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PROCEEDINGS

OF THE

Society for Psychical Research

VOLUME XLV

(CONTAINING PARTS 152-160)

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PROCEEDINGS

OF THE

Society for Psychical Research

PART 152

PRESIDENTIAL ADDRESS[1]

THE PROBLEM OF PHYSICAL PHENOMENA IN CONNECTION WITH PSYCHICAL RESEARCH

BY

LORD RAYLEIGH, Sc.D., LL.D., F.R.S.

THE field covered by our Society's work is broadly divided into physical and mental, or as Richet preferred to say, objective and subjective. Upon the whole, the Society has turned away from studies of the former kind, not of set purpose, but partly because good cases were difficult to find; and partly because other lines of investigation yielded more definite results. I think that this policy was and is justified, and if I occupy your attention with a somewhat unfashionable topic, it is only because I happen to be less ignorant about it than about the other and more fruitful branches of the Society's work.

In the time at my disposal I cannot pretend to be exhaustive, and moreover I must confess to being imperfectly acquainted with some of the continental literature, particularly on Eusapia Paladino. For the rest, I believe I have read most of what would be counted of first rate importance, and will try to sum up the impression I have gained from it.

The weak point of most of the earlier records of physical phenomena seems to be that the observers did not succeed in getting the phenomena sufficiently under control to allow them to have the same effect repeated again and again, so that specific doubts could be

[1] Delivered at a General Meeting of the Society on November 24 1937.

cleared up by renewed observation. So far as general scientific experience goes, this is the essential condition for getting light on any unexpected effect. Our natural gifts and faculties are not such as to allow us to observe and note everything that happens on a given occasion. Natural limitations make us pick out particular facts which strike us as salient, and we make our reconstruction of the event depend upon them. But, if the selection has to be made in a hurry, and without full warning as to what we are to observe, it is more than likely that we shall make the wrong selection from the observable facts: and then our reconstruction will be altogether wide of the mark: for we shall lack the essential material for a correct reconstruction. The success of the conjurer is one of the best illustrations of this. A good conjurer will almost always refuse to do a trick over again: or, if he pretends to comply with such a request, we may be fairly sure that he will really use quite a different method of producing the same effect: for he knows that if he really repeats his trick, he gives the observer an enormously improved chance of picking out the essential facts. What passed muster as a chance occurrence the first time will not do so again. Attention is concentrated on the apparently irrelevant circumstance, and this sets the observer thinking in the right direction. He arrives at a provisional hypothesis, and this may suggest to him a further question, to answer which he requires to see the performance a third time, or even more.

I believe all this will be much clearer if it is illustrated by a concrete example. As I have said professional conjurers will not repeat their tricks on request: but the regular performer on a public platform is at a disadvantage in this matter, for his repertoire is limited, and he must repeat his tricks: so that the earnest seeker after truth can see them as often as he pleases. For two generations Messrs. Maskelyne had a regular afternoon exhibition of first-class conjuring: and a young man of my acquaintance made it a hobby to try to penetrate their secrets, not always without success. In one of their tricks a man was placed on a kind of chair, with his knees doubled up to his chin. What purported to happen was as follows: A canvas cover was placed over him, but the audience could see underneath; and, moreover, an observer from the audience was behind. The right hand protruded through a hole in the canvas, and was touched by a member of the audience. The left arm was held up by a string attached to a strap buckled round his wrist, and this string passed out over pulleys to another member of the audience, who held it taut. At the word go, the right hand was withdrawn, the string attached to the left wrist became slack, and on removal of the canvas cover the man had disappeared. Where had he gone to?

My young acquaintance was completely mystified the first time he saw it. The second time he noticed that during the preliminaries, the canvas covering, which was extensible concertina fashion, and was lowered from above, was allowed to fall, as if by accident, to the floor for a moment, being then pulled up again. He vaguely remembered that the same thing had happened before, but since the right hand protruded from the opening throughout, he had not doubted that the man was still there, and had attached no importance to the incident. But the second time set his mind working, and he was inclined to regard this as a salient fact. He went home and thought about it. Was there no possibility that the man had escaped through the floor while the canvas was down? Perhaps the hand was not really his hand, though it was not easy to see how it could be otherwise. Anyway this point could be tested. Next time he went up on to the stage as an observer from the audience: when invited to examine the man who was to vanish, to see that he was not a "collapsible automaton", my friend took occasion to examine closely the characteristics of the finger nails of the right hand, whether they were long or short, clean or dirty, and so on. When the hand was thrust out for him to hold, he saw at once that it was not the same. He noticed further that the front legs of the chair were of hollow metal work—heraldic animals or something of the sort, so that an arm might readily be thrust through from below the stage, and allow the hand belonging to it to come out through the slit in the canvas screen. Thus the mystery was practically explained, for I need not pause over the details about the other hand. The moral I wish to emphasise is that the essential facts that the canvas screen momentarily descended to the ground, and that the front leg of the chair was big enough to contain a human arm, were displayed quite openly to the observer. But he did not at first see that they were at all relevant, and they did not fully penetrate his consciousness: so that he could not use them in his reconstruction of the scene, and he reconstructed it in a way that in fact depicted an impossibility.

The great, and indeed almost the only possible security against this kind of thing is that the phenomenon under investigation should be seen again and again, with full consideration between whiles, in order that any doubtful points in the observer's conception of what occurred should be noted, and the omission repaired on the next occasion.[1] I have, for the sake of clearness, used as an illustration a case where deception was admittedly being practised. But the

[1] A cinematograph film of the whole series of events would no doubt be equivalent.

same applies in all cases when we wish to give an accurate account of observed matters of fact, even when no question of intentional deception enters—*e.g.* in the ordinary researches of the physical laboratory. No experienced worker is satisfied by doing an experiment once; or even twice. If he sleeps on it, he will usually find that doubts have arisen by the morning, and he will perhaps repeat the observation many times, improving the conditions if possible, so as to concentrate on any weak points which have occurred to him.

After a somewhat prolonged study of the early accounts of observations of physical phenomena, particularly those observed with D. D. Home, the chief criticism I have to make is that they are deficient in this regard. Mr Podmore [1] usually emphasises that the description is inadequate for the reader to judge of what really happened: and he interprets the omission in the way least favourable to the phenomenon: indeed, to my mind, he strains the available data in this direction, suggesting for example that if a phenomenon is not stated to have occurred in the light, it must be assumed to have always occurred in the dark. We could easily make mincemeat of many of the classics of scientific investigation, if we allowed ourselves to criticise them in this sort of way. Upon the whole, the observer himself must be the judge of whether he had enough light to see what he describes, though no doubt an explicit statement about the light is desirable. I do not think that an attempt to reconstruct the inwardness of what occurred from the written account is likely to be often fruitful, even if it has been so occasionally. It is impracticable to record *every* condition of a phenomenon in case it might be wanted. Would anyone draw the moral from the case I have given above that we must always describe the legs of the chairs and tables used, or examine finger nails in case these details might be relevant? Clearly not. To do this kind of thing would over-burden the account intolerably. The essential thing is that the observer himself should detect when his account requires to be supplemented, and himself supply the omission: and he can only do this if he can get the opportunity to repeat his observation, if possible under improved conditions.

Upon the whole, the well-known accounts of observations made in the presence of D. D. Home do not show the observers as alive to the importance of this; and so far as they are deficient in this respect, they are to my mind subject to a heavy discount. Lord Dunraven indeed tells us that he was brought into contact with

[1] *Modern Spiritualism*, London, 1902; and *The Newer Spiritualism*, London, 1910.

Home by accidental circumstances and that he did not feel himself specially qualified or inclined to pursue the subject in its scientific aspect: when the first sensation had worn off, his interest seems to have diminished.[1] The Master of Lindsay (afterwards 26th Earl of Crawford), one of the other chief observers of D. D. Home's phenomena,[2] did not give very systematic accounts of what he had seen. He gives indeed general assurances that he had been able to satisfy any doubts he may have originally felt, but he does not seem ever to have contemplated a detailed dissection of *e.g.* the phenomenon of levitation, such as was afterwards made by Dr Crawford of Belfast.[3]

When we come to the investigations of Sir William Crookes,[4] we find, as might be expected, more attempt at systematic investigation. Experiments were planned to improve the conditions, and clear up doubts and obscurities. For example, in reference to "materialisations" Crookes made repeated attempts to see the medium and the materialised figure together, and considered that he had succeeded. This, however, was effectively criticised by Podmore.[5] Further, he took photographs of the "materialised" figure. The unexplained movements of a pivoted lever obtained in the presence of D. D. Home were recorded graphically on a smoked glass, and various experimental precautions were taken to improve the conditions under which the other phenomena were obtained.

Whatever may be thought of its conclusiveness, there is no doubt that this work represents a definite advance in the spirit in which it was conducted. It does not go far enough however. As well as can be judged, much of the time was spent in waiting for fresh marvels,

[1] *Proceedings, S.P.R.*, vol. xxxv, pp. 21-25, 1926.

[2] *Report on Spiritualism of the Committee of the London Dialectical Society*, London, 1873, pp. 206, 213.

[3] I have asked the present Lord Crawford whether any unpublished letters or memoranda of his father's on this matter survive. He kindly made search but failed to find any. He has also written to me of conversations with his father, who adhered to the position that he had seen levitation in Home's presence, but coupled this with the impression that Home was a charlatan. It is difficult to know what to make of this, but I think it must tend to discount the force of his evidence in favour of the phenomena.

It need scarcely be said that the work of the late Dr W. J. Crawford of Belfast is not connected in any way with the experiences of the late Lord Crawford with D. D. Home. The identity of names is merely a coincidence.

[4] *Researches on the Phenomena of Spiritualism*, London, 1874.

[5] On the ground that this seance was not in Crookes' house but at the medium's own home and that the medium might have been personated by a confederate. See *Modern Spiritualism*, vol. ii, p. 155.

under easy-going conditions, rather than in trying definitely to confirm and elucidate those already obtained. I am far from saying that Crookes was to blame for this. It is quite likely that he pressed the policy of persistent investigation as far as he was allowed. The same may be said of many subsequent investigators, who would no doubt have liked to push their enquiries in the direction here advocated, but found that they were not given a chance of doing so.

This brings us to near the close of the epoch of D. D. Home and his contemporaries. Many striking facts had been recorded, by apparently trustworthy witnesses. Their evidence has been discussed at great length by numerous critics, and the utmost has been done to shake it: but, upon the whole, with only limited success. The statements then made still constitute a *prima facie* case for investigation, and it is hard to see how anything that can be done in the future, however negative, can altogether destroy the effect of these records: for failure to repeat can hardly constitute a disproof, when the identical conditions cannot be re-established.

Up to this point some of my hearers may think that I have been over lenient, if not credulous, in my estimate of the evidence. Perhaps what I am now about to say may be a partial corrective. Even if it should eventually become possible to establish the reality of phenomena of this kind to everyone's satisfaction, it would be of very little use, unless we could succeed in going further, and fit them into their proper place in the general scheme of things. Merely to realise that there are more things in heaven and earth than are dreamed of in one's philosophy is hardly an end in itself. The end should be to expand one's philosophy so as to include them.

Consider the phenomenon of lightning as an illustration. There has of course never been any doubt about the reality of this, though in early times it appeared so anomalous and unaccountable that it was regarded as a direct sign of the anger of the gods. (Then, as now, there was a tendency to jump to the conclusion that unexplained phenomena were to be attributed to the arbitrary action of invisible beings.) The observed fact did not at all contribute to the general edifice of scientific knowledge. It lay about like a loose stone of odd size, and served no purpose, until Franklin, by his experiment with a kite, demonstrated that lightning was an electrical phenomenon, and thus fitted the loose stone into the general structure. Builders take no interest in stones of odd size until they can find a place to fit them into. That gives the key to why our studies are so little regarded by the scientific world.

We may go a step further, and consider the more obscure phenomenon of globe lightning, which, as described, consists of detached balls of fire. My father made reference to this in his presidential address to the S.P.R. in 1919. Since he wrote, many more cases have been recorded, and photographs have been published [1] which, while not altogether in accordance with some of the descriptions, seem to prove the objective reality of occurrences having this general character. But they are scarcely understood any better than before. The reason of failure is no doubt that we cannot command them at pleasure so as to devise and carry out experimental tests. That is the same kind of difficulty as we meet with in connexion with our own problem. Some people think it is in itself a ground for incredulity: but this is a train of thought which I have never been able to follow. What they probably have at the back of their minds is that when an effect occurs sporadically, at a time that cannot be foreseen, it is not likely that the best witnesses will be on the spot, and even if they are, they may be taken by surprise, and cannot confirm or revise their impressions of what happened by frequent repetition, as I hope I have convinced you it is essential to do.

In the early days of the Society much attention was paid to physical phenomena, and now the spirit of persistent investigation became dominant, largely under the influence of Mrs Sidgwick, and, later, of Dr Hodgson. The investigation turned chiefly on the question of whether control was adequate or not, and did not get much further. It must be admitted that the results were disappointing, for after spending much time and trouble, especially in connexion with the medium Eusapia Paladino, no definite result was arrived at which commanded the general assent of prominent members. It was indeed common ground that Eusapia had been caught fraudulently evading control, and almost common ground that she must have assiduously practised the methods of deception which she used; but here agreement ended. Some, including Sir Oliver Lodge, Mr Everard Feilding, Mr Hereward Carrington and Mr W. W. Baggally, were convinced that on occasion she exhibited genuine phenomena of telekinesis and the like. On the other hand Dr Hodgson, Mrs Sidgwick and Mr Podmore remained unconvinced.

Since those who were concerned with the actual observations were not able to agree, it is not likely that any subsequent armchair study will help to clear the matter up. My own feeling is that the effects which were relied on by the believers did not differ enough from those generally admitted to be fraudulent to afford any firm basis on which to build further.

[1] Jensen, *Physics*, 4, p. 372 (1933).

We come next to the investigations of the late Dr W. J. Crawford of Belfast with Miss Kathleen Goligher as medium.[1] Dr Crawford was a lecturer in mechanical engineering at the Municipal Technical Institute of Belfast, and from enquiries I have made he seems to have impressed other scientific men favourably. The medium usually sat with a circle of her own friends and relations, a fact to which due prominence must be given.

Dr Crawford's work was largely directed to determining the mechanical reactions of the forces which came into play in the levitations he observed. For this purpose he placed the medium on a weighing machine, to determine whether the seat of the reaction was on her. He worked by the light of a red lamp, of which more will be said later.

Dr Crawford describes how he was able to have a table weighing about 10 lbs. levitated and kept steady about eight inches up in the air for as long as he required to make a test of the addition to the ordinary weight of the medium. This was two or three minutes, and apparently he could have had more, for on each occasion he indicated that he had finished. It was found that the medium gained weight about equal to that of the table. These steady conditions could only be obtained after the sitting had continued for some time.

Dr Crawford interprets this gain of weight by the medium as due to an invisible cantilever or rigid bracket, which comes out from the body of the medium, and supports the table. Mrs Sidgwick, in a review of Dr Crawford's first book, hints that this " cantilever " is nothing else than the medium's leg. Many statements in Dr Crawford's various publications, however, are definitely at variance with this hypothesis. Thus (*E.P.S.*, p. 119) " Practically no palpability is experienced when one cuts through the psychic structure with the hand, or, say, with a piece of wood." [2]

Dr Crawford's hypothesis is, however, so fraught with mechanical difficulties that it is questionable whether it really helps much to correlate the facts he has determined, assuming that these latter are correct. To begin with, it is almost self-contradictory to postulate a structure which is rigid to act as a cantilever, and not rigid at all for the hand or a piece of wood to pass through it. The attempt to imagine a medium rigid for some purposes but not for

[1] Recorded in his three books : *The Reality of Psychic Phenomena*, London, 1919, here referred to as *R.P.P.* ; *Experiments in Psychical Science*, London, 1919, referred to as *E.P.S.* ; *The Psychic Structures at the Goligher Circle*, London, 1921.

[2] See also *R.P.P.*, p. 87.

others is not new to scienee. Problems of this eharaeter arose in eonnexion with the elastie solid theory of the luminiferous ether, whieh was to show rigidity for earrying rapid transverse vibrations, and fluidity to allow solid bodies, *e.g.* the earth in its orbit, to pass through.[1] Lord Kelvin at one time appealed to the properties of bodies like hard pitch or eobbler's wax, whieh while reaeting to very rapidly alternating forees, will yield viseously in time to forees applied steadily in one direction.[2] But to satisfy Crawford's hypothesis the requirement is that there should be great stiffness for persistent forees, and fluidity for more transient ones. This is too much to ask.

Another diffieulty is boldly stated by Dr Crawford (*E.P.S.*, p. 117). "How ean it be", he says, "that a rigid strueture two or three feet long can issue from the medium's body and support 30 or 40 lbs. weight at its end, and the medium experienee no ineonvenienee?"[3] Dr Crawford has his own tentative answer to this question, though I eannot personally feel satisfied by it. But in this and other instanees the eandid way in which speeifie questions are faced produees a favourable impression, compared with the mere appeal to mystieism of so many writers on these subjeets. Dr Crawford's theory perhaps raises more diffieulties than it answers; nevertheless, if work of this kind is ever satisfactorily built into the scientifie edifiee, I do not doubt that he will rank as a pioneer.

[1] A short explanation of this matter may be useful. When it had been established that light, like sound, was of the nature of a wave movement, it was considered necessary to postulate a medium in order to convey it. If the waves were waves of compression, as is the case of sound travelling in air or water, then a fluid medium would do, and there would be no particular difficulty in understanding how solid bodies could pass through it. But waves of compression could not account for the phenomenon called the polarisation of light. When a ray of light passes through a suitable polariser, such as a tourmaline crystal, it acquires "sides" as Sir Isaac Newton expressed it. It is no longer an indifferent matter if the beam is rotated on its own axis. We can prove this by a second tourmaline crystal. This will only transmit the beam if it is placed parallel to the first tourmaline. If crossed with the first there is no transmission.

Now waves can only give room for effects of this kind if they are transverse to the direction of propagation. It is clear that there can be nothing of the sort in compressional waves for in these no one transverse direction can have preference over any other. It was therefore concluded that the vibrations were transverse. But (apart from what happens at a free surface) fluids cannot transmit such waves. Elastic solids can do so in virtue of their stiffness. Hence the elastic solid ether.

[2] I need scarcely say that the elastic solid ether is now superseded by quite a different order of ideas.

[3] This of course applied to a different experiment from that already referred to with the 10 lb. table.

Space is lacking to go into further particulars of Dr Crawford's work. It is necessary to mention that he died by his own hand before the whole of it was published. I shall return to this point a little later.

About a year after Dr Crawford's death, the late Dr E. E. Fournier D'Albe proceeded to Belfast and had a series of sittings with Miss Goligher, with a view to confirming and extending the work of Crawford,[1] which had impressed him favourably. He failed to obtain any phenomena which he could regard as evidential. It was apparently admitted that they were not so (p. 43). Although he expressly reserves the question of whether *any* of Dr Crawford's results could be accepted as supernormal it is pretty evident that he thought they could not be. Towards the end Crawford had obtained numerous photographs of what he regarded as "psychic structures" rendered visible under special conditions. They are published in his last book. The half tone blocks made from these photographs are undoubtedly very suggestive of pieces of muslin or the like, hung from the bottom of the table or knotted on to its legs. Fournier gives similar pictures of much better definition published as actual photographic prints, not half tone blocks, and I fully agree with him that they show the material to be a woven product. This is the most damaging feature in the whole case.

Fournier also tells in detail how he saw the medium raising a stool with her extended foot. Fournier worked as far as possible under the same conditions as Crawford. He had the same circle of sitters, and in one instance actually held the sitting in Dr Crawford's house. He also used the identical appliances, lent by Mrs Crawford. These circumstances are of some importance because they show that she remained on friendly terms with the medium, and can scarcely have attributed Dr Crawford's collapse to his having been ultimately convinced that the medium had deceived him. Dr Crawford stated in his posthumous letter that this was not the reason of his breakdown, and I, for one, accept his statement.

No really valid reason seems to be known for doubting the candour and accuracy of either Fournier or Crawford, so far as they are describing what they themselves observed. Fournier says (pp. 48, 49): "I have no reason to doubt the conscientious and accurate character of Dr Crawford's observations and records."

Fournier thinks that Crawford was too soon convinced that all was well, and relaxed his vigilance prematurely. But Crawford's letters written during the last few months of his life, and given by

[1] *The Goligher Circle*, London, 1922.

Fournier (pp. 66-70), negative this view, for they are full of details of the various precautions which he took.

Fournier emphasises strongly that the shadow of the table afforded protection for fraudulent manipulation, but there are passages in Crawford's books which seem to be a complete answer on the point. Thus (*R.P.P.*, p. 13) : " Even with the largest table it is sometimes possible to see completely underneath (as I have done), to see the feet and bodies of all present at rest, and hands held together in chain order, while the table has been steadily levitated."

Again (*Psychic Structures*, p. 8) : " A strong red light was falling upon the space below the levitated table while another source of red light was showing from behind so that the whole area between the medium and the levitated table was itself quite visible, and I shifted my position into various positions in the circle, looking at the space below the table from different angles. But to all appearance the space was empty. . . ."

If we accept this statement as being, in Fournier's own words, " conscientious and accurate," I think it is clear that his criticism fails.

Fournier says (p. 49) : " The tests to which he [Crawford] submitted the medium completely satisfied him as to her *bona fides* so that he no longer thought it necessary to control the other sitters as well." It is, however, instructive to compare this with a passage from Crawford (*R.P.P.*, p. 16) : " The experiments in Chapter III show conclusively that while the table is steadily levitated nearly the whole of its weight is upon the medium. Therefore it follows that if anyone is lifting the table with any part of his body, it is the medium, and the others are not concerned."

Moreover Crawford states (*R.P.P.*, p. 81) that he was allowed to move anywhere between the sitters and the levitated table *except immediately in front of the medium*. If this statement too is accepted as " conscientious and accurate ", it is difficult to see the force of Fournier's remark above quoted, in which he suggests fraud of the sitters as the factor neglected by Crawford : the more so that he claims to have seen " levitation " achieved by the medium's foot. Fournier does not seem to have really made up his mind whether the table was fraudulently raised by the medium, or fraudulently raised by the sitters.

I must frankly admit that I am unable to sum up this case to my satisfaction. Fournier does not profess to discuss Crawford's work in detail, and he seems tacitly to admit that for all he can say some part of it may have been correct, though he evidently does not think so. Crawford's publications contain a complete answer to

Fournier's general objections, and I am unfavourably impressed by Fournier's failure to notice this. On the other hand, Fournier does seem to have proved that the medium was on occasion fraudulent. It is difficult to understand what could have been her motive in continuing to deceive Crawford for the first three years, during which there was no payment. It is also difficult to discount either witness. Crawford is confirmed on the main points by several other observers.[1] Fournier stands alone, but produced his photographs, showing the woven texture of what purports to be a "psychic structure". Both records are very satisfactory in point of detail and internal consistency, standing far above the available accounts of D. D. Home in this respect; and the very matter-of-fact style of Crawford's narrative makes any idea of hallucination seem altogether out of place. In this unsatisfactory position I must leave the case.

Finally, I come to Rudi Schneider. A great deal has been written on this medium, much of it of a polemical character. It would be tedious and unprofitable to go over this in detail, and discussion must be limited to the work of MM. Eugene and Marcel Osty in Paris,[2] passing over other investigations with the remark that I am not convinced by the evidences of fraud which have been put forward. I shall, however, give due consideration to the question of whether fraud could have affected MM. Osty's results.

MM. Osty initially fixed their attention on the phenomenon of telekinesis, or moving of objects by an unknown agency in the presence of the (controlled) medium. It is generally known that a beam of light can be used to guard a treasure, a warning bell being rung when a potential thief approaches too near and intercepts the beam. The method depends on an obvious application of the photoelectric cell, a device which passes an electric current when light falls upon it.[3]

Now it occurred to MM. Osty that by using a beam of infra-red light instead of visual light the method could be carried out in a dark seance room and that the object (flower, handkerchief and the like), proposed for telekinesis could be guarded by the beam, so that if the medium succeeded in escaping from the controller who was by

[1] See in particular Whately Smith, *Proceedings S.P.R.*, vol. xxx, p. 314, 1920.

[2] *Les Pouvoirs Inconnus de L'Esprit sur la Matière*, Paris, 1932.

[3] It is necessary of course to arrange a relay to switch the bell current *on* when the photoelectric current is cut *off*.

way of holding him, and attempted to seize the handkerchief in order to move it, a bell would ring and give warning.

It was found in fact that the bell did ring on frequent occasions when the medium was, or purported to be, in trance. The ringing was sometimes maintained for half a minute or even a minute. Flashlight photographs were taken while the bell was ringing, and they revealed the medium sitting in his usual hunched position, with his head sunk forward, his hands held, and his knees between the knees of the controller. Nothing was to be seen in the path of the infra-red beam.

It seems therefore that whatever it was that obstructed the beam, it was not an ordinary solid obstacle. Whatever its nature, it seemed to fade away under the influence of ordinary light: for lateral illumination of the beam, although it did not reveal anything, had the effect of promptly stopping the ringing of the bell.

Although the infra-red beam was intercepted as if something was trying to reach the handkerchief or other object, the latter was not often moved, and the experimenters wisely decided to abandon this as a primary object of study, and to concentrate attention on the phenomenon that was more easily obtained, namely the obscuration of the infra-red ray. The bell was replaced by a galvanometer with a photographic recording drum adapted to give a continuous graph of the deflections. The obscurations previously indicated by the bell were now photographed on the drum, which gave a record of intensity and duration.

After the work had been in progress for some time the ordinary galvanometer which had been in use was replaced by one of quick period ($\frac{1}{10}$ sec.) with the view of examining in more detail how the obscuring action set in. [I must explain for the benefit of readers not versed in physics that scientific measuring instruments are not effective for recording changes appreciably more rapid than their own free period of vibration. Instruments of long period are too sluggish to follow quick changes.] With the quick acting galvanometer, a very significant fact was noticed. When the ray was (partially) obscured, it was seen that the galvanometer spot moved in sympathy with the loud and rapid breathing of the entranced medium. The expiration and the inspiration each involved a muscular effort: and the number of obscurations of the infra-red ray corresponded with the number of these muscular efforts. It was clear therefore that the obscuring action was connected directly or indirectly with the medium's muscular processes. This fact alone seems to rule out confederacy.

When the above relations had been recognised arrangements

were made for automatically recording the motions of the medium's chest on the same chart as the infra-red absorptions. Several such charts are reproduced by the authors, and allow the frequency relation to be verified. For example when the infra-red absorption has a frequency of 5 per second, the breathing has a frequency of 2·5 per second.[1]

These graphs published by MM. Osty are in my view one of the most valuable contributions ever made to our subject. The critics who have discussed their work adversely appear to me to have totally failed to face up to them : indeed many of the criticisms that I have seen are completely answered in advance by the original publication, so that it hardly seems worth while to answer them further. The great value of the graphs is that they carry their own tale, and that every student can examine them for himself, nearly as well as the original experimenters could do. The destructive critics have here an unrivalled field for the exercise of their talents, but so far they have not made any effective use of it.

I have read a criticism which suggests that Schneider made fraudulent use of the phenomenon of resonance. It is suggested that he timed his breathings in such a way that the movements of his body were in unison with the free period of swing of one of the supports of the apparatus. It is assumed that a large oscillation of the support was worked up in this way, enough to secure that only part of the beam entered the photo electric cell, thus simulating an absorption. I do not say that a suspicion of this kind is unreasonable *prima facie*. On the contrary, it is the sort of thing that constantly haunts the night thoughts of the careful experimentalist, and should cause him to look to the supports in the morning, and to see whether a vibration can in fact be worked up in the way suggested. Unfortunately that sort of test can only be applied at the time : but there are other tests which can be applied to the graphic records. One point is that, on the view suggested, the frequency recorded must be rigidly equal to the frequency of the free vibrations of the support, and therefore constant as long as the arrangements are unaltered. This test is not satisfied. For example,[2] on June 11, 1931, at 22 h. 49 m. the rate was 5 absorptions a second, and three minutes later, the rate was 8 absorptions a second.

Another point is that on this hypothesis the mechanical oscillation is gradually worked up, and therefore that its effect progressively increases. The successive absorptions ought to increase regularly in

[1] *Complete* periods are always referred to, *e.g.* in breathing, from the beginning of one inspiration to the beginning of the next inspiration.

[2] *loc. cit.*, p. 125.

amplitude and then gradually die down. But when we examine the graphs, we find nothing of the kind. In the first absorption of a series they have an average value, and exceptionally large absorptions occur quite sporadically, and not as a culmination. So far from dying down gradually, in one case the very last absorption is much the most intense.

In various criticisms it is suggested that Schneider had a confederate in the room, or that he succeeded in evading control. In some investigations, for example, those concerned with Eusapia Paladino, such questions were fundamental. I do not think they are so here. It is useless to suggest that Schneider might have evaded control unless that will help us to explain how he could have produced the periodic absorption at this very high frequency, in exact time with his breathings. Again, as regards the hypothesis of confederacy, I have already pointed out that the connexion with his breathings indicate that *he*, and not a confederate, is producing the effects.

The point that specially appeals to me is that we have a definite correlation between two phenomena, the deep breaths taken by the medium, and the absorptions of the infra-red ray, which correlation depends on a very large number of repetitions of each phenomenon. So much of what has been done in this field, however earnest the attempts of the experimenters, has only led to casual and sporadic observations; not helping, so to speak, to connect any two pieces of the puzzle. Here, however, two pieces of it are definitely connected: the loud rapid breathing and the periodic infra-red absorptions.

If it is objected that this connexion is a trick carefully arranged by Schneider, we must remember that the connexion itself was not perceived until well on in the investigation a quick period reflecting galvanometer of the necessary high sensitivity was procured. This is an out-of-the-way instrument (I doubt whether most University professors of physics were aware that such an instrument was on the market, at any rate I was not aware of it), and even when it was procured the above connexion was only noticed accidentally. To suppose that Schneider knew about quick period reflecting galvanometers, and laid his plans in anticipation of this sequence of events, would on the whole be absurd.

What, it will be asked, is the nature of the substance or agency that obscured the infra-red ray in these experiments? Is it the same as the material of the "psychic structures" in Crawford's experiments? Or is it identical with the "ectoplasm" of which we have heard so much? I am afraid I must decline to enter upon

these questions, which are altogether premature. I am not sure that Crawford's conclusions are correct, and I do not know of any observations on ectoplasm under really satisfactory conditions. We can and probably shall speculate about it in our own minds: but the Society's publications should not be encumbered with guess work. What is wanted is an independent repetition of Osty's experiments.

In the meantime, it is necessary to emphasise what seems to be a serious difficulty, when we come to consider the properties which Dr Osty finds in the invisible substance.

So far as we may claim to know anything about the action of light, I think we may say that it cannot modify matter without being absorbed in the process. Many illustrations of this principle might be given, but I will limit myself to one. Ordinary photographic plates are sensitive to blue light: and corresponding to this we find that the sensitising substance, bromide of silver, absorbs blue light. On the other hand, yellow and red light are not absorbed, and we find that the plate is not sensitive to these kinds of light. We may make it sensitive to them by staining it with certain dyes. But in doing so we have introduced an absorbing power for red and yellow light, so that the principle still holds.

Dr Osty's whole investigation depends on the fact that infra-red rays are absorbed by the "invisible substance". He considers that under the condition that prevailed this absorption was effective for waves of length 1μ ($\mu = \frac{1}{1000}$ of a millimetre) upwards, and he emphasises that shorter waves were not absorbed,[1] though he thinks that under other and better conditions they might be.

Further, the fact that the substance is "invisible" shows that it does not absorb ordinary visual light. If it did so, as, *e.g.* iodine vapour does, it could not of course be invisible, but would be seen dark on a luminous background. Or if it absorbed by lateral scattering, like an ordinary aqueous mist, it would itself appear as a bright cloud when illuminated.

But if the substance does not absorb visible light, how can it be that visible light destroys it, or at all events makes it ineffective?[2] If we accept the principle which I have explained, that light can only modify a substance when that substance absorbs it, then an invisible substance cannot be destroyed by visual light. Until this paradox is cleared up, it cannot be said that we are standing on firm ground.

[1] Myers Memorial Lecture, 1933. *Supernormal Aspects of Energy and Matter*, p. 21.

[2] See above, p. 13.

The supernormal knowledge which, as all real students of the subject are now pretty well agreed, is shown by some mediums in trance, is largely admixed with incorrect or nonsensical matter, or with the attempt to get information from the sitter by "fishing" and attempting to pass it off as supernormally acquired. It is an empirical fact that the attempt (whether genuine or fraudulent) to produce apparently supernormal physical happenings frequently accompanies these trance utterances, and the trance personality without doubt wishes to claim the credit of having produced them. If we accept the view that these are occasionally genuine, then it would be natural to regard the fraudulent tricks played in trance as having the same relation to the genuine phenomena as the worthless utterances have to those which show supernormal knowledge. The trance personality, we may suppose, is willing to deliver genuine goods, whether of the mental or physical kind: but if not successful in doing so, it will produce the fraudulent ones rather than none at all.

This line of thought, it seems to me, should make us less ready than we otherwise might be to at once discard all phenomena produced by a medium who has once been detected in fraud. If we had at once discarded the whole series of trance utterances or automatic writings in which one misstatement of fact had been detected, the Society's labours would have been less fruitful than we venture to hope they actually have been.

I am inclined to sum up by saying that while a great part of what has been reported under this head, *e.g.* slate writing, materialisation of complete figures, is fraud, deliberately prepared by impudent imposters, and having nothing to do with the phenomena of trance, yet there is another part intimately bound up with the trance personality. Much of this latter is likewise fraudulent, but there seems to be an appreciable residue which has not been successfully dissolved by the acid of destructive criticism which has been so freely poured over it. The evidence seems to stand, and if we dogmatically reject it we shall be open to the reproach of laying down what *ought* to be the order of nature, instead of observing what *is*. If it is difficult to reconcile with our other notions, that may only be because these require to be revised or extended. Physical science has had to make adjustments of that kind often enough in the last few decades, and it would be rash to conclude that we have reached finality. But if adjustment is really necessary it will hardly be made until we have the phenomena under control for detailed examination to a much greater extent than has usually been attained. The investigators have usually been driven to give in to

every whim of the trance personality, to submit to producing noise, and to wait for interminable hours in the darkness. They have been threatened with dire results if they ventured to touch, or examine without permission. It is almost impossible to find out anything definite under these conditions: and I cannot but agree with Dr W. F. Prince and others who have insisted that it is a waste of time and energy to attempt it any more. That better conditions are not impossible of attainment, the work of Crawford and of Osty seems to show: and if the present unsatisfactory condition of affairs is to be bettered, it can only be by following the example which they have set. It would be of the utmost value to obtain independent confirmation of their results.

In this address I have tried to be strictly fair. It will perhaps be felt that the result is an unsatisfactory halting between two opinions. I must admit that in the course of study my estimate of the evidence has in several instances tended to become less favourable as I proceeded. Nevertheless, in some cases I am still quite unable to form any probable guess as to how the investigators could have been deceived.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 153

THE QUESTION OF LIGHTS SUPPOSED TO HAVE BEEN OBSERVED NEAR THE POLES OF A MAGNET

By Lord Rayleigh, Sc.D., LL.D., F.R.S.

