.)

In any other part of the world except Dauphiny the coincidence of the boy's illness and the presence of an underground spring would

<sup>1</sup> The exact date of his birth is uncertain, nor is it of any consequence. The above date is deduced from an incidental observation in the evidence about to be cited. In 1773, as appears from another witness, Bleton's faculty was so well known that he was then in request as a water-finder or *sourcier*. Our principal knowledge of Bleton, as will be mentioned presently, is derived from the writings of Dr. Thouvenel, one of Louis XVI.'s physicians ; he always spells the word Bleton, not Bléton.

<sup>2</sup> See Dr. Thouvenel's *Mémoire Physique et Médicinal*, 1781, p. 251 ; *Monthly Review*, 1782, Vol. LXVII., p. 554 ; Figuier's *La baguette divinatoire*, p. 365. Figuier quotes from a letter written by a contemporary of Bleton to the Editor of the *Journal des Spectacles*.

probably soon have been forgotten. But, a century before, Jacques Aymar and his rod had made this province famous for its diviners, Aymar having been followed by many who claimed a similar "gift." As, however, the Inquisition had forbidden the use of the rod in the "moral world," that is, for tracing criminals, or determining boundaries, or settling lawsuits, etc. (its use for these purposes having become a most mischievous superstition and scandal), these diviners, or *tourneurs*, were chiefly water-finders, or *sourciers*. Bleton was therefore at once considered to be a new and sensitive *sourcier*. Some tests followed which confirmed this view. Doubtless, custom demanded that he should have a rod, and the subsequent use by Bleton of a nearly straight rod, resting on the forefingers of each hand, satisfied the sense of *sourcier* propriety. The rod was very slightly curved, and rotated more or less rapidly on its axis when Bleton came over an underground spring. Dr. Thouvenel states he counted from 30 to 80 revolutions per minute, and upwards.

The rotation of the rod was doubtless caused by involuntary muscular action on Bleton's part. There is no reason to suppose he moved it intentionally; in fact, it is a very difficult, if not impossible, feat to accomplish by volition, except to one long practised in sleight of hand, as any one can prove who supports a short slightly-curved stick on his forefingers, and attempts to cause it to rotate from "30 to 80 times a minute." On the other hand, there is no reason to assume any other motive power than muscular action, as the famous astronomer Lalande showed that Bleton's rod could be made to rotate by this means. To this I have already referred in my previous paper, and will quote Lalande's own words later on, on p. 273.

How Bleton came by this novel form of rod is not told us.<sup>1</sup> Possibly it was a survival in Dauphiny of an old type of the *virgula divina*. A straight rod, supported horizontally between the forefinger and thumb of each hand, is described and depicted on the title page of that interesting old book on the divining rod, *La Verge de Jacob*<sup>2</sup>

<sup>1</sup> The only notice I can find of the peculiarity of Bleton's rod is in a letter "from a distinguished physician" cited by Thouvenel in support of Bleton's faculty. The writer says (p. 277 of Thouvenel's *Memoire*) that although nearly all the *tourneurs*, i.e., dowzers, he has known, use a forked rod, yet some in Germany use a simple rod, very slightly curved, placed across the back of the hand, "ou comme Bleton, sur l'extrémité des deux doigts indicateurs." This reference to Germany is probably taken from De Vallemont's *Physique Occulte* (1693) where there is a picture of a man holding the rod in this way (see next woodcut). Still earlier, in 1640, the learned German Jesuit, A. Kircher, describes a straight-pivoted divining rod which rotates when held horizontally between the forefingers of each hand (see upper figure on the wood-cut, Fig. 17). This, he remarks, he has seen used by some German diviners: see Kircher's *Magnes sive de arte Magnetica*, p. 724 of the 1640 edition.

<sup>2</sup> It need hardly be said that this, like every other early work on the divining rod, is perfectly worthless from the point of view of modern science.

(see D on the woodcut, Fig. 16), which was published at Lyons in 1693. In the third chapter of this book the writer says that in order to ascertain if a person really has the faculty of finding a hidden spring, he must let a straight stick rest across his hands, in the manner shown in the lowest figure of the woodcuts on this and the next page; if he has the faculty the stick begins to rotate.<sup>1</sup> Bleton was probably tested in this way, and subsequently used this rotating rod as the outward and visible sign of the inward commotion he

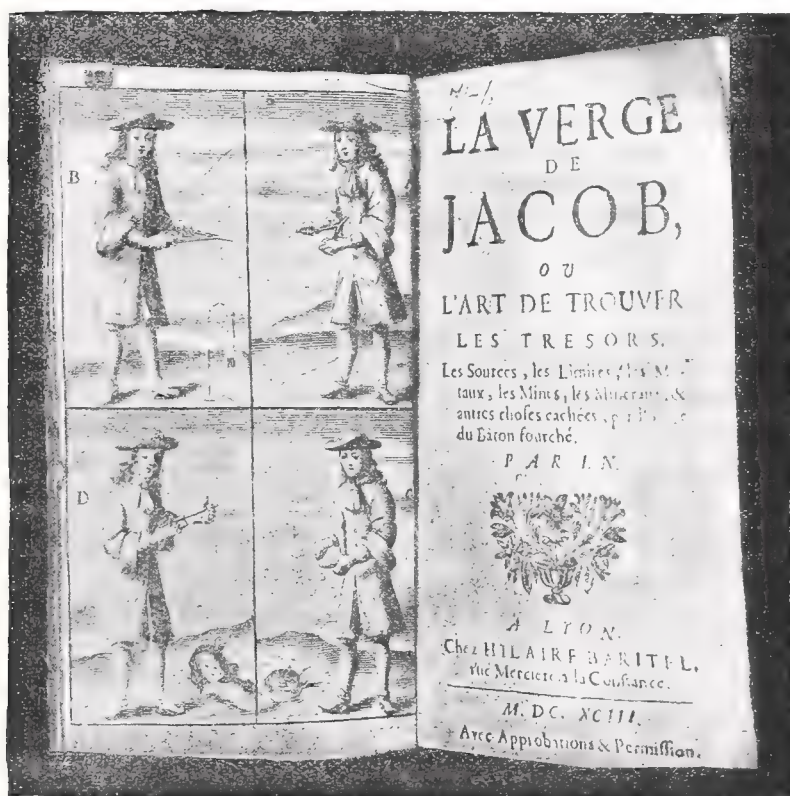


FIG. 16.

experienced. This is the more probable as the little book, *La Verge de Jacob*, was likely to be well known in Dauphiny, being published at the neighbouring town of Lyons, where also Bleton was frequently engaged. (See also the woodcut on p. 274.)

The singular physiological effect produced upon Bleton by underground springs seems to have been retained more or less throughout his

<sup>1</sup> I am indebted to the kindness of the Editor of *Pearson's Magazine* for these two woodcuts; Fig. 17, he may be interested to know, was originally taken from Lebrun's *Lettres qui decouvrent l'illusion des Philosophes sur la Baguette*, Paris, 1693; see also De Vallemont's *La Physique Occulte*, 1693. De Vallemont says the rod was sometimes held in the ways shown in Fig. 17.



life. A sort of convulsive spasm seized him, affecting the diaphragm and pulse. Abundant evidence of the genuineness of this "commotion" is given by a number of unimpeachable witnesses. But this aspect of our subject I will return to in a later section, when the medical report Dr. Thouvenel gives of the peculiar symptoms exhibited by his "patient" will be quoted. Here I will only add that Bleton's sensibility appeared to vary, being greater in dry weather and before meals. It entirely ceased during an illness he had, and did not return

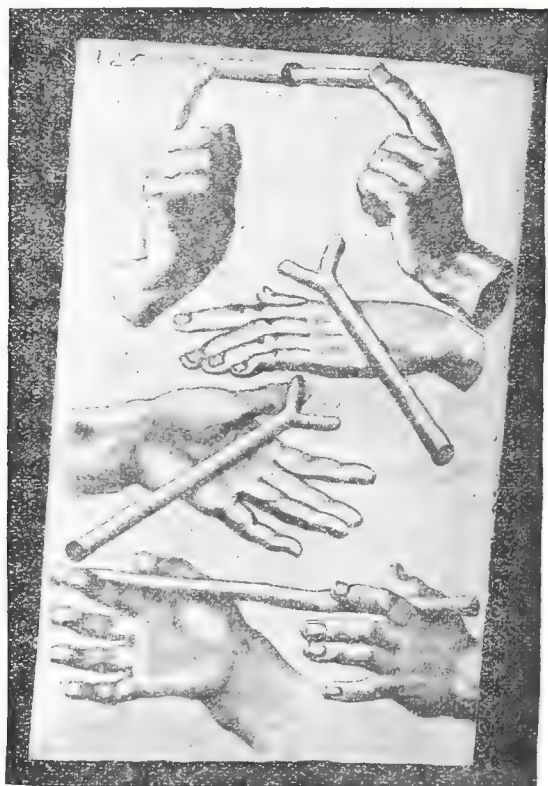


FIG. 17.

until three months subsequent to his recovery. The rate of rotation of the baguette was observed closely to correspond with the physiological effect produced. When Bleton moved away from an underground spring, the symptoms disappeared as rapidly as they arose, and the pulse resumed its normal rate. Stagnant water appeared to make no impression on him, nor the water of exposed rivers, lakes, &c. This latter is indeed a singular, and at first sight appears a suspicious circumstance, difficult to reconcile with any physical theory of the phenomena. But equally singular is the fact that it is affirmed by all diviners from the time of Jacques Aymar in 1693 down to the present

day,<sup>1</sup> and not in one country but wherever the divining rod is in use, not the least singular fact how this tradition (if it be only such) has spread, for diviners, as a rule, are illiterate men, ignorant of any language but their own. Let us now examine what experimental evidence history has preserved on behalf of Bleton's alleged powers.

The principal source of information is found in two treatises entitled *Mémoire Physique et Médicinal*, by Dr. Thouvenel, who was a distinguished French physician.<sup>2</sup> The first treatise was published in 1781 and the second in 1784, and both are almost entirely devoted to a record of observations with Bleton and an exposition of Thouvenel's electrical theory of the phenomena.<sup>3</sup> The best English summary of these *mémoires* is to be found in that old but well edited register of current literature, the *Monthly Review* for 1781, 1783, and 1784. There are, unfortunately, few details given by Thouvenel of his own experiments with Bleton.

No. 1.—Here, however, is one, which he says is a complicated test to which he submitted Bleton. I will give an abridged translation (pp. 69 and 70).

Over a stone bridge, of one arch, pass four small wooden aqueducts, carrying water to Nancy. Only the engineer, who had never seen Bleton, knew the exact position of these four rows of pipes, their distance apart, depth underground, etc., the whole being well covered by earth and vegetation. He gave me secretly information on this, of which Bleton was entirely ignorant. The latter was then taken across this bridge, as though to return to the town, after various experiments had been made in the neighborhood, and without being told that fresh experiments were to be made. Just before reaching the bridge, he asserted water was flowing beneath him, and the sensation continued with slight gaps, while crossing and in front of the bridge to a distance of five or six feet. He retraced his steps several times before finding distinctly the four channels, and was much astonished to

<sup>1</sup> In 1689 an interesting correspondence (to which I will return) took place between those acute intellects, Fathers Malebranche and Le Brun, on this very point. For a discussion of the sensations alleged to be experienced by dowzers, see *Proceedings S.P.R.*, Part XXXII., Appendix D., p. 272.

<sup>2</sup> The great French Biographical Dictionary gives an admirable account of Thouvenel's life, which deserves to be more widely known. Like Dr. Elliotson, in defence of mesmerism, Dr. Thouvenel had to face a storm of obloquy for his courage in defending Bleton. His long and patient investigations were treated with contempt. He left France for Italy at the time of the Revolution, and died in 1815.

<sup>3</sup> I have to thank M. Rolland, of 2, Rue des Chantiers, Paris, for enabling me to obtain a copy of Thouvenel's earlier and more important *Mémoire*, together with other rare French, German and Italian books, pamphlets, and newspaper cuttings of the last century bearing upon the *baguette divinatoire*. To those engaged in any special literary research I can heartily commend M. Rolland's courteous help and excellent agency.

find them so near. He was then told that they were simply four hollow tree trunks made to serve as aqueducts.

Thouvenel then describes other tests as to the distance apart of these pipes, and remarks that as a small stream about 3 feet wide was flowing some 10 feet beneath the bridge, he endeavoured to ascertain what effect it had on Bleton; a careful experiment showed that the agitation of the diviner and the rotation of the *baguette* sensibly increased on crossing the stream. There is nothing, however, in these experiments to exclude the effect of telepathy or even involuntary *suggestion* upon Bleton's mind. It is, however, only within recent years that the powerful influence of suggestion, both on mind and body, has received the attention of physiologists, and therefore we need not be surprised that some of Thouvenel's experiments are capable of a different interpretation from that which he gave. For example, he found that when his *sourcier* was over a spring and the *baguette* turning strongly, the convulsive movements of the body and the rotation of the rod were almost arrested the moment he touched Bleton with various "magnetic compositions recently electrified"! Experiments were also made when Bleton was insulated, but these I will deal with in another part, as the belief that insulation stops the movement of the rod is almost as widespread as that of the different effects of running and stagnant water.

On pp. 77-80 of his first *mémoire* Thouvenel gives a summary of the tests he made with Bleton in Lorraine. The following is a translation of the passage<sup>1</sup> :—

I took the precaution to repeat several times all the experiments just described in detail, after having carefully *blindfolded* Bleton; in addition his arms were sometimes fastened behind his back leaving his forearms only just sufficiently free to hold the *baguette* at the extremity of his fingers, sometimes even confining these, in order to hinder if possible all mechanical movement. These precautions were not taken for my own satisfaction, as I was already entirely convinced.

I conducted Bleton to places which he had never seen; I took him towards springs of which I knew, but which he could not know of, at other times over ground where neither of us knew what might be found. Whenever he experienced his peculiar sensations—and whenever the *baguette* repeatedly turned at the same spot,—I then led him far away, bringing him back by quite different roads, still with his eyes bandaged. . . . When he had followed the course of an underground spring—sometimes for more than a quarter of a mile, across mountains, rock, or forests, and indicated on the way numerous sub-divisions of the same spring—I made him return. He then re-conducted me himself, though still blindfolded, only supported by one arm, to the point from which we set out, without straying a single step

<sup>1</sup> Already quoted in Part V. on "Blindfolding the Dowser."



from the line previously traced and marked by pegs, which were often hidden beneath the surface. He re-found all the subterranean rivulets already pointed out, and followed exactly the sinuosities of the underground stream. It frequently happened that we came across springs whose course was interrupted by walls, terraces, or wide ditches, so that in order to enable Bleton to overcover these obstacles, I had to procure ladders, or take long détours, or in some way get him conveyed across; nevertheless, although blindfolded, he soon regained the course without the aid of his eyes. Sometimes in order to try and deceive him, if his senses were concerned, I placed false marks as if to indicate a spring; sometimes after he had followed a spring across several fields, I moved the pegs some feet away without his knowledge. Nevertheless, he was never led astray and always rectified such errors. In fine, I tried all sorts of ways to deceive him, and I can testify that *in more than six hundred trials*, I did not succeed in doing so one single time.

M. Jadelot, the Professor of Medicine in Nancy, Thouvenel states, was a witness of, and co-operated in, all his experiments, which extended over a space of two months, and was no less struck than he was with the strength and importance of the evidence obtained. Another distinguished savant who was converted to a belief in the *baguette* was M. Sigaud de la Fond, whose published testimony Thouvenel quotes (p. 289 *et seq.*). But the most important evidence on behalf of Bleton is contained in the numerous letters and affidavits Thouvenel appends to his work. These are the replies he received from various well-known persons who had employed Bleton, and whose opinion he had asked. I will quote some of these directly. In addition to the foregoing, Bleton found one or more valuable springs (as testified by letters or *procès-verbaux*) for the Marquis de Torcy in Poitou, for the President of Lamoignon in Basville, for the Bishop of Laon in Annisy, for the Count d'Adhémar in Thun, for the Duke d'Uzès, Count de la Blanche, Count du Bourg, President d'Ornacieux, M. de la Borde, and many other French personages of distinction. There are also brief statements from 20 other persons for whom Bleton found *plusieurs belles sources*, in addition to the 17 letters or affidavits quoted at length by Thouvenel. In these *pièces justificatives* Thouvenel as a rule gives only the initials of his correspondents; the documents, he tells us, are all signed and some legally attested, but some of the writers might object to their names being published; moreover, he adds that in physics names add nothing to the facts themselves.<sup>1</sup>

No. 2.—In one statement, however, this rule is departed from, and Thouvenel humorously prefixes the heading: “*Procès verbal*

<sup>1</sup> This may account for the omission of his own name (initials only being given) as the author of these *Mémoires*.

*sur Bleton . . . papier timbré, paraphé, collationné, etc., etc.* (*Bon pour ceux qui aiment ces petites formalités*),” and the testimony is certified by, and attested before, a list of signatories whose names and official titles are appended. In this case the evidence of the various municipal officers of the Commune of St. Jean-en-Royant in Dauphiny is given. Bleton was a native of, and resided in, this commune (Bouvantes being a parish therein), and the *procès-verbal* states that Bleton had possessed the gift of finding springs for about 30 years, and had practised it much to the advantage of the inhabitants of the commune. Specific instances are then cited of the more important springs Bleton had found in arid ground—springs which were still running, the witnesses affirm, and had largely increased the value of the property in the commune: owing to these discoveries, fertile and productive land had, in several places, now replaced the previously barren soil. The Chief Clerk of the neighbouring commune states that Bleton found for him an ample spring in a district where no water was previously known to exist, the result of which was that the ground, which formerly was of little or no value, now is a valuable property. The signatories of the *procès-verbal* further testify that Bleton had discovered during the preceding 20 years a quantity of springs in other neighbouring communes, some of these being of so great a volume that machinery was worked by them.

No. 3.—Another document also signed by M. A., an inhabitant of St. Jean de Royant, states that the value of his property had been increased *tenfold* by the springs discovered on it by Bleton.

No. 4.—*The Prior of the Chartreuse de Lyon* testifies (March 3rd, 1781) that Bleton had found several springs for him and had not once been mistaken, though his estimate of the depth and volume of the water he acknowledged to be conjectural. Bleton’s discovery of his liability to err on this point being, he adds, the only progress he had made since he was seven years old, when “il etoit aussi savant qu’il l’est actuellement.” The Prior states that when he held Bleton’s wrist, the change in Bleton’s pulse was so perceptible upon arriving over an underground spring that the fact was as obvious to him as to the *sourcier*, a baguette being perfectly needless.

No. 5.—*The Chevalier de M*——, formerly Captain of the Piedmont regiment, testifies that he has been convinced of Bleton’s powers in spite of the prejudices he previously entertained. Having put Bleton to all the proofs he could think of, he never once found him mistaken. “Whatever the power is,” he remarks, “it is not the mere movement of the *baguette* that astonishes me—that might be a matter

of skill—but the effect on Bleton's pulse and nervous system were unmistakeable, and these cannot be simulated. Moreover," he continues, "*j'ai fait toutes les chicanes que mes doutes m'ont inspirées,*" until doubt became impossible. The Chevalier describes in detail one of several tests he made; he brought Bleton to his own house, arriving after dark; in passing through the village, which Bleton had not visited before, Bleton suddenly stopped and said water was there; he followed it in the darkness and arrived at a spot where he declared the spring existed; he was right; it was, in fact the source of the fountain of the castle. Other tests are also given: altogether a remarkable and weighty testimony.

No. 6.—*M. de F.*, the receiver of taxes for Autun, states that a strong spring was discovered on his place by Bleton. In order to test the *sourcier*, *M. de F.* afterwards blindfolded him, and took him over the same ground; Bleton marked precisely the same spots he did previously when not blindfolded; this experiment he repeated several times.

No. 7.—*Dr. de C.* (a neighbour of *M. de F.*) took Bleton to his house, where Bleton accurately traced the course of the water pipes, a fact confirmed by the workmen who had laid them.

No. 8.—*M. le Comte de M.*, of Chagny, describes several careful tests he made of Bleton. In one, after Bleton had accurately indicated an underground spring (the existence of which was known to the Count, but unknown to Bleton) and traced its direction, which was noted, the Count then carefully bandaged Bleton's eyes with a thick handkerchief, and, in order to mislead him, turned him round several times; nevertheless Bleton correctly indicated the same course of the spring, in spite of frequent attempts made to divert him. Holding his wrist, the Count noticed that the change in the rate of Bleton's pulse was very marked when he came over the spring.

No. 9.—*The Bishop of M.* describes how his Archbishop tested Bleton's ability to discover running water: (1) Bleton correctly traced the buried water pipes in the Archbishop's grounds when water was flowing through them; (2) Unknown to Bleton the water was then turned off and he was asked to repeat the trial, and he completely failed to find the pipes; (3) Again the water was secretly turned on, and Bleton once more correctly indicated the direction taken by the pipes.

No. 10.—*The Chevalier de S.*, *commandeur de Mâlte*, states that many years previously his grandfather had made several attempts to find water at his *Château près de la Côte de Saint-André*, had



even sunk a well to the depth of seventy feet, which had to be abandoned. The Chevalier de S., and his brother, the President d'O., in 1735 sent for a *sourcier* they knew of who traversed the estate, using an iron rod they had picked up as a *baguette*. Upon their expressing surprise at his ability to use so novel a divining rod, the *sourcier* replied, "the rod is of no consequence, it is the peculiar feeling I have when over an underground spring that guides me." In 1773 (thirty-eight years later) the Chevalier heard of Bleton and sent for him. On his arrival he was taken to the terrace, where the rod began to rotate, and the site of the well was correctly indicated. To estimate its depth Bleton went to a certain distance on each side, the rod rotating in an opposite direction as he moved away from the spring. By observing the distance and decreasing force of the rotation, Bleton estimated the depth of the well to be 64 feet: its actual depth was 66 feet.

Bleton was then taken to a hill behind the château, when he pointed out the existence of several springs in the spots marked; the following winter a well was sunk at one of these spots and water found at a depth of 18 feet, a narrow layer of sand being struck after piercing through the upper very hard rock. This spring has never run dry, even in times of great drought. Two years later (in 1775) Bleton was again tested on the same estate and he indicated another spring, the depth of which he estimated at 33 feet; a well was sunk at this spot and the prediction was verified. Some time after, the two springs were joined by underground conduits; these conduits Bleton accurately traced when he was again summoned to the château in 1779.

In 1781 a fourth visit to this estate was made by Bleton. This time he discovered another spring, which he estimated at 25 feet deep; a well was sunk and at 35 feet an abundant supply of water was obtained; this also issued from a layer of sand beneath the hard superincumbent rock.

Some experiments were then made by the President d'O. and his brother as to the movement of the *baguette* when Bleton was placed in various positions. The rod turned when he lay on his back, but not on his stomach, nor did it turn when it was placed on his loins when he was lying down. Bleton was then suspended upside down and the *baguette* placed on the soles of his feet; it did not rotate. Bleton, however, showed them that when the rod was placed nearly perpendicularly with one end resting on the palm of the left hand, and the other in the air, encircled by a ring formed of the finger and thumb of the right hand placed a little below the point, the *baguette* "pirouetted slowly," when over a spring.

This interesting document ends by suggesting the desirability of placing *le tourneur de baguette* on a sheet of resin or a glass-legged

stool and noticing whether his peculiar sensations are still experienced, or whether they resemble the sensations one has when thus insulated and connected with an electric machine. If, remarks the Chevalier, running water gives off subtle emanations, they must be comparable to those of light, and possibly suffer refraction and reflection in their course. The document is signed by the Président d'O., who states that he did not write it, but he approves ; and that the facts narrated were all witnessed by him, his son, the Chevalier de S., and other persons, and he adds his conviction that Bleton employed no *charlatanerie* in any of his proceedings.

No. 11.—On the lands of the Abbé de Vervains there were certain springs, all trace of which had long been lost ; they were, however, known to exist from the ancient title-deeds of the estate. A lawsuit hinged upon the question of their existence being actually proved. When search for the springs had been made in vain, Bleton was sent for. He came and indicated the place where these springs existed. Their actual existence was then discovered, and the lawsuit terminated.<sup>1</sup>

No. 12.—*Surgeon-Major N., of St. Geny Laval, near Lyons*, testifies that Bleton came to a part of his estate which was rocky and arid, and found one spot where the baguette indicated a considerable spring, which he estimated to be at a depth of some 55 feet. A well was sunk, but no water found at this depth. Bleton was, therefore, sent for again. He tested the ground once more, asserted confidently that a good spring would be found at a few feet lower, remarking that he was often mistaken as to the depth when it was over 30 feet.<sup>2</sup> The well was, therefore, deepened, and at 7 or 8 feet lower so powerful a spring gushed forth that it was impossible to sink deeper, as the water rapidly rose in the well, which was 5 feet in diameter. The witness adds, “A useless land has thus been converted into a meadow.”

No. 13.—*P. I. G., and Seigneur D., Conseiller du Parlement de B.*, affirm that they brought Bleton to Tanyot, in Bourgogne, where he was subjected to various tests, three of which are cited : (1) He was asked to find the course of some water pipes that had been laid some time previously and which were completely covered by earth and grass, so that no difference was apparent on the surface of the ground. Bleton immediately discovered the position of, and accurately traced,

<sup>1</sup> Quoted from the *Monthly Review* for 1784, p. 572.

<sup>2</sup> There is evidently a misprint in the French text here, which reads “au-dessous de trente pieds” ; from the context obviously *au-dessus* is meant.

the course of these pipes. (2) He was asked to find the spring which supplied a neighbouring well, and to estimate its depth. This he did quite accurately, and stated it was a very feeble spring, which the owner of the well certified was also correct. (3) He was asked to find a supply of water for their château. For some time he searched in vain, but eventually in another direction he found six small springs: their position was marked by pegs. Upon digging at these spots, springs were found in each place at about the depth and volume he indicated. These springs were then connected by a deep trench, and furnished an abundant supply of water for both house and garden.

It is needless to translate and summarise the other documents, which contain similar evidence. Thouvenel's treatise, however, is not the only source of our information about Bleton. Accounts of this *sourcier* are to be found in many of the French journals of that period. I am indebted for much of the subsequent evidence to the excellent and impartial narrative of Bleton given by Louis Figuier in the seventh chapter of the second volume of his *Histoire du Merveilleux*, which contains some facts I have not met with elsewhere.

No. 14.—In the *Journal de Paris* of May 13th, 1782, an account was published of some careful experiments made to test Bleton's powers in the gardens of the Luxembourg under the direction of M. Guillaumot "intendant général des bâtiments du roi," who was accompanied by inspectors and officials of the gardens. The report states :—

Sorti du Château d'Eau, Bleton a suivi dans la campagne l'aqueduc d'Arcueil avec une précision telle que, pour nous servir d'une de M. Guillaumot, si ce plan venait à se perdre, on le referait sur les traces de Bleton.

The report states that M. Guillaumot verified from the plans the angles and sinuosities made by the (I presume subterranean) aqueduct, and these the report adds: "nous dirons presque mathématiques désignés par Bleton." Two days later the experiments were repeated in the presence of the municipal authorities and a crowd of spectators. The report states :—

Ici les expériences ont été telles qu'à l'exception de deux seuls témoins, qui avaient publiquement avancé qu'ils ne croiraient pas, même en voyant, qu'à cette exception près, sur cinq cents spectateurs, il n'en est pas un qui n'ait été convaincu de la faculté dont est doué Bleton de suivre les eaux souterraines avec la plus rigoureuse précision. Les yeux bandés, la baguette posée sur les doigts, il n'a pas quitté l'embranchement de l'aqueduc.



Other trials followed in the presence of ministers of state, ambassadors, magistrates, scientific men and the clergy, and the *Journal de Paris*, in summing up the experiments, records that Bleton followed

Jusqu'à présent, de notre connaissance, plus de quinze milles torses des conduites, sans avoir jamais commis une seule erreur et sans avoir trouvé, dans le nombre de plus de six mille personnes, un seul témoin compétent qui ait pu faire une objection fondée. Il a été soumis à toutes les épreuves les plus rigoureuses qu'ait pu suggérer l'incrédulité, même l'esprit de parti, et il n'en est résulté que plus de lumières, plus de convictions et plus d'étonnement.

No. 15.—Some rigorous experiments were made by a committee of six *savants*, who drew up a report dated, "Paris, May 25th, 1782." This report, which was signed by all the members, states that Bleton, having been blindfolded with extreme care by a succession of black and then white linen bandages, with cotton wool stuffed up the sides of his nostrils, was taken to the garden of one of the members of the committee, wherein a water-pipe ran underground to a distant fountain. The jets of this fountain had been removed, so that the water ran into the basin, and off through an overflow pipe, quite noiselessly. Here Bleton indicated running water at certain spots which were marked; he was then made to retrace his steps 10 or 12 times, and it was found that the *baguette* turned nearly, but not quite, invariably when he crossed the same spots: at the places where the water entered the basin and where the overflow pipe discharged he was always right. This they verified at least a dozen times. The experiments lasted two hours, with a short interval for rest; during the whole time Bleton was blindfolded, and during part of that time M. Thouvenel was not present.

According to the writer of the article in the *Monthly Review* for 1782:—

These successes led the Queen of France to employ Bleton, and the springs that have been found in consequence of his indications have fertilised and embellished several arid districts, among which Trianon is a striking example, as that delightful seat has acquired new charms by Bleton's discoveries.<sup>1</sup>

The Editor of the *Journal des Spectacles*, in a figure of speech, we must assume, states that Bleton, by his discovery of numerous springs, had changed Dauphiny from an arid soil, which produced nothing, to one of the richest in France. Dr. Ginetz, writing in the *Journal de Paris* for November, 1807, states that, "The efficacy of the *baguette* is nowhere contested in Dauphiny. I have myself," he says, "frequently

<sup>1</sup> According to Figuier, however, Bleton had some failures at Trianon, mistakingaverns and dry conduits for underground water.

observed the effects of the baguette, and can state from personal experience its success in seeking for springs and metallic veins.”<sup>1</sup>

In Thouvenel's second *Mémoire*, published in 1784, a further account is given of his experiments with Bleton. This eminent physician was commissioned in 1783 by Louis XVI. to examine and report on the mineral and medicinal waters of France, and to aid him in the discovery of any fresh mineral springs, he obtained permission to engage the services of Bleton.<sup>2</sup> Most of this second *Mémoire* is therefore occupied with tracing hot springs and mineral waters, with conjectures as to the source of the former, and attempts to verify the electrical hypothesis which Thouvenel had formed, of the influence of subterranean springs on the diviner.

We need not concern ourselves with the latter, as the experiments cited are certainly open to criticism, and would only excite a smile in the wider scientific knowledge of the present day. It is, however, now known, as Thouvenel conjectured, that there are subterraneous electric currents, the strength of which fluctuates in various places and from time to time. It is certainly a curious coincidence, if it be no more, that upwards of one hundred years ago this French physician, on the faith of Bleton's sensations, maintained that these electrical manifestations appeared in general to run east and west.<sup>3</sup> Current electricity itself was not discovered till ten years later, the existence of earth currents was not known till Barlow's experiments in 1849, and the general trend of these currents east and west was a still more recent observation.<sup>4</sup>

<sup>1</sup> It was in Provence (the adjoining province to Dauphiny) that Lady Milbanke, in 1772, first saw the use of the divining rod. In a dry, mountainous region north of the Durance, the Marquis d'Ansonis had found water by a *sourceier*, and the Marquis, whose faith in the divining rod was unbounded, converted Lady Milbanke to his views after witnessing experiments on his estate.

<sup>2</sup> Figuier states that, owing to Bleton having indicated the existence of some natural oil springs on this expedition, he was engaged on his return by the Government Department of Mines to search for similar springs in the neighbourhood of Paris. Figuier adds, the high sanction thus given to Bleton's peculiar faculty shows the esteem in which he was held, and distinguishes him from the common run of *tourneurs de baguette*.

<sup>3</sup> Thouvenel thought that the electrical effects, east and west, were associated with all metals, except iron, which he affirms gives a north and south direction; as he associated the latter with terrestrial magnetism, this looks as if the results of his experiments were often vitiated by involuntary *suggestion* on Thouvenel's part.

<sup>4</sup> There are some other curious observations in this treatise which illustrate Thouvenel's scientific spirit. Thus, by the aid of Bleton, Thouvenel traced the source of the hot springs of Bourbon Lancy to a mountainous region abounding in coal; not only so, but a similar result appeared constantly to occur with other hot springs he examined, and he affirms that eventually their origin was to be found in or near masses of coal, to chemical changes in which he attributes their high temperature. Geologists would not, I imagine, endorse this view. Variations in the conductivity of

*Contemporary Opinion Adverse to Bleton.*

Among those who, after experimenting with Bleton, formed an unfavourable opinion of his reported powers was the Abbé Mongez, one of the principal contributors to the articles on Natural Philosophy in the Paris *Encyclopédie*; but, according to Dr. Thouvenel, these experiments were intended to be unfavourable, and the experimenters, by designing a number of ingenious tricks to deceive Bleton, in reality deceived themselves. Certainly the *parti pris* of most of the scientific investigators of Bleton was very evident, and to a large extent vitiated both the experiments made and the conclusions drawn from them.<sup>1</sup> These experiments were made for the most part in the church and garden of St. Geneviève in the presence of several *savants*. An account is given of them in the *Journal de Paris* for June 16th, 1782, and *procès-verbaux* of the experiments are to be found in the *Journal de Physique* of that year.<sup>2</sup> It is needless to enter into the details; suffice it to say that the report concludes by stating that Bleton found water pipes, springs, etc. on all sides, whereas there were no water pipes and no springs beneath him. The attempt to explain away this failure, which Thouvenel made, shows that believers in the divining-rod can be no less blind and prejudiced than their opponents. Doubtless the experiments were wholly inconclusive in face of the abundant testimony in Bleton's favour, some of which we have cited. The writer in the *Monthly Review* seems to have given the true explanation of these experiments. Admitting that Bleton did fail, he remarks, "this will not appear surprising when we consider this poor timorous man led about blindfold, harassed, fatigued, and perplexed with cross questions," and he adds, "even the Abbé Mongez admits that Bleton was sometimes quite correct." In fact, as the Reviewer I have already quoted justly remarks:—

"The public have no curiosity to know how far tricks and ingenious means of deception can go in disconcerting or suspending the exercise of

rocks and the existence of past or present volcanic action,—in a word, the internal heat of the earth,—are, I believe, the sufficient causes now recognised: but we must remember our author wrote upwards of a century ago.

<sup>1</sup> As the able contemporary reviewer, to whom I have already referred, remarks (*Monthly Review*, 1782, Vol. LXVII, p. 554), "It is certainly possible that even an honest zeal for the discovery of imposture or enthusiasm may be exerted in a manner not perfectly adapted to the discovery of truth. . . . It is observed by all that Bleton is uncommonly timorous and easily disconcerted, even so far as to suspend his *impressions*. This we can well conceive, be his talent ever so real. The very talent seems to announce a sensibility of nerves that may render him peculiarly susceptible of perturbation. Who has not seen schoolboys of the most retentive memories lose the remembrance of the best learned lesson by being intimidated?"

<sup>2</sup> I have not seen these, and am indebted to Figuier for the references, who adds that Guyton de Morveau (a well-known *savant*) gave some interesting details about Bleton in the *Journal de Nancy*.



Bleton's natural talent; they only desire to know whether he in reality possesses this talent, when left to himself and allowed the free use of his faculties."<sup>1</sup>

But, like all other dowzers, Bleton unquestionably failed at times. Figuier names four places, and also "*dans quelques autres lieux*," where wells were dug at the spots indicated by Bleton and no water found, even when the wells were sunk to a depth lower than had been estimated by the *sourcier*. In his able paper on the Divining Rod, read before the American Institute of Mining Engineers in 1883, Dr. Rossiter Raymond quotes,—without, however, naming his authority,—several instances where Bleton failed to find the same spot, after being blindfolded, that he had indicated previously. These experiments are probably those to which we have referred on the last page, and commented on by the *Monthly Review* for 1782.<sup>2</sup>

After the lapse of a century it is instructive to notice how frequently the adverse criticism of Bleton arose from his critics assuming a particular explanation of the lad's sensitiveness, or of the motion of his rod, and having demolished their theory, they roundly asserted the boy was a charlatan,—a mode of argument not unknown to scientific critics of the S.P.R. at the present day.

Thus, Bleton's sensitiveness being supposed to be due to some electric influence, he was mounted on an insulating stool and his rod at once ceased to move, but resumed its motion when he was on the ground. A famous physicist, Charles, who conducted this experiment a century ago, when Bleton was on the insulating stool, secretly connected the lad with a wire to the earth; still the rod remained passive although the insulation was destroyed. Whereupon Bleton was openly denounced as a charlatan by his scientific critic. All that this experiment proves is the influence of *suggestion* on the motion of the rod. As the lad knew nothing of electricity, he must have been told that the insulating stool would intercept the power, and so the rod ceased to move when he was mounted on the glass-legged stool, and it remained motionless when the insulation was destroyed, as care had been taken to avoid any suggestion of this reaching Bleton. I made a precisely similar experiment with a dowser at Waringstown, in

<sup>1</sup> *Monthly Review*, Vol. LXVII. p. 555.

<sup>2</sup> This is the case, I have since ascertained. Dr. Raymond has derived his information from Figuier's excellent essay on the *Baguette Divinatoire*, published a few years ago, but he does not give the facts quite correctly: moreover, following the example of many others who ignore what they cannot explain, he dismisses in a few words the abundant testimony that exists as to Bleton's successes, fairly enough given by Figuier, and emphasises (in fact, exaggerates) any failures that were reported. Chevreul did just the same;—see, *e.g.*, p. 113 of his work on the *Baguette Divinatoire*, published in 1854.

Ireland, in 1896 (see previous Report, Vol. XIII., pp. 80 and 246), and the result was exactly the same ; when the dowser believed he was *not* in electrical connection with the earth, his twig ceased to move ; when he believed he *was* electrically connected, the forked twig instantly twisted round, though the electrical condition was the same in both cases. The object of my experiment was to dispose of the widely spread but mistaken idea that insulation prevents the motion of the rod. Even on his own hypothesis, the French experimenter with Bleton was mistaken ; for the theory that some electrical *influence* from underground water affects the dowser, and so starts the motion of the rod, is not touched by insulating the dowser ; as every tyro knows, standing on a glass-legged stool merely prevents electric *conduction* from the earth and does not impair electric *induction*. This latter, though a plausible hypothesis, has, however, no experimental evidence in its favour and need not be here discussed.

Again, the eminent astronomer, Lalande, believed he had conclusively demonstrated Bleton was a rogue because he established the fact that the peculiar rod Bleton employed could be rotated by sleight-of-hand. The fallacy of this line of argument, though persisted in at the present day, has already been pointed out (see p. 141, etc). As, however, Lalande is often quoted as having “exposed” Bleton, it is worth giving *in extenso* the communication which this famous *savant* made to the *Journal des Savants*, (or *Sçavans* as it was then spelt) for August, 1782, p. 558. Here is the quotation from the copy in the British Museum :—

“Un nommé Bléton, né dans un village, près de Grenoble, a prétendu avoir un propriété extraordinaire de sentir les eaux souterraines par un tremblement convulsif. Ce sourcier, ou hydromyrete, plaçoit sur ses doigts une baguette ou une verge de métal, courbée en arc, et on la voyoit tourner rapidement. Ce stratagème étoit plus adroit que celui des sourciers qui courent les villages, et qui marquent des sources aux paysans, moyennant la plus mince rétribution. Ceux-ci serrent leur baguette dans leurs mains, et pour peu qu'on ait envie des regarder, on s'apperoit facilement qu'il suffit de ferrer la baguette inégalement ; sa courbure détermine nécessairement un mouvement de rotation . . . il [Dr Thouvenel] n'étoit plus assez calme pour se rendre aux raisons de ses adversaires, ni même pour appercevoir la petite charlatanerie dont il avoit été la dupe . . . [il] a été parfaitement séduit par l'adresse de Bléton, à faire tourner sur ses doigts une verge courbe de métal ; il n'a pas apperçu que cela tenoit à une cause mécanique.

En effet, si l'on place sur deux doigts une baguette de métal courbée en arc, de manière que le sommet de l'arc soit plus bas que les deux extrémités, mais que le tout soit presque en équilibre, le plus petit rapprochement des doigts, ne fût-il que d'une ligne, suffira pour que les extrémités l'emportent à leur tour, et que le sommet de l'arc vienne en haut. Si on les écarte, à l'instant le sommet de l'arc descendra, et avec une pareille alternative, le mouvement peut continuer aussi longtemps qu'on le jugera à propos. Un

homme très-exercé n'a besoin pour cela que d'un léger tremblement qui est à peine sensible, quand on n'est pas prévenu.

Faute d'avoir apperçu ce petit mécanisme, M. Thouvenel a fait un livre sur la baguette, mais M. Demours, fils de l'Académicien très-connu, a fait tourner une baguette pareille dans une assemblée de l'Académie des sciences, de manière à lever toute espèce de doute à cet égard.

. . . M. Needham . . . cite même quelques faits contre les prétentions de la baguette, qui sont renouvellées de tems à autres par des

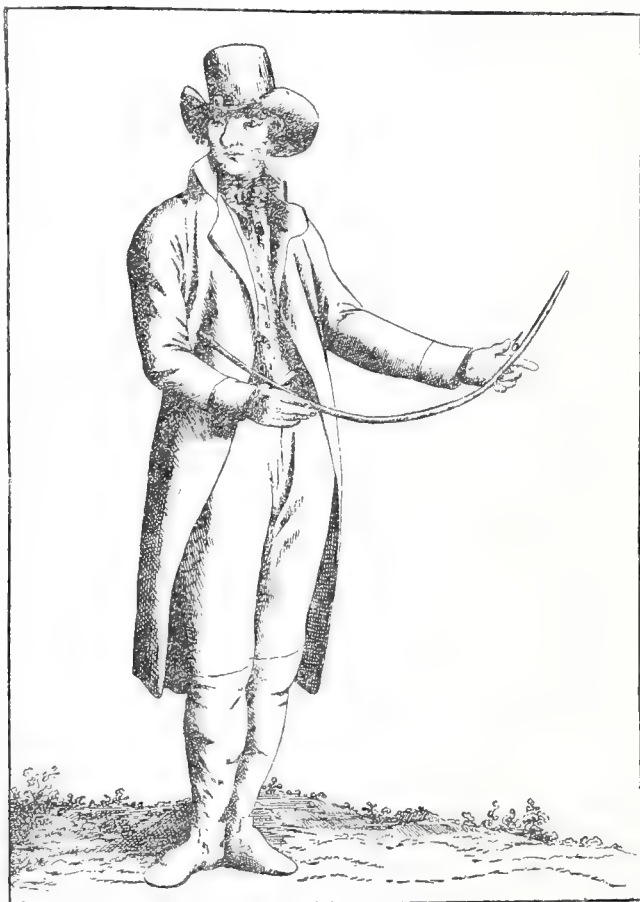


FIG. 18.

fripons, ou par des dupes. Enfin, M. Paulet, dans la *Gazette de Santé* du 10 Juin, 1781, s'est moqué de la nouvelle physique, ainsi qu'on l'avait fait dans le dernier siècle.

But whilst M. de Lalande clearly demonstrates that slight muscular action can move the rod, somewhat as Bleton moved it, he does not trouble to make any enquiry on the only point of real value, viz., whether Bleton was more successful in finding underground water than chance or shrewd observation would account for. Nevertheless, the



weight of Lalande's authority crushed Thouvenel and his *protégé* Bleton. It was taken as a matter of course that the latter was a clever trickster, who had duped the public. Conjuring books showed how to work the rod *à la Bleton*. A book by H. Decremps, called *La Magie dévoilée*, published in Paris in 1784, devotes a lengthy chapter to this, showing not only how to rotate a slightly curved rod (held as in Fig. 18) by the quivering of the index fingers, but also how a manikin can be made to imitate the search for water in this fashion. This chapter was copied into the *Encyclopédie Méthodique* for 1792, and an English translation of Decremps' book was published in 1785 under the title of *The Conjuror unmasked . . . with directions for the tricks of the Divining Rod*. But the whirligig of time brings its revenge; Decremps, whose book was all the vogue a century ago, is to-day forgotten, and Bleton is now the subject of scientific study.

The rotation of Bleton's nearly straight rod was no doubt due to the same cause as the twisting of the forked twig of the dowser at the present day, viz., an involuntary muscular movement arising from some sub-conscious suggestion (see p. 291); how he, in common with other dowsers, derived this usually correct suggestion of underground water, when no one else knew of its presence, is the problem that we must consider in the sequel to this paper (see Part XIII.).

Thouvenel gives no picture of Bleton nor of his manner of holding the rod. I have searched through many works in the hope of finding a picture given by some contemporary writer of the use of this curved rod, and at last discovered the accompanying drawing given in the reprint of Amoretti's papers entitled *Rhabdomanzia*, from the copy of the German translation of which, published in 1809 (in the Royal Society Library), Fig. 18 is reproduced. The picture shows the lad Pennet holding the rod as Bleton did, but the rod appears rather larger and more curved than Bleton's rod. Thouvenel, and afterwards Amoretti, experimented with Pennet (see p. 246).

Here I must conclude the report of this remarkable dowser, which has grown to a length much greater than I anticipated. My apology must be that it is the first time a full summary of the evidence has been presented to the English public.

## PART XI.

INVOLUNTARY MUSCULAR ACTION AND THE MOTION OF THE ROD,  
PENDULE, OR OTHER AUTOSCOPE. MOTOR AUTOMATISM.§ 1. *Evidence as to the Involuntary and Uncontrollable  
Motion of the Rod.*

To an onlooker, who sees a dowser at work for the first time, one of the most startling things is the sudden and apparently spontaneous motion of the forked twig, a motion so vigorous that one of the limbs of the twig is frequently broken, whilst the dowser is apparently doing his utmost to restrain its motion. The common explanation of a sceptical public is that this is merely a trick on the dowser's part to mystify his dupes, but the evidence I have adduced, both in the previous and present Report, shows that this view is quite untenable.<sup>1</sup> The only other alternative recognised by scientific men is that the motion of the twig must be caused by some involuntary muscular action on the part of the dowser. It is true Melancthon and the rest of the learned world of the sixteenth century thought that the motion originated in the twig itself, and was a necessary consequence of the "law of sympathy<sup>2</sup>," but the learned Jesuit Father Kircher (one of the founders of experimental science) about the year 1650 showed that the twig itself was inert, and that in some way the motion was communicated from the dowser himself<sup>3</sup> to the forked twig. Malebranche and Lebrun, fifty years after this, urged with resistless logic that the explanation of the divining rod was to be found in the sport of good-natured or mischievous devils, "the badinage of demons"; but whether these philosophers thought the demons got hold of the end of the stick and twisted it, or gave supernormal strength and skill to the dowser, I do not know<sup>4</sup>. This spirit theory, apparently, is the view of some people at the present day. Spirits *may* swarm in the

<sup>1</sup> I am referring to honest dowzers; impostors may exist here as elsewhere.

<sup>2</sup> Every age has its fashionable fetishes, or *idola theatri*; this "law of sympathy" was a notable idol of the learned world in the 16th and 17th centuries. The movement of the rod to hidden veins of metal or water was supposed to be due to the "sympathy" between certain kinds of wood and metals, etc.

<sup>3</sup> See Kircher's *Magnes sive de Arte Magnetica* (1640), p. 724, and his later work, *Mundus Subterraneus*, Vol. II., p. 200.

<sup>4</sup> Malebranche refers Lebrun to both St. Augustine and Porphyry in support of his view that the devils cannot be always at work tempting mankind, but must have moments of relaxation like human beings! Hence come their little jokes in table rapping, opening doors, etc., so Porphyry thinks. (See *Lettres qui découvrent l'illusion des philosophes sur la baguette*. Paris, 1693, p. 230, *et seq.*)

neighbourhood of mediums and dowzers, only it needs a good deal of evidence to prove it, and it will be some time before science will accept any evidence of that kind. Hence the only alternative before us is that some involuntary and more or less unconscious muscular action on the dowser's part causes the twig to turn and sometimes break.

In the previous Report I have discussed this question (see pp. 243 and 244) and shown that the curious muscular spasm, which probably causes the motion of the rod, is certainly involuntary, and,—when the rod is held as some dowzers hold it,—cannot be intentionally imitated by a conscious effort, without considerable skill in legerdemain. To this statement the only criticism I have received is from those who maintain that this is an inadequate explanation, and that the evidence points to some power *beyond* involuntary muscular action. Here is the view of that distinguished naturalist and acute observer, Dr. A. R. Wallace, F.R.S., who writes to me as follows :—

If the rod *does* move wholly by muscular action, it does not at all affect the power of the dowser in finding water,—but the *fact* should be proved. To me, the evidence you adduce shows that it is *not* muscular action, and if *this* can be proved it, of course, places the dowser in the rank of a physical “medium,” which I have always held him to be. If the two facts you state *are facts* : (1) That the motion of the rod cannot be intentionally produced (by any novice) without *visible* muscular action of an energetic kind ; and (2) that in an outsider's hands, holding the rod for the first time, it will often move *if the dowser holds his wrists*, and with no conscious, and little visible, muscular action on the experimenter's part,—then it follows that the motion is *not* produced by *muscular action* at all, but is a physical phenomenon analogous to hundreds of others occurring in the presence of “mediums.”

I think you should have said : “The *obvious* explanation, of course, is that the rod is moved by the hands of the operator, acting consciously or unconsciously. There are, however, many difficulties in the way of this view, and many facts which seem directly opposed to it.” After which your various statements would follow naturally. Now, they seem to me to be in the nature of a *non sequitur* ! . . .

Of course, *I* am a confirmed lunatic in these matters, so excuse the ravings of a lunatic, but sincere, friend,

ALFRED R. WALLACE.

All that Mr. Wallace writes is worth attention ; it would certainly have been wiser on my part, in the last Report, to have used the words he suggests rather than the more dogmatic phrase I employed. Other correspondents have also urged that muscular action, whether conscious or unconscious, is an insufficient explanation of the phenomena actually observed. In the *Journal* of the S. P. R. for December, 1897, Mr. E. T. Bennett cites some of the evidence I gave in the previous Report in support of this view. Mr. Bennett urges, with much cogency, that as Faraday's explanation of table-turning being due to involuntary



muscular action is now recognised as inadequate to cover all the phenomena of this kind, so in like manner this explanation fails to cover all the cases of the twisting of the divining rod, and hence some other cause, external to the dowser, is probably at work. There is no doubt some force in Mr. Bennett's argument, to which I replied in the same number of the *Journal*.

The descriptions given by different observers are so similar that we may take them as correct, and certainly it would seem that no *conscious* muscular action could produce the effects described. Mr. Bennett (*Journal* S.P.R., December, 1897) therefore asks, "Have we any grounds which justify us in attributing to unconscious muscular action, physical effects which it is beyond the power of conscious muscular action to produce?" My reply is: in hypnosis, somnambulism, hysteria, etc., subjects can perform muscular feats impossible to them in their normal self-conscious state. Whatever tends to concentrate muscular action upon one single dominant idea enables the subject to accomplish what otherwise he could not do. Moreover known causes must be assumed as operative until they are *proved* to be inadequate. The two facts, (1) and (2), named by Mr. A. R. Wallace in his letter, are, however, sometimes disputed, and it is therefore desirable to cite some of the facts already given and add further information, from different witnesses, on these two points.

The violence of the motion of the rod is shown in numerous cases I quoted in the previous Report. Thus on p. 7, Mr. Enys, F.G.S., who is an amateur dowser, states "the rod broke short off in front of my hands, and did so a second time in the same place," *i.e.*, where underground water existed. On p. 26, Miss Grantham (daughter of Judge Grantham), describing what occurred with the Rev. J. Blunt, another amateur dowser, states "so strong was the impulse, that we found unless Mr. B. relaxed his hold, the twig broke off near his fingers." Lady Milbanke, also an amateur dowser, had the same experience, p. 42.<sup>1</sup> Mr. Percy Clive states, on p. 35, that when he held the rod and Mullins put his hands on his wrists, the rod "twisted round in my hands with such force that when I held it tight it broke." Mr. Cecil Woolley, of Lincoln, the agent to Trinity College, Cambridge, states, on p. 242, that the late J. Mullins having gone over the ground and indicated water in one spot, "I took the twig in my own hands and went over the same spot with no result. He (J. Mullins) then took hold of my wrists without touching the twig himself and when we together walked over the same place, the twig turned up in my hands. This was, I suppose, caused by muscular action on my part, but if so, it

<sup>1</sup> The pages in this paragraph all refer to the previous Report, *Proceedings* S.P.R., Vol. XIII.

was certainly, as far as I was concerned, perfectly unconscious action. My mental attitude was one of neutrality!" Lord Burton makes a very similar statement, p. 103. Mr. Budd, a geologist, describes what occurred with Mullins when he came over underground water at Waterford. He writes, p. 110, "Mullins held the forked twig between his second and third fingers as if you were going to write, the point of the fork downwards. At No. 1 [*i.e.*, over the spot where a large supply of underground water was found] the point lifted itself up, until it turned over backwards and twisted itself until it broke . . . The clerks then held [another forked twig] with him, and held his hands; always the same effect." In another place, seeing the frantic motion of the twig when Mullins came over underground water, a gentleman tried to stop its motion by gripping the twig in two places with smiths' tongs, "one pair securing the tips and the other the fork, but the contortions still went on between the points held," p. 87, and so on.

In the present Report numerous independent witnesses of unimpeachable integrity and some with high scientific attainments testify to the same class of facts, viz. :—(1) the automatic and apparently irresistible motion of the twig in the hands often of a complete novice, and (2) that, when the forked twig does *not* move in a person's hands, if the dowser takes one limb of the twig, or even places his hand on the wrist of the insensitive person, the previously inert twig now turns vigorously and often breaks in two in the effort to resist its motion. As regards (1), see the letter from the President of the Royal Geological Society of Cornwall on p. 219,<sup>1</sup> who states that the clerk of his Parish Council, on finding the rod suddenly twist in his hands, called out, "It is alive, sir, it is alive!" Mr. Enys adds: "This exactly describes the sensation when the rod moves." Mr. Dixon, a large fruit-grower in California, on p. 250 states: "I held the stick as tight as I could to prevent its moving, but it twisted right round." Mr. Denison, of the Toronto Meteorological Observatory, on p. 206, gives a careful record of the violent twisting of a forked plum stick or bent wire used as a divining rod by an amateur dowser. Mr. Bennett, of Oxford, on p. 176, refers to the frantic motion and ultimate breaking of the twig "held firmly" in the dowser's hands. Other similar cases will have been noted by the careful reader. As regards (2), see Mr. Morton's letter to *The Engineer* given on p. 172; Mr. Morton found the rod would not move in his hands, but when the late John Mullins, the dowser, "laid his hands on my wrists and grasped them firmly, then the twig instantly began to turn and continued turning till he removed his hands. He never touched the twig

<sup>1</sup> The pages in *this* paragraph refer to the present Report.

while it was in my hands." Mr. Montague Price, in his letter on p. 181, states :—

I held one side of the forked rod myself and the "diviner" the other, and when we came to water [alleged underground water] the strain was so great on my fingers I was obliged to ask him to stop. From the position of the rod it was absolutely impossible for him to produce the pressure, which increased with the strength of the stream.

Mr. Denison, of Toronto, states, on p. 206, that his friend "Mrs. Harris tried without any result ; but when Mr. H. [who was an amateur dowser] grasped her wrists, as she approached the spring, the rod turned forcibly," and so on.

The usual practice, after watching a dowser at work, is for some of the onlookers to try if the forked twig will move in their hands. Generally speaking, one or more, out of perhaps ten or twelve persons, discover to their astonishment that the twig curls up in their hands at the same places at which it did with the dowser. Here is such an experience. Mrs. Hollands writes to me as follows :—

Dene Park, Tonbridge, *October 9th, 1899.*

In answer to your note of inquiry about the divining rod, the whole thing is rather a long story, but the practical result of the water dowser's visit was to find water which now supplies the house. One of my daughters found she had the strange power which moves the divining rod, and it works for her now quickly over any spring. It is most interesting, as you can feel the rod if you take one side of it and take one of her hands, she holding the other end of the rod—it struggles up, and would break off altogether if you did not allow it to move. My daughter has since found several springs on the estate, where we have sunk wells. They have stood us in very good stead these last dry seasons.

MINNIE HOLLANDS.

A similar experience is given by Miss M. Craigie Halkett, who published some excellent photographs of a dowser at work in *Sketch* for August 23rd, 1899. Miss Halkett writes to me as follows :—

Lauriston, New Eltham, Kent, *September 8th, 1899.*

The man depicted in the photographs is not a water-finder by profession. He is a tenant farmer residing at Catcott, a village near Bridgwater, and merely exercises the art to oblige his neighbours. Several of the country people in this neighbourhood (Somerset) have the gift. It has never been known to fail.

Personally, I was rather sceptical on the subject, but was converted by the stick turning in my hands when standing over a spring. There were about six persons present at the time ; all tried it, but it would turn for no one excepting the man in the picture and myself. I experienced a sort of tingling sensation in my arms and wrists, but otherwise was quite unaware when the forked stick began to turn, it seemed to go over so quickly.

MAUDE CRAIGIE HALKETT.



Miss Halkett does not say how she knew she was "standing over a spring" when the twig turned in her hands: this statement is very characteristic of many others that have reached me.

I received the following from a Civil Engineer, Mr. J. W. Parry, who also sent me an interesting article he had written on water-finding, which appeared in the *Pioneer of India* :—

19, Southmoor Road, Oxford, *November 2nd, 1898.*

I have read with much interest your work on "dowsing" in the Psychological Research Society. I was a sceptic, but after reading the 140 cases you mention, it is quite impossible to doubt any longer. I was lately at Padstow, Cornwall, where the following authenticated case occurred which you might like to verify.

It appears a dowser was lately sent for, as some waterworks were projected. The squire, Mr. Prideaux-Brune, and his two daughters came to see the operations of the divining rod. On a spot being indicated by the dowser, one of the young ladies said, "Oh! let me see if I can do it." Unfortunately, she had not the gift, nor had her sister. Squire Prideaux-Brune, then taking the twig, walked back a few paces. On passing the spot his arms shook violently, and would not be controlled. The young ladies were laughingly sceptical, and said, "Oh! papa, you are humbugging." He replied, "I am doing nothing of the kind, and can't help my arms shaking," or words to that effect.

You do not mention any cases as occurring in India, where there is a good deal of so-called divining. I have seen diviners lying naked with their ears to the ground, pretending that they could *hear* the subterranean water. Don't you think that's somewhat beyond our ken, even though the Asiatic has marvellously sensitive organs of hearing on certain occasions?

J. W. PARRY,

Late Executive Engineer, Indian State Railways.

I wrote to Mr. Prideaux-Brune, who is a Deputy-Lieutenant and J.P. for the county, and received the following reply :—

Prideaux Place, Padstow, Cornwall, *February 17th, 1899.*

Some months ago Messrs. Merryweather sent a diviner here, so I witnessed his extraordinary powers. I myself manifested a *slight* power, and when the diviner held my wrists it was intensified—one man, a non-believer, was as successful as the diviner. Another man attempted, and was not in the slightest degree affected, even when the diviner held his wrists. A very strong man tried to wrest this rod from the diviner's hands without success, and, finally, in the struggle the rod broke. In my own case I felt the muscles in my arm between the hand and the elbow for a day or two affected in the same way as if I had a slight electric shock.

CHARLES G. PRIDEAUX-BRUNE.

In this case, as in the former, there is no evidence to show that the involuntary motion of the "rod" had any connection with the nearness of underground water.

I will now cite some evidence that shows (1) how little visible muscular action there is when the twig moves in the hands of a dowser; *e.g.*, Mr. A. Lang says (see below) he “could detect no muscular action;” and further (2) how the amateur in whose hands the twig turns believes he is exerting his will and muscular force to *prevent* its motion.

In *Longman's Magazine* (November, 1897) Mr. A. Lang writes :—

I have thrice been with amateurs, in whose hands a twig twisted over subterranean water. One was a very learned professor of the Greek language, who, having seen the thing done by a water-finder, found that he had the same faculty. The second was a land bailiff, who had discovered his gift in the same way, and employed it for his own purposes on his employer's estate. . . . The third was a lady, whose mother had the faculty, and found her own well by using it. I tried, in company with each of the three. With the two men I failed—my twig would not turn where theirs turned. It did twist where the lady's twisted, and the sensation was curious [“just as if the rod were alive,” Mr. Lang adds in writing to me]; but I am inclined to fancy that the mere resistance and spring of the wood caused it to jerk itself upward at one spot, though I cannot say why it failed to do so at other spots. The bailiff tried holding my wrists while I held the twig, but without effect. I held *his* fingers, when the twig writhed in his hands, but could detect no muscular action on his part. In no case did we dig, or make any attempt to find water where the twigs twisted; all we were able to do was to examine the action of the twig; active at certain spots, passive at others. The forked twig is so held that you resist its natural spring. Of course, no twig at all is used by some water-finders, who rely on their sensations merely, and speak of the twig as a mere index or stage property.

In Sir Herbert Maxwell's pleasant book of sport and natural history, *Memories of the Months*, is an account of experiments with Mullins, a professional, and not a man of education. Mullins had worked at Middleton, and Lord Jersey “had been not only gratified, but astonished at his success.” Experiments were next made at Osterley, before scientific characters . . . Mullins's rod, in this case, “twisted so violently that, when he held it tight, it broke in his hand.” He said that he had a shivering along his spine on these occasions. When he stood on a plate of thick glass the rod did not twist, which he attributed to the non-conducting of electricity. An uneducated man always chatters about electricity. The glass acted, if at all, by way of negative “suggestion.” Mullins was then blindfolded, but here Sir James Crichton-Browne objected that the blindfolding was inadequate. The researches of the Psychical Society have proved (in other experiments) that practically no blindfolding *is* adequate, or, at least, is beyond suspicion. Sir James wanted to introduce cotton wool. Mullins said, “Don't you believe my word?” Sir James replied, “I believe nothing but what I see” (not a very scientific posture, I fear), and Mullins would not play any longer. He was an uneducated man; he did not understand the position; he took Sir James's agnosticism as a personal affront, and so the matter ended. Nevertheless, Sir Herbert says, “were I in straits to find water, I should employ without hesitation a professional water-finder—rod and all—if there remains one as successful as Mullins was.”

Practically the dowser finds water in a paying ratio of successes over failures. *How* he finds it he probably does not know himself. I do not doubt that the rod is wagged (where the finder is honest) by *unconscious* muscular action, as in "table-turning." So we are left at large in that cheerful and luminous field of inquiry, the Philosophy of the Unconscious, or Subconscious. Some indication, through the normal senses perhaps, or through some unexplored sense, reaches the successful finder, and, unconsciously obedient to this hint, he unconsciously uses his muscles and wags the stick. I conceive that to be the humour of it, but a large number of experiments is needed before one can even raise a good presumption in favour of the hypothesis.

The following letter which Dr. Hodgson has sent me is an illustration of the involuntary and apparently uncontrollable motion of the rod in the hands of a mining engineer, who tried it for the first time:—

350, East 69th Street, New York, *July 23rd, 1892.*

I see a paper on the divining rod in Vol. II. of your *Proceedings*, and would bring to your notice a curious experience of mine with regard to this which I published in the *Engineering and Mining Journal* of December 9th, 1876, which created quite a discussion in that journal.

"I was much interested in that part of a lecture on Mining by Professor W. Smyth, F.R.S., of the Royal School of Mines, London, relating to the use of the divining rod for finding mineral veins or subterranean springs of water, and published in your journal of September 9th. Because a reason cannot be given for an alleged phenomenon is no cause why it should not exist, or if existing, why it should not be inquired into. Most of the discoveries of science would not have been made, had inexplicable phenomena been set aside as being unworthy of attention because no reason could be given for them at the time when first observed.

"I have heard of the Cornish Witch-hazel from my childhood, but always thought it a fable till one day in the woods of Pennsylvania one of my men mentioned that springs could be found by means of a forked switch of any kind of fresh cut hard wood. I laughed at the idea, but he insisted, so I made a trial by standing astride a small brook, one branch of the fork in each hand, the stem standing out horizontally. What was my amazement to find that the latter bent slowly down with a force I was entirely unable to counteract. The experiment was quite sufficient to make me credit all that had been attested of the divining rod, though I have never tried it over mineral veins.