A quotation from Sir William Barrett (1924) will fitly introduce the present discussion. He says :

" Personally I am very anxious that earnest attention should be given to the so-called Reichenbach Phenomena, wherein certain sensitives after long immersion in complete darkness perceive a luminosity emanating from the poles of a magnet, and also from the human fingers. I have published in the *Philosophical Magazine*, and also in the early volumes of our own *Proceedings*, the experiments which led me to the conviction that such phenomena do really occur under suitable conditions." [1]

When Barrett speaks of " perceiving ", he seems to mean visual perception which makes use of the physiological mechanism of the eye. There are several definite reasons for interpreting him in this way. One is the mention of long rest in the darkness. It is, of course, well known that visual perception becomes more acute after such rest, which may be prolonged with advantage up to at least half an hour. Secondly he uses in some of his experiments the test of interposing an opaque screen between the magnet and the observer, which was thought to cut off the effect. If the perception in question were of an extra-sensory nature, there would seem to be no point in either rest in the dark or use of an opaque screen. Further it would

[1] *Proc. S.P.R.*, Vol. XXIV, p. 290. 1924. The earlier papers referred to are : *Proc. S.P.R.*, Vol. I, pp. 99-100, and *Phil. Mag.* (fifth series), Vol. XV, p. 27. April 1883. Reichenbach's original " Researches " are known to me in a translation by Gregory, London 1850. They are quite uncritical.

be hard to explain the pain in the eyes experieneed by one of the observers from the strain of watehing in the dark.

The mention of " sensitives " suggests that some persons are able to see much fainter lights than others. We are reminded of the " hyperaesthesia " sometimes invoked to explain away apparently suceessful experiments in telepathy. We may at this point usefully ask what evidence there is that some persons have a far better vision for the pereeption of faint lights than others ? It is probable that young eyes tend to be better than old ones in this respect. Further, it is eertain that some persons are " night blind ". But sueh cases are definitely pathologieal. I have not been able to find or hear of eases of very exeeptional sensitiveness, but if anyone elaims to be able to see the Reiehenbaeh phenomenon, he should eertainly be tested with ordinary lights to determine whether, and if so in what degree, his power of deteeting faint sourees exeeeds the ordinary. Further diseussion of this point is given below.

Barrett proposed the use of a photographie test, and speaks of some preliminary experiments in this direetion but he does not give the impression that the test was pushed to anywhere near its limits. We know definitely that the modern (panchromatic) plate ean detect all the radiations to whieh the eye is sensitive, and some others (ultra violet and X-rays) as well. The general experienee of astronomers shows that when dealing with faint sourees photographie exposures of a few hours reveal mueh more than the eye ean see. It appeared worth while therefore to put the question to a more severe test than has hitherto been applied.

The luminous appearanees deseribed purport to be seen in the air near the poles of the magnet, and if the problem is regarded as suseeptible of seientifie treatment at all, it would seem hard to escape from the conclusion that the luminosity is conditioned by the intensity of the magnetic field, and by its depth in the direetion of vision. Electro magnets have certain advantages, but a permanent magnet was preferred. It has the great merit that the plate ean be exposed to it for any length of time without maintenanee, and with no possible complieation arising from the heat of the exeiting eurrent, which might possibly lead to eertain spurious effects.

It is true that an eleetro magnet is more powerful, but the modern magnet steels have enormously inereased the power of permanent magnets, so as largely to discount this consideration.

In the experiments I am about to describe, I used a permanent magnet of the kind used for loud speakers (Figs. 1 and 2). It was kindly given me by the Atlas and Norfolk Works, Sheffield, through the kind interest of Dr W. H. Hatfield, F.R.S., and was made of the

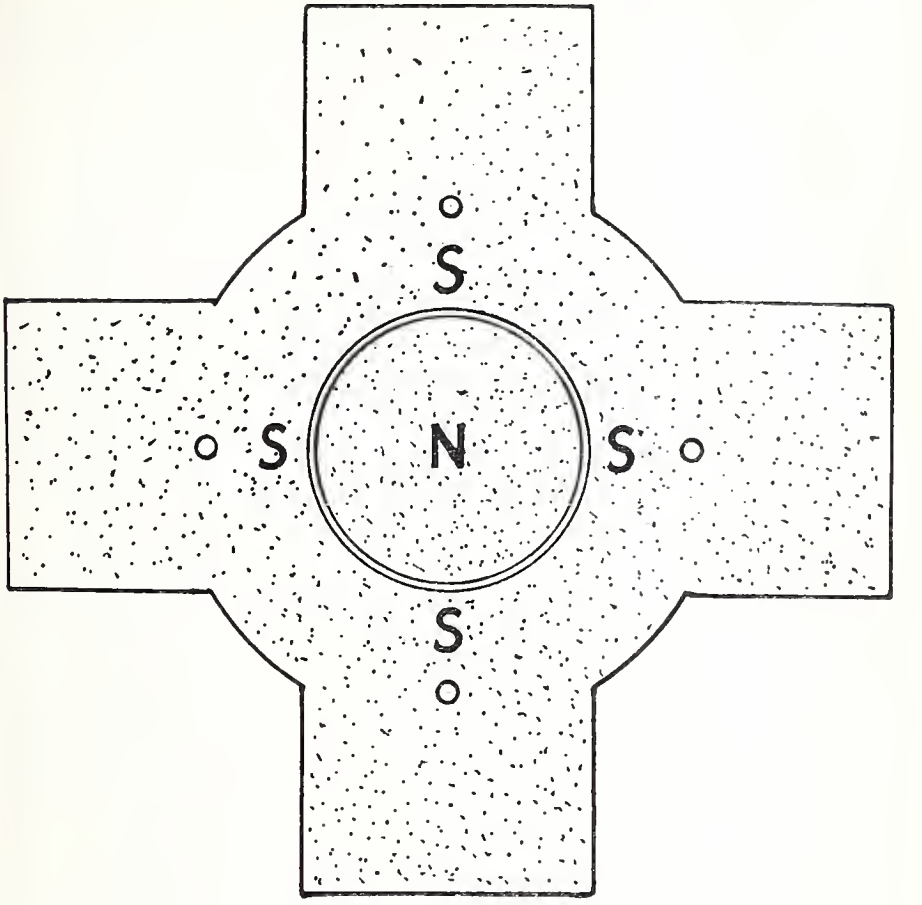


Fig. 1.

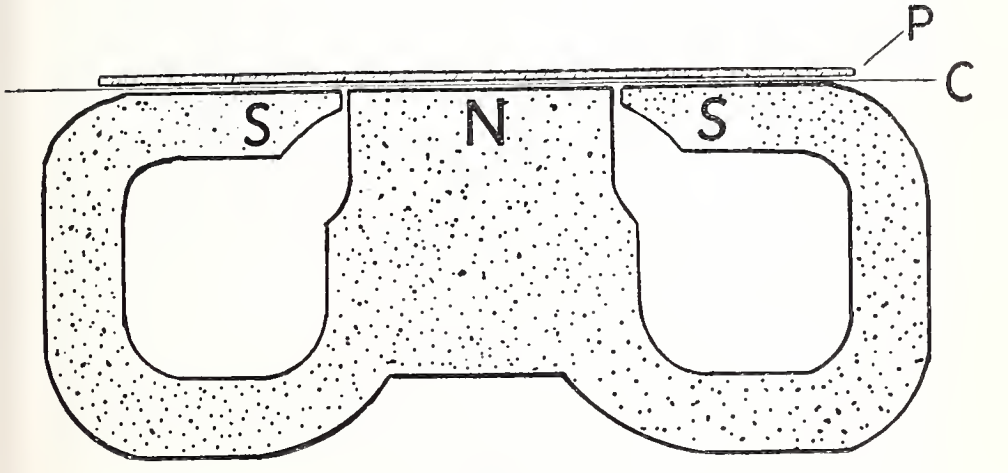


Fig. 2.

cobalt magnet steel developed by them. The narrow gap between the poles in magnets of this type is advantageous, since the magnetic force there is very intense, being only a few times less than the maximum attainable with powerful electro magnets.

Fig. 1 shows the face of the magnet, Fig. 2 shows a section through two of the four arms.

The north pole *N* is constituted by the central circular piece, and the south pole by the edge of the ring-shaped aperture in which *N* lies. The narrow gap between them is the region of intense magnetic force.

It is 26 mm. (about 1 inch) diameter and 1·2 mm. breadth. The flux density in the gap was 8000 lines per square cm., and the depth of the gap 6·5 mm. The magnet was placed in a light-tight box in a dark cellar, and the photographic plate laid on it (*P*, Fig. 2), with a thin film of celluloid sheet *C* in between. This, however, was not used at first.

Laying a "hypersensitive" panchromatic plate directly on the magnet, and exposing for a month, nothing could be detected on development. But on giving a five months' exposure a dark ring appeared on the developed plate corresponding to the size of the circular gap; and I thought at first that it might possibly represent the effect that was being sought. On further examination a similar darkening was noticed in the region outside the magnet, and also opposite the screwed holes intended for attaching other parts to the magnet (these holes are indicated in Fig. 1). These regions are not the seat of magnetic force, and therefore the darkening is to be referred to some other cause. It is known that certain vapours give rise to a chemical action on the photographic plate, simulating the action of light: and in long exposures effects of this kind are always to be feared.[1] For our present purpose it is not necessary to discuss them further. The point is that part of the plate was protected by contact with the planed face of the steel magnet, and that the other parts not so protected were blackened.

To guard against this, a sheet of thin cellophane was placed between the magnet and the photographic film so as to cover the latter all over, and protect it from free access of vapours. Another exposure of five months was given and the result was definitely negative. No trace whatever of the circular area where the magnetic force subsists could be seen on the plate.

It would scarcely be practicable to carry out a much more drastic test than this one. It may occur to some readers to ask why the

[1] See W. J. Russel. *Proceedings* of the Royal Society, Vol. 63, p. 102, 1898; Vol. 64, p. 409, 1899.

plate was placed in immediate juxtaposition to the supposedly luminous layer, instead of using a photographic camera and lens in the ordinary way. Accepted optical principles show that the method actually adopted is far the best for making the most of a faint light.

In order to test the value of the photographic experiment and to compare it with what can be done visually, trials were made with a source which really *is* faintly luminous, the luminosity being fairly distributed through the visual spectrum. This source consists of potassium-uranyl sulphate—a substance the lumineseence of which is permanently maintained under the stimulus of its own slight radioactivity—on the same principle as the luminous compound used for watch dials, though the light is much fainter. To reduce the brightness to near the limit of visibility, the substance was finely powdered. It was tightly packed in a flat metal box of three inches diameter with a glass lid, and not afterwards disturbed.

This source took two hours to produce a definite photographic impression of a black paper stencil, the plate being put right up to it, with the stencil between.[1]

For comparison with the photographic result, the same faint source could be observed visually. The intensity could be provisionally reduced by means of sheets of perforated zinc gauze of varying opacities. Three such were provided, coarse, medium and fine. The scale of intensities (determined by a photoelectric method) was as follows :

Gauze	*Intensity (relative value)*	*Number of Observers in the Class*
None - - - -	100 (assumed)	2
Coarse - - - -	71	8
Medium - - - -	55	22
Fine - - - -	34	8
Fine + Coarse - - -	24	0

At these low intensities the structure of the gauzes could not be discerned. They only produced a uniform darkening of the visual field. The observers were asked to take the source and gauzes with them on retiring for the night ; to observe only after being at least forty minutes in the dark ; and to report what was the least intensity which they could be *quite sure* of seeing.

Since the unscreened source required two hours exposure for a photographic impression, the source screened with the fine gauze, which represents the faintest light that any of my observers can see,

[1] It may occur to some readers to ask whether the radioactivity of the substance might not have vitiated the photographic test. The glass cover was, however, thick enough to eliminate any error from this cause.

should require $2 \times \frac{100}{34}$ hours, or say six hours, to produce one.[1] Actually the exposure given to the magnet was one hundred and fifty days, which is 600 times more.

We find then that the photographic plate as applied is 600 times more effective than even the best visual observers that were met with in this work. The photographic plate cannot detect any luminosity; and it seems quite certain that the claim that any visual observer can do so is unjustified.

The results, given in the last column of the table, do not encourage the idea that there are such people as "sensitives" who can see much fainter light than the ordinary man. The largest class are those who could see intensity 55 on my scale. A few could see intensity 34. No one could see intensity 24.

We do not expect to encounter people of even double the ordinary height. Still less should we expect to find people with hundreds of times the ordinary capacity for seeing faint lights.

There are strong *a priori* objections to a belief in the Reichenbach effect, and also particularly to the claim of some of the observers that they could distinguish between the poles by the colour or extent of the luminosity: for in all well tested magnetic phenomena, the poles differ in their behaviour only in respect of sign. But I wished to avoid judging the question on *a priori* grounds.

The experiments are regarded as definitely disproving the alleged effect. Had there been a luminosity, it could not have failed to come into evidence, even though very much fainter than anything the eye could detect.

In conclusion, we may perhaps ask why the question should have ever been considered to be within the domain of psychical research at all? The earlier workers do not appear to have quite fairly faced the question of whether they supposed the perception to be sensory or extra-sensory, though for the reasons explained above, it seems to me that on their own showing they should have pronounced for the former. This point seems to have been left rather nebulous, and there was thus room for the old ideas about some mysterious *rapport* between the magnet and human personality, which dates from the time of Mesmer, but which has never been satisfactorily proved to have any foundation.

[1] This is on the assumption that for a given photographic density the exposure time is in inverse ratio to the luminous intensity. This law is nearly enough true for the present purpose.

A SERMON IN ST PAUL'S[1]

By Mrs W. H. Salter

Between the years 1903 and 1908 my father, the late Professor A. W. Verrall of Cambridge, published three essays on the Latin poet Statius, whom Dante in the Purgatorio (XXI and XXII) declared to have been a convert to Christianity. These essays originally appeared in different magazines, but in 1913, after my father's death, they were reprinted in a collection of his essays entitled "Literary Essays, Classical and Modern". To two of these essays (*To Follow the Fisherman* and *Dante on the Baptism of Statius*) there were allusions in Mrs Willett's scripts of 1912 and 1913 which were the subject of a paper read by Lord Balfour (then Mr Gerald Balfour) to the Society on March 27, 1914, and afterwards published in Volume XXVII of *Proceedings*, pp. 221-243. It is with my father's third essay, *The Altar of Mercy*, first published in 1907 in the *Oxford and Cambridge Review*, that the present paper is more particularly concerned.

To make the point of the paper clear it will be necessary to say a word about Statius and to quote some passages from *The Altar of Mercy*. Statius, who was born near Naples, then the centre of Greek culture in Italy, lived in the latter half of the first century A.D., and his principal work, *The Thebaid*, an epic in twelve books, describes the war of the Seven Against Thebes. Half-way through the poem the invading army reach the river Asopus, which forms the frontier of Thebes, and finding it swollen, hesitate to cross it: to this passage Dante refers when making Statius describe his own hesitation to receive baptism. In the two essays referred to by Lord Balfour in his paper, my father had suggested that Dante wished to draw a parallel between the delay of the invaders in staying on the other side of the Asopus and the delay of Statius in "staying on the other side" of the river of baptism. Allusions to this point were prominent in Mrs Willett's script.

The invaders are defeated with great slaughter and the victorious Thebans refuse to allow their bodies to be buried. The widows of the slain repair for justice to Athens, where, says Statius (*Thebaid* XII), is an Altar of Mercy (*Clementia*).

[1] This paper was read at a Private Meeting of the Society on September 29, 1937.

" In the midst of the city was an Altar, pertaining not to Might, nor the powers thereof, but to gentle Mercy. Mercy there had fixed her seat and Misery made it holy. Thither new suppliants came ever without fail, and found acceptance all. . . .

" The ritual takes no tax, accepts no incense-flame, no drench of blood, but only the dew of tears upon the stone, and the shorn hair of the mourner for a wreath above, and for drapery the cast robe which sorrow puts away. . . .

" Image there is not any : to no mould of metal is trusted that Form Divine, who loves to dwell in minds and hearts. . . .

" The legend is that it was the children of Hercules who founded the sanctuary, in the city whose warriors protected them when their sire had passed from the pyre to the sky.

" So the tale sayeth, but sayeth not worthily. Rather should we believe that it was those Visitants from Heaven whom Athens had ever made welcome to her soil."

(The above are extracts from my father's translation, *Literary Essays*, p. 227.)

The essay called *The Altar of Mercy* is a commentary on this passage of Statius's poem. Since allusions have been made in scripts which I shall have occasion to mention to parts of the essay, I shall now quote, as briefly as possible, the relevant passages :

" What is solid and evident, what leaps to the eye, is the sentiment of the whole passage, the spirit, the general conception of religion from which it proceeds. For this there is only one suitable word. It is exactly that sentiment to which Christianity appealed. To console the miserable and guilty, to heal the wounds of the world and the sense of sin—these are the offices of that Altar to which Statius directs the worship of Mankind. ' I will have mercy and not sacrifice ' ; ' Come unto Me all ye that labour and are heavy-laden ' : for such inscriptions and such only, the shrine of his imagination is prepared. . . .

" For the venerable legend associated with it, the story of Hercules and his children, he has nothing but scorn. The Altar of his thought was not founded by the children of Hercules, nor in fact by any earthly hand. Like other such emblems of aspiration, it was ' never built at all, and therefore built for ever '. . . .

" And in the thought of the poet it stands alone. We cannot miss nor misunderstand the sweeping depreciation by which, in comparison with this Deity of the Soul, and with the uncostly sacrifice of a broken heart, the whole art and ritual of polytheistic superstition are waved away. . . .

" It is certainly not from the actual leaders of the Christian move-

ment that we shall learn to depreciate the importance of its relation to that species of thought whose ideal centre was the Areopagus of Athens. 'Whom therefore ye ignorantly worship, Him declare I unto you. . . . Ye men of Athens, I perceive that in all things ye are exceedingly God-fearing.' To an exhaustive commentary on these words, and upon the discourse which follows them no small contribution should be furnished by the *Thebaid*."

My father was born on February 5, 1851, and on February 5, 1912, the illness from which he had long been suffering took a decided and noticeable turn for the worse; he died on June 18 of the same year. For a double reason therefore February 5 was a memorable day in his life, and for the remaining years of my mother's life, that is the years 1913 to 1916 inclusive, she made a practice of seeking to obtain automatic writing on each February 5, and also on the 4th and 6th. The scripts so produced form a natural group, being united both by the intentions of the automatist, and by internal links which I could not within the scope of this paper completely set out. I wish, however, to emphasise this matter of internal linkage. The point has been made that evidence of any supernormal content in the scripts is weakened, if not destroyed, by arbitrary selection of a few scripts, regarded as relevant, from a large irrelevant mass. This objection would be valid, if the scripts *were* arbitrarily selected. But this is not the case. The problem as it presents itself to the student of scripts is this: here are a number of scripts which are obviously and normally linked together, e.g. by date, or by the repetition in them of a phrase or topic which does not occur elsewhere in the general mass of scripts. Taking these scripts as a natural group, do we find in them any evidence of supernormal cognisance or intent? This question has been discussed by Mr J. G. Piddington in *Proc. S.P.R.*, Vol. XXXIII, pp. 443ff.

As I am now about to quote from my mother's scripts, I wish to make it perfectly clear that I do not consider the fact that the scripts refer to events in my father's life or to passages in his books as in any way supernormal or even remarkable. I quote them as they are part of a story the significance of which flows from the actions of other persons.[1]

M. V. script of February 4, 1913, opens with these words:

[1] It should, however, be noted that my mother frequently did not herself recognise that her scripts were referring to passages in my father's writings and I believe she never recognised that she had alluded to *The Altar of Mercy*. I believe this tendency of automatists not to see in their own productions a meaning which is subsequently recognised by others is of common occurrence.

> Oranges and lemons—and the rhyme in St Clemens—San Clemente with the mosaics. No wait—you have it wrong Clementia et misericordia sorores [Merey and Pity sisters].

A later script of April 5, 1914, opens with the words:

> Oranges and lemons say the bells of St Clements St Clements Danes

and the name of this church is repeated a few lines later in the same script. My mother associated the name with "Danes Inn" (in the neighbourhood of the church), where my father resided for three years. The script therefore combines three references to my father: in the date, in the implied allusion to Danes Inn, and in the word "Clementia", the name Statius gives to the Deity to whom the Altar of Merey was dedicated. The word "mosaics" recurs in the scripts next to be quoted.

M. V. script of February 6, 1914, begins as follows:

> Pen or feather—penna—the wings of the dove—silver and gold And one of Michael Angelo's prophets holds a great pen—Combine that with the "twain" with which he covered his face. I think I have now made clear what was not understood in a script of yours of long ago, about St Mark's golden-tipped pen which wrote without his conscious aid.

The script of long ago was one written by my mother in October 1905 in which mention was made of an angel giving St Mark a winged pen. In her script of February 6, 1914, there is, it is true, no explicit reference to mosaics, but I shall presently explain why there may well be an implicit one. But before doing so I will quote the last of this series of birthday scripts, my mother's script of February 4, 1915:

> Till I have built Jerusalem in England's green and pleasant land. There is still much building to be done—building without hands—for the City that will be an abiding City the true eternal City crowned with the olive wreath. Fortress and fostress of sons born free [Swinburne's *Erechtheus*] Combine these ideas The Roman Imperial City and the free Athens.

This script, like the twelfth book of Statius's poem presents the picture of an idealised Athens, and in the words "building without hands", taken in conjunction with an earlier script of my mother's, January 21, 1913, which speaks of "an altar not made with hands", there seems to be a clear reference to the passage in which my father says of the Altar of Merey that "Like other such emblems of aspiration it was 'never built at all, and therefore built for ever'".

That there should be references to my father and his writings in

my mother's scripts is, as I have said, nothing extraordinary, but it is, I think, curious, in view of facts shortly to be related, that the "birthday scripts" of 1913 and 1915 both alluded to a particular essay, *The Altar of Mercy*, and that the scripts of the intervening year contain allusions which my mother, as the sequel shows, connected with a mosaic of St Mark in St Paul's Cathedral.

In quoting from a script of 1915 I have departed from the chronological order to which I now return.

On March 24, 1914, my mother, who was staying at Guildford with a friend, came to Woking in order that Lord Balfour might read privately to her the paper already mentioned in which are discussed references in Mrs Willett's scripts to my father's essays *To Follow the Fisherman* and *Dante on the Baptism of Statius*. This paper was to be read at a meeting of the Society at 4 p.m. on March 27, 1914. My mother did not feel inclined to be present at a meeting at which evidence of my father's *post mortem* agency was to be presented and discussed, but she wished to attend the Council Meeting to be held at 6 p.m. on that day. Her friend at Guildford had suggested to her that she ought to go to St Paul's Cathedral to hear the Lenten music, Allegri's *Miserere*, which was being performed there, and it occurred to my mother that this would be an appropriate way to spend the time of the meeting. In her diary for March 29, 1914, she records her visit thus: "On March 27th from 4.15 to 5 I was in St Paul's (for the first time !) to hear Allegri's *Miserere*. There came a gleam of sunlight on the St Mark, and I wondered if he held a golden pen, but it was too far off to see." By "the St Mark" my mother referred to the mosaic of that saint in the series designed by Sir William Richmond, and was also clearly referring back to her scripts of October 1905 and February 6, 1914, already mentioned. *Miserere* is the name given to the fifty-first Psalm, the Vulgate version of which begins *Miserere mei, Deus, secundum magnam misericordiam tuam*. Verses 16 and 17 in the Prayer Book version are as follows: "For thou desirest no sacrifice, else would I give it thee: but thou delightest not in burnt offerings. The sacrifice of God is a troubled spirit: a broken and contrite heart, O God, shalt thou not despise." This is one of the scriptural passages quoted or referred to in the essay on *The Altar of Mercy*: "In comparison with this Deity of the Soul and with the uncostly sacrifice of a broken heart, the whole art and ritual of polytheistic superstition are waved away."

Lord Balfour's paper, which it is to be observed, neither deals with nor makes mention of *The Altar of Mercy*, but only of my father's other two essays on Statius, was published in July 1914 in

Part 69 of *Proceedings*. This Part and Part 70 contain several other papers discussing it.

The paper was read at the time of its publication by Mrs Stuart Wilson, who had been for many years an Associate of the Society. When in the *Journal* for February 1915, an appeal was made for Members to offer themselves for experiments in thought-transference Mrs Wilson responded, and she and I undertook a series of experiments which I reported on in a paper published in Vol. XXIX of *Proceedings*, p. 306.

Mrs Wilson, who is an American by birth, was the wife of Brigadier-General Charles Stuart Wilson, C.B., of the Royal Engineers. My first acquaintance with her was through her response to the appeal of February 1915. She never knew either my father or my mother, and she had not, at the date of the experience I am about to relate, read either *The Altar of Mercy* or any other of my father's writings.

Concerning Mrs Wilson's automatism, readers of the paper referred to above will realise that she and I obtained several cross-correspondences of considerable interest: I may mention in particular the connections between her impressions of May 16 and June 4, 1915, and my script of June 4, 1915, on the subject of *Der Tod als Freund* (Death the Friend) and Sintram (*Proc.*, Vol. XXIX, p. 324). Although little has been published since concerning Mrs Wilson, she has continued to record her impressions from time to time and has become an important member of "the S.P.R. group of automatists".

To remind my readers of the nature of Mrs Wilson's automatism, I will quote a passage from my report of the 1915 experiments:

"Mrs Wilson's procedure is as follows: having provided herself with pencil and paper, she lies down in the dark and lapses almost at once into a light stage of hypnosis. In this condition a series of very vivid mental pictures present themselves to her, amounting sometimes to visual hallucinations. Not infrequently she hears a voice, it may be her own voice or the voice of some other unknown person. On some occasions the voice also has been completely externalised as an auditory hallucination.

"Mrs Wilson makes brief notes of her impressions at the conclusion of the experiment; sometimes, after she thinks the experiment is over, she has some further impressions, which are duly noted. Once or twice it has happened that the impressions have taken the form of a vivid dream during ordinary sleep of which Mrs Wilson has noted as much as she could remember on waking. The detailed records which she sent to me were based on her own contemporary notes, and were usually written out on the day following the experiments.

These records have all been preserved, together with the postmarks showing the dates upon which they were despatched."

I should like to emphasise the point that Mrs Wilson's own personal associations were quite apart from mine or my mother's. The objection has sometimes been made that the S.P.R. automatists knew each other too well, and were too much, as it were, "in the same set." This objection does not apply to Mrs Wilson. Up to the time when she and I began to experiment, her closest links, as I have said, were with America and with the British Army, and for some years before the outbreak of war in 1914 she had been living abroad. Mrs Wilson and I could not be said to know each other well until after the incidents and scripts discussed in this paper.

On February 15, 1916, Mrs Wilson wrote to me as follows: "I have been meaning to write about the following incident ever since it occurred, but my husband has been home on leave, and I have had no time. On Sunday morning February 6th I woke up feeling that I must go to the morning service at St Paul's Cathedral. I have never been to a service there before, or wanted to go. As I lay meditating the matter, I got a visual impression of what I took for bus 13, which I often take to Regent's Park. I wondered why, as it ran in quite a wrong direction for St Pauls. It was only when really started for church, that I discovered that the bus I wanted was 15, and only when I saw it coming, that I realised that with my astigmatism 13 and 15 looked identical at a short distance. Of course I expected to find a long-lost friend, or a German spy, or some one really interesting, sitting near me, either in the bus or in church, but there was not a soul I knew, or any one of the slightest interest. I was in a very absent-minded mood, and the service seemed to go very fast. I heard the first words of the sermon and no more, until I heard the preacher say, "Be careful that like Statius you do not linger too long upon the brink." That attracted my attention, because, except in the S.P.R. Proceedings last spring, I had never consciously heard of Statius. Certainly never apart from the S.P.R. literature. Oh, I said to myself, I suppose that is what I came for. Afterwards it seemed a little puzzling to decide whether the words had really been said by the clergyman or I had evolved them from my own mind, and that is a question, my dear Mrs Salter, that only the man who preached can settle, and I for one should not like to ask him. He cannot, I think, have mentioned the name [i.e. Statius] before, as it would probably have brought me to attention at once.

"Last night [February 14, 1916] I was getting a lot of impressions,

but I can only remember a very wide and turbulent river, and a man sitting near it who looked like St John the Divine in his youth. There was also some one of the same sort on the other side, but old and with a grey beard, I think."

My contemporary record of my reply acknowledging this letter is as follows: "I told Mrs Wilson that her experience was appropriately timed, Feb. 5 being my father's birthday." The occasion was more appropriate than I then realised, as I had not seen my mother's "birthday scripts" of February 4, 5, 6, 1913, 1914, 1915.

On February 18, 1916, Mrs Wilson wrote to me as follows:

"I have looked up the name of the preacher at St Paul's on Sunday, February the 6th, at the morning service, and find it was Dr Nairne. Even if he did preach of Statius [i.e. if the words Mrs Wilson thought she heard were actually spoken and not a subjective impression], it looks like some sort of telepathy, that I should have felt impelled to go and hear him."

The Dr Nairne in question was the Rev. Dr Alexander Nairne, a Cambridge Classical Scholar, and later Regius Professor of Divinity at Cambridge: he was personally known to my father and mother. On February 23, 1916, I wrote to him as follows:

"A friend of mine, a lady with whom I have lately been doing some experiments in thought-transference, attended morning service in St Paul's on February 6 and heard you preach there. She thinks that in your sermon you made allusion to Statius, whom she associates in her mind with an essay on Dante by my father, the late Professor Verrall. But her recollection on this point is not perfectly clear, and I should like to ascertain definitely whether she is right in attributing the allusion to you. Could you therefore tell me whether in your sermon on February 6 you referred to Statius and spoke of him as 'lingering on the brink'?"

Dr Nairne replied on February 24, 1916:

"In the sermon (Feb. 6) of which you wrote, I alluded to Statius and any one who had read Dr Verrall's essay would have known that I had it in mind. But I am as sure as I can be without a written record of the sermon that I did not speak of Statius 'lingering on the brink'. That would—as far as I can remember, and I have little doubt upon this—have had no point in the sermon. I just spoke of Dante thinking him Christian but it was the 'altar of pity' not Statius himself I was concerned with. . . .

"I am yours very truly,

"ALEX. NAIRNE.

"After writing this I feel uneasy about the strength of the denial.

If the phrase 'lingering too long upon the brink' is in the essay I feel that it is possible that I may have used it unconsciously. That happens so often. I will ask a friend who was present if he remembers."

On February 28, 1916, Dr Nairne wrote me the following postcard:

"'Lingering too long upon the brink.' The friend I spoke of is almost certain I did not use those words."

Dr Nairne speaks of "Dr Verrall's essay", but as I have already pointed out, there are three essays on Statius. Dr Nairne was "concerned" in his sermon with one essay, *The Altar of Mercy*: Mrs Wilson knew nothing of this essay, but erroneously thought that she heard a mention of the main subject of the other two essays, of which she had indirect knowledge through Lord Balfour's paper, though she had never seen them herself.

To sum up: at the time of Mrs Wilson's first attendance at a service at St Paul's there already existed, quite unknown to her, the following connections:

(1) Between the date of her attendance, February 6, and the essay *The Altar of Mercy*, through the passages already noted in my mother's scripts of February 4-6 in the years following my father's death.

(2) Between that date and St Paul's Cathedral, owing to references in the same scripts which my mother attributed to the mosaic seen by her in St Paul's on March 27, 1914, the date of her first visit there.

(3) Between St Paul's Cathedral and my father's other essays on Statius, owing to my mother's visit there while Lord Balfour's paper was being read to the Society.

Mrs Wilson's experience of February 6, 1916, seems to me to draw together into one these pre-existing connections: for the first time, the date, the place and this particular essay, *The Altar of Mercy*, occur together. Sermons in St Paul's on Statius are, I fancy, rare, and sermons inspired by my father's essay on Statius's Altar of Mercy perhaps rarer still: ought I to apologise for not being prepared with statistics of their frequency?

The coincidence is the more noteworthy when we bear in mind that Mrs Wilson's visit to St Paul's was the first time she had ever attended a service there and my mother's visit the only time she was ever there at all.

I leave it to members of this Society to judge whether the impulse which induced Mrs Wilson to go to St Paul's on February 6, 1916, and so to hear a sermon on The Altar of Mercy was wholly fortuitous.

Chronological Table

February 5, 1851. A. W. V. born.

February 5, 6, 1912. Serious illness of A. W. V.

June 18, 1912. A. W. V. died.

February 4, 1913. M. V. script: "San Clemente with the mosaics."

February 6, 1914. M. V. script: "St Mark's golden-tipped pen" (cf. mosaics in St Paul's Cathedral).

March 24, 1914. M. de G. V. heard Lord Balfour's paper concerning *To Follow the Fisherman* and *Dante on the Baptism of Statius* privately read at Woking.

March 27, 1914. M. de G. V. went to St Paul's Cathedral for the first and only time, heard Allegri's *Miserere*, and observed the figure of St Mark in the mosaics. At the same time Lord Balfour read his paper at an S.P.R. meeting.

February 4, 1915. M. V. script: "building without hands" (cf. A. W. V.'s essay *The Altar of Mercy*).

February 6, 1916. Mrs Wilson went to St Paul's Cathedral, the first time she had ever attended a service there, and heard Dr Nairne preach on Statius's Altar of Mercy.

II

THE LIGHTHOUSE

I now propose to leave the topic of Statius altogether, and to quote from my mother's scripts and my own some instances of the manner in which the scripts approach a topic, using any associations of ideas which happen to lie ready in the mind of the automatist, who often does not realise the goal aimed at. The element of cross-correspondence is not, I think, lacking, but my purpose is not to stress this but to give illustrations of a process frequently employed in the scripts.

This being my intention, I shall follow the example set by some writers of detective stories and state at the outset the answer to the problem, and then go back to the beginning and show the steps in its solution. The answer in this case is that the scripts, taken as a body, show clear evidence of intention not only to refer to the topic "lighthouse", but to lay strong emphasis on it. But the topic was wrapped up in such a way that during the period to which I am referring, that is to say until June 1915, neither I nor my mother realised this intention.

In a script of September 20, 1908, I wrote some automatic verse (which I do not commend for its literary merit !) as follows :

> By the ocean shore where the billows roar
> When the West Wind wields his flail,
> And seagirt rock scarce bides the shock.
> Woe for the vessels frail.
> A broken spar by the harbour bar
> Lies tossing in the foam
> The wave-tost bell rings out the knell
> Of those who come not home.

I will next quote from two scripts written on consecutive days in September 1911. On September 11, I wrote :

> The midnight sun and northern lights . . . the light leaped like a living thing . . . and great whales go sailing by round the world for ever and aye.

and on September 12 :

> Mutantur omnia irrevocabile tempus (all things change time which cannot be recalled) . . . the evening light and drowsy tinklings lull the distant folds a stormy haven Three fishers went sailing.

On June 18, 1915, I was staying with my mother at Cambridge. In the evening we both wrote scripts, she writing before me. I did not, however, see her script till a month later. I will quote my script first :

> Aequora ponti (the waters of the sea)—the great deep—the deep calls—a land of many harbours—three fishers went sailing—one clear call.