"Professor Smyth says that all persons are not able to produce the effect, which may very well be, just as Spiritualists pretend that some persons are naturally more adapted to act as mediums than others. And may not the power exerted by them on chairs and tables have some connection with the force manifested by means of the divining rod? Unless, indeed, the force which makes the twig bend be caused by the vital power of the person holding it. These, however, are the very questions to be solved, should a competent person take them up.



"I am glad to see that so distinguished a man as Professor Smyth has the courage to assert his belief in what it is the fashion to consider as an exploded fallacy by persons who have never tried it or made proper inquiries regarding it."

That some unknown force is at work I am confident. It seems melancholy that the very men who pooh-pooh such phenomena merely because they cannot explain them, are of the class generally called scientific.

D. COGHLAN.

Dr. Hodgson wrote to Mr. Coghlan suggesting that unconscious muscular action was the probable cause of the motion of the rod; Mr. Coghlan replies as follows:—

350, East 69th Street, New York, *July 29th*, 1892.

Your favour of yesterday received. In answer to your suggestion that my own unconscious muscular force caused the bending of the rod, I can answer decisively, No!

When I said to my informant regarding the phenomenon that I could by my muscular exertion prevent the bending, he expressed the situation forcibly by saying the bark of the twig would be first twisted off. Of course in this he exaggerated, but still it expressed, rather forcibly to be sure, the tendency of the twig's point downwards. The switch is held with the palms of the hand turned upward and the branches of the fork grasped firmly, the main stem projecting horizontally. I cannot conceive how any unconscious muscular action could cause the horizontal stem to become vertical, which position is attained by a twisting of the fibres of the wood.

I have lately tried the same experiment under what I considered similar circumstances, but failed completely, whether owing to the wood used not being of the proper kind, (for you may remember that in Cornwall they suppose the witch-hazel the proper thing to use in searching for mineral veins), or whether owing to decay of nervous power on my part through age, I cannot tell.

D. COGHLAN.

The opinion so emphatically expressed by Mr. Coghlan, that the motion of the rod was *not* due to any muscular action, conscious or unconscious, on his part is shared by nearly all who find the rod twist in their hands. Here, for example, is one of numerous instances I might cite. In a letter published in the *Standard* newspaper for January 2nd, 1889, Mr. W. Bell, of East-Lyss, Petersfield, states that after seeing a dowser at work, he tried the forked twig himself and to his utter amazement, when he came near the spot at which the twig moved with the dowser, he saw the point bend forward and then it suddenly turned down towards the ground as he crossed the spot. He adds:—

Some force independent of my will, and acting contrary to my muscular resistance, drew the point of the twig towards the earth. Of this I am as firmly convinced as I am that the sun rose yesterday. It was not a matter in which I *could* have been self-deceived, and the same effects could not be produced by trickery [voluntary effort is no doubt meant].

In the same number of the *Standard* a professional dowser, J. Blanchard, of Wisbech, writes to say that he has "proved the rod is not moved by the involuntary action of mental expectancy on the muscles of the performer" (!), by putting on a pair of gloves, when the rod ceases to move. No doubt the rod would be more difficult to manipulate, whether voluntarily or otherwise, with gloves on. But all these results, however, like the standing on insulating supports, etc., are mainly due to what Mr. A. Lang, in the passage just quoted, calls "negative suggestion." That is to say, anything which gives rise to a sub-conscious suggestion determines the motion or arrest of the rod.

In connection with this I may cite the distinguished anthropologist, Dr. E. B. Tylor, F.R.S., who, in a lecture delivered at Oxford in 1883, relates his experience with the divining rod as follows:—

"I happened to be staying at a friend's house in the Mendip district, where it [the divining-rod] is still used by well-sinkers and miners, and at my request a regular practitioner was sent for. . . . It does not appear that he fraudulently moves the rod, but my sensations led me to agree with Chevreul that the slight movements of the hands are unconsciously guided to accumulate into impulses sufficient to cause the twig to dip or rise. I noticed that when I could allow my attention to stray, the rod would from time to time move in my hands in a way so lifelike that an uneducated person might well suppose the movement to be spontaneous. It is hardly necessary to say that the rod always moves where the bearer's mind suggests an object." [Dr. Tylor then gives an amusing illustration on this point: the dowser having stated the rod always moved when he reached the *main* spring underground, the rod also moved over a watch, the dowser suggesting that it did so because it was over the *mainspring* of the watch; his mind, Dr. Tylor remarks, being controlled by the verbal association into a delusive analogy of effects] (*Nature*, May 17th, 1883, p. 58).

## § 2. *Evidence that the Motion of the Rod is due to Unconscious Muscular Action.*

We will now pass on to evidence which shows that the motion of the rod is really due to the muscular action of the dowser, notwithstanding that there are certain positions in which the rod is held where it seems to be impossible for the dowser to move it. Such, for example, as that shown in the plate on page 134, in my previous Report, where the dowser, Stone, is shown holding a thick forked rod depending from the forefinger, second finger, and thumb of each hand. Mr. Stone himself asserts that there is no movement of his fingers, and that he holds the rod in this way because it cannot be moved by the dowser; but a careful eye-witness, Mr. R. J. Charleton, writes to me as follows:—

I must contradict the assertion that no movement of Mr. Stone's fingers could be detected whilst he is using the divining rod. I watched him most

closely, and distinctly noticed that his forefingers, second fingers and thumbs, between which he held the ends of the forked stick, were strongly compressed upon the pliant wood. At the same time there was an inward twisting action of the fingers which had the effect of raising the apex of the rod. Tremendous muscular force was apparently being used, to such an extent, in fact, that the operator's hands became quite swollen and tremulous when he had completed his experiments. I have myself been able to verify this explanation in my own person repeatedly, though I could not move such thick twigs as Mr. Stone employs, but his muscular development is greater than mine and he is in constant practice.

I, too, watched Mr. Stone carefully in his use of the forked rod. In the Carrigoona experiments, the sudden *leaping up*, as it were, of the twig when Mr. Stone came to the position No. 1 in Fig. 3 was most remarkable, the more so considering the way the twig was held. There was evidently some muscular spasm, as Mr. Charleton says, but it was obviously involuntary, and neither I nor a friend who was with me could imitate the action by any conscious exertion on our part.

But perhaps the best testimony is afforded by the following letter I received from Mr. J. F. Young, of Llanelly, a most successful amateur dowser, animated by a truly scientific spirit. Mr. Young was at first inclined to think that the movement of the rod was entirely spontaneous and quite independent of the dowser, but after I had drawn his attention to the matter, he writes as follows: "I see that the motion of the rod, which is always held in tension by the water-finder, is really due to *unconscious* muscular action; this is specially noticeable with a watch-spring which I generally employ. In fact, I am so convinced in this matter, after endless experiments, that I defy any one to prove the contrary." A Somersetshire incumbent, writing in *Notes and Queries*,<sup>1</sup> gives corroborative testimony, and states that, when holding the rod in the same way as the professional dowser he employed, "the harder I grasped the stick to prevent it turning, the more it turned, till at last it broke in two, and hurt the hand that held it."

But this is no new explanation of the motion of the rod, for the fullest and best account of its motion,—based upon experimental evidence,—is given by the American writer to whom I have already referred, Mr. Emerson, in the pages of the *American Journal of Science* (*Silliman's Journal*) for 1826. The writer shows how startling and apparently miraculous is the sudden motion of the rod, in the hands of a good water-finder,—and remarks, if there be a fraud, *the diviners are themselves the dupes*. It is true, he goes on to say, that nearly every one can urge it to turn in a fashion, but only in the hands of a very few does it move, not only without urging, but contrary to their best

<sup>1</sup> Series 1, Vol. X. (1854), p. 155.



efforts. He himself tried again and again, but failed. At last, one day, watching a young and successful diviner, he noticed the peculiar spirit and air of determination with which he handled the rod.

Hoping to catch his lively manner [Mr. Emerson says] I took the rod and tried my hand again. When I got to the bank of the rivulet the rod began to move, and I could not restrain it. He who for the first time in his life has received an electric shock will recognise the sensation which I experienced when I felt the limbs of the rod crawling round, and saw the point turning down in spite of every effort my clenched hands could make to restrain it. In this contest between myself and the rod the bark was stripped off the twig. The secret appeared to be to hold the rod in a *spirited* manner [by this he apparently means a determined and confident, not a weak and hesitating, manner], for since then the rod has never failed to move in my hands, nor in the hands of those I have instructed.

Mr. Emerson then gives directions how to hold the rod in order to exhibit its startling motion ; if the diviner releases his firm grasp, he goes on to say :—

The rod can no more turn than an unbent bow can throw an arrow. By grasping the forked rod smartly the bow is strained and then if the rod be pliant and the hands moist, it will creep round slowly and mysteriously. But if the rod be large, its motion when smartly bent may become ungovernable. The direction of the motion of the rod forwards or backwards depends on whether the wrists are allowed to turn to their natural position or are slightly more strained outwards.

Two large goose quills tied together at their tips, or two pieces of whalebone similarly fastened and held as a divining rod, Mr. Emerson found to work quite as well, or even better, than a forked twig. This part of Mr. Emerson's paper is excellent, but of course explains nothing more than the apparently spontaneous and vigorous motion of the rod.<sup>1</sup>

Professor Wadsworth, of Michigan University, has recently published a letter in *Nature*<sup>2</sup> giving a similar explanation of the motion

<sup>1</sup> Mr. Thos. Blashill, F.R.I.B.A. (late superintendent architect of the London County Council) has sent me a lengthy and careful analysis of the illustrations given in my previous Report, showing the method of holding the rod, and he concludes that, "In every case in which the rod is shown in the hands of the water-finder, while he is actually searching for water, the two prongs of the fork are *bent*, sometimes slightly but often very considerably," and he infers that this bending accounts for the motion of the rod. No doubt a considerable strain is put on the forked rod by some professional dowzers, but not by all; the explanation of the motion of the rod is not quite so easy as Mr. Blashill and others who have not themselves seen it in use imagine. Even Mr. Emerson's and Mr. Young's statement that the rod is always held in a state of strain is not invariably true. Moreover the rod used by Bleton and others a century ago simply rested on the two forefingers : we shall deal with this in the sequel.

<sup>2</sup> "A Mechanical Theory of the Divining Rod," *Nature*, January 6th, 1898.

of the twig to that given by Emerson seventy years previously. It is amusing to see how entirely ignorant of the whole subject are many scientific men who are ready to instruct us. Professor Wadsworth says, in concluding his letter :—

The whole secret of the divining-rod seems to lie in its position in the hands of the operator, and in his voluntarily or involuntarily increasing the closeness of his grasp on the two ends of the branches forming the fork. If the foregoing conditions are fulfilled, the twig will always bend down—water or no water, mineral or no mineral. Any one can be an operator, and any material can be used for the instrument, provided the limbs forming the fork are sufficiently tough and flexible.

As I have said, any one can twist the rod if it is held in a particular manner, but I am afraid Professor Wadsworth's contribution can hardly be said to have solved "the whole secret of the divining rod."

Just as a pen or pencil registers *conscious* acts of the brain by small muscular motions of the fingers, so the divining rod indicates *sub-conscious* acts of the brain by small and usually involuntary and insensible muscular motions on the part of the so-called diviner. It is not necessary to call the man who writes or who reads a letter "a diviner," though he is so in a sense, and it is equally unnecessary to call a man "a diviner" who employs a forked stick to indicate the twitching of the muscles of his lower arm. Though the honest dowser himself declares he does *not* move the forked twig, this is simply a proof that the motion is to him both involuntary and unconscious. It is needless to labour this point, but it may be worth while to dispose of it once for all.

How, ask several correspondents, can a man move a stick, the motion of which he is consciously resisting? The muscles, it is urged, cannot simultaneously be doing opposite things. It certainly looks mysterious, but it is not really so to the trained physiologist. Through the kindness of my friend, Dr. Purser, Professor of Physiology at Trinity College, Dublin, I have been able to have this question tested in the person of an amateur dowser, an Irish gentleman, to whom I have already referred, Mr. J. H. Jones, who was good enough to allow himself to be made the subject of experiment in the grounds of Trinity College, Dublin. Dr. Purser subsequently wrote to me as follows :—

T. C. D., December 19th, 1897.

The interview with Mr. Jones was held under rather unfavourable circumstances, owing to the storm, but I think I was able to see the movement by which the turning of the stick is effected. •

The movements by which the stick is turned are :—(1) A rotation of the forearms, or one of them ; (2) a flexion of the inner fingers, by which the

stick is made a lever of the first order with very short distance between the fulcrum and the power, or perhaps a lever of the third order. It was impossible to follow the movements when the stick was rapidly twirled, or when Mr. Jones walked rapidly and the rod suddenly turned and he said : "There is water here." But when he stood over the place where he said underground water existed, and professed to struggle against the motion of the stick, the movement was evident, and I had no difficulty in imitating the movements myself, although, of course, not so dexterously as Mr. Jones did. As to whether the movements are conscious or unconscious I cannot express any opinion,—but that the stick is moved by the muscles of the arm and not by any occult influence cannot be doubted, I think, by any reasonable being.

F. PURSER.

To the lay reader, no doubt, there is considerable difficulty in understanding how this explanation covers all the facts, if such cases as those I have quoted earlier are correctly described. The breaking of the forked twig can only be accomplished by a rigid grasp of one of the forks, and a rotation of the twig by the hand holding the other fork. This is probably what occurs ;—the skill and strength required to do this, without much visible motion of the hand, being as much beyond the conscious effort of the dowser as the feats of a somnambulist or hypnotised person are beyond the power of the same person in his normal state ; and the physiological explanation is probably much the same in the two cases, namely, an automatic concentration and discharge of most of the available nervous energy of the individual into one narrow channel. Only a trained anatomist is competent to give an opinion on this question, for he knows which muscles to observe, whilst the startling effects of auto-suggestion are well known to physiologists.

In support of this view I will here quote an extract from the report of a small committee who, in 1894, critically examined a dowser at work. The committee of investigation consisted of the pathologist and the assistant physician of one of the Bristol hospitals, Mr. Mole, F.R.C.S., and Dr. F. H. Edgeworth,—the latter having made neurology a special study—and the Rev. R. A. Chudleigh, of West Parley Rectory, Wimborne, Dorset. Mr. Chudleigh, who acted as reporter, though not an M.D., has made a life-long study of both physiology and pathology.<sup>1</sup> The committee were therefore well qualified. They were fortunate in securing the co-operation of a skilful amateur dowser, who placed himself, as well as his estate (on the border of the Mendips), entirely at their service. In the course of a lengthy report, Mr. Chudleigh says : "If there be one thing which is perfectly clear, it is

<sup>1</sup> I have since had a considerable correspondence with the Rev. Mr. Chudleigh, and have been much struck with his true scientific habit of mind and wide range of knowledge.



that the movement of the wand is due to an unconscious muscular contraction, just like other muscular contractions, except that it is unconscious." Albeit, he goes on to say, "The violent tremor which convulsed the over-strained arm is itself enough to suggest witchcraft to an ordinary spectator, and yet I am sure that it is nothing more than what is known as *muscle-clonus*." Anatomical reasons are then given to account for the sudden violent motion of the rod, and the report continues: "A precisely analogous phenomenon is seen in those cases where a spinal wound or a spinal poison throws the whole body into universal spasm; but the flexors master the extensors and the back muscles overpower the front ones, the result being the frightful and well-known pose called *opisthotonus*." The writer then points out that the sudden spontaneous tension of the muscles of the arms which occur when the dowser believes himself to be over a spring is probably due to auto-suggestion; "this auto-suggestion makes a diviner positively tetanic when he knows or thinks that water is present."<sup>1</sup> The symptoms described in the foregoing were more strikingly exhibited by the late Mr. W. Scott Lawrence than by most other dowsers; a vivid description is given by an eye-witness on p. 69 of the previous Report.<sup>2</sup>

Severe muscular spasm, as is well known, can be caused by suggestion in hypnotic subjects. In Hittell's *Somnambulism and Cramp*, New York, 1860, muscular contractions are described as caused by auto-suggestion—*e.g.*, "If Miss A. ventured to eat with a silver spoon, the muscles of the mouth soon became rigid, etc." Every physician is acquainted with cases where often alarming muscular spasm, as of the glottis, is due to purely *ideo-motor* action. "The *idea* of a particular motion," as the physiologist Müller long ago remarked, "determines the current of nervous action towards the necessary muscles, and gives rise to the motion independently of the will." Dr. Edgeworth sends me an instance of this under his care at the Bristol Infirmary. He writes:—

A man who had an affection of the spinal cord was given mercury by injections into his arm. His legs (the part affected) got well, but he developed the idea that his arm had been poisoned, and as a result has a

<sup>1</sup> See *Farm and Home*, May, 1894, p. 132.

<sup>2</sup> When Lawrence came over a place where he asserted an underground spring existed—and which proved true on sinking a well at the spot,—the writer states:—"I can only describe the antics of that twig as a pitched battle between itself and him! It twisted, it knocked about, it contracted and contorted the muscles of his hands and arms, it wriggled, and fought, and kicked, until it snapped in two—and then—what made it painful to watch until I got used to it, the old man reeled, and clutched hold of any one nearest to him for a few moments. It evidently exhausts him very much, though afterwards I asked him what effect it had on him, and he said it only made his heart beat *most* violently."—*Letter to Mr. W. Whitaker, F.R.S.*

hysterical paralysis of the injected arm. These auto-suggestions are very difficult to remove. . . . In my opinion they afford an explanation of the motion of the "divining rod."

F. H. EDGEWORTH.

### § 3. *Consideration of Bleton's Motor-Automatism.*

It may be argued by some readers that the foregoing explanation of the motion of the forked twig cannot apply to the singular and rapid rotation of the slightly curved rod used by Bleton. Certainly the facts, as carefully narrated by Dr. Thouvenel on pp. 53-76 of his first *Mémoire*, were very curious and are worth attention.

In the first place it is wholly improbable that the rotation of Bleton's *baguette*, any more than the twisting of the forked twig by the dowsers of to-day, was due to any *conscious* muscular effort on the part of the *sourcier* or water-finder. In fact, if the reader will try to rotate a slightly curved stick resting on his forefingers, as shown in Fig. 18, p. 274, he will realise how difficult it is to accomplish, keeping, withal, his hands and arms as far as possible motionless. But difficult as it is to imitate this motion, it is not *impossible*, as Lalande showed in the passage I have quoted on p. 273, though I have no doubt the attempt to imitate the rotation was a very clumsy one. For the effects of involuntary muscular action are surprisingly difficult to imitate by voluntary effort. That some muscular action really took place on Bleton's part is seen from Thouvenel's own admission, opposed as he was to this idea. On p. 114 of his first *Mémoire*, he says, in a passage I will translate :—

I have perceived several times that the approach of the arms and a certain *tour de main* on the part of the *sourcier* [Bleton] contributed to give the *baguette* its first impulse of rotation over *feeble* springs.

It is true that he does not notice this at other times: but a careful observer (M. le Comte de M——) quoted by Thouvenel on p. 188 of his *Mémoire*, says :—

I have reason to suspect that [the motion of] the *baguette* is a little aided by an almost insensible movement of the shoulder.

Albeit the Count himself tried without success to turn the *baguette* at a spot where it moved with Bleton. Thouvenel tells us Bleton judged with singular accuracy of the *volume* of the hidden spring by the rate of rotation of the *baguette*, as well as by the violence of the convulsive movements of his body which simultaneously occurred.<sup>1</sup>

<sup>1</sup> I have mentioned already (p. 260) that the motion of the rod with Bleton (as with some dowsers of to-day) was subsidiary to the convulsive spasm and *malaise* which he experienced, when, as he alleged, an underground spring existed beneath.

Furthermore, he was able to judge of the *depth* of the spring in the following manner: on leaving the spot beneath which he alleged water was to be found, the baguette ceased to turn, but when he had gone a certain distance the baguette suddenly gave a single rotation in the *opposite* direction. The distance at which this occurred from the spot at which it stopped turning was, according to Bleton, the approximate depth of the spring. The results given in the depositions quoted by Thouvenel show that Bleton was usually fairly correct in his estimate of the depth when it did not exceed 30 ft. ; beyond that he did not profess to be able to judge. One witness, however, states that even at small depths Bleton was often at fault.

The rate of rotation and the retrograde as well as the direct motion of the baguette were no doubt simply ideomotor phenomena, the result of an involuntary muscular effort prompted by some auto-suggestion. The difficulty here, as elsewhere in our inquiry, is to trace the *genesis* of this probably subconscious suggestion.

#### § 4. *Apparent Transmission of Motor-Automatism from a Sensitive to an Insensitive Person.*

There is, however, another problem connected with the motion of the rod which we must not pass over, and that is, the remarkable fact so frequently noticed that when the dowser lays hold of the wrist or hand of a person with whom the rod will not turn, the twig instantly moves. I have quoted numerous instances of this from excellent witnesses on pp. 278 and 279. This apparent transmission of the power of involuntary muscular action was noticed by Thouvenel to occur with Bleton a hundred years ago. On p. 59 of his *Mémoire* Thouvenel says that, when Bleton placed his fingers on the hand of a person with whom the baguette would not turn, then the rod instantly rotated when they approached an underground spring. M. le Comte de M——, whom I quoted just now, states that, whilst the baguette would not move in his hands, yet when Bleton held them the rod then began to turn, much to his astonishment. A few years later Amoretti independently discovered the same thing when Pennet touched his hands. He states the rod then turned against his (Amoretti's) will whenever he stood over veins of metal. I have referred to this on p. 247. Compare this with the similar experience (only with a forked rod) described by a mechanical engineer, Mr. Morton, given on p. 172 of the present Report, or with the precisely corresponding experience of an able lawyer (the land agent to Trinity College, Cambridge), Mr. Woolley, given on p. 278.

There can, therefore, be no doubt about this curious fact, which seems to occur most conspicuously only with notable dowsers, such as



Bleton and the late J. Mullins. What is the explanation of it? I expect it will be found not in the transmission of any voluntary or involuntary *motion* from the sensitive to the insensitive person, but in the transmission of a *suggestion* to the latter. If so, any mode of impressing such a suggestion would do as well, if it be emphatic and *indirect*. The charming away of warts, of which we have such striking and well authenticated instances, is a case where any kind of indirect suggestion will do, if it be strongly impressed on the recipient.<sup>1</sup>

And by the word *suggestion* is meant, as Professor Pierre Janet defines it, *the influence which one person exercises upon another independently of the voluntary consent of the subject*.<sup>2</sup> That is to say, certain individuals will submit to a foreign influence and obey it, without having consented or intended to obey and without even knowing they were obeying. Now the influence of suggestion is most strikingly seen in these involuntary or automatic phenomena, and cases analogous to the apparent transmission of the motion of the *baguette* from one person to another occur with the *pendule* and in the so-called *willing game*. If one person, A, finds the *pendule* will not oscillate in his hands and another, B, finds it does oscillate freely with him, then when B clasps the free hand of A, the *pendule* now oscillates for A or the direction of the oscillation can be instantly changed by C clasping B's free hand, and again changed if D clasps C's other hand and so on through a large circle; B, C, and D desiring the change to occur. Again, in the "willing game," I found in 1879 that

The intervention of a second person (who was ignorant of what had to be done) between the willer and the subject, the hands of each resting on the shoulders of the one in front, did not seriously interfere with the result attained.<sup>3</sup>

Furthermore, with a sensitive subject, if the operator or willer merely placed three fingers lightly on the back of the subject's head, the latter would write correctly words known only to the operator, even if Greek words and characters were used, though the subject knew nothing of Greek.<sup>4</sup> Professor Janet gives a simpler case where

<sup>1</sup> See *Journal S.P.R.*, Vol. VIII. (1897-98), pp. 7, 40, 96, etc., also an admirable work by Miss Feilding on *Faith-Healing*, Chaps. I. and IV. (Duckworth and Co., 1899). In hypnosis a *direct* suggestion appears effective, whereas in the normal state an *indirect* suggestion seems most potent. *e.g.*, touching or stroking the warts, tying a thread round the wrist, etc. But why is it that only *some* persons have this suggestive power? Is it the faith of the charmer in his remedy or the faith of the patient? And why is *faith*, with its necessary self-surrender, the  *nexus* between the conscious and the sub-conscious life? the powers of the latter being so miraculous, because so foreign to the former.

<sup>2</sup> *L'Automatisme Psychologique*, by Professor Pierre Janet, p. 140; see also *Les Suggestions Hypnotiques*, by Professor Paul Janet.

<sup>3</sup> *Proceedings S.P.R.*, 1882, Vol. I., p. 50.

<sup>4</sup> *Ibid.*, p. 51.

the operator holds the left hand of the subject and the latter with his right hand writes the words known only to the operator.<sup>1</sup>

If we admit telepathy, *i.e.*, true thought-transference, as a *vera causa*, no doubt it plays an important part in many of these phenomena of suggestion, and my original object in 1879, when making the experiments I have described, was to show that the theory of the transmission of an idea by involuntary muscular action had to be pushed to such a grotesque extent that it became easier to believe in a purely *mental*, rather than in a *muscular* transmission of the suggestion made by the operator. Whichever view is taken, the important part played by *suggestion* in all these phenomena is unquestionable, and it was to this fact I drew the attention of psychologists, I believe for the first time, in a paper read before the British Association so long ago as 1876.<sup>2</sup>

### § 5. *Historical Note.*

In concluding this part we should not forget that one of the first scientific men in this century who drew attention to the varied effects of involuntary muscular action was Chevreul. In his work on the divining rod (*La Baguette Divinatoire*, Paris, 1854), Chevreul shows how the rod may be moved in this way. The principle which Chevreul enunciates, and which he maintains accounts for many diverse phenomena, is one with which we are now of course perfectly familiar; he thus expresses it:—

Le développement en nous d'une action musculaire qui n'est pas le produit d'une volonté, mais le résultat d'une *pensée* qui se porte sur un phénomène du monde extérieur, sans préoccupation de l'action musculaire indispensable à la manifestation du phénomène, va servir de centre de raillissement aux faits disséminés; je le désignerai par l'expression de *principe du pendule explorateur*.<sup>3</sup>

Twenty-one years before the publication of his work on the *Baguette*, Chevreul had addressed a letter to the famous Ampère, (published in the *Revue des Deux Mondes* for 1833) in which he clearly states that the movement of the *pendule*, which was exciting much attention at that time, was due to “un mouvement musculaire de mon bras, quoique insensible pour moi” and he points out the intimate connection between the will or intention of the operator and the resultant movement of the little ring or ball suspended by a thread from his fingers. He thus clears away a mass of perplexing and misleading observations which had been published by numerous

<sup>1</sup> *L'Automatisme Psychologique*, p. 373. Paris, 1889.

<sup>2</sup> Printed in *Proceedings S.P.R.* for 1882, Vol. I., p. 240.

<sup>3</sup> Chevreul, *La Baguette Divinatoire*, p. 187.

previous experimenters on the *pendule*, all of whom believed some intimate connection existed between the oscillation of the little ball and the electric state of the operator or of the object that was tested.<sup>1</sup> A German savant, Professor Gilbert, had before this shown in Gilbert's *Annalen der Physik*, Vols. 26 and 27, how worthless were the inferences drawn from these experiments by Ritter, Amoretti, Gerboin and others; Gilbert subsequently published his papers in 1808.<sup>2</sup>

In the numerous papers and books on the *baguette* and *pendule* published on the Continent between 1796 and 1816, the controversy, often violent in tone, does not turn upon the main question whether the so-called diviner or dowser could or could not discover underground water better than ordinary mortals,—for except in Thouvenel's original *Mémoire*, little or no evidence is adduced on this point—but whether “animal electricity” was or was not the cause of the twisting of the rod. The disputants, as usual, cared less to know the actual facts than to prove their particular theory to be true. Moreover the attention of the scientific world at the early part of the 19th century was directed to electricity by Volta and Galvani's discoveries, and it was natural that many investigators should see in the sudden and mysterious motion of the *baguette* and the *pendule* an effect analogous to the spasmodic contraction of the legs of a dead frog which Galvani had shown was due to an electric stimulus. In fact, Volta himself was consulted and took part in some of these experiments with the *baguette*, but his opinion is not stated.

From the *baguette* and the *pendule* public attention was afterwards directed to “table-turning,” and the illustrious Faraday, in 1854, conclusively proved by experiment that it was quite possible for the sitters to make tables gyrate by involuntary muscular action on their part.<sup>3</sup> Chevreul refers to Faraday's experiments on p. 220 of his

<sup>1</sup> See, for example, Professor Gerboin's *Recherches Experimentales sur un nouveau mode de l'action électrique*, Strasbourg, 1808; or Ritter's and Amoretti's writings from 1800 to 1807 on *Der animalische electrometrie, die baguette, und der pendel*, forming the subject of these papers.

<sup>2</sup> *Kritische Aufsätze über die in München wieder erneuten Versuche mit Schwefelkies-Pendeln und Wünschelruthen*, von L. W. Gilbert, Halle, 1808. I have to thank Miss E. Stokes for making an abstract for me at the British Museum of this and other old German and Italian works on the subject of the *baguette* and *pendule*.

<sup>3</sup> Faraday's explanation of table turning is undoubtedly a *vera causa* for a certain class of so-called spiritualistic phenomena, but it is only applicable within a limited range, as every investigator of spiritualism knows. It is of course not impossible that some of the phenomena of the divining rod may, in like manner, be found to transcend any explanation now recognised by science; for the varied manifestations of automatism insensibly pass from the normal to the supernormal. At present, however, we are not, in my opinion, justified in assuming the *motion* of the rod to be explicable by other than known causes.



work on the *Baguette*, and groups all these cognate phenomena under one common explanation: the essential factors, according to both Faraday and Chevreul, being—(1) the *intention* of the operator that the movement should take place in a certain direction, and (2) that he should be *unaware* he is exerting any effort of will, or any muscular force, in carrying out his intention. Professor Ch. Richet has more recently discussed the subject with wider knowledge, but I have not yet read his work.<sup>1</sup> Dr. Liébeault<sup>2</sup> takes much the same view as Chevreul, but believes that the dowser, from gazing at the point of his rod, becomes partially hypnotised, a view that I have also suggested, but the hypnosis, if it exists, is certainly very slight in most dowsers.

In his masterly work, *L'Automatisme Psychologique*, Professor Pierre Janet devotes a section to the consideration of the motion of the divining rod. He confirms Chevreul's view as to the involuntary motion of the rod and *pendule*, but he goes further and shows that these automatic actions are “*1<sup>o</sup> sans le vouloir, et 2<sup>o</sup> sans le savoir.*” Chevreul admits the first, but asserts that the movements are due to a *conscious intention* on the part of the operator to make the rod or pendulum move in a certain direction; the knowledge and thought of the operator are, therefore, involved, though his *will* may not actively co-operate. Janet shows that this is a mistaken view; the movement, as he rightly insists, takes place *without* the conscious intention of the operator. He remarks:—

M. Chevreul pousse aussi loin que possible l'explication des faits par la tendance au mouvement créée par les images conscientes, mais quand les faits dépassent cette théorie, il retombe dans les explications banales par la fourberie et la simulation. . . . Il faut aller plus loin que M. Chevreul et, après avoir admis des actes sans volonté, il faut parler des pensées sans conscience [consciousness] ou en dehors de notre conscience.<sup>3</sup>

Chevreul, we see from the foregoing, was no exception to that familiar type of mind which fancies trickery or simulation afford the only explanation of facts that lie beyond the range of its experience or its theories. In one of his conversations with Eckermann, Goethe shows how every new scientific truth has to encounter the fierce opposition of this class (see the motto on p. 132).<sup>4</sup>

Dr. Hodgson sends me, as this sheet was passing through the press, a case (given in the foot-note on next page) he has received from an

<sup>1</sup> *Des mouvements inconscientes*. Paris, 1886. This work is dedicated by M. Richet to Chevreul.

<sup>2</sup> *Le Sommeil provoqué*, p. 241. Paris, 1889.

<sup>3</sup> *L'Automatisme Psychologique*, p. 375. Paris, 1889.

<sup>4</sup> *Conversations of Goethe* (Bohn's Standard Library, p. 47).

American friend which illustrates the entire inadequacy of Chevreul's point of view.<sup>1</sup>

Professor Pierre Janet goes further and shows that in these and other phenomena of automatism,

Il y a donc, dans certains cas, plus qu'un acte automatique, manifestation involontaire d'une image visuelle et auditive ; il y a une véritable action sub-consciente, une véritable collaboration de la seconde personnalité avec la première.—(*L'Automatisme Psychologique*, p. 374.)

The class of persons who exhibit one or more of the varied phenomena of automatism are those whom M. Janet calls *les individus suggestibles* ; persons who are readily influenced by suggestion from within or without, whose mind is easily dominated by a single idea. This may explain why it happens, as was noticed 150 years ago, that the best dowzers are not educated people, but more or less ignorant persons, who—according to Pryce—(*Mineralogia Cornubiensis*, p. 118) “hold the rod without puzzling their minds with doubts or reasonings.” Such persons best exhibit the “reflex” action to which the motion of the rod is due, the prompting of suggestion not being interfered with by volition. But where will and reason are supreme, such automatic actions are brought into subjection by the intelligence. In other words, when our conscious self, which speaks through voluntary muscular movements, is dominant, the subconscious self is submerged. Hence in order that any subconscious suggestion may take effect in some form of automatic action, the reason and will must be in abeyance.

*In fine, the controlling force of SUGGESTION is to the subconscious life and its expression in involuntary muscular action and tissue-change what the WILL is to the conscious life and its expression in voluntary muscular action.*

#### SUMMARY.

Summing up this lengthy discussion we see that the curious phenomena attending the *motion* of the so-called divining rod are capable of explanation by causes known to science. They are not isolated phenomena, but belong to the large group which exhibit in various ways the play of motor automatism, and illustrate in a striking manner the profound effect which *suggestion* has in determining, directing or modifying these phenomena.

<sup>1</sup> “New Lebanon, N.Y., July 1st, 1900. I spent a whole day nearly with an old gentleman here—a farmer, rather above the average in intelligence—a man of much local influence. He says that ten years ago he laughed at the witch-hazel ‘superstition,’ thinking it nothing more. About that time he bought a farm, which he found—after buying—to be poorly watered. A neighbour, coming with a threshing machine, had difficulty in getting water for his engine. This neighbour said one of *his* neighbours had found water with the ‘witch-hazel crotch.’ My man sneered at the idea. Thresher man persisted ; didn’t know that it would work with him, but might with another. Got a stick ; tried it ; wouldn’t work. Gave the stick to my man. The

When some instrumental means is used to exhibit these involuntary muscular movements, it is desirable to employ a generic term, such as the word *autoscope*, as suggested in the previous Report. The divining—or dowsing—rod is, therefore, a convenient out-of-door autoscope, and when held in a state of strain, as it often is, becomes, from its unstable or balanced equilibrium, very sensitive to almost imperceptible muscular action on the part of the dowser. (See, on this, Professor de Mortillet's own observations, given on p. 244.)

As I said in the previous Report: It is just because these automatic actions appear to be so novel, and detached from ourselves, that they are apt to be so misleading to some and so mischievous to others. Interpreted on the one hand as the play of a wonderful occult force, science has refused to have anything to do with phenomena which seem to obey no physical laws, but are capricious and self-determined. Interpreted on the other, truly enough, as the exhibition of a free and intelligent agent, some infernal or discarnate spirit has been fixed upon as the cause, and a fictitious authority for which there is no warrant has been given to their indications. Whether in any case these intelligent automatic movements exhibit information outside the memory of the individual who uses the autoscope; or a knowledge beyond that which may have been unconsciously derived (*a*) from those present by sign-reading or thought-transference, or (*b*) from the surroundings of the automatist, by his hyperæsthetic discernment of faint indications, is a problem which can only be solved so as to gain general acceptance by long and patient enquiry, of which our *Proceedings* are an earnest, and to which this monograph may afford a small contribution.

latter took it merely in complaisance; walked along a few rods; suddenly it went down. He couldn't stop it. Thought it was an involuntary muscular contraction; tried it again; held it as tightly as possible; passing over the same line, a few rods down the hill, it turned again, and so strongly that the bark 'peeled' in his hands. The thresher man declared there was water beneath. My man said he should want to see it before he believed it, the surface giving no indication of it. Thresher man wanted water for his boiler and dug. Only a few feet down struck a small stream, sufficient to feed his boiler. Since then my man has experimented a good deal. As a result of his work, he now has a small reservoir on the hillside back of his buildings, in a place where no water was known before, which supplies his barn the year round and affords a limited supply for use in case of fire. The reservoir is fed from two small springs discovered by the turning of the witch-hazel fork in his own hands. While with me he walked about over a good deal of territory. The fork turned down every time he passed over a trickle of visible water, without exception. It also turned several times where no water was visible or suggested. In two of these places I and a man with me dug down a few feet. In the one case we found a vein in the rock filled with iron pyrites crystals; in the other we struck a sheet of solid slate rock about 30 in. down, which we had neither tools nor time to drill into or through: there is an old tradition of lead veins—from which the Indians used to smelt out lead—in the very place where his fork turned over the slate rock."



## PART XII.

## THE MALAISE OF THE DOWSER AND ITS ORIGIN.

Nearly all dowsers assert that when the rod moves in their hands, or when they believe that underground water is beneath them, they experience a peculiar sensation, which some describe as felt in the limbs like the tingling of an electric shock, others as a shivering or trembling, and others as an unpleasant sensation in the epigastric region. With all there is more or less of a convulsive spasm, sometimes of a violent character. This *malaise* is very marked in some cases, but not experienced in others. That these physiological disturbances have a purely psychological origin is obvious—(1) from the fact that they are not experienced when the dowser is off duty, that is, when he has no suspicion that he is in the neighbourhood of underground water, and (2) that like effects are not produced by the much greater masses of visible water in rivers, lakes, or the sea. The interesting point is that these psycho-physiological phenomena have a real existence; they exist among dowsers in all countries, and can be traced back, as historical investigation shows, for upwards of two centuries. In the preceding Report I devoted an Appendix to this subject, and to avoid repetition would beg those of my readers who are interested to refer to the cases I have there quoted.<sup>1</sup>

Let us briefly note the principal facts. In the first place it is not, as some imagine, only when the “diviner” is in the presence of underground *water* that this physiological disturbance occurs. In the *Journal des Savans* (Savants) for January, 1693, a copy of which I possess, a physician of some note, Dr. Chauvin, writes that, when the well-known Jacques Aymar was sent for to trace, by means of his rod, the murderer of a Lyons *Marchand de Vin*, Aymar was taken into the cellar where the murder was committed; suddenly his *baguette* moved violently, and he was seized with convulsive spasms. Dr. Chauvin, who was present, adds:—“Il ne fut pas plutôt entré qu’il se sentit tout ému, et que son poux s’éleva comme une grosse fièvre.”<sup>2</sup>

<sup>1</sup> *Proceedings S.P.R.*, Vol. XIII., p. 272, *et seq.*

<sup>2</sup> Aymar, as my readers probably know, set out on the track of the murderer, who had hitherto baffled all the attempts of the police to discover his whereabouts, and, guided by his *baguette*, tracked him “for 45 hours on land and 30 hours on the water,” crossing and recrossing the Rhone, until at last he pointed out a man, found in a soldiers’ camp, as the assassin. The man was arrested, taken to Lyons, tried, found guilty, and subsequently confessing his crime, was put to death by being broken at the wheel. There are numerous corroborative official depositions in this remarkable case; among others is the *procès verbal* of an eye-witness, M. de Vagny, the Procureur du Roi.

A century later another distinguished French physician, Dr. Thouvenel, independently notices much the same thing with the water-finder, Bleton. Dr. Thouvenel gives a detailed medical report of his own long-continued observations, and states that when Bleton believed he was over a subterranean spring he was seized with an extraordinary *malaise*, which affected his diaphragm and produced a sense of oppression in the chest ; at the same time a shivering sets in and the pulse falls, his body trembles, and, in a word, he exhibits "all the characteristics of an attack of convulsive spasm."<sup>1</sup> Similar symptoms manifested themselves in the Prior of a convent at Autun, who was an amateur dowser and contemporary of Bleton. A few years later the Italian *savant*, Amoretti, noticed the same symptoms occur whenever the lad Pennet came over a vein of mineral ore or of coal. Amoretti states that a surgeon, Sanzio, an amateur dowser, found his pulse accelerated twelve to fifteen beats per minute when the rod moved in his hands (see p. 247).

Dr. Mayo, F.R.S., who, as Professor of Anatomy and Physiology in King's College and in the College of Surgeons in London, was a most competent observer, describes corresponding symptoms which he observed in 1847 in a youth in Russia. The lad had never seen a "divining rod" before, but when Dr. Mayo instructed him how to use it, and made him walk over a spot where he had reason to believe an underground spring existed, the forked twig twisted round, much to the lad's astonishment, and at the same time Dr. Mayo states the lad declared that

he felt an uneasy sensation which quickly increased to pain at the pit of the stomach, and he became alarmed, so that I bade him quit hold of the rod, when the pain ceased. Ten minutes later I induced him to make another trial ; the results were the same.<sup>2</sup>

As Dr. Mayo was apparently unaware of Thouvenel's writings, he could hardly have anticipated or suggested the *malaise* experienced by his subject, but the effect observed was doubtless due to the same psychological cause as in the previous cases.

Abundant modern instances of a similar physiological disturbance and convulsive spasm occurring with various dowsers in different countries will be found in the cases cited in the previous and present Report (see, *e.g.*, pp. 230, 244, 245, 251, 252, etc.). Mr. J. F. Young, whose experiences as an amateur dowser are given on p. 219, says :—

I have noticed, when divining, unpleasant and peculiar symptoms always occur when I am over an underground spring ; often a convulsive feeling and staggering comes on.

<sup>1</sup> *Mémoire Physique et Medicinal*, p. 53 ; Paris, 1781. The passage is quoted in full in the previous Report, *Proceedings S.P.R.*, Vol. XIII., pp. 272 and 273.

<sup>2</sup> *Truths Contained in Popular Superstitions*, p. 18 ; London 1856.

He goes on to describe how the sensation is chiefly felt at the epigastrium, and that his father, who was also an amateur dowser, used to stagger and vomit when the rod turned in his hands. An experiment was once made with old Mr. Young to test whether these symptoms were genuine; it is described in detail in my previous Report, p. 276. Mr. Young was carefully blindfolded and led about by a circuitous route, but directly he came over the spot where he had been seized with these symptoms before, and which had been purposely marked, "he reeled as before and would have fallen if I had not held him up. Directly he came off the place he was all right." The convulsions that seized the famous dowser, Mr. Lawrence, whenever he came to a place beneath which he asserted underground water to exist, have been described on a previous page (foot-note, p. 290).

There are some sceptical friends who would explain these phenomena by asserting that these different dowsers conspired to exhibit similar symptoms as a bit of stage business in order to impress the onlookers. It is, I think, unnecessary to waste time in disputing such a belief if any one cares to hold it.

How, then, are we to explain these curious pathological phenomena? The facts are certainly incontestable and, I venture to think, deserve more attention from physiologists than they have yet received. They are not, however, peculiar to the use of the so-called divining rod, but are found to exist more or less conspicuously in other cases of motor automatism. Professor Pierre Janet has drawn attention to very similar convulsive phenomena and physiological disturbances as associated with other phases of automatism.<sup>1</sup> Prior to this, however, in the first volume of the *Proceedings* of our Society, I pointed out that in trials with the "willing game,"—which is one phase of these varied automatic phenomena,—curious physiological disturbances were often produced, such as dizziness, hysteria, and incipient trance.<sup>2</sup> In fact, a *malaise*, manifesting itself in different ways, and with different degrees of intensity in different subjects, is a usual concomitant of motor automatism and its allied phenomena.

The singular connection of visceral sensation,—a visceral consciousness as it were,—with a particular psychical state is familiar to us all in emotion<sup>3</sup>. Emotion, in fact, is a *feeling* excited by an idea or train of ideas, and therefore the sensations experienced by the dowser

<sup>1</sup> *L'Automatisme Psychologique*, p. 208, etc.; Paris, 1889.

<sup>2</sup> *Proceedings* S.P.R. for 1882, Vol. I., p. 57.

<sup>3</sup> Professor James holds that our feeling of the bodily changes as they occur is the emotion; he writes, "If I were to become corporeally anæsthetic, I should be excluded from the life of the affections, harsh and tender alike." Physiologically, may we not really define emotion as certain visceral or vascular disturbances, set up by *suggestion*, the sensation of these disturbances being the feeling of emotion?



are strictly *emotional disturbances*. Whether emotion is primarily a cerebral process, as some physiologists maintain, the visceral or vascular disturbance being secondary; or whether, as other eminent physiologists hold, the psychical process of emotion is secondary to the excitation of the visceral organs,—through certain stimuli causing the discharge of a nervous impulse into those organs,—is a matter that does not concern us here, albeit physiologists may find in the facts I have cited some fresh light thrown on this controversy. The point of interest to us is that (1) the *malaise* or other sensation felt by the dowser is probably an emotional effect, and (2) the fresh evidence afforded of the nexus existing between emotion and muscular action, whether this latter be conscious or, as with the dowser and his rod, unconscious. As Professor Sherrington, F.R.S., an able physiologist, in a recent paper on Emotion (*Nature*, Vol. 62, p. 331), has said :—"It would be consonant with what we know of reflex action if the spur that started the muscular expression should simultaneously and of itself initiate also the visceral adjunct reaction."

Furthermore, in many cases where subconscious acts are performed, as M. Janet points out, a state of *partial catalepsy* supervenes. Catalepsy, as Dr. Ochorowicz has shown, is a state of *mono-ideism*,<sup>1</sup> that is, a "mental condition which concentrates every action upon one single and dominant idea and is not counterbalanced by any other." Now this is precisely the condition of the dowser when he "sets" himself to dowse, and in some cases he passes into a state of complete catalepsy when the idea culminates. It is not, therefore, a question of underground water or mineral ore, but merely *the result of suggestion acting upon a state of mono-ideism*.

The *malaise*, or other sensation felt by the dowser, is, therefore, in all probability, *an emotional disturbance, the mind being dominated by a single idea and the subject being a person on whom suggestion is operative*: using the word "suggestion" in the sense which I have already defined as an impression or influence exercised without the knowledge or consent of the subject being concerned.

We have now narrowed the issue down to the problem of "*how does this subconscious suggestion arise in the case of the successful dowser?*" Here we enter upon the final stage of our inquiry.

<sup>1</sup> *La Suggestion Mentale*, p. 112; Paris, 1887.

## PART XIII.

ORIGIN OF THE STIMULUS THAT GIVES RISE TO THE MOTOR AUTOMATISM  
OF THE DOWSER.§ 1. *Is it Derived through the Ordinary Channels of Sense?*

It now remains for us to try and ascertain what gives rise to the suggestion or stimulus which causes the involuntary motion of the rod—in other words, what is it originates the motor automatism of the dowser? What is it “pulls the trigger”? Evidently this suggestion or stimulus cannot be purely accidental and worthless, for, as we have seen, the operations of a good dowser are attended with unquestionable success, a success far beyond that which mere chance coincidence can account for. This being conceded, we now turn to the possible explanations already stated at the beginning (p. 143).

(i.) *Is this success due merely to knowledge acquired by the dowser from experience and observation of the ground and perhaps occasional hints from bystanders?* In that case success would be arrived at by an exercise of judgment, or some would say cunning; shrewd observation and the piecing together of stray information would be the main business of the dowser. Under these circumstances sub-conscious suggestion and motor automatism are wholly out of the question—the motion of the rod must be a purely voluntary act; and if so the forked twig of the dowser, like the wand of the conjuror, becomes a piece of stage property, or of charlatanry, to mystify the public. This has hitherto been the usual scientific view, but it is, I maintain, now proved to be untenable; the motion of the rod is unquestionably, as Professor Janet has said, “*sans le vouloir et sans le savoir*” on the part of the dowser. It is quite possible the experience gained by the dowser, or any observations he has made on the spot, may be of service to him, but if so, it must lie at the background of his consciousness and not form part of a process of *reasoning*. We are thus driven to inquire:—

(ii.) *Is this information or suggestion derived from some sub-conscious process of observation on the part of the dowser, or hyperæsthetic discernment of surface signs too faint or complex to be perceived by the ordinary observer?* There is much to be said on behalf of this view, for surface signs of underground water do exist and for shallow wells such signs are of great value independently of geological knowledge. On p. 248 of the previous Report, I pointed out the nature of some of these signs, and since that Report was published, an excellent little Manual on the discovery of water-sources has been published in France, wherein the

surface indications of underground water are very clearly set forth.<sup>1</sup> Much of this knowledge goes back to ancient times. Vitruvius, Augustus Caesar's famous engineer, gives a lengthy account of surface signs which were employed in his day to find underground water : some appear fanciful, but many are still in use. Pliny, in his *Natural History*, refers to the same thing. Moreover, the letters of Cassiodorus (the statesman and secretary of Theodoric, King of the Ostrogoths), written at the beginning of the sixth century, show that in his time there were certain people held in high repute, and called "Aquilegi," who appear to have been professional water-finders. Cassiodorus explains how this valuable art had arisen from careful observation of the surface of the ground, etc.<sup>2</sup> Now the modern dowser may more or less unconsciously note these indications and,—without being able to give any rational explanation of how it comes about,—the impressions made upon him by these surface signs may create the sub-conscious suggestion which causes the involuntary motion of the rod.

The extraordinary success of the Abbé Paramelle as a water-finder, or "hydroscope" as he was called, supports this view. Paramelle tells us he was guided entirely by the knowledge he had acquired from a careful study of the surface of the ground, and to defend himself from the attacks made upon him, he wrote a treatise to put on record the methods he employed. As this work appears almost wholly unknown in England, I have given an outline of it in Appendix D, see p. 352 ; to this I would refer my readers. But the rapidity and certainty with which Paramelle is stated, on trustworthy evidence, to have located the site, probable depth and volume of water in wells under 50 feet deep, can hardly be accounted for by an exercise of ordinary conscious observation and judgment. It is much more like an act of intuition, for between 1839 and 1854 he had located 10,000 sources of underground water and his failures appear to have been only from 5 to 10 per cent. of the whole. Though Paramelle believed he could teach his system, his pupils did not achieve their master's success and the school he hoped to found died out. On the whole, there can, I think, be little doubt that the French Abbé arrived at his conclusions sub-consciously, though he employed no rod to give automatic expression to his instinctive opinion.

But it is not only in water-finding that the trained scientific observer, such as a field geologist, is often less successful in reading surface indications than those who appear possessed of some instinctive discernment of the signs sought for. Thus, in the discovery of china

<sup>1</sup> I have given a brief notice of this little book by M. Auscher on p. 382 of the present Report.

<sup>2</sup> See Appendix F., p. 370.



clay (the basis of our best English china), I am informed by a high authority, Mr. Fred. W. P. Jago, that :—

The workmen in the china-clay district (near St. Austell, in Cornwall) will point out where a bed of china clay in its natural state of decomposed granite lies hidden under what they term the *slad*. [Mr. Jago goes on to say]: As my father and myself have been engaged in this kind of mining for nearly seventy years, I also could fairly point out where there was a bed of china clay below the surface, but I would defy the *scientific* geologist to do so from *his* inspection.

Yet our workmen will even tell you the *extent* of such a bed—from their knowledge of the peculiar formation of the surface of the ground—before an atom of the clay has been seen or dug up from, say 8 or 10 feet below. The term *slad* expresses this, but it is as hard to describe as it would be for one to describe his own handwriting.

I have always suspected that dowzers have a somewhat similar knowledge as to water and lodes.

FRED. W. P. JAGO.

Here then we have a case where success can hardly be due to any conscious process of reasoning; the judgment seems to be more or less instinctive, though doubtless it has been gained by experience. Much the same thing occurs in the location of metallic lodes. From very early times, the signs of underground ore have been noticed by miners, references to these indications are to be found in all the leading works on mining published in the 16th and 17th centuries.<sup>1</sup> A distinguished authority on mining, the late Professor Warington Smyth, F.R.S., remarked in a lecture delivered in 1869 :—

There is some foundation for the idea that metallic lodes affect vegetation; the herbage is often brighter and better over certain lodes and worse over others. A certain class of plants will grow over the lode and not on adjacent parts. Veins containing decomposing iron pyrites are slightly warmer, so that there is rather a higher temperature over them; dew or hoar frost will not lie long on the lode, so that its presence can be traced early in the morning. A peculiar odour from the sulphurous gases pervades the direction of a lode, and sometimes at night a faintly luminous appearance is seen over certain metallic lodes.<sup>2</sup>

As we shall see in Book III., the dowser has gained remarkable success not only in finding underground water but mineral lodes, and hence the probability that he is guided by some surface indications is greatly increased from the fact that such indications are found in both these cases. It may, however, be urged that ordinary observation and judgment would be sufficient in each case to explain success without assuming any instinctive or subconscious process. But this view, as

<sup>1</sup> See especially G. Agricola's *De Re Metallica*, Basle, 1546.

<sup>2</sup> Similar statements will also be found in the 9th Edition of the *Encyc. Brit.*, under the article "Mining." See also Pryce's *Mineralogia Cornubiensis*, etc.

we have seen, is inconsistent with the phenomenon of motor automatism, and, moreover, common sense, aided by observation, knowledge and reasoning, would long ago have displaced the dowser had the latter not been found to be of greater practical value.

It must, therefore, be admitted that if surface indications determine the suggestion that starts the automatic motion of the rod, these indications are not deliberately sought for, but that *a subconscious and hyperæsthetic discernment of faint signs that escape the ordinary observer is quite possible on the part of the successful dowser.*

Many instances have been given in our *Proceedings* of definite motor reactions, sometimes of a remarkable character quite unaccompanied by consciousness,<sup>1</sup> and many illustrations have also been given of what at first sight appears an almost miraculous sense of perception, really due to a transitory hyperæsthetic and subconscious condition on the part of the subject.<sup>2</sup> It is, therefore, I think, unnecessary for me to contend further that the foregoing explanation is really a *vera causa*, but I now proceed to show that it is *not the whole cause* and may really play a very unimportant part in the matter.

## § 2. *Is the Suggestion Pure Chance or Derived from some Supernormal Source of Perception?*

Whilst the explanation just given may cover some of the facts, it fails to explain those cases where surface indications of underground water do not exist, owing either to the surface being made ground or covered with buildings, or with a layer of 30 or 40 feet of "drift," or the water itself existing in fissures or cavities so far beneath the surface that it is inconceivable its presence could be indicated by any superficial signs. Now, an attentive study of the cases I have cited in the present and the previous Report shows that the dowser's success is *not* limited to localities where surface indications may be conjectured. Take, for example, the classical case of Waterford, so fully described and discussed in the previous Report, pp. 106–117. The able geologists who have examined that case clearly show that surface signs could have nothing to say to the dowser's success. Take again in the

<sup>1</sup> Experiments in "normal motor automatism" form the subject of an interesting paper published in 1896 among the "Studies from the Psychological Laboratory of Harvard University."

<sup>2</sup> See Professor Guebard's experiences, *Proceedings S.P.R.*, Vol. II., p. 411, and many similar illustrations cited by Mr. Myers in his papers on the "Subliminal Self" published in our *Proceedings*. I wonder the divining rod has never been used for *weather prognostication*. If a code could be arranged so as to interpret its indications, I have no doubt it would become as famous in foretelling the weather, say twelve hours ahead, as in finding underground water or mineral ores; *supposing* that a subconscious discernment of faint signs really occurs with the dowser. Leeches used to be employed as weather prophets: see on this subject of animal presentiments my last Report, *Proceedings S.P.R.*, Vol. XIII., p. 250.

present Report the Errol case, p. 171, when the same dowser (the late John Mullins) was employed, or the Lytes-Cary case, p. 174, or the Carrigoona experiments, p. 144, or the Shanklin experiments, p. 164, or the case of Beraz, p. 248, or many of Bleton's cases, p. 264, or others I might quote. All these cases point to one of two conclusions—either they are the result of chance coincidence, lucky hits on the part of the dowser, or that the dowser possesses some faculty new to science.

Let us take the first alternative. What are the chances of success in, say the Waterford case? Here the geologists show that Mullins must have hit upon a line of fault in the Silurian rocks "80 ft. below the surface," or "a porous stratum concealed beneath the drift,"—which is here over 40 ft. in thickness (see the Report by Mr. J. R. Kilroe of the Geological Survey of Ireland, given in my former Report, *Proceedings S.P.R.*, Vol. XIII., p. 116). Now before the advent of Mullins three borings (in one case to the depth of 1,000 ft., in another to a depth of 392 ft.) had been made on scientific advice in places near the spot selected by Mullins; one of these borings had been made on the advice of Mr. Kinahan, F.G.S., of the Geological Survey, and yet in each case no water had been found. The chances of hitting a line of fault or porous stratum were, therefore, extremely small. I have talked over this with Mr. Kilroe, who has visited the spot and is one of the best geological authorities on that district, and he tells me that a "lucky fluke" on the part of the dowser, though conceivable, is almost incredible. This also was evidently the opinion of the local geologist, Mr. Budd, who was present at the borings, and of Mr. Kinahan himself. Moreover, Mullins predicted the exact depth of the fissure within a foot or two and was right in the volume of water that was found. These are contemporary statements made by those who were at first prejudiced against the dowser.

From the study of these and other cases I have therefore been driven to the conclusion that neither surface signs, nor hyperæsthetic discernment, nor chance coincidence, can account for all the successes of a good dowser like Bleton in the last century or the late J. Mullins and others in recent times. This is precisely the conclusion which Mr. Westlake, F.G.S., has independently arrived at from a careful geological examination on the spot of other cases selected from my previous Report, see Appendix A., p. 337. In his analysis, Mr. Westlake assumes the dowser knew all the local indications of the underground water, and he is disposed to read back into the dowser's mind the hydro-geological knowledge which was gained *after* the dowser's well had been sunk. Notwithstanding all this, Mr. Westlake concludes that a certain percentage of cases (10 to 15 per cent.) defy any explanation known to science except chance coincidence. Here then the issue is knit. Can it be shown that the outstanding cases are explicable as merely lucky



hits due to pure chance on the part of the dowser? I think the *onus probandi* of this must be left to those who challenge our methods of inquiry, or deny the existence of any avenue of knowledge other than the recognised channels of sense.

### § 3. *Is any Physical Cause at all Likely?*

In seeking for an explanation of these outstanding cases, I do not think we need consider any *physical cause*. Recent scientific discoveries, such as the X-rays and wireless telegraphy, have made the public familiar with the fact that bodies like earth and bricks and flesh, which are opaque to light, may be transparent to electric waves. But though the penetration of opaque bodies by invisible radiation has long been known to scientific men, the unscientific mind naturally seizes the idea that just as electric waves have been shown to pass unimpeded through walls and mountains, so some electric influence from underground water is likely to pass through the superincumbent earth and affect the dowser. But then we must assume an electric influence also proceeds from mineral ores and that the dowser is a delicate electroscope, and this is pure assumption. It is true our knowledge of the world around is very limited, but 200 years ago the philosopher Malebranche showed, from the uniformity of nature, how untenable was any explanation based upon an imaginary physical influence produced by underground water or metals upon the dowser.<sup>1</sup> Whatever effect is produced by *underground* water or ore would be produced in a greater degree by the proximity of water or ore on the surface. Nevertheless, the dowser appears quite insensible to the mass of water in a river, lake, or the ocean, or to masses of metal or ore within his sight.

There is, however, another physical hypothesis which is more plausible, and which at one time suggested itself to me. We are insensible to certain physical forces which are ever streaming through us, such as the magnetism of the earth; we are likewise insensible to the motion of the earth in space, but we should become alarmingly sensible of the latter if it were interrupted or suddenly changed. It is conceivable the dowser may be a person gifted with a sense affected by any slight interruption in the continuity or modification of the lines of terrestrial, magnetic, or electric force. Underground fissures may be imagined to produce such a modification or want of continuity in the ambient field of force, and thus their presence might be detected by certain organisations. But I do not attach the least value to this conception as an explanation of the success of the

<sup>1</sup> See "Réponse de l'auteur 'De la recherche de la Vérité'" to Father Le Brun, published in the *Lettres qui découvrent l'illusion des philosophes sur la baguette*, p. 9, et seq. Paris, 1693.

dowser; for, *inter alia*, there is no evidence to show that the dowser is more affected than ordinary persons by any of the physical forces; and the interruption of a magnetic field of force enormously greater than that due to the earth is absolutely unfelt by the dowser. No, the key to the mystery that remains must, in my opinion, be sought in the *psychical* and not in the physical world.

#### § 4. *Probable Explanation of the More Remarkable Successes of the Dowser.*

There is abundant evidence in our *Proceedings* that the information subconsciously given by some automatists often transcends the ordinary sense perceptions. Cases of so-called clairvoyance resting upon excellent testimony are known, see *e.g.*, the cases cited in Mrs. Sidgwick's paper on the "Evidence for Clairvoyance," or Dr. Alfred Backman's "Experiments on Clairvoyance," and the supplementary papers to both of these contained in Vol. IX. of the *Proceedings* S.P.R., also the evidence cited by Mr. Myers in Vol. XI., pp. 367-404. It is true the science of to-day does not recognise any such super-sensory extension of knowledge, yet nothing is clearer to those who have made a careful and critical study of psychical phenomena, not only that telepathy is a fact, but that to the subconscious or subliminal self, the elements of time and space have not the same limitations as they have to our conscious self. Even Schopenhauer, in his *Versucht über Geistersehen*, long ago said, "He who doubts the fact of clairvoyance is not sceptical but ignorant." This is, perhaps, a stronger assertion than I should be disposed to make, but of the reality of this "far seeing" power, I, among others, have had indubitable proof after carefully conducted experiments with persons in the deep hypnotic state, and the same condition is often seen in the allied state of somnambulism. Those who on *a priori* grounds deny the possibility of any such transcendental perceptive power should read the conclusions unanimously arrived at by the nine distinguished members of the French Royal Academy of Medicine, who were appointed by the Academy to report on mesmeric phenomena. After five years' investigation this Committee presented their lengthy Report to the Academy in June, 1831. They state they began the inquiry with "inexperience, impatience and distrust," which at first militated against them. Ultimately, after the most rigorous tests, they "conclude with certainty" that the faculty which has been designated clairvoyance does really exist in certain subjects in the mesmeric state.<sup>1</sup>

<sup>1</sup> See Colquhoun's translation of this Report published in 1833. In an Appendix the Editor gives several remarkable cases of clairvoyance, attested by some eminent Continental physicians as occurring in certain cataleptic patients, who—like the dowser—*appeared* to have the seat of this perceptive faculty in the epigastrium.

The question arises, Is this condition ever found in persons who, like the dowser, are in their normal waking state, not in a hypnotic or somnambule trance? I have tried to put this question to the test of experiment with an amateur dowser, and the result of the preliminary experiments (which are all that have so far been made), affords considerable support to the hypothesis of some kind of clairvoyant faculty in the particular dowser tested. I have given an account of these experiments, together with some additional evidence bearing on the same subject, in Appendix E., p. 359.<sup>1</sup>

Professor Janet, in a passage I have quoted on p. 297, points out that evidence of a secondary personality is afforded in cases of motor automatism such as the dowser exhibits. Now, we know that in automatic writing the secondary self frequently exhibits transcendental sources of information; the Ego seems to pass beyond its usual narrow threshold of sensibility, the veil is lifted which hides scenes spatially distant.<sup>2</sup> Mr. Myers has cited cases of what he terms *cosmopathic impressibility* on the part of the automatist; that is, transcendental impressions which come to men from the surrounding world, impressions "borne like seed on the wind, rather than wittingly directed towards him by the voluntary or involuntary sympathy of any assignable intelligence."<sup>3</sup> The sensibility of the dowser may be of such a kind, but humbler, *geopathic* and not *cosmopathic*. I have no doubt, however, that in years to come we shall see in all these phenomena the manifestation of the transcendental Subject which lies in the background of our being, and remains unrevealed to our self-consciousness. Thus we have been led from the study of such a matter-of-fact and practical question as the discovery of underground water by the dowser to the very centre and mystery of our complex personality.

Here, however, we do well to remember that there is but a step from the sublime to the ridiculous. For the emergence of the subconscious self and its expression in some form of motor automatism involves the more or less complete surrender of our reason, of self-consciousness and the voluntary control over our actions; the reins of will are, as it were, thrown down and the automatist becomes the

<sup>1</sup> A belief in the existence of clairvoyance among certain persons in the normal state is, I find, the foundation of the repute in which the so-called Zahoris were once held. As the Zahoris are sometimes referred to in modern literature, and no one was able to give me any information about them, after some search I was able to obtain the historical information given in Appendix F., p. 367.

<sup>2</sup> Upon this subject see Baron Du Prel's *Philosophy of Mysticism*, translated by Mr. C. C. Massey.

<sup>3</sup> *Proceedings S.P.R.*, Vol. XI., p. 367.



sport of every wayward impulse or passing suggestion.<sup>1</sup> Hence, the worthlessness of many of the indications and messages given through different forms of autoscope, whether forked rod, tilting table or scribbling pencil.

We may, therefore, expect any auto-suggestion to make itself apparent in the motion of the divining rod, and this is the case ; when, however, the dowser "sets himself," as he expresses it, then the indications afforded by the movement of his rod are directed towards the object of his search, and the suggestion may now arise : (1) from various hints he has gathered or knowledge he possesses becoming unconsciously operative ; or (2) from his subconscious and perhaps hyperæsthetic discernment of the surface signs of underground water or ore ; or (3) from some kind of transcendental discernment possessed by his subconscious self. For my own part, I am disposed to think that this last cause, though less acceptable to science, will ultimately be found to be the truer explanation of the more striking successes of a good dowser.<sup>2</sup> My reasons for thinking so are : (i) the success which certain dowsers have achieved in discovering underground water, when surface indications were out of the question, and when previous borings, as deep or deeper, in the immediate vicinity failed to discover water ; (ii) the success which has attended young or ignorant dowsers, such as Bleton, who cannot be supposed to have an intuitive knowledge of the surface indications of underground water ; and (iii) the evidence afforded by carefully blindfolding the dowser.

The only alternative explanation in these cases appears to me to be that of *chance coincidence*, and to the majority of thoughtful persons this will doubtless seem the more probable alternative ; but the probability of its being so will be recognised as extremely small by those who will take the trouble to apply, so far as can be done, the doctrine of chances to the facts I have cited. On the other hand, the probability that an explanation is to be found in some extension of our knowledge of human personality, something new to science, and something akin to what has been termed clairvoyance, gains considerable weight from a critical study of cognate phenomena described in our *Proceedings* and elsewhere. And surely, even if the majority of

<sup>1</sup> It is obvious that such persons cannot be as fully responsible for their actions as the rest of mankind. Readily influenced by their environment and controlled by suggestion, such "suggestible persons" (among whom are all so-called "mediums") when met with suspicion and the suggestion of fraud, frequently exhibit the very characteristics their critics have imagined them to possess. The investigators, in fact, have unconsciously suggested the rôle that the subjects have played.

<sup>2</sup> If this be the case, the dowser ought to be able to indicate the position of *any* buried object, as well as underground water and mineral lodes, albeit surface indications in these latter may count for something. I have dealt with this wider question in Appendix E., p. 359.

cases of the success of the dowser are explicable by known causes, yet, as Kant said of the attested story of Swedenborg's clairvoyance, "the amazing inferences that would have to be drawn if only *one* such event could be proved" will, I hope, stimulate further inquiry, so that the conclusion I have reached may be confirmed or disproved.

I do not for one moment profess that the explanation I have suggested is anything more than a provisional and tentative one. A final solution is not likely to be reached until the subject has been critically examined and discussed by scientific men from different points of view. But if many years' experimental investigation gives me any right to express an opinion, I am convinced that the only method by which we can hope to bring these elusive and perplexing phenomena out of the "disorderly mystery of ignorance," in which they have lain neglected for so long, into the "orderly mystery of science," is by a careful collection of trustworthy evidence, supplemented by experimental investigation wherever it is possible, and then by the fearless suggestion of some working hypothesis which will enable future investigators to direct their inquiries to definite issue, and thus overthrow or establish the provisional explanation that has been suggested. This is obviously the method by which the whole edifice of modern science has been reared; it is nothing more than the application of the inductive philosophy to psychical research. And we may confidently expect much more rapid progress in this difficult region of inquiry when it becomes evident that there is not only something to investigate, but that some solid addition to our knowledge or some useful practical result will issue from the inquiry. A larger number of trained investigators will then be attracted, and though psychical research can never offer the commercial stimulus to which the recent development of electrical science is almost wholly due, yet it presents to us many problems of profound interest.

For thirty years those of us who founded this Society have striven to ascertain whether the information derived through the recognised channels of sense embraced the whole of the accessible sources of knowledge; whether also the normal phenomena of Nature,—manifested to us as the accidents of *time*, *space*, and *mass*, in terms of which all natural phenomena may be expressed,—were really the bed-rock upon which the *whole* superstructure of human science must be raised; or whether the order of Nature can be proved to contain an even vaster procession of phenomena than is now embraced within the recognised limits of science: in fine, whether we could ever reach by scientific methods the profounder, more enduring, and transcendent realities that lie beneath the material surface, as thought lies behind its utterance in language.

There are some avenues of approach to this great quest which seem barred to many earnest searchers after truth; the limitation of our faculties may possibly render certain pathways to knowledge inaccessible under the conditions of our present existence. But we cannot, *a priori*, condemn any inquiry unless it has been conclusively proved to be phantasmal and worthless, issuing in a dreary and fatal waste of human effort, or beset with intellectual and moral confusion to those who engage in it. Such warnings can hardly be said to apply to the present investigation. And if the outcome of our inquiry only establishes the fact that what appeared supernormal and transcendental is, after all, explicable by an extension of known perceptive powers, even then our time will not have been wasted, for we shall have enlarged our knowledge of human faculty and human usefulness. For my own part, I have been driven to believe that some dowsters—

“ Whose exterior semblance doth belie  
The Soul’s immensity ”

nevertheless give us a glimpse of—

“ The eternal deep  
Haunted for ever by the eternal mind.”

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#### CONCLUSION.

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(1) For some centuries past certain individuals locally known as dowsters have declared that they can discover the presence of underground water, mineral lodes, coal,<sup>1</sup> building stone,<sup>2</sup> or other buried objects which may be sought for by the apparently spontaneous motion of the so-called divining rod; when their pretensions have been tested, the result, though by no means uniformly in their favour, has been so remarkable that chance coincidence appears a wholly inadequate explanation.

(2) Any explanation based upon trickery or unconscious hints from bystanders, or the detection of faint surface indications of the concealed object, or other known cause is insufficient to cover *all* the facts.

(3) The *movement* of the rod or forked twig is only a special case of motor automatism exhibited by a large number of individuals,<sup>3</sup>

<sup>1</sup> Both Pennet and Bleton were tested for this a century ago, and several competent witnesses were convinced of the fact.

<sup>2</sup> See, for example, the full and excellent narrative of a Belgian dowster, given by Professor D’Outrepoint, translated by Mr. Bennett, Assistant-Secretary of the S.P.R., and published in the *Journal* of the S.P.R. for June, 1899.

<sup>3</sup> A century ago Professor Sementini estimated that 80 per cent. of Italians could “work” the rod, but Amoretti thinks this number excessive; he gives 20 per cent. My own impression is that Amoretti is not far wrong. In England, perhaps, one or two in every dozen persons would be found susceptible.



and arises from a subconscious and involuntary "suggestion" impressed on the mind of the dowser.

(4) Accompanying the involuntary and usually unconscious muscular contraction which causes the motion of the forked twig or rod, many dowsers experience a peculiar *malaise* and some a violent convulsive spasm. This is a psycho-physiological effect, akin to emotion. Moreover, the state of mono-ideism of the dowser creates a condition of partial catalepsy when some suggestion causes the idea to culminate.

(5) This subconscious suggestion may arise from a variety of causes; sometimes it is merely an auto-suggestion, at others it is unconsciously derived through the senses from the environment, but in a certain number of those who exhibit motor automatism the suggestion appears to be due to some kind of transcendental perceptive power.

(6) Such persons appear only able to exercise this transcendental faculty when their normal self-consciousness is more or less in abeyance, or when it is completely submerged, as in profound hypnosis.

(7) This subconscious perceptive power, commonly called "clairvoyance," may provisionally be taken as the explanation of those successes of the dowser which are inexplicable on any grounds at present known to science.

I wish to add a word of hearty thanks to the Editor of our *Proceedings*, Miss Johnson, for the kind assistance she has rendered in the reading of the proofs of this paper and for many useful suggestions.

## APPENDIX A.

REPORT ON SOME CASES OF WATER-FINDING BY THE  
DIVINING ROD.

BY E. WESTLAKE, F.G.S.

During the printing of Professor Barrett's paper published in July, 1897, I made an analysis of his cases, and selected forty in which it was stated that, prior to the dowser's success, a deeper but unsuccessful well or boring had been made in the immediate neighbourhood. These are cited, at the foot of page 252 of his paper, as offering presumptive evidence of something in dowsing beyond chance or experience. As it seemed desirable they should be looked into from a geological point of view, I have since undertaken, at Professor Barrett's request, an examination of twenty-one of them, *i.e.*, of seventeen published in his paper (Nos. 9, 17, 24, 26, 27, 28*b*, 42, 43, 47, 48, 57, 60, 77, 90, 99, 105, and 121),<sup>1</sup> and of four others subsequently received. Beyond aiding in their understanding, I soon found that geology had little to do with the matter; the dowser does not profess, and probably seldom has, a knowledge of it; his conclusions seem usually drawn from more obvious considerations.

In psychological experiments, essentially modifiable by suggestion, it is absolutely necessary to have the conditions, if not under control, at any rate precisely recorded; if they are not, it is impossible to arrive at certain results. An observer coming after the event must allow that the dowser *may* have seen, or heard of, the position of neighbouring water, and have drawn his conclusions accordingly.<sup>2</sup> This I have done throughout the paper, with the result, it may be, of minimising marvels that really exist: if so, it will only need experiments more precisely noted to lead to their establishment.

Of the twenty-one cases I examined, five were in the South and sixteen in the West of England; seventeen similar cases in the North and East I was unable to visit. Those in the Isle of Wight I saw in November, 1897; those in Surrey and Sussex in June, 1898, and those in the West of England in May, 1899. I have arranged them, according to evidential considerations, in the following groups,

<sup>1</sup> These numbers refer to the numbering of the cases given in *Proceedings*, Vol. XIII., Part XXXII.

<sup>2</sup> As a matter of fact, dowsers do *not*, as a rule, seek for information about the local water supply; they believe so firmly in their "gift," that they trust implicitly to the twisting of the hazel twig to guide them. Merryweather is the only dowser of whom I have heard that looks for surface indications of underground water.—W.F.B.

beginning with what appears the weakest evidence on behalf of the dowser.

(A) <i>Too old for evidence to be now obtainable.</i>	Nos. 24, Sodbury ; 26, Shepton Mallet gaol ; 48, Bristol ...	Total 3
(B) <i>Where the old well was too deep.</i> <sup>1</sup>	Yarlington House ,,	1
(C) <i>Where the old well was in the side of a hill.</i> (See Group (D), No. 28) ... ..	,,	—
(D) <i>Where the dowser's well was the deeper.</i>	Nos. 9, Evercreech ; 17, Sturminster ; 27, Shepton Mallet Brewery ; 28, Locking ; Ladyswood Park ... ..	,, 5
(E) <i>Where a heading was driven towards known water.</i>	Nos. 42, Henbury ; 60, Shepton Mallet Station ...	,, 2
(F) <i>Where the dowser's well was placed near known water, the old well being usually remote.</i>	Nos. 43, Hedgecock's ; 47, Park Farm ; 57, Norton Hall ; 77, Arnold's ; 90, Wootton ; [Arreton] ; 121, Stroud ; 99, Melksham ... ..	,, 7
(G) <i>Where there was little or no surface sign of water.</i>	Lytes Cary ; Wimblehurst Farm ; 105, Toy Farm ...	,, 3
		— 21

#### GROUP (A)—TOO OLD.

Owing to remoteness of date, I have been unable to obtain sufficient information about these cases to include them in a critical examination, but append the following notes:—

No. 24. *Sodbury*.—The Rev. R. S. Nash, of Old Sodbury Vicarage, writes that in 1853, the date of the publication of Phippen's book, the Rev. Mr. Foster was rector of Dodington, about one mile S.W. of Sodbury. The Rev. J. A. James, the present rector, informs me that there are three wells on the premises, the deepest of which is about 60 feet. The water in it stands two or three feet from the surface, and has never been lowered to 20 feet even in two exceptionally dry summers, so that nothing is now known about the alleged tunnel. The strata are Lower Lias, as in the celebrated case at Shepton Mallet Station (Group (E), No. 60), to which this is, therefore, closely parallel.

No. 26. *Shepton Mallet Gaol*.—I am informed by a warder, Mr. J. G. Barnes, that all the wells at the prison are filled up ; but the strata being Lower Lias, and only a quarter of a mile west of the well in No. 60, the geological conditions are doubtless similar.

<sup>1</sup> The term "old well" designates the deeper, but unsuccessful, well sunk prior to the dowser's.



No. 48. *Well at Mr. Samuel Lang's Kennels, Bristol.*—Inquiries in Bristol elicited that Mr. Lang had not been heard of for some years. Professor Lloyd Morgan, F.R.S., tells me that the kennels were at the further end of Cold Harbour Lane, Redland, on Lower Lias or Rhaetic, so that we have here another case of a tunnelled well in this formation. Seven out of the eighteen cases which follow were also in Lower Lias, which thus appears to present conditions, *i.e.*, as to difficulty of finding water, which are adapted to the dowser's art.

GROUP (B)—OLD WELL TOO DEEP.

*Yarlington House* [Additional Case]. In a letter to Mr. Andrew Lang, Mr. T. E. Rogers, M.A., Deputy Chairman of the Somerset Quarter Sessions, Chancellor of the Diocese of Bath and Wells, etc., writes :—

Yarlington House, Wincanton, *December 4th, 1897.*

Over fifty years ago a very respectable farmer at Ditchat, Kingston by name, a tenant of Squire Daves, was well known in this part of the country for his success in the discovery of underground springs.

In my own case he was attended with extraordinary success. My grandfather, when building this house in 1782, sank 135 ft. for a well, and only then came to a very slight supply ; so trifling, that after having for some few years drawn up water with the aid of a donkey, he closed the well altogether, and used to send nearly half a mile for spring water. Upon my sending for this man Kingston, in 1846, he found a spring not 20 yards from the old well, which has quite sufficiently supplied the house, and at a depth of about 32 ft.

I should mention that Kingston used not a thorn but the main-spring of a watch. He had no idea how the action was produced, and was not conscious of any sensation passing through him. He assured me that the influence acted with equal force on the main-spring in his hand whether the spring [of water] was 10 or 50 yards deep. I have walked with him holding one end of the main-spring, which in my hand was perfectly stiff and immobile, while the end in his hand was twisting and turning so violently that, unless he let it go, it must inevitably have snapped ; and on some occasions it did absolutely snap.

T. E. ROGERS.

On visiting Yarlington, Mr. Rogers showed me the sites of the two wells, which are about 40 yds. apart. He informed me that the depth of the old well was 130 ft. or a little more, and that the water in the new well was found at a depth of 35 ft. in oolite.

At the entrance lodge, 300 yds. E.N.E. of the new well, and on the same level, is a well 40 ft. deep, containing 5 ft. of water : the lodgekeeper said it was a good supply, sinking only 3 ft. in the driest summers.

Mr. Rogers said that below the loose oolite (Inferior Oolite) which forms the top of the hill, is a little layer of marl, and below this a

sandy loam (Midford Sands). The oolite and sands are seen in the sides of the road going down to Yarlington village, about 130 feet below the house.

From the water in the wells at the house and the lodge occurring at the same level in the same horizontal bed of limestone, I infer that it is held up by the marl and forms a continuous stratum, which in the old well was lost by penetrating into the permeable sands below.

Since writing the above, I see that sheet 18 of the Geological Survey, which was published July 5th, 1850, four years after the new well was sunk, shows a line of fault passing N. and S. exactly over it; but, as there is no reason to suppose that geological maps are drawn with this degree of accuracy, it is more likely, if a fault has anything to do with the matter, that the *old* well was on it, since its tendency would be to let out the water at the bottom—in which case the dowser may claim whatever credit there is for not putting his well on the same line. Mr. Rogers has revised the foregoing.

#### GROUP (C)—OLD WELL ON THE SIDE OF A HILL.

A well so situated is no evidence for any unusual faculty in a dowser who finds a better supply in the interior of the hill at a slightly higher level, especially where the escarpment is steep and the strata rather impermeable, as in the case of two wells (Nos. 2 and 3) on either side of Locking churchyard (see Group (D), No. 28, par 2; p. 321).

#### GROUP (D)—DOWSER'S WELL THE DEEPER.

No. 9. *Evercreech Junction*.—[An unsuccessful well was sunk to nearly the same depth as a second, which was located with the twig 6 yds. from it, and yielded 36,000 gallons a day. Date about 1892. *Proceedings S.P.R.*, Vol. XIII., p. 33.]

The well at Messrs. Roles' milk factory is situated a few yards to the south of the railway station. The strata are the upper part of the Lower Lias. The proprietor called an old man answering to the name of Tommy, who had worked in the unsuccessful well. He said it was from 50 to 55 ft. deep in hard blue marl, and that about 40 to 50 ft. down they had tunnelled in three directions—in one direction for about 17 ft. This well is now filled up.

The new well is 12 to 15 ft. distant from the old one. The proprietor said its depth was from 65 to 67 ft., and that it contained over 40 ft. of water. The top soil consisted of 10 to 12 ft. of yellow clay, blue at the bottom, and below this 55 ft. of very hard blue marl which had to be blasted, but which fell to pieces on exposure to the weather. At the bottom was a thin seam of "scale" or hard rock about half an inch thick, and on breaking through this the water rose quickly. About 3,000 gallons a day are used for the milk factory.

At Owley Farm, about 180 yds. S.W. of the wells and about 18 ft. lower, is an old well 49 ft. deep, containing  $44\frac{1}{2}$  ft. of water. The bottom of this well is thus on a level with that of Roles' new well, and the water in it also stands apparently at the same level. From this we may infer that the strata are approximately horizontal, and that the same stratum of artesian water has been tapped in each case. I therefore quite agree with "Tommy," who said, referring to the unsuccessful well, "I believe if we had gone down deeper we should have found the spring." Mr. H. J. Roles, to whom I submitted the foregoing, writes:—"Your report *re* the well at this place appears to me to be correct."

The local opinion of the difficulty of finding potable water is shown by the circumstance that the supply for the Junction is brought by rail.

No. 17. *Sturminster Newton*.—[A well was sunk 50 ft. without a drop of water. Mr. J. F. Young and three of his family pointed out a spot a few yards away where a well was sunk, which at a less depth delivers a plentiful supply. *Proceedings S.P.R.*, Vol. XIII., p. 40.]

The wells referred to by Mr. Young are situated a few hundred feet from an escarpment of Corallian rock (Coral Rag on Calcareous Grit) about 50 ft. high, facing west and overlooking the river Stour. The "village doctor," Dr. J. Comyns Leach, The Lindens, Sturminster, informed me that the unsuccessful well, referred to by Mr. J. F. Young, was sunk by him (the doctor) at the house about 60 yds. to the N.E. of his own, now occupied by Dr. R. A. Beaver. It was first sunk to a depth of about 35 ft., when it held 16 ft. of water, but on sinking it 3 ft. lower the water was lost. In June, 1893, the well sinker lost his life in the well from bad air, in consequence of which it was abandoned, after going to a total depth of 41 feet.

The second or successful well is 30 yds. from the first, and was placed at a convenient spot in the stable yard without reference to any predictions by Mr. Young or his family, of which the doctor was unaware. It is 45 ft. deep and holds 6 or 7 ft. of water.

Eighty yards west of the second well, there is at the doctor's own house a third well (marked P. on the Ordnance map), which is only 38 ft. deep and holds from 7 to 14 ft. of water, according to the season. Hence, as these three wells are on level ground, and in a tolerably pervious rock, there is nothing remarkable in having found water in the stable well below the bottom of the unsuccessful one. Dr. Leach, who has revised this account, never had to pay for carting water; and this is confirmed by Mr. J. F. Young's father, Mr. Robert Young (the author of works in the Dorset dialect), to whom, as also to Mr. J. F. Young, I have submitted the foregoing. The latter writes that his report to Professor Barrett was first sent for local correction, but



that probably part of the information sent him might have been based upon hearsay.

No. 27. *Shepton Mallet Brewery*.—[A boring was carried to some 140 feet without success. Then the water-finder, Sims, indicated a spot on another part of the premises, where a well was sunk and a magnificent spring was discovered in a fault of the rock at a depth of 40 feet. *Proceedings S.P.R.*, Vol. XIII., p. 49.]

The diviner in this case was Mr. Charles Sims, a professional dowser, now living at Pilton, a village rather more than two miles from Shepton Mallet. The Anglo-Bavarian Brewery is situated on a submerged island of Carboniferous Limestone, and the unsuccessful well was sunk in the front yard, through Lower Lias and Limestone, to a depth, according to a foreman who had been present, of about 100 or 120 ft., the lower half of this being a 6 inch boring.

Mr. Sims' well is in the hamlet of Bowlish, 500 yds. distant from the unsuccessful well and 95 ft. below it. The land is at the extreme point of the Company's premises, and was acquired consequent on the successful sinking. The well is 22 ft. in diameter and 40 ft. deep. It is in the bottom of a narrow valley, and 65 ft. south of a small stream, the river Sheppey, which runs west from Shepton Mallet. Just above the well a quarry is being worked in littoral Lower Lias limestones without the usual clayey partings; on the other side of the valley, 200 yds. to the north, is a protrusion of Carboniferous limestone; but the well itself, according to Mr. Phillis, a geologist formerly at the brewery, is almost certainly in Lias.

He said they got no water at first even at 40 ft.; but he went down the well, and, seeing a place that looked damp, told them he thought they would get it by driving there, which they did, and it was found to drain the wells in Coombe Lane to the south. The water came in from both sides of the valley, but he thought it too far from the stream to filter through, though possible. The supply was insufficient for the brewery, and is now discontinued in favour of that from an open spring at Darshill, a mile lower down the valley.

The case is not evidential on account of the wells being so remote the successful one being also the lower of the two, and placed at the most likely spot for water. This was also the opinion of the people at the brewery and of Mr. Phillis, who have both revised the above.

No. 28. *Locking*.—[Two wells, Nos. 3 and 4, sunk without a dowser, 33 ft. and 12 ft. deep respectively, are dry during summer. No. 1, a dowsed well, 150 yds. distant, is 20 ft. deep and has a constant supply. No. 2, a dowsed well on the Vicarage lawn 120 yds. distant, is 31 ft. deep with a plentiful and constant supply. *Proceedings S.P.R.*, Vol. XIII., p. 50.]

On visiting Locking, in company with our informant, Mr. W. G. Hellier, I found (referring to the sketch-map given on page 51 of Professor Barrett's paper) that just north of well No. 4 an east and west fault throws down horizontal Lias on the north against Red Marls on the south. The marl has been worked out by a stream, and the dowsed well (No. 1) is in the bottom of the valley not far from the same; whereas the non-dowsed wells (Nos. 3 and 4) are near the top and bottom respectively of the steep scarp of Lias, and are not less than 30 and 10 ft. respectively higher than No. 1. Hence, as No. 1 reaches a level 20 ft. or more deeper than either of them, the occurrence of a constant supply of water in it is no evidence for dowsing.

The church and vicarage on the top of the hill are on practically level ground, which, at the other dowsed well (No. 2), may be a few (say 5) feet above No. 3: No. 2 is also stated to be 2 ft. shallower, and to contain plenty of water. On the other hand, No. 3 is sunk on the steep side of the hill, which cannot be expected to hold water so well as at No. 2, some 50 yds. back from the edge.

Locking is the only place where I did not take measurements; going round with Mr. Hellier I could not at the time, and it did not seem worth while to return for the purpose. At the well on Sandford Green, which we next visited, I found the measurements had been correctly given by Mr. Hellier, so presume those he gives for Locking are correct. The well is in Red Marl of the Trias, and contained  $19\frac{1}{2}$  ft. of water. The site of the old well, now filled up, was about 20 ft. distant, but, as no record of its depth has been kept, no conclusion can be drawn.

*Ladyswood Park* [Additional Case].—The following paragraph appears in the *Bristol Times and Mirror* of March 7th, 1891, p. 16; and, with the words in brackets, in the *Medium* of March 13th, p. 167:—

Ever since the erection of the new mansion at Ladyswood Park, Sherston Magna [Wiltshire], the question of its water supply has been one of trouble and serious consideration. A few years ago, Sir T. Dancer, Bart., had two wells sunk, but without success in finding water. The estate having recently been purchased by Mr. Francis Davis, he determined to requisition the services of Daniel Lacy [a local diviner]. Lacy went to work with his little twig, and discovered a spring about 15 yds. from the two existing useless wells. He at once set his men to work, and after sinking to a depth of 60 ft. found an abundant spring, the output being practically inexhaustible.

Ladyswood Park is the "Ladyswood Farm" of the old one-inch map; it is about a mile S.E. of Sherston, on a plain of Forest Marble. On visiting Ladyswood in May, 1899, Mr. Davis showed me

the position of the three wells, and said he had just measured the depth of Lacy's, which was 74 ft., and that the water stood 53 ft. from the surface, higher than he had ever known it before. I then saw Sidney Lacy, of Sherston, who said he had helped his father sink the well, which was in thin bedded rock much the same all the way down. They struck the water at the bottom after putting in a blast. The new well is certainly lower than the old one, but they would not have got water by sinking the old one deeper, because his father, after trying the stick, said they would not, and he had never known his father wrong.

With regard to the old wells, Sidney Lacy said he went down the second (B) to remove the pump, and should think the depth was from 60 to 70 ft., but couldn't say exactly. The first (A) was also about 60 ft.: it was Lawrence, of Bristol, who dowsed it and sunk it. Mr. Davis said the B well was sunk by Mr. Simon Evans, of Sherston. Evans said that, as near as he remembered, its depth was 44 or 54 ft., with some water, and that on boring another 25 ft. with a three-inch bore they got plenty: the A well was, he said, 25 ft. deep, with no water.

As water in thin-bedded, horizontal limestones would probably lie in a uniform stratum, the failure of the old wells is sufficiently explained by, according to Lacy's figures, their not having been sunk deep enough, or, according to Evans', not deep enough to hold the water met with in his boring.

#### GROUP (E)—HEADING DRIVEN TOWARDS KNOWN WATER.

No 42. *Henbury*.—[In this case Mr. Crisp, the architect, writing in 1883, says that when water was required at Mr. Butterworth's new house, a geologist predicted plenty at a depth of 150 ft., where he said the Mountain Limestone rested on a bed of clay. They sank 150 ft. and bored 10 ft., but found no water. Then the diviner, Lawrence, predicted water about 20 ft. from the well, and advised driving a heading about 100 ft. from the surface. They drove 30 ft., and got a good and regular supply. *Proceedings S.P.R.*, Vols. II., p. 104, and XIII., p. 67.]

Rockwell, which is the name of Mr. Butterworth's house, now occupied by Mr. Frank Jolly, is situated on the north side of the road, one mile S.W. of Henbury and five from Bristol. The well is in Mountain Limestone, and about 50 yds. from its escarpment, which runs parallel to the Severn and shows the rock much faulted and contorted, as it was in the well. This escarpment is a Triassic cliff flanked by a beach deposit of Dolomitic Conglomerate. On the lower slopes are Red Marls which help to hold up the water in the limestone; the ground at the foot of the hill at Lawrence Weston being 120 ft. or more below the ground at the well.



The principal point omitted in previous reports of this case is the existence of an old well 123 yds. to the S.S.W. of Rockwell well and 123 ft. deep, with water standing 94 ft. from the surface. This old well belongs to a disused Friends' Meeting House: the place is now called Fern Hill and is occupied by Mr. H. Fedden. At Rockwell well the ground is about 19 ft. below the old well, and the water stands 81 ft. from the surface, or about 6 ft. below the water in the old well, which, allowing for the distance, shows both wells to be probably in the same stratum of water. And, as Rockwell well is 150 ft. deep, the bottom of it is 75 ft. below the level of the water in the old well. The heading at Rockwell runs from the well under the front of the house, which lies to the S.S.W. The length of the heading, Mr. Jolly told me, is about 75 ft.; but Mr. Crisp, the architect, in his report says it is about 30 ft.; and Mr. Mereweather, the contractor, gives it as 24 ft.; the important point is the direction, directly towards the old well.

Mr. Mereweather informed me that the Rockwell well was sunk without getting water beyond a soakage from the top layers after rain; he noticed, however, a damp place on one side about 100 ft. from the top; Mrs. Bengough then tried the ground with a divining rod, which turned at a point 16 ft. from the well. Mrs. Bengough in her own account (*Proceedings S.P.R.*, Vol. II., p. 104) says the distance was about 6 ft., and that Lawrence came later the same afternoon, and in the presence of herself and of many persons who had witnessed her trials, pointed to the same place. [Crisp says the point Lawrence indicated was about 20 feet from the well.]

Mr. Mereweather told me that neither Mrs. Bengough nor Lawrence went down the well, and that he was not present when Lawrence came. But as we cannot now be sure that Mereweather's information about the damp place had not reached one or both of them indirectly, or that they had not heard of the old well—which indeed the old house itself was sufficient to suggest—it remains doubtful whether Lawrence's indication shows more than knowledge normally acquired. At any rate he proposed the obvious course of driving a heading in the direction of the old well, and 25 ft. below the level of the water in it.

No. 60. *Shepton Mallet Station*.—[The contractors write in 1882 that in 1874 they sank a well 103 (or 99) ft. through blue lias rock with no traces of water. Then John Mullins with a twig predicted an abundance at 15 (or 50) ft. from the well, and advised a heading, on driving which the water rushed in and the miners rushed out, and their tools and watches are there till this day. *Proceedings S.P.R.*, Vols. II., p. 107, and XIII., p. 86].

The well in question supplies locomotives at Charlton Station, of the Somerset and Dorset Railway, on the eastern side of Shepton

Mallet; it is placed between the rails and the building marked "Tank" on the 25-inch map. My principal informant here was Wm. Baiss, an old man at the station, who said that, though not present when Mullins found the water, he was working on the railway nearby and heard all about it. He said they sank through several sorts of stone to a depth of 90 ft., without water. (Mr. Hickes, in the *Daily Graphic*, also gives the depth as 90 ft.; Mr. Whitaker, the locomotive superintendent at Highbridge, writes that it is now about 80 ft.) Then about 8 or 9 ft. from the bottom they drove a heading in an easterly direction through some blue marl, and struck a spring at from 15 to 20 ft. from the well. No red marl was met with, and he thinks the well is probably all in Lias. Mr. John Phillis, a local geologist (who has revised this account), agrees in this, and does not think they got down on the limestone (Carboniferous), the nearest outcrop of which is a mile distant. The engine-attendant stated that they pumped some 20,000 gallons a day, which has never failed even in the driest summers. Mr. Whitaker says that, when no pumping is done, the water stands below rail level about 37 ft. in winter and about 51 ft. in summer.

At a point 280 yds. north-east of the well, at the bottom of a valley in which runs a little stream—the river Sheppey—rises an open spring. It supplies Charlton brewery, and may have been long known, as the Roman Fosseway passes within a few feet of it. Its escaping level is 43 ft. below the ground at the station well, and hence within a foot of the average level of the water in it.

Close by the station well, and on the same side of the rails, a quarry shows 15 ft. of Lower Lias, dense, well-jointed blue and white limestones in beds of about one foot thick, alternating with from one to three inches of hard blue sandy clay. Notwithstanding the generally impermeable appearance of these beds, we must take it as possible, not to say probable, that, with a head of 50 ft. of water at a point variously given as from 15 to 50 ft. distant from the well, there would be at least a dampness on that side. In the account written by the contractors in 1882 (*Proceedings S.P.R.*, Vol. II., p. 107), we read that "on his arrival he [Mullins] went down the well," so that his location of the water "after he came up" cannot now be taken as showing more than his conscious or unconscious observation, or inference from the position of the brewery spring, which he was probably told of, as I was.

Since writing this I have seen the quarry a few hours after heavy rain and found it hold the water, except in the N.E. corner, where a main joint, running exactly in the direction of the brewery spring, forms a swallow-hole. It was doubtless such a joint as this that Mullins' heading cut into.

GROUP (F)—DOWSER'S WELL NEAR KNOWN WATER; OLD WELL  
USUALLY REMOTE.

No. 43 (the same as No. 59) *Hedgecocks*.—[The architect, who in 1878 (or 1879) was erecting a house for Mr. J. T. Renton, at Hedgecock's Farm, writes in 1883 that the contractors "proceeded to form a well close to the new building, and had bored a very considerable distance without coming to water." Then Lawrence predicted water a few yards from the boring, and they sank about 40 ft. (some 42 ft.) exactly on a good spring B, which, he believes, continues to supply the house. There was an old well A some 100 yds. distant with little or no water. *Proceedings S.P.R.*, Vols. II., pp. 103-104; and XIII., p. 68.]

In 1897, W. Renton, son of J. T. R., writes that the two wells, about 27 ft. deep, were only land springs and apt to get dry. So he had John Mullins divine a place, C, within six inches of which he found a good supply at about 26 ft. He also employed Mullins on three other successful wells. *Proceedings S.P.R.*, Vol. XIII., p. 86.]

Hedgecock's Farm is on the border of Sussex about four miles N.W. of Horsham. On visiting it in June, 1898, I found it in the occupation of the Duchess di Santo Teodora, and the name changed to Oakwood House; and now (1899) to Oak Grange. Mr. Jupp, a well sinker, who had been employed on Mullins' wells, told me there were three wells on the premises, of which two had been found by Mullins about 1893. One C, at the back of the house, now built over, is, he said, in hard blue marl 25 ft. deep with only  $2\frac{1}{2}$  ft. of water, and can be pumped out in about twenty minutes, though Mullins said there was water sufficient for everything. Another well, D, marked by Mullins close to the road some 50 yds. west of the house, is 20 ft. deep in blue marl and has a good lot of water. Another, E, dug about 1893 is 15 (or 30) ft. deep and supplies the vineries.

I also saw a new well, F, that had just been sunk 100 yds. or so north of the house, 43 or 44 ft. deep in soft dark marl (Weald Clay). A slight spring had been met with at 25 ft.; and at 40 ft. in a dove-colored marl water was found, which rose 7 ft. in a night and stands about 21 ft. from the surface.

Thus against one unsuccessful boring we have at least six successful wells, of which A and F sunk without a dowser are 27 and 44 ft. deep; while the dowsed wells B, C, and D, are 42, 25, and 20 ft. As the ground is approximately level, I infer a main water-bearing bed at about 40 ft. and a minor one at from 20 to 25 ft.

This is supported by the occurrence of water at other points along the level ridge which extends from Hedgecocks over two miles westwards to Rudgwick (Ridgewick). Thus the entrance lodge has a draw-pump; the adjoining cottage, 15 ft. lower, has a well 25 ft. deep; Rudge Farm, 600 yds. W., has a draw-pump; a cottage 1,600 yds. W. has a well 18 ft. deep with 5 ft. of water; and a quarter mile further a spring rises about 13 ft. below the top of the ridge.

This ridge is the first high ground in the Weald south of Leith Hill, and represents the escarpment of a hard or porous bed; which, according to Topley (*Geology of the Weald*, p. 104) is the same, "No. 2, sand and sandstone," that forms the water-bearing bed at Warnham Lodge,  $1\frac{3}{4}$  miles



E.S.E.—already described, *Proceedings S.P.R.*, Vol. XIII., pp. 278–279. The elevation, about 280 ft. O.D., is also the same.

At Hedgecocks, though the bed is less permeable, water seems to have been found everywhere except in the contractors' boring, the actual depth of which I have been unable to ascertain; in their letter (Vol. II., p. 103) they do not refer to it at all. If as deep as the wells, it corresponds with the dry ground in the Warnham Lodge bed. The case is interesting from the successful employment of two leading English dowers, though the necessity for their services may not be altogether demonstrated.

No 47. *Park Farm*.—[Lawrence marked two spots at each of which, after boring 10 ft., water was struck. As many persons said water might be found anywhere in the field, the tenant, as a test, bored to the same depth midway between the places, and found the subsoil to be perfectly dry. *Proceedings S.P.R.*, Vol. XIII., p. 71.]

This is the same place as the "Parks Farm" of the old one-inch map: it is on the Gloucester Road, S.E. of Frampton-on-Severn, and two miles north of Dursley Junction. The subsoil is stiff clay of the Lower Lias. Lawrence's well is just outside the garden gate, and is, Mrs. Prout said, 25 ft. deep; 10 yds. south of it is a pond about 10 yds. in diameter and 2 ft. deep, the level of which is  $4\frac{1}{2}$  ft. below the ground at the well.

Sixty yards north of the well is another pond about 15 yds. in diameter. The unsuccessful boring was made midway between these ponds.

The other successful boring was made, I was informed, by a man on the farm, about 20 yds. to the north of the north pond; he said they got at the bottom a sort of marly stuff, not very hard, in which the water came.

Mrs. Prout said that the water in Lawrence's well is salt, and is not used, and that all the water to Stonehouse is salt. The ponds go dry in summer, but Lawrence came at the end of March, or beginning of April, when they were full.

I think the proximity of the successful borings to the ponds (10 and 20 yds.) raises a presumption of their connection; and similarly the boring midway between the ponds may have been unsuccessful on account of its comparative remoteness.

I have not been able to communicate with Mr. Prout, but the steward, Mr. T. T. Vizard, of Dursley, writes in regard to the foregoing:—"Your particulars are, so far as I know, correct. I do not think the well can be supplied with water from the ponds near, because I believe the ponds become dry in the summer, but that the well always contains water."

Perhaps, however, as the well is not used, it would be safer to say that there is no evidence either way, as water would remain below ground after drying at the surface.

No. 57. *Norton Hall*.—[A neighbour had bored through over 1,200 ft. of Lias clay, then over 100 ft. more, and got no water he could utilise. At the lodge local well-sinkers got no water at 30 ft. J. Mullins predicted water in the stable yard at about 30 ft., which was found at about that, and rose 20 ft. He also predicted close to the house a very strong spring about 20 ft. down, and on sinking 17 ft. it came so fast they could sink no further. *Proceedings S.P.R.*, Vol. XIII., p. 84.]

This is the same place as the "Lower Norton Farm" of the old one-inch map: it is three miles north of Campden. The Hall stands at the end of a ridge of Lower Lias. Mr. Bruce said that the "gravel" in the clay consists of pebbles, which is suggestive of Drift (?). He gave me every facility for examining the wells at the house, of which there are three, viz:—

(a) An old well in the house yard 25 ft. deep, and 11 ft. 9 in. to the water; supply insufficient; this had been shown to Mullins.

(b) Mullins' well in the stable yard, 28 ft. 6 in. deep, and 8 ft. 9 in. to the water.

(c) Mullins' well, "close to the house," by the drawing-room window, 17 ft. deep, and 11 ft. 8 in. to the water.

On levelling, I found *c* to be 6 in. above *a*, and *a* 2 ft. 5 in. above *b*. Hence the water in *b* and *c* is at the same level, and that in *a* 7 in. lower. But *a* had been pumped that morning. Hence the water in all three wells is on a level. Mr. Bruce said also that pumping *a* lowers *c*. Hence the water in all three wells is in the same stratum.

The unsuccessful well at the entrance lodge, though on the same ridge and nearly the same level as the Hall, is 600 yds. distant. It is probably in a different stratum, and at any rate is too remote to be any proof of the absence of water at other points in the neighbourhood of *a*, *b*, and *c*.

Mr. Bruce, who has revised the foregoing, writes:—"I have always thought that *immediately* round my house was 'Oolite' ('Brash' the country people call it) but only a little on the surface and a very limited area, 100 yds. or so." If stone occurs here it would tend to form a natural reservoir. He adds, "The deep boring is at Kift's Gate, 1½ miles to N.E. of this, and lies a good deal higher"—i.e., on or near Meon Hill (Marlstone, or top of the Lower Lias).

No. 77. *Arnold's*.—[The owner writes that the old well, about 60 ft. deep, had a quite inadequate supply, and was bored 100 ft. without getting more. He says, "I sent for W. H. Mullins, and he said there was water at about 60 ft. deep on the top of a hill 300 yds. from the old well. He dug a well on the site and found water at a depth of 90 ft., and now we have an abundant supply." *Proceedings S.P.R.*, Vol. XIII., p. 99.]

This place is in Surrey about three-quarters of a mile south of Holmwood Station, and is all on the upper part of the Weald Clay.

Mr. Bayley said that the old well at the house was 60 ft. deep in grey, soapy, shaley clay, and gave an inadequate supply of water; it was bored another 100 ft. without getting any more.

Mr. W. H. Mullins' well is about 650 yds. distant at the northern extremity of the property. It is close to the railway, on a little rising ground so as to be convenient for gravitation, and is 40 yds. from and 18 ft. above a stream of water which is fed by springs from Leith Hill and flows all the year round. Mr. Bayley's gardener informed me that the well was sunk 99 ft. deep in grey, soapy clay without stone (Weald Clay). There were little springs the size of a straw all the way down, but the supply was small—about 3 ft. in the twenty-four hours—and it became necessary to augment it.

Between the well and the stream is a narrow, flat piece of ground apparently alluvial in character, and about midway on this the rod indicated water. Here,  $6\frac{1}{2}$  ft. above the stream, a second well was sunk to a depth of 10 ft., and an abundant supply was found which rises to about the level of the stream. This supply is carried through by a drain into the first well, which, in Mr. Bayley's letter (*Proceedings S.P.R.*, Vol. XIII., p. 99), gets all the credit of it.

As, however, I see no reason to suppose that the water comes from anywhere but the adjoining stream, I cannot regard this case as showing more than ordinary observation.

No. 90. *Woodside, Wootton, Isle of Wight*.—[An old well, B, 12 ft. deep had a small supply very scarce through the summer. Another well, A, was sunk 120 ft. and got no water. The diviner, W. Stone, then indicated an enormous spring at a point 200 ft. from and on the same level as B, and about 550 ft. from and 14 ft. above A. Here he sunk a well, C, 7 ft. deep, and got an abundant supply nearly always running. *Proceedings S.P.R.*, Vol. XIII., pp. 130–133.]

The unsuccessful well A (see plan on p. 132 of Professor Barrett's paper) is about 225 yds. (not 350, as Taylor said) to the north of Stone's well, and about 50 to 60 ft. lower (not 14 ft.). It is the lower part of the Bembridge Marls, and, as it is stated to be 120 ft. deep, must pass through the Bembridge Limestone into the Osborne Marls.

The other well B, according to Mr. Taylor, the gardener at Woodside, is 12 ft. deep, and afforded a small supply. According to Mr. W. H. Cole, a former tenant of Wootton Farm, it was dug when the Woodside house was built, about 1850, and the water was brought down in pipes. Mr. Stone's statements, that it is "30 ft. deep and no water," and that "the owners of the estate had never been able to obtain water," are incorrect. This well is in sands of the Hamstead series.



The first well sunk by Stone was, Mr. Taylor informed me, at a point about 20 yds. to the east of B, but he did not get enough water and the well was abandoned; if so, Stone's statement that he found an enormous spring in ten minutes is incorrect.

Stone then dug the well C 60 yds. to the S.E. of B, and thence carried a trench about 80 yds. to the south as far as the road, the ground at all these points being practically at the same level. This trench was about 10 ft. deep and cut through gravel (Pleistocene Plateau), sand (Hamstead Beds), and some clay (Bembridge Marls). The water is collected by a drain and carried into C; the principal flow was met with a few yards from C.

More recently, in order to supply another house, another collecting drain has been carried up on the south side of the road, and curved round into the field to the west of Stone's drain, so as to head off the further end of it, since when, Taylor said, there has been a noticeable falling off in the supply. Subsequently also to Stone's visit a sand pit has been opened about 130 yds. west of C, showing 3 ft. of gravel on sand, in which at a depth of 15 ft. a body of water was found. The outcrop of this sand coincides nearly with that of the plateau gravel; the two together forming a natural reservoir over a quarter of a mile wide capping the hill.

Along the eastern edge of the hill I saw some ponds, and rills of water running down to the creek: Mr. Cole said there were nine such "runs" of water within a mile, and that they run all the year round. I also discussed the matter with the agent for the Woodside estate, Mr. George Barton of Wootton Farm, with Mr. W. H. Cole, and with Mr. John Newbury of Wootton, all of whom agreed that there was water everywhere on the top of the hill, that it was known to every one, and that it did not want a divining rod to find it. These opinions expressed after the event must be taken for what they are worth; on the other hand, the occurrence of water at other points on the hill-top, and in particular at B, 60 yds. distant, from which the house had been formerly supplied, was a matter of common knowledge.

*Arreton, Isle of Wight.*<sup>1</sup>—The diviner, W. Stone, writes that a dry well had been sunk to a depth of about 50 ft., within 20 yds. of which, he says, "I discovered in a few minutes a spot beneath which a good supply of water was flowing," and all the natives were astonished; "I however, guaranteed a spring at 10 ft. . . . and my men tapped water at 9 ft., which quickly

<sup>1</sup> This case was quoted from the report appearing in the *Morning Post*, and was given for what it was worth. It has nothing to do with the so-called evidential cases, and is only included in Mr. Westlake's report as he happened to be in the neighbourhood. It well illustrates, as already pointed out, the untrustworthiness, as a rule, of newspaper reports on any scientific question.—W. F. B.

rose to the top," and he laid it on to the rectory and village by gravitation. *Proceedings S.P.R.*, Vol. XIII., pp. 130-131, 133.]

This is the only case inserted by Professor Barrett without local inquiry, which he asks for on p. 133 of his paper; as such it was not numbered or claimed as evidence; but, having been published, I give a more correct version as showing the humours of the subject. The village of Arreton lies at the foot of the downs that form the central Cretaceous escarpment. Besides going over the ground, I obtained information from Mr. George Barton, of Wootton Farm, who had got the dowser to go to Arreton, from a resident at Arreton, and from Mr. Orchard, the parish clerk.

They say that the dowser's quoted statements are incorrect, and that where he placed his first reservoir he never got any water at all. He was so sure there was water that, before getting any, he began by making a large reservoir 9 ft. deep with pipes down to the cottages on the S.W. side of the church. There was rain and snow at the time (the *Daily Graphic* of April 18th, 1892, says: "the snow on the 16th was four inches deep in most parts of the Isle of Wight") which filled the reservoir 2 ft. deep, and with that and the water in the pipes he got, when the tap was turned on at the cottages, a powerful jet which was photographed; but after a few minutes it ran out and there has never been a drop since. The photograph appeared in the *Daily Graphic*, of April 23rd, under the heading "A Water Wizard in the Isle of Wight," together with a statement that the spring had proved more than sufficient to supply the wants of the village.

The dowser then tried, said Mr. Orchard, to find a spring higher up the hill by running back a trench from the top of his tank, but when he got 22 ft. deep he got into the freestone and then he stopped, because he knew that if he got any water it would run away. This point would be some 30 yds. north of the reservoir. Forty yards further up the hill Upper Greensand is seen dipping 40 deg. into the hill, and, if the "freestone" forms its base, then the dowser's reservoir and trench must be on the apex of a wedge of Gault Clay, the last place in the district where there could be any water. Or if they occupy (as according to the new six-inch geological map they do, though I doubt its accuracy) a similar position on the outcrop of the Carstone, a coarse, hard grit beneath the Gault, the apex of this mass of sand rock would be equally incapable of holding water. If, as the dowser states, an unsuccessful well was sunk within 20 yds. of his, to a depth of 50 ft., without finding a drop of water, it shows that the water-level in this Neocomian sandrock is still lower.

Failing to find water here, the dowser then made a second reservoir, 40 ft. below the first, on the east side of some marshy ground in a little valley above the churchyard, and thence carried a drain alongside

the marsh some 30 yds. to the north. This reservoir is 7 to 8 ft. deep to the spring of the dome. Its bottom, which is 5 to 6 ft. below the level of the marsh, is a peaty "sog" (*Anglice*, saturated bog) through which the water rises freely. The water contains iron and peat, and a new supply will have to be obtained.

This case is only quoted as an illustration of the untrustworthiness of statements made by interested parties and of newspaper reports.

No. 121. *Stroud*.—[A boring had been made to a depth of 70 ft. but no water found. At a point 300 yds. distant Mr. Gataker predicted water at 50 ft., and a good spring was actually found at 45 ft. *Proceedings S.P.R.*, Vol. XIII., p. 180.]

In this case the unsuccessful boring had been made at the bottom of an old well at Farm Hill House, in the terrace at the top of the lawn. The butler who showed it me said it was about 50 ft. deep, and contained plenty of water except one very dry summer. Before Mr. Gataker came, Messrs. Orchard and Peer, of Stroud, bored some 20 ft. at the bottom of it with the idea of getting more water, and did not. This firm, who are not professional well-sinkers, write that the well is 30 ft. deep, and that they bored 35 ft. deeper in clay, clay bats and marl clay. The well is about 240 yds. south of Gataker's, and at a level from 50 to 60 ft. lower. There is also a well under the house still in use. These wells are in the top of the Upper Lias.

Mr. Gataker's well is in the field above the house and is 45 ft. deep, with water 24 ft. from the surface. He (Gataker) said the water would be found 60 ft. down, but they got it at 40 ft. all round the sides of the well. The rock, according to Mr. Bastin, the gardener, consisted of 20 ft. of rock rubble and sandy freestone (Inferior Oolite remanié), with yellow clay at the base: below this was 10 ft. or so of slate-coloured clay, and several feet of loose reddish grit like sea sand, and gravelly rubble with water (Midford Sands).

At the house of Mr. Allen's gardener, 110 yds. west of Gataker's well and from 6 to 7 ft. above it, is a well which the gardener, Mr. Harwood, said contained plenty of water. Mr. Riley, a working plumber in Stroud, who had repaired it two years ago, said it was from 35 to 40 ft. deep, and contained more than 5 and less than 10 ft. of water, which would make the water about 30 ft. from the surface. Hence the water in Gataker's well stands at the same level.

There is also, 300 yds. E.S.E. of Gataker's well and from 60 to 70 ft. below it in the same field, a well 16 ft. deep, containing 8 ft. of water, affording a small but good supply for cattle. About 50 yds. short of this a spring rises in wet weather and forms a bog; this, according to the Survey map, marks the base of the Midford Sands.



Also along the ridge going to Whiteshill (called "The Plain"—see *Geological Survey*, sheet 34, N.W. corner), from quarter to half a mile north of Gataker's well and from 65 to 50 ft. above it, are five wells about 50 ft. deep, containing from 3 to 13 ft. of water at the base of the Inferior Oolite.

The occurrence of these wells to the south, west, east and north of Gataker's renders it probable that there is water everywhere in the field; especially as it is on the outcrop of the Midford Sands, which are the main water-bearing bed of the district.

Mr. G. P. Milnes, the water surveyor at Stroud, who had had charge of the job for Mrs. Holloway, agreed with me that the case did not need a dowser.

No. 99. *Melksham*.—[A well was sunk 21 ft. and bored a further 18 ft., but no water of any consequence was met with. In the extreme corner of the field Tompkins predicted water at not over 25 ft. At 30 (or 22) ft. a spring came up like a fountain, and has yielded a plentiful and unfailing supply. *Proceedings S.P.R.*, Vol. XIII., pp. 146-148.]

The town is situated just in the middle of an Oxford Clay vale. The unsuccessful well was sunk in the meadow at the back of the butter factory, and 33 yds. from the river Avon. It contained a little water which percolated from the river.

Mr. Tompkins' well is from 80 to 90 yds. to the W.S.W. of the first well, and 50 yds. from the river and from 10 to 12 ft. above it. The water is 11 ft. from the surface. The engineer said there was a good supply, and he did not think it had to do with the river. The soil, Mr. Maggs' son said, was stiff white clay and blue marl without any rock, and he thought was mostly made ground, as they found pipes and old chain. Since the feather factory had cut a drain 20 ft. deep, some 30 yds. to the south of the well, it had not had so much water; this a neighbour confirmed, and said that lots of water was met with in the drain at the point nearest to the well. There is also a well which supplies an old house some 40 yds. from Tompkins well on the side remote from the river.

Hence, as Tompkins' well is in what appears to be an alluvial flat, where water was known to exist within 50 yds. on either side, I cannot regard its occurrence at the point indicated by him as surprising. He did, however, find a much larger supply and at a greater distance from the river than in the first well.

Mr. J. Maggs, who has revised the foregoing, writes:—"In my opinion the general tendency of [your] report is to minimise the importance of Tompkins' well.

"But the facts are: The first well was absolutely useless as a water supply, whilst the present one rarely fails to supply 3,000 to 5,000

gallons *daily*. Further, Tompkins was unacquainted with the neighbouring well. Water was found at the precise depth named by Tompkins, and, when tapped, the men at work in the well had to hurry up or would have been overwhelmed with the sudden rush."

Two attempts I made to interview the well-sinker failed, so that I cannot say whether Tompkins hit on a permeable stratum in the Oxford Clay or on a permeable patch in the river alluvium.

#### GROUP (G)—LITTLE OR NO SURFACE SIGN OF WATER.

*Lytes Cary* [Additional Case].—Here water was found by a dowser at a depth of 25 ft., and only 40 ft. from a dry well 60 ft. deep. Further off were two or three other wells, one of which contained some potable water, which lessens the improbability of finding it, but the spot remains an unlikely one, nor could I discover any reason for its selection. I have described the case more fully on pp. 174–175.

*Wimblehurst, Horsham* [Additional Case].—On this estate, which is half a mile north of Horsham station, water has been found at two places by Mr. W. H. Mullins for the owner, Mr. E. Allcard. The well at the house is 76 ft. deep and supplies about 500 gallons a day. Fifty yards to the north of this, Mullins found water at 25 ft. only; the amount, however, that has been pumped daily is comparatively small, less than 100 gallons.

At the farm in North Heath Lane, about 350 yds. further north, is a well 67 ft. deep which always went dry in dry seasons, though bored to 83 ft. In October, 1897, at a point 50 yds. to the east, Mullins predicted a spring at 70 or 80 ft., and proceeded to sink on the terms of "no supply, no pay." Water was struck at 54 ft., and the well, which is 60 ft. deep, holds 10 ft. of water and yields a large supply. The rock consisted of sand and sandstones of varying hardness with some lignite and ironstone; the water-bearing bed being a dove-colored marl. These beds are the Upper Tunbridge Wells Sand; and being of freshwater origin, and hence usually lenticular and patchy, may be expected to hold water in pockets. We have here a comparatively large body of water standing 17 ft. above the bottom of the old well and 33 ft. above the bottom of the boring; the surface of the ground being practically level, with no surface indication whatever to show that there should be water at one place rather than another.

Since writing this, I hear that at the two cottages by the road, 130 yds. S.W. of Mullins' well, there is a well (marked p. on the 25 inch map), only 11 ft. deep, which always supplies enough water for the two houses. Mullins did not put his well in the direction of this surface water; still its occurrence diminishes the argument from its

absence in the deep well, and makes chance discovery a possible alternative.

Mr. Allcard, who has revised the foregoing, writes, November 16th, 1899 :—"The well at the two cottages has been dry lately, whereas Mullins' well at the farm has given a good supply all the dry summer. What we should have done without it I do not know, and I recommend any one wanting water to employ him, and not waste money on chance on their own account."

No. 105. *Toy Farm*.—[The following abstract is from the accounts by the steward, Mr. T. W. Pickard, written in 1893 and 1896 (*Proceedings S.P.R.*, Vol. XIII., p. 154). It includes additions received from him dated November 6th and 25th, 1899, and the whole has had his revision :—

About 30 years ago a well was sunk at the Farm for some distance, and then abandoned on account of bad air. After this another well was dug, 167 ft. deep, but no water was found. Some short time before 1893 I personally took the depth; it was then quite dry, and the old men living at the place never remembered water there.

In 1893 Mr. Tompkins, of Chippenham, was engaged. I purposely met him at the station and drove him to the place. He had no idea to what part of the estate he had to go until he got there, and he had no private conversation with any of the men there. He went over the ground tracing all the springs to one point, 450 ft. from the old well and from 8 to 10 ft. above it, and said that water would be found at a depth of 70 ft. I have marked on the enclosed tracing from the 25 in. map, as near as I can recollect, the lines he traced (they form a star of six sinuous lines over 100 yds. long).

After he had found the place for the new well I took him to the old well, which had been covered up with strong timber; to mark the site there was a small heap of mould on which cucumbers were growing. I sent him over this with his twig, which made no movement. I then told him he had been over an old well several times: I told him the depth, and asked him how he thought it possible to find water nearer the surface such a short distance away. He replied that the old well was out of the run of the springs.

We dug at the spot selected, and the first 20 ft. of chalk was rubbly [probably decomposed]. Nearly 70 ft. down we came on a level bed of solid flint about 6 to 9 in. thick; and at 70 ft. on open fissures mostly vertical, which were wet as though water had recently passed through; there was a very slight dribble at the time.<sup>1</sup> At about 118 ft. we struck two springs on opposite sides of the well giving a strong supply, and sunk an additional 6 ft. before the water gained the upper hand. The well is 124 ft. deep, in ordinary white chalk with flints, which continued to the bottom. The well holds 12 ft. of water, the same as when first dug. The water at

<sup>1</sup> Through one of these, according to Mr. Sherlock, a young man at the Farm, a draught of air came with such force as to be heard at the surface, showing an open fissure of great extent. He said the well takes an hour to pump out, and refills in twelve or less.



the bottom came through vertical fissures and horizontal veins, and principally from the west side from the directions Tompkins had named from either end of the plantation (an enclosure on the hillside to the W. and S.W.). We have since bored some 20 ft. to obtain a better supply, but I do not think it has improved it. The old well has been filled in with the chalk, etc., from the new one.]

This I regard as the best case I have yet met with in favour of the dowser. The farm is situated in a remote hollow of the Sussex downs, midway between Glynde and Newhaven. The valley of which it is a ramification falls into the sea at Bishopstone, three miles to the south: half way down the Newhaven Water Company are pumping a considerable supply from wells. The old well was close to the farm, and the level of the ground at the well is about 260 ft. above the sea.

The new well is 150 yds. further up the valley, and about 22 ft. above the old well. Hence the water standing 12 ft. deep in the new well has a level of no less than 77 ft. above the bottom of the old well. The new well is in chalk with flints. On the heap from it, shown in the photograph (p. 337), I found a spine of *Cidaris sceptrifera*, which is common in the zone of *Micraster cor-anguinum*, so that the thick flint-bed 70 ft. down is probably the strong and constant *M. cor-anguinum*-tabular described by Dr. Rowe as occurring between Beachy and Seaford Heads about 62 ft. above the zone of *M. cor-testudinarium* (*Proc. Geol. Assoc.*, 1900, Vol. XVI., pp. 322, 323, 329, 332, 336, Plate IX.). According to this reading the new well is sunk entirely in the *M. cor-anguinum* chalk and to within 8 ft. of its base; which last may account for finding some other fossils, as two or three specimens of *Holaster placenta*, which are commoner in the lower zone. The bottom of the old well is similarly 57 ft. deep in the zone of *M. cor-testudinarium* and 52 ft. above its base—it being 109½ ft. thick on the coast (*Ibid.*, p. 327).

Water in the chalk usually lies at a gentle gradient<sup>1</sup> which is however, modified by the density of particular beds, and by the presence of fissures. In the lithology of the beds as seen on the coast there is nothing to explain the absence of water in the old well, or its preferential occurrence in the new well, which is doubtless due to fissures. Water in chalk usually flows in fissures,

<sup>1</sup>The gradient of water in chalk varies considerably, *e.g.*, 13 ft. in a mile (Clutterbuck); 45 and 47 ft. in a mile (Bland); these are normal conditions. Exceptionally we have 93 and 102 ft. in less than a mile (Bland); 155 ft. in half a mile (Lucas); and 50 or 90 ft. in less than 100 yards (Anstead). This last, which Lucas says is greater than he had ever met with, is the only instance I have found at all parallel to that at Toy Farm. (See Prestwich's *Water-bearing Strata*, p. 62, and *Trans. Inst. Surveyors* for 1878, Vol. X., pp. 306-314.)

*i.e.*, in the main planes of jointing, which in this district, as shown by the direction of the valleys, run N. and S., and E. and W.

The narrow valley, approximately N. and S., in which the farm lies, is slightly sickle-shaped. The house with the old well is about the middle of the handle, so to speak, and the new well at the ferrule end, the valley at this point being contracted by a slight bulge from the east, which may, and probably does, indicate the position of an underground fissure. With this may be taken the fact, noted by the steward, Mr. Pickard, that the water flowed into the well from the sides of the valley rather than from its upper end.

Whether Mr. Tompkins noticed a slight surface feature of this kind—I did not myself till I was leaving and saw it foreshortened—and, if he did, whether he drew the right inference from it, may be doubtful. I think a geologist at any rate would have taken no account of it, but would have advised deepening or tunnelling the old well.<sup>1</sup> We must remember, however, when we are dealing with automatism, that the mere existence of a difference in the valley unnoted, still less reasoned on, by the dowser, may have been felt by his subconsciousness, intensely alert for every indication, and so have determined his action—on which, unlike the geologist, he can, when his judgment fails, fall back. He can revert to that prehuman thinking of which Cowper says:—“Reasoning at every step he treads, man yet mistakes his way, whilst meaner things whom instinct leads, are rarely known to stray.”

Hence, at Toy Farm, we have, I think, either a case of discovery by automatism, or else of accidental coincidence. In the latter connection we may note that where water has not been found at one end of a property it might be natural for a dowser, in the absence of anything to guide him, to try for it at the other end, where also the pump at (p) may have suggested it.

Since writing the foregoing I have heard from Mr. David Caplin, Messrs. Duke and Ockenden's foreman, who writes in reply to inquiries:—

New Road, Buxted, *November 27th*, 1899.

By what I can remember about the well I sunk at Toy Farm, the chalk down to about 60 ft. was of a very hard yellow nature with a lot of black flints. Below this for about 10 ft. there were places where I should think water had been passing through at one time. From 70 ft. to about 110 ft. the chalk was in more of a block, but not so hard. The remainder of the

<sup>1</sup> Professor Barrett asks what a geologist would have done under the circumstances. He would hardly have sunk the old well another 50 ft. to the nodular beds in the *H. planus* zone, as they are here too doubtfully water-bearing; and, failing a damp place to drive on, the best plan would have been to drive from the bottom on the chance of cutting fissures. Water could so have been found, and the probable quantity and cost computed from similar work in the district, as at the Brighton Waterworks.

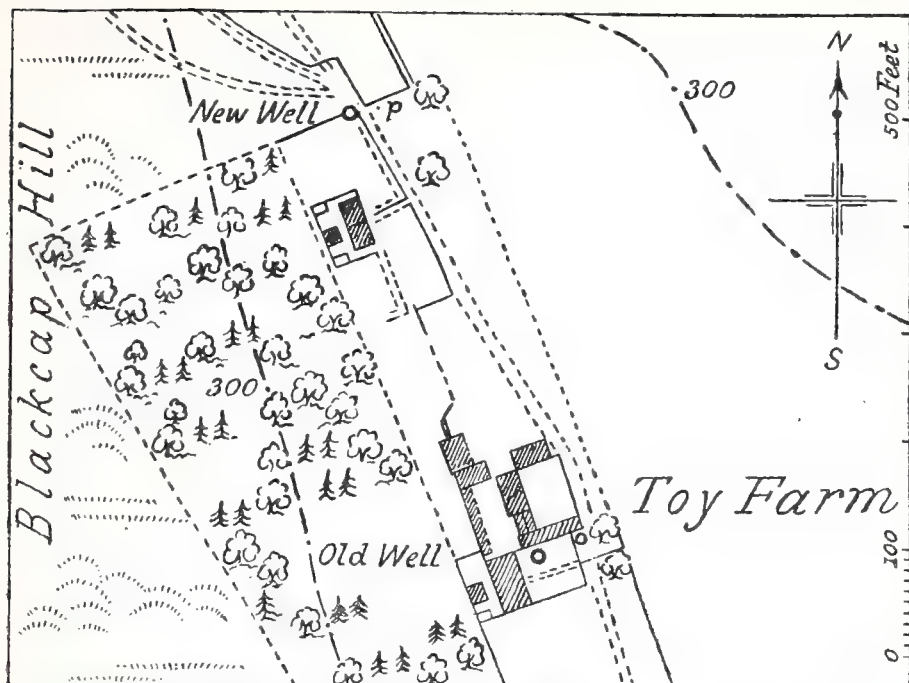


FIG. 19.

## PLAN OF TOY FARM.

*From the 25 in. Ordnance Survey Map of Sussex.*

The strong dotted line is the 300 ft. contour. The well at the farm, to the east of the old well, is a storage tank: the pump at (p), connected with a storm-water tank, was removed after the sinking of the new well, dowsed by Tompkins.