That completes the tale of my own scripts that I propose to quote in connection with this approach to the lighthouse topic. I wish to emphasise that they form a natural group, being united by internal links which I shall shortly explain. But first I will quote my mother's script of June 18, 1915, written, I repeat, before my script of the same date :

> Bells ringing in the darkness of the night ringing till the grey dawn rises Mary, go and call the cattle home—and the bells of Enderby The Bell Buoy Rock No I can't get it clearer—leave it tonight.

It will be noted that the word "lighthouse" does not occur in any of the scripts quoted, nor, I think, would the idea "lighthouse" readily suggest itself to a reader, but if the literary allusions implicit in the scripts are traced and combined, the lighthouse topic emerges clearly enough.

The most obvious link between my scripts is Kingsley's poem *The Three Fishers*. The opening words of this poem are quoted in my scripts of September 12, 1911, and June 18, 1915, and my script of September 20, 1908, contains phrases reminiscent of the same poem.

The poem consists of three stanzas, of which the first opens thus :

> Three fishers went sailing out into the West,
> Out into the West as the sun went down.

The second stanza opens :

> Three women sat up in the lighthouse tower

The third :

> Three corpses lay out on the shining sands

and each stanza ends with mention of the " harbour bar and its moaning ".

References to this poem may, in view of the second stanza, be lighthouse references, but they are not necessarily so ; and in fact the one topic which appears on the surface in all of my scripts except the second, and still more emphatically in my mother's scripts, is that of bells. My first script has " wave-tost bell "; my third leaves for a moment the sea association and gives sheep bells at evening, " drowsy tinklings lull the distant folds," and then returning to the sea " the stormy haven—three fishers went sailing ". The fourth script introduces by the words " one clear call " a reference to Tennyson's *Crossing the Bar*, the third stanza of which begins " Twilight and evening bell ",[1] and my mother's script is all about bells, bells ringing in the darkness of night, the bells of Enderby, the Bell Buoy Rock.

Combined with these in my mother's script is a quotation from another well-known poem of Kingsley's *The Sands of Dee*. Like his *Three Fishers*, it is a tale of drowning off the coast, but in this case it is a girl who is drowned. It thus forms a convenient transition between *The Three Fishers* and *The Bells of Enderby*, mentioned in Jean Ingelow's poem *High Tide on the Coast of Lincolnshire*, in which also a girl is drowned in the rising tide. But whereas *The Three Fishers* has a lighthouse but nothing about bells, Jean Ingelow's poem gives bells without the lighthouse, although there is in it a reference to a church-tower which acts as a " beacon-light to sailors ". The lighthouse association is therefore still not unmistakably established.

[1] The first stanza has " And may there be no moaning at the bar ", thereby linking up with *The Three Fishers*.

There remain to be considered the second script of my series, that of September 11, 1911, and two phrases, one in my script of September, 12, 1911, "irrevocabile tempus," and one in my mother's script of June 18, 1915, "The Bell Buoy Rock." These two phrases have a close association of a very curious kind, and that association is "lighthouse".

"The Bell Buoy Rock" was at the time identified by my mother with the Bell Rock or Inchcape Rock off the east coast of Scotland. Between 1807 and 1811 a lighthouse was built there by Robert Stevenson: before that there had been a bell attached to a framework which was moved by the waves (hence the name of the rock), and when the lighthouse was built, the machinery in it was arranged to ring two bells in foggy weather. Note the appropriate phrases "sea-girt rock" and "wave-tost bell" in my first script, which does not anywhere imply that the more common arrangement of a bell buoy is being described.

In the summer of 1814 the Commissioners of Northern Lights, the body responsible for all the lighthouses in Scotland, made an official tour of inspection of lighthouses in the northern part of Scotland, including the Orkneys and Shetlands. One of the unofficial members of the party was Sir Walter Scott, who kept a diary of the tour (printed in full in Lockhart's *Life of Scott*, Vol. III, pp. 136-277). Scott jestingly heads the diary "Voyage in the Lighthouse Yacht to Nova Zembla and the Lord knows where". The party left Leith on June 29 and on the evening of the first day Scott "watched the progress of the ship round Fifeness, and the revolving motion of the now distant Bell Rock light until the wind grew rough and the landsmen sick". Early next day the party were put ashore and went over the lighthouse, which is described at some length. My mother, the day after she wrote her script of June 18, 1915, noted that she had read Lockhart's *Life of Scott* and knew that Scott had visited the Bell Rock Lighthouse.

On September 8, 1814, the tour ended at Greenock, where Scott went ashore, and he closed his diary with words expressing the pleasure he had had, and the necessity of returning to work: "we had constant exertion, a succession of wild and uncommon scenery, good humour on board, and objects of animation and interest when we went ashore—

Sed fugit interea, fugit *irrevocabile tempus*."

There you have the connection between "Bell Buoy Rock" in my mother's script and "irrevocabile tempus" in my script of September 11, 1911. The one refers to an incident at the very beginning of

Scott's tour, the other to the last words of the diary describing the tour, the whole object of which was to inspect lighthouses. Scott's Latin tag, which is, of course, a quotation, is the 284th line of Vergil's Third Georgic: or rather it is a misquotation, for the texts of Vergil have "*irreparabile* tempus", while Scott has "*irrevocabile*".[1] That my script perpetuates Scott's error shows that it was referring to his quotation and not giving a direct quotation of a line of Vergil with which in my normal state I am familiar. It also shows that the scripts, which are sometimes charged with nebulous vagueness, can on occasions be as precise, even in small details, as any one could desire.

Several phrases in my scripts are appropriate to the diary, though not indubitable references to it. Such for example are "a stormy haven" in my script of September 12, 1911, and "a land of many harbours" in my script of June 18, 1915; possibly also the "great whales" of my script September 11, 1911, as Scott makes several references to the whaling industry. But of special interest are the words in this script "The Midnight sun and northern lights". Scott did not go far enough north to see the midnight sun: in a poem written during the tour he speaks of islands

> "Where late the sun scarce vanished from the sight
> And his bright pathway graced the short-lived night".

But if he had gone to Nova Zembla in fact, not only in jest, he would of course have reached the latitude of the midnight sun.

More certain is the phrase "Northern Lights", which has a double meaning and is, I think, used in the scripts precisely for its ambiguity. It may mean the Aurora Borealis, a subject naturally mentioned in association with the midnight sun. But "Commissioners of Northern Lights" is the name Scott gives the Board on whose yacht he made his tour. If my interpretation is right, an allusion to lighthouses is thus surreptitiously introduced in a phrase apparently referring to celestial phenomena.

I am certain that at the date of these two scripts of September 1911, I had never read Scott's diary of 1814, and that I did not know that he had there quoted, or misquoted, the Vergilian line. My mother, who saw both scripts on September 12, 1911, very possibly remembered Scott's misquotation, as at some date that I do not know she read the diary. But her knowledge of my scripts of

[1] The misquotation ("fugit irrevocabile tempus") reappears in *The Heart of Midlothian* (first published 1818) in Chapter XII, where the words are put into the mouth of Butler, then in prison. But whereas the context of the scripts is entirely appropriate to the diary, it is quite inappropriate to the situation in the novel in which the misquotation occurs: this sufficiently appears from the scripts I have quoted.

September 11 and 12, 1911, would not account for the fact that on June 18, 1915, she wrote a script linked with them in the manner already explained, whilst on the same evening, I, writing quite independently, produced a script also linked in the clearest possible way with the same scripts. However, as I said earlier, my object is not to stress elements of cross-correspondence, but to illustrate a method which would lose little interest if it had no relation to cross-correspondences.

In the other instance which I shall now relate, Members of the Society may be relieved to learn that I shall steer clear of the Classics altogether and not bring in even a Vergilian tag. Two nursery rhymes and a short story and some verses by one of the best-known writers of our time are practically the whole literature involved. With one exception all the scripts were written in the first few months of 1914. "Lighthouse" is again the hidden goal.

The one script not written in this period is my script of December 15, 1908, which ends: "A carving knife—three blind mice—a single thread." For five years the mice topic was dormant in my scripts, not emerging till January 1914. In a script of January 21, 1914, occur the words, "A golden boat a poem something about the moon and a golden boat," with a drawing of a crescent moon.

On January 28, 1914, my script had the words:

> The golden boat—something about the masts being of silver—it is an English poem—we have tried to quote it twice before—to sail upon the sea—last night the moon had a silver rim three blind mice—yes, write it down—I wish you could find that poem.

On February 4, 1914, come the words:

> The masts were all of silver—the crew were mice.

These scripts made it perfectly clear (though I did not myself appreciate this at the time) that the poem referred to the nursery rhyme called *The Fairy Ship*, of which several slightly variant versions exist. I shall give that which was familiar to me as a child:

A ship, a ship a-sailing
A-sailing on the sea;
And it was deeply laden
With pretty things for me.
There were raisins in the cabin
And almonds in the hold;
The sails were made of satin
And the mast it was of gold.

The four and twenty sailors
That stood between the decks
Were four and twenty white mice
With rings about their necks ;
The Captain was a duck, a duck
With a jacket on his back,
And when the fairy ship set sail
The Captain he said, Quack !

The trail had been confused by reminiscences of another poem well-known to me since childhood, a poem called *The Fairy Boat*, by T. Hood. It begins :

A lake and a fairy boat
To sail in the moonlight clear,

and accounts for allusions in the scripts to the moon, which plays no part in the nursery rhyme. The " three blind mice " are, I suppose, introduced to bring in the topic of mice in a nursery rhyme setting, and to indicate by a reference back to my script of December 15, 1908, that this was a previous, but unsuccessful attempt to introduce the topic of *The Fairy Ship*.

Although Lord Balfour and Mr Piddington, who had been studying my scripts, identified the " poem " as *The Fairy Ship*, they were at a loss to understand why so much trouble was being taken to drag it in. It happened that about a week after my script of February 4, 1914, my mother, who did not know of the references in my scripts to *The Fairy Ship*, was staying at Fishers Hill (Lord Balfour's house). Dame Edith Lyttelton was also there and was to do some experiments in automatic writing with my mother. At Lord Balfour's suggestion Dame Edith on February 12, 1914, put the following question out loud to my mother while she was waiting for automatic script to come, in the hope that the answer might throw some light on why the nursery rhyme had been dragged in :

" Have you anything to say about these lines

And when the ship began to move
The Captain cried Quack Quack ? "

After this question had been put my mother's script began :

> The Old Moulmein Pagoda—Eastern scents and eastern sounds—Bells upon the breeze.

The relevance of this answer is not immediately obvious, nor was it discovered until many years later Mr Piddington happened to read Kipling's short story *The Disturber of the Traffic*. But its relevance

is apparent when we consider the following points: the reference in my mother's script was to a poem by Kipling called *Mandalay*, which speaks of " the old Moulmein Pagoda, looking eastward to the sea " and emphasises the topic of bells,[1] which in the scripts I have quoted above was closely interwoven with the lighthouse topic. This was evidently an approach (probably the best the automatist could do) to the real answer to the question, *The Disturber of Traffic*. Kipling's story is a story of the eastern seas told in a *lighthouse* on the English coast by the *lighthouse* keeper about a man who tended a *lighthouse* in the Dutch East Indies. He goes mad and has to be taken away in a survey ship's boat: " When Dowse [the madman] was in the boat, he found, so he says, he couldn't speak to the sailors 'cept to call them ' white mice with chains about their neck ' and Lord knows he hadn't seen or thought of white mice since he was a little bit of a boy with them in his handkerchief. . . . Then he heard the Captain talking to him very kind . . . but all he could do was to keep his eyes on the rigging and sing—

I saw a ship a-sailing
A-sailing on the sea
And oh, it was all lading
With pretty things for me

Then he remembered that was foolishness, and he started off the saying about the Ombay Passage but all he said was—The Captain was a duck—meaning no offence to you, Sir—but there was something on his back that I've forgotten.

And when the ship began to move
The Captain says, Quack, quack ! "

It might be interesting to enquire how many people with a good knowledge of modern English literature would be able to answer the question, " Where is a story about a lighthouse combined with a quotation from the Nursery Rhyme about a Fairy Ship whose crew were mice ? " I put this question to my audience at the Society's meeting at which this paper was read, but I got no answer.

In conclusion I may say something about the first *explicit* reference to a lighthouse in my script, which was not made until April 27, 1918. It was made in this form:

" Dominus illuminatio mea—φαρος "

[1] " For the wind is in the palm-trees, and the temple bells they say. . . . "
" An' the sunshine and the palm-trees an' the tinkly temple bells; . . . "
" For the temple bells are callin, an' it's there that I would be—
" By the old Moulmein Pagoda, looking lazy at the sea."

φαρος means the famous lighthouse at Alexandria. " Dominus illuminatio mea " is a reference back to a script of mine written exactly ten years earlier, on April 27, 1908. This script has " Dominus illuminatio mea A golden harp ".

" A golden harp " is mentioned once again, and once only, in all my scripts, and that is in my script of March 24, 1914. Mr Piddington was present while this script was being written, and in the course of it this question was asked him :

" Is it clear about the silver sails, etc ? "

I now quote the record of what followed this question :

J. G. P. I can't say it is clear. I know the poem that is meant [i.e. *The Fairy Ship*].
we will try to say more later—

J. G. P. Right. I want to give a message. Can I give it now? Wait a minute—
a golden harp—we wanted to say that—Now the message

Observe how permission to give the message is not granted until the words " a golden harp " have been added, the object of the addition being to connect *The Fairy Ship* with " Dominus illuminatio mea A golden harp " in my script of April 27, 1908.

When " Dominus illuminatio mea " is repeated on April 27, 1918, it is immediately followed by a reference to the Pharos at Alexandria. This combination of " Dominus illuminatio mea " with the Pharos may be taken as the fulfilment of the intention expressed in my script of March 24, 1914 : " we will try to say more later."

These facts, besides furnishing another example of how topics emerge in scripts, go to show that long before it was realised that references to *The Fairy Ship* mean the lighthouses mentioned in Kipling's story, the intelligence responsible for my scripts was aware of a close connection between *The Fairy Ship* and the lighthouse topic.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 154

REPORT ON A SERIES OF EXPERIMENTS WITH MRS EILEEN GARRETT

By K. M. Goldney and S. G. Soal

PART I

AN EXAMINATION INTO PHYSIOLOGICAL CHANGES ALLEGED TO TAKE PLACE DURING THE TRANCE STATE

By K. M. Goldney

I. Introduction

Before Mr Soal and I give our report of the experiments carried out by us with Mrs Garrett in London during 1937, I will shortly mention a few facts concerning her mediumship.

Mrs Eileen Garrett is one of the best known of our English mental mediums, and has come to occupy a unique position inasmuch as she has placed herself of late years in the hands of scientific investigators who have been at liberty to carry out experiments in whatever way they wished, without restriction.

Mrs Garrett tells me that she first became aware of psychic powers as a child, and that she started upon her career as a professional medium at the age of 30. She has worked as a medium on the staff of the London Spiritualist Alliance and of the British College of Psychic Science. She has worked for Mr Harry Price at the National Laboratory of Psychical Research, for Dr William Brown (a member of the S.P.R. Council), for the International Institute for Psychical Research, and for this Society.

I might, perhaps, particularly mention the interest aroused some years ago as a result of Mrs Garrett's tranee utteranee eoneerning the loss of the airship R101. Its destruetion was *predicted*, she tells me, in tranee eommunieations purporting to eome from the well-known airman Hinehliffe, who lost his life in an attempted east-to-west erossing of the Atlantie. These eommunieations have been deseribed in a book entitled *The Return of Hinehliffe*, published by the Psyehie Press. Forty-eight hours after the news was first published of the *loss* of the R101 in 1930, its eommander, Lieut. Irwin, purported to eommunieate through Mrs Garrett at a sitting being held by Mr Harry Priee. This has been deseribed by Mr Priee in his book *Leaves from a Psychist's Case-book* (London, 1933). The deseription given of the disaster seemed to be beyond the knowledge of the man in the street. Mr Priee therefore got into toueh with the airship base at Cardington, Bedfordshire ; and I ean myself testify that an offieial from Cardington, who subsequently visited Mr Priee at the National Laboratory of Psychical Researeh, was definitely of opinion that the tranee utterance eontained information not only beyond the knowledge of the man in the street, but known only to a small group at Cardington, of whieh the purporting communieator was one. So far as this individual testimony is eoneerned, therefore, a *prima facie* ease for paranormal knowledge appears to have been made out, whieh might repay further investigation.

But Mrs Garrett's aetivities have not been eonfined to this side of the Atlantie. She is now on her fourth visit to the United States, and during these visits she has worked for the Ameriean Soeiety for Psyehieal Researeh ; the Boston Soeiety for Psyehie Researeh ; with Dr Rhine and Mr J. G. Pratt at Duke University ; with Mr Hereward Carrington ; and with various Ameriean doetors.

When Mrs Garrett returned to England in 1937, Mr Soal and I determined to ask her to repeat the various experiments earried out in Ameriea about whieh so mueh has been heard over here during the last couple of years.

It should be noted that these experiments fall into two distinet eategories : those about whieh a Report has been published, and those of whieh we have heard only verbally and by rumour.

In the former category come the experiments in Extra-Sensory-Pereeption earried out at Duke University and reported upon by Dr Rhine, and two series of Proxy Sittings reported upon by Mr J. G. Pratt.

In the latter eategory—experiments about whieh no Report has been published but about whieh much has been heard verbally—

come the experiments undertaken in doctors' laboratories in America, relating to physiological changes alleged to take place during tranee.

Mr Soal repeated the experiments in E.S.P., and I carried out a short series of Proxy Sittings and a triple repetition of the doctors' physiological tests.

Before we report on these in detail, we wish to place on record our very great appreciation—an appreciation which must be shared by all students of the subject—of the attitude maintained by Mrs Garrett. She has throughout given us her services entirely voluntarily, and has grudged neither time nor work. Moreover, she faces with courage the possibility that these searching tests may prove disappointing to the hopes and possibilities of mediumship, but never fails to appreciate that it is by such exhaustive tests alone that we can hope to progress in our knowledge of the subject.

We had hoped to give a detailed Report on *all* the work carried out, but time does not permit of this and I am unable on this occasion to report on the Proxy Sittings experiment. I will only mention here that 8 Proxy Sittings were held, with specially selected sitters; that all 8 scripts, bearing no name or distinguishing mark, were sent to each of the sitters; and that only one sitter succeeded in selecting what was, in fact, his own script. In doing so he commented that he "would have classified the result as 'Poor' had it been a normal (sitter-present) sitting" and that "at best there were only streaks of relatively good hits". The outcome cannot, therefore, be considered successful; but it had interesting repercussions which I shall hope to report upon on another occasion.

The experiments in E.S.P. and the result of the enquiry into alleged paranormal physiological conditions during tranee, were ENTIRELY NEGATIVE; and I suggest that the onus now lies on the American investigators who have claimed positive paranormal results to show why such results are obtained in one hemisphere and not in another.

With this as a foreword, I will now proceed to give my Report on the doctors' examination into physiological conditions obtaining during the trance state. These experiments were carried out under the auspices of the International Institute for Psychical Research, and I am indebted to the Council of that Society for permission to report here on the result; but I was myself entirely responsible for arranging the experiments, and was present myself throughout to take down shorthand notes.

2. Report on the Experiments

Date: June and September 1937.

Medium: Mrs Eileen Garrett.

Doctors participating in the experiments:

(1) Geoffrey Bourne, Esq., M.D., F.R.C.P.; Asst. Physician, Officer in charge of Cardiographic Dept., St. Bartholomew's Hospital, London; Physician, King George Hospital, Ilford; Hon. Consulting Physician, Letchworth Hospital; Examiner, Pharm. and Therapeutics, University of Cambridge, and in Med. Soc. Apoth.; Fellow of the Royal Society of Medicine, etc., etc.
Address: 47 Queen Anne Street, London, W. 1.

(2) Cuthbert Dukes, Esq., O.B.E., M.Sc., M.D., D.P.H.; Pathologist, St. Peter's Hospital, London; Director, Research Laboratory, St. Mark's Hospital, London; Lecturer, Bac., King's College for Women; Fellow of the Royal Society of Medicine, etc., etc.
Address: 1 Queen Anne Street, London, W. 1.

(3) William Nunan, Esq., M.D. (Dublin), M.B., B.Ch.; B.A.O., L.M. (Rotunda); Fellow of the Royal Society of Medicine; Late Police Surgeon, Bombay; Prof. Med. Jurisprudence and Toxicology, Grant Medical College, Bombay; Psychotherapist on the staff of the Institute of Medical Psychology, London.
Address: 3 Upper Wimpole Street, London, W. 1.

(4) V. J. Woolley, Esq., M.D. (Camb.); Lecturer Pharmacol., St. Thomas's Hospital, London; Mem. Physiol. Soc.; Former Hon. Research Officer, Society for Psychical Research, London.
Address: St. Thomas's Hospital, London, S.E.

(5) Dr Helena Wright, M.R.C.S., L.R.C.P., M.B., B.S.
Address: 9 Weymouth Street, London, W. 1.

The medium, Mrs Eileen Garrett, gave her services entirely voluntarily.

The American figures quoted in this Report and the originals of documents, correspondence, notes, etc., etc., are filed with the International Institute for Psychical Research, Walton House, Walton Street, London, S.W. 1, and can be studied there with the permission of the Director.

NOTE :

Mrs Garrett's Trance " Controls "

During trance, Mrs Garrett is controlled by two personalities calling themselves

(1) UVANI,
(2) ABDUL LATIF.

So far as I am aware, this is the first time that a Report has been issued concerning the experiments that have been made during the last two or three years with Mrs Eileen Garrett, with regard to physiological conditions during the trance state.

The subject, however, has not lacked publicity for lack of authoritative data. It would be difficult to exaggerate the wild and—as it has proved—unwarranted statements that have appeared in the " Psychic Press " in this country. " In America ", we are given to understand, it has been " scientifically proved " in doctors' laboratories that mediumistic Controls are entirely separate entities from the medium through whom they operate. I will quote a few extracts from Journals devoted to this subject.

The Editorial Notes of the July 1937 issue of *Psychic Science* (the quarterly transactions of the British College of Psychic Science) speak of " the researches, psychological and clinical, made with those Controls in the U.S.A. laboratories, when to all tests their reactions were very different from those of the normal Eileen Garrett. These secondary personalities are able apparently to produce such a powerful dramatic effort as can completely change the metabolism of the bodily functions."

At the Twelfth Annual Dinner of the British Society of Psychic Science, held at the Café Royal on May 5th, 1937, the Chairman again told listeners that " it was found that the metabolism of the body changed completely under particular ' Controls ' ; that was something that had never been demonstrated practically in the history of the world before. Those of us who are psychic students know it, but we have not been able to put it down in black and white as these doctors could."

The July 30th, 1936, issue of *Light* speaks of the experiments done in America as follows : " Not once did these exhaustive experiments prove anything but that each of these three personalities yielded entirely divergent reactions to every ' test ' applied." " For six months this year this intrepid medium has allowed herself for an average of six hours a day to be put . . . through a long series of medical tests." " Proofs of the separate identity of these three

personalities came forth so clearly and undeniably one after the other, that at one point Dr Traeger of New York, who was conducting the experiments, exclaimed, at the realisation of the implications involved, ' I hardly DARE to go further '."

" *Cardiographs* obtained in the three states showed clearly the somewhat delicate heart action of Eileen Garrett, the vigorous beat of Uvani, and the unmistakably feeble action of the aged Abdul."

" The *blood counts* varied correspondingly : the very constituents of the blood were not the same : where Mrs Garrett showed only about 70 components of haemoglobin, Uvani registered approximately 85, and Abdul as high as 110 ; their *blood chemistry* was completely different."

" Similarly with bleeding-time, coagulation times, respiration : none of them were left untried, and not one of them failed to register difference of personality."

In May 1937 Mrs Garrett was invited by the London Spiritualist Alliance to address the members of that Society. The following is an extract from *Light* of May 20th, 1937, on what she is reported to have said as to the American experiments : " We worked for nine months and found enormous and alarming changes. The *cardiographs* were entirely different, the activity of the heart under the Controls changed : one Control has no relationship to the other, and my cardiograph would have no more relation to their recordings than mine would, perhaps, to Dr Dingwall's." " The *blood chemistry* of the alleged Control, Abdul Latif, contained the blood of a man in the last stages of diabetes ; the *blood corpuscle* of the Uvani personality was very much lower than my own. These were quite alarming."

Now I am not necessarily suggesting that Mrs Garrett gave her listeners incorrect information ; but I do suggest that it is very easy for a layman dealing with a technical subject imperfectly understood—or understood not at all—to misinterpret facts, and draw conclusions unwarranted by those facts ; and I do suggest that Societies incur a grave responsibility when they invite other than technical experts to address their members on so technical a matter *without laying very great stress on the warning that judgment should wait upon the publication of a first-hand and authoritative Report* ; and that Journals incur a similar responsibility when they give publicity to stupendous claims arising out of hearsay evidence.

An article I have just been reading in *The Listener* comments upon how easy it is to be impressed by technical jargon we do not understand, and that the very fact that we do not understand it seems to be in its favour. When, therefore, a lay audience is told—quite

correctly—that two successive blood counts taken at short intervals from the medium showed a difference in the red blood cell count of *two hundred thousand*, their amazed eyebrows rise hardly less high than were they to hear of the miracle of the loaves and fishes being repeated in their midst. Yet to the medical expert the increase indicates nothing abnormal, still less para-normal.

All that I had heard concerning the experiments carried out by American doctors made me determine to get, if possible, first-hand information regarding these experiments and, further, to get these tests repeated in London. Mrs Garrett was more than ready, as ever, to co-operate. I aimed at obtaining the help of specialists whose standing would lend to their opinions the highest authority. Through Dr Helena Wright I was fortunate enough to obtain for the blood-count experiments the co-operation of Dr Cuthbert Dukes, O.B.E., M.Sc., M.D., D.P.H., etc.; and for the electro-cardiographic experiments, the co-operation of Dr. Geoffrey Bourne, M.D., F.R.C.P., etc.

The experiments were carried out in the consulting rooms of these doctors with Dr Helena Wright assisting. Dr Nandor Fodor, the Director of the International Institute for Psychical Research, was also present at the experiments. I was present to take down short-hand notes dictated to me by the doctors, and my notes and transcriptions were sent to them for their approval.

In no instance was there anything to suggest para-normality in the results of any of the tests made; in no instance was there any corroboration of the claims I have quoted concerning physiological changes during the trance state.

In addition, with the exception of the electro-cardiographic tracings, these experiments were repeated by Dr William Nunan, M.D., and Dr V. J. Woolley, M.D. (former Hon. Research Officer of the Society for Psychical Research), both members of the Medical Committee of the International Institute for Psychical Research. Their finding agreed with those of Dr Dukes in being entirely negative from the point of view of para-normality.

I have already mentioned that there existed no published Report on previous experiments to which I could refer, and my determination to obtain if possible first-hand information. Mrs Garrett provided me with tables of figures, which she said were the results of the American experiments, undertaken by Dr. Traeger in New York. I made copies which I then sent back to Dr Traeger, asking for corroboration and for advice regarding the conduct of further experiments. Further figures, charts and diagrams were sent me by Dr Traeger, and I am indebted to him for very ready co-opera-

tion. These charts and figures were, however, accompanied by a warning from Dr Traeger that the work already done had been interesting but merely tentative, and that no conclusion was claimed. I was told that copies of the Journals I have quoted had caused perturbation by "their attempt to journalize and even sensationalize the results of these rather tentative experiments" and "by their attempt to lend them an aura of accuracy and finality which they do not possess"; and it is at Dr Traeger's own request that I here clarify his position.

All the American figures were confidentially submitted to the London doctors who undertook to repeat the experiments.

The Tables attached to this Report give in detail the results obtained in the London experiments from the various tests made:

(*a*) Before trance.
(*b*) During trance: Uvani in control.
(*c*) During trance: Abdul Latif in control.
(*d*) After trance.

I will here touch upon these results only in summary, and give the final opinion of our doctors on the results of their own tests, and their comments upon the American figures which I submitted to them.

These opinions should be contrasted with the false conclusions of the Psychic Press already quoted.

Electro-cardiographic tests.

Commenting upon the electro-cardiographic tracings from America submitted to him, Dr Geoffrey Bourne writes as follows:

"On examining this set of tracings, there is a similar uniformity with one exception, that of the tracing described as Trance No. 2, date Oct. 18th, 1935. Here there is a change in the *R-S* complex in Lead 3; but in my opinion this change is within the limits of what might be produced by some overdistension of the stomach, or variation in the position of the diaphragm.[1] Such changes in Lead 3 are common."

Regarding the electro-cardiographic tracings he himself took, Dr Bourne comments as follows:

"I took electro-cardiographic tracings of Mrs Garrett before her two trances, during each of them, and afterwards. The prints are enclosed. I can find no significant change in any of these tracings, which obviously come from the same individual."

[1] NOTE. This latter could be controlled.—K.M.G.

Basal Metabolic Charts.

Commenting upon the basal metabolic charts from America submitted to him, Dr V. J. Woolley writes me as follows :

" All that these charts show is that when the subject made violent inspiratory gasping efforts she consumed more oxygen than when she was lying quietly at rest. This is not surprising : in fact it would have been surprising if any other result had been obtained. In all Basal Metabolic Rate determinations, the first essential is for the subject to remain quietly at rest throughout. All these charts show is how not to do it."

Blood Chemistry.

Commenting upon the American figures submitted to him, Dr Dukes writes as follows :

" The figures for these tests are such as one would expect if the same tests were carried out on normal people. The blood sugar varies in healthy persons between 100-180 milligrammes, and may change 10 or 20 per cent. in the course of a few minutes. The urea nitrogen also fluctuates throughout the day, so that the variations recorded for this individual are such as would be found under normal circumstances."

NOTE. In view of the fact that nothing paranormal was found when considering the more important claims, other tests in blood chemistry, changes due to injection of drugs, etc., were not considered worth while repeating.

Haemoglobin.

Commenting upon the American figures submitted to him, Dr Dukes writes as follows :

" I regard the variation in the haemoglobin recorded for this lady such as one would expect when repeated tests are carried out at short intervals. There is perhaps an exception in the figures you have underlined on page 4, and I admit that this is difficult to account for. However, since all the other figures are so much within the limits of what might be expected under normal circumstances, I should not draw any conclusions from this isolated observation without knowing more about the conditions under which they took place."

Regarding his own tests, Dr Dukes writes :

" I estimated the haemoglobin by both the Sahli and Haldane methods."

Dr Dukes found nothing unusual in the results of the tests he made.

Red Blood Cells Count.

Commenting upon the American figures submitted to him, Dr Dukes writes :

" It must be admitted in the first place that the degree of accuracy with which blood counts can be carried out does not extend beyond the first 100,000. In fact, unless very special precautions are undertaken, and the person carrying out the test is working under the most perfect conditions, I should think that one might expect variations of $\frac{1}{4}$ million in 2 counts carried out at short intervals. This does not mean that the blood actually changes, but these are the limits of accuracy of the method employed. Unless special care is taken in the graduations of the pipettes and in the manner of collection of the blood, I should say that even wider variations might be expected in normal people. This being so, I find that most of the blood counts are definitely within normal limits. There are two recorded which are slightly high, and then there is this peculiar count on page 4.[1] Apart from this I do not think that these blood counts show any evidence of any change taking place in the blood during trance."

Dr Dukes also drew attention to the fact that in the American figures numbers are given down to too small a figure—one count, for example, is quoted as 4 million and 50 thousand ; another as 3 million and 10 thousand ; whereas, as Dr Dukes pointed out in the quotation I have already given, the degree of accuracy with which blood counts can be carried out does not extend beyond the first 100,000. A quotation of 50,000, still more of 10,000, would therefore be meaningless.

Regarding his own tests for red blood cells, Dr Dukes writes :

" My general conclusion is that there is no variation in the blood count in these different states. Such minor changes as are shown in these recorded figures are within the limits of the accuracy of the methods employed. . . . I should be grateful if you would thank the subject of our experiments for her willing co-operation."

Summarising what he felt on considering the American figures *as a whole*, Dr Dukes writes :

" My general opinion about the whole question is this : I should say that these chemical and microscopical tests are such as one

[1] About which comment had already been made when dealing with the haemoglobin, *q.v.* p. 51.

might expect to find by repeated tests carried out on the same person. No conclusion can be drawn from the tests that any unusual change takes place in the blood during the period of trance. . . . My own feeling is that the matter is not worth looking into any further along these lines."

A word as to the variations shown in the London experiments in :

The pulse rate.
The plantar reflex.
Eye reflexes.

Pulse Rate.

This showed considerable change, which might appear surprising to those unfamiliar with pulse variations. Dr Nunan wrote me :

" A variation of 10-30 could be caused by a cough—a laugh—the IDEA of being examined, etc., etc. I have myself suggested variations from 60 to 120 in a patient ' put to sleep ' by suggestion. Breathing rapidly will increase pulse rate naturally (i.e. for physiological reasons)."

Experiments which I carried out with a couple of friends showed variations in ourselves of 10 in successive counts due merely to difference of posture ; a difference of 15 was effected by puffing twice at a cigarette.

Plantar Reflexes.

There was an absence of plantar response during trance.

Dr Woolley considers that plantar anaesthesia would be common in hysterics or during a state of light auto-hypnosis.

Eye Reflexes.

Perhaps the most spectacular of the trance conditions was the ability shown by the control Abdul to hold the medium's eyes open without blinking for a prolonged period, notwithstanding a torch being flashed into them. At the tests carried out in Dr Bourne's consulting room, this state was maintained without blinking for 7 minutes, and released then only upon instruction by Dr Wright.

Upon this result Dr Nunan comments :

" Very many normal people can stare unblinkingly, even at a bright object, for indefinite periods. Hysteria or hypnosis would produce similar results."

Dr. Bourne summarises his comments on these tests as follows :

" The other sensory changes noted (conjunctival and plantar) are

to my mind similar to what is found in cases of functional sensory change, i.e. functional anaesthesia."

It is disappointing to have to issue a negative report to any investigation. The extensive setting forth of purely negative results in this instance is necessitated by the publicity given by the Psychic Press to unjustified conclusions regarding the trance state, upon which I commented at the beginning of this report. Truly the greatest enemies of matters psychical are its own devotees. It is not, I feel, too strong a comment to make that not only do such exaggerated statements bring the whole subject into disrepute amongst careful and serious enquirers, but that these travellers' tales, so lightly disseminated, are largely responsible for the fraud so extensively practised in our midst to-day which feeds upon public ignorance and readiness to accept as established facts phenomena which should be regarded merely as experimental hypotheses.

APPENDIX 1

Copy of letter from Dr Cuthbert Dukes commenting upon the American figures submitted confidentially to him for his consideration.

No. 1 Queen Anne Street,
Cavendish Square, W. 1,
20*th May*, 1937.

Dear Mrs Goldney,

Many thanks for your letter giving the results of the tests on Mrs Eileen Garrett. I have read these through carefully and may perhaps be able to help you if I tell you what sort of impression they make upon me.