FIG. 20.

General view looking up valley from Toy Farm; Tompkins' well is at corner of road to left of shed.



sinking was in a much whiter chalk with black veins in it, and we did not get any water till within about 2 ft. of the fissure, which I believe is about 6 ft. from the bottom of the well. There were two fissures coming into the well, not opposite one another, but one I should think about due west and the other north-west, and there was no water coming from the east side at all.

This rather favours Tompkins' idea of springs meeting at a point than mine of a main east and west fissure; Mr. Pickard, however, mentions springs from opposite sides. The point could only be settled, if at all, by examining the well, which I was unable to do at the time of my visit owing to the obstacles of the pump and staging.

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### CONCLUSION.

Out of 21 cases of a water-supply obtained by means of a dowser, which I have now personally examined on the spot, there are, in my opinion, only three—the three last described—which may be called evidential of some supernormal faculty. To these may perhaps be added the partial evidence at Melksham (No. 99); and at Warnham Lodge (No. 58, described Vol. XIII., p. 279), where, though the bed was known to be water-bearing, the dowser had, I found, succeeded in predicting a dry place. I have thus met with from three to five cases, or from 15 to 25 per cent., in which his success could not have been due to information normally acquired.

In the other cases, while the dowser may have known but a part of what I describe, I could seldom feel certain what he did or did not know. From what I have seen of Somersetshire dowsers at work, I agree with Professor Barrett that they do not seek for information—neither do they avoid it, and in the general conversation that ensues when the folks come out to see the water-finding it would be surprising if something was not said about existing water. Firmly believing in an external force moving the rod, the dowsers seek for water as one might seek for iron with a compass, and a group of them over a particular peg will confirm each other's indications with as much satisfaction as if "suggestion" had never been heard of. But as Professor Barrett has shown that the involuntary motion of the rod is due to unconscious suggestion, of normal or supernormal origin,—the subconsciousness standing ready to act on the slightest hint—we must make sure that normal hints have been kept out of our experiments before we can use them as arguments for the supernormal. It seems to me, therefore, that most of the success in dowsing (say three-fourths) may be attributed to information reaching the dowser in a normal way, the subconscious synthesis of which determines the movement of the rod. The evidential fourth may be due to chance coincidence, or to some yet undiscovered faculty.

The popular vogue which dowsing has obtained in this country, and which appears to be increasing, is due to something more than mere success. The involuntary but visible movement of the twig inspires the dowser with a confidence in himself, and his employers with a confidence in him, that no amount of reasoning would. This confidence in subconscious pronouncements, which is as old as humanity, implies here a belief in some supernormal relation between the rod and water. If existing, and involving the dowser's mind, my own opinion is that it is likely to show itself in the same way as other subconscious faculty, *i.e.*, fitfully and occasionally.

It is, I suppose, well within the mark to assume that three-fourths of most automatisms are subjective, *i.e.*, revivals of memory, or dream-like imagination, and that one-fourth is a liberal estimate for the supernormal. To suppose, then, that the supernormal element in dowsing, if such there be, will appear in any larger proportion, seems to me highly improbable. We may expect it to form a residuum which will be discovered only when, as in stellar parallax, many concurrent causes have been eliminated. This is what I have attempted to do in this report.

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NOTE.

[The foregoing able report by Mr. Westlake shows on the one hand the value of investigation on the spot by a competent geologist, and on the other how much easier it is to be wise *after* an event than before it. Moreover, with laudable anxiety to avoid any bias in favour of the dowser, Mr. Westlake has no doubt gone to the opposite extreme. For he assumes the dowser possessed minute local and stratigraphical knowledge, had inspected all existing wells, and had ability to draw inferences from all these data. These assumptions are not justified. The dowser, usually an ignorant man, is brought into a district he has probably never seen before, he asks no questions, but immediately sets to work perambulating the ground with his forked twig, and the whole business is over in a very short time. If under similar conditions, and before opening the ground, a field geologist would tell us whether the dowser's prediction is likely to be right or wrong, that would be a fairer test of whether the dowser's success is mainly due to "a sharp eye for the ground," and I hope this test will be made by any who may have the opportunity. I agree, however, with the general conclusion arrived at by Mr. Westlake, but think that probably a larger percentage of the dowser's success—especially of certain dowsers—is due to some cause at present unknown to science.—W. F. B.]

## APPENDIX B.

## NOTE ON THE GEOLOGY OF SOMERSET IN RELATION TO DOWSING.

BY E. WESTLAKE, F.G.S.

The mineralised ridge of the Mendip Hills just above the middle of the county has been the seat of mineral-dowsing since the time of Queen Elizabeth. The practice was probably introduced into Cornwall simultaneously, but has become practically extinct for this purpose in both localities. In Somersetshire, however, local conditions, favouring the later application of the rod to water-finding, have preserved its use to the present day. The Mendip itself consists of permeable Palæozoic rocks—Mountain Limestone on a core of old Red Sandstone—in which the water for the most part sinks to too great a depth to be reached by wells. It is the Secondary rocks lying to the north and south—the Trias, Lias, and Oolite—forming what De Rance calls a “supra-pervious” series and yielding underground water, which have determined that Somersetshire and not Cornwall shall be the centre of water-dowsing.

The two last (Jurassic), which occupy about half the area of the county, may be described as alternations of clays and limestones, the sand beds being few and subordinate. The Oolites, especially in the upper part, consist of clays often of great thickness, alternating with massive limestones forming reservoirs in which it is comparatively easy for a geologist to predict water; although even here, the limestones being well consolidated, success where quantity is required may depend upon the accidental striking of a water-bearing fissure.

In the Lower Lias, which forms most of the low-lying arable land of the county, clays and limestones alternate every few feet or inches, and are alike impermeable, as De Rance indeed terms the whole formation. When uncovered in the quarries they are seen, however, to be traversed by cracks and occasional faults, like much-skated-upon ice, which allow of a slow percolation and the storage of small bodies of water.<sup>1</sup> There is no record of any copious supply, and the finding even of a domestic supply depends on a chance meeting with these water-bearing joints of which there is no sign at the surface. This, which is the geologist's extremity, is the dowser's opportunity; or, in

<sup>1</sup> Water not only sinks through the Lias, but *rises* through it, as in the springs of Bath and Cheltenham.



other words, it is not surprising that where common-sense ends, the uncommon-sense attributed to the dowser should be in demand. The water, it is true, is sometimes impure, the fissured character of the formation being more favourable to pollution near farms or villages than to purification. Thus at the town of Somerton the well water was analysed by the Rivers Pollution Commission and found to be nearly all dangerously polluted.<sup>1</sup>

We may note, moreover, that dowsing is *not* carried on on the Somersetshire moorlands, which comprise the high-lying impermeable Devonian areas of Exmoor and the Quantocks drained by surface streams, and the low-lying alluvial levels debouching on the Bristol Channel where water lies near the surface. Cornwall also consists of palæozoic clay-slate ("killas") with granite and other igneous rocks, all of which throw off the water, except the little that percolates in fissures.

There are two main types of underground water—what may be termed *bed-water*, or seepage, filtering through the pores of beds;<sup>2</sup> and *fissure-water*, or drainage, flowing through their joints.<sup>3</sup> The first type characterises permeable beds, the second the impermeable. To the latter condition the consolidation due to ages tends to bring all the aqueous rocks; all the igneous are so from the time of cooling:

<sup>1</sup> The local dowser, however, an old farmer, has proved equal to the occasion, and guarantees a pure supply by "andalyzing" the water, as he terms it, by means of two twigs held simultaneously.

<sup>2</sup> There is no English term for this: "interstitial water," or "quarry-water," is properly the water *held* in the capillary pores of a rock and not expelled short of drying. The Scotch dialect word, *seepage*, is, however, used in the United States to designate the water that soaks through and oozes from the soil, and is so employed by Mr. King (see under). It has the sense of *oozage*, and contrasts with *drainage* (water flowing freely in non-capillary channels).

<sup>3</sup> The movement of underground water, or ground water as it is called (Germ. *Grundwasser*), has just been elaborately treated in two papers by Messrs. F. H. King and C. S. Slichter, in the *19th Annual Report of the United States Geological Survey*, 1899, Part II., pp. 59-294, 295-384; *Washington*. The motion of water in the pores of beds, typically sands and sandstones, is very thoroughly discussed both from the experimental side and from the mathematical laws of capillarity. The authors allow, however, that their conclusions are radically modified by the occurrence of fissures, and that the *actual* motion of the water is very different from what it would be in homogeneous beds in which percolation proceeded through the pores alone. Thus, in the Dakota artesian basin, which consists of Cretaceous sands and sandstones particularly favourable for the storage and transmission of water, Mr. King shows that the largest theoretical seepage, or capillary flow, is only one-sixth of the observed flow (*Ibid.*, p. 249), so that about 80 per cent. must take place through fissures. In less permeable beds this proportion is probably largely exceeded. Thus, in a flume of the West Los Angeles Water Company, the observed flow exceeded the largest capillary computed nearly tenfold (*Ibid.*, p. 255). Mr. Slichter appends a list of 79 papers on the subject, chiefly in German.

such rocks are seen in the Palæozoic of Cornwall, Wales, the Lake district, etc.<sup>1</sup>

Bed-water is the more *usual* form, in the sense of being the one most met with in the populated lowlands. But as we pass from the looser rocks in the south-eastern counties to the more consolidated in the west, the proportion of fissure-water increases, till in the mountains there is nothing else. Tending to keep in compartments, it is difficult for a geologist to predict its level, and still more its exact place or quantity. In these Palæozoic districts, however, surface water is usually abundant and good, and the dowsers are superfluous; and so also in the Tertiary where the rocks hold only bed-water. His happy hunting ground lies therefore, as we have seen, in the intermediate conditions of the Secondary rocks, such as exist in Somersetshire.

E. W.

<sup>1</sup> An illustration from Llanelly is furnished by Mr. J. F. Young, who says in a letter to Professor Barrett:—"In the Coal Measures exposed in a new dock near here numerous springs have been intersected, issuing with considerable force through fissures in the rock, some of which were horizontal whilst others were vertical. In a new coal pit they are sinking near my residence I noticed the same thing, showing beyond dispute that underground water in this and similar districts *does* circulate through fissures, or follows the dip of the strata, and that intervening portions at some distance below the surface may be quite dry."

## APPENDIX C.

## USE OF THE ROD IN THE SEARCH FOR MINERAL LODES.

Owing to the length to which the present Report has grown, I have been compelled to postpone the publication of the mass of evidence

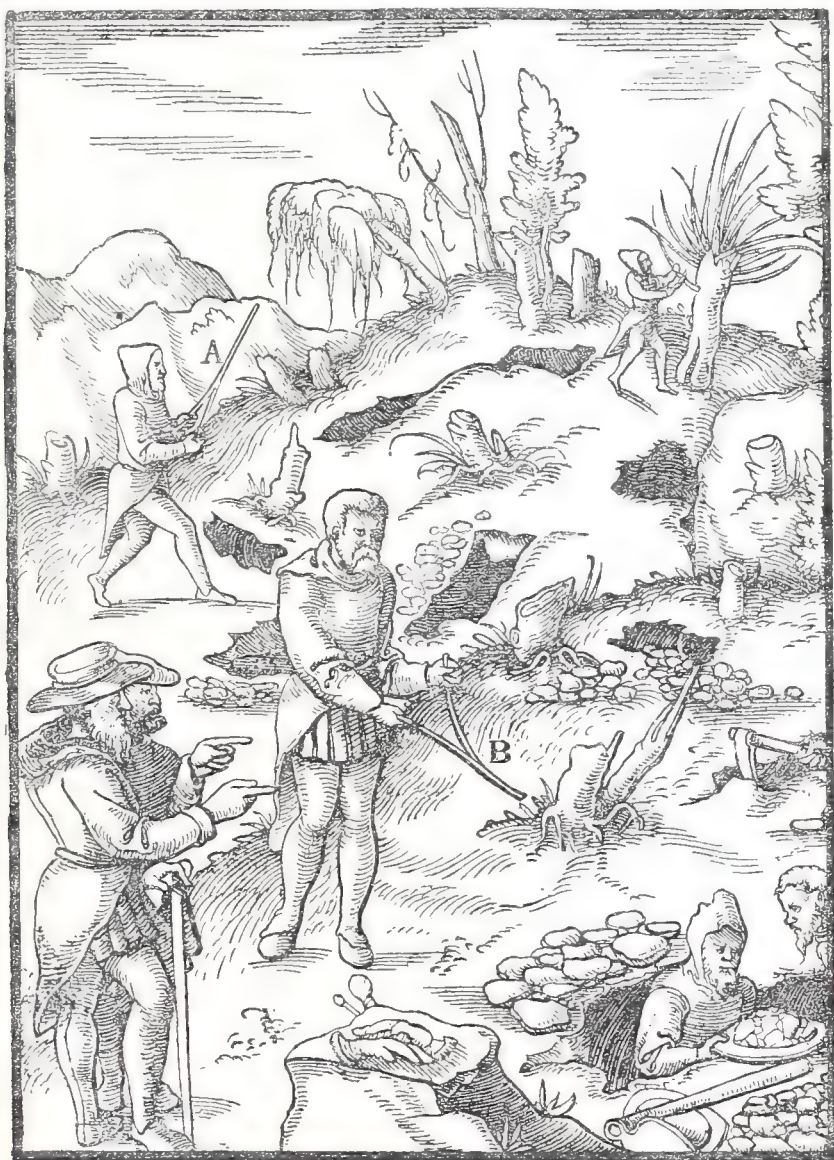


FIG. 21.

From G. Agricola's "*De Re Metallica*" (Baste, 1546).

I have collected regarding the use of the rod in the search for mineral veins. As already mentioned, this use goes back to a period long



anterior to the first employment of the rod in the search for underground water. The introduction of the *virgula divina*, or, as it should be called, *virgula furcata*, into England took place, I have little doubt, in the reign of Queen Elizabeth, when German miners were brought over to develop the mineral wealth of Cornwall. They appear to have brought with them their *schlag-ruthe* (striking-rod)—our modern forked divining-rod—and the phrase “striking” the vein, now extended to “striking” oil, etc., is probably a survival in modern English of the term used by these old German miners. The word “dowsing-rod” is, I venture to think,—with all deference to my philological friends,—the translation of *schlag-ruthe* into middle English. (See note to p. 135.)

On p. 343 is a picture (Fig. 21) of these German miners at work with their *virgula furcata*. It is taken from the edition published at Basle in 1546 of the great folio on mining, *De re Metallica*, by G. Agricola. Successive scenes are shown in one picture: at A the dowser is traversing the ground; at B he has struck ore, which on digging is subsequently found and pointed out by two overseers. The cutting of the rod from a willow is seen on the right. The serious, business-like air of the men holding the rod is evident; this is also apparent in another woodcut to be found in an edition of Sebastian Munster's *Cosmography*, published in 1550. These are the *earliest* authentic descriptions and representations of the modern forked divining rod that I have been able to discover.<sup>1</sup> Another early picture of dowsing for minerals (from a rare Italian work on mining) is given on p. 345; a photograph was taken by permission from the copy in the British Museum.

From Cornwall the use of the rod spread to the lead mines in the Mendips, and in the 17th century it was widely employed in the search for ore in that district of Somersetshire. Billingsley, in his *General View of Somerset*, published in 1797, gives a lengthy account of its use and of the esteem in which the rod was held in his time for the discovery of lead ore. But as the mines became exhausted the dowsers found employment in the search for underground water. A few

<sup>1</sup> Chevreul and other writers attribute the earliest description of the rod to the alchemist who went by the name of Basil Valentine, quoting a passage from his *Letztes Testament*, in which the use of the rod (*schlag-ruthe*) in the search for ore is described, and erroneously giving a date in the 15th century for that work. The first edition of *Basilius Valentinus*. . . *His Last Will and Testament* was dated 1657, and translated from *Fratrīs B.V.* . . . *Geheime Bücher oder letztes Testament* (1645). Mr. Westlake tells me that this is abridged from *Bergwerckschatz*, etc. (1618), edited in 1600 by Elias Montanus, a physician of Brieg, in Silesia. The authorship is attributed in a concluding poem to Nicolaus Soleas, but whether—as an MS. note on the title-page of the copy in the British Museum says—“genuinus Autor hī libri fuit,” is uncertain. This earlier work describes the *schlag-ruthe* and other rods at much greater length.



FIG. 22.

From M. A. Montalbano's "*Pratica Minerale*" (Bologna, 1678).



Somerset dowzers still use the rod in the search for lead and zinc ore, and it was important to make a strict experimental test of such dowzers,—under the supervision of a competent geologist,—before they disappeared altogether. Mr. Westlake, F.G.S., kindly undertook to do this for me, and the authorities of the Friends' school at Sidcot generously placed their grounds at our disposal for this purpose. The following Report gives the result of Mr. Westlake's experiments with the different dowzers he employed. Some of these, however, were water, and not mineral dowzers. This should be borne in mind in reading the Report, for if the detection of surface indications affords an explanation of the dowser's success, as some maintain, then the results obtained with dowzers unaccustomed to search for mineral ore ought not to show greater success than chance coincidence would account for, whereas the report shows that they do.

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#### EXPERIMENTS IN MINERAL-DOWSING AT SIDCOT.

BY E. WESTLAKE, F.G.S.

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Dowsing for mineral in the Mendips has been practised, as we know from various historical notices, for the past 250 years. On visiting, in the course of an inquiry into water-dowsing in 1899, the mining villages towards the western end of the hills, I found that mining operations had ceased some eighteen or twenty years back. I interviewed one or two of the old dowzers still living, who had used their art in former times, but found it was not now possible to obtain details or verification of the circumstances.

The only course open seemed therefore to be to make fresh trials with those dowzers who professed themselves able to find mineral. Several acres of land in the neighbourhood of Winscombe belonging to a Friends' school, which, while known to be mineralised, had not been mined, offered a favourable spot for experiment. I accordingly went over the ground with four dowzers independently, viz.:—

(1) Thomas Day, of Shipham, an old mining dowser, who professes to be able to find lodes, not necessarily mineralised.

(2) Rowland Pavey, a young miller of Cheddar, who professes to find underground objects by some secret art ; (had not found mineral).

(3) Thomas Foord, a labourer of Shipham, an inexperienced dowser.

(4) William Kerslake, of Wells, a tailor and professional water-finder ; (had never searched for minerals before).

The localities tried were within a space of 300 yards east and west, and 100 yards north and south, and comprised :—

(a) An old cricket field west of the school, at a quarry in which ore had been found some twenty years since by Thomas Day.

(b) Some ground in and adjoining the girls' tennis court.



(c) A burial ground in which ore had been found in 1866, and a garden adjoining.

(d) A ploughed field a little to the north.

The soil at these places is mostly thin; the underlying rock is the Dolomitic Conglomerate at the base of the Trias, which consists of pebbles of various Palæozoic rocks, the whole weathering reddish, and having a southerly dip of a few degrees. The rock is traversed by occasional lodes and veins, nearly vertical, running east and west, and carrying the ores of lead, zinc, iron, and manganese, but they are here too thin to have been worth mining.

The points indicated by each dowser separately I marked in such a way as to be imperceptible to those who came after, and I also so arranged that they did not have an opportunity of communicating with one another. I went round with each dowser, but except in a general way I did not know where the previous marks were, and therefore sources of error from conscious or unconscious collusion may be considered excluded. With each dowser the twig turned over a space of a yard or two, corresponding probably with their ideas of the width of the lodes, and I marked the central points. Pavey indicated 5 places, Day 28, Foord 2, and Kerslake 26. The marks are numbered consecutively 1 to 61 on the accompanying plan, and the dowsers who indicated them are distinguished for brevity by an initial, *e.g.*, P3 = mark No. 3 indicated by Pavey (see plan on p. 350).

At seven places two of the dowsers approximately coincided within distances varying from  $1\frac{1}{4}$  ft. to 8 ft. Two of these, adjoining the tennis court, I found to be in made ground, and did not open up on account of the expense. Of the other five I opened, one in the field and one in the garden showed only solid rock; in the remaining three we found a well-defined vein of ore.

At three other single marks I opened up there was no sign of ore, though at one or two Mr. Clark thought there were signs of a lode.<sup>1</sup> In reckoning the dowsers' success their marks should be taken as extending a yard on either side the centre.

The results are given on the next page in tabular form.

At No. 4, though Kerslake's mark, K 56, was 4 ft. south of the vein, it was only 2 ft. from the south wall (foot wall) of the lode, and 1 ft. from the buff coloration due to the same, and was hence a practical success. Day's mark 3 ft. further south is neither a success nor altogether a failure; it may perhaps reckon as half successful. This No. 4 pair of marks I had thought before digging to be the most

<sup>1</sup> I am indebted to Mr. George Clark, a Mendip and Kimberley miner living at Winscombe, for the interest he has taken in these experiments.

promising, on account of two others, D 33 and K 58, in the Friends' burial ground on the east side of the hedge, having also coincided within 6 ft.

	No. <sup>1</sup>	Place.	Nos. of marks.	Distance between marks.	Length, width and depth of hole in feet.	Nature of rock, etc.
Five coincidental places dug at	1	Old cricket field	D 9, P 3	8 ft.	$10 \times 2 \times \frac{1}{2}$	Conglomerate with an inch vein of <i>calamine</i> ( $\text{ZnCO}_3$ ) carrying lumps of <i>galena</i> ( $\text{PbS}$ ). Vein 1 ft. north of D 9 and 6 ft. south of 3 P.
	2	„	D 11, K 44	$1\frac{1}{4}$ ft.	$6\frac{1}{2} \times 2 \times \frac{1}{2}$	Solid conglomerate.
	3	Garden	D 28, K 55	$2\frac{1}{4}$ ft.	$5 \times 2 \times 2$ to 3	Decomposed conglomerate on solid ditto.
	4	„	D 30, K 56	$2\frac{3}{4}$ ft.	$16 \times 2 \times 2\frac{1}{2}$	Conglomerate with a <i>lode</i> 9 ft. wide having a high dip to N., and carrying a half inch vein of <i>galena</i> . Vein 4 ft. north of K 56 and 7 ft. north of D 30.
	5	Burial ground	D 33, K 58	6 ft.	$7 \times 2 \times 4$	Conglomerate with a <i>lode</i> carrying an inch vein of <i>galena</i> . Vein 3 ft. north of D 33, and 9 ft. north of K 58.
Two ditto not dug at	6	Tennis court	D 17, K 50	5 ft.	—	These points are on the strike of the No. 1 vein.
	7	„	D 20, K 54	$1\frac{1}{2}$ ft.	—	
Three single marks dug at	8	Old cricket field	K 36	—	$6 \times 2 \times 1\frac{1}{2}$	Conglomerate with an E. and W. joint, which Clark thought might widen in depth and carry a lode.
	9	„	P 4	—	$6 \times 2 \times 3$	3 ft. rotten conglomerate on solid ditto. Clark said there was “every indication of a lode here,” but there were no defined walls or sign of mineral.
	10	Ploughed field	P 5	—	$6 \times 2 \times 4$ to 5	4 ft. of reddish loam on conglomerate: no mineral.

<sup>1</sup> The figures in this column correspond with the large black figures on the plan.

Subsequently to the above dowsing I was told by Frank Knight that the grave of William Tanner, who died November 8th, 1866, had yielded lead ore ; he was watching the meteors of that date from the school-house roof, and noticed that the grave-diggers stopped blasting, thinking the end of the world had come, and that graves were superfluous ; they afterwards sold the ore in Bristol for 25s. The dowser Day was aware that ore had been found in one of the graves, and after he had made his marks I asked him to point it out, but he could not identify it. He might, of course, have retained some latent memory of its whereabouts ; still as the gravestones are small, uniform in size, and flat on the ground, and hence neither legible nor distinctive 12 yds. away, I was interested to find on plotting the plan that his mark, D 33, was exactly on the straight line, 18 yds. long, joining the No. 4 vein with W. Tanner's grave.

At this point (No. 5) I opened the ground to verify the lode, and found it running just north of D 33 towards a point 2 or 3 yds. north of the grave aforesaid. Clark said it was "very promising lead-bearing ground," and, on sinking in it to a depth of 4 ft., we struck the vein. Day's mark was  $1\frac{1}{2}$  ft. south of the foot wall, and 3ft. south of the vein—another success. Kerslake's mark, K 58, was 6 ft. further south ; so that, taking the two sides of the hedge, each dowser made one decided success and one doubtful one.

Taking the success of individual dowsers, Day, the miner, was right in  $2\frac{1}{2}$  places out of 5 (= one half). As he had in former years seen the ore at the adjoining points, and knew the general run of the lodes, it may be said that his memory, conscious or subconscious, may have caused the twig to turn at the right places ; and this view is favoured by the circumstance that in the garden, which was quite shut off by the hedge from any view of the burial ground, he was 5 ft. off the lode. Still, in making this criticism, we should bear in mind that one of the uses—according to Day, the chief use—of dowsing in practical mining, is to "pick up" the "lode" or "course" of mineral at no great distance from its working face, and that this he did at Nos. 9 and 33 with absolute accuracy, and of course without the ore being in the least visible anywhere. Mr. Knight told me that ore had also been found at the school at the S.W. corner of the inner court, near D 27, but I have not reckoned this anything for Day, not knowing how far he knew the circumstances.

Kerslake, the tailor from Wells, a comparative stranger to the place, was right in  $1\frac{1}{2}$  places out of 5 (= one-third nearly) ; I may have told him that ore had been found in the burial ground, but beyond this his success was not derived from local knowledge.

Pavey had a partial success at P 3 where he was 6 ft. off the vein, but in the two other holes dug on his sole indication there was nothing



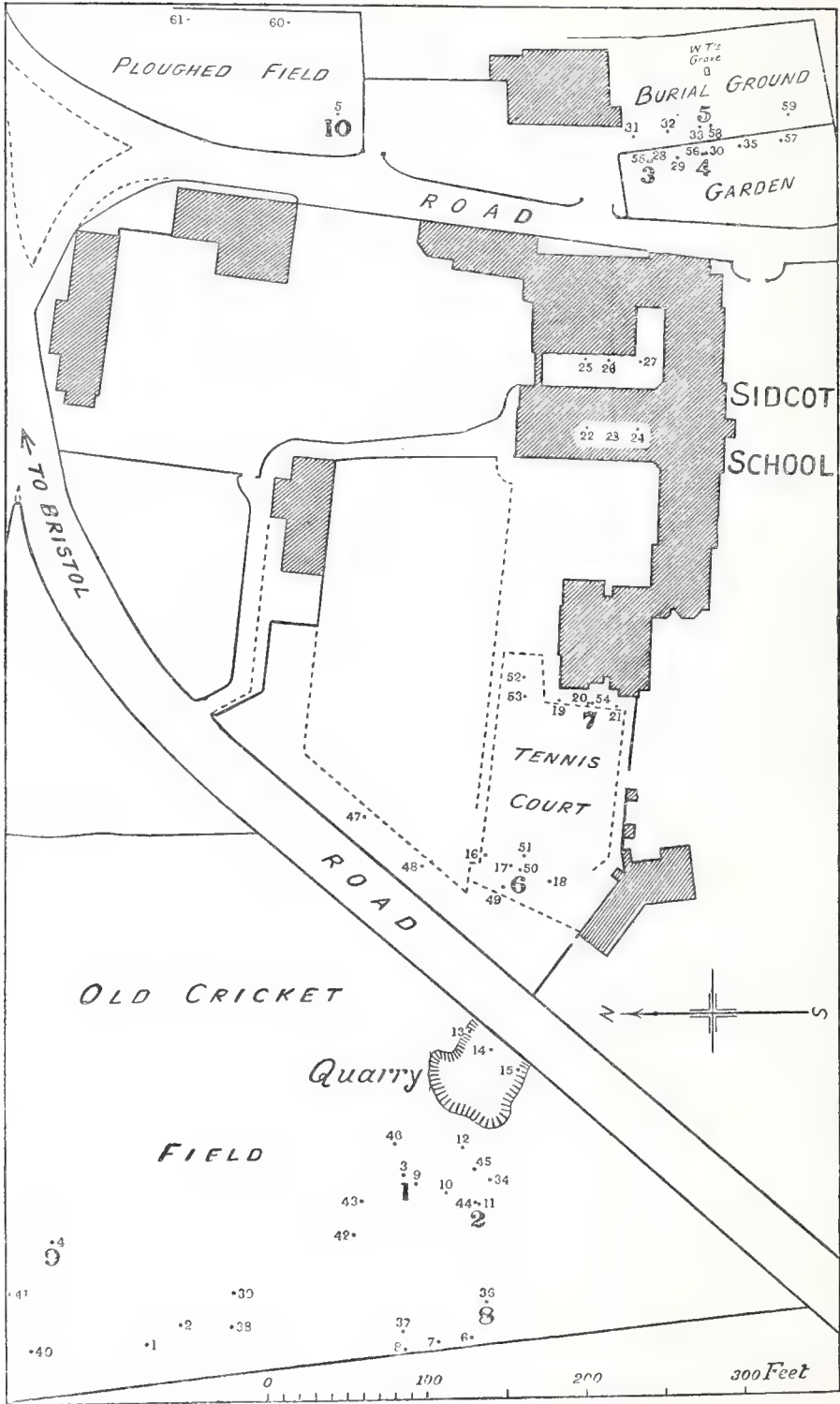


FIG. 23.

whatever, unless we reckon Clark's opinion about P 4. His proportion of success was thus one-sixth.

Among general objections which may be taken to these experiments, which I could only do quickly and extempore, are : (1) that the ground was known to be mineralised ; (2) that the miner Day, having formerly dowsed over the property, refused to waste time by trying over the northern or southern portions (he made, however, one N. and S. traverse of the cricket field without result) ; (3) that to save the time of the other dowzers and with a view to getting coincidences I took them over the same ground ; (4) that, by way of encouraging them, I mentioned to one or other that ore had been found in the quarry or burial ground, and must therefore assume it known to all. While such points make it difficult to calculate whether the success exceeds chance or local knowledge, I am inclined to think that it does.

The probable result of opening the ground at haphazard may be judged to some extent by the section exposed in the turnpike road, which lies in a cutting several feet deep for twice the distance shown on the plan, but shows a lode only at one place opposite the quarry. It is also practically certain that the discovery of the mineral lodes did not depend on any surface indications, as both in the cricket field, garden, and burial ground, there was a layer of turf or vegetable soil over and completely concealing the lode.

These experiments in their practical success and in the difficulty of proving more, are in a line with ordinary water-dowsing. They are, I think, a further illustration of the power of the automatic mind to gather and utilise information from all sources. What these sources are can only be settled by more systematic trials, which should if possible take the form of tracing *known* objects, the chances of finding which can be calculated. Such are, *e.g.*, water pipes, gas pipes, electric wires, and subways carrying water or mineral.<sup>1</sup> An old German mining-book describes the twig as used to find where to sink a shaft from the surface of the ground to some particular point in the mine. With known objects all the dowzers' predictions can be checked, which is impossible where, as in the above experiments, each point has to be opened up. Opening the ground to find unknown objects is not only expensive but inconclusive, as the dowser may always maintain that the object will be found at a greater depth.

E. W.

<sup>1</sup> Mr. Pavay is, I understand, prepared to trace underground objects of any kind ; he has succeeded, I am told by Mr. M. P. Porch, Magdalen, Oxford, in finding human and mammalian fossils in the Cheddar Caves beyond the result of chance excavation.

## APPENDIX D.

PARAMELLE.

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The story of the Abbé Paramelle and his extraordinary success as a water-finder (or *hydroscope* as he was called) is so little known in England that it is worthy of more than a passing notice. Paramelle was born in 1790, and in 1818 was appointed curé and afterwards Abbé of St. Jean L'Espinasse, moving from there to St. Céré in the Department of Lot in the south-west of France. A man of keen observation and scientific spirit, he was struck with the remarkable difference between the numerous springs, streams, and well-watered area of the eastern half of the Department and the difficulty with which water was obtained in the twenty-four arid cantons of the western half. Here the population were impoverished by having to expend a large part of their time in laboriously carrying water from distant wells to supply the wants of their families and their cattle. Prompted, he tells us, by charitable feelings, he spent two years in the fruitless search for springs, in the hope that he might be able to supply the poor folk of the arid region with the benediction of a bountiful water supply. In this he failed, and got the reputation of searching for the quantities of buried treasure which the English were supposed to have left behind when long ago they evacuated that district. He then set to work to compare the geological characteristics and rainfall in the two regions; the arid part was calcareous, but had the same rainfall. The springs, he rightly conjectured, were only due to the rainfall; where, then, did the rainfall go to in the arid region? It must penetrate the soil and accumulate, ultimately making its way underground to the river valleys. Guided by his own observations, Paramelle was gradually led to put his theories to the test, and found them verified. He then travelled further afield, and at last he tells us that after nine years of exploration and observation he felt sufficient confidence to go to what we should now call the County Council of the Department of Lot and ask them to place a certain sum at his disposal to test his theories by sinking wells in certain places where he predicted water would be found in the arid communes. The Council acceded, and in 1827 granted him a sum of 600 francs to make experiments, the communes that were benefited to contribute an equal sum. Eight places were selected by Paramelle in as many communes, but it was considered at that time so impossible to find water in these calcareous plateaux that three communes refused to incur the expense, though five who did were in each case rewarded by an abundant supply.



In the *procès verbal* of the Council of the Department of Lot for 1829 these remarkable successes are recorded. Paramelle,—now the learned savant, M. l'Abbé Paramelle,—is called in to explain his theories, and a further sum of two thousand francs placed at his disposal and a generous recognition paid to his learning and self-sacrificing devotion. Two years later the Council record that in sixteen out of seventeen localities the predictions of Paramelle had been verified, a perennial water supply being found at the place, depth, and of the volume he had indicated. His fame becoming known, applications to find water in waterless districts poured in to such an extent that with his Bishop's permission he relinquished his ecclesiastic duties and became what we should now call a hydro-geologist, what he called a “geognostic,” and the public of that day a *hydroscope*. Disclaiming the infallibility which was thrust upon him, the local newspaper accounts of the period, some of which I have read, all speak of his modesty and marvellous success. Many explain his secret as a gift of God, others as the work of the devil. The poor people, however, answered very much in the words of the blind man cured by our Lord, “Whether he be a sorcerer or a messenger of God we know not; this we know, that whereas we were perishing for want of water, now by his help we have an abundance.” By permission of the authorities, Paramelle hereafter made a fixed and modest charge of ten to forty-five francs for his services in each case, except to the poor, to whom no charge was made (I am afraid this would not satisfy our English water-finders, whether geologists or dowsers), and for twenty-one years he spent from sunrise to sunset every day, except Sundays, for nine months in every year at the work of water-finding.

In 1843 the *procès verbal* of the Council General of Lot records that in their department alone 338 wells had been dug at places indicated by Paramelle, and, of these, 305, or 90 per cent., yielded an abundant supply of potable water, in every case found at the depth he had predicted. In other parts he had also found 683 sources of water supply; and when in 1854, at the age of 64, he practically gave up his active work, the Abbé asserts that in 25 years he had located over 10,000 sources of underground water, and he estimated that between 8 and 9,000 wells had been dug at the sites he had selected. He endeavoured to find out the percentage of failures he had had, but in spite of circulars he sent out, got very few replies; he believes, however, that his failures were under 5 per cent. of his total trials. Whether 5 per cent. or 10 per cent., as the above official records indicate from his entire results, his astonishing success is, I venture to think, beyond anything I have heard modern expert geologists claim. A remarkable testimony to the value of his work I found in an old number of the

*Journal d'Agriculture Pratique* for April 1845. The *Journal* quotes a letter the Prefect of Lot wrote to the Prefect of Versailles, stating that up to that time about 6,000 new sources of water supply in 30 departments had been discovered by the Abbé Paramelle, and the actual money value of the springs thus found was, he says, estimated at not less than four to five million francs.

In 1846 the *Académie* of Reims published a report in their *Comptes Rendus* from a committee of six of their members appointed to enquire into Paramelle's theory and work. In this lengthy and careful report the committee express their high opinion of Paramelle's character and the results of his work, and quote a number of letters and certificates from various officials in different parts of France, testifying to his success. His proportion of failures is estimated as *rather less than 5 per cent.* In almost every instance quoted, the depth at which water was found was under 50 feet. As to his theory, the committee report that, though true in a sense, it is imperfect and not always applicable, nor do they consider that a knowledge of it would enable another, not possessed of Paramelle's special talents, quick recognition of all surface indications and long practice, to be equally successful. Paramelle himself declared, however, that in a few months of study and three of field practice, he could teach anyone of ordinary intelligence all that he knew. That might be, but he could not communicate his almost unerring instinct.<sup>1</sup>

Paramelle, however, unlike most prophets, seems to have had more honour in his own country than elsewhere. His success was declared by outsiders to be impossible, doubt was thrown on the reality or permanence of the springs he had found, one *savant* asserts that his procedure was unscientific and valueless, whilst a writer in one of the French journals, for 1842, says whatever success he had was due to impressions, sensations, and convulsions, and, doubtless, diabolic visions that he experienced. This writer adds: "The only difference between Paramelle and other sorcerers is that he conceals the diabolic signs he receives, glossing over his magical proceedings with a lot of scientific jargon."<sup>2</sup>

<sup>1</sup> M. Auscher, in his recent handbook of hydro-geology (*L'Art de découvrir les Sources*; Paris, 1899), states that the best known of Paramelle's pupils was M. Amy de Pannessières, who also wrote upon the discovery of underground water. But I do not know of his writings nor of any record of his success as a water-finder. M. Auscher refers repeatedly to Paramelle's work and successes, although his theory is said to have been found in part defective or erroneous. Some of Paramelle's occasional failures M. Auscher attributes to his neglect of surface indications, such as the comparatively rapid melting of snow over underground water, local mists, the character of the vegetation, etc., but of some at least of these Paramelle was aware, and it is difficult to know to what extent he did neglect or rely on them in practice.

<sup>2</sup> *L'Eclaireur du Midi*, Juillet, 1842.

To meet his critics Paramelle determined to publish the methods which had guided him. He states that in 1827 he had written a work "on the art of discovering springs." This he revised and published in 1856; three editions were quickly sold, a German translation was made by the Professor of Geology in Freiberg, a Spanish translation followed, and so highly was the work esteemed that the Spanish Ministry ordered every municipality to purchase a copy. A fourth enlarged French edition was published in 1896, after Paramelle's death. This I have read, and if the earlier ones resemble it, I do not hesitate to express my amazement at the neglect of this work in England. The wealth of geological and useful practical knowledge gained, through field observation, by this French Abbé two generations ago is astonishing. He must have been an odd mixture, for he includes in his work a number of laudatory press notices.<sup>1</sup> From these, however, we gain a description by eye-witnesses of his method of procedure; and these merit some attention. First, there is the unimpeachable evidence of one of the most distinguished French *savants*, M. Geoffroy St. Hilaire, who, in a memoir read before the Paris Academy of Sciences, in 1836, writes as follows:—"The Abbé Paramelle's skill in discovering springs rests on the science of observation, and not on the instinctive movement of the divining rod. He has acquired by practice such acuteness of observation that, after a single and rapid inspection of the surface of the ground, he can indicate the place and the depth of any underground sources that may exist. His success has been so remarkable as to convince the most incredulous." Nearly every eye-witness says the same thing. A Journal at Aix says that, "Without any hesitation, and after a rapid glance, Paramelle at once indicates not only the very spot where to sink a well, but the depth it will have to be sunk and the volume of water that will be obtained. All this in so laconic and precise a fashion that scepticism vanishes." Another journal, *Le Rhutenois*, writes, on February 15th, 1837:—"The Abbé Paramelle simply looks round, says, 'Here you will find the spring at such a depth and of such quantity; it comes in this direction, and the water will be of such and such a quality'; and so on, in upwards of a score of other Press notices which are included in the chapters XXX. and XXXI. of Paramelle's work. Similar testimony is borne in the Report of the

<sup>1</sup> It is possible that some of these laudatory notices were inserted after Paramelle's death by the editor of the fourth and last edition, the only edition I have been able to consult. This contains the preface to the third edition, published in 1865; in this preface, which Paramelle himself wrote, he states that in spite of some hostile criticism, none of his statements have been controverted, and after exercising the greatest care in revision, he has found nothing essential to withdraw. Hence, he adds, the present (third) edition is merely a reproduction of the earlier ones with some additional facts and quotations which support the conclusions he had reached.



Commission appointed by the Academy of Reims already referred to, and in a paper published in 1835 by the French *Société Centrale d'Agriculture*, LVII<sup>e</sup> Cahier, p. 326.<sup>1</sup>

The last chapter of Paramelle's book gives a very interesting summary of the various methods that had been adopted in ancient and in modern times (*i.e.*, before the present century) for the purpose of discovering underground water ; to this part of our subject we may return later.

What then was the secret of Paramelle's method ? This he has disclosed, or professed to disclose, in his book, *L'Art de Découvrir les Sources*. He did not use any form of divining-rod ; he tells us the rod would never turn in his hands, though he had often tried it, and he regarded the users of the *baguette* with undisguised contempt.

The hypothesis upon which he worked, to state it in the most general form, was that underground water behaves precisely like water that is visible on the surface of the earth. Just as in the latter case waters that are precipitated upon the surface of the earth and fail to penetrate the superficial soil, gather into rivulets and join streams and rivers, following in their course certain suitable channels, so must underground waters behave. Thus the waters which percolate through the upper earth, on reaching a bed of impermeable rock or clay, will form into little filaments of water, these will unite into rivulets, which again will join larger subterranean water courses, and so on until they finally reach the surface or possibly emerge in the bed of a river or lake. Throughout their course they will follow hollows, depressions or folds in the impermeable stratum analogous to those in which surface waters flow. The position of these underground channels, our author asserts, can be determined by observation of the surface. This branch of his subject Paramelle explains with much detail, but it will be sufficient here to give a bare outline of his views.

Subject to certain exceptions, it may be said that he held the conformation of the surface to correspond in some important features with that of an impermeable stratum beneath it. A valley or longitudinal depression, whether large or small, marks, he says, the position of an underground stream. In broad valley bottoms the principal watercourse will usually be found on the line of intersection of the sides. Subsidiary gullies or hollows indicate the affluents. In a fairly level country, where the depressions can be only slightly marked, he describes how it is still possible with minute precautions and by close observation to detect them. ¶

<sup>1</sup> For these and other rare papers relating to Paramelle I am indebted to M. Rolland, of 2, Rue des Chantiers, Paris, whose excellent book agency I have previously had to thank.

The existence of such outward signs of hidden streams of water Paramelle attributes to the natural tendency of the surface to conform to its supporting bed, and also to subsidence due to the constant carrying away of matter by the underground current.

The quantity of water likely to be found at a selected point he estimated from the area drained. The quality of the water he inferred from the nature of the soil through which it had filtered, and the depth at which it ran mainly from ordinary geologic data.

Paramelle did not claim that his method was applicable to the discovery of sheets of underground water or water-logged strata of wide area, or, one may suppose, to water at a great depth. The former case, however, he believed to be of very rare occurrence, and to others, for which his general plan would be unsuitable, he refers candidly, and suggests several ingenious considerations.<sup>1</sup>

It may be inferred, from the foregoing brief sketch of Paramelle's views, as well as from the contemporary notices of his work, that his success lay in the discovery of shallow, or what are sometimes called surface, wells, that is, wells from 10 to 30 or 40 feet deep, and his observations are evidently based on the superficial beds which lie on the older rocks. As Mr. T. V. Holmes, F.G.S., has pointed out in the interesting letter I have already published,<sup>2</sup> "This perception of water-bearing surface beds is quite independent of a knowledge of geological structure. Indeed the rise of geology as a science tended to put this older knowledge of water-bearing surface beds into the background. The Geological Survey had been at work many years at the deep-seated geology of England and Wales, before any '*drift-maps*,' or those showing these superficial beds, were even thought of. . . .

<sup>1</sup>Mr. B. St. G. Lefroy, to whom I am indebted for kindly making, at my request, the foregoing concise summary of Paramelle's working hypothesis, writes to me as follows:—"Paramelle's theory can hardly have been developed, *ex post facto*, to disarm the popular superstition which his abnormal success created, for in 1829 a local French journal (quoted on p. 382 of Paramelle's work) states that his success was based on geological theories he had arrived at, and the local newspaper reports emphasise the fact that he *viewed* the land, in some cases not going on it at all.

"The impression which he makes on me is that, granting his premises, his conclusions are drawn with admirable common-sense and candour: and that even if he be a scientific heretic, twenty-five years of observation, a naturally good eye for ground and some modicum of truth in his conjectures, might well account for his successes without invoking any supernatural faculties.

"The gist of the difference between Paramelle and orthodox geologists, I venture (with the deference of conscious ignorance) to submit lies in the question:—Is a water-bearing stratum a *sponge* or a *filter*? (I mean principally. Of course it must be both to some extent). If the latter, would not his view be correct—*ex necessitate rei*? (Water would percolate by gravity into depressions). Another way of contrasting Paramelle and hydro-geologists might be to say that the former fixed his mind on the impermeable stratum while the attention of the latter is mainly directed (I think) to the presence or absence of a water-soaked bed above it.

<sup>2</sup> *Proceedings S.P.R.*, Vol. XIII., p. 222.

But geologists seldom bestow much thought on these various drift-beds unless interested in flint implements." Perhaps I may here add parenthetically my earnest hope that good "drift-maps" of the *whole* of the British Isles will soon be published by the Geological Survey. The need for and practical value of such maps has been most strongly impressed upon me during these investigations. Both Mr. Kilroe and Mr. Holmes have pointed out to me how misleading the ordinary maps may be to a person who wishes to obtain a moderate water supply by *sinking a well*.

I am not in the least competent to give any opinion upon Paramelle's theories. They may excite a smile among geologists of the present day, or they may awaken interest or arouse criticism. In any case, his phenomenal success in the location of underground water needs explanation, even assuming that he did not discover the more abundant sources found in deep wells. For my own part, I believe that the experience he had gained by long observation in the field, and the instinct he had thus acquired, accounted for a good deal more than he could rationally explain. Hence, like the successful dowser of past and present times, it was his sub-conscious far more than his conscious life that was concerned in the process of water-finding. That no mere knowledge of the theories he sets forth in his book will enable any one to become a second Paramelle is obvious from the fact that, although his book has been widely read on the Continent during the last seventy years, no such renown has been subsequently gained by any Continental water-finder, nor, I believe, by hydro-geologists anywhere, as attached to this simple-minded French Abbé.



## APPENDIX E.

## EVIDENCE OF CLAIRVOYANCE IN DOWSERS.

It is obvious that if a good dowser has any power whatever of subconscious supersensory perception, surface indications of the object sought for will have very little to do with his success, and he ought to be able to tell the position of a hidden coin or any buried object as well as that of underground water and mineral lodes. Now it is well known that the majority of dowsers from the earliest times have always asserted that they could do this; in fact, the first use of the divining rod was for the purpose of discovering buried treasure.<sup>1</sup> It would take me too far aside from my present purpose if I entered into the history of this part of my subject and the evidence that exists on its behalf; this I propose to do in another paper. It is sufficient to say that I have collected a large amount of evidence which shows,—in spite of my strong previous belief to the contrary,—that the success of the dowser in finding concealed objects is greater than seems possible to account for by chance coincidence. Indications may in some cases have been unconsciously afforded by those present who knew where the object was hidden, but, allowing for this and other sources of error, there appears to be in these experiments a degree of success with certain good dowsers like the late John Mullins that points to the existence of some faculty analogous to clairvoyance.<sup>2</sup> With several amateur dowsers there is similar evidence. Mr. Tosswill, of Budleigh Salterton (see p. 223), made in the presence of two witnesses a series of successful experiments in finding concealed coins, every care being taken to avoid unconscious indications

<sup>1</sup> There is an amusing account in the life of William Lilly, the astrologer, of how he accompanied Davy Ramsey, "his Majesty's clockmaker," in the year 1634, to search for buried treasure by means of the divining rod in the cloisters of Westminster Abbey, the Dean having given permission, and how the demons raised a storm and drove them away. One of the epigrams of Sheppard, published in 1651, satirises this use of the rod; but Continental and, to a less extent, English, literature of that and an earlier period, contains frequent reference to the use of the rod for finding buried treasure. Even so recently as September 28th, 1882, the Paris correspondent of the London *Times* relates how the Government Director of Fine Arts in Paris had that week ordered, in his official capacity, a search for treasure (supposed to have been buried a century previously), to be made by means of the divining rod. The search was, however, fruitless. A leading article on the subject appeared in the *Times* for October 6th, 1882.

<sup>2</sup> A few cases were cited in the previous Report. See *Proceedings S.P.R.*, Vol. XIII., pp. 78, 82, 84, 85, 94, etc.

being given by the witnesses. I have specially to thank Mr. Tosswill for his kindness in assisting this inquiry, and taking a railway journey of four hours solely for the purpose of enabling these tests to be made. Mr. Young, of Llanelly (see p. 221) has made similar experiments in the presence of a previously sceptical witness. Mr. Jerman, F.R.I.B.A., a well-known architect in Exeter, who is an amateur dowser, found he also was successful on making an attempt at finding a carefully concealed coin, though one such experiment is of little value. Enough evidence, however, exists to make it worth while for those who find they have the power of motor automatism to pursue somewhat similar experiments, and I hope this will be done.

I regret that I have not made more experiments of this kind myself with different dowsers. The fact is, I was for a long time entirely sceptical of the statements made, and my own experience confirmed this scepticism. For early in this inquiry I made some experiments with a dowser, Mr. Stears, on concealed masses of metal, which were unsuccessful; these are related in the previous Report, p. 176. Again, Colonel Taylor made, at my request, a careful experiment at Cheltenham with the dowser, Mr. Tompkins, on concealed coins; these also were unsuccessful. And yet both these dowsers seem to have had singular success in this very direction on other occasions with different experimenters. It is natural to assume such success was due to careless experiment. Perhaps so. On the other hand, we do not require to assume—we *know*—how very fitful are the psychical conditions which ensure success in any experiments connected with the sub-conscious state, such, *e.g.*, as telepathy. Hence it would be an error to attach as much importance to the failure of a particularly *psychical* test experiment as we should to the failure of a corresponding *physical* experiment the conditions of which are known.<sup>1</sup>

Recently I have, however, had a certain amount of success in a more severe test to which I submitted this hypothesis of “clairvoyance.” In my correspondence and interview with Mr. J. F. Young, of Llanelly, who is a member of our Society—and to whom I have frequently referred as a successful amateur dowser—I found that he had occasionally with success tried automatic writing. I therefore asked him to try if a pencil held in his hand would write certain

<sup>1</sup> In his admirable address to Section A of the British Association last year, Professor Poynting, F.R.S., pointed out how essentially different are the results to be expected from experiments with living compared with non-living matter. He remarks: “Taking the psychical view, in the living being there is always some individuality, something different from any other living being, and full prediction in the physical sense, and by physical methods, is impossible.”

words or numbers I would enclose in a sealed envelope: he agreed. Whereupon I printed some words of three letters, placed them between a fold of thick paper, and enclosed each in an opaque envelope which was sealed with my own seal as well as otherwise securely fastened. Three such envelopes I posted to Mr. Young at Llanelly, and asked him to try if by automatic writing or by the rod he could ascertain what words of three letters I had written. In reply he said:—

On receiving your letter I gave the envelopes to my sister-in-law, who took charge of them till I was ready to make the experiment. When at leisure in the evening she gave me one of the envelopes marked (3), which I placed inside my cap, and put cap and envelope on the top of my bald head. I sat at a table, as usual, with a pencil in my hand, and made up my mind as blank as possible, patiently waiting till my hand appeared controlled to write without any volition on my part. After waiting for a little while my hand suddenly scribbled out, on an old postcard which was lying near, the enclosed, which looks like O N W, or else the last letter is E, ONE. I send the postcard to you with the scribble on it. Will you please say what the word is? I return the envelope.

I found the experiment very tiring, as if some vital force were exhausted, and will try the other envelopes another time.

J. F. YOUNG.

P.S.—Before posting this letter, I made a second experiment with another of your envelopes, the one marked (2). First I tried with the rod in my hands, my sister slowly repeated the alphabet aloud; this she did three or four times. After the first repetition the rod moved at the letters A, B and C and no others. I then tried automatic writing with my eyes shut; the enclosed came, starting from the  $\times$ —it looks like A. B. On a second attempt the same letters came somewhat clearer.

On receiving the envelopes from Mr. Young I examined them carefully; they had not been tampered with in any way. On opening No. 3, I found the word ONE, and in No. 2 the word CAB. Here is



FIG. 24.

an exact reproduction of the writing Mr. Young sent me. Holding the writing of Fig. 24 sideways the last letter may be read as an E, and in Fig. 25 there seems to be an attempt at the letter C when the pencil began to write. I myself had no idea which of numerous



words I had written was in any particular envelope. The experiment, therefore, seems a fairly good one as a preliminary test, and encourages

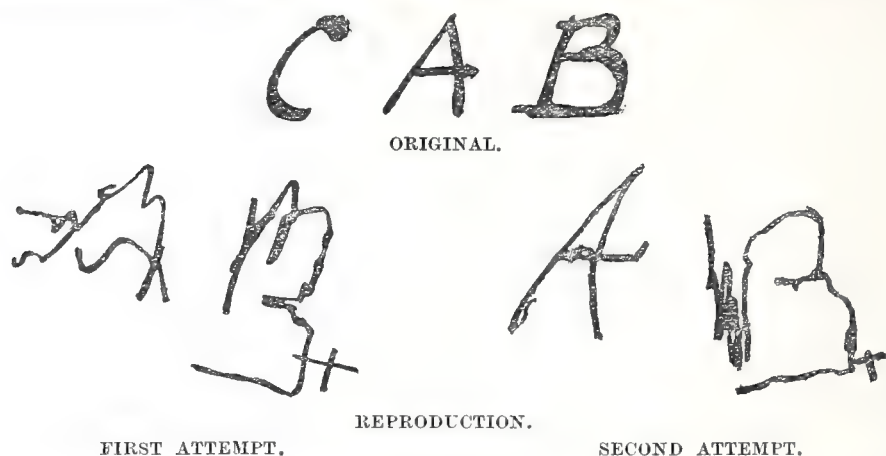


FIG. 25.

the hope that a more extended series of trials may yield results of no slight value.<sup>1</sup>

I begged Mr. Young to try the third envelope and let me know the result. He promised to do so, but unfortunately serious illness and death invaded his family, and after his own recovery he has not yet felt disposed to go on with the experiments. Prior to his experiment with my envelopes, he gave me the result of some experiments of a similar kind I had asked him to make in his own way. I have entire confidence in Mr. Young's integrity, but the results which I will give must be taken for what they are worth. Mr. Young writes on January 5th, 1900 :—

I made a few trials this evening. I cut some squares of paper all alike, put a number of one figure on each, then turned the square upside down,

<sup>1</sup> I should be very glad if any of my readers, who find that a forked twig held between their hands automatically moves when they pass over certain spots, would make some experiments to ascertain if they possess an incipient clairvoyant faculty. Let them, for example, take two packs of cards, spread out one pack (or one suit from the pack), on the table, faces up, draw from the other pack (or corresponding suit) a card, face down, and place it, unseen, say beneath the waistcoat; this humours the idea (p. 366) that the epigastrium is the most sensitive seat of this occult vision. Then walk slowly round the table on which the cards are disposed, note if the twig moves over any particular card, and compare the result with the concealed card. Or they might try the simpler card game which Mr. A. Lang has described in *Longman's Magazine*, Vol. XXXI., p. 279: viz., take up a card from the top of a shuffled pack and, without looking at it, try if some vision of a card floats into the mind; then compare the result with the actual card held. Mr. Lang finds he is generally successful in this game on the first two or three trials, but afterwards his guess becomes mixed or hopelessly wrong; this I have also noted in thought-transference experiments. I should be grateful for any record that may be sent to me (addressed Kingstown, co. Dublin) of such experiments, or of any similar ones that may suggest themselves to my readers.

shuffled them about in every way, and then picked up one of the squares with my eyes closed, put it on the top of my head and placed my cap on, it fitting close to the crown of my head. Then I made myself as passive as possible, and either slowly repeated the figures till a certain one seemed to be right, or the impression of a particular figure came as soon as I put the cap on my head; when it came thus quickly, it was invariably right. This is the result, and the order in which I took up the figures, of course one at a time.  $\times$  denotes a wrong guess :—

Figure on square	...	2		4		7		5		3		2		9		3		6		1	
Figure guessed	...	$\times$		4		7		5		$\times$		2		9		$\times$		6		1	

The experiment was very exhausting, so that I could not go on any longer.

It is curious that the figure 3 should both times be wrong. Mr. Young also tells me he was successful three times out of four with a similar experiment, only using a rod in his hands and repeating the figures to himself.

Previous to making these experiments, Mr. Young had written to me as follows :—

In the year 1893 I had a remarkable experience when out water-finding with the rod. (It happened to be very dry weather at the time. Whether this had anything to say to it I don't know, but this peculiar experience ceased when rain set in.) I found that after "setting" myself to use the rod, *i.e.*, getting into an abstracted mental condition, lost to all around, when, or just before, the rod turned, I could,—as it were clairvoyantly,—*see* the underground springs and actually appeared able to trace them out as I walked along. My friend Mr. Robertson, who, as you are aware, also uses the rod with success as an amateur water-finder, tells me he also had a similar experience,<sup>1</sup> and we have since read that a "diviner" named Adams, a Somerset man, frequently asserted the same thing.

All this may, of course, be a mere illusion, but that it is occasionally a veridical illusion seems to be indicated not only from the subsequent test experiments which I have narrated, but also from the fact that in the case of others who have had a similar experience, water has been found beneath the spot, though previously searched for in vain in the immediate neighbourhood.

Here, for example, is such a case. In the English edition of Reichenbach's researches on his so-called Od force, Dr. Ashburner devotes a lengthy note on pp. 90–106 to the divining rod, and gives a remarkable instance of a girl who when mesmerised appeared to be an excellent dowser; when entranced, if the hazel twig moved in her hands, she exclaimed she *saw* the water a few feet beneath the surface, and gave a vivid description of it. Dr. Ashburner quotes in full the

<sup>1</sup> Mr. Robertson is joint author with Mr. Young of a little book on the divining rod, in which they narrate their experience and give a good deal of miscellaneous information.—W. F. B.

letter he had received from a lady, a friend of his, giving a minute account of this experiment, which was made in a field adjoining the lady's house in Hertfordshire. At the spot where the rod turned and the girl declared she saw the water, a well was dug, and an abundant supply of good water *was found* a few feet below the surface, though previously the lady states that they had "very bad water and had long been unable to find a good spring."<sup>1</sup>

I hope that some tests may be made in England or in the Continental schools of hypnotism on the possible dowsing faculty of patients under hypnosis. Of the occasional "lucidity" or "clairvoyance" of subjects in the deeper hypnotic trance I have received some striking evidence, and have personally verified the fact in one very sensitive subject whom I had hypnotised.<sup>2</sup>

Though of very slight evidential value, it may be worth referring to the curious old case of Parangue, narrated by Figuier in Chapter VIII. of his excellent treatise on the *Baguette Divinatoire*. Parangue was born near Marseilles in 1760, and when quite a child appeared able to discern clairvoyantly the presence of underground water, and was successfully employed at several places for this purpose. Figuier is naturally somewhat sceptical about this case, for the particulars of

<sup>1</sup> This took place more than 50 years ago, and it seemed hopeless to obtain any confirmation of this case, as no names were given by Dr. Ashburner; but, curiously enough, a letter was sent me from a lady living at Waterford, who, writing to a friend *à propos* of my previous Report, gave an account of a visit she paid in 1847 to her aunt in Hertfordshire, Miss B., and narrates the very circumstance described by Dr. Ashburner as happening in her aunt's field before her visit. The writer confirms several of Dr. Ashburner's statements, that the girl dowser was Miss B.'s lady's maid, and also that Miss B. (who narrated the facts to Dr. Ashburner) was an exceptionally clever and intelligent lady, not likely to be mistaken, and that there had previously been great difficulty in getting good drinking water, but the spring found by the blindfolded [this is probably a slip of memory] girl was still used in the house. Reference is made to Miss B.'s deep interest in mesmerism, but not to any mesmerising or clairvoyance of the girl. Miss B. probably would not speak of this to her young niece. My correspondent was unaware that Dr. Ashburner had written anything on the subject.

<sup>2</sup> On a future occasion I propose to publish the details of a remarkable case of this kind sent to me some years ago by a competent observer, the rector of a large parish in Cumberland, who was also Chairman of the local Bench of Magistrates. The subject in this case was a lady, a sufferer from chronic illness, who had greatly benefited by hypnotic treatment carried out by my informant under the direction of her medical attendant. In the course of time she developed what appeared to be a clairvoyant faculty, and the particular test experiment to which she was submitted,—and which I carefully investigated and verified (so far as it was possible) at the time,—convinced me, as it had already convinced my informant and others, that sooner or later science will *have* to admit the existence, in certain cases, of a supersensory perceptive power. In this instance any explanation based on fraud, or hyperæsthesia of vision, or chance coincidence, or thought-transference was more inconceivable than the admission of some faculty new to science.



which he quotes the Abbé Sauri, who firmly believed in Parangue, and who states that the child was thrown into a paroxysm whenever it was brought over a hidden underground spring.<sup>1</sup>

Whatever value is to be attached to such narratives as the foregoing, there undoubtedly exists a body of testimony from many able and distinguished men, in different countries and in different professions, who have become convinced by personal investigation that certain persons can detect the presence of hidden objects in some transcendental manner. I have already referred on p. 247 to the opinion expressed by Zschokke. He was a man of great erudition and versatility, possessed of a wide knowledge of men and affairs, holding a high position as member of the Great Council of State, Lieut.-Governor of Basle, and historian of Switzerland. He is therefore an excellent witness. In his Autobiography, first published in 1842, he states:—

In almost every canton of Switzerland are found persons endowed with the mysterious natural gift of discovering, by a peculiar sensation, the existence of subterranean waters, metals, or fossils. I have known many of them, and often put their marvellous talent to the proof. One of them was the Abbot of the Convent of St. Urban, in the canton of Lucerne, a man of learning and science: and another a young woman, who excelled all I have ever known. I carried her and her companion with me through several districts entirely unknown to her, but with the geological formation of which and with the position of its salt and sweet waters I was quite familiar, and I never found her deceived. To detail circumstantially every experiment I made to satisfy myself on this point would take up too much space at present, but the results of the most careful observation have compelled me at length to renounce the obstinate suspicion and incredulity I at first felt on this subject, and have presented me with a new phase of nature, although one still involved in enigmatical obscurity.

The evidence on behalf of clairvoyance is really more extensive and of greater weight than is commonly known, and those who contest this evidence will find themselves impaled on the horns of a dilemma; they must either admit thought-transference and stretch that explanation to an absurd length, or they must admit the fact of clairvoyance,—meaning by this latter the transcendental perception by certain individuals of an object or writing which cannot be seen by, and is unknown to, those present. It is to be noted that cases of clairvoyance with persons *not* in the mesmeric sleep usually occur with

<sup>1</sup> In an article on *Modern Magic* by M. Shele de Vere, published in 1873, it is stated that “Catherine Beutler, of Thurgovia, Switzerland, and also Anna Maria Briegger, of the same place, were both so seriously affected by the presence of water that they fell into violent nervous excitement when they happened to cross places beneath which large quantities of water were concealed, and became perfectly exhausted.” Long ago, a Portuguese lady, one Pedegacha, is also stated to have been similarly affected by underground springs.

sufferers from catalepsy or some allied disorder.<sup>1</sup> The sense of vision in these cases (and also in some hypnotised subjects) *appears* to be transferred to the *epigastrium*; the subject pressing the card or writing to the pit of the stomach in order "to see it," as if the viscera were the seat of vision. Now it is worthy of note that this inquiry has led us to the conclusion that some dowisers also possess, in greater or less degree, a subconscious clairvoyant faculty, and that such persons exhibit symptoms of induced catalepsy and experience singular sensations in the epigastrium when the object sought for is transcendently "perceived" by them. I have already pointed out in Part XII. that the visceral sensations of the dowser are probably emotional disturbances, arising from a psychical state, and it is likely enough that a similar explanation accounts for the cataleptic subject believing he sees with his stomach, the sensation being there. But this explanation merely accounts for the secondary effects observed; the induction of the psychical state still remains a mystery. And here it seems to me that a wholly independent and different investigation should ultimately lead to conclusions so similar to those arrived at by other investigators nearly a century ago can hardly be a mere accident. A cause common to both, to be found in some perceptive power new to science, appears *primâ facie* to be indicated.

<sup>1</sup> See foot-note on p. 309.

## APPENDIX F.

## THE ZAHORIS.

In a footnote on p. 5 of my previous paper on the Divining Rod (*Proceedings S.P.R.*, Part XXXII.), I quoted a writer in the *Quarterly Review* for 1822, p. 373, who states that "the faculty of using the divining rod is evidently the same as that possessed by the Spanish Zahoris, though the latter do not employ a hazel twig." The *Spectator*, in an article on the Divining Rod on October 14th, 1882, also refers to the Zahoris in the same casual way, as if every one knew all about them. On inquiry I could find no one who could give me any information beyond a reference to the meaning of the word as given in Spanish dictionaries. Thus, in *Lopes' Spanish Dictionary* the word "Zahori" is explained as follows:—"A vulgar impostor pretending to see things, although hidden in the bowels of the earth, if not covered with blue cloth." Again, "Zahoria" is said to be "The art of seeing as above. The performer must be born on Good Friday." *Neuman and Barretti*, in their Spanish dictionary, use identically the same words, and the *Dictionary of the Royal Spanish Academy* has a similar description, ending, "Lynceus homo subterranea videns."

The word "Zahori" is really from the Arabic, meaning "clear," "enlightened"; it was, in fact, equivalent to the term a "clairvoyant," as that word is now used. The same root occurs in Hebrew, and is the origin of the title "Zohar," the famous bible of the Kabbalists.<sup>1</sup>

It seemed, therefore, to be a matter of historical interest to ascertain what was known concerning these Zahoris. The earliest account I have been able to find of the Zahoris comes to us from Mexico in the year 1557. It is contained in a folio volume in the British Museum, entitled *Phisica Speculatio*, etc., *Mexici, 1557*,<sup>2</sup> written by one *Alphonsus (Gutierrez) à Vera Cruce*. This work was reprinted in Salamanca in 1559. In his discourse on the Soul,—

<sup>1</sup> The Zohar is a mystical commentary on the Pentateuch, the title being suggested by the command in Gen. i, 4, "Let there be light," with the exposition of which the work begins; it was compiled between the 3rd and 13th centuries. The "Z harites" were an ephemeral Jewish sect of theosophists, swearing by the Zohar; founded about 1750 they had, of course, nothing to do with the Zahoris of two centuries before.

<sup>2</sup> I have to thank Mr. E. Westlake,—who has devoted unwearied patience to the compilation of a complete bibliography of the divining rod,—for the discovery of this rare volume in the British Museum. It must have been one of the earliest books ever printed in Mexico. To Miss E. Stokes my best thanks are also due for the excellent translation of this and other passages in the British Museum.



*De Anima*, lib. ii., *speculatio* ii., pp. 300–301 (or pp. 376–377 of the Salamanca edition), Alphonsus writes as follows :—

*Speculation II. of Enchanters, Salutators,<sup>1</sup> and of the People vulgarly called Zahoris.*

There is no one who denies that there may be such an arrangement in a man's eyes, that he may be enabled to see at a great distance to which another man's eyes cannot reach. In the same way there may be such an arrangement as to enable a man to see an object through some diaphanous medium, whilst other eyes are incapable of discerning the shape of the object through the same medium. But I cannot conceive that any one's sight can be so constructed that he can see an object behind a wall or any opaque substance ; for sight is the effect either of outward transmission, [*i.e.* from the eyes] as some maintain, or of inward reception, as the majority declare. It is not clear how the shape of anything can reach the eye, when it is hidden in the earth, and behind something opaque and not diaphanous ; for how would it pierce through the earth or penetrate the stone ? For such shape would necessarily be absorbed on coming in contact with an opaque body, and could not find transit, because it is not a diaphanous body. Wherefore such shape could not reach the eye, neither could the object itself be seen. And I do not see how they [the Zahoris] can perceive (as they say) abscesses or humours in the internal organs of a man, (their sight not being impeded by the body or clothes) ; nevertheless they themselves assert that they can, and in the same way, on entering a temple where dead bodies lie, that they can clearly perceive the internal organs of the corpses, if they cast their eyes down. Nevertheless I do not give full credence to their assertions. For these things may be illusions wrought of the devil. Nevertheless I do not condemn them ; for I have known men otherwise upright, who have declared they see these things. I confess I do not know what cause can produce such natural power ; for it is not the same as that alleged by enchanters and salutators ; but is quite different, for the Zahoris say the object is made visible by natural means, by radiating its shape to the eye ; if so, it is necessary that the medium through which it passes should be diaphanous ; otherwise the shape does not become visible. All philosophers say that air and water are diaphanous mediums, and all things that partake of their nature ; but the earth and opaque bodies are not of that kind.

A few years later a famous Jesuit of Louvain, Martin Delrio, published his great work *Disquisitionum Magicarum* in three folio volumes. I came across a copy of this work, published in 1599, in the famous library founded by Archbishop Marsh in Dublin.<sup>2</sup> In the first volume, ch. iii., pp. 11 and 12, of this work, Delrio refers to

<sup>1</sup> The word *salutator* means in low Latin a “ pointer out.” The index finger was called *digitus saluatorius*, not only from its being used for familiar salutation, but also for pointing out.—W. F. B.

<sup>2</sup> Mr. Elworthy, in his treatise on “ The Evil Eye,” refers to Delrio's work as published in 1603. This must be a later edition. My own slight literary researches in connection with the history of the *Virgula Divina* have impressed me with the enormous labour and erudition displayed in Mr. Elworthy's classical work.

the Zahoris (he calls them *Zahuris*), and the following translation gives the principal statements he makes :—

A race of men in Spain are known who are called *Zahuris*; we may name them Lynxes; when I was staying at Madrid in the year 1575, such a boy was to be seen there. They relate that these people see things that are hidden in the inward bowels of the earth, veins of water and treasures of metals, and corpses placed within sarcophagi. This thing is most fully received and well known. Not only Pindar, Tzetzes, and other poets have thought this possible, but philosophers also (*e.g.*, Cœl. Rhodig., lib. 16), some of whom ascribe this power to a melancholy humour.<sup>1</sup> . . . I think they know veins of water by the vapours exhaled from those places morning and evening. They know veins of metal from the grass, of whatever kind, that usually grows there. Treasures and corpses I would consider to be indicated by demons. . . . They are accustomed to restrict this faculty of seeing to certain days, the third and sixth day of the week, which is a token of a secret pact. Besides, the redness of the eyes, which is particularly to be observed in *Zahuris*, would rather injure than assist clearness of sight.

It is interesting to note the prevalent idea of that period, and long afterwards, that demons have the principal hand in the discovery of hidden treasure, otherwise Delrio's conjectures indicate shrewd observation on his part. The "redness of the eyes" is a curious trait.

Another reference to the *Zahoris* is to be found in a work published in 1643 by Juan Eusebio Nieremberg, entitled *Curiosa y Oculta Filosofia*, 3rd edition; Madrid, 1643. (Reprinted in Barcelona, 1645, see pp. 81-82.) Chapter LXXVI. of this work is devoted to the power of the *Zahoris*, and the following is a translation of the chief portion (pp. 284-285) :—

Less is heard of the sight of the modern *Zahoris*, though it cannot be taken as certain. Celio Rodiginio favours their cause, judging that there may be a natural means of seeing through large, opaque bodies. Another attributes what they say they see to the effect of a melancholy humour. This would be a good explanation if facts did not follow their imagination or sight; but springs are found where they point them out, metal where they say they see it, and the dead with all their marks which they perceive, so that it must be attributed to some other cause than melancholy.

But knowledge of where water is to be found underground may be acquired without much difficulty, without its being necessary for the sight to pierce the earth. It was the ancient office of the *Aquilegos*,<sup>2</sup> whose art was the knowledge of where water was to be found, at what depth, and of what quality it was. Marcellus wrote of these things among the Romans, and is quoted by Cassiodorus. In the time of the king Theodoric a

<sup>1</sup> Melancholy here means madness; as Milton says, "Moonstruck melancholy, moping madness."—W.F.B.

<sup>2</sup> *Aquilegus* (in Latin) was one skilled in seeking out water; see Facciolati's *Latin Dictionary*.—W. F. B.

very celebrated Aquilegus came from Africa, whom Theodoric affectionately recommended to Apronianus that he might give him a suitable salary. The mode of discovering the water was by the presence of certain herbs, briars, reeds, and other green trees, by certain species of mosquitoes, and light vapours which arose from the spot, and by other means which they took, such as placing dry wool in certain parts at night, well covered, to see if it would be found damp. Sweet and salutary waters are qualified by the south and east wind, and the heavy and less salutary fall with the north and west wind. The same thing may be urged concerning the knowledge of mines of metal, for there may be in the same way natural signs of them, and certain herbs which signify their presence.

As to the assertion that the Zahoris can see the buried dead and certain particular marks upon them, Alphonso de Vera-cruz, in the second book of his *Anima*, and Father Delrio, on the *Medea of Seneca* and in the first book of his *Magia*, attribute this to evil arts; and I can see no grounds for opposing them, the more so that the power of the Zahoris is limited to certain days, such as Tuesdays and Fridays, which to me is a suspicious circumstance; also the redness and inflammation of the eyes usually found among these people seems to prove that they rather blind their sight than sharpen it.

The most interesting part in this quotation is Nieremberg's reference to an ancient cult of water-finders, or "aquilegos," as he calls them. In the State papers and letters of Cassiodorus, which form our chief knowledge of Theodoric, king of the Ostrogoths (circa A.D. 454-526), whose chief minister Cassiodorus was, the reference to the *aquilegus* will be found in Epist. LIII., lib. 3, p. 58. The famous Jesuit, Father Kircher, in his *Mund. Sub.* lib. 5, p. 266, also refers to the method employed by the *aquilex* (as he terms it) in water-finding.

Another Spanish writer, Feyjoo, in his *Theatro Critico Universal*, Madrid, 1732, tome III., pp. 101-118, published a paper entitled *Vara Divinatoria, y Zahories*.<sup>1</sup> This paper is principally concerned with the discussion of the well-known case of Jacques Aymar, whose story was then attracting universal attention. In section VII. the Zahories are referred to, and the author treats them with scepticism, remarking that the multitude is generally credulous, and among all people men of critical faculty and sound judgment are few. He continues:—

The name Zahoris is applied to a class of men of whom it is said that their vision penetrates opaque bodies, thus causing to appear whatsoever may be hidden away fathoms deep below the surface of the earth. Perhaps

<sup>1</sup> My friend and colleague, Professor J. P. O'Reilly, Foreign Secretary to the Royal Irish Academy, most kindly translated the whole of this lengthy Spanish paper for me, and I wish here to express my great indebtedness to my learned friend for this and other laborious translations he has cheerfully made for the purpose of my inquiry.



we have inherited them from the Moors, since the word *Zahori* seems Arabic.<sup>1</sup>

It cannot be advanced that this virtue is either natural or supernatural; consequently it must be condemned as either feigned or as superstitious. It is not *natural*, since light does not penetrate into the depth of opaque bodies. In consideration of this we have declared (in the 2nd Vol., Second Discourse) as fabulous what is pretended of the penetrating vision of the lynx, and at present we will include under the same rule that son of Aphareus, King of the Messenians, to whom many ancient authors attribute the same excellence of sight as that ascribed to the lynx, giving him in consequence the title of "Lynx-eyed" (*Linceo*), because, said they, he could see through the trunks of trees and rocks, a falsehood which Apollonius, in his poem of the Argonauts, immensely exaggerates, pretending that he fathomed with his vision the depths of the earth, even to being able to see what was taking place in Hell. Nor do I think that more credit should be accorded to what Varro, Valerius Maximus, and other writers tell of that man called Strabo, who in the first Punic war saw from the promontory of Lilybœum, in Sicily, and counted the vessels which were leaving the port of Carthage, the distance being 130 miles. Even were the atmosphere perfectly diaphanous, which it is not, there would still be the difficulty that the vessels situated at a distance of 130 miles would form in the centre of the retina an angle so extremely acute that the image would be insensible, as those know who are versed in optics.

As little can it be alleged that the talent or power of the Zahoris is *supernatural*. In the first place, it is not credible that it has God as its special author, since it is a virtue whose only use is to serve greed. It is not announced that the Zahoris disinter treasures to furnish assistance to the poor, or to make war on infidels. In the second place, because, neither in the sacred writings nor in ecclesiastical history do we read that God has granted this virtue as a permanent habit to any of His many illustrious servants. How then can it be believed that, while refusing it to all these His most intimate friends, He has reserved it for men in no way remarkable by their merits? In the third place, supernatural graces are not limited to any particular nation, and of Zahoris, they are said to exist only in Spain.

<sup>1</sup> In the past century (says the Marquis de St. Aubin, tome 3, lib. 4, cap. 2) it was declared that there were in Spain certain men who saw what was under ground to a depth of 20 pike handles (*picas*). Many philosophers failed not to discover (as they thought) reasons for persuading people that this might happen quite naturally. He then states that the *Mercure français* of the year 1728, published an account of a Portuguese lady (named Pedegascha). She declared she saw what was in the earth to a depth of thirty to forty fathoms, but as regards the human body, she could not see into it if clothed, the clothing preventing her vision. But the body being uncovered, she was able to observe all the interior parts, even the abscesses or any other defect there might be, as well in the humours or soft parts as in the solid. It may be that this fable had its origin not in Portugal, but in France. But this author does not give credence to the existence of the Zahoris, justifying himself mainly for his refusal of assent to my testimony, since having cited my work, he terminates thus: "The testimony of this Benedictine, being that of a Spaniard, is of great weight to give assurance of the falsity of this opinion." [Feyjoo was a Spanish Benedictine.]

Among the vulgar there is the belief that God dispenses this grace only to those who are born on a Good Friday, without considering that there should be an infinity of Zahoris, since many there are who are born on that day. Others limit the power to the circumstance of being born at the particular moment at which the Passion is being chanted. Even with this restriction it would follow that there should be in the whole extent of Spain from 700 to 800 Zahoris, since this total, more or less, results from the supposition that about the same number of men are born every day and hour of the year, and that Spain (including Majorca and excluding Portugal) possesses seven and a-half millions of persons, which is the population determined by Señor Don Geronymo de Urtariz in his excellent work, *Theory and Practice of Commerce and of Sailing*. Consequently, on this computation there would not be a province of Spain which had not four or five dozens of Zahoris. Where are they, that we may see them? Nor can it be said that those who pretend to this grace hide it, since God does not concede virtues that they may be of no use.

There remains then but to say that this virtue is *superstitious*, and that those who exercise it have a compact, either expressed or implied, with the devil. In truth, the work of extracting gold from the depths of the earth is more of a nature to be attributed to diabolical influence than to the Divine assistance, since an abundance of that precious metal rather promotes vice than favours virtue. Such, indeed, appears to have been the thought of the ancients when they pretended that Pluto, the infernal divinity, was the first discoverer of mines of gold and silver.

The author then goes on to say that if the Zahoris really do exist, they are either wizards or rogues, and he prefers to think the latter, as the former would involve a diabolical compact, a far greater crime than being a mere rogue.

I have quoted this extract at some length, as it gives an interesting picture of the habits of thought of a learned Spaniard 120 years ago. It is evident the writer had no personal knowledge of the Zahoris, his information about them appears only to be derived from traditional stories coming from a preceding century. During the present century I have only come across casual references to the Zahoris, and this race of pretended seers, some of whom possibly may have had supernormal vision, has long died out, even the very meaning of the word being known to comparatively few.

In concluding this note I wish specially to acknowledge my indebtedness to the wide learning and scholarship of my friend the Rev. Maxwell Close M.A., for much kind assistance both here and elsewhere in this research.

## APPENDIX G.

## SCIENTIFIC AND LITERARY OPINION.

Before closing the present Report it may be instructive to refer to some of the scientific and literary reviews of the *previous* Report. Before doing so I will quote, by permission, the following interesting letters sent to me by two distinguished men of science.

I will first quote a letter from one of the highest authorities on geology, Sir Archibald Geikie, D.C.L., F.R.S., etc., to whom my hearty thanks are due for writing at such length :—

28, Jermyn Street, London, S.W., *November 8th, 1897.*

On my return last week from a prolonged tour in Eastern Europe I found among the papers and books awaiting me your Monograph on the Divining Rod, and I spent most of yesterday in reading it. First let me thank you for sending me a copy and next congratulate you on the publication of so fair, temperate and interesting a statement of the whole case.

As you may believe, my experience has chiefly been among the failures of the dowzers, and I have necessarily been led to form a rather low estimate of these men and their pretensions. But your narrative leads me to recognise that I have never seen the other side, and that I may have done them injustice. It seems to me that a large proportion of their successes are such as any intelligent man, giving himself up to the observation of water supply, might easily accomplish without anything more than mother wit to guide him. It is most difficult to get accurate information regarding their failures, and the true proportion these bear to their successes. The evidence of the men themselves in this matter is not wholly satisfactory, nor is that of their employers who have found water.

In all such matters I try to keep my mind open. There are so many mysteries in nature that one learns every day how foolish is the attitude of those men of science who dogmatically assert that such and such phenomena are “bosh,” “rot,” “deception,” “fraud,” and all the other choice terms they have so freely at command. I have been accustomed to look at the dowzers as either self-deceived or consciously deceiving others. And even after perusing your most interesting volume I am not prepared at once to abandon this attitude. But you have brought forward such a body of evidence that I must hold my judgment in suspense for a time. If I can be satisfied that there is any instinct, faculty, or whatever you choose to call it, whereby a man can detect the presence of underground running water, I shall never say another word against dowsing, though I shall still think some of the dowzers little better than rogues.

If there is such an instinct, why should it not show itself as unfailingly in a young and inexperienced hand as in an old and hardened sinner? You attribute some of the failures to the inexperience of the performers. But if



the effects are due to some direct reaction of the water below upon the operator above, and not to any observation or reasoning on his part, it may be open to question whether the faculty or instinct requires experience for its development.

ARCH. GEIKIE.

Two criticisms are contained in the foregoing interesting letter : (1) Sir Archibald Geikie suggests that a large proportion of the successes of the dowser are such as mother wit would account for. The evidence adduced in the present Report enables us to judge whether this is so, and the question has been fully discussed. (2) The other criticism is that contained in the last paragraph of the letter. I do not for one moment believe there is any "direct reaction of the water below upon the operator above"; neither a direct nor an indirect action of a physical nature is at all probable. If, however, the dowser is able to detect the presence of underground water by any sub-conscious process, or even supernormal instinct, it is conceivable that experience and practice may consciously or subconsciously be of some assistance, and I am disposed to think they are. Sir Archibald Geikie refers to his experience of the failures of dowsers. I wish particulars of these had been sent to me, as sometimes they rest on hearsay. A great failure of Mullins was reported at Carlow and referred to by my geological friends. Here is the result of an investigation of it on the spot which I asked an able geologist to make :—

Geological Survey of Ireland, Dublin, *July 16th, 1900.*

I have made inquiries about Mullins at Clonmel, as promised, and found, to my disgust, that the people there made the poor man drunk and turned his performances into a laugh !

J. R. KILROE.

I will here add the letter of another distinguished naturalist, Mr. A. R. Wallace, LL.D., F.R.S., who writes as follows :—

Parkstone, Dorset, *September 12th, 1897.*

I read your excellent and very thorough paper with the *greatest pleasure*, and noted a few points of slight disagreement on which I determined to write to you.

I have long been convinced of the reality of the power of the "dowsers." Your *résumé* shows how impossible it is to convince men by any amount of evidence till the time is ripe for them to receive it. The large amount of the evidence is even a disadvantage. Few will read and *weigh* the mass of evidence you have collected, and I have no doubt many of the sceptics will accuse you of believing anything you are told.

Now for my criticism. At p. 239 you say there are two points *adverse* to the dowser—one being their "absurd" idea of the general distribution of underground water—either as springs on definite spots or as narrow

underground rivers. But if they really and as a rule believed the former—of which I can find no evidence in your Report—it would, as it seems to me, *not* be adverse to their possession of some exceptional faculty, but in their favour.<sup>1</sup> For if they are totally ignorant of the real laws of water-distribution, the chief objection, that they work by acute observation and knowledge, falls to the ground. And as to the second “absurd” supposition—of water being often in narrow underground streams or veins—if your evidence proves anything, it proves that it is often a *fact*, and how can believing in a *fact* of nature be “absurd”? Again, the whole of the dowsters’ business would be gone if the geologists’ theory of saturated strata or surfaces was always or generally true. For then, as is the case over considerable areas, *any one* could get water by sinking to the necessary depths to reach the water-bearing stratum. Consequently, in such districts dowsters are never required. But where the reverse is the case—and your neighbour may have a good well at 30 ft. while you sink 60 ft. in vain—in such districts alone the dowser is employed; hence, *to him*, all the available evidence shows underground water to be strictly limited. If he professes to know that this is the *universal* mode of distribution, of course he would be “absurdly” ignorant, but I am not aware that he ever says so; whereas the geologists *do say*, or *imply*, that the reverse is so generally the case that the dowser can do nothing except by guess, etc., etc. ! If either of the two is “absurd,” it is *not* the *dowser*.

Some geologists are sublime in their inconsistency. One geologic critic says “any cottager could have given the same advice” as the dowser, —as if in a district where “every cottager” knew where to obtain water, any man would be fool enough to get either a dowser or a geologist to find it !

Mr. Wallace then discusses the question of unconscious muscular action as the cause of the motion of the rod, but this part of his letter I have quoted in Part XI. when dealing with that subject. As before remarked, whatever Mr. Wallace writes is worthy of most careful consideration, for his opinions generally prove to be right in the long run, his observation being keener, and his range of knowledge wider, than most of his critics.

In connection with the question of the influence of evidence on belief referred to in the second sentence of Dr. A. R. Wallace’s letter, it may not be out of place to quote here the opinion of one of the

<sup>1</sup> I stated on the page referred to by Mr. Wallace that according to geological opinion “underground water usually exists in wide saturated areas” and this opinion was discussed more fully in Appendix B to the previous Report. At the same time I ventured to say that the evidence cited in that Report pointed to the frequent occurrence, underground, of permeable *channels* as well as permeable *areas*, and hence that geologists had not sufficiently recognised the truth of the dowser’s point of view. As a matter of fact, the dowser’s most successful achievements are just in those districts where geologists most often fail, viz., in regions where underground water *does exist* in narrow fissures or channels. This is shown in Mr. Westlake’s useful Appendix, p. 340.—W. F. B.

earliest and best friends of the S.P.R., Mr. C. C. Massey, who writes to me as follows :—

I am more than ever convinced that it is not so much *evidence* as a *disposition to credit it* that is wanted. Is there such a thing as a purely objective standpoint towards evidence? I doubt it, as also I doubt that any evidence whatever can avail to overcome an adverse subjective standpoint. I think the true function of evidence is quite misconceived when evidence is supposed to be that which *generates* belief.

C. C. MASSEY.

There is much truth in what Mr. Massey says; whether it be due to the *inertia* of our mental processes, which tends to preserve habits of thought in the straight line along which they have been started, or whatever be the cause, there is a real difficulty in believing in anything unfamiliar to us, and it requires a genuine, and often painful mental effort to overcome our tendency to keep on the old lines of thought. Hence it is that the flimsiest evidence, if it appeals to our preconceived ideas, is usually accepted without question; whilst, by the majority of people, the weightiest evidence is rejected, if it requires a dislocation of our familiar ideas. This is amusingly illustrated by a story recently told me of an old countrywoman, who firmly believed her sailor son, when he mentioned Pharaoh's chariot-wheels as lying on the shores of the Red Sea, but was insulted by his silly stories when he told her that he had seen *flying* fish!

On page 221 of my previous paper I refer to the then President of the Geologists' Association of London, Mr. T. V. Holmes, F.G.S., as having recently read a paper on the divining rod before the Anthropological Institute. That paper has since been published in the *Journal* of the Institute, and I have to thank Mr. Holmes not only for the kind reference to my work in his paper, but also for much valuable help which he has from time to time freely given to me, and for his suggestive criticisms from the point of view of a field geologist. And here let me say that in the *practical* examination of this question the co-operation of a geologist is necessary. With the exception of Mr. Westlake, F.G.S., whose valuable aid I have already acknowledged, the difficulty has been, just as in other departments of the work of this Society, to induce those to give us their help whose scientific training in a particular direction renders them specially qualified to conduct an investigation such as the present. If, however, I may judge from the letters that have reached me from some eminent geologists, the evidence given in my previous paper renders such co-operation less improbable in the future.

Turning now to the scientific Press, the leading organ of British science, *Nature*, had a lengthy review of the Report, a review which



could hardly be described as written in a judicial spirit.<sup>1</sup> Nevertheless I am grateful to the reviewer for his kindly personal references, and for drawing my attention to a weak piece of evidence that was inserted in the Report, which I will deal with first.

On p. 130 of the Report particulars are given of what appears to be a good case on behalf of the dowser at Wootton, in the Isle of Wight. The report in the *Morning Post* was confirmed by the dowser, and also by the owner of the estate, a clergyman, who, in reply to my inquiries, stated that "it is quite true that he [the dowser] discovered an abundant supply of water where efforts had previously been made in vain." The *Nature* reviewer, speaking from personal knowledge of the place, challenges this, and says that three useless wells were sunk by the dowser's advice, and that it did not need a diviner to discover water where it was eventually found. I asked Mr. Westlake, F.G.S., who has made a special study of hydro-geology, to visit the place and report to me: he kindly did so, and from his report, contained in Appendix A, p. 328, there seems little doubt that this case affords no evidence for or against the dowser. It appears, however, that the *Nature* reviewer is inaccurate in one respect. After visiting the spot in November, 1897, and making careful inquiries from residents in the neighbourhood, Mr. Westlake writes to me as follows:—

I found that local opinion did not coincide with that of the reviewer in *Nature*. My informants, Messrs. Cole, Turner, Newbury and Barton, said, after reading the review, they knew nothing about the three useless wells which, according to the reviewer, had been sunk at the diviner's instance in the waterless Oligocene clays. It was true that wells had been sunk in these clays near the railway station, and one at Woodside, without success, but they had not been located by diviners. It was the local people without the rod who made these mistakes, and as they were jealous of the diviner, a stranger "from the mainland," I think I should have heard of any mistake he made. What the diviner did was to find water where everybody—*after* the event—knew it was. The reviewer says it "did not need a diviner to discover" it—meaning, *should* not have needed—with which last I quite agree, and regard the Wootton experiments as no evidence one way or the other.

Curiously enough, not far from the place singled out by the geological reviewer in *Nature* is a case where the geologist failed to find water and the dowser succeeded. This has been given in detail in Part III., p. 164.

To return, the "principal criticism," remarks the *Nature* reviewer, "we have to make on Professor Barrett's collection of facts is, that he does not give enough weight to the natural tendency of mankind

<sup>1</sup> *Nature*, October 14th, 1897.

to conceal their failures." I was well aware of the difficulty of arriving at a just estimate of the number of failures on the part of the dowser, but every effort was made to ascertain them, and this point is specially referred to at the outset of the previous Report on p. 4, again on p. 63, and again on pp. 238 and 239. As I wish to emphasise this aspect, I will repeat what was then said:—

It must be borne in mind that (especially among amateur dowsers) one is more likely to hear of success than failure, and therefore an extensive and searching inquiry is necessary before any safe induction can be drawn (p. 239) . . . All that was possible in the present investigation was to make the range of evidence as wide and unbiassed as possible, and not exclude a single case of failure that was substantiated. This has been done (p. 238). (The pages refer to the previous Report.)

It must not be forgotten that, even with skilled geologists, failures are by no means unknown in their predictions of water supply, though I am not aware of any treatise on hydro-geology that thinks it essential to arrive at and set forth the percentage of failures which different geologists have had. Because some people have had a costly experience of the vanity of human opinion, even of expert geological opinion, that is no reason for rejecting the indispensable help which expert geologists can give in selecting a suitable site for the boring of an artesian well. It is true they sometimes lead to an outlay of £19,600 to no very obvious purpose, but then what the town loses in water and rates, geology gains in stratigraphical knowledge. To some of these scientific gains, at the cost of the ratepayers, I have referred on p. 235 of the previous Report. In fact, one of the best English hydro-geologists, Mr. De Rance, F.G.S., states in a letter to me, which I quoted on p. 234, that the predictions (as regards water sources) of even leading geologists, who are not specialists, "rest upon chance," and he would not be surprised to hear their failures were as numerous as the dowsers'! If both rest upon chance, that certainly would be the case, but if either the dowsers' or the geologists' failures are notably fewer than chance would suggest, then the predictions of one or the other do *not* rest upon chance. Obviously, the question of the dowser's *success* is one thing; *how* he arrives at it quite another, and the latter is a matter of insignificance to any one who merely wants to sink a well. A scientific inquiry, such as I have attempted, necessarily embraces *both* these questions.

Following upon this review is a letter from Mr. Wadsworth, of the Michigan College of Mines,<sup>1</sup> a letter which was largely quoted by the

<sup>1</sup> *Nature*, January 6th, 1898.

English press as exploding the whole of the dowser's claims.<sup>1</sup> I have already referred to and quoted from this letter on a preceding page, p. 288, so that I need only say that Mr. Wadsworth's theory is a little belated, apart from its failing altogether to account for the correspondence of the movement of the rod with the presence of underground water.

I will now briefly refer to some of the comments on the Report that have appeared in the ordinary newspaper press. A large collection of press notices, reviews and articles based on that report have reached me<sup>2</sup>; but as might be expected, instructive criticism on any of the subjects which occupy the attention of our Society is rarely to be found in the columns of the ordinary daily or weekly newspaper press, with the honourable exception of one or two papers such as the *Spectator*, and from erudite critics such as Mr. A. Lang, to whom I am also indebted for the excellent outline of the Report given in *Longman's Magazine* for December, 1897.

A passing reference must also be made to an able and interesting review of the Report that occupied several columns of that influential French newspaper, the *Journal des Débats*. The review was signed, and was from the pen of a naturalist and critic, M. H. de Varigny, to whom my hearty thanks are due for his careful study and summary of my previous paper, and also for some useful information he has subsequently sent to me.

A well-known Cardiff newspaper devoted a leading article to a concise and fair outline of the Report, which led to a vigorous and protracted correspondence in its columns, between a worthy self-taught local geologist and our friend the Rev. A. T. Fryer, whose kind assistance I have already acknowledged, and to whom my thanks are due for his able and temperate letters in the *Western Mail*. I am also indebted to the editor of the *Richmond Times* for information he has kindly sent me, and for devoting so large a space in his journal to an outline of my Report and its discussion.

<sup>1</sup> Mr. Wadsworth's theory was anticipated by his own countryman, Mr. Ralph Emerson, in 1826. As mentioned in my previous paper, this is not Ralph Waldo Emerson, but an able contemporary of his who entered the ministry a little earlier than his famous namesake; it was some time before I discovered there were two contemporary Ralph Emersons, probably fellow-students.

<sup>2</sup> It is only right that I should here acknowledge the care and skill with which Durrant's Press Cutting Agency have supplied me, not only with these press notices, but also with many hundreds of paragraphs and articles on the subject of the divining rod and the search for underground water in general. All of these have been read and a few have led to useful correspondence, but, as a rule, these newspaper extracts are wearisome and worthless.



Several columns of a weekly journal, the *New Age*, were also devoted to a friendly discussion of the report and on the S.P.R. in general. The article is signed, and the writer, Mr. D. Balsillie, like so many others, assumes that a plausible explanation of any obscure phenomenon is probably the correct one, forgetting that any experimenter worthy of the name is sure to have exhausted all obvious explanations before he ventures upon the publication of any experiments which he believes point to some fresh addition to our knowledge. Mr. Balsillie takes it for granted, from Hansen and Lehmann's experiments, that nasal whispering "explains" thought-transference, and says, almost in Dr. Scripture's<sup>1</sup> words:—

Thus the mystery of thought-transference without contact has been cleared up in a way which any one can verify for himself who has access to a psychological laboratory.

This has been so conclusively answered by Professor Sidgwick and Professor James that I will not dwell upon it; but Mr. Balsillie draws from it the inference that, as the dowser only detects running, and not stagnant, water, he probably derives his information from some sub-conscious perception of sound, and hence he adds:—

It might be worth Professor Barrett's while to give some heed to what we have said above about the psychological explanation of thought-transference.

The possible exaltation of sense perception on the part of the dowser has been dealt with in the previous and the present Report.

It only remains for me to thank the Editor of *Light* for the full and excellent summary of the Report that appeared in his journal.

A brief notice of two books on the divining rod, which have been recently published, may here be added. Neither of these books has any scientific or literary value. One is an extraordinary medley, written by an enthusiastic believer in the rod, and the other is simply a trade advertisement of a professional dowser. I should have passed them both over, but they formed the *points d'appui* for two interesting articles in the *Daily News*, and one was the subject of a lengthy review in *Nature*, usually considered a scientific journal.<sup>2</sup> The first-named book is by Mr. Beaven, and is called *Tales of the Divining Rod*. It is quite impossible to notice this book seriously, as it is a rhapsody in turgid English on the glorious "mysteries of rhabdomancy," compiled by a tenant farmer and fruit grower of Hereford, who has found the

<sup>1</sup> *The New Psychology*, p. 260.

<sup>2</sup> *Nature*, November 2nd, 1899.

dowser of considerable use to his fruit farm. Mr. Beaven's experience of the rod I gave in the previous Report, on pp. 157 and 158, but in his book, fiction is so intermingled with fact that the author tells us, when we come to Chapter XXI :—

All that I have hitherto written concerning the divining rod has been mixed more or less with the imaginative . . . but now, to the end of the book, the truth, and the truth only, shall be stated. That at least is my intention.

This laudable intention he strives to carry out by transposing to the remaining half of his volume a large part of my previous paper; but, just as in the earlier half he allowed his readers to guess which was fact and which fiction, so in the latter half he leaves them, as a rule, to guess which are his own words and which are mine.

Mr. Beaven tells us on p. 201 his theory is that, owing to the beneficence of the Creator, every

imprisoned spring sends up an urgent and unmistakeable message to the surface . . . for man's special benefit. . . . Were those electrical influences visible, we should probably see every hill and dale dotted over with electric waving mounds, each uprising coil containing a central column which whirls in an eccentric manner precisely over each fountain head; while connected with most of these magnetic mounds are electrical currents waving on the earth's surface over the exact track of every subterranean watercourse. (!)

These "waving electric currents" are perceived by certain "gifted individuals" who have a "secretive electric power," etc. But I need not proceed further, we shall all agree that somebody's brain certainly "whirls in an eccentric manner," and Mr. Beaven's book will at any rate afford entertainment to any scientific man who happens to read it.

The other book, as I have said, is an advertisement issued by the dowser, Mr. B. Tompkins, who writes after his name "W.F." (meaning water-finder). Mr. Tompkins' *Theory of Water-finding* (as his book is called), is even more amusing than the descriptive letters he has conferred upon himself; his theory is, of course, "Electricity." He and Mr. Beaven resemble the rustic who, describing to me a ghostly light he had seen at a certain spot, said (in answer to my inquiries as to what the light resembled), that "it was *just* like an electric light." When I asked him where he had seen the electric light, he replied "I never seen it, maister, but it *were* just like it." So Mr. Tompkins tells us all about electricity, what it really is, how it moves the rod, and how he has thus been able to "unfold the

bewildering enigma of the divining rod" . . . and how he trusts ere long to receive the reward due to his efforts.

Of a very different character is an excellent little manual on the discovery of underground water by E. S. Auscher, entitled *L'Art de découvrir les Sources* (Paris, Ballière, 1899). This book is not meant for hydro-geologists, but to meet the needs of those who require a general knowledge of the method of finding underground water. It also includes a wide range of subject matter subsidiary to its main object, such as the analysis of water, the best methods of boring, etc. A section is devoted to the *baguette divinatoire* (divining rod), but here the author, like so many other writers on this subject, falls into numerous errors, not only in the history of the *baguette*, but in its mode of use, etc. He states (p. 144) that

the skill of the operator consists in moving the index fingers (*les index*) so slightly that the rod appears to turn automatically, as if it obeyed a supernatural impulse.

And yet, a few pages further on, the author states that the distinguished members of the commission appointed by the Paris Academy of Sciences to inquire into the *baguette*,<sup>1</sup> arrived at the conclusion that the motion of the rod was due to unconscious muscular action on the part of the operator. Moreover, M. Auscher appears to be unaware that one of the most eminent anthropologists in France, Professor Gabriel de Mortillet, was once a successful dowser with the *baguette*, and has written a little book on the subject, entitled *Histoire de l'hydroscopie et de la baguette divinatoire*, which was published in 1850. M. Auscher often refers to the work of L'Abbé Paramelle as a water finder, and speaks of him with a certain respect. So little is known of Paramelle in England that I have given an outline of his life-work and book in Appendix D (see p. 352), and have devoted some space to a discussion of his methods, in so far as they relate to the general subject of this paper.

As these sheets are passing through the press a statement has appeared in several English and foreign newspapers that a new French Commission had been appointed to inquire into the evidence on behalf of the *baguette divinatoire* (divining rod), and that a hydraulic engineer was appointed president of the Commission, giving his name and address. After several futile attempts to get further particulars, I received at last two badly printed circulars—one advertising the

<sup>1</sup> Referred to in my previous Report, p. 14. The facts are briefly these: in 1853 the French Academy nominated a Committee of three eminent men, MM. Chevreul, Boussingault, and Babinet, to report on a paper on the *Baguette Divinatoire* which had been presented by a M. Riouet. Chevreul was requested by his colleagues to draw up the report, which led to the publication of his work on the Divining Rod in 1854.



business of the engineer in question and soliciting the favour of a commission, and the other stating that the *Société Magnétique de France* (whatever that may be) had appointed a committee of five persons, chiefly members of that Society, to report on the "apparatus" employed by *sourciers* to seek for underground water. In answer to inquiries from an English amateur dowser, the business president of this so-called commission states that he is deputed "to make a precise inquiry into the mathematical laws (!) of the *baguette*," and he adds his opinion that money ought to be made out of the *sourciers'* business. From which the scope and scientific value of this commission may be inferred ; very different from the 1853 Committee referred to in the foot-note on the previous page.

# SUPPLEMENT.

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## I.

### PSEUDO-POSSESSION.<sup>1</sup>

BY F. W. H. MYERS.

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I have selected for joint discussion two books which, while differing widely in scope and purpose, do nevertheless agree in furnishing striking examples of a kind of study which is at present of prime importance to the psychologist.

Both works contain careful analyses of certain cases of modified or multiplex personality;—ranging from the hysterical *idées fixes* with which Dr. Janet deals, to the complex case of apparently sane and healthy, yet enormously active, subliminal mentation which forms the subject of Professor Flournoy's book.

Now I hold that all accessible modifications of personality should be studied in detail and in connection;—so that all may ultimately fall into their place in that long series which extends from the confines of insanity on the one hand up to possession and ecstasy on the other.

We ought through such study to attain a much wider conception than of old of the possibilities which lie hid in man. For the psychologist must surely feel more and more the insufficiency of the physician's conception of ordinary health and sanity as a summit from which all other states constitute a decline. It will not do any longer to regard human nature only with the aim of maintaining it as it already is. That is the doctor's view, and it was the necessary view for science to begin with; but the time has come for disinterested curiosity as well as for mere therapeutic care. There exists a greater range, we may fairly suppose, in the scale of cosmic existence *above* human nature than there exists *below* it; and, while taking care not to narrow or injure our existing powers, it should also now be our object to learn, if possible, how to *extend* them. Janet's book is important on the one side; Flournoy's takes us one short step onwards on the other. Both books are not only learned and painstaking but even brilliant. Few students of

<sup>1</sup> *Névroses et Idées fixes*. Vol. I. By Professor Pierre Janet. Vol. II. By Professor F. Raymond and Professor Pierre Janet. (Paris, 1898.)

<sup>2</sup> *Des Indes à la planète Mars: étude sur un cas de Somnambulisme avec Glossolalie*. Par Th. Flournoy. (Paris and Geneva, 1900.) [Translated by Daniel B. Vermilye, under the title of *From India to the planet Mars*, published by Harper and Brothers, New York and London, 1900.]

morbid psychology have ever shown an acumen equal to Dr. Janet's ;—few writers on matter of difficult controversy have ever produced so delightful an impression of absolute, instinctive candour as Professor Flournoy. The literary execution is in each case first-rate, and the mastery of these works is one of the most agreeable as well as essential tasks of the experimental psychologist.

To begin with Professor Janet. It must be remembered, of course, that the aim of his work is medical, and great though its philosophical interest is, a certain allowance must be made throughout for the environment of the Salpêtrière. In that atmosphere human beings are apt to get divided somewhat too simply into *médecins* and *malades*,—*mon excellent maître* and *la pauvre déséquilibrée*. In hospital lectures it is not easy to remember that there is a great world outside the hospital where faculties are more mixed and ideals perhaps more expansive.

Once outside the hospital—to make at once what small reserves I have to make as to Dr. Janet's competence—this acute physician walks with a less certain step. The paper on crystal-gazing, for instance, in Vol. I. of the present work, while ingeniously illustrating such part of the subject as hospital practice can teach, does yet merely evade the harder problems involved ;—evades them so adroitly that beyond this one sentence it calls for no comment here.<sup>1</sup>

Since there are over a thousand large closely-printed octavo pages to deal with in Professor Janet's book, some slight preliminary sketch of its main topics will be of use to the reader.

The *first* volume deals with experimental studies on the troubles of the will, the attention, and the memory ;—troubles which are in most cases associated with *idées fixes* or hypertrophies of some one “systematised group” of memories or emotions.

Thus the volume contains in Chap. I. the striking case of “Marcelle,”—a patient in whom “aboulia,” or defect of *will*, is the most marked of many symptoms. Chap. III. is devoted to cases of loss of memory ; Chap. IV., V., IX., to other extensive confusions and disabilities, induced by the persistence of *idées fixes* below the conscious threshold. Chap. X. is a history of actual pseudo-possession, showing the mischief wrought by a fixed idea self-suggested in continued reverie. Other chapters on allochiria, hemianopsia, contractures, paralysis, etc., show these subconscious self-suggestions operating in curiously limited and definite ways.

<sup>1</sup> Since I shall have nothing else but admiration and adhesion for Dr. Janet's work, I think it right here to call attention—his own attention most of all—to Mr. Andrew Lang's criticisms in *The Making of Religion*, p. 367, seq. These strictures, coming from an eminent writer who is assuredly not chargeable with any *parti pris* in favour of the marvellous ;—assuredly not lacking either in urbanity or in acumen ;—deserve, I think, to be pondered by Dr. Janet, with no mere thought of controversial rejoinder.



The *second* volume contains observations on no less than 152 cases. Whether the disorders are classed as "psychical" or as "somatic," they all illustrate the same general thesis;—namely, the great variety of ways in which the hypertrophy of some idea, impression, impulse, or memory, can invade and disintegrate that human personality, which needs the maintenance of an equilibrium between so many complex and competitive agencies within.

It is satisfactory to find that hypnotic suggestion,—used by Dr. Janet with great patience and tact,—is able gradually to remove a great number of these mental spasms and insistent memories. It is not, of course, enough to make the suggestion crudely and bluntly. In order to enable it to *take*, it must be grafted adroitly upon the patient's mental condition of the moment. First of all, one must discover the hidden sources of her trouble,—of which sources, strangely enough, she is often herself unaware;—and then one must gradually suggest successive slight modifications in the painful memory. "The incident which gave you the shock was not so bad as you think;—it really happened thus and thus," says Dr. Janet,—until at last the old horror is forgotten,—or transformed into something grotesque or innocuous.

Thus one patient is led to believe that the haunting word "cholera" is really the name of a Chinese general;—another, who has loved not wisely, but too well, is induced to see the lover of her hallucinatory memories with head transformed into a pig's;—and undergoes a revulsion of feeling, *à la* Titania. (Vol. II., p. 135.)

Another curious case (Vol. II., p. 256) in which hypnotism first explained the trouble and then did much to amend it, is that of a boy "Rou," whose short life has been plentifully interspersed with hysterical "fugues" or escapades, of which (like Ansel Bourne, *Proceedings*, Vol. VII., p. 221) he loses all memory when he returns to normal life. He comes to himself suddenly,—in a forest,—in a convent,—in a street-passage,—and, as I say, remembers nothing;—except an occasional *drenching* from rain or river,—which has, in fact, for the moment, partially awakened him from his somnambulist state. Hypnotisation, however, brings back the memory of the "fugues" (just as it did with Ansel Bourne), and we find that they have all been the working out of an *idée fixe*,—that of going to sea as a cabin-boy. During these *fugues* the lad was anæsthetic, and consequently endured cheerfully extraordinary hardships, allowing himself with perfect *insouciance* to be treated by rough canal-men like a beast of burden. Hypnotic suggestion was able to avert these *fugues* whenever the boy was dimly conscious that they were coming on;—although his brain was too profoundly affected to make the prognosis a hopeful one.

Many of these cases remind us of the narratives of Drs. Breuer and Freud, in their *Studien über Hysterie*, noticed in S.P.R. *Proceedings*,

Vol. IX., pp. 12-15. I give as a sample one case of this type, (Janet, Vol. II., p. 234), where hysterical attacks depending on the revival of a scheme of emotion (*état émotif systématique*) which has become subconscious are cured by this same process of first *discovering*, and then gradually *removing*, the alarming memory.

A girl of 18, designated as Lie, has suffered for two years from almost daily convulsive attacks. Each attack constitutes a revival of a past scene, constituting in rudimentary form a secondary state of personality. The attack begins with syncope, and the return to conscious life is a return to a condition of terror, with cries of "Lucien, Lucien" :—as if appealing to some one for defence ;—then she rushes to the window and cries "Thieves !" and then gradually re-enters her ordinary state. Asked what she can remember of such a scene, the girl can recollect nothing. She thinks that her attacks were originally induced by distress and fear at the sight of her father's drunkenness. She knows no one called Lucien. She came to Paris alone, and there seems to be no external way of supplying any possible defect in her memory.

Hypnotism, however, comes promptly to the physician's aid. Thrown into the hypnotic sleep, the patient recovers at once the details of a tragic story of her childhood ;—of an insult offered to her, from which a "Lucien" had defended her ;—and of a theft at the château where she worked, which followed a few days later. These terrifying events gave rise to attacks of syncope, somnambulism, etc. ;—and those attacks had now, in their turn, obliterated the memory of the events from the patient's waking mind. When she was reminded of them, they gradually recurred to her ;—and at the same moment the convulsive attacks which had been troubling her more or less ever since the events occurred, entirely ceased.

Here is another remarkable case (p. 248) of the efficacy of hypnotic suggestion, first in discovering the nature of a mental confusion, and then in curing it. A young officer, Pk., is brought to the Salpêtrière, having fallen down in the Champs Elysées, and seeming entirely bewildered on rising up again. Arrived at the hospital, he holds out his hand to Dr. Janet and addresses him as Dr. N.—a doctor at a military hospital at Brest. Asked what he means, and where he supposes himself to be, he replies, "We are at the military hospital at Brest, and you are Dr. N. They brought me here a few days ago—in May, 1896." As a matter of fact, this occurred on July 6th, 1895. Next day, the patient is in a different phase, and knows quite well that he is in Paris,—although he remembers nothing about imagining himself to be at Brest. On inquiry it appears that this young man of twenty-eight is suffering from the combined effects of typhoid, dysentery, marsh-fever, sunstroke, an unfortunate marriage, and a tendency to

absinthe. These troubles have made him hysterical, so that if he receives any shock or stimulus—as a threat of divorce or a glass of brandy—he is apt to become unconscious, and then to act in all sorts of odd ways, till he wakes up again in surprise and with no recollection.

Fortunately he can be thrown at once into a somnambulic state which brings back the memory of the spontaneous somnambulisms. He is then able to explain how he came to suppose himself to be in the hospital at Brest and in the year 1896. He had, in fact, while walking in the Champs Elysées, been planning to apply for treatment in a military hospital, and calculating that by about May, 1896, he would be in the hospital at Brest, where he knew the aforesaid Dr. N. Falling into his secondary state, he had no longer the ability to distinguish between this reverie and reality;—and the continuing reverie substituted itself for the actual surroundings, so that he imagined himself to be already in the Brest hospital,—as if his calculation had already worked itself out.

So soon as his excessive modifiability had thus been discovered, it was comparatively easy to give him the suggestions needed to set right his habits and to inspire him with strength for the future. He has since distinguished himself in military service abroad.

The stories of these distressed patients are not always thus tragic. The characteristic “anguish of doubt,” for instance,—the *recherche angoissante* which makes many lives miserable,—may centre itself on some subject not intrinsically painful. Thus a patient of Legrand du Saulle’s (as Dr. Janet reminds us) suffered from a perpetual anxiety to feel sure whether any woman whom he happened to have met was pretty or plain. An attendant was paid to answer, “Plain, sir! I should say, decidedly plain!” to each inquiry;—and the too sensitive heart was kept at rest. But on one occasion his master asked this man during a railway journey as to the looks of the young woman at the ticket-window of the station whence they had started;—and the man was stupid enough to say that he had not observed her. This led to a terrible crisis, and the valet had to travel back the whole way in order to bring his report, “Plain, sir! decidedly plain!”

Another distressing but somewhat grotesque case was that of a kleptomaniac who stole from everybody, but especially from himself. Worse than the somnambulic hero of an old *Ingoldsby Legend*, who buried a fresh pair of his own trousers every night in the garden, this boy could keep nothing by night or day from his own marauding hands. He stole from himself when he was asleep, but he stole from other people when *they* at any rate were wide awake, and saw perfectly what he was doing. In fact, the psychological interest of this case lies in the poor fellow’s own condition at the moment of his thefts,—which was a kind of semi-hypnotisation by involuntary self-suggestion,



closely resembling the half-hypnotised state into which an awakened subject falls when the time comes for him to execute some post-hypnotic suggestion. This unwilling kleptomaniac seemed to be subject to a permanent self-suggestion to carry off any object which he particularly noticed ; and his own gaze at a silver spoon would throw him into a dreamy condition in which he inevitably put that spoon in his pocket.

Another melancholy aberration, which is not without its comic element, is described by Janet under the heading of “Dipsomania of *café au lait*, complicated with kleptomania of *petits pains*.”

M<sup>me</sup> Pt., a nervous personage, suffered from a dyspepsia which her doctor mistakenly attributed to dilatation of the stomach. He limited her to a wineglass of liquid at each meal, and made her live mainly on dry toast. Her husband supported the doctor, but meantime she saw *him* drinking *café au lait* and eating rolls with gusto and freedom ;—“*c’était pour elle le supplice de Tantale*.” One day her patience gave way. When her husband’s back was turned, she threw herself upon a roll which he had left on his plate, then rushed to the *crémérie*, and made herself an excellent *café au lait*. She was none the worse for it ; her husband it was who died. After his death the passion for *café au lait* grew on her, so that when her stock of milk was exhausted at night she would tread her room in anguished longing till the *créméries* opened in the morning. Not venturing to drink more than two cups at the same *crémérie*, she would wander about Paris and drink at various shops 20 or 30 cups per day.

All these cups she duly and promptly paid for. But a morbid aggravation came. She wanted rolls with her coffee ; but now that there was no one to forbid her the rolls, they lost their savour,—the savour of *petit pain défendu*. A way of supplying this savour occurred to her ;—she *stole* the rolls. She would go to a shop and pay for one roll,—and adroitly make several others fall into a Gampish umbrella. Having collected a dozen or so, she would go home to eat them, and start again.

Now, this poor woman—this coffee-megalomaniac, as an erudite friend of Professor Janet’s has termed her—becomes not infrequently very uncomfortable inside. She detests her own *obsession*, her *idée fixe* ;—but she cannot cure herself ; nor had Professor Janet, at the time of writing, succeeded in curing her.

A grotesque, yet a mournful story ! a tragedy of the “free breakfast table” ! nay, a satire on all the inordinate cravings of men, whose insatiability is the penalty for their self-abandonment, and which disgust by the very fulfilment of desire. Dr. Janet’s book contains examples of “fixed subconscious ideas” at various depths of submergence ;—the most completely forgotten being often the most potent for mischief. A fixed memory of this kind,—removed, so to say, from the

general mental circulation,—may act in precisely the same way as an actual lesion to motor nerves, inducing definite physical disabilities.

“The contraction persists,” says Dr. Janet of one such case (Vol. I. p. 344) “because the *emotion* persists, involving always the same psychological and physiological consequences. It is, so to say, a *fixed emotion*;—a variety of fixed idea of which we from outside can see only one exterior manifestation, while the patient herself is unaware of the interior phenomenon which determines it.”

To attempt to cure such patients without first discovering the true cause of mischief by some automatic message (such as writing or crystal vision or utterance in hypnotic trance) which emanates from subliminal strata of their being, is like trying to open a secret drawer without having discovered the spring. The minute and patient attention which Dr. Janet bestows on such cases contrasts strikingly with the rough and ready methods thought good enough for hysterical patients by doctors of the old school. It means, of course, one more application of true scientific method to problems which the Philistine physician did not think worth his science. There can be no better example of this than Dr. Janet’s treatment of a singular problem which the mistakes of brutal ignorance turned in old times into a veritable scourge of our race. I speak of *demoniacal possession*;—in which affliction Dr. Janet has shown himself a better than ecclesiastical exorcist.

I give here a typical case of pseudo-possession. Achille,—as Professor Janet calls him,—was a timid and rather morbid young man; but he was married to a good wife, and nothing went specially wrong with him until his return from a business journey in 1890. He then became sombre and taciturn,—sometimes even seemed unable to speak,—then took to his bed and lay murmuring incomprehensible words, and at last said farewell to his wife and children, and stretched himself out motionless for a couple of days, while his family waited for his last breath.

(Vol. I., p. 382.)—“Suddenly one morning, after two days of apparent death, Achille sat up in bed with his eyes wide open, and burst into a terrible laugh. It was a convulsive laugh which shook all his limbs,—an exaggerated laugh which twisted his mouth;—a lugubrious, satanic laugh which went on for more than two hours.

“From this moment everything was changed—Achille leapt from his bed and refused all attentions. To every question he answered, ‘There’s nothing to be done! let’s have some champagne! it’s the end of the world!’ Then he uttered piercing cries ‘they are burning me;—they are cutting me to pieces!’”

After an agitated sleep, Achille woke up with the conviction that he was possessed with a devil. And in fact his mouth now uttered

blasphemies, his limbs were contorted, and he repeatedly made unsuccessful efforts at suicide. Ultimately he was taken to the Salpêtrière and placed under Professor Janet, who recognised at once the classic signs of possession. The poor man kept protesting against the odious outrages on religion which he attributed to a devil inside him, moving his tongue against his will. "Achille could say, like a celebrated victim of possession, Père Surin, 'It is as though I had two souls; one of which has been dispossessed of its body and the use of its organs, and is frantic at the sight of the other soul which has crept in.'"

It was by no means easy to get either at Achille or at his possessing devil. Attempts to hypnotise him failed, and any remonstrance was met with insult. But the wily psychologist was accustomed to such difficulties, and had resort to a plan too insidious for a common devil to suspect. He gently moved the hand of Achille in such a way as to suggest the act of writing, and having thus succeeded in starting automatic script he got the devil thus to answer questions quietly put while the raving was going on as usual. "I will not believe in your power," said Professor Janet to the malignant intruder, "unless you give me a proof." "What proof?" "Raise the poor man's left arm without his knowing it." This was done,—to the astonishment of poor Achille;—and a series of suggestions followed, all of which the demon triumphantly and unsuspectingly carried out, to show his power. Then came the suggestion to which Professor Janet had been leading up. It was like getting the djinn into the bottle. "You cannot put Achille soundly to sleep in that armchair!" "Yes, I can!" No sooner said than done,—and no sooner done than Achille was delivered from his tormentor,—from his own tormenting self.

For there in that hypnotic sleep he was gently led on to tell all his story;—and such stories, when told to a skilled and kindly auditor, are apt to come to an end in the very act of being told.

Achille had been living in a day-dream; it was a day-dream which had swollen to these nightmare proportions, and had, as it were, ousted his rational being;—and in the deeper self-knowledge which the somnambulic state brings with it the dream and the interpretation thereof became present to his bewildered mind.

The fact was that on that fateful journey when Achille's troubles began he had committed an act of unfaithfulness to his wife. A gloomy anxiety to conceal this action prompted him to an increasing taciturnity, and morbid fancies as to his health grew on him until at last his day-dream led him to imagine himself as actually dead. "His two days' lethargy was but an episode, a chapter in the long dream."

What then was the natural next stage of the dream's development? "He dreamt that, now that he was dead indeed, the devil rose from the abyss and came to take him. The poor man, as in his somnambulic state



he retraced the series of his dreams, remembered the precise instant when this lamentable event took place. It was about 11 a.m. ;—a dog barked in the court at the moment, incommoded, no doubt, by the smell of brimstone ;—flames filled the room ;—numbers of little fiends scourged the unhappy man, or drove nails into his eyes,—and through the wounds in his body Satan entered in to take possession of head and heart.”

From this point the pseudo-possession may be said to have begun. The fixed idea developed itself into sensory and motor automatisms ;—visions of devils, uncontrollable utterances, automatic script—scribed by the automatist to the possessing devil within.

And now came the moment when the veracity, the utility, of this new type of psychological analysis was to be submitted to yet another test. From the point of view of the ordinary physician Achille's condition was almost hopeless. Physical treatment had failed, and death from exhaustion and misery seemed near at hand. Nor could any appeal have been effective which did not go to the hidden root of the evil ;—which did not lighten the load of morbid remorse from which the whole series of troubles had developed. Fortunately for Achille, he was in the hands of an unsurpassed minister to minds thus diseased. Professor Janet adopted his usual tactics ;—what he terms the *dissociation* and the gradual *substitution* of ideas. The incidents of the miserable memory were modified, were explained away, were slowly dissolved from the brooding brain ;—and the hallucinatory image of the offended wife was presented to the sufferer at what novelists call the psychological moment,—with pardon in her eyes. “Such stuff as dreams are made of !”—but even by such means was Achille restored to physical and moral health ; he leads now the life of normal man ; he no longer “walketh in a vain shadow, and disquieteth himself in vain.”

The title of “pseudo-possession ” which I have given to this review is partly justified by the case just described. Yet, as we shall presently see, this is not the form of pseudo-possession which is most difficult to understand and classify.

In Achille's case we have nothing but disease and delusion ; we shall presently find it harder to explain phenomena one stage higher, where the submerged mental action is complex and in no way hurtful ;—yet cannot be interpreted as evidencing an external control.

I will conclude my notice of Dr. Janet's book by touching on a cognate problem of which again only the early stages have come under his observation ;—but where the relation of each stage to the stage above it is as yet inextricably obscure. I speak of *ecstasy* ;—of all that range of sensations which present themselves as a temporary “standing-forth” from the body, with more or less conscious or definite communion with or entry into a spiritual world.

This phenomenon, alleged in many centuries and many nations, has usually been explained in one of two ways ;—neither of which seems to me satisfactory.

a. In the first place, it has been connected by theological writers with the dogmas of the special religion in which they believed ;—and which their ecstatic visions are assumed to corroborate. As an example of such treatment I may allude to the recently published Bampton Lectures by Dr. Inge on *Christian Mysticism*, which have justly attracted admiration from their tone, alike scholarly and devout. Unfortunately, however, these qualities are not sufficient for the task of showing that there is any real connection between the ecstasies into which Pagans and Buddhists just as much as Roman Catholics or Protestants have fallen and the dogmas or traditions of any one of the great sections of the Christian Church. The impression which a reader familiar with the operation of self-suggestion is likely to carry away from Dr. Inge's book is rather of wonder that there is not a *stronger* case for such connection ;—that self-suggestion has not given a more decided flavour of Christian dogma to the very vague,—though often deeply genuine,—accounts of ecstatic experiences which Dr. Inge has been able to collect. I will not pursue a line of argument which it would be invidious to press, and which will hardly meet with any scientific opposition.

Rather I will turn to the other habitual type of explanation,—that which explains away all ecstasy as a mere subjective symptom—a hysterical experience which medical science should treat as it treats an *idée fixe* or a recurrent contracture.

Here, I think, is just one of the points where the psychologist should be on his guard against the physician. The psychologist, as I have so often said, must remember that there are phenomena *above* ordinary human nature as well as *below* it, and that if anywhere we can trace an ascending series of vital phenomena, it is very unlikely that we are ourselves standing at that series' superior limit.

There is undoubtedly a *lower* limit to what I may term the ecstatic series ;—a rudimentary or hysterical ecstasy which seems to be nothing more than a mere blankness and vacancy. First above *that* perhaps we may place two cases which Dr. Janet briefly mentions ;—adding (I am glad to see) a hint that he may return to the subject later.

The first of these cases is that of a girl, "Célestine," who is a definitely hysterical subject. (Vol. I., p. 302-3). After an attack of hysterical anger she remains absolutely motionless on her bed, arms and legs stretched out, fists closed, and teeth clenched.

"This condition, which may be prolonged for two or three days if not interfered with, is psychologically very remarkable. The patient, entirely insensible to all external stimuli, retains a certain

consciousness; she has complex dreams, which she has forgotten on waking, but of which she recovers memory in hypnotic trance. These varied dreams are specially characterised by a feeling of well-being, of beatitude; the patient feels no link with earth, is unconscious of her body, and seems to float in air. I have noticed the same feeling of beatitude in another patient, who loses the possibility of motion and the consciousness of her body. One wonders whether there is any relation between the two phenomena."

The next case of ecstasy (Vol. I., p. 95) carries us one step higher, and occurs in a woman who is intelligent and not anæsthetic, but who presents certain symptoms classed by Drs. Raymond and Janet as hysterical—especially *stigmatisation*—or the mark as of a wound imitating Christ's Passion, which used to rank as a sign of sanctity. This *stigmatisée's* wound is on the foot, —in imitation of the mark of the nail. Her ecstasies are of the accustomed saintly type;—and the following accidental test of her condition during ecstasy forms an incident probably unique in the long history of *stigmatisés* from S. Francis to Louise Lateau.

Dr. Janet was collecting series of reaction-times from various subjects;—testing, that is to say, the quickness and regularity with which they could make a motor response (as pressing a button, etc.) to some irregularly recurring stimulus, of touch, sound or sight. Of course the quicker and more regular the response, the more highly does the experimenter rank the attention and capacity of the subject.

"This subject," says Dr. Janet, "was very docile, and tried to fix her attention on the sound of the signal and on nothing else. But after about a quarter of an hour, I perceived that an unforeseen incident seemed likely to disturb the experiment. Vt. began an attack of ecstasy. Her eyes half closed, her head sank backward, and her face assumed an expression of beatitude. . . . Little by little her body rose on tiptoe, the left arm took a horizontal position, and she placed herself in the attitude of crucifixion which is common in her ecstasies. I was struck by observing that her right arm did not raise itself as much as usual, for the top of the forefinger continued to touch the button used in the experiment (*interrupteur*). Thus, in spite of the complete ecstasy, the tracing continues to be produced. I prolonged this singular experience of taking reaction-times during a religious ecstasy for two hours and a-half. She then awoke, but with a remarkable fidelity to her promise, she continued to reply to the signals during and after her process of awakening, so that I was able to get a graphic reaction-curve taken before, during, and after the ecstasy."

Omitting details, I may say that the graphic response was quickest and most regular during the height of the ecstasy;—in spite of attempts made by Dr. Janet to disturb the subject by sudden noises, pinching,



etc.,—attempts which had no effect at all in distracting her from her duty, although she must have felt them, since she retains her power of sensation in the trance.

The general lesson which Dr. Janet draws from this and other experiments is that the more automatically you can get the reaction performed the *better* it is performed;—in short that subliminal attention is more trustworthy than supraliminal. On the present case he adds the following reflections :—

“Is it likely that Vk., during the crisis of ecstasy, when she raises to heaven a face of beatitude—when she takes the position of crucifixion,—is bestowing on my signals a closer attention than at the moment when she is quietly sitting in an armchair with nothing to do but to listen ?

“Moreover, she does partially remember her ecstasies, and she tells us of the unspeakable joys which she has been experiencing. While she was responding to my signals she felt herself flooded by religious emotion. ‘I felt an ineffable sweetness on my lips; presently they closed; a torpor stole over my limbs, but the torpor was full of sweetness.’ Then in complete ecstasy: ‘I saw the Holy Sacrament in a light, a radiance, a glory inexpressible, and I was filled with unspeakable gladness. A voice within me made me understand that by the act of communion I carried Jesus within me as though I were a living monstrosity.’