With regard to the blood chemistry (page 5). The figures for these tests are such as one would expect if the same tests were carried out on normal people. The blood sugar varies in healthy persons between 100-180 mg., and may change 10 or 20 per cent. in the course of a few minutes. The urea nitrogen also fluctuates throughout the day, so that the variations recorded for this individual are such as would be found under normal circumstances.

Regarding the haemoglobin figures, I should like to point out that there is always an error of at least 2 or 3 per cent. in estimating the haemoglobin even when carried out with the greatest care, using the same pipettes and standards. Unless these precautions are taken, the error might be even greater. I regard the variation in the haemoglobin recorded for this lady as such as one would expect when repeated tests are carried out at short intervals. There

is perhaps an exception in the figures you have underlined on page 4, and I admit that this is difficult to account for. However, since all the other figures are so much within the limits of what might be expected under normal circumstances, I should not draw any conclusions from this isolated observation without knowing more about the conditions under which they took place.

I feel much the same about the figures for the Red Blood Cells. It must be admitted in the first place that the degree of accuracy with which blood counts can be carried out do not extend beyond the first 100,000. In fact, unless very special precautions are taken, and the person carrying out the test is working under the most perfect conditions, I should think that one might expect variations of $\frac{1}{4}$ million in 2 counts carried out at short intervals. This does not mean that the blood actually changes, but these are the limits of accuracy of the method employed. Unless special care is taken in the graduations of the pipettes and in the manner of collection of the blood, I should say that even wider variations might be expected in normal people.

This being so, I find that most of the blood counts are definitely within normal limits. There are two recorded on pages 1 and 5 which are slightly high, and then there is this peculiar count on page 4. Apart from this I do not think that these blood counts show any evidence of any change taking place in the blood during trance. Personally I should interpret these chemical and microscopical tests in the opposite sense and say that in so far as they go they rather lead to the conclusion that the condition of the blood is as constant as might be expected under normal circumstances. In saying this I must, of course, make an exception of the count recorded on page 4 after the injections of strychnine ; but as I say, one would have to have a great deal more information about this before drawing conclusions from it.

My general opinion about the whole question is this—I should say that these chemical and microscopical tests are such as one might expect to find by repeated tests carried out on the same person. No conclusion can be drawn from the tests that any unusual change takes place in the blood during the period of trance. . . . My own feeling is that the matter is not worth looking into any further along these lines.

Yours sincerely,
(Signed) CUTHBERT DUKES.

(NOTE. In spite of his opinion that the matter was not worth looking into any further along these lines, Dr Dukes very kindly consented to repeat the tests.—K.M.G.)

APPENDIX 2

Mrs Eileen Garrett

Examination into the physiological changes alleged to take place during trance

Blood tests carried out by : Dr Cuthbert Dukes, O.B.E., M.Sc., M.D., D.P.H. Pathologist, St. Peter's Hospital, London ; Director of Research Laboratory, St. Mark's Hospital, London ; Lecturer, Bac., King's College for Women ; Fellow of the Royal Society of Medicine.

Address : 1 Queen Anne Street, W. 1.

All other tests carried out by : Helena Wright, M.R.C.S., L.R.C.P., M.B., B.S.

Address : 9 Weymouth Street, W. 1.

Date ; 16th June, 1937.

Tests carried out on Eileen Garrett (before Trance)

Time	Test	Remarks
5.35		Sterilisation of 2 c.c. Record Syringe completed.
5.40	pulse	76. (Right radial.)
5.43	temperature	98·5.
	respiration	24.
5.45	blood test	(See below.)
5.47	eyes	Pupil reflexes to light ; quick and adequate, both sides.
5.50	knee jerks	Sluggish on left ; absent on right.
	plantar reflex	Flexor on left : none on right.
5.45	blood test	(Preparation : rubber band applied round upper right arm ; skin over vein, anterior of right elbow, sterilised with pad of cotton, alcohol and ether. Blood poured into an anticoagulate : sodium oxalate ; placed in screw-top bottle and bottle closed.) Puncture made in vein with sterilised syringe : 2 c.c. blood withdrawn. Result (reported by Dr Dukes : see letter) : Haemoglobin (Sahli) - 81%. Haemoglobin (Haldane) - 100%. Red Blood Cells : 4,100,000 per cm.

TESTS CARRIED OUT ON " UVANI " GARRETT (DURING TRANCE)

Time	Test	Remarks
5.51		Medium commences to go into trance (yawns, whining, etc., following by quiet, regular breathing).
5.56		Uvani comes through. Is told we want Abdul Latif later.
		Uvani states he is satisfied he is in control of medium, and that tests can be proceeded with.
6.1		(Blood test done—see below.)
6.3	PULSE	108. Very different " feel ". Previously pauses, with slower beat. Now obviously quicker, even, feels stronger.
	RESPIRATION	19. *Very* shallow : uneven.
		(Uvani told by Dr Wright : " I want to count how often you breathe.")
6.10	EYES	*Right eye* (method of opening : left hand of medium trying to force up upper eyelid, right hand of observer pushing down lower lid).
		Eye turned upwards ; apparent difficulty in moving eyeball downwards. Only half the pupil visible. Torch flashed on, quick contraction of pupil, but not maintained while torch kept on : i.e. relaxation occurred in the presence of the light.
		Left eye. (Same technique for opening.)
		Same apparent difficulty in moving eyeballs downwards. But whole pupil visible. Torch flashed on : no contraction at first, then sluggish contraction. No relaxation observed in presence of light.
		For Uvani's remarks, see note (*a*) on page 61.
6.16	PLANTAR REFLEX	No sign on either foot.
		For " Uvani's " remarks, see note (*b*) on page 61.
6.17	KNEE JERKS	Left knee : marked response.
		Right knee : slight response.
		For " Uvani's " remarks, see note (*b*) on page 61.
5.59	BLOOD TEST	Fist clenched.
6.1		Bandage held in right hand. Same process of preparation as above : same syringe, re-sterilised ;

TESTS CARRIED OUT ON "UVANI" GARRETT (DURING TRANCE)—*Continued.*

Time	Test	Remarks
6.2		Blood withdrawn from same vein, 2 c.c. No bleeding. Bandage taken from hand.
		RESULT (reported by Dr Dukes: see letter):
		Haemoglobin (Sahli) - 83%.
		Haemoglobin (Haldane) - 104%.
		Red Blood Cells: 4,200,000 per c.mm.

TESTS CARRIED OUT ON "ABDUL" GARRETT (DURING TRANCE)

Time	Test	Remarks
6.21		"Uvani" goes, being told we now want "Abdul". Medium shifts about, whines, yawns; eyeballs, previously upturned, come down.
6.25		"Abdul" comes through. (See note (*c*) on page 61.)
	KNEE JERK	Left knee: brisk reaction.
		Right knee: exaggerated reaction.
6.29	BLOOD TEST	(See below.)
6.34	PULSE	88. Different quality. Excursion of the beat feels shallower and is irregular, but with small irregularities. Beats very strong.
	RESPIRATION	7. Dr Wright tells "Abdul": "I want to count the number of times you breathe." Each breath slow, much deeper than previously, marked abdominal movement. Absolutely different from "Uvani's" breathing.
6.36	EYES	"Abdul" instructed to try and look at medium's nose.
		Right eye: very slight reaction; wavering.
		Left eye: the same.
		Both eyelids apparently spontaneously held wide open without any blinking; eyeballs staring and quite still; reaction to torch flash is instantaneous contraction then wavering relaxation. Same on both sides.
6.41		Eyelids released: stare maintained from 6.36 without blinking.
after 6.41	PLANTAR REFLEX	Absolute lack of response, both feet.
6.44	TEMPERATURE	98·7. ("Abdul" previously remarks he does not think temperature has changed.)

TESTS CARRIED OUT ON "ABDUL" GARRETT (DURING TRANCE)—*Continued.*

Time	Test	Remarks
6.29	BLOOD	Bandage offered—refused by "Abdul", who shows that he has an excellent grip. Preparation as previously; same syringe: resterilised. $1\frac{3}{4}$ c.c. blood withdrawn from same point on vein.
		RESULT (reported by Dr Dukes: see letter): Haemoglobin (Sahli) - 80%. Haemoglobin (Haldane) - 102%. Red Blood Cells: 4,400,000 c.mm.
		Dr Dukes left consulting room before Dr Wright had finished her tests on "Abdul". Following these, Dr Wright had conversation with "Abdul" on general matters.
6.50		"Abdul" leaves, standing up from couch, very active, and sitting in chair. Usual signs and movements on medium coming out of trance.
6.51		Eileen Garrett back.
	AFTER TRANCE	Pulse 76.

Summarised Results

Personality	Pulse	Temp.	Resp.	Knee jerks	Plantar reflex	Eye reflex
Eileen Garrett	76	98·5	24	*Left*: sluggish. *Right*: absent.	Flexor on left, none on right.	*Right eye*: quick and adequate response. *Left eye*: the same.
"Uvani" Garrett	108	—	19	*Left*: marked response. *Right*: slight response.	No response in either foot.	*Right eye*: quick contraction of pupil not maintained (i.e. relaxation in the presence of light). *Left eye*: no contraction at first, then sluggish contraction. No relaxation in presence of light.
"Abdul" Garrett	88	98·7	7	*Left*: brisk reaction. *Right:* exaggerated reaction	No response in either foot.	*Right eye*: instantaneous contraction, then relaxation, wavering. *Left eye*: the same. Both eyelids held wide open without any blinking for 5 minutes.

Personality	Haemoglobin (Sahli)	Haemoglobin (Haldane)	Red blood cells
Eileen Garrett -	81%	100%	4,100,000 per c.mm.
"Uvani" Garrett -	83%	104%	4,200,000 per c.mm.
"Abdul" Garrett	80%	102%	4,400,000 per c.mm.

Appendix A

Remarks made by "Uvani" (Mrs Eileen Garrett in trance)

Note. When "Uvani" was asked whether he was properly in control of the medium when tests were commenced, he replied:

"The moment that I am able to get response in words (NOTE: presumably this means directly the medium is able to speak as "Uvani"), you must know that a moment *before*, I have control of that part of the mind with which I work; and from the moment that I begin to make preparation for speech, I am using MY attitude through the medium."

Question: "Then by the time we took tests you were in full control of the medium?"

Reply: "I am never in FULL control, but I am in control."

Note (*a*). During the eye tests, Uvani was asked:

Question: "Were you aware when Dr Wright flashed the torch in medium's eyes?"

Reply: "Only ONCE. When the eyelid was lifted and light let in, then at THIS point (touching forehead high above *right* eye) I was aware of vibration of light. On *this* side (touching forehead above left eye) I was not aware of anything."

Note (*b*). When the plantar reflex was being tested, Uvani was asked by Dr Wright: "Can you feel when I touch medium's feet?"

Reply: "No."

When LEFT knee jerks taken Uvani reported he felt it *very* much in forehead area on the right side (3 inches above beginning of hair, in right temple corner, 2 inches from middle line) "like 'click', 'click', 'click'—an effect of light."

When RIGHT knee jerks taken, Uvani stated he felt "a *little* blur, like a little shaft of light—zig-zag—" (felt an inch above beginning of hair, in left temple, 2 inches from middle line).

Note. When the tests were finished on "Uvani", the medium was moved back into lying position on the couch, Uvani explaining he could control the thinking process of the medium, but had difficulty and was slow in moving the medium's body.

Appendix B

Remarks made by "Abdul Latif" (Mrs Garrett in trance)

Note (*c*). "Abdul Latif" appears: robust and hearty—in marked contrast to "Uvani". Is able to move medium's body freely, in contrast to Uvani (see previous note).

When knee jerks are taken, Dr Wright asks " Abdul " : " Does that hurt? "

Abdul replies : " No ; should it? "

Dr Wright says : " Do you know what I am doing? "

Reply : " No."

The knee jerk tests repeated, rather hard tap on knee, whereupon " Abdul " says : " Ah! now you have something that pricks a little —there " (indicating high frontal area on right side of head when left knee tapped).

When blood test was to be taken, and bandage placed in medium's hand, " Abdul " remarked : " I do not want this ; I have a *good* grip—a *good* grip—a *good* grip " (all very hearty talk : HAS an excellent grip).

" Abdul " is asked : " Is it trying for you to hold your hand like that? "

Reply : " No—no, no, no! "

When blood was withdrawn, " Abdul " was asked : " Did you feel that? "

Reply : " No."

Question : " Do you know what was done? "

Reply : " Oh! most certainly. Ha! Ha! You have taken from the vein a little blood."

Question : " What did we use? "

Reply : " A little suction."

Question : " And it didn't hurt you ? "

Reply : " No, no! "

When eyelids opened, " Abdul " was asked : " Do you see more with the eyelids open? "

Reply : " I do not see, but I keep them open for your benefit."

Eyelids held wide open—eyes staring—for period of five minutes without any blinking. Dr Wright inclined to think this pose impossible in normal consciousness—Dr Dukes agreeing with this surmise. Torch flashed several times in pupils during this five minutes. At end of five minutes, " Abdul " instructed to release eyelids, and does so.

When the plantar reflex was tested, " Abdul " states he is unaware of medium being touched.

APPENDIX 3

Copy of letter from Dr Cuthbert Dukes commenting upon the results of the tests made by him.

1 Queen Anne Street, W. 1,
17*th June*, 1937.

Dear Dr Helena Wright,

I enclose herewith the figures I obtained for the haemoglobin estimations and red cell counts on the three samples of blood collected yesterday.

The first was before the medium went into the trance, and numbers two and three were taken at intervals noted in your report when the medium was in a state of trance.

I examined each sample of blood separately and in duplicate and give you the average of the readings. I also estimated the haemoglobin by both the Sahli and Haldane methods. The Sahli always gives a reading 20 per cent. lower than the Haldane, Sahli's normal figures for the haemoglobin being 80 per cent. and Haldane's 100 per cent.

My general conclusion is that there is no variation in the blood count in these different states. Such minor changes as are shown in these recorded figures are within the limits of the accuracy of the methods employed. I finished the tests at 10 p.m. after having started my day's work at 8.30 a.m. and I should be more inclined to attribute any slight differences in the figures to myself rather than to the subject of our experiments.

I very much regret that owing to the pressure of other work I had to make such an unceremonious exit and was unable to take any more personal interest in your side of the investigations. As you know I had just received an urgent call and had to spend all my spare minutes in packing my bag. That is why I was unable to join in your other observations. I should be grateful if you would convey my regret also to Mrs Goldney and thank the subject of our experiments for her willing co-operation.

With kindest regards,

Yours sincerely,

(Signed) Cuthbert Dukes.

APPENDIX 4

Mrs Eileen Garrett (Medium)

Examination into the physiological changes alleged to take place during the trance state

Electrocardiographic Changes.

Tests carried out by Geoffrey Bourne, Esq., M.D., M.R.C.S., F.R.C.P.; Asst. Physician, Officer in charge of Cardiographic Dept.; Demonstrator Prac. Med., St. Bartholomew's Hospital, London. Physician, King George Hospital, Ilford. Hon. Consulting Physician, Letchworth Hospital. Examiner, Pharm. and Therapeutics, University of Cambridge and Med. Soc. Apoth.; Fellow of the Royal Society of Medicine.

Address: 47 Queen Anne Street, London, W. 1.

Assisted by: Helena Wright, M.R.C.S., L.R.C.P., M.B., B.S.
Address: 9 Weymouth Street, London, W. 1.

Date: 27th September, 1937.

Tests carried out on Eileen Garrett (before Trance)

Time	Test	Remarks
5.43	knee jerks	Brisk both sides.
5.45	plantar reflex	? Defensive Reflex. No true Babinsky.
5.46	pupillary reflexes	Both pupil reflexes very marked. No movement of lid. Conjunctival reflex. Corneal reflex.
5.48	ability to keep eyes open without a blink	Test No. 1. Test No. 2. Well under a minute.
5.50	respiration	*c.* 14 a minute.
5.54	electro-cardiograph	Deep breath taken and let out at Dr Bourne's instruction; then deep breath taken, held, let out.

Tests carried out on "Uvani" Garrett (during Trance)

Time	Test	Remarks
5.57		Mrs Garrett instructed to go into trance. Yawns; whines; little gasps; deep breaths (usual signs preceding trance with this medium). "Uvani" greets those present in 2 minutes. States his satisfaction that the electrocardiogram experiment
	electro-cardiograph	should be repeated. Is instructed by Dr Bourne to keep the medium still, while the graphic tracings are made.

TESTS CARRIED OUT ON "UVANI" GARRETT (DURING TRANCE)—*Continued.*

Time	Test	Remarks
6.0	RESPIRATION	*c.* 14 to the minute. Breaths much shallower.
6.2	PUPILLARY REFLEXES	"Uvani" told to open medium's eyes. Apparently opened with great difficulty. *Immediate* pupillary contraction to flash of torch, and relaxation when light turned off. Conjunctival and corneal reflex absent.
6.4	PLANTAR REFLEX	Knee jerks both sides exaggerated. *Plantar reflex*: no response of any kind, both sides.
	ABILITY TO KEEP EYES OPEN WITHOUT A BLINK	Apparent inability to maintain eyelids wide open.
6.5		"Uvani" asked to depart in order to allow Abdul to come through. Abdul through in about 2 minutes with much chatter.

TESTS CARRIED OUT ON "ABDUL" GARRETT (DURING TRANCE)

Time	Test	Remarks
6.7		"Abdul" appears, and chats affably and volubly.
6.9	RESPIRATION	Deep slow breathing. 5 to the minute. (First test.)
	ELECTRO-CARDIOGRAM	Third graphic tracing made.
	RESPIRATION	(Second test.) 7 to 8 to the minute.
6.12	PUPILLARY REFLEXES	Brisk both sides. Conjunctival reflex and corneal reflex absent.
	ABILITY TO HOLD EYELIDS OPEN	NOTE. *Breathing becomes more rapid during this test.* About 20 to minute. Eyelids are held open without any movement for 7 minutes—then are released *on instruction from Dr Wright.*
	KNEE JERKS	Knee jerks exaggerated both sides.
	PLANTAR REFLEX	No plantar responses. (For "Abdul's" remarks, see note on page 67.)
6.30		"Abdul" asked to leave and allow medium to come out of trance. Usual movements, head slightly tossing on pillow; whines, grunts, etc.
6.31		Trance state ends.
	AFTER-TRANCE STATE	NOTE. A fourth electrocardiographic tracing was taken after the trance state had ended; but none of the other tests repeated.

SUMMARISED RESULTS

Personality	Resp.	Knee jerks and plantar reflex	Eye reflexes	Ability to keep eyes open without blinking
EILEEN GARRETT -	*c.* 14	Brisk both sides. No true Babinsky (defensive reflex?).	Both pupil reflexes very marked. Conjunctival reflex. Corneal reflex. No movement of lid.	Two tests made. Both well under a minute.
"UVANI" GARRETT	*c.* 14 (shallower).	Knee jerks exaggerated both sides. Plantar reflex: no response of any kind both sides.	Pupillary contraction to torch flash; relaxation when light turned off. Conjunctival reflex and corneal reflex absent.	Apparent inability to maintain eyes wide open. Apparent difficulty in opening eyes at all.
"ABDUL" GARRETT	5 (1st time) 7 to 8; (2nd time) deep, slow breaths.	Knee jerks exaggerated both sides. No plantar responses.	Pupillary reflexes brisk both sides. Conjunctival reflex and corneal reflex absent.	Eyelids are held wide open, apparently without difficulty, for 7 minutes without blinking. Then released on Dr. Wright's instruction. (N.B. Breathing increases in rapidity—about 20 to the minute—during this test.)

Appendix

Remarks made by "Abdul", in answer to questions put by Dr Helena Wright when knee jerks were taken

Abdul's knee jerks were exaggerated; there were no Plantar responses.

When questioned by Dr Wright, and asked to deseribe what he felt, Abdul replied that he felt a response on the *left* side of head when the *right* knee jerked; and on the *right* side of the head when the *left* knee jerked. He described this response to the jerks as the "sensation of a glow—like the quiek strike of a mateh in the head".

(Note. Compare Abdul's remarks on sensation in head produeed by knee jerks with those made by Uvani during the same (knee jerk) tests on 16th June, 1937—during the experiments earried out by Dr Cuthbert Dukes and Dr Helena Wright. *Uvani* reported an "effeet of light—eliek, eliek, eliek"—on the right side of head when left knee jerked; and "a *little* blur—like a little shaft of light—zig-zag" on left side of head when right knee jerked.) Abdul also deseribed his sensations on this oecasion (16th June), saying: "Ah! now you have something that prieks a little—there" (indieating high frontal area on right side of head when left knee tapped).

Abdul was then asked to suggest how he thought it eould be proved that he—Abdul—and the medium were separate entities. After some ehaff on this subjeet, Abdul suggested that 24-hour tests on Mrs Garrett, followed by 24-hour tests on Abdul (the tranee personality) would neeessitate the opinion that there were two distinct personalities: a male and a female personality; and that a radieal ehange would be shown.

APPENDIX 5

Copy of letter from Dr Geoffrey Bourne commenting on

(1) Results of the tests made by him.
(2) The American figures submitted eonfidentially to him for his eonsideration.

47 Queen Anne Street,
Cavendish Square, W. 1,
23rd November, 1937.

On September 27th, 1937, I examined Mrs Garrett here, with Dr Helena Wright.

I also took eleetroeardiographie traeings of her before her two

trances, during each of them, and afterwards. The prints are enclosed. I can find no signifieant ehange in any of these tracings, whieh obviously eome from the samc individual.

On examining the set of traeings taken by Dr (X) in Ameriea thcre is a similar uniformity with one exeeption, that of the traeing dcscribed as 2 (Tranee) Date Oet. 18th, 1935. Here there is a ehange in the *R-S* eomplcx in Lead 3, but in my opinion this ehange is within the limits of what might be produeed by somc over-distcnsion of the stomach or variation in the position of the diaphragm. Sueh changes in Lead 3 are common. The fact that Leads 1 and 2 remain identical throughout lends support to this view.

The other scnsory changes noted (conjunetival and plantar) are to my mind similar to what is found in eases of funetional sensory ehangc, i.e. functional anaesthcsia.

(Signed) Geoffrey Bourne, M.D., F.R.C.P.

APPENDIX 6

Copy of letter from Dr Helena Wright

The Stone House,
9 Weymouth Street,
Portland Place, W. 1,
Nov. 30th, 1937.

On Junc 16th, and September 27th, 1937, I was present whilc Dr Dukes did the blood tests reported, and Dr. Bourne took thc cleetrocardiograms.

Dr Dukes and Dr Bournc havc sent their conelusions on the results of thcse tests. During the course of both sets of tests I took the various observations detailed in the reports.

The opinions of Dr Dukes and Dr Bourne on the absence of abnormal readings I take to be eonelusive.

In my opinion the cxamination of Mrs Garrett should be extended with the purpose of trying to determine whether any of the phenomena obscrved, sueh as the abscnee of blinking, ete., are different from, or impossible in, a state of hysteria.

(Signed) Helena Wright, M.B., B.S. Lond.

PART II

A REPETITION OF DR J. B. RHINE'S WORK WITH MRS EILEEN GARRETT

By S. G. Soal

I. Introduction

Although my investigation of Mrs Garrett is only a small part of a much larger programme of research dealing with the work of Dr Rhine, I have thought it worth while to pay special attention to Mrs Garrett's work for two reasons.

In the first place Mrs Garrett is one of the very few professional mediums who have placed their services unreservedly at the disposal of the scientific investigator, and for this attitude alone she merits very high praise. In the second place Mrs Garrett quite definitely ranks as one of the subjects for whom Dr Rhine makes very remarkable claims. These claims Dr Rhine has given in considerable detail in an article published in *Character and Personality* (Vol. III, No. 2, December 1934). According to this article Mrs Garrett obtained remarkably successful results in both telepathy and clairvoyance.

Thus in a clairvoyant series of 3525 guesses carried out in the normal state, she wins ·888 correct hits. This is a deviation from chance expectation of +183, i.e. over 7 times the standard deviation. There can be no doubt that this result is highly significant, whatever statistical procedure is adopted in evaluating it. Of this series Dr Rhine remarks: "Out of 3525 trials for clairvoyance in the three days, 1550 were made with a distance of at least 15 feet and with at least one wall between the cards and the sensitive, and these 62 runs of 25 yielded a higher *average* score than did the 1975 made with the cards on the table with the sensitive" (6·5 per 25 as against 6·1 per 25).

In another series of 625 trials at Pure Telepathy she scored 336 correct hits, this being equivalent to an average of 13·4 correct hits per 25 guesses sustained over 25 packs of cards. The reader will be able to appreciate the remarkable significance of this result when I assure him that in a series of over 100,000 guesses (using a random sequence of cards) I have obtained only a single set of 25 with as many as 13 guesses correct and no set with more than 13 correct, a result which is in accordance with chance expectation.

Of course, in the above 625 experiments of Dr Rhine presumably

the experimenter constructed a mental series of card-images in lieu of using actual cards, and it would be an assumption under such circumstances to take mean chance expectation as 5 successes per 25 guesses. At the same time I fail to see how the average could reach the level of 13·4 unless both guesser and experimenter practically confined themselves to the *same pair* of symbols, and one can hardly suppose that Dr Rhine could pass over without mention such a glaring absurdity as this. I can therefore find no *normal* means of accounting for Dr Rhine's success, and it would be superfluous to quote more figures from his article. I will merely note that although the experiments with the medium in the trance state were few in comparison with those in which she was normal, there was one series of 100 for telepathy in the trance state in which she scored no less than 44 correct hits.

When, therefore, Mrs Garrett at the beginning of last May expressed her willingness to try some experiments with Zener cards, I eagerly acquiesced. In order to create a friendly atmosphere it was arranged that Mrs Garrett and I should meet at lunch at Mrs Henry Richard's house in company with a biologist and a medical man interested in her work. The first 375 guesses were done at Mrs Richard's house after lunch with Mrs Goldney acting as agent, but the remainder of the series were carried out in the Psychological Laboratory at University College, Gower Street. Full details of the experiments will, I hope, appear in my forthcoming report on Extra-Sensory Perception, but as this may not be available for some time I feel it is advisable to put on record now a fairly complete account of the experiments and results obtained.

Mrs Garrett has now returned to America, and until she comes back there is no prospect of further experiments being carried out with her in this country.

2. General Remarks on the Experiments

I have been assisted by eleven persons in the conduct of the experiments. Five of these were advanced students in the departments of Psychology and Philosophy at University College. These were Miss J. Stephens (J.S.), Mr A. J. Marshall (A.J.M.), Mr A. J. Bould (A.J.B.), Mr D. C. Russell (D.C.R.), and Miss M. Baldwin (M.B.). One of these (A.J.B.) showed me some remarkably significant results which he had obtained at home with his brother acting as agent. The two brothers told me that they sat in different corners of the room, but were not screened from each other. The younger brother (H.G.B.) thought of a sequence of Zener card images to be

guessed by A.J.B., but no actual cards were used. A.J.B. told me that during the experiment he did *not* look at his brother.

In these experiments neither of the sequences would be random sequences, and it would be hazardous to assume that the mean chance expectation for 25 guesses was 5. The two brothers came to University College and carried out telepathic experiments in which a random sequence of actual Zener cards was employed, a screen being interposed between the two brothers. At certain of these experiments I was present as a witness, but at others I was not. None of the results obtained were significantly above chance expectation. On one occasion (in my absence) the brothers reverted to their former method, except that they used a screen, but again the results were without significance, although there was some interesting light thrown by one short sequence on the possibility of similar number or sequence habits being evoked by an external stimulus.

Of the remaining six persons, Miss E. Siderapoula (E.S.) is the Secretary of the Philosophical Department at University College. She is keenly interested in clairvoyance and has assisted me on many occasions. One day she took home with her a few packs of my Zener cards to experiment with alone, and obtained what appeared to be remarkable results. These results, however, could not subsequently be confirmed in my presence in the laboratory. I pointed out to E.S. that the three packs she took home contained each an unusually large number of the same particular symbol. As E.S. would become aware of this after doing a few sets of 25, no great importance could be attached to the high scores obtained. E.S. has by training now become a very competent experimenter, and under good conditions does not exhibit any faculty of clairvoyance (that is in my presence).

Mr H. J. Heckle (H.J.H.), Mrs M. Johnstone (M.J.) and Miss Rita Elliott (R.E.), who took charge of the experiments in Series (ii) (Table B), have assisted me for periods varying from one to three years, and I feel that now I can trust them to carry out my various techniques without deviating from the prescribed routine.

The experiments fall naturally into two distinct series, which I shall call Series (i) and Series (ii). In Series (i), which was carried out between 8th May and 24th June of last year, I was present inside the room on every occasion and manipulated the cards, there being also present Mrs Garrett and the agent or witness.

In the second series, carried out between 30th August and 30th September, I arranged for the agent to be alone with Mrs Garrett while I remained in an adjoining room. In this latter series the

experimenters were, with one exception (Mrs Naumberg, an American visitor), chosen from my trained helpers, and I exhorted them to do all in their power to give the medium encouragement of a verbal kind and praise for successes, etc.

It is often urged by the more emotional devotees of the supernormal that when a medium fails under test conditions to obtain his or her accustomed phenomena the failure is not due to the exceptional care and good technique of the new experimenter, but to the fact of his critical or sceptical mentality inhibiting the free play of the psychic's powers. It is doubtless true that many fraudulent mediums have tried to explain away their failures under the cover afforded by this hypothesis. Nevertheless there may be an element of truth in the hypothesis itself, though I doubt if it is wholly true. It is maintained by Dr Rhine and others that the secret of success with these Zener cards lies in the power of the experimenter to suggest the idea of success to his subject. Now the most suggestible state we know is the hypnotic state, and I have experimented extensively with at least two hypnotic subjects. I was able to put these subjects to sleep, to induce in them visual and auditory hallucinations, complete insensitiveness to pain as tested by deep pricking with a sterilised hat-pin, and the successful performance of post-hypnotic suggestions. When I suggested that the subject should see what was on the face of a Zener card he saw and described one of the five symbols at once, but statistical tests showed that what he saw by suggestion had no relation to the symbol on the actual card itself. Of these experiments with the hypnotised subject, Mr Ryecardt, 500 were tests in Pure Clairvoyance with 105 correct hits, and 350 were (U.T.) experiments with 63 correct hits. In neither case were the results significant. It does not seem to be true, therefore, that mere power to suggest success can be the real secret. On the other hand I have had at least one subject (a well-known art photographer) who possessed unlimited confidence in his power to guess the symbols on cards. He came to No. 14d Roland Gardens, saying that he had not come to be tested, but to "demonstrate" to us the reality of telepathy. He told us that he had often read a whole pack of cards through correctly from top to bottom. We tested him, using a screen, but to his own disappointment and amazement he failed completely. He then told us that he had usually done this kind of thing in the evening after having imbibed a certain quantity of alcohol. He invited me and a friend to his studio one evening, and after a suitable libation he tried the telepathic tests, but again failed absolutely to beat the laws of chance.

In order therefore to eliminate any disturbing effects on Mrs Garrett due to my mentality, which I admit is somewhat sceptical, I took the precaution of carrying out Series (ii) in which I should be absent from the room. If it is retorted that when I was not in the room I still exerted a telepathically inhibiting effect on Mrs Garrett, then I am afraid I have no answer. At the commencement of the experiments Mrs Garrett told me that she would prefer not to know the exact number of correct hits she obtained on each occasion. However, I thought it advisable to inform her when she scored an eight or a nine ; and when she obtained a six or a seven I said usually, " That's splendid, you're doing fine," etc.

3. Summary of Techniques employed

We have made experiments in both Pure Clairvoyance and Undifferentiated Extra-Sensory Perception, with the medium in the normal state and also in the alleged trance.

I must explain at the outset that three persons are concerned in the experiments of Series (i). In clairvoyance experiments these are the experimenter (S.G.S.), the witness and the subject or guesser (Mrs Garrett or her trance personality). The experimenter is the person who deals out the cards and keeps the record, while the duty of the witness is to check the number of guesses reported correct by the experimenter. The witness also signs the record sheet, and thus testifies that he is in agreement with the entries made by the experimenter.

In the telepathy experiments the experimenter (S.G.S.) again deals out the cards to be guessed, and generally supervises the work ; but in this case the witness or *agent*, as he is now called, has duties in addition to those of checking and witnessing. He is the person who looks at the card while I hold it close to the screen, and tries to transmit its image to the guesser, Mrs Garrett, who sits behind the screen. This second person is therefore called the witness in clairvoyance experiments and the agent in telepathy experiments.

In Series (ii) (Table B) only two persons were present : the subject, Mrs Garrett, and the agent, who was now also the experimenter.

Without exception the Pure Clairvoyance work was done by means of a " screened " matching technique. In these experiments denoted by (P.C.M.) (normal) or (P.C.M.) [1] (Trance) the medium sat behind a black metal screen measuring 2 feet broad by 1 foot

[1] In what follows underlined techniques such as P.C.M. or U.T. signify that the medium was in the alleged trance.

6 inches high, which stood on the table. Between the bottom edge of the screen and the table was a gap $\frac{1}{2}''$ high, through which the medium could push a light metal pointer. The screen stood over the centre line of a row of five " target " cards bearing in order the symbols +, 0, Star, Rectangle, Wave, and fixed faces upwards to the table by means of drawing pins. Between each pair of cards was a gap of about $\frac{1}{4}''$. The experimenter sat facing the screen, and with his left elbow resting on the table held the pack of 25 cards in his left hand close to the centre of the screen, the backs of all the cards being towards him. At the signal " Next " the medium had to guess the bottom card of the pack (i.e. the card nearest the screen) by pointing with the rod at the corresponding " target card ". The experimenter then lifted off the bottom card, and without looking at its face placed it carefully opposite the target card chosen by the medium. At the end of 25 guesses the 25 cards thus found themselves arranged in five piles, all the cards being face downwards. To prevent confusion by mixing, the five piles were planted at considerably wider intervals than were the " target " cards, but in the same order as the latter.

A count was then made of the successes under each symbol, and these were recorded on the scoring sheet by the experimenter thus :

$$5 \begin{cases} + & 1 \\ 0 & 0 \\ S & 2 \\ R & 1 \\ W & 1 \end{cases}$$

The count was carefully checked by the witness.[1] If the score obtained by the medium was 6 or more, she was told, " You are doing splendidly! " etc., and when the score was 8 or more I usually told her the exact number. When the score was 5 or below 5, I passed to the next pack without comment.

During the trance state the medium sat with her chin supported on her left forearm. Her eyes were at first closed, but I managed to induce " Uvani " to open them, and in this state he was able to manipulate the pointer quite satisfactorily with his right hand in both the (U.T.M.) and the (P.C.M.) experiments.

I performed a considerable number of experiments in " Undifferentiated Extra-Sensory Perception " by the use of a " screened " matching technique, since both Dr Pratt and Mr Tyrrell lay considerable stress on the importance of what we might term " auto-

[1] The distance between subject and experimenter was about 3 feet 6 inches.

matie " methods of guessing. This technique denoted by (U.T.M.) (Normal) or (U.T.M.) (Tranee) is not a very rigorous one. Nevertheless I thought it would be interesting to see if Mrs Garrett would obtain any apparent sueeess by its use. If any signifieant results had appeared I should, of course, have made a eareful investigation of its normal possibilities.