“But I will not here discuss (continues Dr. Janet) the ecstasies of this patient; the phenomenon is too striking to be treated thus incidentally. I merely remark that during this ecstasy she made a long meditation, not without philosophical and poetical value, on the union of the soul with God. I observe also that she remembered this reflection, and remembered also the noises which I made in the room, and my pinches on her arm. When I asked her as to the special point of her attention to my signals and her own answering movements, she answered, ‘I believe that I kept my hand on the machine, as you told me, but God must have guided my hand to obey your orders, for I was not myself able to pay them any attention.’”

I must now reluctantly leave Dr. Janet's book;—observing only that in the midst of this record of multiform disintegrations and contortions of personality we have here come, nevertheless, upon certain signs of increased faculty,—of subliminal steadiness of muscular action under difficulties,—of ecstatic happiness, and even of intellectual exaltation.

The book to which I now turn has for readers of our *Proceedings* an even directer interest. It happens, no doubt, to any group which pursues for many years a somewhat unfamiliar line of inquiry that

those of their points which are first assailed get gradually admitted, so that as they become interested in new points they may scarcely observe what change has taken place in the reception of the old. The reader of early volumes of our *Proceedings* will often observe this kind of progress of opinion. And now Professor Flournoy's book indicates in a remarkable way how things have moved in the psychology of the last twenty years. The book,—a model of fairness throughout,—is indeed, for the most part, critically *destructive* in its treatment of the quasi-supernormal phenomena with which it deals. But what a mass of conceptions a competent psychologist now takes for granted in this realm, which the official science of twenty years ago would scarcely stomach our hinting at!

One important point may be noticed at once as decisively corroborating a contention of my own made long ago, and at a time when it probably seemed fantastic to many readers. Arguing for the potential *continuity* of subliminal mentation (as against those who urged that there were only occasional flashes of submerged thought, like scattered dreams) I said that it would soon be found needful to press this notion of a continuous subliminal self to the utmost, if we were not prepared to admit a continuous spiritual guidance or possession. Now, in fact, with Professor Flournoy's subject the whole discussion turns on this very point. There is unquestionably a continuous and complex series of thoughts and feelings going on beneath the threshold of consciousness of M<sup>lle</sup> "Hélène Smith." Is this submerged mentation due in any degree or in any manner to the operation of spirits other than M<sup>lle</sup> Smith's own? That is the broad question; but it is complicated here by a subsidiary question;—whether, namely, any previous incarnations of M<sup>lle</sup> Smith's,—other phases of her own spiritual history, now involving complex relationship with the past,—have any part in the crowd of personalities which seem struggling to express themselves through her quite healthy organism.

M<sup>lle</sup> Smith, I should at once say, is not, and never has been, a paid medium. She occupies a leading post on the staff of a large *maison de commerce* at Geneva, and gives séances to her friends, simply because she enjoys the exercise of her mediumistic faculties, and is herself interested in their explanation.

Her organism, I repeat, is regarded, both by herself and by others, as a quite healthy one. M<sup>lle</sup> Smith, says Professor Flournoy, declares distinctly that she is perfectly sound in body and mind, in no way lacking in equilibrium, and indignantly repudiates the idea that there is any hurtful anomaly or the slightest danger in mediumship as she practises it.

"I am so far from being abnormal," she writes, "that I have never been so clear-sighted, so lucid, so capable of judging rapidly on all points,

as since I have been developed as a medium." No one appears to dispute this estimate, which the facts of M<sup>lle</sup> Smith's progress in her line of business distinctly confirm.

"It is in fact incontestable" (continues Professor Flournoy) "that H<sup>élène</sup> has a head extremely well organised ; and that from a business point of view she manages admirably the very important and complicated department of which she is at the head in this large shop where she is employed ;—so that to accuse her of being morbid simply because she is a medium is to say the least an inadmissible *petitio principii* so long as the very nature of mediumship remains a thing so obscure and open to discussion as is still the case. . . .

"It is clear that there exist amid the ranks of the learned faculty certain spirits narrow and limited, strong in their own specialities, but ready to cast their anathemas at whatever does not fit in with their preconceived ideas, and to treat as morbid, pathological, insane, everything which differs from the normal type of human nature, such as they have conceived it on the model of their own small personalities. . . .

"But in the first place the essential criterion in judging of a human being's value is not the question whether he is in good or bad health, like or unlike other people, but whether he fulfils adequately his special task,—how he acquits himself of the functions incumbent on him, and what may be expected or hoped from him. I am not aware that Miss Smith's psychical faculties have ever interfered with her accomplishment of any of her duties ;—rather they have helped her therein ; for her normal and conscious activity has often found an unexpected assistance,—which non-mediums lack !—in her subliminal inspirations and her automatisms, which effect a useful end.

"In the second place, it is far from being demonstrated that mediumship is a pathological phenomenon. It is abnormal, no doubt, in the sense of being *rare, exceptional* ;—but rarity is not morbidity. The few years during which these phenomena have been seriously and scientifically studied have not been enough to allow us to pronounce on their true nature. It is interesting to note that in the countries where these studies have been pushed the furthest, in England and America, the dominant view among the *savants* who have gone deepest into the matter is not at all unfavourable to mediumship ; and that, far from regarding it as a special case of hysteria, they see in it a faculty superior, advantageous, healthy, of which hysteria is a form of degenerescence, a pathological parody, a morbid caricature."

The phenomena which this sensitive (H<sup>élène</sup> Smith is Professor Flournoy's pseudonym for her) presents cover a range which looks at first very wide, although a clearer analysis shows that these varieties are more apparent than real and that self-suggestion will perhaps account for all of them.



There is, to begin with, every kind of automatic irruption of subliminal into supraliminal life. As Professor Flournoy says (p. 45):—"Phenomena of hypermnnesia, divinations, mysterious findings of lost objects, happy inspirations, exact presentiments, just intuitions, teleological (purposive or helpful) automatisms, in short, of every kind;—she possesses in a high degree this small change of genius;—which constitutes a more than sufficient compensation for the inconvenience resulting from those distractions and moments of absence of mind which accompany her visions;—and which, moreover, generally pass unobserved."

And here I reach an appropriate point at which to remark that this book of Professor Flournoy's is not the first account which has been published of M<sup>lle</sup> Hélène. Professor Lemaître, of Geneva, printed two papers about her in the *Annales des Sciences Psychiques*;—first, a long article in the number for March–April, 1897,—then a reply to M. Lefébure in the number for May–June, 1897. In these papers he distinctly claims supernormal powers for M<sup>lle</sup> Hélène,—implying a belief in her genuine possession by spirits, and even in her previous incarnations, and in the reality of the extra-terrene or ostensibly Martian language. I read these papers at the time, but put them aside as inconclusive, mainly because that very language, on which M. Lemaître seemed most to rely, seemed to me so obviously factitious as to throw doubt on all the evidence presented by an observer who could believe that denizens of another planet talked to each other in a language corresponding in every particular with simple French idioms, and including such words as *quisa* for *quel*, *quisé* for *quelle*, *vétèche* for *voir*, *vèche* for *vu*;—the fantastic locutions of the nursery. M. Lemaître remarks, as a proof of the consistency and reality of the extra-terrene tongue, "L'un des premiers mots que nous ayons eus, *métiche*, signifiant *monsieur*, se retrouve plus tard avec le sens de *homme*." That is to say, having transmogrified *monsieur* into *métiche*, Hélène further transmutes *les messieurs* into *cée métiché*;—in naive imitation of ordinary French usage. And this tongue is supposed to have sprung up independently of all the influences which have shaped terrene grammar in general or the French idiom in particular! And even after Professor Flournoy's analysis of this absurdity I see newspapers speaking of this Martian language as an impressive phenomenon!

Very possibly the same critics might object, say, to the utterances of the Thaw children through Mrs. Piper (S.P.R. *Proceedings*, Part XXXIV.) as too trivial and childish for a spiritual world. Yet surely it should hardly be needful to say that on the principle of continuity a child passing from earth into that world will still be occupied with childish things;—while, on the other hand, the evolution of another

planet, if it has culminated in conscious life at all, can hardly have culminated in a conscious life into which we could all of us enter affably, with a suitable Ollendorff's phrase-book under our arms ;— "*enî cée métiche oné gudé*,"—"ici les hommes (messieurs) sont bons,"—"here the men are good"—and the rest of it.

Yet I am bound in fairness to say that, judging M. Lemaître too sweepingly from this linguistic standpoint, I assumed that one remarkable incident which he quotes could probably be explained away ;—whereas Professor Flournoy has not really explained it away at all. This is the case of *Jean le Carrieur*. Stated briefly, it is admitted that Hélène, after experiencing a hallucinatory smell of sulphur (characteristic of a quarryman's work), had a vision of a man working, never known to her, who sent a message to a M<sup>me</sup> Mirbel, not present at the séance. This man, and the scene in which he appeared, were clearly recognised by M<sup>me</sup> Mirbel as belonging to her childish memories ;—while it is most improbable that the little incidents in question should ever have reached Hélène's ears. M<sup>me</sup> Mirbel had, however, been present at some of Hélène's séances, and Professor Flournoy thinks that on one of these occasions the subliminal self of Hélène had drawn these memories telepathically from M<sup>me</sup> Mirbel's mind. This kind of *possibility*, of course, has always to be faced. Its relative *probability*, as compared with spirit-influence, has to be inferred in each case from a wide group of experiences. But whatever the solution which we adopt for this incident of the quarryman, it must, I think, go some way towards proving M<sup>lle</sup> Hélène's supernormal gifts.

At séances,—where the deeper change has no inconveniences,—Hélène undergoes a sort of self-hypnotisation which produces various lethargic and somnambulistic states. And when she is alone and safe from interruption she has spontaneous visions, during which there may be some approach to ecstasy. At the séances she experiences positive hallucinations, and also negative hallucinations, or systematised anæsthesiæ, so that, for instance, she will cease to see some person present, especially one who is to be the recipient of messages in the course of the séance. "It seems as though a dream-like incoherence presided over this preliminary work of disaggregation, in which the normal perceptions are arbitrarily split up or absorbed by the sub-conscious personality,—eager for materials with which to compose the hallucinations which it is preparing." Then, when the séance begins, the main actor is Hélène's guide *Léopold* (a pseudonym for Cagliostro) who speaks and writes through her, and is, in fact, either her leading spirit-control or (much more probably) her most developed form of secondary personality.

Hélène, indeed, has sometimes the impression of *becoming* Leopold for a moment (p. 117). Professor Flournoy compares this sensation with

the experience of Mr. Hill Tout (S.P.R. *Proceedings*, Vol. XI., p. 309), who feels himself *becoming* his own father, who is manifesting through him. It should be added that, although somewhat pompous, Leopold always appears both sensible and dignified. "Leopold," says Professor Flournoy (p. 134) "certainly manifests a very honourable and amiable side of M<sup>lle</sup> Smith's character, and in taking him as her 'guide' she has followed inspirations which are doubtless among the highest in her nature."

The high moral quality of these automatic communications, on which Professor Flournoy thus insists, is a phenomenon worth consideration. I do not mean that it is specially strange in the case of M<sup>lle</sup> Smith. She appears to be (if the phrase is thought permissible in describing a medium) a person of remarkably well-regulated mind. One is not surprised that her subliminal self should be as blameless as her supraliminal. But in reality the remark here made by Professor Flournoy has a much wider application. The almost universally high moral tone of genuinely automatic utterances,—whether claimed as spirit communications or proceeding obviously from the automatist himself,—has not, I think, been sufficiently noticed, or adequately explained. I will mention two points which have struck me as specially noticeable. In the first place I have read many pulpit and other attacks on "spiritualism," under which name all automatic utterance is commonly included, and I cannot remember any instance in which such an attack has been made effective by the quotation of passages of immoral tendency—base, cruel, or impure. The attack, so far as I know, has always been of a kind which, in the eye of the philosopher, is rather complimentary to the writings attacked. For it seems (and this is the second point to which I wished to call attention here) that no one of the various conflicting Churches has been able to claim the general drift of automatic messages as making for its special tenets. The various controversialists, where they have been candid, have admitted moral elevation, but,—from their various opposing points of view,—have agreed in deploring theological laxity.

I must indeed confess myself unable to explain why it is that beneath the frequent incoherence, frequent commonplaceness, frequent pomposity of these messages there should almost always be a substratum of better sense, of truer catholicity, than is usually to be heard except from the leading minds of the generation. It is possible that in some hidden way the Zeit-Geist affects the subliminal strata even of persons superficially narrow and bigoted by an influence urging them all in somewhat the same direction ;—so that the best available thought of the age is inspiring the age more profoundly than we know. And it is possible also that these utterances may bear in reality some obscure relation to truths profounder than we have as yet normally acquired.



What is omitted, indeed, from current beliefs is as significant as what is added thereto, and the general product looks more like a very poor account of something which in itself is great and new, but dimly apprehended, than like a compromise between conflicting dogmas, or a selection from familiar hortatory themes.

Thus much I think it was fair to say ;—or I may speak more strongly and maintain that thus much it was a positive duty to insist upon. It is only right that this mass of communications, taken as a whole, should be defended from the random accusations of journalist or pulpiteer. Yet, in view of what is here to follow, I must lose no time in defining the limited extent to which my support of automatic messages goes.

I think, then, that in evidential messages, —where there is real reason to believe that an identified spirit is communicating,—there is a marked and independent consensus on such matters as these spirits profess themselves able to discuss. And, again, in non-evidential messages,—in communications which probably proceed from the automatist's subliminal self,—I hold that there is a remarkable and undesigned concordance in high moral tone, and also in avoidance of certain prevalent tenets, which many of the automatists do supraliminally hold as true. But I also insist that these subliminal messages, even when not incoherent, are generally dream-like, and often involve tenets which (though never in my experience base or immoral) are unsupported by evidence, and are probably to be referred to mere self-suggestion.

Prominent among such tenets is one which forms a large part of M<sup>lle</sup> Smith's communications ; namely, the doctrine of *reincarnation*, or of successive lives spent by each soul upon this planet.

The simple fact that such was probably the opinion both of Plato and of Virgil shows that there is nothing here which is alien to the best reason or to the highest instincts of men. Nor, indeed, is it easy to realise any theory of the *direct creation* of spirits at such different stages of advancement as those which enter upon the earth in the guise of mortal man. There *must*, one feels, be some kind of continuity,—some form of spiritual Past. Yet for reincarnation there is at present no valid evidence ; and it must be my duty to show how its assertion in any given instance,—M<sup>lle</sup> Smith's included,—constitutes in itself a strong argument in favour of self-suggestion rather than extraneous inspiration as the source of the messages in which it appears.

Whenever civilised men have received what they have regarded as a revelation (which has generally been somewhat fragmentary in its first delivery) they have naturally endeavoured to complete and systematise it as well as they could. In so doing they have mostly aimed at three objects ; (1) to *understand* as much as possible of the secrets of the universe ; (2) to *justify* as far as possible Heaven's dealings with men ; and (3) to *appropriate* as far as possible the favour or

benefit which the revelation may show as possibly accruing to believers. For all these purposes the doctrine of reincarnation has proved useful in many countries and times. But in no case could it seem more appropriate than in this last revelation (so to term it) through automatic messages and the like. And as a matter of history, a certain vigorous preacher of the new faith, known under the name of Allan Kardec, took up reincarnationist tenets, enforced them (as there is reason to believe) by strong suggestion upon the minds of various automatic writers, and set them forth in dogmatic works which have had much influence, especially among Latin nations, from their clarity, symmetry, and intrinsic reasonableness. Yet the data thus collected were absolutely insufficient, and the *Livre des Esprits* must simply rank as the premature formulation of a new religion,—the premature systematisation of a nascent science.

I follow Professor Flournoy in believing that the teaching of that work must have directly or indirectly influenced the mind of M<sup>lle</sup> Smith,—and is therefore responsible for her claim to these incarnations previous to that which she now undergoes or enjoys.

On the general scheme here followed, each incarnation, if the last has been used aright, ought to represent some advance in the scale of being. If one earth-life has been misused, the next earth-life ought to afford opportunity for expiation,—or for further practice in the special virtue which has been imperfectly acquired. Thus M<sup>lle</sup> Smith's present life in a humble position may be thought to atone for her overmuch pride in her last incarnation,—as Marie Antoinette.

But the mention of Marie Antoinette suggests the risk which this theory fosters,—of assuming that one is the issue of a distinguished line of spiritual progenitors ;—inasmuch that, with whatever temporary sets-back, one is sure in the end to find oneself in a leading position.

Pythagoras, indeed, was content with the secondary hero Euphorbus as his bygone self. But in our days Dr. Anna Kingsford and Mr. Edward Maitland must needs have been the Virgin Mary and S. John the Divine. And Victor Hugo, who was naturally well to the front in these self-multiplications, took possession of most of the leading personages of antiquity whom he could manage to string together in chronological sequence. It is obvious that any number of re-born souls can play at this game ;—but where no one adduces any evidence it seems hardly worth while to go on. Even Pythagoras does not appear to have adduced any evidence beyond his *ipse dixit* for his assertion that the alleged shield of Euphorbus had in reality been borne by that mythical hero. Meantime the question as to reincarnation has actually been put to a very few spirits who have given some real evidence of their identity. So far as I know, no one of these has claimed to know anything personally of such an

incident ;—although all have united in saying that their knowledge was too limited to allow them to generalise on the matter.

Hélène's previous incarnations,—to return to our subject,—do perhaps suffer from the general fault of aiming too high. She has to her credit a pre-incarnation in the planet Mars ;—another as an Indian Princess ; and a third (as I have said) as Marie Antoinette.

In each case there are certain impressive features in the impersonation ; but in each case also careful analysis negatives the idea that we can be dealing with a personality really revived from a former epoch, or from a distant planet ;—and leaves us inclined to explain everything by “cryptomnesia” (as Professor Flournoy calls submerged memory), and that subliminal inventiveness of which we already know so much.

The *Martian* pre-incarnation was naturally the most striking at first sight. Its reality was supported by a Martian language, written in a Martian alphabet, spoken with fluency, and sufficiently interpreted into French to show that such part of it, at any rate, as could be committed to writing was actually a grammatical and coherent form of speech.

This to the ordinary reader sounds like good evidence. To the student of automatisms, on the other hand, as I have already said, it irresistibly suggests the automatist's own subliminal handiwork. It is a case of “glossolaly,” or “speaking with tongues” ; and we have no modern case—no case later than the half-mythical Miracles of the Cevennes—where such utterance has proved to be other than gibberish. I have had various automatic hieroglyphics shown to me, with the suggestion that they may be cursive Japanese, or perhaps an old dialect of Northern China ;—but I confess that I have grown tired of showing these fragments to the irresponsible expert, who suggests that they may also be vague reminiscences of the scrolls in an Oriental tea-tray.

It seems indeed to be a most difficult thing to get telepathically into any brain even fragments of a language which it has not learnt. A few simple Italian, and even Hawaian, words occur in Mrs. Piper's utterances, coming apparently from departed spirits, but these, with some Dutch words recently given through another sensitive, form, I think, the only instances which I know. And, speaking generally, whatever is elaborate, finished, pretentious, is likely to be of subliminal facture ; while only things scrappy, perplexed and tentative have floated to us veritably from afar.

Analysis of the so-called Martian language proves it to be no exception to this rule. It is, in fact, a childish, though elaborate, imitation of French ;—whose true parallel lies in those languages of the nursery which little brothers and sisters sometimes invent—as a tongue not understood of their elders. The outbursts of this Martian speech are noticeable as a parallel to the “deific verbiage,” which used to



throng through the lips of Mr. le Baron (*Proceedings*, Vol. XII., p. 277), and for a long time impressed itself upon him as having some reality in it somewhere.

The most interesting peculiarity, indeed, in the Martian tongue is its *exclusively French* formation ;—which would seem to argue its elaboration in a mind familiar with French alone. Now M<sup>lle</sup> Smith, —who, by the way, is no linguist,—had some German lessons in her girlhood, and one is thus led to the curious supposition that the Martian tongue was invented by some element in her personality which *preceded* the German lessons.

I fancy that I have mentioned somewhere, but I may perhaps recall here, a trivial experience of my own illustrative of this ingenious hypothesis of Professor Flournoy's. I once dreamt that I saw an epitaph in Greek hexameters inscribed on a wall, —of which on waking I remembered only one line—

Αὐτὰρ ὁ μὲν κατὰ γῆν θαλερὸν κύσε δακνόμενον πῦρ.

I could not construe this line, which is, in fact, nonsense ;—till I remembered in a sudden flash a certain sense of shame felt by me as a small boy at having thought that *κατὰ* meant *under*,—as though *κατὰ γῆν* were *γῆς κάτω*.

The line, then, had a meaning : “ But he, indeed, beneath the earth, embraced the strong consuming flame ; ”—not a well-chosen sentiment for an epitaph, perhaps, but yet up to the ordinary level of one's dreaming self. There must, then, have been some fragment of me yet surviving from innocent boyhood, and blundering subliminally in the same old style.

¶ This fact of the different age of M<sup>lle</sup> Smith's life to which her hypnoidal elucubrations in this aspect belong seems to me to constitute one of the most interesting psychological points in her mediumship. It tends to show that her secondary personalities are probably in their origin—as has sometimes been suggested—phenomena partly of *reversion* in the ordinary personality,—survivals or momentary returns of inferior phases, overpassed for a longer or shorter time, and which should normally have been absorbed in the development of the individual instead of appearing externally in strange proliferations. Just as teratology illustrates embryology, which in return explains teratology, and as both of these unite in throwing light on anatomy, similarly one may hope that the study of the facts of mediumship may some day help to furnish us with some just and fruitful view of normal psychogenesis,—which in return will enable us better to comprehend the appearance of these singular phenomena ;—so that psychology in general may thence acquire a better and exacter conception of human personality.

The faculty here touched upon—the strong reviviscence of long-past emotional states—seems to me eminently characteristic, at any rate, of artistic and poetical genius.

The artist must needs desire to have his whole life to draw upon. He must often wish to live in the past more vividly than in the present, and to feel again what he has felt, even more than to see again what he has seen. Visual and auditory memories, pushed to absolute vividness, become hallucinations of vision or audition; and this point of absolute hallucination few artists are able or even desire to reach. But *emotional* or *affective* memory may for some gifted natures be pushed on to all its old actual vividness with pure gain to art:—nay, if the man himself has grown more capable of feeling, then the revived emotion (like certain optical memory-images) may even go beyond the original.

Thus Sully Prudhomme says, in speaking of a hidden insurgent memory of this type: “C’est même cette réviviscence qui seule me permettrait de retoucher les vers que cette petite aventure, si ancienne, m’a fait commettre, et de faire bénéficier de l’expérience que j’ai acquise dans mon art l’expression de mes sentiments d’autrefois.” And he asks whether every memory of feeling does not assume a certain character of *hallucination*. Wordsworth (as Aubrey de Vere has told us, and as the sonnet “Surprised by joy,” shows) had very much the same experience. And Littré (*Revue Positive*, 1877, p. 660) describes what he calls the “affective automnesia,”—or spontaneously arising flow of emotion with which quite suddenly, and late in life, he remembered losing a young sister when he was ten years old: “Ce même événement s’est reproduite avec une peine non moindre, certes, que celle que j’éprouvais au moment même, et qui alla jusqu’à mouiller mes yeux de larmes.”<sup>1</sup>

This train of reflections, I think, well illustrates that kinship between the working of what is admitted as genius and the dreamlike subliminal mentation with which we are here dealing, of which I have often spoken, and to which I must again presently recur.

Turning now to the Hindoo pre-incarnation, we observe that it offers a linguistic problem of a rather different kind. Certain Sanscrit letters are written, and certain Sanscrit words are uttered;—mixed, it is true, with much quasi-Sanscrit gibberish, and not exceeding what a quick eye and memory might pick up in a few hours from a Sanscrit grammar. Hélène, however,—whose complete good faith is vouched for on all sides, and who herself undoubtedly believes with her whole heart in the spirit-hypothesis,—denies that she ever consulted or even to her knowledge saw, a Sanscrit grammar. Again, M. Flournoy’s careful researches have shown that incidents of the Indian *history*, or pseudo-history, on which the narrative of this incarnation turns, are

<sup>1</sup> See Ribot, *Psychologie des Sentiments*, p. 152.

undoubtedly derived from a particular passage in a rare and antiquated history of India by de Marlès,—which M<sup>lle</sup> Smith asserts that she never saw, and which it seems very improbable that she *should* have seen. This knowledge is worked up in a way indicating considerable familiarity with the East, and quasi-Indian tunes and gestures are employed with great verisimilitude.

I need not here go into the details of the more modern and accessible characterisation of Marie Antoinette.

In the facts which I have already given, we have got this problem reduced to its narrowest form ; and I shall set forth, as barely possible, a theory which Professor Flournoy has not invoked. I agree with him that the notion of the truth of the Indian romance must be quite dismissed. But I do not therefore think it certain that M<sup>lle</sup> Smith must have unconsciously seen de Marlès' history and a Sanscrit grammar.

On the contrary, I think it just possible that the knowledge of de Marlès and of Sanscrit may have been clairvoyantly acquired by M<sup>lle</sup> Smith's subliminal self,—or even may have been communicated to that subliminal self by a spirit which may have been concerned in the composition of these romances. I should defend this apparent absurdity somewhat as follows :—

If spirits do act upon human minds, they probably so act sometimes to amuse *themselves*, as well as to please or inform *us*. I know of no evidence, indeed, of their having any power to *injure* us, but I think that there is a good deal of evidence of tricky, playful interference. I think that a kind of literary impulse to write or act out *romances*, through the intermediacy of some human being, is perhaps one form of this mystifying intervention. I think that traces of such a phenomenon occur in the psychical history, for instance, of Mr. Stainton Moses, and of a lady quoted in these *Proceedings* as Miss A. The possibility can hardly be tested ;—but should, I think, be noticed here.

I pass on from these reincarnational romances to certain minor, but interesting phenomena, which Professor Flournoy calls *teleological automatisms*. These are small acts of helpfulness—*beneficent synergies*, as we might term them, in contrast with the *injurious synergies*, or combined groups of *hurtful* actions, with which hysteria has made us familiar. We have already printed several incidents of this type in our *Proceedings* and *Journal*. (See, for instance, the trivial but instructive case of Mrs. Verrall and the envelopes, *Proceedings*, Vol. XI., p. 191.)

“One day,” says Professor Flournoy (p. 35), “Miss Smith, when desiring to lift down a large and heavy object which lay on a high shelf, was prevented from doing so because her raised arm remained for some seconds as though petrified in the air and incapable of movement. She took this as a warning, and gave up the attempt. At a subsequent séance Leopold stated that it was he who had thus fixed



Hélène's arm to prevent her from grasping this object, which was much too heavy for her, and would have caused her some accident.

"Another time, a shopman, who had been looking in vain for a certain pattern, asked Hélène if by chance she knew what had become of it. Hélène answered mechanically and without reflection—'Yes, it has been sent to M. J.' (a client of the house). At the same time she saw before her the number 18 in large black figures a few feet from the ground, and added instinctively, 'It was sent eighteen days ago.' [This was in the highest degree improbable, but was found to be absolutely correct.] Leopold had no recollection of this, and does not seem to have been the author of this cryptomnesic automatism."

A similar phenomenon has also been noted (p. 87) when warning is conveyed by an actual phantasmal figure.

M<sup>lle</sup> Smith has seen an *apparition* of Leopold, barring a particular road, under circumstances which make it probable that M<sup>lle</sup> Smith would on that day have had cause to regret taking that route. (*Cf.* Apparition seen by lady near open lift; and warnings to Socrates to change his route. *Proceedings*, Vol. VIII., p. 345, and Vol. V., pp. 540–542.)

The next question is as to whether supernormal faculty of any kind is manifested in Hélène's phenomena. There does appear to be some telepathy (see p. 363, etc.), and of telepathy Professor Flournoy speaks as follows:—

"One may almost say that, if telepathy did not exist, it would be necessary to invent it. I mean by this that a direct action between living beings, independently of the organs of sense, is a thing so in accord with all that we know of nature that it would be difficult not to assume its existence *a priori*, even were no sign of it perceptible. How could one believe, indeed, that centres of chemical phenomena so complex as the nervous centres could find themselves in activity without transmitting various undulations,—X, Y, or Z rays,—passing through the skull as the sun passes through glass, and going on to act, at any distance, on their homologues in other skulls? It is a mere question of *intensity*. . . .

"If telepathy is considered strange, mystic, occult, supernormal, etc., it is because this character has been gratuitously conferred on it by making of this imponderable link between organisms a purely spiritual communication of soul to soul, independent of matter and of space. That such a metaphysical union does exist I am ready to believe, but it is to introduce a gratuitous confusion if one substitutes this problem of high speculation,—which abandons the strictly scientific ground and sets aside the principle of psycho-physical parallelism,—for the empirical problem of telepathy, which is perfectly concordant with that parallelism and in no way contradicts established science."

Now, of course, it has been obvious from the outset of our researches that it would be very desirable if we could trace some relation between telepathy and ether-vibrations. There are doubtless endless vibrations waiting to be explained ;—and telepathy is a phenomenon greatly in need of an explanation. The more complex any object is, moreover, the more strangely it will vibrate ;—and the more sensitive any object is, the more strangely will it receive and respond to vibrations.

Nevertheless when we have said this,—as Sir W. Crookes has said it with great impressiveness (*Proceedings*, Vol. XII., pp. 348–352),—we have said nearly all that can be said for the vibration-theory of telepathy. And yet the real difficulties of the subject are only beginning.

It is, of course, plain that if we are to get any true notion of telepathy we must in some way *generalise* from our own petty experiments and limited observations. No one supposes that the few emergent cases which happen to have become accessible to our view comprise the whole range of what must by its very nature be a great fundamental law.

Now the generalisation which the vibration-theory calls on us to make is as follows: Ether-vibrations are constantly emanating from every human brain, and every human brain is capable of receiving such vibrations and of representing them in consciousness.

These vibrations are assumed to radiate in all directions equally (as all ether waves must do), and to be unaffected by earthly distances or earthly objects ; but nevertheless only to become perceptible when they impinge upon a brain which is fitted to receive them either by emotional sympathy or by some natural sensitivity. Their action on the percipient's *brain* is assumed to be instantaneous ;—although we may allow some period of latency before they represent themselves in his *consciousness*. Of course, the more this latency is insisted on, the less plausible does the vibration-theory become.

Again, the vibration must represent itself to the percipients as a pattern which must in some way resemble the pattern which emanated from the agent. Thus, to recur to our early experiments with cards, our old friend the two of diamonds is bound to be the two of diamonds still ; or if it cannot come out as the two of diamonds exactly, it yet ought to get as near it as it can, and to appear, say, as the two of hearts. When the idea to be communicated becomes more complex,—and most ideas *are* more complex than the two of diamonds,—we may fairly allow a wider range of similitude. The vibration-pattern received is supposed to be modified by the recipient brain ;—modified often out of any *pictorial* resemblance to any impression which we can suppose the agent's brain to have despatched. The great extent of such apparent modification, indeed, does not affect the vibration-theory alone, but any

theory of telepathic impact as opposed to excursive presence, or *invasion* on the agent's part. Gurney repeatedly, in *Phantasms of the Living*, discussed this topic, and especially that form of difficulty which lies in the habitual appearance of the agent's *person* to the percipient ;—albeit the agent, at a moment of death or crisis, is not likely to be thinking of the aspect of his own body. Gurney argues, however, that a sense of one's own aspect is deeply rooted in all our minds, and may naturally be represented to others as a part of any message which comes from our unconscious strata. But such a theory, difficult in any case, has to be greatly strained if we are to suppose that a pattern of brain-vibrations which for the agent represent, say, the shock of the rupture of an aneurism, become for the percipient a picture of a smiling gentleman dressed for walking, with great coat and cane. (See *Phantasms of the Living*, Vol. I., p. 210, Case of N. J. S.)

A still greater difficulty for the vibration-theory is presented by *collective* telepathic hallucinations. It is hard to understand how A can emit a pattern of vibrations which, radiating equally in all directions, shall affect not only his distant friend B, but also the strangers C and D, who happen to be standing near B ;—and affect no other persons, so far as we know, in the world.

The above points have been fair matter of argument almost since our research began. But as our evidence has developed, our conception of telepathy has needed to be more and more generalised in other and new directions,—still less compatible with the vibration-theory. Three such directions may be briefly specified here ;—namely, the relation of telepathy (*a*) to telæsthesia or clairvoyance, (*b*) to time, and (*c*) to disembodied spirits. (*a*) It is increasingly hard to refer all the scenes of which percipients become aware to the action of any given mind which is perceiving those distant scenes. This is especially noticeable in crystal-gazing experiments. (*b*) And these crystal visions also show what, from the strict telepathic point of view, we should call a great laxity of time relations. The sryer chooses his own time to look in the ball ;—and though sometimes he sees events which are taking place at the moment, he may also see past events,—and even future events. I at least cannot deny *precognition*, nor can I draw a definite line amid these complex visions which may separate precognition from telepathy. (*c*) Precognition itself may be explained, if you will, as telepathy from disembodied spirits ;—and this would at any rate bring it under a class of phenomena which I think all students of our subjects must before long admit. Admitting here, for argument's sake, that we do receive communications from the dead which we should term telepathic if we received them from the living, it is of course open to us to conjecture that these messages also are conveyed on ether-waves. But since those



waves do not at any rate emanate from material brains, we shall by this time have got so far from the original brain-wave hypothesis that few will care still to defend it.

I doubt, indeed, whether we can safely say of telepathy anything more definite than this : *Life has the power of manifesting itself to life.* The laws of life, as we have thus far known them, have been only laws of life when already associated with matter. Thus limited, we have learnt little as to Life's true nature. We know not even whether Life be only a directive Force, or, on the other hand, an effective Energy. We know not in what way it operates on matter. We can in no way define the connection between our own consciousness and our organisms. Just here it is, I should say, that telepathic observations ought to supply us with some hint. From the mode in which some element of one individual life,—apart from material impact,—gets hold of another organism, we may in time learn something of the way in which our own life gets hold of our own organism,—and maintains, intermits, or abandons its organic sway.

Most instructive of all will it be if we can obtain telepathy from discarnate spirits,—and especially if we could get any glimpse of a relation between their mode of being and the cosmic ether. On this point Professor Flournoy writes as follows (p. 394):—

“It is obvious that the hypothesis of spirits involves no *à priori* impossibility or absurdity. It does not even contradict, as is sometimes supposed, that fundamental law of physiological psychology—the psychophysical parallelism—which insists that every mental phenomenon must have a physical correlative. For in spite of our habit of considering the molecular or atomic phenomena of the brain, the catabolism of the neurones,—as the true concomitant of the conscious processes, it is quite possible—it is even probable enough—that these molecular movements do not constitute the ultimate physical term immediately adjoining the mental world (*côtoyant le monde mental*), but that the true physical or spatial correlatives of psychological or non-spatial phenomena ought to be sought in the vibrations of that imponderable matter, the ether, in which ponderable atoms and molecules are plunged somewhat after the fashion of grains of dust in the atmosphere.”

I quote these words because,—obvious as their contention must seem to all thinking persons,—it is common enough to see phrases used as though our notions were still bounded by the molecular;—as though we did not *know*, as certainly as we know anything, that the great mystery of existence is only just beginning, in that inconceivable world of ether, precisely where our utmost analysis fails us, and our mathematics are reduced to a jungle of infinities and of contradictions.

And now as to the question of possible telepathy from the dead in Hélène's case. The instance with most in its favour is described by Professor Flournoy as follows (p. 406) :—

In a sitting at my house (February 12th, 1899) Mlle Smith has a vision of a village on a height covered with vines ; she sees a small old man coming down thence by a stony road. He looks like a “demi-monsieur” ;—buckled shoes, large soft hat, shirt-collar unstarched, with points rising to his cheeks, etc. A peasant in a blouse whom he meets bows to him as to a personage of importance ; they talk a *patois* which Hélène cannot follow. She has an impression that she knows the village ; but she cannot identify it. Soon the landscape disappears, and the old man, now clothed in white and seen in a luminous space [implying that he is in the next world] seems to come nearer. At this moment, as she sits with her right arm resting on the table, Leopold dictates with the forefinger, *Lower her arm.* I obey ; Hélène's arm at first resists strongly ; then yields at once. She seizes a pencil, and during the usual struggle as to the way to hold it [*i.e.*, whether in her own habitual fashion,—between forefinger and middle finger,—or in the ordinary way] “you are squeezing my hand too hard !” she cries to the imagined little old man who, according to Leopold, wishes to write through her ;—“You hurt me ; don't press so hard ; what can it matter to you whether it is a pencil or a pen ?” Then she drops the pencil and takes a pen, and holding it between thumb and forefinger writes slowly in an unknown handwriting, (*Chaumontet syndic*). Then returns the vision of the village ;—we wish to know its name ; and she ends by perceiving a guide-post on which she spells out *Chessenaz*, — a name unknown to us. Finally having, at my desire, asked the old man the date when he was syndic, she hears him answer, 1839. Nothing more can be learnt ; the vision disappears and gives place to a possession by Leopold, who in his big Italian voice talks at length about various matters. I question him on the incident of the unknown village and syndic ; his answers, interrupted by long digressions, are to this effect : “I am looking — I turn my thoughts along that great mountain with a tunnel in it whose name I do not know [Leopold,—the *soi-disant* Cagliostro,—who returns from the 18th century, is naturally not well up in modern geographical names ;—but this is the hill of Fort de l'Ecluse] ; I see the name of Chessenaz,—a village on a height,—a road leading up to it. Look in that village ; you will find the name [Chaumontet] : try to verify the signature ; you will get a proof that the signature is really that of this man.”

I ask him whether he sees all this in Hélène's memories—“No” ;—or whether she has ever been at Chessenaz :—“Ask her ; she will know ; I have not followed her in all her excursions.”

Hélène, when awake, could give no information. But next day I found on the map a little village of Chessenaz in the department of Haute-Savoie, at 26 kilometres from Geneva. . . .

A fortnight later, Helen sees the vision of the other day reappear ;—the village, the little old man ;—but accompanied by a *curé*, who seems intimate with him, and whom he calls “my dear friend Bournier.” Leopold promises that this *curé* will write his name for Helen.

At the next sitting in my house, March 19th, I remind Leopold of this promise . . . The *curé* at last takes her hand as the syndic had done, and writes very slowly the words *Burnier salut*.

I wrote to the Mairie at Chessenaz, and the Mayor, M. Saunier, was good enough to answer me at once. "During the years 1838 and 1839," he said, "the syndic of Chessenaz was Jean Chaumontet, whose signature I find in various documents of that date. We had also for *curé* M. André Burnier, from November, 1824, to February, 1841, during which period all the *actes des naissances*, etc., bear his signature. But I have found in our archives a document with both signatures, which I send you."

[Reproductions of actual signatures, and of signatures given by M<sup>lle</sup> Smith. The handwritings were markedly similar.]

Professor Flournoy's first idea naturally was that M<sup>lle</sup> Smith had seen at some time or other some acts or documents signed by the syndic or the *curé* of Chessenaz, and that these visual impressions had reappeared in her somnambulic state, and had served as internal models for the signatures which she traced in trance.

Careful inquiry, however, made it clear that M<sup>lle</sup> Smith (whose probity, I may here repeat, is beyond question) had never been at Chessenaz,—still less consulted its parish archives. Whether the handwriting of its syndics, before the cession of Savoy to France, could ever have fallen under her eyes in some old record or certificate belonging to friends in that neighbourhood is a question incapable of absolute proof.

As the matter now stands I cannot claim that any true communication from the departed has as yet been established for M<sup>lle</sup> Smith. Her case is still on the verge of proof.

And here I shall of course be met with the inquiry: What influence, then, must the case of M<sup>lle</sup> Smith have upon your interpretation of the case, say, of Mrs. Piper? Can you maintain that there is a fundamental difference between this elaborate case of pseudo-possession, and that other case, so closely resembling it, in which you claim that George Pelham was no mere personification effected by Mrs. Piper's subliminal self? Have you really a criterion by which to judge the two groups of phenomena? and can you explain how it is that, if fundamentally different, they are superficially so much alike?

My answer to the first question must be, as it has ever been, that we *have* a criterion,—consisting not in the manner in which the messages are given, but in the actual evidential content of those messages themselves. I claim that, whereas in M<sup>lle</sup> Smith's messages there is hardly any real indication of an individual and recognisable focus of memories, belonging to a human person, and such as no other known person could simulate; in George Pelham's case on the other hand there *is* such indication;—pushed so far as to render it highly improbable that that focus or chain of memories is decomposable into fragmentary



recollections,—is a mere aggregate of scraps of knowledge telepathically gathered from many quarters, and subliminally fused into an apparent unity. I am, in short, simply more confirmed than ever in my conviction (as against the mere facile admissions of spiritist writers) that a strict and thorough analysis of automatic messages is absolutely essential,—and will result in showing the genuineness of a very few of these messages by dint of their intrinsic evidential superiority to all the rest.

To the *second* question asked above,—as to the explanation of the *similarity* between pseudo-possession and true possession, I reply that from my point of view it was to be expected that such cases should occur,—that the action of subliminal phases of personality upon the organism should reach a point of close resemblance with the action of external spirits upon the organism.

I regard the human organism as an entity whose life, as apart from any informing spirit, is beyond our intellectual grasp. Whatever independent life the cells of my organism may possess, I can only regard that organism as a machine, which my own spirit habitually works—partly supraliminally, partly subliminally,—but which may be partially worked also by some other incarnate spirit or by a discarnate spirit. Each of these controlling agencies necessarily controls the organism in much the same way;—that method being determined in fundamentals by the structure of the organism itself;—in detail by the supraliminal education which I happen to give to the organism. Thus the power of making cries and gestures inheres in the structure of the organism: I educate this power into speech and writing, which I use for ordinary conscious purposes.

Now suppose that some submerged phase of my personality gains the power of manifesting itself, it will have to use the accustomed methods. It may not, indeed, be able to use all of them. It may not, for instance, be able to *speak*. Or it may not have the whole of my *knowledge* at its command. Thus, to take the weakest case of subliminal control, a fraction of myself uses my organism in a dream, but can neither make me write nor make me speak,—although it may cause restless gestures. Nor has the dream command over more than a small fraction of my *knowledge*; and the knowledge over which it has command may be knowledge taken from a far-back period of my history.

And now suppose that another incarnate spirit telepathically influences my organism. In order to do this it can use practically the same mechanism which a dream can use. It can affect me with sensory hallucinations, or it can prompt me to brief utterances and gestures, like those of somnambulism. It cannot, as a rule, do much *more* than this, as our telepathic cases show. Yet sometimes, especially

if the agent is asleep, he can influence the percipient to automatic writing in a rather more prolonged way.

Let us pass on to the further hypothesis that a discarnate spirit is influencing the organism. We shall now, at any rate, have something like analogies to which to appeal. Falling back upon known phenomena, we can suggest, for example, that the possessing spirit may act like an extraneous *idée fixe*, developing into an extraneous secondary personality. It is, indeed, through the working out of this parallel—here merely hinted at—that we may best be able to trace, as I hope may be some day possible, a real continuity between subliminal and extraneous influences upon the ordinary personality;—and to discover, perhaps, that spirit-control (or pneumaturgy) may play the same part in expanding our ideas of multiplex personality as hypnotism has already played in expanding our ideas of hysteria and somnambulism.

Deferring such discussion till a more fitting moment, I will conclude with a few general reflections which this remarkable case of Professor Flournoy's,—nay, which even some of Dr. Janet's morbid cases,—seem to me to suggest to the psychologist who cares to look beyond either the narrowing hospital-walls or the perplexing séance-room.

The chief reflection is that we are as yet but in a childish or rudimentary stage of our use of the human brain. That brain is in truth (as some one has said) but “a virtual organ”;—an organ *en voie d'acquisition*, which man is slowly learning to use,—but with very little notion either of how it came to be what it is, or of what it ultimately may be.

No one, indeed, knows how it came to be what it is. No one has even plausibly explained that great difference between the simian and the savage organ which seems so incommensurate with any observable difference between the hairy chimpanzee and the hairy Aino. Some naturalists argue that much of the savage's brain-power must lie dormant;—must somehow be a mere reserve for the later calls of a more complex existence. If this be admitted, we must surely go on to admit that a large proportion of the brain's capacity is dormant still.

I do not mean to predict that our race will necessarily include in the future individual minds more powerful than any minds in the past. This question ultimately depends upon that unknown factor,—the nature of the sources from which spiritual life is infused into mortal organisms. Let us admit that judging by the last two thousand years we have little ground for confidence in any such advance of the highest individual level. Let us not be rash enough to prophesy that there will ever be a sculptor better than Praxiteles, or a lyric poet better than Pindar. Yet the important fact is that the great spirits of the past made use only of certain portions of the human brain which had through some historic cause been the first to be

developed. The spirits of Pindar or Praxiteles dwelt, so to say, in the palace-chambers of their mysterious habitation; they knew nothing of the foundations on which the City of Mansoul was builded, or of its treasure-houses in the living rock.

Of course Hélène's intellect, though serviceable enough in common life, is not of a calibre to confer new knowledge or delight on educated men; but for Hélène's one-in-a-hundred mind substitute the one-in-a-million mind of Louis Stevenson;—let him dream—not Hélène's insipid tale of "Esenale," but "Dr. Jekyll and Mr. Hyde," and one sees at once the advantage of relegating voluntary ends to automatic execution,—and so at once saving brain-centres from expensive friction and getting the needed thing done (like those reaction-time measurements of Dr. Janet's during ecstasy), with a verve and a completeness which conscious effort finds it hard to rival. May not that effortless concurrent utilisation of all elements of the personality hold ultimately somewhat the same relation to our present painful thought as multiplex telegraphy holds to the old slow and single transmission along the wire?<sup>1</sup>

Let us frankly confess that we cannot in the least tell what the brain will do till we try. The assumed balance of our faculties and our environment is altogether chimerical. The environment is unknown and infinite, and the faculties must simply be trained as far in every direction as they can. We have attained no position of permanent stability, and we have nothing to tell us *à priori* what will be the next faculties which our race will evolve. And yet popular science sometimes speaks as though nearly everything in human nature had been observed already!—as though normality had been defined, aberrations classified, a mass of experience acquired which our successors will only have to work out in detail! A vain conceit! a monstrous prematurity! Rather let us remember that only by an abiding consciousness of our own inevitable childishness can we prevent those successors from looking on our religions with pity, and on our science with contempt;—while they analyse with a smile our rudimentary efforts at self-realisation,—remarking "how hard a thing it was to found the race of man."

<sup>1</sup> The same characteristics were observable in M<sup>lle</sup> Smith's other forms of mental and bodily effort. "It may be remarked," says Professor Flournoy on this topic, "that the drawings, embroideries, and other artistic works which were at all times the favourite occupation of her leisure, and in which she excels, were almost always, from her childhood onwards, not copies of external models, but products of her own invention, marked with the original and quaint characteristics of her inward pictures. Moreover, these pieces of handiwork wrought themselves out under her fingers with an ease and a rapidity which astonished herself;—they *worked themselves*, so to say."



## II.

## REVIEWS.

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*The Scientific Investigation of Physical Phenomena with Mediums.* [Mediumicheskiya Fizicheskiya Yavleniya i ikh Nauchnoe Izsledovanie.] By M. M. PETROVO-SOLOVOVO. (St. Petersburg, 1900.)

The name of Mr. Petrovo-Solovovo, now Secretary of the S.P.R. for Russia, is well known to our members. He has often contributed to our publications, and his judicial and acute mind marks him out for a valuable inquirer into psychical problems. We heartily welcome his new book—the most important publication which he has yet essayed in this region. It is strictly confined to a review of the work which has been done in the scientific investigation of physical phenomena, and discusses no theories; it hardly even shows leanings. It is, in short, at once a most useful review of the reports of previous observers and a criticism of their methods; and it contains, moreover, a certain amount of hitherto unpublished observations. Most of these refer to the Russian medium, Sambor, but we are glad to see a chapter of experiments from the pen of our veteran corresponding member, Mr. Aksakoff, to whom the book is dedicated. To Sambor we will return presently, first giving a short *résumé* of the rest of the book.

In his Introduction Mr. Solovovo tells us at once that he has (most wisely) limited his field by rigorously excluding all reports which seemed inadequately guaranteed either by the competence of the observers or the conditions of their experiments. Mr. Stainton Moses, for instance, appears only in a footnote, on the ground that faith in his reports is solely founded on personal belief in his veracity. On the other hand, Mr. Solovovo does not exclude experiments with mediums detected in fraud—to have done so would indeed have reduced his work almost to vanishing point; but while duly mentioning such detection in the case of the mediums who have been proved fraudulent, he urges that the fact, while calling, of course, for redoubled vigilance, does not invalidate experiments where the conditions have been really satisfactory. He discusses very soberly and wisely the difficulties of the inquiry, and, after rejecting all *à priori* objections to the possibility of the asserted phenomena, admits that all previous experimenters, with the single exception of Crookes, have left something to desire; but this he urges not as an argument against the facts themselves, but as a summons to further and more scientifically conducted experiments. The spirit of his Introduction is, in short, thoroughly scientific; he himself says that he feels himself further from the thorough-going spiritist than from the hardened sceptic.

From these preliminary considerations Mr. Solovovo passes to the record of the facts themselves. Most of them, if not familiar, are at least accessible

to English readers. He speaks, in chronological order, of de Gasparin's experiments on the movement of tables without contact in 1854, Hare's in 1855, and the Report of the Committee of the Dialectical Society in 1869—a body of observers, by the way, who seem to be unique in the facility with which they obtained physical phenomena almost *ad libitum* with unpaid and unprofessional mediums. On this follows an account of Sir William Crookes's experiments, especially those with Home—a series of observations which, as Mr. Solovovo remarks, still, after thirty years, stands quite alone in its value as scientific evidence for the phenomena in question. Next comes an account of Zöllner's experiments with Slade, with especial reference to “passage of matter through matter” in the production of knots on closed loops of string, and in this chapter, as confirmatory of Zöllner's results, are included the Russian experiments with Sambor. Mr. Solovovo then gives a brief account of some experiments made in 1883 by Mr. Aksakoff with Mrs. Fox-Jencken, hitherto unpublished. In this case the medium's hands were held by one of the sitters, and were further rendered visible by being laid upon a surface of luminous paint. Under these conditions objects at a distance from the medium were moved, a small bell, while floating in the air, was visible owing to the luminous paint with which it was coated; the sitters were touched by hands not belonging to any of them—hands of which the faint outline could be seen against the various luminous surfaces, and of which marks were left upon smoked paper not within the medium's reach. We then go on to an account of the inquiry into “direct writing” on slates carried out by the S. P. R., where Mr. Davey's experiments played so remarkable a part. The importance of these Mr. Solovovo fully recognises; but he is still of opinion that the testimony of Mr. Emmette Coleman and Professor Coues, who say that they saw such writing actually going on before their eyes at a séance with Mrs. Francis, must weigh sufficiently to establish the possibility of the phenomenon. And when he urges that those members of the Society who hold that Davey's experiments render worthless all evidence dependent on continuous attention should have refused to investigate Eusapia Paladino, it must be admitted that he has logic on his side. He is however hardly correct in supposing that this criticism affects Mr. Hodgson, who took part in the Cambridge experiments not because he thought them of any value, but at the request of the S.P.R., because he was convinced that he could demonstrate their worthlessness.

An account of Eusapia herself occupies the last and longest section of the book. Mr. Solovovo himself was present at some fifteen sittings with her, first at Milan and afterwards at St. Petersburg. His own evidence, however, he modestly relegates to a footnote, admitting that he was not satisfied with the way in which her hands were held on most occasions. But after a full consideration of the results of the Cambridge sittings, he comes unhesitatingly to the conclusion that in spite of her proved deceit, many of her phenomena are genuine; his belief is founded mainly upon M. Richet's gradual progress from scepticism to conviction, and on Mr. Myers's final adhesion after the Cambridge experiments had led him to avow his disbelief.

But the special interest of the book for Western readers lies rather, as I have already intimated, in the detailed accounts of experiments with Sambor. Some of these have already appeared in the *Annales des Sciences Psychiques*;

but much at least of the following is, I believe, inaccessible except in Russian, and deserves translation as a typical sketch of Sambor's phenomena, though from the point of view of evidence it leaves something to be desired. The first portion of this very remarkable narrative is supplied by Dr. Fischer from his own notes; the remarks interposed are in Mr. Solovovo's words.

*Séance of April 13th, 1894, in the rooms of Mr. S. A. Bezsonoff.*

The séance began at 9 p.m. A circle of fourteen persons, excluding the medium, joined hands, holding them on their knees. In the middle of the circle stood a small oval table, on which were laid a guitar; a handbell; two slates tied together with string, the ends of which were sealed to a piece of cardboard (a piece of slate pencil was placed between the two); a sealed envelope containing a piece of pencil and a clean sheet of paper, one corner of which had been torn off and kept by Mr. F. (two members of the circle signed their names on the envelope); and a piece of string, both ends of which were sealed to a card with the seals of two persons present. The medium, having been searched, sat with his back to the fireplace, and Messrs. Bezsonoff and V. took charge of him. Those present began to sing, and the lights were then put out. By the heavy breathing of the medium it was clear that, with brief intervals, he fell some four or five times into a trance, during which luminous stars appeared, chiefly on his right side. The sitting lasted for half an hour, and, after an interval, was resumed at 10 p.m. under the same conditions, except that Mr. V.'s place next the medium was taken by Mr. Tour. The light was again put out. Luminous stars began to appear frequently. The medium stood up in a trance, when there was a sudden tinkling on almost all the strings of the guitar. This continued audible for some time, as though some one were running fingers over the strings. A piece of glass coated with luminous paint lying on the table was thrown to the ground. Then there was a sound as though something dropped on to the table and thence to the ground—a piece of stearine candle, as appeared later. At the same time a musical box standing on the chimney-piece behind the medium began to play of itself; it had run down at the beginning of the séance and had not been rewound. An interval was taken at 10.30 and the sittings resumed at 11. Those present rearranged their seats and the medium was placed with his back to the door leading to the next room. The same pair as before took charge of the medium, and in addition Mr. Tour's right hand was fastened to the medium's left by a tape wound twice about the medium's wrist and fastened with fixed knots. Mr. Bezsonoff held the medium's right hand in his left. The light was put out. After a short time the medium began to shiver and move about. He was sitting on an Austrian chair, and began gradually to move further and further away from the table, so that the rest had to extend the chain towards him. The medium suddenly fell to the ground and began to snore loudly and groan. The two who were holding his hands declared that he rose into the air, drawing their hands after him, and that they could not find his legs. At this time Mr. Tour moved his leg round over the part of the floor where, to judge by the position of the medium's hands, his legs should have been, but the space was vacant. When Mr. Tour mentally desired to



know where the medium's legs were, he suddenly felt on the hand which was bound to the medium the soles of two feet, which convinced him that the medium was really in the air. On the other side Mr. Bezsonoff declared that the medium was upon him, and was touching him with his hips. The medium then came down to the ground and again rose into the air with his chair. Mr. Bezsonoff, who held his right hand, immediately announced this, and drew forward his right-hand neighbour's hand for him to feel the chair in the air. During all this time there were two or three flashes of stars. After this Mr. Bezsonoff declared that an Austrian chair was hanging by the back on the hand by which he was holding the medium, and had slipped on to it from the medium's hand. The medium then fell with violent convulsions on to a sofa close by, drawing his two neighbours with him, and while still in a trance called for light. As soon as it was turned on the whole party saw an Austrian chair passed by its back over Mr. Bezsonoff's left hand, with which he was holding the medium's right; the medium was in a trance.

After an interval of half-an-hour, the sitting was again resumed at twelve midnight, with a fresh arrangement of sitters. The medium sat with his back to a writing-table, between the same neighbours. Mr. Tour's right hand was again fastened to the medium's left. At Mr. Tour's request, the guitar was laid on the writing-table outside the circle. The medium sat on a heavy chair of ash-wood, which was brought in from the entrance hall. The light was put out and ringing began. One of us expressed a wish to hear sounds on the guitar, but stars flashed two or three times behind the medium instead. The medium fell in a trance off the chair, snoring and struggling. His right hand neighbour, Mr. Bezsonoff, declared that, without losing hold of the medium, he had got the chair on which the medium was sitting passed over his arm by the back; and the next minute said that the chair had been taken off again. One or two minutes later the medium's left-hand neighbour declared that the same chair had been passed by the back over his arm which was tied to the medium. At the same instant all the strings of the guitar twanged as though some one had passed a finger over them. The medium in trance called for light. When it was turned on, all saw the above named chair with its back passed over Mr. Tour's arm which was fastened to the medium. The séance closed at 1 a.m. Among those present was Mr. Aksakoff.

Dr. Fischer, in a letter to me of July 29th, 1899, adds:—"The medium was under very careful control, as the chair experiment was not unexpected, but was particularly looked for, though we were by no means sure of success. During the sitting, while Mr. Tour had his right arm fastened to the medium's left, and in addition was holding the latter's hand in his own, we kept on asking him if he was holding on, or noticed any attempt of Sambor's to get free from the fastening. Mr. Tour definitely and categorically asserted that no such attempts were made by Sambor, and that he was holding his hand very firmly all the time. The same question was constantly addressed to Mr. Bezsonoff on the other side, and he too constantly replied that the hand of which he had charge never escaped for an instant from his own."

We have been favoured with a very curious and detailed account of the same phenomena by Mr. Tour. After some general observations on the

composition of the circle which took part in the séances with Sambor in the spring of 1894, and on the occurrences observed, Mr. Tour continues :—

“One peculiarity of all the experiments carried on with Sambor is his extreme restlessness when entranced. He is constantly twitching, cracking his joints, and producing groans which are obviously genuine ; he sways from side to side, and slips from his chair to the floor. It is needless to say that with a man of Sambor’s youthful and rather powerful frame such experiments are very fatiguing for both his neighbours. Moreover, the close atmosphere throws him into a violent perspiration, and the moisture on his wrists, in spite of all the efforts of those who are holding him, often enables him to slip them free for a moment. This possibility inevitably undermines an observer’s faith in any phenomena where there is room to suspect that the medium’s hand may have been employed in a moment of freedom. This was emphatically the case with myself in one of the sittings at Mr. Bezsonoff’s in the spring of 1894, when I was holding the medium’s hand and Mr. Bezsonoff his right.

“When, in the midst of Sambor’s convulsions and struggles, I suddenly felt a slight pressure on the right arm with which I was holding the medium’s left, I immediately called for light. I then saw, like the rest, an Austrian chair hanging by the top bar on my right arm below the shoulder. It was one of the chairs which had been put round the table for the sitters, who had been obliged to stand up owing to the medium having risen to the ceiling, as they must otherwise have broken the chain.

“It is right to point out that, in our previous sitting that evening, a chair had been hung in precisely the same manner on Mr. Bezsonoff’s arm ; a fact to which some of those present wished to attribute great importance. Now, when exactly the same thing happened to me, I remembered for certain that during the trance and levitation of the medium his hand had more than once slipped out of mine for a moment. Of this I told those who were present, explaining that in my opinion no weight should be laid on the appearance of the chair on my arm, as it might have been seized by the medium during one of the moments when his hand was free, and dropped upon my arm when the circle was reformed. I remember that my shameless incredulity was attacked without mercy by others present, and Mr. Aksakoff and Mr. Pribytkoff, if I am not mistaken, tried to convince me, from their experience of spiritism, that similar phenomena were common enough, especially with weak mediums ; and that if anyone doubted, we should try to repeat the experiment, but this time with hands bound together, which would do away with all doubt.

“I accepted the proposition with pleasure, and once more we all sat round the table in a circle. I took control of the medium on the left, and my right hand and wrist were tied to Sambor’s left with a plain new tape, fastened with several knots. Bezsonoff took control of the right side, but his hand was not tied like mine. The light was put out, and we waited.

“The usual sparks began to appear, floating in curved lines through the air. The medium fell as before into a stormy trance, with convulsive movements ; he then rose to his feet, and at this moment I felt some sort of touch on my right arm near the shoulder. As I could not use my hands without breaking the circle, I bent my head down to the right, and with my temple struck something hard below my shoulder ; this I announced, and asked

for light. It was turned on, and there, hanging on my right arm, was a chair—not a drawing-room chair, but a heavy arm-chair from the hall; one that had not at the beginning of the séance been in the study, the door of which had remained all the time carefully closed. The fastening by which my hand was attached to the medium's remained perfectly intact, just as it had been put on; the knots were perfect, and the efforts with which they had been drawn tight were not trifling, as the livid mark of the tape round my wrist remained visible for several days. The whole of this part of the sitting did not last more than four or five minutes.

“Of course I was amazed, and my incredulity was put to shame, though it would not capitulate without an effort. The first idea that flashed across me was that there might have been some remissness on the part of the medium's right-hand neighbour, whose hand was not fastened to Sambor's as mine was. In that case the medium, I thought, might have caught the chair (on the appearance of which through the closed door of the study I did not dwell) with his right hand, and somehow or other got through the aperture in the back, so as to pass it on to my arm. I now sincerely regret the slur which I thus cast upon Mr. Bezsonoff, whose interest in the investigation of truth was certainly no less than my own; but facts or appearances which strike at the root of convictions which have grown up with us from infancy are not to be accepted without a struggle of the inner consciousness. In my blind scepticism I tried to pass the back of the arm-chair over Sambor; but the efforts of three of us only succeeded in getting the upper aperture  $3\frac{1}{2}$  in. above his knee, beginning from the ground; further up the thighs we could not move it, for the dimensions of the aperture were only  $10\frac{1}{2} \times 8\frac{3}{4}$  in. It was perfectly clear that in order to pass the aperture in the chair-back over the body of the medium it would have been necessary to slice off 2 or 3 in. of both his sides from shoulders to thighs, and in addition to prune off his right arm!

“Foiled in this, I had recourse to another supposition: how was the chair made? Was there any secret flaw in it? We all set about examining it, but after the most rigorous search could find no ingenious joints or secret springs of any sort. The chair was just a plain, high-backed arm-chair of ash-wood, such as is commonly to be seen in entrance halls, and sometimes in dining-rooms.

“There was nothing for it; scepticism had to sing small and make the best of the fact, however strange at the present stage of our knowledge of nature.”

Mr. Tour concludes with an expression of his conviction that none of the phenomena he saw with Sambor require, or even suggest, any “transcendental,” supernatural, or spiritistic explanation. He regards them as manifestations of a still unknown force innate in the medium, and perhaps capable of development.

Mr. Solovovo points out that the narrative of Mr. Tour in some points differs from that of Mr. Fischer, but not in matters which touch the essence of the problem. The most noteworthy, of course, is the discrepancy about the chair found on Mr. Tour's arm. In Dr. Fischer's notes, written at the time, this is the chair on which the medium had been placed when the circle



was formed : in Mr. Tour's, written five years later, it becomes an "apport," brought into the room through a closed door. We have evidently another instance of the tricks played by memory even on the most conscientious witnesses, and a warning of the little value of all that is not set down in black and white at the moment.

It only remains to congratulate Mr. Solovovo on the thoroughness and skill with which he has marshalled his facts, and on the sobriety and caution of his criticism. He has of necessity laid the greatest weight on the personal standing and scientific reputation of his witnesses ; but it must not be forgotten that no real solid foundation will have been laid for the scientific establishment of these investigations till the mass of them is so great that this personal element is finally eliminated. This will, to all appearance, be a long task ; but Mr. Solovovo has contributed a valuable stimulus to the necessary patience and perseverance in inquiry.

WALTER LEAF.

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*L'Inconnu et les Problèmes Psychiques.* Par CAMILLE FLAMMARION. (Paris, 1900.)

M. Camille Flammarion is known in this country not merely as the author of numerous works on astronomy, scientific and popular, and several philosophical romances, but also as one who has almost from the outset taken a keen interest in the problems of Spiritualism and those other debatable phenomena which form the subject matter of psychical research. His card of election to the "Société Parisienne des études spirites," signed by Allan Kardec himself, is dated, he tells us, November, 1861. Between thirty and forty years ago his name was very familiar in this country as that of a scientific man who had borne testimony to the reality of the physical phenomena of Spiritualism and as an adherent of the Reincarnation doctrine then current among the French Spiritists. And if his utterances on such matters have of late years been less frequent and less pronounced, it is not apparently from any diminution of his general interest.

The present work contains the results of an appeal, avowedly suggested by our own Census of Hallucinations, inserted in the *Annales Politiques et Littéraires* and other Parisian papers. The questions, slightly differing from those employed in our own census, which were addressed to the readers of the *Annales*, ran as follows :—

(1) *Have you, at any time, experienced when awake, a clear impression of seeing a human being, or hearing him, or being touched by him, without being able to refer this impression to any known cause ?*

(2) *Did such impression coincide with a death ?*

In the event of having had no such impression, readers were asked to write *No* on a postcard, and sign it with name or initials.

In the result M. Flammarion received 4,280 replies—2,456 negative and 1,824 affirmative. Of these latter he has selected several hundred for publication in the present work.

Of course, in writing (p. 88) that his *questionnaire* is analogous to the Census of Hallucinations conducted by a Committee of the S.P.R., M. Flammarion is under a misconception. The objects achieved by that

Census were, no doubt, manifold ; but two of the primary aims were to ascertain the *proportion* of sane persons who had experienced hallucinations, and of such hallucinations to ascertain the *proportion* which coincided with some outside event, chiefly death. It is obvious that neither of these aims is met by M. Flammarion's *questionnaire*. Clearly a very small proportion of those who had had no hallucinatory experience would spend two sous and some trouble on the despatch of a postcard. And, again, of those who had experienced hallucinations, such whose hallucinations were, on the face of them, significant, would naturally be much more likely to take the trouble to write them out for the gratification of a stranger. Our own work in the Census has, of course, familiarised us with these considerations ; but M. Flammarion has not apparently realised their importance, for in one place (p. 222) we find him arguing as follows :—

“If these things were hallucinations, illusions, freaks of the imagination, the number of those not coinciding with a death would be considerably greater than the number which do so coincide. Now, we find the contrary has been the case. My inquiry proves it to demonstration. I asked my readers to be good enough to send me all cases, whether coincidental or not. [Of the cases sent] there were not more than seven or eight per cent. of apparitions without coincidences. Precisely the reverse ought to have been the case if we were dealing with hallucinations.”

The last sentence puts the case in a nutshell. The reverse ought to have been the case, and if M. Flammarion's inquiry had been a real census, he would no doubt have found that the reverse is the case, in France as in England.

It must sound ungracious thus to criticise a book which is a substantial and most acceptable contribution to our knowledge of this obscure subject. But so much was necessary at the outset ; and before proceeding to quote some of the illustrations of telepathy given in the book, some further words of caution are necessary. M. Flammarion, as he tells us, received over 1800 affirmative answers. The mere work of reading through and assorting this mass of correspondence must have been considerable. It would clearly have been a most laborious task for one man to undertake the further correspondence and minute inquiries necessary to raise these preliminary accounts to the evidential standard which the S.P.R. has learnt in the course of eighteen years' investigation to set before itself. The appeal was issued only in the spring of last year. If M. Flammarion had undertaken, singlehanded, to write in all cases where necessary for further details, to examine into the question of other hallucinations experienced by the percipients, to obtain corroborative evidence, to verify dates, to interview in person the more important witnesses, and in short to do all that we have thought it necessary to do in the course of our inquiries, the result of his labours could not have been published until far on in the first decade of the coming century. We must be grateful to him that he has done what he could, and we must remember that no other writer probably on the same lines would have effected so much. I spoke just now of correspondents being reluctant to take the trouble of writing out their experiences for a stranger. But it was no doubt precisely because M. Flammarion is not a stranger to them that these 1,800 persons who have

never seen him in the flesh have been willing to do so much. They have read his works, they have found profit in his luminous and vivid descriptions of the vast mechanism of the heavens, and delight in the fairy tales of Science and the glimpses of unimagined worlds which he has given to them. And having received so much they have been glad to make such return as was possible.

Excluding cases quoted from *Phantasms of the Living* and other public sources, M. Flammarion has given us no less than 157 cases of apparitions coinciding with a death. Fifteen of them were related to him by persons of his acquaintance; the remainder were sent in answer to his *questionnaire*. Of the 157 cases the greater number are naturally second or third hand, and many are introduced by such phrases as "When my maternal grandfather, who died in 1882 at the age of eighty, was a young man . . ." Some are purely traditional. Again, a large number of the first hand cases are concerned simply with inarticulate noises, such as raps, clangs and heavy thumps, which are afterwards found to correspond in time with a death. Such noises, as not being *primâ facie* of human origin or necessarily suggesting a human presence, were excluded by the terms of our own Census question and might equally have been held to be excluded by the terms of the questions already quoted. There remain perhaps a score of first-hand cases relating to visions or voices definitely suggesting a human presence. The following is one of the cases received from his own acquaintances which induced M. Flammarion to undertake his more extended inquiry.

"III. (pp. 70, 71).—M. André Bloch, a young musician of great talent, holder of the *prix de Rome* and a member of the Astronomical Society of France, sent me quite recently the following account of an incident of the same kind, which happened in 1896—only the other day, that is (*C'est d'hier*):—

"DEAR MASTER,—In June, 1896, during the two last months of my stay in Italy, my mother came to join me in Rome. She lodged close to the Academy of France in a family boarding-house in the Via Gregoriana, where you yourself lodged. As, at that time, I still had a piece of work to complete before returning to France, my mother went sight-seeing in the city alone, in order not to interrupt me, and only joined me for luncheon at the Villa Médicis towards noon. One day, however, I saw her come in, about eight o'clock in the morning, quite upset. On questioning her, she replied that, whilst dressing, she had suddenly seen, close to her, her nephew René Kraemer, who looked at her and said laughing, 'Oh! yes, I am really dead.'

"Being very much frightened at this apparition, she had hastened to join me. I soothed her as well as I could, and then I kept up a conversation on other topics.

"A fortnight afterwards we returned to Paris, after having travelled through part of Italy, and we then heard of the death of my cousin René, which had taken place on Friday, June 12th, 1896, in the flat in which his parents lived, 31, rue de Moscou. He was fourteen years of age.

"Thanks to a certain piece of work on which I was engaged at the time of my mother's entrance, I was able to ascertain the date and even the hour of the incident.



"Now on that day, my little cousin, who had been ill for several days with peritonitis, began the struggle with death at about six o'clock in the morning and died at noon, after having several times expressed a wish to see his aunt Bertha, my mother.

"It is to be noticed that not a word had been said to us of my cousin's illness in any of the numerous letters which we received from Paris. It was well known that my mother had a special affection for this child, and that she would have returned to Paris had he had but the slightest finger-ache. They had not even telegraphed the news of his death to us.

"I must add that, owing to the difference of longitude, when it is six o'clock in the morning at Paris the clocks at Rome indicate the hour of seven, and it was exactly at that time that my mother had this vision.

"ANDRÉ BLOCH.

"11, Place Malesherbes, Paris."

The case, it will be seen, depends upon a single memory. But it is fairly recent (M. Flammarion's view of what constitutes quite recent evidence differs, it will be seen, from ours); and, though technically second hand, it is for evidential purposes, at any rate as regards the bare fact of the vision and of the impression made by it on the percipient, on a par with a first hand case, since the narrator heard of the vision before he received news of the death.

No. XVII. is an interesting case and is again recent. M. Georges Merlet, Juge de paix at Malesherbes, on the evening of the 1st December, 1898, at 9.30, experienced a violent and unaccountable panic which appears to have produced a quite unusual impression upon him. The next morning he received news of the death of an aged uncle, and immediately connected it with the panic of the previous evening. The death had in fact occurred at 9.30 p.m. Unfortunately the attack of panic, in itself perhaps not unique, or sufficiently distinctive, was not connected at the time with the death. Moreover, as the Oxford Tutor said, we could wish that it had been a nearer relative.

The next case is less recent, but attested by a second witness.

"XIX. (p. 97).—On December 4th, 1884, at half-past three in the morning, I was wide awake, and had just got up, when I saw very distinctly the apparition of my brother Joseph Bonnet, ensign in the 2nd regiment of Spahis, in garrison at Batna, in the province of Constantine (Algeria). At that time he was engaged in the manoeuvres, and we did not know exactly where he was. My brother kissed my forehead, I felt a cold shiver, and he said to me very distinctly, '*Good-bye, Angela, I am dead.*' Very much touched and quite upset, I woke my husband at once, saying 'Joseph is dead; he has just told me so.'

"As that day, December 4th, was my brother's birthday (he had completed his thirty-third year) and we had been talking of this anniversary the evening before, my husband assured me that this apparition was only inspired by my own thoughts and considered it the result of nervous excitement. During the whole of that Thursday I was quite distracted. At

nine o'clock in this evening we received a telegram ; before opening it I knew its contents. My brother had died at Kenchela (Algeria) at three o'clock in the morning.

“ANGÈLA ESPERON, née BONNET.”

“I certify the above account, given by my wife, as perfectly accurate.

“OSMAN ESPERON.

“Retired captain, chevalier of the Legion  
of Honour, at Bordeaux.”

In the next case we have a clairvoyant vision, said to have been related at the time. But the incident again depends upon a single memory. It should perhaps be pointed out, however, that in a case of this kind it is difficult to estimate and quite easy to exaggerate the weight which the corroboration of the second witness really adds to the original statement. With members of the same household constantly occupied with the same interests and having to a large extent the same common memories, any circumstances which affect the memory of the original witness will tend to some extent to affect the memory of him who corroborates. If, in the case which follows, the vision had grown more definite with years, if even the identification of the vision with the percipient's mother was not made until some days later, it is possible that the husband would still be found to corroborate the account given in the text. Probably few husbands could afford to disappoint legitimate expectations in such matters.

“XXX. (pp. 104-5).—My dear mother died on Saturday, April 8th, 1893. The previous Wednesday I had received a letter from her, in which she told me that she was not suffering very much from her heart complaint, and spoke of an expedition she had made on Saturday, April 1st, near our country house at Wasselonne. I had been intending to go out on that Saturday, April 8th. I dined quietly at noon, but towards two o'clock a terrible fear possessed me. I went upstairs to my room and threw myself into an armchair where I burst into sobs. I saw my mother lying on her bed, with a cap of gathered muslin on her head, which I had never seen her wear, and *dead*. My old servant, uneasy at not hearing me moving about, came up to me, and was surprised to see me in such despair. I told her what I had seen, and how afraid I felt. She would have it that my nerves were out of order, and persuaded me to finish dressing. I went out of the house, hardly knowing what I was doing. Five minutes later I heard my husband's quick step behind me ; he was bringing me a telegram : ‘No hope. Mother will not live through the night.’ ‘She is dead,’ I said ; ‘I know I have seen her.’

“I went home and we prepared to start by the first train. It was half-past two o'clock by Paris time when I saw my mother on her deathbed, and three hours later we learnt by telegram that she had died suddenly at half-past three o'clock by Strasbourg time. She had not been ill, and only went to bed two hours before her death, complaining of cold and of being very sleepy. She did not think she was dying, for she had a letter read to her by my father who was standing at the foot of the bed. She did not ask to see her children, but I think she must have thought of me when she was passing

away. As I did not reach Strasbourg till Monday, about eleven o'clock, my mother was already laid in her coffin ; but those who had laid her out, described to me, *just as I had seen it*, the muslin cap, which they left on when they buried her.

"A. HESS.

"Alby."

Strasbourg time, it should be noted, is Central European time—that is, about one hour in advance of Parisian time.

The next case is interesting as belonging to a type of which we have very few authentic instances ; so few, indeed, that it is permissible to doubt whether such incidents do really occur. The present case, depending again upon the unsupported memory of an anonymous narrator, can hardly be said to add much to the weight of the evidence.

"XXXIII. (p. 107).—On the 22nd January, 1893, I was summoned by telegram to my aged aunt of eighty-four, who had been ill for several days.

"On my arrival I found my dear aunt at death's door, and scarcely able to speak. I took up my place by her bedside and did not leave her again. Towards ten o'clock in the evening I was keeping watch, seated in an arm-chair near her, when I heard her call with astonishing strength : '*Lucy ! Lucy ! Lucy !*' I got up quickly and I saw that my aunt had quite lost consciousness, and heard the death-rattle in her throat. Ten minutes later she drew her last breath. Lucy was another niece, and my aunt's god-daughter ; she did not come to see her as often as my aunt wished ; my aunt had several times complained of her to the nurse.

"The next day I said to my cousin Lucy : 'You must have been very much surprised to receive the telegram informing you of our aunt's death.'" She replied : 'Not at all, I was rather expecting it. Just imagine, the night before, about ten o'clock, when I was sound asleep, I was rudely awakened, hearing my aunt call me "*Lucy ! Lucy ! Lucy !*" I could not sleep for the rest of the night.' There is the occurrence, which I certify to be quite accurate, and ask you only to give my initials if you publish it, for the town in which I live is made up for the most part of frivolous, ignorant people or canting hypocrites.

"P. L. B."

The case which follows is remarkable because the narrator admits that she cannot prove the coincidence to be exact. In the numerous secondhand cases here recorded I have not found one in which the coincidence was not exact to the hour—sometimes even to the minute.

"XXXVIII. (p. 110).—It was a few days before July 24th, 1895 ; I had had just undressed myself and was standing near my bed ; my husband was in his dressing room at the time ; being wide awake, I saw my grandmother's face all wrinkled, much more puckered than in reality, and as wan as the face of a corpse. This passed like a flash of lightning, but it distressed me extremely. I did not say anything about it at the time, such things always appear ridiculous when repeated, and the next day my mother let me know that my grandmother had had a serious paralytic seizure which had rendered her quite unconscious. She died indeed a few days afterwards. I have not



ascertained whether the hour at which I had seen her corresponded with the time when she lost consciousness. Being an ardent Catholic, aged 35 years, the wife of a barrister, everything which concerns the world beyond interests me extremely. But I ask you not to make my name known, this town being chiefly made up of empty-headed people, much given to gossip.

“L.M., à A.”

In the next case we have the simultaneous occurrence of inexplicable sounds to witnesses at a considerable distance, the impressions coinciding with a death. Unfortunately the year is not given, but perhaps it may be assumed to be the year in which the narrative was written—*i.e.*, 1899. It is not, of course, a strong case.

“XLVII. (pp. 116–117.)—On the night of the 19th to 20th May, shortly before eleven o'clock, I was still awake, my wife already asleep at my side, when I very distinctly heard a heavy body fall on the floor of the upper story. My wife at once sat up and said, ‘What is the matter?’—‘A loaf must have fallen,’ I replied. The stock of loaves was kept in this upper room.

“While I was speaking we heard another noise similar to the first, then a third much louder. I then got up, struck a light, and going up the wooden staircase which led to this attic, satisfied myself that all was in order there, and the loaves in their places. A dread presentiment took hold of me concerning my brother John, who was ill; but I did not let it appear, and when my wife asked me what had caused these unusual noises, I told her, that I might not frighten her, for I knew her to be very nervous, that some of the loaves had fallen.

“The next day what was my astonishment to see my sister, who then lived at Nîmes, come in in great agitation. She told me that about eleven o'clock she had heard a noise on her table, and again when she was hardly awakened a great commotion in the large cupboard. I then took her into the kitchen. ‘John is dead!’ I said—‘Yes,’ she replied, ‘it was he.’

“A month later we heard that our dear John had died in the Birkaden hospital (Algeria) during the night of the 19th to 20th May.

“MARIUS MARIOGE, RÉMOULIN (GARD).

Here, again, are two recent cases; the fact that the narrator in each case admits not having ascertained the exactness of the coincidence gives some assurance of their accuracy in the main circumstances.

“LXV. (pp. 126–7).—Two years ago my brother went as draughtsman with M. de Bonchamp’s expedition to explore Africa. I had had no news of him for a long time, when one night I woke with a start, and saw my brother distinctly *killed by the spear of a savage*. This occurrence made such a vivid impression on me that I remained awake till morning, and was haunted by the vision for several weeks. A few weeks later I received the news of my brother’s death in Abyssinia; he was killed by the spear of a savage on November 14th. The event must have coincided with the vision. Unfortunately I omitted to write down the exact date at the time. I can, however, affirm that my vision took place in November.

“A. NYFFELEY-POTTER,

“Kinchberg.”

“XCII. (pp. 141-2).—I was making a stay of a few months in Zürich when one day, at three o'clock in the afternoon, I saw some one who I knew was in Italy pass along the street in front of my window. It made so strong an impression upon me, that I felt uneasy for the rest of the day and told one of my cousins about it. (I was wrong not to make a note of the exact day and hour.) A few days later I heard that the person whom I had seen pass (a doctor who had attended me, and to whom I was much attached) had just died suddenly in Italy from the rupture of an aneurism. I think I may affirm that not more than twenty-four hours elapsed between the time of the apparition and that of this doctor's death. The doctor died December 25th, 1897.

“LUCY NIEDERHAUSER.

“Mulhouse.”

In the next case we have the testimony of a second witness before the event.

“XCIV. (pp. 142-3).—In response to your request with regard to facts of the psychical order, I beg to report the following case. My father, M. Fleurant, a retired schoolmaster, and my mother, a schoolmistress, living at Thénay (Indre), will vouch for its authenticity. It was in 1887, in the month of February. My mother had then at Evreux an only brother, for whom she had a great affection, and who on his side made much of her.

“Unfortunately, my uncle was attacked by a disease which was to bring him down to the grave, in spite of science and of the good nursing of his family. Towards the end of the previous year my mother had been to see her brother, and saw for herself the gravity of the disease. The doctor assured her that the end was more or less near.

“On the 11th of the month already mentioned, *towards six o'clock in the evening*, my mother was in the cellar of her school, and came up from it overcome with indescribable emotion: she had heard three piercing cries addressed to herself in the course of a few seconds; they seemed to come through the air-hole of this cellar, *which faced north*.

“She said to my father: ‘My brother is at the point of death; I have just heard him calling.’

“The next day but one she received a letter, dated the 12th, in which was the announcement of my uncle's, Ernest Barthélemy's, death.

“M<sup>lle</sup> Blanche de Louvigny, the writer (*i.e.*, amanuensis) of this letter, who had nursed the invalid up to his last moments, said that he had incessantly called my mother. My mother has often repeated these details, and she is still convinced that she was really in connection with her brother in thought for a few moments, although she cannot explain this phenomenon. I acquaint you with these details, hoping that they may be of service to you in the search after the causes which produce such effects.

“A. FLEURANT.

“Schoolmistress at Reuilly, at the present time living with her parents at Thénay (Indre).”

“The undersigned certify that the information given by their daughter in this letter is most precisely accurate.

“G. FLEURANT.

“Retired Schoolmaster.

“S. FLEURANT.

“Schoolmistress at Thénay (Indre.)”

In case CIX. M. Flammarion finds it necessary to append a note that he is not responsible for the opinions expressed by his contributors. The narrator explains that when a child of twelve he was *encore un peu dévot*, and in the habit of saying his prayers nearly every evening. One evening he prayed with unusual fervour for his grandmother, and immediately after closing his eyes had a vision of that relative. The next day he learned that his grandmother had died at that very hour. The effect of that experience on the trained intellect of a French schoolboy was singular. “Depuis ce moment,” he concludes, “comme je m’étais adressé à Dieu pour me conserver ma grand’mère longtemps, et qu’il ne m’a pas exaucé, j’ai cessé avec raison de croire en lui. On dit qu’il exauce les prières de tous ceux qui ont recours à lui. En voilà une preuve, et aussi de la blague qu’est la religion catholique. Elle ressemble aux autres tout simplement.”

In case CXXX. again the coincidence is not exact. But there are some fine dramatic touches in the story :—

“CXXX. (pp. 166–7).—One of my fellow students (I am a lady doctor) had gone to India as medical missionary. We lost sight of each other, as often happens, but we were still attached.

“One morning, during the night of the 28th to 29th October (I was then at Lausanne) I was awakened before six o’clock by little knocks at my door. My bedroom opened into a corridor, which ended on the staircase of the storey. I left my door half-open to allow a big white cat, which I then had, to go hunting during the night (the house swarmed with mice). The knocks were repeated. The night-bell had not rung, neither had I heard any one come upstairs.

“By chance my eyes fell upon the cat, which was in its usual place at the foot of my bed. It was sitting up, quivering and growling, its hair on end. The door shook as though moved by a slight gust of wind, and I saw a figure appear wrapped in a sort of vapoury white stuff, like a veil thrown over something black. I could not distinguish the face well. The figure came up to me. I felt an icy breath pass over me; I heard the cat growl furiously.

“Instinctively I shut my eyes, and when I opened them again everything had vanished. The cat was trembling in every limb, and was bathed in perspiration.

“I admit that I did not think of my friend in India, but of another person. About a fortnight later I heard of the death of my friend during the night of the 29th to 30th October, 1890, at Srinagar, in Cashmere. I heard afterwards that she had succumbed to an attack of peritonitis.

“MARIE DE THILO,

“Doctor of Medicine, St. Junien, Switzerland.”



Case CLV. presents an incident which, so far as I know, could be paralleled only in the annals of witchcraft. The narrator, who does not give the date of the incident, was a boy of twelve at the time.

“(p. 184).—Towards half-past seven o’clock, I felt a *box on my ear* of extraordinary violence. I began to call out; my mother came to me directly, and asked me what was the matter. I told her that I had been struck and that my cheek hurt me; besides this, my mother observed that my cheek was red and swollen. Uneasy at what had happened my mother was impatient for my father and brother to come home. My father did not come in till nearly nine o’clock; my mother told him at once what had taken place, and when she mentioned the time, my father replied, ‘It was just that time when her grandmother drew her last breath.’ For more than six months I kept the marks of a right hand on my right cheek; it was very apparent, especially after I had been playing and my face was redder, the traces of the hand being white; this was observed by hundreds of people.

“A. MICHEL,

“Dyer in the factory of Valabre, near Entraigues (Vaucluse).”

The next case, CLVI., furnishes us with a well-marked instance of what Professor Royce has called *pseudo-presentiment*.

“(p. 185).—Now, this same day, May 31st, 1895, about half-past nine in the morning, whilst my wife was attending to domestic duties in the house, our little girl, then at the age of two years and a-half, came up to her mother and said in her baby language: ‘Mother, look at godfather (my eldest son was his sister’s godfather), look, mother, I am playing with him.’

“‘Yes, dear, play away,’ answered her mother, who did not attach any importance to the child’s words at the moment.

“But the little girl persisted, in spite of her mother’s indifference, and added: ‘But, mother, come and see godfather—*Look at him, he is there!* Oh! How nicely he is dressed!!!’

“My wife noticed that while she was talking the child seemed as though she was transfigured. She was aroused by it just at first, but soon forgot the incident, which had only occupied a few minutes. It was not till two or three days later that she remembered all the details.

“Shortly before noon we received a telegram, informing us of the horrible accident which had befallen our dearly-beloved son, and I knew later on that death had taken place about eight o’clock.

“ROUGÉ.”

Villa des Tilleuls, Salon (Bouches du Rhone).

Psychical research has no use for details which are not remembered until two or three days after the receipt of the news which gave them significance.

In addition to the apparitions of the dying, the book contains a few instances of transmission of ideas and images not at times of exceptional crisis; and a large number of dreams, telepathic, clairvoyant, and prophetic. As these cases also lack corroborative evidence it has not been thought worth while to quote any illustrations here. M. Flammarion’s own attitude to the subject has already been partly indicated. To begin with, he does not consider that the apparitions of which specimens have been quoted above

are rightly classed as Hallucinations. We know what hallucinations are, he says, and we know how they are caused, and these are not of them nor do they proceed from similar causes (p. 221). He objects therefore to the title "Hallucinations télépathiques," given to the French abridgment of *Phantasms of the Living*, as "titre inexact et dénaturé" (p. 223); he objects equally to Mr. Myers' felicitous coinage "veridical hallucinations" (p. 248). But his own conception of the nature of these apparitions is a little difficult to grasp. He has no doubt that they are caused by some action of the dying man's brain; that this action (psychic force) gives rise to movements in the ether which are propagated to a distance like any other ethereal movements: and affect kindred brains. This transformation of psychic force into ethereal undulations, may, he suggests, be analogous to the transformation of sound into electric waves, and back again into sound, in the telephone (p. 373).

Later, after explaining that the object of his researches is to ascertain whether the human soul has an existence independently of the body and survives its destruction (p. 564), he proceeds to consider which of the forces known to us have an independent existence. Light, heat and sound are clearly not independent existences—they are only sensations produced in our organism by ethereal or aerial movements. But electricity, he is inclined to think, probably exists as a real entity, the ether being an electrical substance (p. 571), and in considering the physical effects produced on kindred brains by the apparitions, he finds himself compelled to conclude that "something" has proceeded from the dying man. Proximately, no doubt, the "something" is but an ethereal vibration, as in the case of light. But the vibration must be caused, and he is satisfied that no anatomist or physiologist would venture to say that the cause would be found in the physical structure of the brain (p. 573).

But it may be argued, as against M. Flammarion, my organism will affect not merely a kindred organism, but any man with the normal complement of senses, or any appropriate mechanical receiver, at a distance which is limited only by the imperfection of our optical and other physical instruments. What is the difference between the vibrations starting from me which affect my friend's eye, and those equally starting from me and equally transmitted by the ether, which, *ex hypothesi*, affect my friend's brain direct? If the one of the two vibrations is of the earth, gross, mechanical and subject to the law of the conservation of energy, how can we prove that the other is not also? Or, conversely, if these prove immortality and the supremacy of the soul of man, have not those an equal claim to give like testimony? In truth M. Flammarion can give no reasonable grounds for the faith that is in him. He distinguishes between the origin of the one set of effects and the other; but he has no argument to offer. "On sent là une propriété inconnue, non de l'organisme physique, mais de l'être pensant (p. 573), and again "Nous ne pouvons nous laisser guider dans nos jugements que par le sentiment intime qui résulte en nous de la discussion des phénomènes" (p. 574). M. Flammarion may be right; but he must give up talking of "scientific methods" and "scientific demonstration."

FRANK PODMORE.

*Treatment by Hypnotism and Suggestion: or, Psycho-Therapeutics.* By C. LLOYD TUCKEY, M.D. Aberd. Fourth Edition, revised and enlarged. (Baillière, Tindall and Cox, 20, 21, King William Street, Strand, London, 1900.)

Cure by suggestion is perhaps the oldest of all methods of treatment. In the childhood of all races the few simples used for their known power as drugs pure and simple were largely overshadowed by charms, incantations, and fantastic methods of cure which depended for their effect on their power over the imagination of the patient. In those early times the priest and physician were usually one and the same, and it followed that, until quite lately, all such methods of cure have been in the hands of priests, soothsayers, white witches and the numerous tribe whose interest it was to keep the idea of the supernatural before the minds of their patients as the true means of cure. In the present day, even in the most civilised parts of the world, we still have miraculous cures at grottoes and wells, Christian Scientists, faith healers, and quack healers of every description. It is a matter of regret that almost up to the present medical men have been so much prejudiced by the element of obvious quackery and humbug surrounding these cures as to confound with them the results obtained by students of psycho-therapeutics. The comparatively recent emergence of the art of medicine from empiricism or charlatanism has perhaps made the present professors of the art morbidly sensitive to anything which smacks of that dark age, and which it is difficult to analyse by the scientific methods they have now become accustomed to. It was inevitable in starting a new study of so difficult a nature that numerous mistakes should be made and theories promulgated which were soon to be proved untenable. The English medical school has hardly made a fair allowance for this. Part of the reason may be that, whereas in Germany and France large numbers of works on the subject have appeared, in England the books which have debated the subject in a fair and equable manner have been but few, and some of the most notable are by foreign authors. Dr. Lloyd Tuckey's volume states the case for psycho-therapeutics in a manner which should convince an impartial critic of the value of its methods. He justly insists on the importance of such a powerful aid to the physician's means of combating disease. His quotation of Professor Pitres is apt and to the point. "Be physicians, gentlemen, and not hypnotisers, but learn to apply hypnotism and be ready to use it in suitable cases." With all the resources of latter-day scientific research at his disposal, there still remains a large field of disease which is the despair of the physician. A large section of it consists of diseases caused by the imagination or by causes which have had their first effect on the imagination. These, as is truly pointed out, are anything but imaginary diseases. It is in these cases that the method of treatment by suggestion strikes at the root of the evil in a manner that no other kind of treatment can approach. And here a point on which Dr. Lloyd Tuckey lays stress may well be emphasised. This method does not weaken the will-power of the patient, but strengthens his power of self-control. The dangers of hypnotism have always been a bugbear which, like many others, requires but careful investigation to reduce to a very small quantity.



Though the main use of hypnotism must be in diseases of a psychical nature, yet, as Dr. Lloyd Tuckey points out, the influence of the mind over the processes of the body is so great that we may fairly look to it as a most valuable reinforcement in the treatment of many other diseases. We all know the value of personality in a doctor's curative powers, and it would be well if the influence of mind over body were a subject which all medical men had a scientific knowledge of. The characteristics enumerated by Dr. Lloyd Tuckey as necessary for the successful practice of psycho-therapeutics for curative purposes are those which every medical practitioner should possess:—"Firm will, unlimited patience, calm temperament, tact, medical knowledge and judgment."

The common belief that hysterical and mentally unstable people are specially good subjects for hypnosis is here well refuted,—experience shows that strong, healthy, intellectual men are easily influenced, though those also who are sick and reduced in bodily strength are peculiarly susceptible. The value of the different methods of sending to sleep is discussed; the less strain, the better. No mention, however, is made of the method of inducing sleep by passes, which is of all methods the one that imposes least strain on the subject. Charcot's interpretation of the hypnotic state and method of inducing it is in my opinion rightly condemned in favour of that of the Nancy School. Many interesting cases are given of the effect of hypnotism on various diseases. It is extremely satisfactory to note the number of dipsomaniacs cured, one third of two hundred cases. The shortness of time required,—one month,—also contrasts favourably with the length of residence found necessary in homes for the cure of such cases. The relegation of the practice of hypnotism to the hands of medical and other scientific men is, with justice, strongly advocated.

In a treatise on the therapeutic side of hypnotism, the purely psychical side has necessarily been left in the background. Still, to the student of Psychical Research, Dr. Lloyd Tuckey's book affords many valuable hints on psychology. I do not know whether all would agree with the statement "that vivid consciousness only exists when nervous impulses are sent through new or little-used channels." Education, or the establishment of these paths as easily travelled tracts, is, I think, required to attain a vivid consciousness of æsthetic feelings.

In the course of so many years' familiarity with the hypnotic state, it is inevitable that many of the psychical phenomena which often accompany it must have come under Dr. Lloyd Tuckey's cognizance. Although he rightly deprecates experiment on patients, yet I am sure he must have accumulated enough material for a treatise on the workings of the psychical side of man which would make a very desirable companion to the present lucid work on Psycho-Therapeutics.

G. F. ROGERS, M.D.

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*Occult Japan: or, The Way of the Gods; an Esoteric Study of Japanese Personality and Possession.* By PERCIVAL LOWELL. (Houghton, Mifflin and Co., Boston and New York, 1894.)

When the normal man sees the word "occult" attached to the title of a book, he is generally disposed to shy from its contents. Even the most

normal, however, need have no apprehensions in approaching Mr. Lowell's very lively and entertaining volume, while students of the curious class of phenomena described,—phenomena of which there are traces in varying forms from earliest times to the present day, and in all countries from farthest East to farthest West,—will find the subject dealt with in a manner showing much care and observation. Most Orientals are apt, as a rule, to withhold their marvels from the eyes of strangers; in Japan, however, to judge from Mr. Lowell's experiences, there seems to be but little of such reticence. So readily indeed was he welcomed by the priests of all sects to the performance of their sacred mysteries, that it is a matter for astonishment that the widespread system of induced divine possession which he was thus enabled to witness should have waited until now for investigation. Mr. Lowell even supposes that the very existence of the system was unknown to foreigners until he himself, as the result of a chance encounter on the summit of the sacred mountain Ontakè, was led to explore it. While practised also among the Buddhists, Mr. Lowell is inclined to trace the origin of these esoteric rites to Shintō, the primitive religion, which, for many centuries displaced by, and intermingled with, imported Buddhism, is now, as a consequence of the nationalist movement of 1867, restored in a purified form to popular favour. "Shintō," he says, "is the Japanese conception of the Cosmos. It is a combination of the worship of nature and their own ancestors . . . A lack of psychic development has enabled these seemingly diverse elements to fuse into a homogeneous whole. Both, of course, are aboriginal instincts. Next to the fear of natural phenomena, in point of primitiveness, comes the fear of one's own father, as children and savages show. But races, like individuals, tend to differentiate the two as they develop. Now, the suggestive thing about the Japanese is that they did not do so. Filial respect lasted, and by virtue of not becoming less, became more, till it filled not only the whole sphere of morals, but expanded into the sphere of cosmogony. To the Japanese eye, the universe itself took on the paternal look. Awe of their parents, which these people could comprehend, lent explanation to dread of nature, which they could not. . . . The thunder and the typhoon, sunshine and the earthquake, were the work not only of anthropomorphic beings, but of beings ancestrally related to themselves. In short, Shintō, their explanation of things in general, is simply the patriarchal principle projected without perspective into the past, dilating with distance into deity." Hence the union of the Japanese and their gods into a kind of family whole, each section standing on terms of more or less intimate intercourse with the other,—truly a communion of the saints. The gods vouchsafe themselves daily to the people and are ever ready to oblige with small services, either by way of working a miracle or so when required for the entertainment or edification of their children and admirers, or by directly entering into possession of one of them in order to converse with and impart useful spiritual information to the rest. It is with these miracles and these possessions that Mr. Lowell's book mainly deals. Of the former, it is clear that the author does not think much. The chief forms of miracles, or god-arts, are the ordeal by hot water; the ordeal by hot coals—a rite very similar to the fire-walking of Fiji recently written on by Mr. Andrew Lang; the walking on edges of sharp swords; and the calling down of fire from heaven—

to light a pipe or other useful end. So gracious are the gods that, while usually reserving these manifestations of divine skill for stated festivals, they are perfectly willing, by private treaty with their priests, to perform these prodigies to order, at any time or place that may be convenient. With a little good will, and perhaps a slight tendency to suggestively or expectationally induced anæsthesia, a tougher cuticle, and perhaps—for the more sure performance of the last-named miracle—a convenient burning-glass, Mr. Lowell hints he could have done just as well himself, given a similar audience. A good stout sole and some useful skill to the thaumaturge; a good thick skull to everybody else; and the gods will work many marvels.

So much for the miracles. In the case of the possessions, or induced incarnations, the problem is more obscure. They are practised by many different sects, each with its own rites and formulæ and its own habitual phenomena. It is not given to every one to become the home of a god, and he who would achieve it must undergo an apprenticeship, long or short, toilsome or easy, according to the purity of the apprentice,—such purity being measured according to his initial capacity for complete vacuity of mind. In the Shinshiu sect a kind of Sunday-school for boys and girls is held, in which instruction is given in the art of temporarily becoming god to the most pious young people of the parish,—a Delphic Academy, in fact, for the cultivation of self-hypnotisation. The phenomena develop gradually. An apt pupil begins with convulsions: the god manifests himself by causing the subject to engage in violent gymnastic performances, somersaults over a table, climbing round a cornice, and so forth. As education proceeds he begins to grunt; later, the grunts become articulate; then comes intelligible speech; and finally, a distinct personality is developed with recognisable attributes distinguishing it from other and succeeding personalities. For a full account the reader must refer to Mr. Lowell's book. The similarity of the phenomena with those attending the performances of Western mediums dominated by "spirit" controls is very close.

And the explanation? After showing the educative effect on the trance state of the dominant idea, of the expectancy of the subject, Mr. Lowell traces the resemblance of the personality developed to the personality so expected. The subject believes he will become a god—and gradually a god he becomes. The subject has certain initial ideas, simple and vague enough, of the behaviour of gods,—and the gods behave just so. A long process of self-annihilation has ended by annihilating self; the ideas are habitual, inherited, racial; and the god is the personification of the racial idea.

Could we wholly annihilate all individual abnormality, could we have the perfectly normal man for subject, "we should, as the trance state deepened and the man lost himself, see him lose, first, his individual characteristics, then his family traits, then the habits of his class, and so down, till only the broadly human ones survived. The trance state would undo what evolution has done, and return to us a primeval savage in the body of an end-of-the-century man." The man has become his own ancestor! "And so," says Mr. Lowell, with joy at his triumphant paradox, which will come as a shock to such as have not foreseen the end of his nimble reasoning, "this lands us . . . at an unexpected conclusion, to wit, that these gods are really



what they claim to be. . . . We are viewing the actual incarnation of the ancestral spirit of the race. . . . If these, his [the subject's] ancestors were gods in the past, gods they are that descend to embodiment to-day."

This is very pretty, yet one cannot but feel that for this bold stroke Mr. Lowell has sacrificed the construction of a theory to account, not only for Japanese possessions, but for all other allied phenomena. Dr. Nevius examined them in China, and called them devils; Dr. Hodgson in America, and there they were discarnate spirits. Is Mrs. Piper nothing but her own great-grandmother? Will Mr. Lowell please discuss this in his next?

EVERARD FEILDING.

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*Aberglauben und Zauberei.* By DR. ALF. LEHMANN, Director of the psychophysical laboratory in the University of Copenhagen. *German Translation* by DR. PETERSEN (Stuttgart, Enke, 1898. Pp. 556.)

On the whole this is a good book. It is written in an interesting, lucid, and easy style, contains a good deal of information well-arranged and in a compact form, and is for the most part animated by a spirit of reasonable and scientific curiosity, which seems to aim at fairness and to fall short of it only occasionally and in consequence of the difficulty of stating a complicated case in so limited a space. Dr. Lehmann is commendably free from the virus of apriorism and the habit of jumping at conclusions without investigation; he does not, as so many scientists do, hold that faith in the infallibility of 'science' (and of himself as its exponent) is a substitute for works, and his ingenious methods of experimentally testing and illustrating the assertions of others often make real contributions to the subject. In fact he shows so much of the spirit of the true psychical researcher that one is disposed to welcome him among the select band of experimental psychologists who have had the good sense and the courage to take to psychical research. Unfortunately, however, this is not quite possible; for owing either to his early training or to a cautious deference to popular and scientific prejudice, Dr. Lehmann occasionally lapses into crudities of judgment for which the bulk of his narrative and his admissions elsewhere hardly prepare the reader.

To give a few flagrant examples. In his historical survey of ancient sorcery the Jews receive a 'favoured nation' treatment which it would be hard to justify on scientific grounds. *Per contra* in his preface Dr. Lehmann takes up quite a comic and Canute-like attitude of one stemming the rising tide of superstition. Yet in the rest of his book he is largely occupied in showing, with great skill and success, how most or all of these superstitions arose as well-meant, though erroneous, attempts at a scientific explanation of phenomena. Again in his final paragraph he warns us that the modern 'occultists' (among whom he seems for the moment to include the S. P. R., although he ordinarily makes much and appreciative use of its work) have burdened their consciences with a terrible responsibility for the new superstitions, to which he fears the new conceptions of telepathy, subliminal

consciousness, etc., will lead. Yet in the body of his work he found these new conceptions useful and even indispensable. He admits (p. 462) that there is no intrinsic absurdity in the assumption of telepathic forces, and, on his own showing, their assertion has at least enriched science by the discovery (made by himself) of "involuntary whispering." As for the subliminal, he is constantly compelled to have recourse to it or its equivalents in explaining hypnotic phenomena, automatisms, etc. Not that he likes it: indeed he has to explain (p. 374 *note*) that he prefers to use the term 'unconscious' in such cases just because unconscious ideas or states of consciousness are sheer nonsense (!), while 'subliminal' and 'subconscious' (though intrinsically preferable, *cf.* p. 431) imply theories. Are we to infer from this that it is scientific to adopt a nonsensical, and superstitious to adopt a workable formula?

It would seem then that of Dr. Lehmann as of Dr. Faust it must be said

*"Zwei Seelen wohnen, ach, in seiner Brust!"*

and if in this review attention is mainly directed upon the utterances of the less admirable of these two souls, the reason assuredly is that it is more profitable to the S. P. R. (and perhaps also to Dr. Lehmann) to discuss points of difference rather than of agreement, and not any desire to detract from the feeling of respect in reciprocating which the S. P. R. may well imitate Dr. Lehmann's handsome references to its own work.

Singling out points for criticism on this principle, I may mention first Dr. Lehmann's attempt to invalidate Sir Wm. Crookes's evidence as to the phenomena observed with Home. His method of doing this is to quote, against Sir Wm. Crookes's accounts in the *Quarterly Journal of Science*, the original notes published in the *Proceedings*, Part XV. (Vol. VI.), p. 98. Between these two versions, he holds, such discrepancies exist as to justify the conclusion (1) that Crookes's experiments are not rigorously scientific but a product of his imagination (p. 270, 273) and (2) that he either had no inkling of the importance of the circumstances which were omitted in his first account, or else that he was guilty of conscious deception.

I own that it passes my comprehension how Dr. Lehmann should have arrived at conclusions of such gravity from premisses so flimsy as those which he adduces. For the two accounts correspond so completely in all essentials that it seems to me a gross exaggeration to speak even of discrepancies between them, and the only legitimate inference would seem to be the very obvious and harmless one that the first account was a highly condensed, and not a *verbatim*, account of what occurred.

But in condensing his report Sir Wm. Crookes was only availing himself of the universal privilege of all competent scientific observers, who take it for granted that they may be trusted not to omit any material circumstance from their descriptions. No doubt in this case, as in all psychological experimentation, it would have been better to have diverged from ordinary scientific practice by publishing absolutely complete records, but then Sir Wm. Crookes wrote long before the S. P. R. had impressed on the scientific world the need for such quixotic caution, and, by producing what Dr. Lehmann himself calls (p. 342) "one of the most interesting series of experiments which experimental psychology has to show," had rendered the scientific world alive to the possibilities of mal-observation in such matters.

No doubt it was this change in the situation (due to Messrs. Hodgson and Davey's work) which led Sir Wm. Crookes to publish his full notes, in order that they might be reconsidered, if necessary, in the light of subsequent knowledge. But one would at least have expected his critic to take note of his emphatic declaration that he finds "nothing to retract or alter" and has "discovered no flaw in the experiments then made or in the reasoning based upon them." (*Proceedings*, Vol. VI., p. 98.) Instead of this, Dr. Lehmann prefers to judge by his subjective impressions. He unfortunately derived from Sir Wm. Crookes's first account an 'impression' that the conditions in detail had been exactly analogous to those of physical experimentation (p. 271).<sup>1</sup> He then proceeds to derive from Sir Wm. Crookes's notes another impression that the conditions were very like those of ordinary *séances*, and, being impressed with a discrepancy which exists rather in his impressions than in the actual narratives, he summarily declares Sir Wm. Crookes's whole evidence to be unworthy of credence!

What renders his cavils the more surprising is that he himself is unwittingly sitting in the same glass house with Sir Wm. Crookes. His own report on the experiments in 'involuntary' whispering by which he proposes to explain telepathy is very far from being complete. As Professor Sidgwick pointed out in the criticism published in these *Proceedings* (Part XXXI., Vol. XII., p. 299) he gives only highly condensed summaries which leave a good deal to be desired in several important respects.

Fortunately, however, Dr. Lehmann makes admissions in the present volume which enable us to fill up the *lacunæ* in his former evidence, and seem to me to throw a good deal of light on his explanation of telepathy, though they do not exactly strengthen one's belief in its correctness.

In the first place it appears from p. 455 that Dr. Lehmann has observed in himself a strong natural tendency towards vocal automatisms which no doubt would go far to render the "involuntary whispering" theory plausible to his mind.

Again we find on p. 457-8 an avowal which throws quite a new light on the short series of experiments which he made in order to test the possibility of transferring telepathically *visual* images, before he adopted the idea (of the 'involuntary' whisper). (*Philosophische Studien*, XI., 4, p. 473). In these experiments results looking very much like telepathy were produced when Dr. Lehmann was the percipient, while his colleague Dr. Hansen failed entirely. 'Involuntary' whispering being then taken up, the first type of experiment was abandoned and the evidence remained inconclusive. Dr. Lehmann indeed regards his success as wholly illusory, but the passage (referred to above) in his present volume renders it possible that he is wrong. He there tells us that in consequence of his strenuous efforts in *séances* to produce automatic writing<sup>2</sup> he got into such a state of excitement and neurasthenia that the beard on one side of his face dropped out. It hardly therefore seems too audacious a suggestion that he is himself

<sup>1</sup> It is, to be sure, somewhat strange that Dr. Lehmann should have got such an impression, for it appears from various passages that he has attended a large number of *séances* and is quite familiar with their conditions.

<sup>2</sup> Why, by the way, has he never published an account of these experiments?



something of a 'medium,' or—as his scientific friends would call it—of a neuropathic subject, and that, if so, there may really have been a trace of telepathy about the experiments which he did not persist in. (Cf. also p. 351, where he all but writes himself down a 'medium').

Indeed, we might proceed to extend this theory to his later experiments and to point out that a little real telepathy would help greatly in the interpretation of a faintly heard and quite inarticulate whisper, if only their inconclusiveness were not already quite apparent on other grounds.<sup>1</sup> Personally, I feel that so long as a scientific critic of telepathy can satisfy himself with Dr. Lehmann's explanation of Mr. Newnham's experiments, viz., by 'unconscious ideas aroused by unnoticed whisperings' (p. 454) we are still a long way from an explanation of the matter.

Other admissions which are of interest in view of Dr. Lehmann's general position are found on pp. 338, 351, and 456, that disbelief leads to non-observation, and that even fraudulent mediums need not be wholly fraudulent, but may exhibit genuine automatisms which contribute to their success and differentiate them from ordinary conjurors. Indeed, Dr. Lehmann declares, on the strength of his own experience, that Mr. Davey was probably an excellent 'medium'!

On the other hand, he must by this time feel that he was a little hasty in writing on p. 457 that in consequence of the 'exposure' of Eusapia Paladino "*spiritism has ceased to exist as a scientific problem.*"

Altogether he is not quite fair to the spiritist theory. He tends to regard it as intrinsically superstitious and *a priori* inadmissible (esp. p. 471-2), in a way in which 'occultism,' i.e., the assumption of unknown forces, is not. He is consequently puzzled (p. 314) to understand why both 'spiritism' and 'occultism' have so often been held together. But surely a moment's reflection would have shown him that if there is room in the universe for undiscovered forces, 'spirits' may be among them, while if a spirit world interacts with ours, the forces whereby this occurs are practically unknown. In point of fact, the spiritist hypothesis is intrinsically as good as any other, and has certain well-marked advantages, *if it is treated in a scientific manner*. It must therefore be repeated as often as is needful that superstition consists, not in holding any particular theory, but in the way in which it is held.

It must be noted, further, that Dr. Lehmann has, curiously enough, omitted all account of the modern evidence as to 'haunted houses.'

<sup>1</sup> It seems to me a strange abuse of language to describe the whispering as 'involuntary,' and I do not wonder that when Professor Sidgwick tried to repeat Dr. Lehmann's experiments he could not attain to the 'involuntariness.' The fact is that Dr. Lehmann's whisperings, as is practically admitted by Dr. Hansen (*loc. cit.* p. 503), were quite voluntary and conscious. The agent was perfectly aware of what he was about and could regulate the loudness and distinctness of his whispers to a nicety. I am surprised only at the moderation of the experimenters in limiting the percentage of their successes to 33 per cent. They might just as well have made it 100 per cent.! I suppose they stopped where they did in order to 'mimic' better the telepathic series of the S.P.R. But the analogy between Messrs. Lehmann and Hansen's experiments and *bona fide* telepathy seems to me wholly illusory.

On the other hand, it is surprising that he should have found it worth while to mention (p. 303), and apparently to credit the stories of the 'buried fakir.' For the only case of the sort which is evidentially respectable is, I believe, that which *seemed* to occur for Ranjit Singh's benefit, as reported by Sir Claude Martin Wade. But a slight reflection upon the powers of political intrigue in a Hindu court to bring about the apparent result is enough to render the story extremely suspect, without casting the slightest doubt on the good faith either of Sir Claude Wade or Ranjit Singh himself, and to bring out the great scientific rashness of crediting the alleged fact on such evidence. Verily Dr. Lehmann also is sometimes capable of the feat of swallowing a camel!

Typographically there are few errors, except that Messrs. F. W. H. and A. T. Myers generally appear as *Meyrs*. In Miss X.'s case also the real name is not *Goodrich* but *Goodrich-Freer*. This error is perhaps the more worth correcting as it occurs also elsewhere, *e.g.*, in Fouillée's *Psychologie des Idées-Forces* (II., 397, etc.).

In conclusion, it is perhaps permissible to express a hope that Dr. Lehmann may find himself able to continue his experimental investigations of subjects whose obscurity has long been a disgrace to science, and that when he does so, he will recognise how greatly co-operation conduces to success, and that not only in the collection of psychological statistics. Hitherto he seems to have asked and received little aid from those who have made a special study of such subjects, but Professor Flournoy's recent success has shown that it is quite possible to maintain the sympathetic attitude which enlists support, without derogating from the demand for scientific proof.

F. C. S. SCHILLER.

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*Psychism: Analysis of Things Existing.* By PAUL GIBIER, M.D. (New York Bulletin Publishing Company. Fifth Edition. No date.)

Dr. Gibier, as he has told us in his title page, is Director of the New York Pasteur Institute and sometime Assistant Professor of the Museum of Natural Philosophy at Paris. He has also conducted various independent investigations as to bacteriology, and has been entrusted by the French Government on several occasions with the mission of studying epidemics of cholera, yellow fever, etc. When a man with such qualifications undertakes the analysis of the Cosmos, his views are certainly entitled to a respectful hearing. It is to be regretted, indeed, that in the present book Dr. Gibier gives us so few detailed accounts of his own experiences. He has many striking facts to relate, but they are for the most part either borrowed from other writers or given in the way of allusion or reference. Thus he tells us that he has witnessed the phenomenon of "direct spirit writing" at least 500 times (p. 229), but the only instance here quoted occurred in the presence of the medium, Slade. Dr. Gibier gives an account of this experiment written by a third person, now dead, but it is not even clear whether he himself was present (p. 211). One very striking series of sittings with an American medium, "Mr. S.," who we may fairly presume to have been Slade himself, he does, however, describe in some detail. The sittings

were held in a biological laboratory close to a large dissecting room at the College Rollin, in Paris, in 1886, for the purpose of obtaining casts of spirit hands. Some plaster of Paris was placed in a large vessel under a wire cage, on which the sitters placed their feet. The conditions apparently were not favourable to the particular form of manifestation: and the medium felt bad influences about. No results of note were observed. At the second séance, also, no casts of hands were obtained, but the investigators were not left without some proofs of spirit agency. As they mounted the stairs in semi-darkness, the medium in advance, a glass jar was thrown on the stairs by an unseen hand and smashed to pieces, and in the séance room itself the medium was suddenly possessed of a violent and malicious spirit, and, seizing a chair, threatened the company with it. Then, to quote Dr. Gibier's account of the impressive scene (pp. 204-205), "He advanced on us [*i.e.*, the author, his friends apparently standing aloof], still swinging the chair, and we were prepared to parry with our stool, when suddenly, we know not by what force, we were prompted to try an experiment, said to be infallible in such occurrences, which had been taught us by a man familiar with these matters. We threw aside the stool which we held and advanced both hands toward the unfortunate entranced one, strongly 'willing' he should become immobilised. We projected, as it were, our will against him, adding to this cerebral effort an energetical gesture. The effect was instantaneous, and we were the first to be surprised at the happy result: instead of hurling the chair at us it was thrown backward, and, although quite a strong one, was broken past the possibility of repair. 'S.' became as if struck by lightning, his body was shaken with a convulsive tremor, brusquely carried three or four yards distant from where he stood, and dropped on the floor against the wall. All his limbs were contorted, his joints cracked, and finally he shrivelled and curled up like a ball."

It was through such scenes as these that our author was initiated into the secrets of the Cosmos. Briefly, for him, as for Hegel, but with a difference, the factors of the universe are threefold. There is mind, matter, and that which mediates between them—animic or ethereal energy. It is this third constituent which in its most generalised form penetrates as ether the interstellar spaces, which radiates from the operator in hypnotism, which forms an aura round the entranced subject, which can be manipulated by Eastern Yogis and photographed by Parisian *savants*. In a word, Dr. Gibier belongs to the school represented in modern France by de Rochas, Baraduc, Barety, and the late Dr. Luys; and farther back by Tardy du Montravel, by Mesmer, and by the long tradition of the Alchemists. In searching through his pages for circumstantial evidence of the all-pervading fluid, I have found instead an admirable maxim which has already done good service in a similar capacity for students of the Occult in this country:—

"There is no religion higher than Truth."

But Dr. Gibier gives us the means of testing for ourselves the presence of the animic fluid. Here is an experiment which we may all try (pp. 127-8.) "It is to-day an acquired fact for all those who study the question, that a *force*, which can very easily be put in evidence, sallies forth and acts at a distance according to the wish of the active subject, or operator: or still, when we come to the passive subject this force manifests itself in



the percipient under the influence of a suggested order ; or again, spontaneously in the medium during a passive, conscious, or unconscious state. For instance, one may with some hypnotisable subjects make the following experiment, which we shall call the experiment of Horace Pelletier, inasmuch as it was made for the first time by that experimenter under the following conditions :—If one, or better, several, sensitive subjects, having their hands placed over a jar of water, are ordered (verbal suggestion) to cause the liquid to move as if it were boiling without touching it : one may after a relatively short time (it is useless to wait more than half-an-hour at each sitting) see the water beginning to ripple at first and then moving as if a small fish were disturbing it by playful evolutions : then it begins to bubble as if it were boiling, so that sometimes it overflows and partly runs out of the jar . . . Mr. Pelletier, who has frequently written to us of this experiment, has not mentioned the fact that the subjects sometimes complain, as soon as the experiment is begun, of a pain in the arms and hands.”

When our readers have carried out this experiment for themselves to a successful conclusion, they will no doubt be prepared to accept Dr. Gibier's account of other manifestations of the animic fluid.

FRANK PODMORE.

[Whilst the foregoing notice was passing through the press, we heard with much regret the news of Dr. Gibier's sudden death through an accident. Dr. Gibier, though still a young man, had already, as stated, done good work in other branches of scientific inquiry : and it can hardly be doubted that wider knowledge of these problems, and experiences of the numerous and subtle sources of error which beset them, would have led a man of his abilities and scientific acumen to employ in his psychical inquiries more of the critical faculty, and to adopt methods of investigation more in harmony with those which he had already employed with conspicuous success in other fields.—F. P.]

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*Faith-Healing and Christian Science.* By ALICE FEILDING. (London : Duckworth and Co., 3, Henrietta Street, W.C. 1899.)

This book mainly consists of a destructive criticism of the methods, beliefs and claims of the so-called Christian Scientists, with some hints as to the principles underlying the success of the movement. There is a good summary, for its extent, of the various methods of faith-healing from early times up to the present. Kindred hysterical epidemics are also dealt with. So many authors have been consulted that it seems a pity no bibliography has been added for inquiries into the subject. Mrs. Eddy's *Science and Health* furnishes many quotations to show its innate foolishness and want of the most elementary principles of logic. Reiterated and overbearing assertion seems to be the keynote of the book and its system. The valuelessness of inexpert testimony is well shown. The ways and means of Mental Therapeutics are discussed in a very fair and open-minded manner, and it

is shown that cures claimed by Christian Science are really attributable to the principles of mental suggestion now becoming more widely recognised, especially in Continental schools. The quotation in the Appendix from Mr. Haggard's *Prayer for a Dyspeptic* affords a most amusing, and yet sorrowful, example of the utter confusion in which the mind of the Christian Scientist seems to be as regards the elementary principles of thought and reason. "Help us to stoutly affirm . . . that we have no Dyspepsia, that we never had Dyspepsia, that we never will have Dyspepsia, that there is no such thing, that there never has been any such thing, and that there never will be any such thing. Amen." Cold comfort for the average dyspeptic, I am afraid. I should recommend any friend of a "Christian Scientist" to try the effect of this book on the "Scientist's" common-sense. A careful record of the results might prove a valuable guide to the inner workings of that modern miracle of contradictions—the mind of the Christian Scientist.

G. F. ROGERS, M.D.

## III.

THE FOURTH INTERNATIONAL CONGRESS OF  
PSYCHOLOGY.

*Paris, August 20th to 25th, 1900.*

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For the Society for Psychical Research this Congress marks an epoch, and is surely the most decided success it has ever had. Yet there were very few Englishmen present, and at the former Congresses members of the S.P.R. were much more prominent. But now, as it seemed to me, the work and the spirit of the English society met with less resistance and a more general acknowledgment and approbation than at any of the three previous Congresses.

This was not, however, clear at the outset. In the two opening speeches by Professor Ribot and Professor Ebbinghaus, describing the development of Psychology in the last century and in the last few years, the work of the S.P.R. was not particularly insisted upon. Professor Ribot mentioned it, shortly but appreciatively; Professor Ebbinghaus gave it no place among the characteristic features of modern psychology. But if we may regard the members of the Congress as a truly representative body of modern psychologists their attitude afforded evidence of a remarkable deviation from this official standpoint. For if there was anything characteristic in the company, it was the general interest and more or less acknowledged belief in things that some ten or twenty years ago would have been pooh-poohed and called unscientific,—supernatural, occult, or mystical.

It is true that the lines of psychology are not well marked out; its limits are unknown, its borders vague and ill-defined. Under the same flag of psychology were gathered together the brain-anatomist, the psychophysiologicalist, whose laboratory work shows no essential difference from that of the physiologist, the speculative philosopher, the spiritualist, the mystic, the Christian priest, the Indian Yogi. We hold it as certain that many members of the Congress did not at all acknowledge the right of the majority of their fellow-members to represent the science of psychology. Still, there were celebrated names, important communications, a serious and dignified tone. And yet the opposition to subjects of an occult or mystical character, if not weak, was certainly not overpowering.

The extremes were well balanced and the distribution of subjects at the general meetings was cleverly and impartially arranged. There were brilliant demonstrations of brain-physiology, showing the influence of different narcotics and toxicants, and also of passions and emotions, on the cellular elements of the cortical matter.<sup>1</sup> On the other hand, an eloquent

<sup>1</sup> Dr. Paul Heger and Dr. Jean Demoor (Bruxelles), *Contributions à la physiologie de la couche corticale*.



vindication of the validity and importance of that most ancient and venerable doctrine of mystical wisdom, the Indian Yoga, was delivered by one of its distinguished followers, T. Ch. Chatterji, of Benares.

It was remarked by Professor Ebbinghaus, as a peculiar feature of modern psychology as compared with the psychology of a hundred years ago, that the old psychology had a religious and mystical tendency, while the modern aimed simply at becoming a science, exactly like any other science.

But it is useless to conceal that this does not fully meet with the views and the motives of the S.P.R. as a whole. While that Society is extremely anxious to proceed along strictly scientific and unbiassed lines, many of its members seem aware at the same time that (taking religion in its widest and deepest sense) science is only part of religion, and can have no beneficial effect without a deep moral conviction, embracing and unifying all branches of science, thus giving them their worth and true meaning.

From this point of view Psychology is the tenderest and most vital organ of modern science, for in it lies the link between the science of exact observation and speculative and mystical thought. In fact, psychophysiology, which consists in comparative measurements of sense-impressions and the founding thereon of general laws,—and speculative thought, which tries to express verbally the most profound factors of our being, are separated by an immense gulf, and can hardly be brought under one common name. The former belongs simply to the great edifice of natural sciences, as a subordinate part, the latter being at the same time the foundation and corner-stone of the whole building.

But the two were conjoined here under the name Psychology, and it seems as if the natural relation between science and religion can only be restored in this domain.

Readers of the proceedings of the Congress will easily see what a large part of the communications consisted of what I may call the material of psychical research. From every part of the world observations were forthcoming about mediumship, trance, thought-reading, and the like. And what readers will hardly deduce from the printed reports is the still larger part that these matters played in private conversation. Rumours about facts showing evidence of remarkable faculties were rife, and it is not unlikely that we shall hear ere long of communications, from a German source, which will startle many disbelievers. Only caution and the fear of shaking the well-earned German reputation for thoroughness has thus far withheld them.

And even among the staunchest rationalists, the most rigid believers in exact science without any mystical basis, there was a milder attitude and a greater interest in the subject than I had ever observed before. In the smile wherewith they asked, "Now, do tell me, what did principally convince you?" there was something of a glimmer of allowing that to be convinced could not be so very bad after all. No doubt the psychological rule of the crowd,—which alleges that no individual can keep free from the mental influence of a majority,—was again confirmed here.

Another remarkable fact was the foundation of the *Institut Psychique*, or *Psychologique*. There was, of course, the inevitable quarrel about the right

name, this time not without some significance. We may have our doubts and fears as to the success of this undertaking of M. Youriévitich; but we cannot doubt that it is meant to be a great international development of the work of the S.P.R.

Our President, Mr. F. W. H. Myers, when he took the chair in June, stated that his election was remarkable because it meant the acknowledgment of psychical study as a regular science, as he had no other scientific title but that of a psychical researcher. Both this last Congress and the *Institut Psychique* seem to afford weighty corroborations of his statement. People who devote their lives solely to the most difficult investigations of psychical phenomena are no longer considered for that reason cranks, or scientific outlaws, who may fill volumes of periodicals and hold crowded meetings without the slightest notice being taken by official science. Not only did these persons form a large part of the Congress, which was under the leadership of the most celebrated scientists, but there seems to be a chance now that they may soon possess something like an Academy or Classes which will form that official standard which is at present more lacking, although more requisite, in this particular branch of science than in any other.

For it cannot be denied that the neglect of the subject by academic science has led to a wild and profuse growth of pseudo-science, with reckless theories and worthless observations. So there is great need of sifting and pruning, of the formation of a high and consistent standard of evidence, which may be done by co-operation throughout the whole world. Can the study of the phenomena of the human mind be thought less important than that of the cosmical bodies?

The days of the Congress, August 20th to 25th, 1900, were full of promise. Nearly every communication had something in it of a deep and startling kind, showing prospects of dimly foreseen knowledge;—from the musical wonder, introduced by Professor Richet,—the piano-playing and composing baby,—to the mad and hallucinated frogs of Prince Tarkhanoff.<sup>1</sup> So marked, indeed, was the change in tone that even the young Brahman's confident reiteration of ancient Eastern claims to a higher knowledge attained by ascetic methods fell no longer,—as it would assuredly have fallen in earlier Congresses,—on wholly unsympathetic or unhopeful minds.

FREDERIK VAN EEDEN.

Bussum, Holland, August, 1900.

[To Dr. van Eeden's report, Mr. Myers adds:—]

Dr. van Eeden has modestly omitted to notice the most important paper dealing with definitely supernormal phenomena which was submitted to the Congress;—namely, his own account and discussion of certain sittings with which he has been favoured by Mrs. Thompson. This paper was very well received by (I believe) the largest audience which met during the Congress.

Two other papers which had been prepared on Mrs. Thompson's phenomena,—in French by Mrs. Verrall, and in English by myself,—were

<sup>1</sup> Prince Jean de Tarkhanoff, *Illusions et Hallucinations des Grenouilles en dépendance de leur espèce.*

perforce omitted for lack of time ; but both Professor Richet and I addressed the Congress briefly on the subject. Professor Moutonnier also gave some account of an excellent sitting which Mrs. Thompson had given him. The subject of trance-phenomena and "possession" was thus frankly admitted within the purview of the Congress, and it is hoped that the various papers may subsequently appear in our *Proceedings*.

Certain other papers, perhaps intended to support our views, suggested to me the imminence of a danger from which our obscurity has till now kept us fairly free ;—I mean the danger of indiscriminate popularisation. This cannot possibly be avoided. We must learn to submit to hearing our own achievements exaggerated,—and at the same time mixed up with narratives and opinions for which we have no intention whatever of making ourselves responsible. All that we can do is to repeat that only in our *Proceedings* will the reader find any authorised account of our work ;—and to insist that our object is still to stimulate inquiry far more than to propagate beliefs. We are not missionaries, but researchers.

Finally we have deferred to the last the mention of a paper of extreme interest, novelty, and importance, by Dr. Morton Prince, which we hope shortly to be able to publish in our *Proceedings*. Students of "secondary personalities" will recognise that the case described in this paper is absolutely the most remarkable which has yet been given to the world. It is fortunate indeed that it has fallen into such competent hands, and Dr. Prince's complete report of it will be awaited with impatience.

F. W. H. M.



PROCEEDINGS  
OF THE  
SOCIETY FOR PSYCHICAL RESEARCH,  
PART XXXIX.









Portrait of Henry Sidgwick

Henry Sidgwick

*From a Photograph by Mrs. F. W. H. Myers, taken in 1895.*

*Reproduced by permission.*

## IN MEMORY OF HENRY SIDGWICK.

BY F. W. H. MYERS.