The chief danger lies in the possibility that the agent or experimenter watehing the motion of the pointer along the row of eards may make some involuntary audible movement that will eause the medium to stop the pointer at the eorreet eard, whieh, of eourse, is known to both agent and experimenter. The objeetion does not apply to the (P.C.M.) teehnique. I have, therefore, in addition, done a series of telepathy experiments using my ordinary more rigorous (U.T.) teehnique.

In the (U.T.M.) and (U.T.M.) series the experimenter sat at the side of the table elose to the sereen, and the agent faeed the sereen. As before, the experimenter held the paek close to the eentre of the sereen with the baeks of the eards towards the agent's faee. Having enquired if the medium was ready to begin, the experimenter elosed his eyes immediately before lifting off the top eard of the paek for the agent to visualise, and then with elosed eyes ealled out " First Guess " or " Next ", as the ease might be. Immediately the eard was exposed the agent looked at it, and then formed a visual image in his mind or sometimes a verbal image of the name of the eard. The agent remained silent throughout. By this method the experimenter, who is ignorant of the eard exposed when he signals " Next " to the medium, ean give nothing away by the infleetions of his voiee. Not until the medium's pointer had eome quite definitely to rest on some partieular eard did the experimenter plaee the exposed card faee downwards on the appropriate pile. The details of reeording and eheeking are the same as in the (P.C.M.) experiments.

In the ordinary (U.T.) (Normal) and (U.T.) (Tranee) experiments the subjeet was hidden both from experimenter and agent by a light wooden sereen, measuring 3′ 0″ by 3′ 0″, which rested aeross the eentre of the table, there being no gap between sereen and table. A vertieal board fixed underneath the table and used in both (U.T.) and (U.T.M.) experiments prevented the subject from getting a glimpse of the agent's legs or feet, or from making any eontaet with them beneath the table. As explained in the (U.T.M.) technique, the experimenter was ignorant of the card exposed when giving the signal " Next " to the medium. In the (U.T.) (Normal) experiments the medium reeorded her own guesses in peneil in the left-hand column of a seoring sheet designed to hold two sets of

25 guesses. In the (U.T.) (Trance) experiments the control "Uvani" called out aloud the guess, which was recorded by the experimenter on his own scoring sheet. In both (U.T.) and (U.T.) experiments the experimenter recorded the actual card sequence by means of five rubber stamps of similar make and weight, the "actual" symbols being thus inscribed in the right-hand column of an identical scoring sheet. When the medium had recorded her guess she intimated the fact by giving two taps with her pencil on the table.

At the end of 25 guesses the medium read aloud her column of guesses, still remaining on her side of the screen, while the experimenter copied the list on his own scoring sheet. The process was then repeated for the next 25 guesses, and when the 50 guesses were completed, the medium, still retaining her seat behind the screen, handed the sheet to the experimenter, who then copied his own "actual" card columns on to the medium's sheet. In this way the medium saw nothing of the actual card sequences which had appeared.

In the (U.T.) (Trance) experiments only a single record sheet was kept, since it was felt that the control "Uvani" would find it impossible to use a pencil while the medium was in the trance state. In this case the control "Uvani" called aloud his guesses, which were recorded by the experimenter. Both scoring sheets in (U.T.) work were signed by both agent and experimenter before passing on to the next set of 50 guesses.

Whenever the technique employed admitted the making of a duplicate record sheet, this was posted by the agent to Dr C. E. M. Joad in an envelope sealed in my presence.

As regards the elimination of involuntary whispering by the agent or experimenter, no especial precautions were taken beyond instructing the agent to keep his mouth tightly closed throughout the work. Before the experiments commenced, the room was carefully studied for the possibility of reflections, and the necessary adjustments made in this respect.

It should be added that all the agents and witnesses employed were favourably disposed towards a belief in the reality of telepathic phenomena. Two of them (K.M.G.) and (M.J.) had obtained what they believed to be genuine supernormal results with trance mediums; and another (E.S.) thought she had obtained remarkable results when using a pack of Zener cards at home, which results, however, failed to materialise under more critical conditions.

In the tables which follow I have been unable to make any analysis of "runs" of successes (e.g. 2, 3, 4, 5, etc., consecutive

successes), since the Screened Matching methods do not readily allow of the experimenter keeping any account of such runs. For the same reason it was not found possible to analyse the distribution of a smaller group than 25 guesses.

4. Summary of Results

In Tables (A) and (B) the results opposite the same index letter but with different suffix numbers were obtained on the same day. For instance, B_1, B_2 correspond to the date May 13 and D_1-D_6 to May 27.

Table (A)
(May 8-June 24, 1937)

Index letter	Agent or witness	Technique	No. of guesses	No. of hits	Value of χ^2
A_1	K.M.G.	U.T.	375	76	0·017
B_1	J.S.	U.T.	500	112	1·800
B_2	S.G.S.	U.T.	325	74	1·558
C_1	A.J.M.	U.T.	550	111	0·011
C_2	A.J.B.	U.T.M.	375	74	0·017
D_1	A.J.B.	U.T.M.	225	41	0·444
D_2	D.C.R.	U.T.M.	600	110	1·042
D_3	E.S.	U.T.M.	200	33	1·531
D_4	D.C.R.	P.C.M.	450	85	0·347
D_5	A.J.B.	U.T.M.	200	52	4·500
D_6	A.J.B.	U.T.M.	200	36	0·500
E_1	M.B.	P.C.M.	1000	198	0·025
E_2	E.S.	P.C.M.	1425	277	0·281

For Table (A), Series (i), we have $S(\chi^2)=12{\cdot}073$, which, with $n=13$ degrees of freedom, gives a value of P lying between 0·5 and 0·6.

This may be interpreted to mean that if the series of results in Table (A) had been produced by chance only, a value of $S(\chi^2)$ as large as 12,073 or larger might be expected to occur between 50 and 60 times in every hundred such series. In other words we ought to be no more surprised at getting such a value than if we predicted that a halfpenny should turn up "heads" and found on tossing it that it actually did turn up a head. If however we had obtained $S(\chi^2)=28{\cdot}0$ or $S(\chi^2)=3{\cdot}0$ we should

have had grounds for suspecting that some other factor than pure chance had been at work since either of these values might be expected to occur on an average less than once in 100 trials.

TABLE (B)

Series (ii). (S.G.S. absent from room)
(Aug. 30-Sept. 30, 1937)

Index letter	Witness or agent	Technique	No. of guesses	No. of hits	Value of χ^2
F_1	E.S.	U.T.M.	1000	220	2·500
G_1	H.J.H.	P.C.M.	1000	190	0·625
H_1	R.E.	U.T.M.	1000	191	0·506
J_1	M.J.	P.C.M.	400	80	0·000
J_2	M.N.	U.T.M.	400	81	0·016
J_3	M.J.	U.T.M.	200	52	4·500
K_1	M.J.	U.T.M.	1000	212	0·900
L_1	E.S.	P.C.M.	725	150	0·216
L_2	E.S.	U.T.M.	275	60	0·568

For Table (B), Series (ii), we have $S(\chi^2)=9{\cdot}831$, which, with $n=9$ degrees of freedom, gives a value of P lying between 0·3 and 0·5.

NOTE. Series F, G, H, J were done on consecutive days, as were series K, L.

TABLE (C)

Totals for Series (i) and (ii)

		Guesses	Hits	Dev.	St. Dev.[1]
Series (i)	Telepathy - -	3550	719	+ 9	±23·8
	Clairvoyance -	2875	560	−15	±21·4
	Total - -	6425	1279	− 6	±32·1
Series (ii)	Telepathy - -	3875	816	+41	±24·9
	Clairvoyance -	2125	420	− 5	±18·8
	Total - -	6000	1236	+36	±31·0
	Grand Totals -	12,425	2515	−30	±44·6

[1] St. Dev. denotes the standard deviation calculated from the Binomial formula $\sqrt{n \times \frac{1}{5} \times \frac{4}{5}}$, which for my distribution is permissible.

It will be seen from Tables (A) and (B) that there is no reason to suppose that any of the agents have obtained paranormal results with Mrs Garrett. The values of χ^2 represent the squares of the deviations from mean expectation divided by the squares of the corresponding standard deviations. If the value obtained for χ^2 exceeds 4·0 this means that the deviation of the actual number of correct hits from the expected number is either in excess *or* deficit more than twice what is called the " Standard Deviation ". Now in a " Normal " distribution a deviation which is *numerically* greater than twice the Standard Deviation occurs on the average about once in 20 trials. Such a deviation is therefore sufficiently remarkable to excite our interest, and to suggest at least that something more than chance may have been at work. The deviation is said to be " significant " if the odds against its being due to chance exceed 20 to 1. Thus, if I take a perfectly constructed cubical die whose six faces are marked 1, 2, 3, 4, 5, 6 respectively, and shake it inside a box 240 times in succession, the expected number of times the face " 3 " should turn up is $240 \times \frac{1}{6} = 40$.

This does not mean, of course, that " 3 " will turn up exactly 40 times in 240 throws, for sometimes there will be more than 40 " threes " and sometimes less. But if I did the experiment of throwing the die 240 times again and again, the *average* of the numbers obtained would approximate to 40 in the long run. The chance that a " three " will turn up at a single throw is clearly $\frac{1}{6}$, and hence the chance that it will *not* turn up is $1 - \frac{1}{6} = \frac{5}{6}$.

Now if, as in this ideal case, the chance is constant at each trial, the number of " threes " which turn up will obey what is known as the Binomial Distribution. Thus if I tossed 12 such perfect dice simultaneously in 12 cups, the possible number of " threes " that could appear would range from 0 to 12. And if I did the experiment of tossing the 12 dice 1000 times in succession, the number of cases in which I should get 0, 1, 2, ... 12 " threes " would approximate to the successive terms of the Binomial expansion of $1000 \times [\frac{5}{6} + \frac{1}{6}]^{12}$. That is to say the expected number of cases in the 1000 trials in which 0 threes turned up would be $1000 \times [\frac{5}{6}]^{12}$.

The expected number of cases with 1 " three " is

$$1000 \times 12 \times [\tfrac{5}{6}]^{11} \times [\tfrac{1}{6}],$$

etc., while the expected number of cases with 12 " threes " would be

$$1000 \times [\tfrac{1}{6}]^{12}.$$

The " Standard Deviation " of the Binomial Distribution is given by the formula $\sqrt{np(1-p)}$, where n is the number of trials, and p

is the chance of success at a single trial, this chance being supposed to remain constant during the series.

Thus in our first example of throwing a die 240 times in succession we have $n=240$, $p=\frac{1}{6}$.

Hence the Standard Deviation for 240 trials is $\sqrt{240\times\frac{1}{6}\times\frac{5}{6}}=5{\cdot}77$. If I tossed a single die 240 times in succession and counted more than 52 " threes " or less than 28 " threes ", I should have obtained a deviation in either case from the average (40) of more than *twice* the standard deviation. That is the odds against such a result being due to the fluctuations of chance would be nearly 20 to 1, and I should suspect that the die was not evenly balanced about its centre. A solitary deviation of this magnitude would, of course, *prove* nothing, but would suggest that I should repeat the experiment of tossing the die 240 times to note if similar deviations were obtained. If I got a similar result the next time, I should go on making repetitions until I was satisfied that some factor beyond chance was in operation.

If, however, I did the experiment 60 times in succession and found that in only three cases did the deviation numerically exceed twice the standard deviation and in each case by no serious amount, and if in addition I found that the total deviation of the number of " threes " which turned up in the whole $60\times240=14{,}400$ trials differed from the chance expectation (2400) by less than twice the standard deviation for $n=14{,}400$ trials, I should have no reason to suspect any other factor at work than the fluctuations produced normally by chance.

The reader may apply the above to the case of Zener cards by taking $p=\frac{1}{5}$ instead of $\frac{1}{6}$ and n the number of guesses. Thus the standard deviation in 200 guesses with Zener cards is

$$\sqrt{200\times\tfrac{1}{5}\times\tfrac{4}{5}}=5{\cdot}65.$$

It should be pointed out, however, that the above formula for standard deviation only strictly holds if the series of cards to be guessed is chosen in a " random " or haphazard order. This would be the case if at each guess the card chosen by the experimenter was determined by tossing a perfectly made *five*-sided die, each face of which was inscribed with one of the five symbols (assuming the construction of such a die to be feasible). The formula would not, for instance, be strictly applicable to packs of 25 cards made up according to Dr Rhine's plan with exactly 5 cards of each symbol per pack. For in this case the chance that a card turned up should be (say) a *circle* fluctuates as we work through the pack. The first five cards might happen to be all circles, and in that case the chance

of the *sixth* card being a circle is clearly zero and that for each of the other four suits is $\frac{1}{4}$. Dr Rhine's series of cards do not therefore constitute a random distribution, and a slight correction is required in the formula giving the standard deviation for n guesses.

The actual problem is, however, further complicated by the fact that a person guessing 25 cards in succession will not guess exactly 5 of each symbol, even though he knows that the pack contains equal numbers of each suit. In the absence therefore of the knowledge of how the various guessers have distributed their guesses over the five symbols Dr Rhine's problem is strictly indeterminate, and even if we possessed this knowledge the exact solution would appear to be a rather formidable exercise in combinatory analysis.

Although the ultimate corrections that have to be applied may be only slight, I was fully alive to these difficulties when I commenced my experiments in 1934, and I decided to circumvent them if possible by using a random sequence of symbols. I had at my disposal exactly 1200 cards, there being 240 of each symbol. From these I made a random distribution of 1000 cards as follows.

I first associated with each of the symbols +, 0, Star, Rectangle, Wave the respective numbers 1, 2, 3, 4, 5. I then provided myself with Chambers' *Seven-figure Mathematical Tables*, and read from them the *last* digits of the *logarithms* of the following numbers :

10078, 10178, 10278, ... 99978.

The numbers chosen were thus taken at intervals of 100, so as to ensure that the last digits in the logarithms should be independent. If the digit happened to be one of the numbers 1 to 5 the digit was entered on the list, or rather the corresponding symbol was written. If the digit happened to be 0 or 6, 7, 8, 9 it was not entered. From this sequence I thus obtained a random series of about 450 cards.

The process was then repeated with, say, the following numbers :

10043, 10143, 10243, ... 99943,

and so on, until a list of 1000 cards had been compiled.

The actual cards were then chosen one by one, according to the above list, from the 1200 cards in my possession.

In the end there were, of course, not exactly 200 cards of each symbol, and up to January 1937 I made the mistake of adding or removing a few cards in such a way as to disturb the distribution as little as possible until I had *exactly* 200 of each symbol. After January 1937 I made fresh random distributions, but without the above adjustment, which is strictly illegitimate.

After the random series of 1000 cards had been compiled I

divided it into 40 consecutive blocks of 25, and placed the 40 sets of 25 in their original order into 40 envelopes numbered 1-40. These envelopes were then placed in order in two cardboard boxes, each containing 20 envelopes. Mrs Garrett began with pack No. 1 on 8th May, and worked through the 40 envelopes in order till she had completed the first 1000 guesses. Strictly I should at this point have worked out a *fresh* random distribution of the 1000 cards, but the labour involved would have been prohibitive. Before commencing the next 1000 guesses, therefore, I reshuffled all the cards in each of the 40 packs, and also reshuffled the 40 envelopes in the boxes.

I then worked through the envelopes in the new order in which they stood in the boxes. In this random distribution the numbers of the five symbols in the order +, 0, Star, Rectangle, Wave, were respectively

203, 198, 200, 202, 197.

After series (i) (6425 guesses) had been completed, I went through the labour of compiling a fresh random distribution from Chambers' tables, which was used for series (ii).

The numbers of the five symbols in this distribution were :

194, 205, 206, 195, 200.

Returning now after this digression to Tables (A) and (B), we see that there are just two values of χ^2 which exceed 4 (one in Table (A) and one in Table (B). Both these occur in small sets of 200 guesses. Ignoring for simplicity the first 25 guesses we have in all 12,400 guesses. Now if we take out these two sets of 200 with $\chi^2=4{\cdot}5$ we are left with 60 consecutive groups of 200, and I find that in this batch of 60 groups there is just one with a deviation (negative) with $\chi^2=4{\cdot}0$. This group of 200, however, is split between consecutive days and different agents. In 62 groups of 200 we should expect about 3 groups with $\chi^2>4$, and we have seen there are three such groups. Moreover, when in (J_3) Mrs Garrett obtained $\chi^2=4{\cdot}500$, I was careful to make her do another 1000 experiments with the same agent (M.J.) on the following day. There is nothing abnormal about this group of 1000 guesses, and if it is split up into 5 consecutive groups of 200 none of the values of χ^2 even approach 4. I have split up all the other groups of 1000, 600, 400, etc., given in Tables (A) and (B), and can find no group of 200 with $\chi^2>4$ except the two which I have cited. A comparison of Tables (A) and (B) shows clearly that Mrs Garrett is no more successful in my absence than when I am present in the room. My

experiments with other subjects point in the same direction. None of my trained helpers when left alone with a subject appear to succeed any better than myself. So far as my own observations are concerned the personality of the experimenter appears to have no effect on the results, even though some of my helpers might be classed definitely as " believers ". None of these helpers, however, could be described as " highly emotional " or " temperamental " people, though their mentalities are of a rather different type from my own. If one employs hysterical or excitable persons as experimenters one will assuredly obtain positive results if one is not present to watch them as a cat watches a mouse. But if, on the other hand, one does decide to watch such experimenters, one is immediately told that one's " personality " inhibits the results if these results happen to be negative.

TABLE (D)

(Totals for Telepathy Experiments)

Technique	Guesses	Hits	Dev.	St. Dev.
(UT) and (UT) - -	1750	373	+23	±16·7
(UTM) and (UTM) -	5675	1162	+27	±30·1
Totals - -	7425	1535	+50	±34·5

TABLE (E)

(Totals for Pure Clairvoyance Experiments)

Technique	Guesses	Hits	Dev.	St. Dev.
(PCM) - - -	2400	468	−12	±19·6
(PCM) - - -	2600	512	− 8	±20·4
Totals - -	5000	980	−20	±28·3

TABLE (F)

(Totals for Experiments with Medium in trance)

Technique	Guesses	Hits	Dev.	St. Dev.
Telepathy - -	3050	613	+3	±22·1
Clairvoyance - -	2600	512	−8	±20·4
Totals for Trance -	5650	1125	−5	±30·1

Tables (D) and (E) show that the medium succeeds on the whole with neither telepathy nor clairvoyance. It is also seen that there is apparently no virtue in the "screened matching" or "motor" methods advocated by Mr Tyrrell and Dr Pratt. Table (F) demonstrates that the medium succeeds no better as "Uvani" than when she is her normal self.

An analysis of the various groups of results has been made by sets of 25, but in a mere summary of results I shall content myself with a single analysis of the 12,425 guesses.

TABLE (G)

(For 497 sets of 25)

	A	E	X	X^2/E
0, 1	20	13·613	+ 6·387	2·99669
2	25	35·207	−10·207	2·95915
3	70	67·483	+ 2·517	0·09388
4	84	92·790	− 8·790	0·83268
5	94	97·432	− 3·432	0·11472
6	91	81·195	+ 9·805	1·18404
7	55	55·092	− 0·092	0·00154
8	34	30·993	+ 3·007	0·29174
9	16	14·637	+ 1·363	0·12692
10, 11, etc.	8	8·607	− 0·607	0·04281
Totals -	497	497·049	χ^2 =	8·64417

In the above table column A gives the actual number of sets of 25 guesses with (0, 1), 2, 3, etc., correct hits. In column E we find the expected numbers according to the Binomial Distribution. These numbers were obtained by calculating the successive terms of the expansion of $497 \times [\frac{4}{5} + \frac{1}{5}]^{25}$. The column X gives the value of the difference A-E. Adding up the values of X^{25}/E we find $\chi^2 = 8{\cdot}64$, which with $n = 9$ degrees of freedom gives P lying between 0·45 and 0·50. It is seen, therefore, that using a *random* distribution of 1000 cards to start with we do get excellent agreement with the Binomial Distribution. There is no reason whatever to suppose that Mrs Garrett obtains sevens, eights, nines, etc., more often than chance would predict.

5. CONCLUSION

In the case of Mrs Eileen Garrett we fail to find the slightest confirmation of Dr J. B. Rhine's remarkable claims relating to her

alleged powers of extra-sensory perception. Not only did she fail when I took charge of the experiments, but she failed equally when four other carefully trained experimenters took my place. None of these four persons were of the " academic " type ; they were persons in very diverse walks of life. The one asset they had in common was a long and meticulous training in the manipulation of cards ; they had been carefully drilled in avoiding the different sources of error, and they were not likely to originate any careless methods.

The writer first made the acquaintance of Mrs Garrett in the year 1926, and in the autumn of that year had a series of sittings with her at which half a dozen persons on the staff of Queen Mary College were introduced anonymously. Careful records made by myself (and in several cases by a shorthand notetaker) were annotated by the sitters, who were all highly intelligent persons. Not a single sitter was impressed or gave a verdict favourable to the supernormal.

The more serious question will doubtless arise as to whether Dr Rhine's other major subjects would fare any better if they crossed the Atlantic. If any of them decide to visit England they may be assured of the same courteous consideration and careful but dispassionate examination that we have tried to accord Mrs Garrett.

I append a statement by Mrs Garrett in which she compares the conditions of the present experiments with those obtaining in America. This entirely voluntary statement was written by the medium *before* she was told that the results had been entirely negative.

It will be noted that Mrs Garrett actually prefers my methods to those of Dr Rhine, and believes them to be better adapted to the free play of her psychic powers. Indeed, by preserving a quiet demeanour free from any violent display of emotion or over-anxiety for positive results, I was simply conforming to what has been the traditional creed of mental mediums since the dawn of Spiritualism. Over and over again one is warned in the works of Spiritualists of every generation to " keep calm and passive ", " to relax the mind while sitting with the medium ", etc., etc. Above all we are warned not to badger or press the " spirits " for evidential results or detailed answers to questions ; we are to remain passive and hope for the best.

I did not expect to obtain from Mrs Garrett the really extraordinary results claimed by Dr Rhine, but I must confess that I did hope that a medium who had produced an average of 13·4 correct hits per 25 and maintained it over a succession of 25 packs of cards in North Carolina would, at any rate, be able to demonstrate a few lesser miracles in the country north of the Thames. At the same time I realise that the medium may have been in some peculiar

emotional state at the time when she apparently succeeded with Dr Rhine, and that possibly she has been unable to recapture this favourable condition of mind since.

At any rate, I do not wish to convey the impression that Dr Rhine did not actually succeed in obtaining highly significant results in his work with Mrs Garrett. I merely record the fact that neither I nor my assistants succeeded in getting them.

I should like to add that the impression which Mrs Garrett produced upon me was one of perfect honesty and straightforwardness and on this note of appreciation I will end.

Appendix

Statement by Mrs Garrett

134 Piccadilly, London, W. 1,
22*nd June*, 1937.

I have been asked by Mrs Goldney to describe the difference in conditions, and what I might term " psychological atmosphere " at Duke University (working for Dr Rhine) and in London (working for Mr Soal) ; further to say how I think that I, as an individual medium, would respond to these conditions.

When I first went to Duke University, I had no knowledge at all of what was expected of me. When I found it was card experiments, with something that suggested a game of guessing, I naturally was quite relieved for a day or two, and thought what was required of me was easy, and entered into the spirit of the thing. After a day or two of this, however, I began to suspect, though I was not certain, that I was not using the same receptiveness in guessing that I use when I am working clairvoyantly. When I discovered that, I began to get fidgety, and bored with the cards. So much so that I think Dr Rhine makes a note of my boredom in his Report. Nevertheless I liked his personality, his cheerfulness, his ease and his vitality ; and these qualities in him certainly made me feel I must give results. I still feel, however, that all my results at Duke were given in this vein, rather than in what I would describe as a spontaneously psychic working of my mediumship.

I discussed while at Duke with Dr Rhine the suggestion that I should work with Mr Soal, and remarked that I wondered how his very different personality, his quietness, and what might be called his negative appeal to me, would affect results.

I will now describe my work in London with him. To date I have done about 4000 guessing experiments, and am to continue. I asked that I should not be told the results until the end ; I have,

however, had a letter from Mr Soal telling me that the results are ' most interesting ' and that it is urgent that I continue. Whether ' most interesting ' means that the results are positive, or completely negative, I do not know. I fancy Mr Soal would regard either result as ' most interesting '. I find that while working with Mr Soal I am able entirely to eliminate the cards, and the personality of Mr Soal : in fact, the only thing of which I am conscious is the idea that I am to display my mediumship as best I can. There is no emotional tension, urging, or strain to produce results, such as is noticeable at Duke.

Having worked both ways I prefer the Soal method and set-up ; and would feel more at my ease in continuing these conditions than going back to those at Duke. Without knowing the results of my work with Mr Soal, I feel that his method, and the conditions working with him, would be more likely to bring out my mediumistic capabilities than would the Duke conditions. I would explain this feeling I have by saying that it seems to me that mediumistic powers would best emerge where the conscious mind is allowed to sink back into passivity : this, I feel, is allowed with Mr Soal, whereas at Duke it is being constantly stimulated into activity : the results at Duke, I feel, are CONSCIOUS guessing—not the emergence of pure mediumistic powers.

The above description has been dictated to Mrs Goldney without preparation, and must be regarded as a spontaneous description of what I feel with regard to the difference in conditions of work for Mr Soal and Dr Rhine, and how I respond in my feelings to these conditions.

To sum up : the conditions at Duke are tense and emotional in comparison with those with Mr Soal in London. I, personally, prefer the quieter methods, divorced from constant urging and ' suggestion ', that pertain in London with Mr Soal : and I feel that my mediumistic powers would be given more chance to emerge in these quieter conditions. I shall be interested to find out whether this is so in fact, after hearing the result of my work at the end of the experiments.

(Signed) EILEEN J. GARRETT.
June 22, 1937.

Note by Mrs Goldney. The first paragraph (above) gives my request to Mrs Garrett. The rest has been taken down by me on my typewriter as it was spontaneously dictated by Mrs Garrett in her own words.

(Signed) K. M. GOLDNEY.
June 22, 1937.

REVIEWS

I

ERNEST HUNTER WRIGHT, *The Case for Telepathy. A Record of Some Remarkable Experiments. Harper's Magazine,* Nov. 1936 and Dec. 1936.

PROFESSOR CHESTER E. KELLOGG of McGill University, *New Evidence (?) for "Extra-Sensory Perception". The Scientific Monthly,* October 1937, Vol. XLV, pages 331-341.

In the first of his articles Professor Wright, who is chairman of the Department of English Literature at Columbia University, gives a lucid and popular account of the alleged evidence for telepathy and clairvoyance as set forth by Dr J. B. Rhine in his work *Extra-Sensory Perception.* Professor Wright, who is evidently a convert to the belief that Extra-Sensory Perception has been demonstrated by Dr Rhine and his associates, devotes much space in trying to convince the ordinary non-mathematical reader of the stupendous odds there are against the theory that Dr Rhine's results are the work of pure chance. Now, though there can be little doubt that Professor Wright is justified in his general conclusion that the scores of many series are due to something more than random variations in sampling, he makes no serious attempt to examine any of the complicated statistical problems that are involved. Such an attempt could perhaps scarcely be expected in an article written for a popular magazine, but the author might have said something of the unsatisfactory conditions under which many of Dr Rhine's experiments appear to have been carried out.

In his second article Professor Wright speculates as to the nature of Extra-Sensory Perception, but here again there is hardly anything that is not to be found in Dr Rhine's own book. Following Dr Rhine, the author concludes that clairvoyance cannot be ascribed to any form of radiation from the cards themselves which impinges on the brain of the percipient. Telepathy and clairvoyance are considered as different modes of operation of a single power and it is noted that when a subject has telepathic ability he is also found to succeed in clairvoyance experiments. To sum up, Professor Wright's articles contain hardly anything of interest to the serious student who is trying to obtain a proper perspective of Dr Rhine's work, but they may prove entertaining to the uninitiated.

Professor Kellogg's article, which was written as a reply to Professor Wright, merits, on the other hand, the most serious attention. It is, in the main, an able, if somewhat prejudiced, attempt to undermine Dr Rhine's position by attacking the validity of his statistical methods.

That Professor Kellogg is a little prejudiced against the idea of Extra-Sensory Perception is clear from certain of his opening paragraphs.

He writes (p. 332):

"Since Dr Rhine's reports have led to investigations in many other institutions, it might seem unnecessary to prick the bubble, as the truth eventually will out and the craze subside. But meanwhile the public is being misled, the energies of young men and women in their most vital years of professional training are being diverted into a side-issue, and funds expended that might instead support research into problems of real importance for human welfare. This has gone so far that a new *Journal of Para-psychology* has been founded. . . ."

It may well be of course that future research may show Dr Rhine's claims to be without foundation, but I certainly do not think that further experiments are unnecessary, as the result of Professor Kellogg's article. For while I am in agreement with a great deal that he says, I do not consider that he has disposed of Dr Rhine's case. The author's prejudice is further revealed by his eulogy of Dr J. E. Coover whose experiments with playing cards he describes as "a notable example of painstaking, thorough research, and exact treatment of numerical data." But as a matter of fact Coover's work is by no means free from serious objections. He tried to obtain a random sequence of cards by repeated shuffling and cutting of a single pack of 40 playing cards from which the 12 court cards had been removed. But, as most people know, a pack of cards cuts more easily at certain places than at others. Moreover, shuffling by hand is a very inefficient process and I have found that after several shuffles there are sequences that are not broken up. Cards often tend to stick together here and there. If, therefore, Coover's pack had a tendency to cut at such popular cards as the Ace of Spades, etc., the probability of a successful guess might be considerably greater than 1/40. Actually, as was pointed out by Professor Thouless[1] in a series of 10,000 guesses, the card was guessed correctly no fewer than 294 times giving a deviation from the mean chance expectation of +44. The odds against this result being due to chance are about 200 to 1. In a recent paper by Professor Cyril Burt (not yet published)

[1] *S.P.R. Proceedings*, Part 139, p. 27.

it is satisfactorily shown that the odds against the *whole* of Coover's results being the work of chance are of the order 50,000 to 1. If, therefore, Coover's results are not chance results we must conclude either that there was some experimental error in his method or that Extra-Sensory Perception was shown by certain of his subjects. My own suggestion is that Coover's method of obtaining a random distribution of cards was unsatisfactory for the reasons given above. The correct method is the one I have adopted in my own experiments, *i.e.* forming a random sequence of the digits 1-5 from mathematical tables. As was shown by Weldon in his famous experiments, even dice cannot be relied upon to give a random distribution.

But Coover never even troubled to discover the reason why his results were not in accordance with the laws of chance.

I am in hearty agreement with Professor Kellogg when he says that a large proportion of Rhine's experiments in Pure Clairvoyance are vitiated by the possibility of the subjects learning the cards from marks on their backs, slight flexes, etc. This is a very real source of error and should have been given far more careful attention by the experimenters. Personally, I have never exposed a naked card to a subject in the course of over 100,000 trials. Either a screen has been used or the card has been covered by a rectangle of white cardboard. Further, I have always made my subjects work through 40 different packs in succession.

It is also pointed out with perfect justice by Professor Kellogg that in experiments in so-called "Pure" telepathy, in which the agent thinks of a sequence of card images without using actual cards, the chance expectation is not accurately known. It is in such cases a mere assumption to say that in n guesses the mean chance expectation is $n/5$. There are altogether 5^{25} ways of choosing a sequence of 25 Zener cards. But both agent and subject may confine themselves to an infinitesimal fraction of these $2{\cdot}98 \times 10^{17}$ permutations. My own experiments show that certain types of sequences have scarcely any chance of appearing . The general tendency is to change from one symbol to another and such combinations as [○ ○ ○ + + +] may be absent for months at a time.

Last year two brothers, both highly intelligent, described to me remarkable results which they had obtained by using the above method. Detailed lists were not available but the scores per 25 were highly significant on the assumption of a chance expectation of 5. I immediately set the brothers to work, using a random sequence of real cards in place of the sequences constructed mentally.

The scores fell at once to chance expectation and have never risen above it in any significant sense. As the brothers did not use a screen

in their experiments at home it seems probable that sensory codes constructed unconsciously also played a part in the production of their apparently supernormal results.

Moreover, neither Dr Rhine nor Mr Tyrrell have published detailed lists of the card-images used in their experiments in Pure Telepathy, and as apparently no records are kept of such lists, the information necessary for criticism is not available. Further, as Professor Kellogg observes, Dr Rhine encouraged his agents to plan their mental sequences in groups of five. Now the effort to keep the numbers of each symbol about equal almost *inevitably* tends to cause the agent to choose about one of *each* symbol per set of five. . . .

Hence the success of the Fisk plan in the case when the subject is told when he has made a correct guess. If, on the other hand, the agent tries to avoid this pitfall he will find the greatest difficulty in distributing his total evenly over the five symbols.

The next important criticism that Professor Kellogg makes is that Dr Rhine (and the same observation applies to Mr Tyrrell) refuses to record and analyse the sum total of his records. If the subjects do badly on certain days, then it is said that the failure is due to some psychological disturbance and the results obtained on such days are ignored. But as obviously the statistical data themselves are the *only* criterion of the existence of an extra-chance factor we are, if we adopt such methods, moving in a vicious circle. Professor Kellogg argues that *either* all the data should be included in the analysis or that a *random* selection from the total data should be made and then analysed. I do not agree with Professor Kellogg in his suggestion of a *random* selection. It might well be that Extra-Sensory Perception is an intermittent faculty and it seems to me that a far better method would be to chop up the *whole series* into consecutive groups of 25 guesses or even groups of 5 guesses. Then, in the case of groups of 25, the numbers of scores of 0, 1, 2, 3, etc., successes should be compared with the terms of the expansion of $N[\frac{4}{5}+\frac{1}{5}]^{25}$ where N is the (large) number of groups of 25. Any extra-chance factor which operated unevenly on the different sets of 25 would, in the long run, be revealed by this method. If the method revealed no abnormal number of sixes, sevens, etc., the same method should be applied to groups of 5, using the expansion of $N[\frac{4}{5}+\frac{1}{5}]^{5}$.

We are now brought face to face with one of Professor Kellogg's main criticisms. As Dr Rhine, unfortunately, does not use a perfectly random sequence of cards he cannot in strictness apply the Binomial Distribution in the way suggested above. The packs used by Dr Rhine contain exactly five cards of each symbol. Now

even if the subject guessed exactly five cards of each symbol for every pack, the number of scores of 0, 1, 2, 3, etc., would not precisely agree with the numbers predicted by the expansion of

$$N[\tfrac{4}{5}+\tfrac{1}{5}]^{25}.$$

The true theoretical distribution can only be found by the methods of combinatory analysis and is given by Professor Kellogg in his article, though curiously he omits certain entries. The general effect is for the Binomial distribution to slightly underestimate both the number of *low* scores (*e.g.* 0, 1, etc.) and the number of high scores (*e.g.* 7, 8, 9, 10, etc.) while it overestimates scores near the mean (*e.g.* 5, etc.)

The differences however (*in the ideal case in which the subject guesses each symbol* 5 *times in* 25 *guesses*) are not very serious.