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“From Maximus I learnt self-government, and not to be led-aside by anything; and cheerfulness in all circumstances, as well as in illness; and a just admixture in the moral character of sweetness and dignity, and to do what was set before me without complaining. I observed that everybody believed that he thought as he spoke, and that in all that he did he never had any bad intention; and he never showed amazement and surprise, nor did he ever laugh to disguise his vexation, nor, on the other hand, was he ever passionate or suspicious. He was accustomed to do acts of beneficence, and was ready to forgive, and was free from all falsehood; and he presented the appearance of a man who could not be diverted from right rather than of a man who had been improved. I observed, too, that no man could ever think that he was despised by Maximus, or ever venture to think himself a better man.”

These simple sentences, in which the Stoic Emperor describes his honoured teacher, must recall to all of us, with striking appropriateness, the friend and leader whom we shall see on earth no more. There are others better qualified than I to retrace and estimate his manifold activities in the world of beneficent action and of lofty thought. I shall speak of him only in two capacities;—as the closest and most revered of friends, and as our captain in the great enterprise which it is the object of this Society to pursue.

Yet as I speak I shall feel the perpetual need of his sanction for what I say; and this utterance of mine—the first which will have lacked his helpful criticism—must needs be of all utterances that which needs his help the most. No one but himself could truly depict the progress of that subtle and all-embracing intelligence, and my sketch will be but a blurred projection, where the master's thought is dimmed and deflected by passing through the disciple's mind.

It is just forty years since I went up to Trinity College, Cambridge, as a freshman of seventeen, and began to read with Henry Sidgwick as my private tutor in classics. But he soon ceased to take private pupils and spread his own studies over a wider range. Even then he was, to use George Sand's phrase, *tourmenté des choses divines*, and he sought whether the study of Oriental languages, of ancient philosophies, of history, of science, would throw light upon that traditional Revelation which hung before him with so much of attractiveness in its promises,



so much of uncertainty in its origin and its foundation. The theoretical result of these studies was not unusual; the practical outcome was academically almost unique. Theoretically he concluded that the Christian tradition—so long, at least, as it remains unsupported by cognate evidence—is not strong enough to uphold the orthodox creed as to the existence and the relation to mankind of a spiritual world. Practically he determined that, as he was no longer “a *bonâ-fide* member of the Church of England,” he would resign his Trinity Fellowship, which was held upon that condition.

It may illustrate the bluntness of moral perception which such tests engender, if I add that—even while myself aggressively orthodox—I nevertheless considered that the vague phrase of the Declaration,—which had been substituted for a definite subscription to the Articles—permitted the retention of a Fellowship by any man who had been born into the Church of England, and had not taken any overt step to renounce his allegiance. Sidgwick’s action, however, caused no surprise; he was already a man (as George Eliot later said of him), whose friends tacitly expected him to conform to moral standards higher than they themselves cared to maintain. His resignation, as is well known, brought others with it; it attracted the attention of Parliament, and exercised some real influence in procuring the abolition of University Tests.

In single-hearted devotion to the Right and Reasonable there was no period of life in which Henry Sidgwick had much to learn. But I shall not conceal,—what is indeed to his credit,—that in some other directions his ultimate character was largely of his own making. I cannot claim for him that he was by nature one of those men *ὁσοῖς διδασκτὸν μὴδέν*,—who, like his friend Charles Bowen, seem born to attract every love and to win every victory. Those who knew Sidgwick only in later life, when sympathy, benignity, the very ripeness and mellowness of wisdom, seemed his predominant traits, would be surprised to know of the coldness with which he was regarded by many contemporaries and juniors in his early student days. “High, self-contain’d, and passionless,” like the mystic Arthur,—he almost seemed, as he himself wrote to me later, to be “cased in a bark of selfish habit,” and to fulfil all righteousness with a jejune precision,—almost as anxious not to accord too much to his neighbours as not to accord too little. There was a reserve, a preoccupation, perhaps also a proud sensitiveness, which chilled and checked in him the natural lavishness and *abandon* of youth. But he became, as I have said, the *faber indolis sure*;—by sheer meditation, by high resolve, he made himself such as we all know him;—so that “no man could ever imagine either that he was despised by Maximus, or that he was himself a better man.”

Yet partly also I ought to say of that earlier stage of his character that it was *I* who "half despis'd the height, To which I would not or I could not climb," and that on his part the treasure was not proffered till it was needed ;—that his lesson of wise suspense and high resolve was taught to those only who had found the world's facile pleasures empty, and its confident solutions vain.

My own entry into his intimacy, at any rate, was in an hour of deep inward need.

"Faith at her zenith, or all but lost in the gloom of doubts that darken the schools" :—

I had passed through all these stages, and visiting Cambridge again in 1869 to examine for the Moral Sciences Tripos, I felt drawn in my perplexities to Henry Sidgwick as somehow my only hope. In a star-light walk which I shall not forget (December 3rd, 1869), I asked him, almost with trembling, whether he thought that when Tradition, Intuition, Metaphysic, had failed to solve the riddle of the Universe, there was still a chance that from any actual observable phenomena,—ghosts, spirits, whatsoever there might be,—some valid knowledge might be drawn as to a World Unseen. Already, it seemed, he had thought that this was possible ; steadily, though in no sanguine fashion, he indicated some last grounds of hope ; and from that night onwards I resolved to pursue this quest, if it might be, at his side. Even thus a wanderer in the desert, abandoning in despair the fair mirages which he has followed far in vain, might turn and help an older explorer in the poor search for scanty roots and muddy water-holes.

This was, I fear, but a slow and late conversion to the sense, which so many men had already reached, of Sidgwick's penetrating wisdom. When I think of other Trinity men whom I have found worthy of respect, from rather before my own date to rather after it,—of Montagu Butler, R. C. Jebb, G. O. Trevelyan, Henry Jackson,—of Balfours, Lytteltons, Darwins,—of W. K. Clifford, Lord Rayleigh, F. W. Maitland, Walter Leaf, Henry Butcher, Edmund Gurney, and the rest ;—it seems to me as though all these had been prompter than I to appreciate that which in the end I knew so well. Nay, but in the end, perhaps, of all these, only Arthur Balfour and Edmund Gurney fell into quite the same attitude towards Sidgwick as myself :—the attitude as of "companions of Socrates" :—as it were, say, a Kritias of happier omen, a Theages, a Simmias,—feeling an essential stimulus to self-development in his intellectual search, his analysing *elenchus* ;—and feeling also in the steadfastness of his inward aspiration a prophylactic, as each man might need it, against diletantism, or self-indulgence, or despair.

How strong may be that craving for Wisdom, when once Wisdom has seemed to speak with us in the voice of a man ! "Beauty," says

Plato, "we love best, because we see her clearest; Wisdom with bodily eyes we cannot see, or terrible had been the loves she had inspired."

In that colloquy of which I have spoken above, Sidgwick and I had caught together the distant hope that Science might in our age make sufficient progress to open the spiritual gateway which she had been thought to close;—to penetrate by her own slow patience into the vestibule of an Unseen World.

"I sometimes feel," he wrote to me in 1872, "with somewhat of a profound hope and enthusiasm, that the function of the English mind, with its uncompromising matter-of-factness, will be to put the final question to the Universe with a solid passionate determination to be answered which *must* come to something."

Yet the mode of putting this idea into practice was hard to find. We were forced in the beginning to follow pathways trampled for the most part by quite other than scientific feet. Our efforts of the first few years (1872-6) were tiresome and distasteful enough;—yet what were they in comparison to the hardship which a naturalist will undergo to trace (say) the breeding-ground of a song-bird, or to establish the relationships of a worm of the sea? The efforts would have been as nothing had the results been clear. But the results of those years were on either side unsatisfactory;—so contradictory, so perplexing, that we could neither feel sure that there was nothing discoverable, nor yet that any valid discovery had in fact been made. For some years, indeed (1876-1881), we worked but fitfully,—half-sickened at "Craft, with a bunch of all-heal in her hand, follow'd up by her vassal legion of fools." Yet even those years were not lost. Driven perforce to question ourselves closely as to the validity of the principles from which we had started, we found no reason to desert them. Assuredly, if a spiritual world has ever been manifested to man,—has ever been intermingled with this material scheme of things,—it must be manifest, must be intermingled now. The failure of such an inquiry as ours;—I mean the ultimate relinquishment of every effort in this one direction of real possibility of advance;—must needs prove, if not a death-blow, yet a dire discouragement to every sacred memory, to all spiritual hope. It would be hard for future men to persuade themselves that what in ages of knowledge and clarity was seen to be fraud and illusion had yet been verity and revelation in the confused obscurity of the Past.

And thus it was to men wearied but not broken, discouraged but not despairing, that at the end of 1881 a fresh call to exertion came. An attempt was to be made, under somewhat better auspices than other such attempts which had failed before, to confront spiritualistic and similar beliefs with scientific inquiry. Professor Barrett represented



the scientific, Stainton Moses the spiritualistic camp. One of the first questions, of course, was whether Sidgwick would join in the proposed Society. Edmund Gurney and I made our adhesion contingent on his acceptance of the Presidency ;—but reported to him that there seemed indeed to be a chance here of uniting new inquirers, and of pushing promising experiments.

He took time to consider his reply ;—a reply on which the employment of much of the energy of his remaining eighteen years of life in fact depended. It would have been impossible to press him to consent. Admit that we had been right in making our laborious attempt ;—nay, that it was still right to keep a watchful eye on any possible opening for inquiry. Yet he had done all that utmost fairness could require. *Ought* a man who in so many other ways could definitely advance human knowledge and human happiness to turn aside and commit himself anew—and more deeply than ever—to the gratuitous quixotism of endeavouring to benefit the race in this difficult and uncalled-for way ?—to get the moon for a child who had not even cried for it ?

“To the Jews a stumbling-block, and to the Greeks foolishness.” Who really *wanted* the new knowledge, even if we could manage to give it them ? Men satisfied with an existing revelation did not desire to have that revelation extended. They no more wished to have their unique tradition enrolled and justified in a cosmic order than those old warriors wished to have the eclipse which routed their foemen reduced under astronomical law. And as for the “Greeks,”—the men of science to whom we desired to link ourselves,—from them there was little sign as yet of anything but compassion or scorn.

On the other side was the fact that, however unsatisfactory our quest might have thus far proved, there yet had never been (as Sidgwick himself had said) “any given moment at which we felt that we had a right to abandon it.” Its problems were still absolutely unsettled ; and it was still possible that at any moment light might come. And the original thesis still stood firm—namely, that whether or no it be possible by observation and experiment,—along the paths of science patiently pursued,—to raise the human race into ethical stability,—the Cosmos into intelligible coherence ;—at any rate these results are certainly not attainable in any other way. Without fresh facts none of us can get any further. There are simply not enough known determinants for any valid solution. What use in fondling hallowed traditions, or in juggling with metaphysical terminology ? Unless the human race can find more facts, it may give up the problem of the Moral Universe altogether.

Sidgwick consented to become the first President of the Society for Psychical Research ; and on July 17th, 1882, delivered an Inaugural Address (*Proceedings S.P.R.*, Vol. I., page 78) which in its freedom

alike from pretentiousness and from contentiousness set a precedent from which he never departed.

Of what essential value his services were to the new body has been well shown in a letter recently sent by Mr. Podmore to the *Daily Chronicle* :—

For the first few years of its existence (except for an interval, during which he stepped aside in favour of the late Professor Balfour Stewart) Mr. Sidgwick acted as president of the Society. But the bare statement of that fact conveys a very inadequate idea of the real nature of the services rendered by him. That he gave largely—very largely—of his personal means to help the work of investigation in those earlier years is the least of his benefits. He presided throughout at our councils ; he took an active share in the tedious work of experiment, of examining witnesses, of collecting and appraising evidence ; the lines on which our work could best be done were laid down by his advice and pursued under his personal direction ; all the publications of the Society were issued under his immediate supervision. That of late years he has delegated to others many of these functions was due less to any decay of his personal interest in the work of investigation than to the feeling that his immediate supervision of all details was no longer necessary. But he has throughout these eighteen years been a regular attendant at our meetings, and has taken a constant and predominant part in all our deliberations. Whatever position the S.P.R. may hold to-day, whatever good work it may have done in exposing error or in directing attention to neglected facts in human psychology, its success is due, in the largest measure, to the wisdom, the clear insight, the “particularly sane intellect” (to quote the words of your memoir), and, above all, to the pre-eminent justice and veracity of our first president.

No other man, I think, could have filled that post so well as he. We had before us no straightforward scientific task, such as could be planned once for all, and then parcelled out to committees for execution. What was needed was a wide and scattered inquisition into human faculty and human experience ; an inquisition which could not rely only on familiar canons of evidence, but needed the establishment of many new criteria, consistent with each other and with the old. There was work for many specialists, but at the head of all there was manifest need of a *philosopher* in the widest sense ;—of a man accustomed, like the author of the *Methods of Ethics* and the *Principles of Political Economy*, to weigh conflicting opinions, to comprehend and meet the perplexities of varying minds.

Moral qualities were needed, too ;—patience, and caution, and urbanity ;—and above all a certain doggedness and fixity of resolve, which should pursue its course unbaffled by long delays, by mortifying defeat. And Sidgwick possessed, in an almost unique degree, that motive for dogged persistence which lay in a deep sense of the incurable incoherence of the intelligible world, as thus far grasped by men.

More thoroughly than any other man known to me he had exhausted one after another the traditional creeds, the accredited speculations ;—had followed out even to their effacement in the jungle the advertised pathways to truth. Long years of pondering had begotten in him a mood of mind alike rare and precious ;—a scepticism profound and far-reaching, which yet had never curdled into indifference nor frozen into despair.

His controversial urbanity, indeed, never quite concealed the clearness and even the sternness of his perception of the fallacies which both Christian apologetics and *à priori* metaphysics too often hide at their heart.<sup>1</sup> And thus, while renewing this toilsome quest in no sanguine spirit, he renewed it at least with the conviction that there was nothing else of equal moment to be done ; that if, where religion and philosophy had failed in establishing certainty, Science were to fail also, “the human race,” as he once said to me, “had better henceforth think about these matters” (the basis of morals, the government of the universe) “as little as they possibly can.”

I shall not here attempt to define the various conclusions—or *non-conclusions*—at which this cautious thinker arrived in the course of his long research. I will quote only a few sentences,—for him of unusual definiteness,—to which he signed his name, at the close of that laborious statistical inquiry into hallucinations over which he presided, and which fills the tenth volume of our *Proceedings* (Vol. X., p. 394).

“*Between deaths and apparitions of the dying person a connection exists which is not due to chance alone.* This we hold as a proved fact. The discussion of its full implications cannot be attempted in this paper ;—nor, perhaps, exhausted in this age.”

Now, it is true indeed, as those last words hint, that the scientific mind of the actual generation is scarcely prepared to use in any fruitful manner—or even to realise with full clearness—a discovery so far remote from that synthesis of knowledge on which human thought is at present mainly engaged. The fact alleged has few existing connections ; it remains, so to say, on the mere surface of the hearer’s mind. Yet of course no *savant*, no philosopher, can deny that that one fact, if true, constitutes an inlet into cosmic laws of an absolutely new kind ;—suggests operations and an environment of which we have hitherto possessed no positive knowledge whatever. It forms a first step towards exactly what we are aiming at,—namely, the discovery by scientific methods of a spiritual world. It is a fact whose bare existence must negative much previous theorising and suspend much more ;—must

<sup>1</sup> Thoroughly Socratic was his appeal for instruction to a fellow-examiner, when revising the papers of some ultra-Hegelian young man : “I can see that this is nonsense ; but is it the *right sort* of nonsense ?”



negative that mechanical materialism which asserts that "the world is made of ether and atoms, and there is no room for ghosts;"—must suspend that obscurantist timidity which assumes that we already know as much as we ever shall know, or ever ought to know, as to the soul or the destiny of man. The attraction of straws to amber was in like manner for many years a phenomenon too remote from existing knowledge to sink deep even into the most eager minds: yet that unexplained physical attraction *then*, like this unexplained spiritual attraction *now*, was probably the phenomenon of greatest pregnancy offered to the observation of men.

Thus far, perhaps, but hardly farther, may one here insist on the importance of the psychical work accomplished by one who always insisted that such accomplishment was scarcely to be looked for yet; that we pioneers ought to be well content if we could prepare the scientific mind of the opening century for a forward step such as has never been dreamt of till now;—prepare the unborn workers, in every realm of philosophy and religion,

To cast the kingdoms old  
Into another mould.

It has been said of Sidgwick that "although he was the most influential man in Cambridge, he founded no school." Not at Cambridge only, but over all the civilised world, I think that there are many of us who will say that he did found a school, and that we are his scholars. He did not indeed bequeath to us his wisdom in the shape of crisp metaphysical bank-notes, which the Universe would ultimately decline to cash. Nor did he, like the old man in the fable, tell us to dig everywhere for a treasure which in reality was only to consist in the strengthening of our own minds. Nay, he pointed to a definite spot; he vigorously drove in the spade; he upturned a shining handful, and he left us as his testament, *Dig here*.

We can claim no monopoly in his spiritual benefactions;—but so far as in us lies we can resolve to carry on our work in his spirit. To do this, we must remember that our very *raison d'être* is the extension of scientific method, of intellectual virtues,—of curiosity, candour, care,—into regions where many a current of old tradition, of heated emotion, even of pseudo-scientific prejudice, deflects the bark which should steer only towards the cold, unreachable pole of absolute truth. We must recognise that we have more in common with those who may criticise or attack our work with competent diligence than with those who may acclaim and exaggerate it without adding thereto any careful work of their own. We must experiment unweariedly; we must continue to demolish fiction as well as to accumulate truth; we must make no terms with any hollow mysticism, any half-conscious

deceit. If we act thus, we need fear nothing for our adventure. The humblest scouts who strive loyally to push forward the frontier of Science, even though Science at first disown them, are sure in time to hear her marching legions possess the unfrequented way.

Yet one thing more. To conform to scientific rule and procedure is a task within the power of diligent men. We must not leave that undone;—but if we would follow in our first President's steps, there is something harder than that which we must do. Sidgwick was not only cautious, systematic, self-controlled, he was also unrelenting, undeviating, inwardly ardent to the end;—possessed, as Plato has it, with that “iron sense of truth and right” which makes the least indication of intellectual as well as of moral duty fall on the heart as an intimate and urgent command:—nay, which bids a man place hope and joy in those things only, “by dwelling on which it is”—as Plato again tells us—“that even God is divine.”

In ardour, I say, rather than in circumspection, in force of will rather than in pondering hesitancy, lay the true core of Sidgwick's life. Even his affections—at first somewhat narrowly bestowed, somewhat sternly ordered—widened and deepened, until the emotional nature became as rich and strong as the intellectual. Many there are who know that they gave him of their best, and that he gave of his best to them;—not a few, for whom the Cambridge gardens and avenues will seem still charged with a sacred intimacy, and haunted by a quiet voice, and fragrant with memories of some help or guidance of long ago;—the reason's slowly-broadening outlook, the heart's slow-established calm.

“From kindly act to kindly act,” he passed, like Marcus Aurelius, “with memory of God.” Yet even the calmest existence, if sympathies be strong, must come at times within the wind of some tempest which has swept over other lives. I have watched him play his part in a great tragedy; I have seen with what delicate comprehension, with what inexhaustible tenderness, he could come to the succour of some innocent, long-suffering soul. I have seen him also in the joy of his own complete and fitting guerdon; a bliss which came to him through the affections, and was founded upon the deep community of upward-striving souls. We who most revered Henry Sidgwick desired for him no earthly honours. Those are but trivial ornaments for the life of such a man. We knew that the Primal Life has so woven our lesser lives into one shining fabric,—has interlinked our human hearts in the unity of so strange a joy,—that, when once through some predestined encounter the spirit's kinships have been established for ever, there is no need for further questioning as to the recognition or recompense of even the best and wisest soul. Already in this life of earth heaven's prizes are bestowed with lavish hand; there is no man's achievement

of wisdom or virtue which one woman's reverence cannot amply recognise, one love extravagantly repay.

And still through all personal joy or sorrow, loneliness or companionship, there lived on in this man's heart that impersonal ardour, that cosmic passion, which from suppression takes a whiter glow, and burns in the penetralia of the spirit, a silent inextinguishable flame. I have said above that our life's bark should be steered ever towards the icy pole of absolute truth. But there is a more living, a truer metaphor. My most vivid memory of my friend is as he would recite to me—and I have never known man or woman who could recite poetry like him—that noble apologue of Seekers which was the central expression of his inward life. I speak of Tennyson's poem of "The Voyage," that allegory of the lifelong quest of an Ideal,—through all its semblances the same unknown Reality,—whose pursuit is more entrancing and more imperious than any earthly joy.

For one fair Vision ever fled  
 Down the waste waters day and night,  
 And still we follow'd where she led,  
 In hope to gain upon her flight.  
 Her face was evermore unseen,  
 And fixt upon the far sea-line ;  
 But each man murmur'd, "O my Queen,  
 I follow till I make thee mine."  
 And now we lost her, now she gleam'd  
 Like Fancy made of golden air,  
 Now nearer to the prow she seem'd  
 Like Virtue firm, like Knowledge fair,  
 Now high on waves that idly burst,  
 Like Heavenly Hope she crown'd the sea,  
 And now, the bloodless point reversed,  
 She bore the blade of Liberty.

Nay, and the last stanza also spoke his indomitable resolve ;—and not the less characteristically for the touch of self-mockery with which the poet ends the song :—

Again to colder climes we came,  
 For still we follow'd where she led :  
 Now mate is blind and captain lame,  
 And half the crew are sick or dead,  
 But blind or lame or sick or sound,  
 We follow that which flies before :  
 We know the merry world is round,  
 And we may sail for evermore.

To these hidden fervours Fate vouchsafed few occasions of outward enterprise, of manifest heroism. She left him content with the inward



uplifting, and the unnoted sacrifice, and the secret habit of honour. Yet at last, with strangely solemn prevision, came to him that last opportunity which no Fate can begrudge to us,—the call of slow creeping Death.

I learnt his sentence from his own lips just before he presided at a meeting of the Synthetic Society, at which Mr. Arthur Balfour read a paper upon Prayer. And thus it came about that my friend's last utterance,—not public, indeed, but spoken intimately to a small company of like-minded men,—was an appeal for pure spirituality in all human supplication; a gentle summons to desire only such things as cannot pass away. I will not say how his countenance showed then to my eyes;—eyes dimmed, perhaps, with secret knowledge of what so soon must be.

It was nearly three months later that at Terling Place I bade him—I will not say a last—farewell. Peace was there, without and within. Before the open window lay such a scene as Matthew Arnold had desired for his own departing:—

Bathed in the sacred dews of morn  
The wide aerial landscape spread;—  
The world that was ere I was born,  
The world that lasts when I am dead.

Within was the nobly humble spirit, awaiting the great advancement, before that summer prospect of august serenity, amid that group of steadfast loves. “As I look back on my life,”—almost his last words to me were these,—“I seem to see little but wasted hours. Yet I cannot be sorry that you should idealise me, if that shows that I have made my ideals in some degree felt. We must idealise, or we should cease to struggle.”

Idealisation is not always illusion. *That* was not illusion, when often with look or word that pointed onward and upward he made me feel, like Plotinus gazing on Justice and Virtue, οὔτε ἑσπερος οὔτε ἑως οὐτῶ καλὰ—“neither evening star nor morning is so fair.” *That* was not illusion, if sometimes the veil was lifted and the reality was clear; and, like Odysseus when at the end of all his wanderings that Figure stood by him in the way, I felt that through those eyes shone something that was not of earth, and Heavenly Wisdom walked beside me with the voice and semblance of my friend.

Παλλὰς Ἀθηναίη, κούρη Διὸς ἀνιγόχοιο,  
Μέντορι εἰδομένη ἡμὲν δέμας ἡδὲ καὶ αὐδὴν.

[At the conclusion of Mr. Myers' address, which was delivered at a General Meeting of the Society on November 17th, 1900, DR. OLIVER LODGE communicated the following remarks]:—

I will speak no word of compliment concerning the utterance to which you have just listened; for it has been a soul-stirring tribute from a friend to a friend, and as such is sacred and above outside appreciation.

Nevertheless, as one proud to reckon himself in a minor degree a friend of Henry Sidgwick, as one who by mixing in the world of science has learned something of the attributes of scientific men, I should esteem it a privilege to be permitted to bear witness to the eminently scientific type of mind which he possessed. Others will be impressed with his liberality, his public spirit, his keen interest in education, his brilliancy as a man of letters; I will speak of his scientific side alone. For clear, cautious intellect and for consummate honesty I have not known his superior. Physicists will understand what I mean if I say that for critical judgment and supreme candour he stands in my mind on a level with Lord Rayleigh.

The word honesty or candour sounds weak and commonplace in this connection, and in its ordinary acceptance so it is; but there are degrees of candour even among honest and candid men, and in Henry Sidgwick's case the commonplace aspect of the virtue dropped off and something of singular dignity shone forth. Those who knew him best may best realise what I mean, may remember instances of his analysis of a situation—his comprehension of an opponent's attitude; but even comparative strangers must have been struck with it, for it was a part of his world-wide reputation. Others might be accused of letting their intelligence be governed by their emotions, their judgment be overpowered by their hopes, their scientific acumen be cloaked by their ambition to be of service, their caution be melted by their enthusiasm:—and the accusation might have some grain of truth, why not? I have accused myself of such momentary lapses before now.

But such criticism of Henry Sidgwick was never so much as hinted at: no one, however hostile to our inquiry, ever imagined that Sidgwick let his enthusiasm warp his judgment or his candour be coloured by his hopes. Other faults had to be invented for him: over-coolness, hyper-caution, excessive hesitation, exaggerations of his great virtue, we have heard attributed to him;—yea, I myself have occasionally felt a momentary impatience because even on level ground or on an

upward slope he would keep a tight rein ; but the impatience was momentary ; and ample justification of the precaution was soon perceived in some dangerous dip close ahead.

And of what vital moment to our cause this reputation, this attribute, of his has been ! To it, more perhaps than to any other single influence, is the comparative success of our movement due. Our researches are hardly yet recognised by official science, it is true, but they are far nearer recognition than ever before—far nearer than they were twenty years ago.

And this recognition is important, for in no other way can their continuance be ensured independently of the lives of individuals. Individual men of genius in the past have now and again proceeded as far as we have in actual experience ; but their influence has practically ended with their lives ; and the world, momentarily awakened, has lapsed once more into psychical dark ages.

So it may be again, but so I hope that again it will not be. I trust that now, at the end of a century of science, a new science has been born, and has not been still-born. If it should live, and should be nurtured by the goodwill and the labour of generations of critical and painstaking workers, that result will have been rendered possible not solely by the manifold powers and energies of Edmund Gurney and, of some still living, but by those powers and energies guided and curbed and directed by the serene sovereign wisdom of Henry Sidgwick.



## Obituary.

### FREDERIC W. H. MYERS.

Frederic W. H. Myers, born February 6th, 1843, died January 17th, 1901; formerly Fellow of Trinity College, Cambridge; Honorary Secretary of the Society for Psychical Research, 1888-1899; President of the Society, 1900.

Commemorative addresses by Dr. Oliver Lodge and others will be delivered at the next General Meeting, on March 8th, and afterwards published in the *Proceedings*.

## PROCEEDINGS

OF THE

## SOCIETY FOR PSYCHICAL RESEARCH, PART XL.

### PROCEEDINGS OF GENERAL MEETINGS.

The 108th General Meeting of the Society was held at the Westminster Town Hall on Monday, October 29th, 1900, at 4 p.m.; MR. H. ARTHUR SMITH in the chair.

PROFESSOR W. F. BARRETT gave an account of some of his investigations into the Divining Rod, the full report of which appeared in *Proceedings*, Part XXXVIII.

The 109th General Meeting was held in the same place on Friday, November 16th, 1900, at 4 p.m.; the PRESIDENT, MR. F. W. H. MYERS, in the chair.

MR. J. G. PIDDINGTON read the paper by DR. MORTON PRINCE on "The Development and Genealogy of the Misses Beauchamp," which is printed below.

THE PRESIDENT gave an address "In Memory of Henry Sidgwick," which appeared in *Proceedings*, Part XXXIX.

The 110th General Meeting was held in the same place on Friday, December 14th, 1900, at 8.30 p.m.; DR. LLOYD TUCKER in the chair.

MRS. A. W. VERRALL read a paper on "The Trance Phenomena of Mrs. Thompson," which it is hoped will appear in a future Part of *Proceedings*.

A paper by the REV. STANLEY L. KREBS, entitled, "A Description of Some Trick Methods used by Miss Bangs, of Chicago," was read by MR. F. PODMORE.

## THE DEVELOPMENT AND GENEALOGY OF THE MISSES BEAUCHAMP :

### A PRELIMINARY REPORT OF A CASE OF MULTIPLE PERSONALITY.<sup>1</sup>

BY MORTON PRINCE, M.D.

*Physician for Nervous Diseases, Boston City Hospital, Boston,  
Massachusetts.*

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What I propose to tell in this paper is the way in which the different personalities of Miss Beauchamp became developed, and the relation which they bear to each other and the true self.

From a great mass of phenomena manifested by this case<sup>2</sup> (aboulia, negative hallucination, crystal visions, co-existent conscious states, automatic writing, etc., etc.) I have selected these two points as being, it seems to me, the most important involved in its psychology.<sup>3</sup>

Miss Beauchamp is a person with several personalities. She has at least three, besides three hypnotic states, and during the past two or three years these three persons have been coming and going apparently without any law or order. First one and then the other has been bobbing in and bobbing out, while each one of them claims to be the real Miss Beauchamp, and to have the sole right to existence to the exclusion of the others. Each has objected to the habits and doings of the others and to their being allowed to stay and to any consideration being shown them. It has not been easy to determine which of two personalities at least, if either, is the real self. Cases of multiple personality are not very uncommon, but, so far as I know, no attempt has been made to determine the relation which the different personalities bear to each other. They have been regarded as more or less isolated phenomena, each personality described with its own

<sup>1</sup> Read at the International Congress of Psycho'ogy, Paris, August, 1900, under the title of *The Problem of Multiple Personalities*.

<sup>2</sup> For a briefer account of some of the features of this case, see Dr. Morton Prince's "Experimental Study of Visions," by F. W. H. Myers, *Proceedings S.P.R.*, Vol. XIV., pp. 366 *et seq.*—ED.

<sup>3</sup> I hope soon to publish a complete and extended account of the case in all its aspects. I regret that the necessary limitations of time will allow only a superficial treatment here.

peculiarities, and the whole as representing a succession of psychical events.

This case has been the subject of a continuous study for at least three years, and has occupied hundreds of hours of time. Now, as the end of this study comes, to me at least it seems simple, and it is difficult now to realise the complications that presented themselves during this long period of time, when there seemed to be no relation between the one thing and the other; but the difficulties have been very great.

I have had from time to time a number of theories regarding the case. These have been mere working hypotheses, which I have had to give up one after another; starting with one, it has fulfilled its part for a while, and then I have had to give it up, finding it completely inadequate, until continued study has given what I believe to be the real solution.

When Miss Beauchamp first came under observation she was a neurasthenic of a very severe type. She was a student in one of our colleges, and there received a very good education. But in consequence of her neurasthenic condition it was simply impossible for her to go on with her work. She was a wreck, I might say, in body. In temperament she is a person of extreme idealism, with a very morbid New England conscientiousness, and a great deal of pride and reserve, so that she is very unwilling to expose herself or her life to anybody's scrutiny. This has been one great difficulty in the study of her case. To this I would add that she is a person of absolute honesty of thought and speech. I feel sure we can rely upon and trust her absolutely and completely. I have never known her, nor has any one, I believe, known her,—as herself, or the person whom we call herself,—in any way to indulge in any deception. Nevertheless, every safeguard has been employed to guarantee the *bonâ fide* character of the phenomena.

Now she came to see me in this neurasthenic state, but I found treatment was of almost no use. The usual methods were employed with no result, and it seemed as if her case was hopeless. Finally I concluded to try hypnotic suggestions. She proved a very good subject, and the suggestions produced at the time rather brilliant results. In hypnosis she went easily into the somnambulistic state. This somnambulistic state came later to be known as B. II., while the first personality with whom I became acquainted, Miss Beauchamp herself, was known as B. I. Now I used to notice that as B. II. she was continually rubbing her eyes; her hands were in constant motion, always trying to get at her eyes. Still I paid very little attention to it, or placed very little significance in this fact, merely attributing it to nervousness. One day when I hypnotised her and referred to something that she had done in a previous hypnotic state,—that is to



say, something that she had said or done in a previous state when I supposed she was B. II.,—she denied all knowledge of it and said it was not so. This surprised me, and I attributed the denial at first to an attempt at deception. I waked her up and put her to sleep again, and this time she admitted what she had previously denied. This rather puzzled me, and I made various tests to determine her honesty in the matter. The next time I hypnotised her she denied what she had previously admitted, and so it went on, denying and then admitting, until it dawned upon me that I was dealing with an entirely different personality, and this proved to be the case. It turned out that when she went into the state of which she later denied the facts, she was an entirely distinct and separate person. This third personality, which then developed, came to be known as B. III. We had then three mental states, B. I., B. II., and B. III.

B. I. knew nothing of the others. B. II. knew B. I., but no more. B. III. knew both B. I. and B. II. Thus far there was nothing very unusual.

Now B. III. has proved to be one of the most interesting of all the personalities that have developed in the case. In one respect it is one of the most remarkable personalities, I think, that has ever been exhibited in any of these cases of multiple personality, as will, I think, presently appear. B. III., like B. II., was constantly rubbing her eyes, so that I was frequently compelled to hold her hands by force to prevent her from doing so. When asked why she did this, she said she wished to get her eyes opened, and it turned out afterwards that it was she who was rubbing the eyes of B. II. in the earlier times. At this time I prevented B. III. from opening her eyes for the reason that I feared that, if she got her eyes opened and was thereby able to add the visual images of her surroundings to her mental life as B. III., these same images of her surroundings which she would also have, of course, when she was B. I., would by force of the association awaken all her mental associations as B. III., and that, in consequence, B. III. spontaneously would be constantly coming into existence of her own accord. This afterwards proved to be the case. B. III. always insisted upon having her eyes opened, complaining that she wished to see, and had a "right to see." One day, some time after this, when she was at home, owing to some nervous excitement, she was thrown into the condition of B. III., and then, I not being there to prevent it, she rubbed her eyes until she got them opened, and from that time to this she (B. III.) has had a spontaneous and independent existence.

This personality dates her whole independent existence from this day, and she always refers to events as being "before" or "after she got her eyes opened." That is the central event in her life, just as

mothers date periods before or after the birth of a child. Now this personality came afterwards to be known as Sally Beauchamp. (The name Beauchamp has been adopted in this account for all the personalities.) She took the name for fun one day, a name that she got out of some book, and by that name she has been known ever since. In character she differs very remarkably from B. I. I would say here that B. I. is a very serious-minded person, fond of books and study, of a religious turn of mind, and possesses a very morbid conscientiousness. She has a great sense of responsibility in life, and with those who know her trouble is rather sad and depressed in her general aspect, the latter the result of the general troubles and trials of her life. Sally, on the other hand, is full of fun, does not worry about anything; all life is one great joke to her; she hates books, loves fun and amusement, does not like serious things, hates church,—in fact is thoroughly childlike in every way. She is a child of nature. She is not as well educated as is Miss Beauchamp, although she reads and writes English well; yet she complains constantly that she cannot express herself easily in writing, but she does it quite well all the same. She cannot read French or any of the foreign languages which Miss Beauchamp knows, and she cannot write shorthand; in short lacks a great many of the educational accomplishments which the other character possesses. She insists, although of this I have no absolute proof, that she never sleeps, and that she is always awake while Miss Beauchamp is asleep. I believe it to be true. Then Miss B. is a neurasthenic, Sally is perfectly well. She is never fatigued and never suffers pain. During the first year Sally and Miss Beauchamp used to come and go in succession. At first whenever B. I. became fatigued or upset from any cause, Sally was likely to come. The periods during which Sally was in existence might be any time from a few minutes to several hours. Later these periods became prolonged to several days. It must not be forgotten that though Miss Beauchamp knows nothing of Sally, Sally, when not in the flesh, is conscious of all Miss Beauchamp's thoughts and doings, and the latter could hide nothing from her. Curiously enough, Sally took an intense dislike to B. I. She actually hated her. She used to say to me, "Why, I hate her, Dr. Prince!" and there was no length to which Sally would not go to cause her annoyance. She would play every kind of prank on her to make her miserable. She tormented her to a degree almost incredible. While Sally would never do anything to make any one else unhappy, she was absolutely remorseless in the way she tormented Miss Beauchamp by practical jokes and by playing upon her sensibilities. For example, I will give a few illustrations. If there is one thing which Miss Beauchamp has a perfect horror of, it is snakes and spiders. They throw her into a condition of terror. One day Sally went

out into the country and collected some snakes and spiders and put them into a little box. She brought them home and did them up in a little package, and addressed them to Miss Beauchamp, and when B. I. opened the package they ran out and about the room and nearly sent her into fits. In order to get rid of them she had to handle them, which added to her terror. Another joke was to take Miss Beauchamp out into the country when she was very tired, and in an unfit condition to walk ; that is, Sally would take a car and go out six or seven miles into the country to some retired place, and wake up Miss Beauchamp, who would find herself far out in the country with no means of getting home, no money in her pocket, and nothing for it but to walk. She had to beg rides when she could from passing waggons, and come back tired, worn out, used up for a week.

A great friend of Miss Beauchamp, to whom she felt under strong obligations, had asked her to knit a baby's blanket. She worked on that blanket for nearly a year ; as fast as she would get it near completion, Sally would unravel it, and then, like Sisyphus, she would have to begin the task again, and regularly every time Sally would pull the whole thing to pieces. Finally she came to herself one day and found herself standing in the middle of the room tied up in a perfect network and snarl of worsted yarn ; it was wound round the pictures and then round and round the furniture, the bed, the chairs, herself, and she had to cut it to get out of the snarl. Another favourite joke of Sally's was to make Miss Beauchamp lie. She had the power when she pleased, of producing *aboulia*, and also of making B. I. say and do things against her will ; for after a fashion she can get control of her arms and legs, and of her tongue.

Sally made her tell most frightful fibs. For instance, when asked who lived in a small squalid little house at the side of the road, she said "Mrs. J. G.," a very prominent lady in society, and very wealthy. "Why, I thought she was rich !" "Oh, yes, but she has lost all her money now." Miss Beauchamp would be mortified at hearing herself tell these astounding bare-faced fibs, which her listener must know were fibs, but she could not help it. Again, for a time at least, Sally put B. I. on an allowance of five cents a day. She would find the money waiting for her in the morning on the table with a note saying that it was her allowance for the day and she could not spend more. Sally took away her postage stamps, and if Miss Beauchamp wrote a letter it had first to be exhibited to Sally, and if Sally approved it, it was posted ; if not, it did not go, and that was the end of it. Miss Beauchamp is a person with a great sense of dignity, and dislikes anything that smacks of a lack of decorum or of familiarity. Sally had a way of punishing her by making her sit on a chair with her feet upon the mantelpiece. B. I. could not take her feet down, and was mortified to think she had



to sit that way. Sally carries on a correspondence with Miss Beauchamp, writes letters to her pointing out all the weak points of her character, dwelling on all the little slips and foibles of her mind, telling her all the reckless acts and secret thoughts, indeed, everything she has done that won't bear criticism. In fact, when she has a chance to stick a pin into her, she does it. When Miss Beauchamp wakes in the morning, she will find pinned up on the wall of the room verses containing all sorts of personal allusions, letters calling her names, telling fictitious things that people have said about her; in short, doing everything imaginable to make her life miserable. Nevertheless, at times when she goes too far, Sally has got frightened, and then she would write me a letter and ask for help, saying that she "could not do anything with Miss Beauchamp, and I really must help her."

One of the most interesting problems is, who is Sally? My original theory about Sally was that she represented an earlier period of B.I.'s life. From Sally's childlike peculiarities, which gave the impression of a girl of about thirteen or fourteen, in connection with certain other evidences of a more mature mind, I arrived at this conclusion, and thought that probably some accident had happened in her life which had split her off from the main consciousness, and that Sally was a reversion to that early period; but this theory I was obliged later to give up. Sally as an individuality goes much further back than this; she goes back to early infancy, and has grown with the growth of Miss Beauchamp. The theory which finally, I think, has been demonstrated, is that Sally represents the *subliminal consciousness*.

Although B. I. knows nothing of Sally, Sally not only is conscious of Miss Beauchamp's thoughts at the moment they arise, but she is capable, as I have said, of controlling her thoughts and her arms and legs and tongue to a certain extent. Sally can produce positive and negative hallucination in B. I. and frequently does so for a practical joke. During the times when Sally is in existence, B. I. is,—as Sally puts it,—“dead,” and these times represent complete gaps in Miss Beauchamp's memory, and she has no knowledge of them whatever. “What becomes of her?” Sally frequently asks. Sally is never “dead.” Her memory is continuous; there are no gaps in it. She not only knows,—simultaneously, as I said,—all of B. I.'s thoughts and emotions and sensations, but more than that;—Sally's thoughts are entirely distinct from and independent of B. I.'s thoughts, with which they are co-existent, but not identical. B. I.'s thoughts are not Sally's thoughts. Sally's thoughts co-exist alongside of and simultaneously with B. I.'s; but Sally's mental life is made up of entirely different and separate thoughts and feelings from B. I.'s, so that Sally will have a train of thought at the same time with B. I., of an entirely different nature. All this is also true of the relation of Sally's mind to that of the third

personality—B. IV.—who came later, excepting that Sally does not know B. IV.'s thoughts. While either Miss B. I. or IV. is thinking and feeling one thing,—is depressed and self-reproachful, for example,—Sally is feeling gay and indifferent and enjoying Miss B.'s discomfiture and perhaps planning some amusement distasteful to her.

Speak to either B. I. or B. IV. and Sally hears you. Say something that Sally alone understands, and you see her smile. She is as much alive as the other consciousness. The only difference is this, that her consciousness for the time being is not in connection with the body. Then Sally remembers things in the past that B. I. knows nothing about at all, things that she apparently never was conscious of, or which she has completely forgotten. The most remarkable part of Sally's personality, I think, is that she has been able to write out for me her autobiography, beginning with the time when she was in her cradle, which she remembers. She actually describes her own thoughts and feelings as distinct from B. I.'s all through her childhood, up to and including the present time; although, as she says, she never got an independent existence until she "got her eyes open." She remembers her cradle, draws a picture of the bars in its sides, and remembers what she, as distinct from Miss Beauchamp, thought at the time when she was learning to walk. Then B. I. was frightened and wanted to go back, but Sally was not at all frightened and wanted to go ahead. She describes B. I. as having had a butterfly mind as contrasted with her own. She, as a small child, disliked the things that B. I. liked and *vice versa*. She describes her school life, her own feelings when B. I. did things, and the different sensations of the two selves when, for example, B. I. was punished and felt badly, and she herself was entirely indifferent and without remorse. Thus I have been able to get *an actual autobiography of a subliminal consciousness, in which are described the contemporaneous and contrasted mental lives of two consciousnesses, the subliminal and the dominant, from early infancy to adult life*. In this Sally has described for me various scenes and incidents which occurred and which she saw during her early life, but of which Miss Beauchamp is entirely ignorant. These usually represent scenes which occurred while B. I. was absorbed in thought, but which Sally as a subliminal noticed. Taking all this into consideration,—taking the present relations of Sally's thoughts to Miss Beauchamp's thought, and many other facts, like automatic writing, which Sally performs with ease, and uses for purposes of correspondence,—I think we are safe in saying that *Sally is the subliminal consciousness*, which has become highly developed and organised and obtained finally an independent existence, and led an individual life of its own.

After Sally's escape from her mental Bastille, the two went on leading their independent lives, coming and going for a year or two,

until one day, June 7th, 1899, an event occurred which had an influence upon the whole history of this case. To understand it, it is necessary to go back six years, to the year 1893. It appeared that in 1893 Miss Beauchamp was in a hospital in a neighbouring city, call it Providence. She had been taken with the fancy that she would like to be a nurse (it was the passion of her life), and in a fit of idealism she entered the hospital. One night she was sitting in her room with a friend, a Miss L., when, upon looking up, she was startled to see a face in the window. It was the face of an old friend of hers, a Mr. "Jones," as we have agreed to call him, whom she had known ever since she was a small girl, and who had been a sort of preceptor to her. At first she thought it was a hallucination, but she presently saw that it was a real person. She then hastily got her friend out of the room, and she went downstairs and out of the side door, where this person met her. It appeared that this person was in Providence on his way to New York, had wandered to the hospital, and seeing the ladder had climbed it for a joke, and looked into the window. Outside the hospital door an exciting conversation occurred. It was to her of an intensely disturbing nature, and gave her a tremendous shock. Perhaps I should say here,—as I have told so much of the story,—that it was the kind of thing that upon the ordinary person would not have had much influence, but with her sensitive and idealistic nature she exaggerated it and gave it an intensity that an ordinary person would not have given to it. At any rate, it did give her a violent shock. The surroundings, too, were dramatic. It was night, and pitch dark, but a storm was coming up, and great peals of thunder and flashes of lightning heightened the emotional effect. It was only by these flashes that she saw her companion. From that time she was changed. She went out and walked the fields at night by the hour; she became nervous, excitable and neurasthenic, all her peculiarities became very much exaggerated and her character changed; she became unstable, developed aboulia, and, in other words, changed into B. I. So that B. I.,—or, more correctly, Miss Beauchamp modified into B. I.—dates from the time of that scene outside the hospital that night. Sally, too, who knows the inmost soul of Miss Beauchamp, says she changed after that night.

So it follows that the Miss Beauchamp who has been the object of this study, has been educated in college, and been the solicitude of many friends, is not properly speaking the original Miss B., but a modified personality rightfully designated as B. I.

On the afternoon of June 7th, 1899, six years later, Miss Beauchamp was in my office. She was not in any way noticeably different from her usual condition. After leaving the office she went to the public library, as was her custom. In the library she met



a messenger, who quite unexpectedly brought her a letter, and this letter was from the person whom we have agreed to call Jones. The letter was couched in almost the same language as was his conversation at the time of the hospital incident in 1893. It threw her into a very highly excited state, and she actually saw a vision ;—the scene outside the hospital with herself and Jones as the actors, and actually saw herself and him, and saw the flashes of lightning and heard the thunder, and through it all his voice. Under the influence of this excitement she went to the reading room, and there had an illusion. In the evening newspaper she saw my name printed in large letters in the headlines, in place of that which was really printed, the name of a relative of mine who had died that morning. Still further upset by this, she made her way home. It was not until many months after this that I learned of this scene, or of the hospital episode in 1893, so that it was long before I found the key to the sequel. (As will appear, if I had known of it all, I should have been saved many hours and much labour in the attempt to understand the later psychical developments.) All I knew then was this. After returning from the library, Miss Beauchamp was in such a nervous state that I was soon sent for. On arrival I found her in a very nervous, highly excited condition. She was unable to sit still ; her limbs were in more or less constant motion. Her condition was one that I had frequently seen after she had been exposed to emotional influences. Presently she changed completely. She became quiet, perfectly natural, talked affably, was very sociable, and, in fact, seemed to be in a condition in which I had never seen her before,—more natural in many ways than I had ever seen her, quiet and calm, and apparently in a perfectly healthy state of body and mind ; but, to my surprise, I found that she did not know me. She said I was not Dr. Prince, and when I insisted that I was, she laughed and took it just as if I was talking nonsense to her,—if I insisted that I was, well, I could have it so, but she knew I wasn't. She said that I was perfectly reckless in coming, that I ought not to have come, and then I discovered that she was under the impression, in fact insisted, that I had come in through the window. As we were in the fourth story, it was plain that she was also under an illusion as to the place where we were. The contrast of these illusions with her normality in other respects was striking. The scene lasted some little time. Finally I showed her my name in my watch. She underwent a slight mental shock. A change came over her. She passed through a brief period of confusion and then became herself again, but without any recollection of what had occurred. The essential points I would emphasise are that,—besides appearing perfectly normal,—she insisted that I was Jones, imagined we were somewhere else, that I had come in through the window, that I ran great risks in coming because of the

publicity, that I ought not to have done it, and that it was a foolish thing to have done. Being ignorant of the preceding events just narrated, it was not clear at first whether this was simply Miss B. under an illusion, or whether it was a new personality similarly affected.

To cut a long story short (for it took a long time to unravel the mystery), it turned out to be a new personality, who had at this moment waked up, and had gone back six years in her life, and now imagined it was the same night and she was in the same room in the hospital where she had seen that face in the window in 1893. The impetus to her awakening had evidently been given by the shock of the letter and the vision in the library a few hours before; awakening she went on with her life where she had left off, with the last vestige of memory, which was seeing Jones at the window. Under the influence of this suggestion (she and B. I. are very suggestible) she mistook me for Jones, and, having seen him, as she thought, a moment before (really six years previously) at the window, *inferred* I had entered by that means. The impropriety and un wisdom of it all was a natural thought. This new personality became known in these studies as B. IV.

The next time I saw B. IV. she was free from all illusion, but I was struck by her formal and distant attitude. It soon transpired that she did not know me or the consulting room, where B. I. had been time and again. In fact she knew absolutely nothing of the events of the past six years, knew nothing of B. I.'s life in college, knew nothing of the friends whom B. I. had made during these years, knew nothing of me, knew nothing of any of these events whatsoever, nor later, after she was domiciled as one of the Beauchamp family, did she know anything about the present times when B. I. or Sally was present. From this it follows that she knew, and continued to know, nothing of either B. I. or Sally, or that there ever were such personalities.

It took her a long time to accommodate herself to the new order of things and to take up the thread of events. Like Rip Van Winkle, she did not know that the world had moved since she went to sleep. Sally, who was invaluable as an informer, used to report that she seemed to live in the past and used to speak to people as if it was still 1893. So the family now was increased by one, and there were three who kept changing with one another.

B. IV. is a very different character from either Sally or Miss Beauchamp. A study of the different habits of thoughts, tastes and emotions of these three people has thrown, I believe, much light upon the psychology of character, but in this report I must limit myself to a mere outline of events. There were, of course, gaps in the memory of B. IV. (as with B. I.) corresponding to the time of the existence of the other two personalities. But this B. IV. was never willing to admit. Unlike the others, she is irritable and quick-tempered, and resented as

an impertinence—especially as she regarded most of us as strangers—any inquiry into her private thoughts and affairs, and above all any interference with her habits of life and private conduct. Though anxious to know, she was not willing to ask about what had occurred in the gaps when the others were in the flesh, and so was in the habit of inferring and guessing, at which she was very skilful. She was, as stated, even unwilling for a long time to admit that there were gaps, but it was easy to convict her here by a few questions. After fibbing and inferring and guessing, she would break down and confess she did not know, which was the fact.

Now, Sally, although her mental life is also continuous during that of B. IV. (as with B. I.) and although she knows everything B. IV. does at the time she does it, hears what she says, reads what she writes, and sees what she does, nevertheless *Sally does not know B. IV.'s thoughts*. Herein is a very interesting psychological distinction between Sally's relation to B. I. and to B. IV. She knows B. I.'s inmost soul, she can only infer B. IV.'s thoughts from what she says and does. But Sally studied her closely and soon discovered for herself that B. IV. knew nothing of the past six years, but was always secretly trying to obtain information, and guessing. Sally, in her astonishment, used to say "Why, she doesn't know anything, she is always fishing and guessing." For this reason, Sally, until she learned to know B. IV. better, had a great contempt for her, and dubbed her the "Idiot," and whenever after this she spoke of her, it was as the "Idiot." From this time on, Sally transferred her hatred from B. I. to the "Idiot." She came to regard B. I. as rather a poor sort of creature and hardly worthy of her consideration anyway, and let her alone; but B. IV. became the object of her attacks. But B. IV. is more than a match for Sally, who is really afraid of her. They quarrel like cats and dogs. One of the most curious and puzzling things was the cause of Sally's hatred of B. I. It was unmitigated jealousy. She was jealous of the attentions B. I. received, jealous of the fact that people liked B. I. and wanted to keep her in existence instead of herself, and therefore—difficult as it is to conceive of a person jealous of herself—Sally was immensely jealous of herself.

I have said that B. IV.'s memory ceases at Providence with seeing the face in the window. She can tell you nothing after that, and knows nothing of the scene outside the door. Both B. I. and Sally recall and describe similarly that scene, but the last thing B. IV. remembers is the face at the window a few minutes before.<sup>1</sup> As one method of corroboration of these events, I produced in B. IV. a crystal

<sup>1</sup> The lack of this knowledge was afterwards the cause of much trouble to B. IV., which was strong evidence of her ignorance.



vision.<sup>1</sup> I gave her a glass globe to look at, and told her to think of Providence. As she looked into the glass she was horrified to see there the scene that took place outside the hospital door. She declared with much excitement that it was not true, that it never had occurred, and this she repeated again and again, and remonstrated against my believing it. Like B. I. in the Library, she saw herself by flashes of lightning standing by the door with Jones. She saw his excited manner, and heard his voice between the peals of thunder. She saw it all as a *vision*, just as it occurred. She was startled by what she saw and experienced over again all the emotion of the original scene.

I am obliged to omit all reference to the extraordinary misadventures and misunderstandings of these three personalities brought about in part by the ignorance of B. I. and B. IV. of each other, and the consequent conflict of their plans and doings, in part by the difference of the characters of all three, and in part by the mischievousness of Sally, who concocted a little *Midsummer Night's Dream* of her own, and as Puck (with a little dash of Iago) played her pranks upon both. Some of these adventures were laughable and some were tragic, as when in despair Miss B. I. tried to commit suicide by going to bed with the illuminating gas turned on, and Sally came, jumped out of bed, opened the window, turned off the gas, and saved Miss Beauchamp's life.

For some time after B. IV.'s appearance I adopted the theory that she was the real and original Miss Beauchamp, who had gone to sleep in 1893 and had now become awakened. It followed from this that Miss Beauchamp No. 1, whom we had all learned to know and respect, who had been educated and made many friends, was nothing but a somnambulistic person, and had really no right to existence. The situation was a tragic one: a cure meant the annihilation of Miss Beauchamp, her psychical murder and disappearance as a person, as much as if she were condemned to physical death. Under this theory I sought to keep B. IV. and annihilate B. I.

When I came to know B. IV. better, however, it became evident that this theory was incorrect. However normal she appeared to be and however much more like the original 1893 Miss Beauchamp than B. I. she was in some ways, in other respects she was less like her. Morally and intellectually she lacks many qualities. For instance, unlike the original, she is quick-tempered (Sally calls it "a nasty temper"), is devoid of idealism, is unimpressible; pictures and books do not affect her, she hates church and religion, and not only has no love of music, but has lost the power of reading it and playing

<sup>1</sup> A study of Visions based on this case may be found in *Brain*, 1898. See also *Proceedings S.P.R.*, Vol. XIV., pp. 366 *et seq.*

the violin. So the theory of the re-awakening of the original self was not substantiated.

This is an important point from the bearing it has on some hysterical phenomena, and I regret I cannot go into it further here. When a hysteric is thrown into another state—is awakened, as I with Sollier would prefer to put it,—and immediately goes back to a day on which an accident occurred and takes up the thread of her mental life at that moment, with amnesia for all succeeding events,—does it necessarily follow that the new state is more normal than the others, and is the original self? I think not.

Now the most interesting and most important question is, what is the relationship between all the personalities? What relation do they bear to the normal personality;—and for that matter, which is the normal Miss Beauchamp; or is any one of them the real and normal individual? Allow me a few moments to sketch,—and it must be literally to sketch only—a few of the characteristics of B. I. and B. IV. Sally has been sufficiently emphasised. The continuity of her memory, her origin in and slow development from early infancy, her persistence as a subterranean consciousness, while the other personalities are in evidence, the co-existence of her consciousness simultaneously with those of B. I. and B. IV., her power to influence the consciousness of these others, and to act upon their body, the independence of her thought, her power to express her thoughts independently in writing during the existence of the other two persons;—all this stamps her as a highly developed subliminal consciousness, who, confined in the chrysalis until 1897, broke out then and flew away as an independent life.

But B. I. and B. IV., who are they?

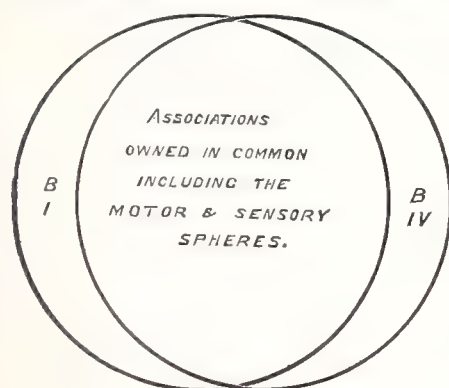
One thing must be insisted upon;—namely, however normal each may appear superficially, neither is quite normal. In each are missing some of the attributes of the original Miss Beauchamp, but in B. I. the departure from the normal is the least. Her neurasthenic condition, her aboulia, her extreme suggestibility, by which negative hallucination can be produced at will, her exaggerated sensitiveness to emotional influences, like music and religion, which produce certain psychical phenomena, the dominion which ideas acquire in her mind, the exaggeration of certain traits always possessed by her, her tendency to disintegration, by which she at times loses temporarily certain acquirements, like the French language: these and other peculiarities are evidence that a certain amount of disintegration took place in 1893 by which the original personality became fractured and modified. Nevertheless, it would be an exaggeration to affirm that she is a wholly new and distinct personality, or that she is a vigilambulist. It is more correct to say that certain components of her personality have become

disintegrated from the rest, certain local areas of her brain, as I have elsewhere put it,<sup>1</sup> have gone to sleep. The original self has become modified into B. I.

B. IV., notwithstanding her greater stability, is a still greater departure from the original self. Her character is totally different; her general attitude towards her environment has changed, for there is missing the taste for, that is, the normal reactions to music, literature and religion. She has also lost her knowledge of music. She has no emotionability, excepting bad temper; in short, she has retained some characteristics, and lost others of the original self.

Now any theory we adopt must explain all this, as well as the particular forms of amnesia and continuities of memory. The explanation which I believe to be the correct one is this: Neither B. I. nor B. IV. is strictly the original self, nor are they somnambulistic personalities, but modifications of the original self. The original Miss B. became disintegrated and as a complete psychical composition departed this life in 1893. B. I. and B. IV. are each different disintegrated parts of the complete Miss Beauchamp. In the disintegration of the primary consciousness a certain portion split itself off and became dormant. The remainder persisted as a modified personality—B. I. Sufficient remained to retain the memories of the past, which from this time became organised with all future experiences and made a continuous memory and personality.

The split-off dormant portion was awakened six years later as a result of an intense excitation of its constituent memories by the shock in the Library, and in the awakening wrenched away from B. I. a portion of her mental associations, which thus became common to both. As in 1893, a certain number of groups of psychical associations, that now belonged to B. I., remained split off and dormant. Those that remained awake became organised into another personality as B. IV.



It is as two circles which intersect one another. A part of each circle remains outside the other, while a part of the field is common to both.

Thus the dominant part of B. IV.'s consciousness, being awake up to 1893, remembers her whole life until that date; but being asleep from 1893 to 1899, she has no knowledge of the events of this

<sup>1</sup> See "A Contribution to the Study of Hysteria and Hypnosis, etc.," in the *Proceedings of the Society for Psychical Research*, Vol. XIV., p. 95.





All this might be expressed in more technical and more purely psychological language, but the principles would not be made clearer thereby, nor would the knowledge of the process be in any way added to. Disintegration means suppression of function of centres and groups of centres, and severance of associations between centres. Personality probably means particular composite groupings of centres by association processes. A difference in personality probably means a difference in the composition and grouping.

As to the physiological process, it must be that there exists normally some kind of physiological mechanism which allows disintegration to occur, and that normally, *within certain limits*, such disintegration is constantly occurring as a part of the mechanism of normal cerebration. In hysteria this physiological mechanism is carried to an extreme and pathological degree.

Whatever the anatomical change may be, I am at one with Sollier in believing it to be the *same as normal sleep, only localised*.<sup>1</sup>

To go back for a moment. There remains B. II. ; who is B. II. ? Now it is possible to hypnotise B. I. and also B. IV., so that both of these hypnotic states are quasi, but not perfect, personalities. At first I thought these hypnotic states were separate and distinct, and the one was called B. II. and the other B. VI. ; but afterwards it transpired that B. I. and B. IV. hypnotised became the same person, or B. II. B. II., then, is a part of B. I. and also of B. IV. Some elements common to them both,—the hypnotic self of both,—in her will are combined. If you ask who B. II. is, she says : “ I am myself,” or “ I am B. I.,” or “ I am B. IV.” or “ Both,” or “ I do not know—I am both.” B. II. knows the thoughts of B. I. and equally knows the thoughts of B. IV., but B. I. and B. IV. know nothing of B. II.

Now, if the original complete Miss Beauchamp is a total combination of the whole of B. I. and of B. IV., then if we could put I. and II. together, we ought to get Miss Beauchamp. This I was able to do by suggestion given to the hypnotic self, B. II., and to obtain the original self for a number of hours at a time. I suggested to B. II. that when awake as B. I. she would know all about B. IV., and as B. IV. would know all about B. I. and feel and think as B. I. did. I then waked her up successively as B. I. and as B. IV. In each case she knew all about the times to which I had special reference when I gave the suggestions. As B. I. she told me what she had been doing as B. IV., and as B. IV. what she had been doing as B. I. As B. I. she said she felt like herself in the early days before 1893, as she had not felt for years, and also knew what she had done as B. IV., which I had

<sup>1</sup> See “ A Contribution to the Study of Hysteria, etc.,” in the *Proceedings of the Society for Psychological Research*, Vol. XIV., pp. 95-6.

suggested she would remember. So that in her sensations and acquirements and memory, *she, when thus put together, to all appearances was the original Miss Beauchamp.*

This new personality is plainly a composite of B. I. and B. IV.; not only in memory, but in character, tastes, and general make-up. Sally calls her "that new thing," and has very little, if any, control over her. She herself does not know which she is,—I. or IV.,—but says she is both. When she is present, Sally tends to sink out of sight and go back—as Sally puts it—"to where I came from."

This final synthesis,—the construction of what appears to be the original self,—seems to me akin to a proof of the correctness of the diagnosis.

I should like to tell something of the way in which the subliminal self has contributed to the elucidation of some of these problems, by listening, so to speak, to the thoughts and studying the characters of these personalities, and reporting her observations; also of the social relations of the members of the "family" amongst themselves, and the extensive controversies carried on by the subliminal self by means of automatic writing; also of the subliminal communicating by speech when B. I. or B. IV. are in existence, and of the positive and negative hallucinations created by this self. But these and many other interesting phenomena must be left to a more extended report.

There is something unique in studying the laws of the mind through the intelligent observations of a subliminal self. This is but a brief sketch, and includes only a few of the salient features of this study.

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By way of Summary, I would offer for discussion the following conclusions which seem to be justified by this case:—

(a) The subliminal self may become developed into a true independent personality, which may be awake contemporaneously with the primary consciousness, or may be alone awake, the other personalities being asleep.

(b) Other so-called and apparent personalities may be nothing more than the primary self, mutilated by disintegration.

(c) The absence of knowledge and hence amnesia on the part of the primary self of the subliminal is dependent on the normal psycho-physiological arrangements.

(d) The amnesia of one mutilated self for another mutilated self is due to disintegration and to a severance and rearrangement of psycho-physiological associations.



(e) Theoretically any number of personalities are possible, according to the number and direction of the lines of cleavage. Each personality would depend on different combinations of different disintegrated pieces of the normal self.

(f') Personalities may develop accidentally, as the result of accidental fracture, without design, and not be the result of education.

(g) The subliminal consciousness is not necessarily the equivalent of the hypnotic-self.

(h) Personalities may represent any different psychical compounds. One may be that peculiar group of psychical elements which is called the subliminal self, and another may be a disintegrated compound of the ordinary supernatant self.

(i) Two or more personalities may have successive existences in time, or when one is the subliminal self they may be co-existent.

(j) Personalities, including the subliminal self, may be hypnotised, and thus the personalities may become still further disintegrated.

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*Additions since the list in "Proceedings," Vol. XIV.*

- BARTH (George), Mesmerism not Miracle. (3 Parts, bound together)  
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# PROCEEDINGS

## OF THE

# SOCIETY FOR PSYCHICAL RESEARCH.

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