For example the number of sets with scores $\geqslant 7$ would according to the *true* distribution be nearly $N \times 0{\cdot}229$, whereas the Binomial distribution would predict $N \times 0{\cdot}220$ where N is the total number of sets of 25. Again the number of sets with scores $\geqslant 9$ would by the true distribution amount to $N \times 0{\cdot}051$, whereas the Binomial would predict $N \times {\cdot}047$ approximately.

Professor Kellogg's table, however, would appear to be of little practical use since subjects hardly ever trouble to guess exactly five of each symbol for each pack of 25. What is required is a solution to the more general problem in which the subject guesses n_1 circles, n_2 waves, etc., where $n_1+n_2 \cdots + \cdots n_5=25$. It is this general problem which I hope to discuss in the forthcoming report on my repetition of Dr Rhine's work. To take an example (which however would never occur in practice) suppose that, using Dr Rhine's packs, the subject says "○" at every guess. In this case all his scores per 25 will be exactly 5 and Professor Kellogg's distribution breaks down entirely. A case which is more likely to occur in actual experiments is where the guesser tends to avoid some one of the five symbols. He may, for example, dislike guessing the + since it reminds him of death. If he avoided the + altogether and used Dr Rhine's packs his scores would—*though only roughly*—be distributed according to the Binomial $N[\tfrac{3}{4}+\tfrac{1}{4}]^{20}$ instead of according to $N[\tfrac{4}{5}+\tfrac{1}{5}]^{25}$. This is not quite accurate but it gives a rough idea of the change that has taken place.

Yet for a large number n of individual guesses there would not be much error in taking the standard deviation as $\sqrt{n \times \tfrac{1}{5} \times \tfrac{4}{5}}$, *i.e.* $0{\cdot}4\sqrt{n}$ since this would now be replaced (approximately) by

$$\sqrt{\tfrac{4}{5}n \times \tfrac{1}{4} \times \tfrac{3}{4}} = 0{\cdot}387\sqrt{n}.$$

It is, I think, probable that Professor Kellogg greatly exaggerates the importance of the error arising from the use of the formula $\sqrt{n \times \frac{1}{5} \times \frac{4}{5}}$ employed by Dr Rhine throughout his work. A further slight error tending in the same direction arises from the use of the Normal Probability tables in connection with estimates of significance based on the Binomial Distribution. But here again the discrepancy is small when n the number of guesses is large, say 300.

Probably the most crucial series of experiments recorded by Dr Rhine are those described in the *Journal of Abnormal and Social Psychology*, Vol. XXXI, No. 2, July-September, 1936. These were carried out with Dr Pratt and the subject, Hubert Pearce, in different buildings of Duke University. The experiments were in Pure Clairvoyance and there are altogether 73 runs of 25, two such runs being made each day. The pack was shuffled after each run through and the records were " sealed after each sitting and delivered to me (Dr Rhine) before subject and observer got together."

Dr Rhine divides the series into four groups A, B, C, D. Groups A, C, D were made with the experimenter in the Physics Building and the subject in the library. The distance was about 100 yards. In group B the subject was still in the library, but the experimenter, Dr Pratt, was in the Medical Building, the distance being now about 250 yards.

In view of Professor Kellogg's criticism of these experiments it will be useful to give the scores here in full.

A : 3, 8, 5, 9, 10, 12, 11, 11, 12, 13, 13, 12.
B : 12, 10, 6, 4, 10, 10, 2, 6, 5, 12, 7, 5, 12, 11, 9, 10, 6, 3, 0, 13, 10, 12, 12, 4, 4, 1, 4, 4, 4, 7, 6, 5, 0, 6, 3, 11, 9, 9, 4, 8, 6, 0, 6.
C : 5, 4, 11, 8, 4, 9, 9, 8, 9, 10, 2, 7.
D : 12, 3, 10, 11, 10, 10.

In Group D Dr Rhine was with Dr Pratt as an observer.

In these remarkable series all sensory cues were of course ruled out and as Dr Rhine explicitly states (p. 221) that these four series represent *all* the tests given to Pearce with the cards in one building and the subject in another it cannot I think be fairly objected that they are a *selected* series. In fact, as we shall see later, a million persons might go on guessing for years without producing such a series.

Professor Kellogg in his article has not made it clear how he estimates the significance of these series. As he has obviously not used the formula employed by Rhine, *i.e.* $\sqrt{npq}$, to which he objects, we assume that he uses a method that does not presuppose a Binomial distribution. Such a method would be to calculate the observed mean and standard deviations from the scores themselves and assum-

ing—as we certainly may—that the theoretical mean expectation for 25 guesses is very approximately 5, then calculate the standard deviation of the mean from the formula $\sigma/\sqrt{N}$ where σ is the observed standard deviation and N is the number of sets in the group. In the case of small groups like A, C, D we should use instead "Student's" formula for "t". Such methods are of course crude as they ignore the true nature of the actual distribution, but the error is on the safe side. Now, of the whole series A, B, C, D which contains 73 sets of 25 (*i.e.* 1825 guesses) I find for the observed variance

$$\sigma^2 = 12{\cdot}90. \quad \text{Hence } \frac{\sigma}{\sqrt{73}} = \frac{3{\cdot}592}{\sqrt{73}} = 0{\cdot}42.$$

But the observed mean score is 7·52. Hence the deviation of the mean from 5·00 is 2·52, *i.e.* 6 times the Standard Deviation and the chance of getting a deviation lying outside the range $\pm 6 \times$ S.D. is about 10^{-9}.

If we use the ordinary Binomial formula

$$\sqrt{n \times \tfrac{1}{5} \times \tfrac{4}{5}} = \sqrt{1825 \times \tfrac{1}{5} \times \tfrac{4}{5}}$$

we find the deviation to be $10{\cdot}8 \times$ S.D.

Dr Kellogg, however, complains that this formula takes no account of the internal consistency of the scores. But we have seen that even using the other method we obtain a highly significant result. Yet all Professor Kellogg has to say about the series as a whole is: "Taking all the groups together, the results are positive and somewhat significant, perhaps sufficiently so to warrant further study of the problem."

Of Group "B" he writes: "Group A remains significant, Group B is just above the border line. . . ." But employing the same method for Group B I find

No. of sets of 25 $= 43$.
Mean Score $= 6{\cdot}70$.
Observed value of $\sigma = 3{\cdot}68$.

Hence
$$\frac{\sigma}{\sqrt{43}} = 0{\cdot}56.$$

Hence the deviation of the mean from 5·00 the expected value is 1·70, which is just over 3 times the standard deviation. The odds against such a deviation + or − being due to chance are about 370 to 1 and would be considered significant by most modern psychologists.

Professor Kellogg states that groups C and D are " well within the limits of ordinary chance accepted by scientists as not demanding special investigation and explanation."

Applying " Student's " method to Group C I find $t=2{\cdot}7$ which with $n=11$ gives $P=0{\cdot}02$ (nearly).

For Group D, I find $t=3{\cdot}30$ which with $n=5$ gives $P=0{\cdot}02$ (nearly).

It would appear that considered separately neither of the groups C and D is very significant by this method, but I have little doubt that the significance is seriously underestimated by using Student's formula. Moreover there is no reason to consider C and D separately since series A, C, D were done under practically the same conditions of distance.

If we applied the formula $\sqrt{150\times\frac{4}{5}\times\frac{1}{5}}$ to series D we should obtain a deviation amounting to over 6 times the S.D. I have never in all my experience met with a set of 6 consecutive groups of 25 which contained 5 sets with scores all $\geqslant 10$. In fact the greatest number of scores $\geqslant 10$ which I have obtained in 6 consecutive sets of 25 is 2.

In the 73 sets of 25 which comprise Groups A, B, C, D the number of sets with scores $\geqslant 7$ is predicted by the Binomial distribution to be 16·1 and by the distribution given by Kellogg 16·7. Actually, there are no less than 42 such scores. Now, although neither Kellogg's distribution nor the Binomial distribution represent the true distribution for reasons explained above, it is improbable that the expected number differs much from 17. We think, therefore, that Professor Kellogg's remarks are misleading; the whole series of 73 sets is stupendously significant. In fact, since $P=10^{-9}$ for the whole series it would take a million men doing each 1,825,000 guesses to win the same degree of significance.

Indeed, if the honesty of the investigators is not disputed this series would appear to establish the existence of a clairvoyant faculty in man. It is at the same time regrettable that Drs Rhine and Pratt, with the knowledge that they were getting epoch-making results under conditions beyond criticism, did not take every possible precaution to guard themselves against any suspicion of collusion. Under such circumstances my own procedure would have been as follows; I should have arranged for the head of another department of the University to be present in the Physics or Medical Buildings to witness and actually perform the shuffling of the pack and the recording of the actual card sequences. *I should* have *instructed him to take every precaution that I did not substitute a " prepared " pack for the one which he had himself shuffled.* The head of another department would have been waiting outside the Library to collect personally

Pearce's guesses immediately they were finished. The two heads of departments would then have met and compared the records. Since Pearce was alone in another building it could scarcely be urged that the presence of the second observer in the Physics Building could have affected his scoring, especially as the experiments were tests in clairvoyance.

A few concluding remarks about the four groups A, B, C, D may not be out of place. Group B, which is the largest, is an excessively variable set of scores. For instance there is one 13 and no less than three zeros. There are 288 successes in 1075 guesses so that the *observed* values of p, and q are $p=0{\cdot}2679$, $q=0{\cdot}7321$. On the assumption of a Binomial distribution the expected variance σ^2 with these values of p and q is given by $\sigma=2{\cdot}214$, whereas the *observed* variance $\sigma_1{}^2$ is given by $\sigma_1=3{\cdot}68$.

On the assumption of a Binomial distribution, the Standard error of the variance is given by

$$s=\sqrt{\frac{\mu_4-\mu_2{}^2}{N}}$$

where N is the number of sets and

$$\mu_2=25pq,$$
$$\mu_4=3\mu_2{}^2+\mu_2[1-6pq],$$

We thus find $s=\sqrt{\frac{47{\cdot}21}{N}}=\sqrt{\frac{47{\cdot}21}{43}}$

$$=1{\cdot}05.$$

But $\sigma_1{}^2 \quad =13{\cdot}549$, $\sigma^2=4{\cdot}903$.

Hence the deviation of the variance from its expected value is about $8{\cdot}6\times$ Standard Error. It will be seen, therefore, that Series B cannot be fitted to a Binomial distribution and we must assume that whatever extra-chance agency is at work operates very unevenly on the different sets of 25. In fact, if the agency is Extra-Sensory Perception we can only conclude that it works in a most fantastic manner, pulling some scores down to zero and raising others to 12 and 13.

S. G. SOAL.

II

Knowledge and Foreknowledge. The Symposia read at the Joint Session of the Aristotelian Society and the Mind Association at the University, Bristol. July 9th-11th. 1937. Harrison & Sons, London. Price 15s. net.

The parts of these symposia which are of special interest to students of psychical research are those contributed by Professors C. D. Broad and H. H. Price. They consist of a paper by Prof. Broad on the Philosophical Implications of Foreknowledge, a commentary thereon by Prof. Price, and Prof. Broad's reply thereto.

It is surely a fact of great significance that a body of men and women of such eminence in the world of philosophy as those assembled at the Joint Meeting of The Aristotelian Society and the Mind Association should have welcomed a discussion on a subject connected with psychical research. Though we have been able to count among our members some of the greatest of modern thinkers, the professional philosopher, as a general rule, has not been friendly towards our subject, even if he has not been openly antagonistic.

It may be said that his opposition need not be unduly feared, less so, indeed, than his apathy, but the friendly co-operation of the professional philosopher would be a great assistance. It would be of the highest advantage if all concepts employed, all hypotheses suggested, should be criticised from the standpoint of logic and philosophy, and none allowed standing room until they have been so " vetted ".

It is this function which Professor Broad undertook in his paper ; he did not examine the evidence for supernormal precognition, nor pass an opinion as to its reality ; but he first of all clarified the meaning of the terms in his own inimitable manner and then discussed what consequences for philosophy would follow if the alleged facts were actually true. He laid down that if it involves a logical or metaphysical impossibility, the evidence for precognition, however seemingly strong, must be rejected.

In this, though many would agree with him, there are doubtless some people who would demur. While, they might say, there is so little agreement among philosophers, even on such fundamentals as the self-evident truths, the claim that metaphysics and logic must be the final court of appeal cannot be admitted. There are many different systems of metaphysics and there will, in all probability, be many more in the future, nor is logic a completed science.

Moreover, metaphysics and logic themselves rest, in the last resort, upon experience, viz. the experience by which we recognise certain propositions to be self-evident ; from experience we cannot derive

necessary truth. When, therefore, logical and metaphysical impossibility is set over against the evidence for precognition, it is only one set of experiences, with inferences drawn therefrom, which is being opposed to another set.

Prof. Broad then examined the three main philosophical objections, viz. the epistemological, the causal and the fatalistic.

In discussing the first of these, he points out that the proper analogue of precognition is memory and not, as many have appeared to think, perception. In elaborating this point, he makes some observations on the subject of normal memory—as does also Prof. Price in his comments—which have value for psychology and philosophy in general, as well as for psychical research. By assimilating precognition to memory, he shows that the epistemological objection is not insuperable.

In this matter of memory both symposiasts agree in holding some form of the " trace " theory as being most probable. Prof. Broad, in his book, *The Mind and its Place in Nature* and in his Presidential Address, postulates traces of a psychical nature, while Prof. Price thinks that the traces are, at any rate partially, physical, that is to say, they involve modifications in the cerebro-neural structure. Trace theories have not met with general acceptance ; Russell has put forward an entirely different suggestion, and seeks to account for normal memory by postulating what he calls mnemic causation. This is a form of causation which operates across a time interval, it is able to jump the ditch. Should Russell's theory be accepted, it is, perhaps, a not very difficult step, from the epistemological point of view, to suppose that the jump can be made backwards, *i.e.* from future to present, as well as forwards, *i.e.* from past to present. Causal difficulties would, no doubt, arise, but mnemic causation is itself so odd and incomprehensible that a little extra oddity need not worry us unduly.

Concerning physical traces I would refer the reader to McDougall's *Modern Materialism*, Chap. IV, and Note 7.

Psychical traces, on the other hand, while avoiding some of the objections which can be brought against the physical theory, are entities of so unimaginable a kind that it is hard to say what they can or cannot do, and when or how they can be formed.

I suggest that until we know more concerning the real nature of normal memory, we cannot draw any safe conclusions from the analogue between it and precognition.

Prof. Price, in his comments, suggests that our conception of an event might be modified by postulating the existence of " *imagy* ", as well as " *sensible* " constituents. The sensible constituents are

those which give rise to sense perception and they exist only for the finite duration of the event, the imagy constituents give rise to images and they may exist both before and after the event. This is, admittedly, a somewhat "wild" hypothesis, but, as both Professors Broad and Price admit, one may be compelled to entertain wild hypotheses when confronted with supernormal facts.

I have made special mention of this suggestion because it seems to me that those who are inclined to accord to it a favourable reception might find therein a possible explanation of several otherwise refractory types of supernormal phenomena, *e.g.* hauntings and psychometry.

Prof. Broad's discussion of the causal objection reveals the full extent of the difficulties. The existence of non-inferential precognition appears to conflict with self-evident propositions concerning causation, but he suggests a possible escape by postulating a second dimension of time, which, even if a wild hypothesis, is not logically impossible.

Prof. Price, in his comments, makes a very valuable amendment by showing that many difficulties could be avoided by bringing in telepathy, and this leads Prof. Broad to an interesting discussion of that subject.

It seems to me that if this suggestion of telepathy combined with a second dimension of time be entertained, the difficulties arising from the linking of precognition with memory might be surmounted. In fact, it might be worth considering whether a novel theory of normal memory might not be constructed out of the same material.

The last, *i.e.* the fatalist objection, is shown, by an acute analysis, to be not insuperable.

The upshot of the whole discussion appears to be that, although acceptance of the reality of non-inferential precognition may commit one to the entertainment of wild hypotheses, there is no insuperable logical difficulty.

It must be admitted that these three papers are not easy reading, but for those who desire to go to the roots of the matter, a close study of them will be labour well spent. It is becoming increasingly clear that some overlapping between psychical research and metaphysics cannot be avoided, and those researchers who have the ambition to push their enquiries beyond the stage of the collection and classification of evidence must make up their minds, however reluctantly, to a certain amount of logical and metaphysical criticism.

H. F. Saltmarsh.

III

C. SPEARMAN, *Psychology down the Ages*. Macmillan & Co., Ltd. 1937. 2 Vols., 454 + 355 pp., 30s.

In his earlier books Professor Spearman has made distinguished contributions to modern experimental psychology. In *The Nature of Intelligence and the Principles of Cognition* (1923), he laid down a system of laws of the intellectual processes of the mind culminating in his three "noegenetic principles" which he regarded as particularly connected with the operation of intelligence or "general ability". These were: (1) *A person tends to know himself and items of his own experience*, (2) *On the presentation of two or more items, a person tends to know relations between them* (as, for example, when the presentation of the items of "heat" and "cold" produces the knowledge of the fact that one is the opposite of the other), and, (3) *On the presentation of an item, together with a relation, a person tends to conceive the correlative item* (as when the presentation of the item of "heat" and the relation of "oppositeness" produces the knowledge of the correlative item of "cold").

In *The Abilities of Man* (1927), he gave an account of the remarkable series of measurements in the field of educational psychology carried out by himself and his pupils following his earlier suggestion (1904) that the problem of whether there was a unitary general intelligence might be tested by examining the mathematical relationships between the correlation coefficients of different test results and school examinations.

The second volume of the present book is mainly concerned with these two lines of investigation. It shows how Spearman believes that the main facts in the intellectual and orectic field can be reduced to a system of laws, and the strength of the evidence for the proposition that there is a general intellectual ability (or *G*) which is the "intelligence" measured by intelligence tests and which determines, to a greater or lesser degree, intellectual abilities in all fields. The first volume is an account of the rival doctrines on such matters as "intelligence", "attention", etc., about which philosophers and psychologists have disputed since very early times. The main impression that one gets from the two volumes is of the way in which verbal disputes can lead to no final settlement of the problems of the mind until these are submitted to the test of crucial experiments. If this is, as I imagine it to be, the central purpose of the book, there can be no doubt that Spearman is demonstrating one of the most important principles in the development of scientific psychologists. Physics would still be a matter of verbal discussions on the nature

of " force " if it had not been redeemed from this state by the experiments of the early physicists. In the same way, psychological laws will only stand firm so far as they are founded on the rock of experimental evidence.

Spearman's theory of general ability is a brilliant example of the replacement of free speculation by experimental testing. It is not, however, the only one, and the author seems not always to be altogether just to experimental workers with whose conclusions he disagrees. The Gestalt psychologists, for example, have performed distinguished service in the experimental study of the field of perception, but the reader of chapter XXIV in vol. I with the unhappy title of " The Confusion that is Gestalt Psychology " might easily gather that Gestalt Psychology has been nothing but a system of speculations.

The title of these volumes might suggest that this is simply another historical account of psychology, of which many have been written during recent years. Valuable though the historical introduction in the first volume is, I think it is true to say that in these volumes Professor Spearman has done something more interesting and more worth while than merely to add another history of psychology.

Robert H. Thouless.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 155

A STUDY OF CERTAIN LEONARD PHENOMENA [1]

By Isabel Newton

Those who have had sittings with Mrs Leonard are doubtless familiar with the kind of impressions that are given through Feda [2] concerning the sitter and other persons—or it may be places—who, for one reason or another, are associated with the sitting. But as Mrs Leonard's sitters are limited in number, and there must be many members who have had no personal experience of her trance phenomena, I will explain that frequently at sittings the "communicator" claims to have visited some specified place, or person, and to have "picked up" certain ideas or "sensed" certain conditions during the visit. The term "impression" is used in the telepathic sense, namely, as denoting the effect produced on the mind or feeling of the percipient, the person who "perceives" otherwise than through the recognised channels of sense.

A considerable number of such impressions are scattered throughout the verbatim records of the Rev. W. S. Irving's sittings, which have been held at regular intervals since 1921, and of these, 228 refer to the S.P.R., its Officers, and the occupants of the Society's house. It is these particular impressions, together with six that were given at two of Mrs Allison's sittings with Mrs Leonard, that form the subject of this paper.

The purpose of the paper is not to put forward a reasoned case for telepathy; I am assuming that in these impressions we have evidence of telepathy, and that the examples illustrating the text will enable readers to form their own opinion on this point. My paper is

[1] This paper was read, slightly abridged, at a Private Meeting of the Society on March 30, 1938.

[2] Mrs Leonard's control.

an attempt to bring order into a confused collection of impressions, at first sight unconnected, fragmentary, and in no way distinctive; and to find some unity in them, which might throw light on the question why trivial and passing thoughts are so often perceived, while vivid and emotional experiences are apparently ignored. The examination of the impressions which relate to myself have suggested an explanation, which I shall put forward, tentatively, when I come to the "Isabel" section.

The impressions are given in series, and are referred either to "the Searchers", Feda's name for the Society, or to "Mrs Isabel", Feda's name for me. They are prefaced by such remarks as "She's been to the Searchers" (or, it may be, to "Mrs Isabel's"), "She's got a test from the Searchers' place", or "She wants to say something about the Searchers"; "She", in each case, being Mr Irving's communicator, "Dora". Then follows a series of impressions varying in number from two or three to as many as a dozen or so. The subject is then dismissed with "That's all about the Searchers", or "That's all about Mrs Isabel". Occasionally an impression is forgotten, remembered later in the sitting, and referred back to a previous context.[1] There is an example of this in the "Isabel" series, where not only is the impression referred back to a previous group of impressions, but apparently is related to a particular impression in the group, as will be seen when I quote the case in another connection.[2]

Mr Hubert Wales in his experiments with Miss Samuels (reported in *Proceedings*, Part LXXX, Vol. XXXI) failed to find evidence of any capacity on the part of the percipient to group various impressions relevant to a particular topic, so as to suggest the unity of any mind other than that of the agent. Now here, it seems to me that in this quite definite grouping of impressions we have evidence of such a capacity, though with regard not to a topic, but to a plan or purpose; otherwise we should expect to find the impressions given indiscriminately with the rest of the trance material. The purpose is, presumably, to give Mr Irving evidence of supernormal knowledge at his next sitting.

The two groupings are distinct and yet connected, "The Searchers" merging into "Mrs Isabel", and vice versâ; but there is no confusion between them. Throughout the series the "Isabel" impressions are personal, and (with three or four exceptions, which I call borderland cases) do not touch on my relation to the Society. In "The Searchers" series I am unrecognised, yet recognition of my connection with the Society is shown in such references as: "The

[1] e.g., *see* pp. 111-112. [2] p. 120.

Searchers' Isabel", and "I went to the Office, where the Searchers are, and she wasn't there, Mrs Isabel wasn't there".

Except in the six impressions given at Mrs Allison's sittings when Professor J. H. Hyslop purported to communicate, the ostensible percipient is Dora, Mr Irving's wife, who died nearly twenty years ago. (Perhaps I had better state here that the question whether it is really Dora or whether it is Mrs Leonard who is the percipient does not come within the range of this paper).[1]

I have said that the total number of impressions was 234. But on examination I found that of these a large number—57, to be exact—were given when either Mr or Mrs Dingwall (Mr Dingwall being then an Officer of the Society), or later Mr Besterman, was present as note-taker, and that they referred to events that were probably within the note-taker's knowledge. These cases furnish some points of interest in a complete study of the whole material, but as they do not come within the limits of this particular study I have eliminated them. We have, therefore, now a total of 177. of which 61 are associated by the percipient with the Society, and 116 with "Mrs Isabel".

Before giving examples, I should like to state again, as Mrs Salter did when quoting a few of the earliest impressions in her "Report on Sittings with Mrs Leonard" (*Proceedings*, Part 99, Vol. XXXVI), namely, that Mrs Leonard has never visited the Society's house at 31 Tavistock Square. (Nor has she ever visited me in my own home.) Nor have we any grounds for suspecting that there may be normal leakage of information about events in this house, though in a few cases it might, by chance, have been possible. Such cases are extremely rare.

I should also like to endorse Mrs Salter's acknowledgement, in her paper, of the Society's indebtedness to Mr Irving, not only for placing at its disposal all his records of sittings, much of which is of a private and intimate nature, but also for the great care and trouble he has taken to comply with its requirements. The accuracy of his records are all independently attested. His annotations were promptly made, and wherever possible he has obtained independent confirmation.

[1] I have not found one impression that could be quoted as helping to determine this question. When stripped of the Feda-Dora context the impressions are comparable with those of Miss Samuels, or so it seems to me; but this opinion is based on a few examples only, taken at random from Mr Wales's Report. The sense of *reality* produced by Feda's lively presentation of the impressions, and by the personality of Dora as therein revealed, is not easy to resist. I hope that Mr Kenneth Richmond will deal with this part of the material in his studies of communicator-personalities.

Dora is represented as coming to the house and "picking up" thoughts and ideas. With reference to her telepathic excursions in general, she says, "I always sense the place and not always when there's some one in it—you know, projected thoughts. I help myself to them." She frequently expresses uncertainty as to whether her percept is of a thought or of something that has been spoken of or discussed; also as to whether it refers to something that has already happened or to something that is anticipated. On this point, Feda observed on one occasion: "I found out before that Dora doesn't always know the difference between to-day and a quick tomorrow."

She does not know either the identity of the mind from which the thoughts are derived.

The majority of the impressions refer to small happenings in the office; the following will serve as examples:

> *January 25, 1927.*
> FEDA: "Is there some one new coming to work in the Psychical Searchers?" She got the feeling of some one new coming to work there.

Mr Irving's contemporary note is:

> Miss Sanderson had recently started work at the Rooms, during the absence of Miss Wallace.

In my confirmatory note I stated that Miss Sanderson began her work for the Society on January 15 (that is, ten days before the sitting).

Feda continued:

> "New arrangement! Now she doesn't know how to put this, for collecting the subscriptions. New arrangement about collecting the subscriptions being thought of in that room.

Mr Irving's contemporary note is:

> Miss Newton tells me that, a few days before the sitting, she advised Miss Horsell [the Assistant Secretary] to give to Miss Sanderson a certain work to do. It was to take over the noting of the receipt of subscriptions.

My own and Miss Horsell's confirmation is added to this note.

At the same sitting (*January 25, 1927*) an impression was given which suggests the identity of the agent by indicating the room with which he was associated:

> FEDA: She's got to show me this! Ask them if the top's come unstuck off something that they use in that room. Look! She got a mental vision of some one doing this (Feda closes her fists,

holds them close together, and twists the right one, slightly tapping the left with the right), and of saying " Isn't it a noosance! " Like trying to press.

Mr Irving's contemporary note is :

On Friday, January 28, 1927, I looked up Mr Dingwall in his room at the S.P.R. When I rapped at the door, he said, " Come in ", but I could not open the door. When leaving the room the same thing happened, I again could not open the door. Mr Dingwall showed me how to open it inside by pressing down the handle. His illustration was exactly like Feda's action, and reminded me of the test.

Mr Dingwall added the following contemporary note :

The door to my room at the S.P.R. is inclined to stick in wet weather. Lately it has been especially troublesome, and few people can open it. To open it from without the handle should be gripped with both hands and the handle then pressed down.

Feda continued :

Dora says " You remember last time I gave you a funny little test . . . the name Violet in connection with it? Well now, I don't want you to think it means the same thing, it doesn't . . . ask has some one had violets in this room, just lately, just now? And was there anything in connection with the violets about forgetting them, or nearly losing them? Forgetting seems the stronger impression.

Mr Dingwall's note is :

On Tuesday, January 25, 1927, I attended a sitting with the apport medium, Mr Mills Tanner ; violets were produced as apports, and with difficulty I managed to secure one. On coming home I looked in my pockets for the flower but had a sudden shock of disappointment when I could not find it. Later it turned up in an inner breast pocket of my jacket. Flower herewith.

This impression of the violets is one of the two impressions in the whole series which seem to have a precognitive element. I shall refer to it again when quoting the other case, in the " Isabel " series.

The next is one of the cases I had in mind when I said that normal leakage might have been possible. It will be noted that there is an association between it and the impression immediately following it.

May 6, 1930.

Feda : Will you ask the Psychical Searchers this? . . . whether they have got, either to-day, or tomorrow, a large quantity of MS. coming in, which must be dealt with? Not ours, not ours. No! But something coming in—like, in the next day or two—which seem to Dora to be . . . a heavy collection, something

that would necessitate, and has necessitated a great deal of work . . . That some part of this MS. would refer to Greek, Greek, The Greek, the Greek.

Mr Salter's [1] note, abridged, is as follows :

Mr Besterman and I attended the 4th International Congress for Psychical Research at Athens in April 1930. On the evening of the 22nd April a Business Meeting was held, at which, on behalf of the S.P.R., Mr Besterman and I undertook to have the *Transactions* edited in England. This Business Meeting was a comparatively small one, consisting only of official delegates from the different National Societies ; there may have been a round dozen of persons present.

On April 27, Mr Besterman and I left Athens by boat for Marseilles. He brought with him a considerable number of the papers read at the Congress . . . I travelled straight back from Marseilles to England. He broke his journey at Paris and stayed two or three days there. I doubt whether on the 6th May any persons other than my wife, Miss Newton, Mr Besterman and myself were aware that there was any suggestion that the *Transactions* of the Athens Conference were to be edited in England. The matter was brought up at the next Council Meeting on May 14, 1930.

Feda continued :

She says that she also felt that they have been ordering some new books. They may possibly have arrived by the time you enquire, but she knew they were ordered, do you see?

Mr. Besterman's note :

On my way back from Athens I spent two or three days in Paris buying books for the Society's Library. This was at the beginning of May, before the above sitting.

Although there was no diminution in the total number of impressions given during the years 1928–1936, those referring to the Searchers grew less and less frequent, the proportion during that period being only 21 (including 14 of the eliminated cases) to 90 in the " Isabel " series.

In July 1936 Mr Herbert [2] began to arrange a special book-test experiment, which Mr Irving broached to Feda at his sitting the same month. A collection of books, including one specially prepared, was placed in the séance room and Dora was encouraged to indicate the position of the special book. It was not until November 1936 that the experiment was fully understood by Feda and Dora, and they agreed to co-operate. At the next sitting, which was held on

[1] Mr W. H. Salter, Hon. Secretary of the Society.

[2] Research Officer of the Society.

January 19, 1937, impressions referring to the Searchers began again, and continued during the year. I will quote a few examples :

The first is a borderland case, probably partly " Mrs Isabel " and partly " the Searchers ". To make this point clearer I will first give my contemporary note, abridged :

> On Sunday, January 17, 1937, Professor and Mme Dessoir, of Berlin, who were visiting London, had tea with me in my flat. Professor Dessoir asked me if I had received the photograph of Mrs Sidgwick which he had asked a member of the Society, with whom he was in correspondence, to send to the Society. We had not received it. He then said he would send it to the office the next day, which he did. We received this photograph of Mrs Sidgwick during the morning of January 18, 1937, the day before the sitting, when the following impression was given :
>
> Feda : Mr Bill, will you please find out too, is there a new picture there that's important? Or, are they just getting a new picture, a portrait one? Dora felt the thought so strongly. Dora says, " I hope I've picked it up, the thought, before they've got the picture. I hope I've picked up the thought of the sender, or some one outside those rooms." She's a feeling this picture is connected with some one passed over, and that it would be a portrait. She picked that thought up pretty strongly. *She pretty sure about this.*

In this case, the idea that some one was going to send a portrait to the Society may have been picked up at my flat on Sunday. Dora confidently hopes she is giving information before it is known at the Society.

Later in the sitting, she again referred to it :

> " I'm very interested in that picture," she says, Mr Bill. She feels that that is, or should be, a good piece of evidence.

At the same sitting (*January 19*, 1937), Feda said :

> Anybody been falling there? Will you ask has any one fallen there, or been talking about a fall? Dora picked up the thought very strongly of a fall. Rather a nasty fall, not just a little slip or stumble, but a nasty fall, something that might have been quite serious.

As neither Miss Horsell nor I knew anything about a fall, Mr Irving and I asked Tolhurst (the Society's caretaker) if he knew of one, and he at once replied that Mrs Despard, our tenant's housekeeper, had had a nasty fall down the top flight of stairs. In Mrs Tolhurst's corroborative statement, she gives the time of the fall as " about 7.30 in the evening a day or two ago ". Her statement is

dated January 20. The accident occurred a few days before the sitting.

The impression immediately following the above refers also to an accident, but one which occurred a month before the sitting. This association of ideas (if not accidental) is interesting, as it suggests that the association lies in the percipient's mind, not in that of either of the agents, as neither knew of the other's accident.

> FEDA: Will you ask if any one has hurt their hand there, or been thinking about their hand: Not both hands, one. Ooo! as if they'd hold it like this, in the other hand (Feda illustrates: [here follows a rough sketch of two hands, one supporting the other]) and say, "Oh, dear!"

Miss Horsell's contemporary note is as follows:

> On December 19 I badly bruised my right-hand index finger when opening a french casement window. It ached considerably, and on my way home by train, which took about 1½ hours, I rested my right hand on my left with the index finger uppermost, as in the illustration, in order to ease it. I did this also at intervals later.

On *April 27, 1937*, an impression was given which suggests clairvoyance, the only case of the kind in the series. After representing Dora as being in the séance room in connection with the special book-test, Feda said:

> No, Dora! I don't think that would be right! Not bricks! Oh dear! it can't be right, can it, Mr Bill? Any bricks there, bricks? Got such a strong feeling of bricks, or pieces of brick. Dora says, "Of course, I know there's a wall there—it isn't that!" She says "You see my point? I might say in this room I'm reminded of bricks. It isn't that." Lots of houses is made of marble! Dora says, "Never mind! Leave it as I've said it. I'm reminded of bricks, in this position, in rather a peculiar way. I'm sensing bricks in an important way." That's right, Mr Bill. "Bricks."

My note is:

> In the corner of the room are 4 pieces of red rubber sponge ($5\frac{3}{4}'' \times 3\frac{1}{2}'' \times 1\frac{1}{2}''$), which look like bricks. Mr Herbert says he is not conscious of ever having thought that these sponges resembled bricks, until he went into the séance room to verify the impression, when he immediately saw the resemblance. I have no recollection of having noticed the sponges, but I am in and out of the séance room and must have seen them.

From towards the end of June Miss Horsell was away on sick leave for three months, and I took her place in the office. At Mr Irving's

next sitting, *i.e.* on *July 22, 1937*, eight impressions were given, in seven of which I was the ostensible agent, and yet no knowledge of a connection with " Mrs Isabel " was shown. I will quote one or two of these impressions. The first is interesting, as showing that an idea may be transferred from its original setting to a setting that is associated with it. To make the point clearer I will again read my contemporary annotation first :

> Some time between July 6–14, 1937, I lost three keys on a ring, one of which was the front door key. The loss of the other two did not matter, but that of the front door was inconvenient as Tolhurst had to come up from the basement every time to let me in. I felt that the keys were not really lost. I used the term " temporarily lost " both to myself when thinking of them and to Tolhurst when speaking of them.
> After a few days I concluded I had left them in the séance room when Mr Herbert and I sealed it up on July 6 for the book-test experiment. As the room was not to be opened until the end of the month I had a new key made ; the bill for this is dated July 14.

Feda said, at the sitting on *July 22* :

> She's sorry they've lost the key. The key of something been missing lately . . . It was only lost temporarily . . . but it was lost. This is important. It wasn't gone, it was mislaid. She said " key ", but I've got a feeling, though she said " key ", there are other keys connected or linked up with it in some way, and the word " keys " was mentioned as well as " key ". That's right.

Later, during the same sitting, when " sensing " conditions in the séance room, Feda said :

> " Why am I getting something about " keys " again here? " Just put that down, Mr Bill. Have you got that about keys? I'm making that remark 'cos Dora asked me to.

Here the idea that was transferred was a mistaken idea, because the keys were not, in fact, in the séance room. They were found some days later elsewhere.

In the next impression which I shall quote the percipient is credited with a capacity to associate ideas derived from two different sources, and to be reminded, by this association, of an incident that had occurred in the office. The impression was given on *July 22, 1937*, during the part of the sitting when Dora was represented as trying to find the test-book. Referring to a vaguely indicated book, Feda said :

> She got a strange feeling of finance . . . Will you see if there are sums of money written on it, or printed on it, either on the outside, or just inside? She kept getting finance, mentions of money. 'Cos

> that reminds her of something she was going to get through, and she's glad it does. Has there been in the Searchers' office something rather puzzling about money, Mr Bill? About a sum of money: something that's puzzled them rather, something that has been very difficult to explain, or account for, do you see? I think that's right, Mr Bill. She meant to say that among the first things she said about the Searchers, but she couldn't get it through without the book. The law of association, we work on that a great deal. We often have to give ourselves a clue by mentioning something that opens the way.

It is a pity that the impression did not relate to something more striking, for it may be supposed that small financial difficulties are of frequent occurrence in the office. But Miss Horsell, who has been in charge of the Society's office for a considerable number of years, says that this is not so. She denies that the impression would apply, for example, at the present time. She told me of one incident which she thought it would have fitted, but this, she said, occurred " three or four years ago ". It would appear, therefore, that " something puzzling about a sum of money . . . that has been difficult to explain, or account for " is not an everyday occurrence in the office.

The following is my contemporary note on the impression:

> On *July 12, 1937* [the sitting was held on July 22] I paid into our Banking account a cheque for £3 3s. 0d. which I had cashed from Petty Cash a day or two before for a member of the Society. I expected to be able to adjust the matter in a few days by receiving cash in the office for a subscription or publications, but it happened that nothing came in until about July 23; the note of final adjustment in the paying-in book is dated July 24. In the meantime I was very puzzled as to how to make the adjustment [before we closed for the vacation on July 31].

In view of the coincidence in time, coupled with the infrequency of the incident, I think we may reasonably assume a connection with Feda's reference. And this assumption is strengthened by the fact that the impression is the only one of the kind in the complete collection.

It was not until December 1937 that Mr Herbert and I attempted to verify Feda's allusion to a book in which finance and sums of money were mentioned. It was merely a matter of routine, for as no definite indication of the position of the book was given, and the probability of finding some reference to finance in a collection of seventy books was great, a " verification " could be of no evidential value.

As it happened, the only reference we succeeded in finding was a 12 pp. pamphlet consisting entirely of prices and code words for

cabling. This pamphlet was inside the cover of a "Catalogue of Optical and Scientific Instruments", down the spine of which is printed, in large black capitals, *W. Watson & Sons, Limited.* When noting the details, Mr Herbert seemed to remember that the name *Watson* had been mentioned in connection with a later attempt to detect the test-book, and on looking through the record of the sitting held on *September 20, 1937* (*i.e.* the one next in succession to the sitting of *July 20*, when the reference to finance was made), we found the following allusion :

> Feda : See if, on the outside of a book, there's a name like Watts, or Watson? This is all round the test-book.

This mention of this distinctive feature of the book containing the pamphlet is interesting, but it is difficult to estimate its significance. We may surmise that on the first occasion Feda's account was imperfect, or that Dora's perception of the book was only partial, and that on the second occasion the omission was rectified. But there is no *evidence* of a connection between the two allusions.

Feda's statement that the book bearing the name *Watson* was near the test-book was incorrect.

Mr Herbert's subliminal mind may be the source of the *Watson* impression. It was he who selected the books and placed them in the séance room. He may have retained a memory subliminally of the pamphlet inside the *Watson* catalogue, and associated it subliminally with the reference to finance and prices. He did not know, however, of the incident in the office which was said to be associated with the idea of finance.

I will conclude this series of examples of "the Searchers" impressions with one which also suggests a subliminal derivation. It was given suddenly during the sitting on *July 22, 1937*, with reference to the séance room.

Feda said with apparent irrelevance :

> Look! You would say that was in the city, in the town! But, she says, has there been any vines there? Such a strong feeling of vines, as if vines once growing there. Will you find out. Dora says, "As well as providing evidence, I do like to find out why I get a thing, especially an extraordinary thing like that. Oh, that came so strongly about the vines, so strongly, as if vines had once been a feature of this place. Isn't it strange!"

My contemporary note is as follows :

> I had a very strong feeling when I read this extract that there used to be a hop plant growing where the séance room is now. I do not know how we can find out whether my impression is correct.

Miss Horsell was the only likely person to know, as she was with the Society when the séance room was built in 1923. She, however, had no recollection of the matter, and finding that my impression was growing more and more uncertain, I went, on December 21, 1937, to Balham, and called on Miss Davis, who had lived for a long time in the house as housekeeper to the previous tenant, Sir Ernest Clarke. She remembered the hop plant. She referred to it as " a big hop tree. They cut it down, when they built the new room." She told me that I had shown her the room one day when she called at the house after Sir Ernest Clarke's death, and that she noticed that the " hop tree " was gone. This must have been at least ten or twelve years ago. It will be noted that my knowledge concerning the hop tree was, at the time of the sitting, subconscious.

I will now pass on to the " Isabel " series. At this point it may be useful to state the extent of my acquaintance with Mrs Leonard. I have never had a sitting with her. I have been present at two sittings as note-taker : in 1918 and 1924. Apart from these two occasions, I do not think I have met Mrs Leonard to speak to more than three or four times. The last time was in 1932. I have never discussed her mediumship with her, nor intimated to her that any one of her communicators was interested in the Society or in me. The persons with whom the " Isabel " impressions are chiefly concerned are not interested in psychical research, and are not associated with Mrs Leonard nor with Mr Irving. I am not aware of any channel by which information regarding my personal affairs could normally have reached them.

The first impression—the thirty-third of the entire series—was given at Mrs Allison's sitting on June 11, 1924. As it is included in Mrs Salter's paper, I will not quote it here. At a sitting in September the same year Feda was asked by Mr Irving to visit my flat, and at a sitting two days later she claimed to have done this and to have picked up certain impressions. These are also included in Mrs Salter's paper. After that, occasional " Isabel " impressions were given until 1926, when they became more and more frequent ; during the years 1928–1936 they completely outnumbered the Searchers references, as we have seen.

It will be easier to follow this part of my paper, I think, if I state at the outset that my examination of the " Isabel " impressions has led me to believe that they are " of the stuff that dreams are made of ". I first suspected this when considering the following two very trivial impressions which were given with others, but not consecutively, on *January 24, 1935*. Feda said :

1. She thought Mrs Isabel had wanted a new cloth, a new cloth to cover the table. She didn't really want to cover the table . . . but she thought she'd better . . .
2. The name Fanny is connected with Mrs Isabel just now. Something been happening in her conditions that's a link up with Fanny and the past, thing of some time ago and the name Fanny.

My contemporary annotations are as follows :

1. On the 21st of January when I was preparing for afternoon tea for a visitor, I found that my small stock of tea cloths were shabby and soiled, and it passed through my mind that I would put no cloth on the table. However, disapproving of the appearance of the tray on the bare table, I put a cloth on, after all.
2. I know only one Fanny, Miss Fanny Lea, who lived some years ago in the house where I still live. I met Miss Lea in the Plane Tree Restaurant on January 21 when buying cakes for my visitor. (See above), and we both exclaimed how long ago it was since we had met . . .

It puzzled me why two such very trivial incidents as these should have been perceived. There was an interval of about an hour and a half between my laying the cloth and my meeting with Miss Lea. During that time I had lunched with a friend, and we had had an interesting conversation, yet no idea associated with it had been picked up.

It will be noted that the two incidents, though separated in time, were connected with one subject, namely, preparations for a visitor. The visitor was one of two people who were passing through a very emotional experience, with which I had been closely associated during the preceding three months. These incidents, it seemed to me, were on the fringe of my reactions to all this emotion : a point of ingress to a preoccupation [1] in my mind.

Here is another trivial impression :

September 23, 1926. Dora's reminded me of something that has got to do with Mrs Isabel . . . Will you ask her if she was . . . dressing something up, like you might dress a doll up, dressing, dressing something up ? Dora got this as well as Feda . . .

On August 7 [a few weeks before the sitting] I went on a cruise with a girl whom I will here call A. The cruise was a great disappointment to A. ; in fact, several things happened which caused her rather acute mental suffering. Towards the end of the cruise a Fancy Dress Dance was arranged and I persuaded A., who by this time had become averse to taking part in any festivities, to let me

[1] I use the term preoccupation in the sense of something that permanently occupies a field of interest at an unconscious, or preconscious, level.

dress her up. Two other girls and two children asked me to dress them too, so that I was busy all day rigging up things for the evening. (I may say that this is my only experience of the kind.)

Now, my most vivid memory of that occasion is of A., when the supper gong sounded, asking me not to leave her. She and I were sitting alone on the deck until two of our fellow-cruisers joined us, and I slipped away under cover of their presence, to pick up in A.'s cabin and to throw into the sea every vestige of material connected with her fancy dress. In short, the occasion of the dressing-up was a culminating episode in a series of emotional upsets for A., and of my reactions to them. Here again, it seems to me, there is a point of ingress to a preoccupation in my mind.

There are several impressions which I suspect are connected fundamentally with a long-standing preoccupation with the affairs of someone whom I will call M., a relative of mine who, in my opinion, has never had a fair chance in life. Here is an example of an apparently indirect connection :

> *January 27, 1931* . . . Ask Mrs Isabel has she been wanting something, well, rather out of the ordinary to wear, and has she been thinking of it in this room? [My sitting room] I got the feeling of— " I know . . . it sounds most unlikely, a *print dress.*" (Here the direct voice said *winter.*) She says, " I know it's the winter . . . I know no one would buy or plan for a print dress now, yet that is what I got."

Feda continued :

> Also has she been thinking of a piece of glass? Not a looking-glass, not quite a complete thing in itself, not an ordinary thing. I felt that I must call it a piece of glass, because it would represent only a part of the entire article."

My contemporary notes are :

> In the summer I bought from an art shop in Burford a gay and striking gardening apron. In December I bought several pieces of printed cotton cretonne to make similar aprons, and I made several of these for Christmas presents during the week or two before Christmas.
>
> I cannot think of anything in connection with the second impression except one thing, which occurred however six or seven months before the sitting. I wanted a piece of bevelled plate glass for the wall behind a washstand in M.'s cottage. I took the measurements, and made several enquiries in London for such an article. Finally, I gave up the idea, on account of the expense of getting it to the small village in the Cotswolds where M. lives.
>
> The description : " not a looking-glass, not quite a complete thing

in itself, not an ordinary thing (it would have to have been made) a piece of glass representing only a part of the entire article" is true of what I had in mind.

In these two impressions, which were given consecutively, there is an association of ideas such as we get in dreams. I was making the print into aprons like one I bought at Burford, when I was staying at M.'s cottage, for which I wanted a piece of glass. The contemporary notes show a correspondence in time between the associated incidents.

Many of the impressions suggest this dream-like association of ideas. Here is one connected with dressmaking, which is apparently another of my preoccupations, of which I shall have something to say later.

July 21, 1932. Dora felt Mrs Isabel holding something in her hands, strips, narrow strips of rather fine, soft material, narrow strips. As if she was mentally measuring it. Stuff seemed to be folded in a rather peculiar manner. Wait, Dora, I know what you're going to say. "Or else she held it in this way." Look, Mr Bill! It wasn't rolled round (Feda illustrates winding something, as if cotton round a reel) but it seemed to be folded like in long loops and Mrs Isabel, look! was holding these loops, pulling them out in her hands (Feda first illustrates throwing something upwards in loops, and then pulling something outward from her hands). Very soft, Dora thinks, narrow. Something between $\frac{1}{2}$ in. and 1 ineh wide . . . And then she seemed to be thinking of some black embroidery. Dora says, "I call it embroidery, I don't know what else to call it . . . fine again, patterned, patterned, patterned."

My contemporary note is :

At about 6.30 p.m. on the day before Mr Irving's sitting I machined the hem of a long narrow strip of dark blue silk marocain, $2\frac{1}{2}$ in. wide, $8\frac{1}{2}$ yards long. When Mr Irving read the extract to me he made certain movements illustrating Feda's, and I recognised them as resembling my own movements when machining this strip. Hanging from the machine to the floor it had got twisted, and at intervals of about three-quarters of a yard I tossed it up to get a straight piece, which I then held out before me while machining it. I mentally divided it into eights, quarters and so on, as I went along . . . The upper part and the sleeves of the dress for which the strip was intended is made of fine patterned material . . . I may add that the machined strip was in my attaché case at the S.P.R. when Mr Irving was reading the extract, and I thereupon showed it to him and to his sister."

Immediately following the above impression was this one :

> Mrs Isabel was also thinking about how dangerous it was of . . . something she wore on her neck coming undone. It has come undone two or three times. Dora thought " She'll lose it if she doesn't have something done to it " . . . Dora gives me letter L connected with it. I got a feeling that L may be more to do with the place that the jewel came from . . .
>
> In my contemporary note I say : that the knotted cord of a necklace of imitation old ivory beads had broken about a fortnight or so before the sitting, that I had then worn it with the ends loosely tied together, and on two occasions when out of doors I had found they had come undone and I thought I should lose it if I did not get it properly restrung. I had bought it at *Liberty's*.

It was one of two necklaces only that I had bought at Liberty's. The other one I bought to wear with the dress referred to in the previous impression. The dress was made for a special occasion a year before (the machined strip was for a pleating to lengthen it) and I had then a good deal of trouble to find a necklace that would tone with it before I saw the one at Liberty's.

The next impression is one in which two single ideas are connected without perception of any associative link.

> *April 30, 1931.* [Dora was represented as taking a book-test from a certain bookcase in my sitting room.]
> FEDA : Anything Dutch here, Dutch, something Dutch? Got such a strong feeling of Dutch. Yes, wasn't very interested in it, but just got the thought of it as she was standing there. Must be something there, she thinks. How funny! She says, " Of course, it sounds ridiculous, as I stood there two thoughts came to me, seemed far apart, both mundane. Tennis, tennis. Wonder if there's anything connected with tennis put there, where I'm standing by these books." She says, " Just ask about that." Very puzzled! Looked round! Something there, felt sure something there, connected with tennis. Something Dutch there, and something connected with playing tennis close there . . .

This is my contemporary note :

> Standing on the top of the bookcase . . . is a small colour print reproduction of a Dutch picture which was bought at the Exhibition of Dutch Pictures in London in 1929. Until July 1930 a large framed photograph of my niece was propped against the wall by the side of this Dutch picture. In July it was moved to the other end of the bookcase near the window, where it has remained ever since. The only suggestion of tennis in my flat is my association of tennis with this niece. She is a champion player and has won many tournaments, and during the tennis season I have every summer followed her progress in the reports of tournaments in

" The Times ". She was in the Wimbledon Tournament one year. I have no other interest in tennis.

I omitted to state in the above note that my niece was wearing evening dress in the photograph ; there was, therefore, no suggestion of tennis in it.

The Dutch picture was given to me by Miss Dutton of Sidmouth. My niece played in the tournaments at Frinton, Budleigh Salterton, and Sidmouth. I had never been to Frinton, nor to Budleigh Salterton, but I had stayed several times at Sidmouth with Miss Dutton, and I had seen the tennis courts there. I had also remarked to Miss Dutton that I was sorry she was always away in August when Edna was in Sidmouth for the tournament. In my mind there was an association between the Dutch picture and playing tennis, although at the time I did not recognise it.

The next two impressions appear to be precognitive. They were given at Mrs Allison's sitting on *Monday afternoon, June 7, 1926*, and are associated with unimportant activities of mine during the following day. The scene is again M.'s cottage. She was then living near Lynton.

In the contemporary note to the first of the four impressions given at this sitting I explain that for certain reasons connected with some special work in the office, it would be inconvenient for me to take the whole of my holiday in August as usual, and that the best arrangement seemed to be that I should take one week from June 8, and the rest later. I also state that this arrangement was not made until June 2. It will be understood from this that my being away from London at that particular time was unusual. The sitter, however, knew that I was away.

At a sitting at which Professor Hyslop was purporting to communicate, on *Monday afternoon, June 7, 1926*, Feda said :

> He has seen her lately in a small and pretty garden not as big as Gladys's, and she was standing at the side of a path and some plants, she was looking at some plants . . . that were blown down, beaten down with rain instead of standing straight, that were lying flat, and she was standing looking at them.

Feda continued :

> Ask her if she wanted some nails recently. He's laughing. Nails. She was wanting them for a special purpose. Wishing she got some nails. I think all this happened quite lately, that's why he's giving it.

My contemporary annotations are as follows (I will reverse the order of them) :

> I rarely want nails. At the time of the sitting I had not used any for a long time—in fact, I cannot remember when I wanted any before Tuesday, the day after the sitting. On Tuesday morning, with the help of a man I put up a summer house, which had been delivered that morning in sections at M.'s cottage. A packet of screws, bolts and very long nails had also been sent. Before we had finished I wanted some short nails.

In M.'s confirmatory note she says the summer-house was delivered about 10 a.m. on Tuesday and that I began to put it up soon after; and that between 2.30-3.30 I went in and collected all the nails she had. Also, that I wanted more nails, and on the Friday afternoon, June 11, we went into Lynton, and I went off and bought nails and joined her later.

With regard to the impression of the garden and of my looking down at plants beaten down by rain: at the time of the sitting it had not rained. There had been a spell of fine weather, which did not break until 6 p.m. on Tuesday, and during the rest of the week there was a good deal of heavy rain. On Tuesday afternoon I had made a bed each side of the new summer-house and transplanted there some ferns, fuchsias, and rose trees, and several times during the evening and the next day or two I went out after heavy rain purposely to see if the plants had suffered. I never found them lying flat.

During the week-end preceding Mr Irving's most recent sitting, on *January 18, 1938*, I was examining the material for this paper, and I carefully considered the impression concerning my wanting nails. I considered its evidential value, from (*a*) the frequency of the references to nails in the entire collection, and (*b*) the frequency of my need for nails. As to (*a*), I found two previous references to nails (they are both in the eliminated cases and were unrecognised). As to (*b*), I thought: "When have I wanted nails before, or since? I never use them. I don't put up shelves, nor make boxes, nor hammer nails in anything. I *never* want nails, except now and then a tintack to hang up a Calendar."

At Mr. Irving's sitting a few days later, Feda said:

> "Will you please ask Mrs Isabel if she was sorry, quite recently, she wasn't a carpenter."

And later in the sitting:

> "Dead violets. Ask Mrs Isabel if she's been thinking of some dead violets lately? Or have there been some dead violets round her? She says 'Dead ones'. She forgot about that. She ought not to have brought it in here, but she forgot to mention it."

I had considered the " violet " impression (included in the Searchers series) at the same time as the one referring to the nails. They were associated in my mind as the only impressions that seemed to be precognitive. The percipient indicates that the latter impression was omitted at some previous point in the sitting. She had forgotten to mention it—probably when she referred to my not being a carpenter.

I suspect that " dead violets " is a dream-like merging of two cases into one. I had selected an impression concerning *dead flowers* for quotation in the Searchers series, before I noticed that it belonged to the eliminated group. Both the " violet " and the " dead flowers " impressions were associated with Mr Dingwall.

In the autumn of 1932 my preoccupation with M.'s affairs became more concentrated, as in 1931 she had lost more than half of her small means and it had become necessary for her and the friend with whom she lives to do something to increase their income. This was not possible in the small village in the Cotswolds where they were living and they took steps to find a more suitable place. An appropriate reference to their situation, which I will not quote, was made at Mr Irving's sitting on April 30, 1931, about three months after M.'s loss of income.

Although they were in touch with house agents in different parts of the country, M. and her friend could hear of nothing suitable and by the autumn of 1932, when the lease of the cottage was nearing its termination, they lost their nerve. It seemed as if all the small properties had been snapped up during the financial depression of 1932.

At a sitting on *September 22, 1932*, Feda said :

> Will you ask Mrs Isabel whether she's been thinking, quite lately, like the last day or two, about helping some people to make a move? " I don't think she was making the move herself," Dora says, " but I do think that she's been helping some one that she's very interested in to make one, or to commence making one."

My contemporary note runs :

> This is true. A few days before the sitting I began definitely to take steps to help M. and the friend with whom she lives to find a suitable place and a suitable house to remove to.

About the middle of November (that is, about eight weeks later) M. and I bought a small plot of land in a place in Dorset which she thought would be suitable for her purpose, and we engaged a builder to put up a small house. The negotiations and arrangements were in my hands.

Two months later, at the sitting on January 26, 1933, Feda said :

> You ask her if she had a long envelope? She took out from the envelope what Dora would call a wad, a wad of papers, wad. And kept unfolding first one and then another. Dora thinks the papers were not all of one colour, not all white, not all the same size. And she seemed to be separating them, spreading them out. Look, Mr Bill! with her hands like that. (Feda illustrates smoothing something on a table with her hands wide open, palms underneath.) As if she wanted to get them flat, she thought. There was something important that Mrs Isabel had to think out with regard to figures or numbers, in connection with them.

My contemporary note is :

> This is correct. During the evening of January 25 (*i.e.* the evening before the sitting) I took from a long envelope a roll of letters and papers relating to a family business [the building of the house]. I laid each one flat on the table, putting them first in chronological order, and then selecting those that I should want on the following day. When Mr Irving read the extract I recognised the reference but denied that there were any but white papers. When I went home, however, and looked again at the papers in the envelope, I found an orange-coloured circular and a yellow one (time tables of day excursions and bus routes in connection with the business matter).

The papers were specifications and letters relating to the building of the house. I was going the next day to Dorset to meet the builder, who had written a few days previously to ask me for instructions regarding the roof, whether it was to be tiled or slated.

The impression immediately following the above reflects the opinion of one or two friends who knew of the house-building project ; an opinion I probably accepted though repudiating it to myself.

> She's neglecting something, Mrs Isabel is, that has a bearing on herself . . . she's neglecting her own interests, with regard to somebody and a place rather a long way off . . . There's a name beginning with a G connected with this condition. And a second name that would seem to begin with a C, Dora thinks a hard C.

My annotation seems to reflect some annoyance :

> . . . The statement in the notes is too vague to identify definitely. I do not know to what G refers, nor the rest of the extract.

In the light of dream association I now suggest that the hard C might refer to the Cotswolds, and the G to Gloucester. Gloucester was a necessary part of the address of M.'s cottage.

> Feda : Would you see roofs from this room? Don't think that's right, Dora. " Well," Dora says, " Mrs Isabel has been thinking very much about roofs." Dora isn't sure whether you see roofs from the window of this room, or whether Mrs Isabel has been mentally visualising roofs in this room [*i.e.* my sitting room, where I had spread out the papers taken from the long envelope].

My note is :

> For some days and especially the day of the sitting my mind was occupied with the question of roof materials. Miss Horsell, who knows the circumstances, can corroborate.

After receiving the builder's letter a few days before the sitting, asking for instructions in regard to the roof of the new house, I went to the Building Centre, in New Bond Street, and saw many kinds of tiles and made enquiries as to their durability and price.

The next reference to M.'s affairs was given three months later, at a sitting on *April 25, 1933*. Shortly before this date something had occurred which made me think that the house project was a mistake, and I had lost heart for the time being. I was convalescing in a furnished cottage on the Quantock Hills after a serious illness, and the greater part of my time was spent in my bedroom there. The impression showed concern for my condition of mind, and a certain discrimination in relation to the sense of defeat I was experiencing, which was rather impressive. I will not read this impression ; I mention it, as there is an interesting point in connection with it, to which I will refer later.

In the last week of July 1933, M. and her friend moved into the new house, which has proved a completely successful venture.

There was no further reference to my preoccupation with this matter until a year later, when on July 26, 1934, an indirect reference was made to the financial side of it, which I will not quote.

The final reference was given on January 24, 1935, when Feda said :

> Is this a joke? I think it must be, it doesn't sound right. " We can't put our hands in a lucky bag and draw out the right number." Oh! it's an answer to a question. I think I got it right, Mr Bill. It's an answer to a question. " We can't put our hands in a lucky bag and pick out the right number." It sounds awful mixed up. She says, " I want it just in those words. In other words, we do *not* help people in *games* of chance, or anything of that kind . . . It's one for Mrs Isabel.

My contemporary note is as follows :

> Still owing money on M.'s house . . . and having a cousin who won about £50 by correctly solving a cross-word puzzle in a com-

> petition, I have tried to hurry on the payment of my debt by taking part in the same weekly competition. No skill is required : it is purely a matter of chance, the clues being simple but presenting a choice of alternatives, to which there is no clue to the one intended. It has occurred to me more than once that it would be nice if Dora and Feda, who say on many occasions that they are helping me, would guide me to the right choice in the matter of the alternatives.

We have seen that in the Scarchers series the thoughts were picked up in the house, and I have assumed that in the " Isabel " series they have been picked up in my flat, which Dora and Feda profess to have visited. The impression expressing concern at my having lost heart about M.'s affairs was picked up in the cottage on the Quantock Hills, for I had been living there since the beginning of April 1933 and the impression was given on April 25, 1933. There were two other impressions given with it, both of them associated with the same room, my bedroom. I will quote one of them :

> FEDA : The fireplace in this room is said to be in an inconvenient position. The fireplace has been grumbled about. " Great pity it was there. Ought to be in another position." I feel quite near the fireplace . . . there's no room to put something that they want to put in between . . . this is connected with Mrs Isabel, a room of Mrs Isabel's. I want to say something. You know, Dora's taken me to Mrs Isabel's before. I don't know if she's changed anything, but it feels different. When I said that Dora nodded her head, and she said, " Yes, there's a reason for you feeling like that, Feda, though I cannot explain."

In my annotation I say :

> I have never thought that the fireplace in either room in my flat was in an inconvenient position.

Then, referring to my room in the cottage on the Quantocks, I say :

> The fireplace was so inconveniently placed that it was impossible to pass between it and the foot of the bed comfortably ; in fact, the position of the fireplace was a bar to every suggestion my sister made for a more convenient arrangement of the furniture.

There is an indication in this impression that both Feda and Dora detected a difference in my environment but could not explain it. It is difficult to imagine the process by which these impressions, picked up in a bedroom in a cottage on the Quantock Hills, were perceived. Neither Mr Irving nor Mrs Leonard knew where I was at the time.

With the exception of those recorded in this paper and a few

relating to holidays, the impressions contain no allusions to the varied interests and pleasures which make up the routine of life. This omission suggests that, for the purposes of the percipient, my thoughts relating to these every-day matters, no matter how absorbing they were for me at the time, fell upon stony places, where they had not much earth, and because they had no root they withered away. But some of the thoughts which related to holidays apparently fell upon good ground, for there are no less than ten such impressions. And holidays do mean, for me, an idea which goes deep into the mental soil. They are not merely transient periods of change. They are an escape from a feeling which I could compare to that of a donkey tethered to one patch on a wide common ; and I know the strength of this feeling to be rooted in a complex that goes far back into my early years.

I have by no means exhausted the analogies between dream formation and these impressions, but time is running on and before concluding I should like to mention one problem, and the solution of it that my subconscious mind has suggested. There are, in the impressions, more references to my dressmaking than to any other subject. I pondered over this, for it seemed to me that my dressmaking had no *real* significance for me, although there was a certain pretence emotion associated with it. I was thinking how I exaggerated this pretence emotion for the amusement of my friends. First, there is the hope and confidence of a perfect creation ; then the successful achievement of preparation ; then the trying-on and the realisation of what an object I look! and the throwing of the thing aside ; and finally the taking of it up and doggedly making the best of it. While I was reviewing this range of mock emotion there slipped into my mind, in the way the " direct voice " slips into Mrs Leonard's trance utterances, the word " frustration ". It was startling, for I had never had an experience of this kind before. Thinking it over, I concluded that the idea of frustration must be a Freudian symbol, for to associate it directly with my dressmaking seemed to be giving the latter too much significance. I dismissed the matter from my mind.

In the morning when I awoke, I found myself thinking of a day long ago when I stood on the sea-shore watching the sea come bounding joyously in, and recede with a hopeless, sighing sound over the shingle. Again the word " frustration " slipped into my mind, and in a flash I remembered my experience of the evening before and understood its meaning. On the occasion long ago when I watched the sea so joyously bounding in and so sighingly receding I had identified myself with it, and had quoted to myself the words

of Goethe, to the effect that " happiest they who soonest find the gulf that lies between their aspirations and their powers ".

I do not know how much reliance one can place on a subconscious prompting of this kind, but my dressmaking emotions do certainly seem to me to be, in more ways than one, a kind of parody of the stormy years of my girlhood. Even the laughter behind them has its significance. I have mentioned the incident, which is unique in my experience, as having possibly a bearing on the nature of the impressions in the " Isabel " series.

In this paper I have tried to show that however trivial the impressions may be, they have a particular significance for me. I suggest that the selection of ideas, haphazard as it may seem, is in fact correlated with this and that system of ideas, and still more of *feelings*, in my own mind; and that if the impressions were adequately analysed there would be found an underlying unity.

In other words, the impressions may be likened to the leaves which are seen on the surface of some ponds, apparently floating and disconnected but in reality connected, each one, with a root, which with other roots form a unity of underlying vegetation. Some of the roots throw up many stems; the M. impressions may serve as an illustration of an analogy in this respect. Some stems throw out shoots near the surface; the associated impressions, of which I have quoted some examples, may be likened to these. I have said " near the surface ", for these associations are self-evident to me, and they are given either consecutively or as part of the series given at any one sitting, as if the association of ideas is perceived.

Below the surface are the interlacing stems, and the roots from which they have their common origin. The leaves float separately, apparently disconnected, upon the surface, clearly visible; the stems, lightly silted over with the mud of the pond, are scarcely seen and soon disappear from sight; the roots are invisible. The image may be faulty, but it suggests the feeling that I have when I come to think over these cases, that details apparently disconnected and trivial in the " Isabel " series are in fact organically connected with systems of feeling which have become organised in my own mind; and I think I have shown that the groupings in which these impressions have actually occurred suggest that this idea of connection and structure does not spring from my mind alone.

But the evidence I have put forward rests on my word only, and therefore any theory it may suggest can have no weight unless it is confirmed by independent observation. I have put it forward in the hope that similar phenomena will be examined, whenever possible, with a view to testing this tentative hypothesis.

REVIEW

G. N. M. Tyrrell. *Science and Psychical Phenomena.* Methuen. xv + 374 pp.

It is a long time since there has appeared so comprehensive and well-ordered a survey of psychical research as Mr Tyrrell gives us. Psychical research, as he points out in his Introduction, is a branch of psychology which "has thrust out a spear-head into the unknown between normal psychology, on the one hand, whose business it is to deal with the mind in relation to the affairs of ordinary life, and abnormal psychology, on the other, which deals with the mind in derangement and disease".

The book is divided into five parts, the first of which is devoted to spontaneous extra-sensory perception. In assigning pride of place to the spontaneous mental phenomena Mr Tyrrell does well. Much of the best work of the S.P.R. in its earlier days was devoted to their study, and *Phantasms of the Living* and the Report on the Census of Hallucinations (*Proceedings*, Vol. X), which were the fruit of these labours, are still pre-eminent in psychical literature. Fashions change in psychical research as in everything else, and for several years past attention has been turned to other lines of enquiry. But there is no other branch of psychical research which affords a better training in applying critical standards or better opportunities for watching supernormal faculties at work. This is because of the great quantity of separate cases available for study, no two of them exactly alike but each of them embodying some notable experience of persons who, apart from these unusual and often, to them, unique experiences, are indistinguishable from the neighbours we meet every day. Evidence thus derived has certain values of its own, and although more striking material is to be met, *e.g.* in the records of the few great mediums, no survey of supernormal powers would be adequate which neglected the spontaneous phenomena or left them in the background.

After a useful discussion of the nature of spontaneous evidence, and of the problem of chance-coincidence as a possible explanation of veridical dreams and hallucinations, Mr Tyrrell gives several well-chosen cases from the S.P.R. *Proceedings* and *Journal* of spontaneous telepathy, clairvoyance and precognition, concluding this part of the book with a brief discussion of "psychometry" or object-reading.

In passing to Part II, which deals with experimental extra-sensory perception, Mr Tyrrell, were he less modest, might well say "*Quorum pars magna fui*", for this is a subject to which his own experiments have largely contributed. Before coming to more recent developments he gives an interesting historical summary of experiments carried out in different countries. Although Mr Tyrrell's own work has mainly been of a kind leading to statistical analysis, he evidently realises the importance of the results obtained by other types of experiment. From these Mr Tyrrell passes to the recent experiments in which Dr. Rhine and himself have played a leading part. It is clear that there are technical difficulties in the way of statistical experiments in extra-sensory perception which will require further exploration, so that the conclusions of even so experienced an investigator as Mr Tyrrell must at present be regarded as in a high degree provisional.

In Part III Mr Tyrrell sums up the conclusions of Parts I and II and discusses the theoretical aspects of extra-sensory perception and their relation to the problem of normal sense-perception. This section should be of great help to many students who complain that in our *Proceedings* and *Journal* they find masses of facts and technicalities of detail without sufficient indications of co-ordinating principles.

The rest of the book is devoted to the mediumistic trance, one chapter being given to the physical phenomena. These are always a problem to any writer attempting to give a general survey of psychical research. Their connection with the mental phenomena is, apart from the accidents of history, extremely slender, and even *inter se* they do not cohere to the same degree as the mental phenomena. If it were not that certain problems of evidence are common to both departments of psychical research, there would be a strong case for making a clean cut between the two. As things are Mr Tyrrell was probably wise in giving the physical phenomena a chapter, but no more, in his book. Separate chapters are given to Mrs Piper, Mrs Leonard, the evaluation of chance in mediumistic material, quantitative studies of the kind pursued by Mr Whately Carington, and book-tests. Mr Tyrrell achieves something of a *tour de force* in condensing an instructive account of cross-correspondence within the limits of a chapter.

Then follow chapters on the *modus operandi* of the trance, and on trance-personalities, with special reference to Lord Balfour's paper on Mrs Willett (*Proceedings*, Vol. XLIII). Particular interest attaches to the chapter headed "Nature of the Communicators: Antecedent Probability of Survival", in which Mr Tyrrell states and criticises

Prof. Broad's " Compound Theory " as put forward in his *The Mind and its Place in Nature*. Mr. Tyrrell argues that neither alone, nor in conjunction with any hypothesis of telepathy between the living, does this theory explain all the facts, particularly those connected with cross-correspondences : on this point I am in complete agreement with him.

The concluding chapter has some very interesting comments on the telepathic and survivalist interpretations of ostensibly spiritistic material. As to the former Mr Tyrrell writes :

" The real drawback to the telepathic theory is that, in order to make it work, we have to regard the living mind as something different from and immensely wider than what we commonly mean by the term, and we have to endow it with such a range of ' subliminal self ' and with such astonishing extra-sensory powers that the proposition of its survival takes on a new aspect. Having granted that the self is unlike the human unit of everyday experience as the telepathic theory demands, the arguments which hitherto seemed to make its survival improbable no longer apply, at least with anything like the same force. And so we find that the object which the telepathic theory was designed to fulfil, the object of providing a ' normal ' explanation, is no longer achieved."

As regards the survivalist theory Mr Tyrrell points out that psychology, religion and everyday experience all recognise division within the self, particularly division into what he calls " the dominant I " and " the synthetic or empirical I ", between which " there is the possibility of disunion and strife without numerical discreteness ". The " empirical I " is a " specialised product " adapted to the common life of the world, and " speaking broadly and generally, most of the anti-survivalist arguments arise from the incongruity of imagining this special kind of personality entering into other surroundings ". Mr Tyrrell suggests that the complex is resolved at death, without necessarily destruction in the process of " the principle of the dominant I ", which would then be free to enter into new and more stable forms of association. Mr Tyrrell's argument would gain strength from such examples of dissociation as the Beauchamp and Doris Fischer cases, which he too lightly dismisses, though admittedly pathological dissociation such as these cases present is not altogether parallel to the divisions within the normal self, which he is discussing.

There is much that is attractive in this hypothesis, nor is it lacking in support from the evidence. But from the point of view of the student the difficulty remains that even the best " communications " ostensibly come, and have all the appearance of coming, from a still

unresolved complex with the " empirical I " well in the foreground. It is doubtless possible to strain off the trivialities typical of the " empirical I ", and to explain them by unconscious dramatisation on the part of a mind (the medium's) endowed with extended faculties of cognition. But the student has to do the work of separation for himself, with whatever objectivity he can command, and without any internal indications in the material to guide him, other than the greater or less degree of seriousness of the different parts. Possibly the process of resolving the complex is a long one, and most of the communications recorded have been received before its completion.

There are points of detail which specialists in some branches of psychical research might wish to criticise, but in general this book may be warmly welcomed as a much-needed, well-arranged, up-to-date survey of our subject.

W. H. Salter

PROCEEDINGS

OF THE

Society for Psychical Research

PART 156

HENRY SIDGWICK AND PSYCHICAL RESEARCH

By Professor C. D. Broad, Litt.D.

Henry Sidgwick, one of the founders and the first President of our Society, was born on May 31, 1838. His centenary has recently been celebrated by a memorial lecture at Leeds, in the neighbourhood of which city he was born, and at Cambridge, where he dwelled and worked throughout the greater part of his life. As the S.P.R. owes its existence and its present status of at least semi-respectability in scientific circles very largely to the courage, patience, wisdom, and generosity of Sidgwick, it is only fitting that the great services he rendered to it should be recalled at this time to our members. From the nature of the case, most of Sidgwick's intimate friends and colleagues are now dead or advanced in years. The present writer never knew Sidgwick personally and has had no access to unpublished sources of information about him. But he happens to have succeeded him, *longo intervallo* in every sense of the phrase, both as President of the S.P.R. and as Knightbridge Professor at Cambridge, and he finds Sidgwick's attitude both in philosophy and in psychical research peculiarly admirable and sympathetic. These seemed to him to be adequate grounds for undertaking to write for the *Proceedings* an account of Sidgwick's relations with psychical research in general and the S.P.R. in particular.

It will be as well to begin with a very brief account of Sidgwick's life. He was born at Skipton on May 31, 1838, being the third son and fourth child of the Rev. William Sidgwick and Mary Crofts. His paternal grandfather was a cotton-spinner at Skipton, and his uncles carried on the business. His father was educated at Trinity College, Cambridge, and entered the Church. He held various cures,

and, at the time when Henry was born, he was headmaster of the grammar-school at Skipton. He died in 1841, when Henry was three years old, leaving his wife with a family of young children. After attending preparatory schools at Bristol and at Blackheath Sidgwick entered Rugby in 1852. His cousin, E. W. Benson, afterwards Archbishop of Canterbury, was then a young assistant master at Rugby. In 1853 Sidgwick's mother moved to Rugby and he and Benson lived with her. His school career was happy and brilliant, and he made several friendships which lasted throughout life. He entered Trinity College, Cambridge, in October 1855, where he studied mathematics and classics. He was a respectable mathematician and a brilliant classic. In 1856 he shared the second Bell Scholarship with J. M. Wilson, in 1857 he won the Craven Scholarship, and in 1858 he shared the Browne's Prize for Greek and Latin Epigrams with G. O. Trevelyan. In 1859 he was thirty-third wrangler in the mathematical tripos and was placed first in the classical tripos. In the same year he won the First Chancellor's Medal and crowned his academic career by being elected to a fellowship at Trinity. The rest of his working life was spent at Cambridge.

During the sixties Sidgwick was engaged in a desperate internal struggle with the intellectual difficulties which the Christian religion, as then understood in England, presented to honest and instructed minds. In the course of these enquiries he gained a thorough mastery of Hebrew and Arabic, made an elaborate study of theology, and immersed himself in philosophy. At that time it was a condition of holding a fellowship that the holder should declare himself to be a "*bona fide* member of the Church of England". This obligation was not usually taken very seriously, but Sidgwick was an exceptionally conscientious man. By June 1869 he had come to the conclusion that he did not fulfil the condition literally enough to justify him in holding a paid office on these terms. He therefore resigned his fellowship and assistant tutorship at Trinity. The college accepted his resignation with deep regret and did what it could to compensate him by creating a lectureship in Moral Science, without theological conditions, and appointing him to it. Nevertheless, Sidgwick suffered a considerable loss of income and amenities for a number of years.

In 1875 Trinity appointed Sidgwick Praelector in Moral and Political Philosophy, which gave him an increased income and an assured position. In the same year he became engaged to Eleanor Mildred Balfour, whom he married in 1876. He and his future wife had met while working at the two subjects which were destined to occupy most of their future time and energy, viz., psychical research

and the higher education of women at Cambridge. It should be unnecessary to remind members of the S.P.R. of the magnificent work which Mrs. Sidgwick did for the Society and for the subject during her long and active life. Anyone who will take the trouble to read the memoirs of Mrs Sidgwick by Miss Johnson, Mr Salter, and Mr Besterman, in Vol. XLIV, and will then refer back to the numerous and masterly articles which she contributed to previous volumes, will see that one of Sidgwick's most important services to psychical research was to encourage his wife to pursue the subject.

By 1880 the movement for the education of women at Cambridge had progressed so far that a new Hall of Residence at Newnham had been built, and Sidgwick and Mrs Sidgwick temporarily moved into it. In the following year Trinity made him an honorary fellow. He had applied for the Knightbridge Professorship of Moral Philosophy on the death of F. D. Maurice in 1872, but another candidate had rather unaccountably been chosen. The professorship again fell vacant in 1883, and this time Sidgwick was elected. He held the chair until his last illness in the spring of 1900.

Throughout his life Sidgwick had been an active participator in various attempts to reform the constitution of his own college and the university. In 1876 Lord Salisbury set up a statutory commission for Oxford and Cambridge on the lines desired by the Cambridge liberals. The new statutes came into force in 1882, and Sidgwick was much occupied during the next ten years in the delicate work of initiating and trying to carry through certain financial and educational changes which they had made possible and which he thought desirable.

In 1892 Mrs Sidgwick accepted the Principalship of Newnham College on the death of Miss Clough. The building at Newnham which she was to occupy was completed at the end of 1893, and the Sidgwicks then gave up their house in Cambridge and moved into Newnham College, where Sidgwick spent the last seven years of his life. Early in 1900 he underwent a serious operation, from which he never recovered. He died on August 28, 1900, at the house of his brother-in-law, Lord Rayleigh, at Terling in Essex. He is buried in the churchyard of Terling.

During the period which has been covered in this sketch of Sidgwick's life he was busily engaged in his academic work in philosophy, political theory, and economics. The most important works which he published in his lifetime were *The Methods of Ethics*, *Outlines of the History of Ethics for English Readers*, *The Principles of Political Economy*, and *The Elements of Politics*. After his death four substantial books were made out of his lectures, viz., *Philosophy, its*

Scope and Relations, *The Development of European Polity*, *Lectures on the Ethics of Green, Spencer, and Martineau*, and *Lectures on the Philosophy of Kant*. He wrote numerous articles on literary, educacational, and other subjects, and a collection of these has been published under the title of *Miscellaneous Essays and Addresses*.

The reader is now in possession of the main facts about Sidgwick's work in other fields than that of psychical research. Let us now consider the history of his activities in the latter field. His interest in ostensibly supernormal phenomena goes back to the beginning of his undergraduate days. When he went up to Cambridge a society called the "Ghost Society" already existed there. One of the founders of this had been his cousin, E. W. Benson. Westcott was secretary of it until 1860, when he left Cambridge for Harrow. Sidgwick joined the Ghost Society while he was still an undergraduate.

It is plain from his letters that he was collecting stories of supernormal phenomena in the late fifties and early sixties. In a letter to his sister of October 30, 1859, he refers to a ghost-story sent him by his mother, and to others which he had had from an Irish friend. In another letter, later in the same term, he mentions a newspaper cutting, sent to him by his Uncle Robert, narrating a dream of her son's death which a poor woman had on the night of the wreck of the "Royal Charter". He makes the characteristically cautious comment "It was curious, but, considering how fruitful of dreams such a night must be, not very strong evidence". In a letter to his mother in July 1860 he thanks her for a ghost-story and says that he has had two very remarkable ones at first hand from a clergyman. "Mind you shut up everybody who says that such stories can only be got from 'cousin's cousin's friends' or such like distant parties" is the admonition which he gives to his mother at the end of the letter.

In 1860, whilst staying in London with his friend Cowell, he had his first experience of a sitting with a professional medium for physical phenomena. In a letter to his sister he describes the medium as "a complete humbug". In 1864 he and Cowell had sittings together for automatic writing. Cowell produced the writing and they were both puzzled by hearing unexplained raps, but they agreed that there was nothing in the contents of the scripts that could not have come from their own minds. Many years afterwards Sidgwick gave an account of these sittings to F. W. H. Myers, which is printed in Myers's article on "Automatic Writing" in Vol. III of the *Proceedings*. Two points of interest emerged. One was the ingenuity which the unconscious part of Cowell's mind displayed in

puzzling the conscious part of it. The other was the elaborate stories which would be developed in the automatic script to account for the failures of the ostensible communicator to pass the tests which Cowell and Sidgwick had devised in order to examine his claims to be an independent entity.

The first period of Sidgwick's investigations into Spiritualism extends roughly from 1865 to 1875. In 1863 he writes to his friend Dakyns " I have not yet investigated Spiritualism, but I am still bent on doing so as soon as I get an opportunity ". He also mentions that T. H. Green " sniffed at " it, as one might perhaps have expected. In writing to his mother early in 1864, in reference to a book which she had recommended to him, he says " I am pretty well read in pneumatological literature, but I have not heard of the book that you mention ". Later in the year he writes to Dakyns saying " As to Spiritualism I have not progressed, but am in painful doubt. Still, I have some personal experiences and much testimony, and I find it hard to believe that I shall not discover some unknown laws, psychological or other. . . ." Writing to Roden Noel in December 1866, he makes some interesting comments on the effects which his recent reading of Lecky's *History of Rationalism* has had on him. The book had set him to consider the *evidence* for mediaeval miracles, a topic which Lecky explicitly ignored. Sidgwick was considerably impressed by this evidence, and he writes to Noel as follows. " I dimly foresee that I shall have to entirely alter my whole view of the universe and admit the ' miraculous ' . . . as a *permanent* element in human history and experience. . . ." He suggests that these reflexions link up with his interest in Spiritualism, and that together they may throw a light on the origin of all religions.

In the summer of 1867 Sidgwick was staying in London and he had many experiences of spiritualistic phenomena. Some of them were impressive, but he could never get absolutely satisfactory evidential conditions. During this period he happened to meet Mazzini at a dinner party, and he was greatly interested by a story of a collective hallucination, due to mass-suggestion, which Mazzini related to him from his own experience. The case is described in a footnote to Chapter XVIII of *Phantasms of the Living* (p. 477 of the abridged edition). It seems to be worth quoting. In or near some Italian town Mazzini saw a group of people standing gazing upwards into the sky. He went up to one of them and asked him what he was gazing at. " The cross—do you not see it? " said the man, pointing to the place where the cross was supposed to be. Mazzini could see nothing in the least cruciform in the sky ; but, on enquiring of others, he found that they also thought they were seeing a cross.

At length Mazzini happened to notice one gazer who looked rather more intelligent than the rest, and also seemed to have a faint air of doubt and perplexity. Mazzini went up to him and asked him what he was looking at. "The cross," he said, "there." Mazzini took hold of his arm, gave him a slight shake, and said to him "There is not any cross at all". A change came over the gazer's face as if he were waking from a kind of dream, and he answered "No, as you say, there is no cross at all". He then walked away with Mazzini, leaving the rest of the crowd to enjoy their collective hallucination. Sidgwick always remained greatly impressed with the importance of this story in relation to the evidence for the ostensibly supernormal physical phenomena of Spiritualism.

From 1869 onwards Sidgwick began to be associated with Myers in a common interest in psychical research. In the very eloquent and moving memoir of Sidgwick which Myers contributed to Vol. XV of the *Proceedings* he states that it was during a star-light walk in Cambridge on December 3, 1869, that he broached the subject to Sidgwick and determined, if possible, henceforth to pursue the elusive quarry with the latter as his guide. Myers had read classics with Sidgwick as his private tutor when he came up to Trinity as an undergraduate in 1860. The occasion of the visit to Cambridge in December 1869, from which he dates the beginning of their co-operation as psychical researchers, was the fact that Myers was then examining for the Moral Science Tripos. The first mention of such co-operation in Sidgwick's published letters is in a letter to Myers dated October 30, 1873. The following passage is so characteristic as to be well worth quoting. "As for spirit-rapping I am in exactly the same mind towards it as towards religion. I believe there is something in it, don't know what, have tried hard to discover, and find that I always paralyse the phenomena. My taste is strongly affected by the obvious humbug mixed with it, which at the same time my reason does not overestimate."

In 1871 Sir William Crookes had published, in the *Quarterly Journal of Science* and elsewhere, an account of his experimental researches in the physical phenomena of Spiritualism. He wrote further articles about it in 1874 and in the same year Alfred Russel Wallace had published in the *Fortnightly Review* a paper entitled "A Defence of Modern Spiritualism". Sidgwick, writing to his mother on July 11, 1874, said "*No one* should pronounce on the *prima facie case for serious investigation*—this is really all that I maintain on behalf of Spiritualism—who has not read Crookes's *Researches*". Sidgwick and Myers now started to investigate together, and they formed a small association for the purpose, which

was a kind of forerunner of the S.P.R. Edmund Gurney, who was to become one of the most active and important workers in the S.P.R., was at first hesitant at joining and contented himself with giving his warmest sympathy to this association. However, A. J. Balfour and Lord Rayleigh both joined, and experiments were conducted in their homes. It was in the course of these experiments that Sidgwick met the sister of A. J. Balfour, whom he afterwards married.

These experiments were subsequently described by Mrs Sidgwick in an excellent article in Vol. IV of the S.P.R. *Proceedings* entitled "The Physical Phenomena of Spiritualism". The mediums concerned in 1874 were Miss Showers, Mrs Jencken (*née* Kate Fox), and Miss Eva Fay. In the first three months of 1875 Sidgwick, Myers, and Gurney had sittings for materialisation at Newcastle with Miss Wood and Miss Fairlamb, and they had another series of sittings with the same mediums in London later in the year. Mrs Sidgwick was not present at these, but she was present at a further series held in London in July and in Cambridge during August and September. These two mediums quarrelled and separated some time during the year 1876, and the final sittings, which the Sidgwicks held at Newcastle in January 1877, had to be conducted with Miss Wood and Miss Fairlamb separately. In 1874, and again in 1876, Sidgwick had a series of sittings with a medium called Williams. Sidgwick and Mrs Sidgwick had some sittings in the summer of 1876 with a young and palpably fraudulent Mr Bullock, who, as Mrs Sidgwick dryly remarks, "may have acted wisely in his own interests when he gave up the career of medium and took to that of exposer of Spiritualism, as he did six or seven months later". In the same year the celebrated Dr. Slade came to London. It was reported that his control by four-dimensional spirits had enabled him to tie knots in a bit of string whose ends had been sealed together by the German psychologist Fechner. He also specialised in causing writing to appear inside a locked double slate in answer to questions put by sitters. The Sidgwicks had ten sittings with him for slate-writing. Mrs Sidgwick also had three sittings with Eglinton, another famous slate-writing medium of the period.

The results of all this work with paid professional mediums for physical phenomena may be fairly summarised as follows. Many of the sittings were complete blanks. In some fraud was actually detected and in some there were circumstances which made it almost certain that fraud had been practised. In the very few cases where it looked as if a positive supernormal effect had been obtained there was always some unfortunate breakdown in some part of the control,

or some diversion of the sitters' attention by external interruption, which made it possible to account for the phenomenon by normal causes. The course of Sidgwick's disillusionment and disgust may be traced in his letters during this period. Writing to Myers at the end of 1874 he remarks that he has had to drop Mrs Jencken and will now have to drop Miss Fay out of his "case for Spiritualism". He adds the following remarks. "What induces me, not to abandon, but to restrict, my spiritualistic investigations is not their disagreeableness (they have never been other than disagreeable so far as paid mediums are concerned) but their persistent and singular frustration." The subsequent experiences of the S.P.R. with physical mediums have emphasised the "persistence" and diminished the "singularity" of such frustration.

In the autumn of 1876 Professor Ray Lankester instituted criminal proceedings against Dr Slade, whom he claimed to have detected in fraud. Sidgwick expected, though he did not desire, to be *subpoenaed* by Ray Lankester's lawyers. Writing on this matter to Dakyns on October 10, 1876, Sidgwick says that, so far as his own experience goes, he would unhesitatingly pronounce *against* Slade. But he admits that there is testimony *for* him which he would like to see examined in a court of law.

This whole period of Sidgwick's dealings with psychical research is well summed up in the following passage from a letter which he wrote to Roden Noel on June 24, 1878. "I have not quite given up Spiritualism, but my investigation of it is a very dreary and disappointing chapter in my life."

We come now to the revival of Sidgwick's interest which led to his consenting to take an active part in founding and guiding the S.P.R. This was due to the apparent success of certain experiments in thought-transference which Professor William Barrett had been carrying out at Dublin. At Barrett's instigation a conference was convened, and it met on January 6, 1882. At this conference the S.P.R. was planned. It was to include persons of all shades of opinion, from sceptical scientists who were reasonable enough to admit that there was a *prima facie* case for investigation to convinced Spiritualists who were reasonable enough to admit that there was a great deal of fraud and imposture and self-deception to be eliminated. Barrett represented the scientific wing and Stainton Moses the spiritualistic wing. Myers tells us that he and Gurney, whilst heartily approving the general scheme, consented to join if, and only if, Sidgwick would do so and would consent to be President. They encouraged him to undertake this task, but it was only after considerable hesitation that he accepted. There were strong and

respeetable motives against doing do. Why should he spend more of his time and energy, both of whieh were very fully oeeupied in work immediately benefieial to his fellow-men, in order, as Myers puts it, " to get the moon for a ehild who had not even eried for it "? Orthodox believers did not want their speeial revelation to be shown to be part of a wider system ; and orthodox seientists treated the whole matter at best with eompassion and at worst with eontempt. On the other hand, Sidgwiek had never eonsidered that the original question, whieh he had spent so mueh time and trouble in investigating with so little result, had been answered in the negative by his abortive researehes in the mediumistie underworld. There had never been any moment at whieh he had felt that he had the right to abandon further investigation of the subjeet. And he had eertain positive motives, eonneeted with his religious, ethieal, and philosophieal perplexities, for wishing the question at issue to be settled definitely in one direetion or the other. To these motives we shall return at a later stage.

At present it will suffiee to say that eventually Sidgwiek consented to join the S.P.R. and to be its first President. His entry earried with it the adhesion to the Soeiety of several others who were destined to play a most important part in its life and work. It brought in Mrs Sidgwiek, her brothers Arthur and Gerald Balfour, and her brother-in-law, Lord Rayleigh, and, as we have already seen, it was the eondition without whieh the Soeiety would have laeked the inestimable serviees of Edmund Gurney and Frederie Myers. Moreover, the faet that Sidgwiek, whose reputation for sanity, truthfulness, and fairness was well known to everyone who mattered in England, was at the head of the Society gave it an intelleetual and moral status whieh was invaluable at the time. It was hardly possible to maintain, without writing oneself down as an ass, that a society over whieh Sidgwiek presided and in whose work he was aetively interested eonsisted of knaves and fools eoneealing superstition under the eloak of seientifie verbiage. Needless to say, this feat was not found to exeeed the eapaeity of some erities ; but, with almost anyone else as President, their numbers would have been far greater and their influenee might have suffieed to kill the Soeiety in its infaney.

Sidgwiek gave his inaugural address to the S.P.R. on July 17, 1882. He delivered a seeond presidential address on Deeember 9 of the same year, and a third on July 18, 1883. These will be found in Vol. I of the *Proceedings*. Vol. II eontains another presidential address delivered on May 28, 1884. He resigned the Presidentship in 1885, thinking that the Soeiety eould now profit from a ehange,

but at the same time he undertook the editorship of the *Journal*. His successor in the presidential chair was Balfour Stewart.

During the year 1884 the S.P.R. appointed a committee to take evidence in London from leading members of the Theosophical Society about the marvellous phenomena alleged to have taken place in India in connexion with Madame Blavatsky and certain other members of her sect. Madame Blavatsky, Colonel Olcott, and a Brahmin disciple called Mohini spent some months in London and gave evidence to this committee. Sidgwick as President was *ex officio* a member. The Theosophical contingent visited Cambridge early in August, attended a meeting in Oscar Browning's rooms in King's, and were entertained to luncheon by Myers. The Sidgwicks rather liked Madame Blavatsky, who was evidently an engaging old humbug with a rich and racy personality and full of courage and resource. They found her, it is true, externally unattractive and not prepossessing in manner; and indeed her habit of smoking incessant cigarettes and indulging in relatively strong language, though it would pass unnoticed in our more enlightened age, could hardly fail to attract unfavourable attention in a Cambridge drawing-room of the eighteen-eighties. Sidgwick says of her in his diary for August 10. "If she is a humbug, she is a consummate one; as her remarks have the air not only of spontaneity and randomness but of an amusing indiscretion." (She had referred to a certain Mahatma, a class of beings for whom the Theosophists entertained the highest reverence, as "the most utter dried-up old mummy" that she ever saw.)

The Theosophical sub-committee issued a balanced interim report which was privately printed and circulated to members of the S.P.R. At the end of it they announced that Richard Hodgson was on his way to India to investigate and report at first hand. Hodgson completed his task and returned to England in April 1885. His evidence as to the fraudulent character of the Theosophical marvels was damning; though one could have wished that he had not had to rely so much on the revelations of two discharged employees of Madame Blavatsky who had quarrelled with her and were busily engaged in biting the hand which had not ungenerously fed them. The final report, embodying Hodgson's findings, was written mainly by Mrs Sidgwick. It occupies a considerable part of Vol. III of the *Proceedings* and is easily the most dramatic and entertaining bit of work that the Society has ever published.

During the latter part of 1884 Mrs Sidgwick was working at the important paper on "Phantasms of the Dead", which she read on January 30, 1885. It is published in Vol. III of the *Proceedings*.

In preparation for it Sidgwick investigated critically the numerous ghost-stories that had been sent to the S.P.R., and in September 1884 he made a tour to interview persons who had contributed such stories. He says that the evidence is not so good as for phantasms of the living, and that out of about three hundred cases not more than twenty or thirty can be pronounced good. After returning from his tour of interviewing he remarks " The stories that become worse after oral examination are those that we had already judged to be objectionable, and some are decidedly improved by the examination ". His comment after Mrs Sidgwick had read her paper was " It looks as if there was *some* cause for persons experiencing independently in certain houses similar hallucinations. But we are not at present inclined to back ghosts against the field as *the* cause."

In the meanwhile Myers, Gurney, and Podmore were busily engaged in comminuting and refining those masses of crude ore from which the two volumes of *Phantasms of the Living* were eventually smelted. This is undoubtedly an epoch-making work, in the strict sense that it laid the foundations of a new subject and still remains a classic indispensable to all students in its own field. Both Sidgwick and his wife were, of course, in constant touch with the authors at all stages of their work.

In June 1885 Myers read his introduction to *Phantasms of the Living* as a paper to the S.P.R. Sidgwick's comments in his diary are of great interest. " *In the end* ", he says, " if the S.P.R.'s work should all be negative, it will be regarded by sceptics as the last element of proof necessary to complete the case against Christianity and other historical religions. But *for a long time* the only difference would be that those religions will have to *support* their miracles instead of *being supported* by them. They can go on doing this for a long time until sociology has been really constructed and the scientist steps into the place of the priest." The same thought is expressed in the following sentence of Myers's Obituary Notice on Sidgwick. " 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." Neither Sidgwick nor Myers could foresee that in another fifty years compulsory education would have produced throughout the civilised world a populace of literate imbeciles, ready to believe or to disbelieve anything with equal passion and unreason, and that science would have provided, in the cheap press and the wireless, an immensely powerful engine for generating irrational beliefs and disbeliefs at will. Before taking leave of this topic we may recall the remark in which Gibbon contrived to twit both

the Jews and the Christians. Referring to the rejection, by the Jews of apostolic times, of those stupendous miracles which, according to the Christians, were happening under their very noses, Gibbon remarks: "Contrary to every known principle of the human mind, that singular people seem to have attached a more explicit credence to the testimony of their remote ancestors than to the evidence of their own senses".

Sidgwick's central position, and his oscillations about it, during this period are well brought out by the following quotations from his diary. On January 3, 1886, after a meeting of the S.P.R., which now had 600 members and associates and could, in his opinion, "run without further nursing", he wrote as follows: "I do not doubt that thought-transference is genuine, and I hope that it will soon be established beyond cavil; but I see no prospect of making any way in the far more interesting investigation of Spiritualism." On March 7 of the same year, after listening to a mildly spiritualistic paper by Sir William Barrett, he wrote "I feel that the natural drift of my mind is now towards total incredulity in respect of extra-human intelligences. I have to remind myself forcibly of the arguments on the other side, just as a year ago I had to dwell deliberately on the sceptical argument to keep myself balanced." On January 28, 1887, he wrote "I am drifting steadily to the conclusion that we have not and are not likely to have empirical evidence of the existence of the individual after death". On July 16, 1888, after giving an address to the S.P.R. in which he pleaded for the collection of further cases of spontaneous telepathic action, he wrote "I have not much hope of our getting at positive results in any other department of our enquiry, but I am not yet hopeless of establishing telepathy".

Even about telepathy, which he regarded as established to his own satisfaction, he was subject to the set-backs and disappointments which are the lot of the psychical researcher. In his diary for November 29, 1884, he writes "I am shaken in my view of the telepathic evidence by the breakdown of Sir E. H.'s narrative in the *Nineteenth Century*. He tells an elaborate story of what happened to him less than ten years ago. His wife, who was an actor in it, confirms it. Her mother bears witness that the wife told her next morning. Yet the story is inaccurate in fundamental details—it is difficult to understand how any of it can be true." Lastly, there was a sad disappointment in his experiments with Miss Relph at Liverpool. Sidgwick investigated her claims to telepathic powers in March 1887. On the 30th the results were so good that he was able to say "they leave no doubt in my own mind that I had wit-

nessed the real phenomena". On March 31 the attempts to repeat the results under unexceptionable "conditions" were a complete failure. Sidgwick still accepted the former results, but realised that they were not enough to convince an outsider.

On June 25, 1888, there befell one of the great tragedies of psychical research, viz., the sudden death, at a comparatively early age, of Edmund Gurney through an overdose of chloroform taken for neuralgia or insomnia. It was a terrible blow to the Sidgwicks personally, and it will be evident to anyone who has studied *Phantasms of the Living* or read the admirable articles which Gurney contributed to the early volumes of the *Proceedings* that his death was an irreparable loss to the Society and to the subject. Sidgwick had now become President of the S.P.R. for a second period after a considerable interval, and he delivered his presidential address on July 16, 1888, three weeks after Gurney's death. This brings us by a natural transition to the next important piece of work with which the Sidgwicks were closely concerned, viz., the S.P.R.'s *Census of Hallucinations*.

Everyone is familiar with stories of the following kind. *A* has an hallucinatory visual, auditory, or tactual perception in which he seems to himself to be seeing or hearing or touching a certain friend or acquaintance *B*. Afterwards, *A* learns that *B* was dying or in serious danger at the time when the hallucination was experienced. The S.P.R. was naturally inundated with stories of this kind and its first business was, of course, to weed out all the cases which might reasonably be explained by misreporting, exaggeration, errors of memory, normal expectation and inference, and so on. When this had been done there remained a substantial residue of such stories which appeared incapable of any normal explanation. As regards this residue only two alternatives were open. Either the approximate simultaneity between the hallucination in *A* and the death or illness or accident in *B* was a mere coincidence, or there was some supernormal causal connexion between the two. It had been quite evident to Gurney that no rational decision between these two alternatives was possible except on a statistical basis. It was essential to know how frequently such hallucinatory experiences occur among sane waking persons in contemporary civilised societies. The more frequent they are, the more likely it is that some of them will happen to coincide with the death or danger of the person whom they concern. Now this was a subject on which no reliable statistics existed at the time. In *Phantasms of the Living* he had attempted an estimate of the frequency of such hallucinations among contemporary Englishmen from the data at his disposal. He had come to

the conclusion that, whilst they are much commoner than one would have been inclined to believe, they are not nearly common enough to make it reasonable to regard those which turn out to be veridical as mere chance coincidences. But he was well aware that the question could never be satisfactorily settled until a direct statistical enquiry on a very large scale had been made in order to determine the frequency of such experiences, veridical and delusive, among the population.

Sidgwick was most anxious that such an enquiry should be carried out, both because of its extreme scientific importance and because it would round off the work of his dead friend and colleague. Accordingly he induced the S.P.R. to appoint a committee, consisting of himself, Mrs Sidgwick, Myers, Podmore, and Miss Alice Johnson, in order to undertake a census by means of a questionnaire. The collection of statistics went on steadily between the spring of 1889 and that of 1894. It entailed an immense amount of very tedious work. Sidgwick introduced the subject to the Society in a special address on July 8, 1889, in which he explained the importance of the census, asked for volunteer collectors, and pointed out the precautions which ought to be taken. He gave a second address on the subject on July 11, 1890, in which he reports the progress already made, urges the members of the Society to fresh efforts, and comments on certain types of hallucination which have been reported. In the summer of 1889 the Sidgwicks attended an international congress of psychologists at Paris. Owing to the presence of Richet, whom Sidgwick had first met in October 1885 and had greatly liked, there was much discussion on psychical research. The congress gave its sanction to a census of hallucinations on the same lines as that conducted in England by the S.P.R.

The S.P.R. committee published its final report in Vol. X of the *Proceedings*. It occupies about 400 pages and is a most masterly production. It was written mainly by Mrs Sidgwick and Miss Johnson, in close consultation with Sidgwick himself. The upshot of the enquiry was as follows. About one visual hallucination in *sixty-three* occurs within a period of twenty-four hours round about the death of the person whose apparition has been " seen ". If such death-coincidences were purely fortuitous concurrences of causally independent events the proportion would be about one in *nineteen thousand*. There is a most elaborate and careful discussion of the fallacies to which such statistics are liable, and a very clear and detailed statement of the precautions which the committee took to avoid them. Anyone who now argues airily on this subject without having studied this report is merely wasting his own and his hearer's

time. Yet I venture to doubt whether so much as one *per cent.* of the teachers and students of experimental psychology in this country have ever troubled to flutter the pages of what is, on any view, a unique and meticulously careful contribution to an important branch of their subject.

From the spring of 1885 onwards the Sidgwicks had from time to time taken part in experiments on thought-transference in connexion with hypnotised subjects. Sidgwick records visits to Brighton for this purpose on March 22 and July 4, 1885. On January 10, 1887, he mentions the abortive conclusion of a week's investigation of a professional mesmerist, Mr D., who pretended to transmit ideas to his mesmeric "subject". In spite of the fact that Mr D. had been a French master in a school and had a brother who was a Cambridge graduate and a clerk in Holy Orders, he was detected by Richard Hodgson using a code which depended on the variations in the subject's breathing. A much more important series of hypnotic experiments was carried out in the summer of 1889 with Mr G. A. Smith as hypnotist and telepathic agent, and two young clerks, whom he mesmerised, as telepathic percipients. The results of these experiments formed the subject of an article in Vol. VI of the S.P.R.'s *Proceedings*. The successes were altogether beyond chance, and in view of the precautions taken it is difficult to think of any normal explanation for them. Further experiments with the same hypnotist and the same subjects were carried out by Mrs Sidgwick and Miss Alice Johnson in the years 1890, 1891, and 1892. The report of them will be found in Vol. VIII of the *Proceedings*. The results are, in some respects, more remarkable, since successes well above chance were scored with Smith and his subjects in different rooms.

A sequel to these hypnotic experiments was the occasion for Sidgwick's last contribution to the *Proceedings*. Two Danish psychologists, Messrs. Lehmann and Hansen of Copenhagen, published in Wundt's *Philosophische Studien* a long paper describing their experiments on what they called "involuntary whispering". Now most of the work on transference of ideas which Sidgwick and his wife had done with Mr Smith and his subjects was concerned with guessing two-digit numbers printed on discs which Smith drew from a bag and concentrated his attention upon. On the basis of the Copenhagen experiments Messrs Lehmann and Hansen claimed to show that the degree of success scored by Smith's hypnotised subjects could be explained by supposing that Smith involuntarily whispered the numbers as he concentrated his attention upon them, and that his subjects were in a state of auditory hyperaesthesia.

As a matter of fact the Sidgwicks had carefully considered this possibility and had discussed it elaborately in their first report in Vol. VI of the *Proceedings*. Moreover, in the second series of experiments, reported in Vol. VIII, a significant degree of success had been scored when Smith was out of the room and on a different floor from that occupied by the percipients. Nevertheless, Sidgwick thought that Messrs Lehmann and Hansen's suggestions were important enough to merit serious attention, since they certainly threw fresh light on the hypothesis of unconscious whispering.

He began by repeating the experiments, in a somewhat simplified form, with his wife and Miss Johnson. They found that in all cases the whispering was completely *voluntary*, and they detected in themselves no trace of that tendency to *involuntary* whispering which the Danish psychologists had alleged to be the natural accompaniment of attempts to concentrate on a number. But they verified the Copenhagen claims to the following extent. They found that it was quite possible for a person *deliberately* to whisper in such a way that an observer who fixed his attention on that person's *mouth and lips* could neither see nor hear any signs of whispering at a distance of two feet. And yet, at a distance of eighteen inches between this person's mouth and a percipient's ear, the percipient could hear enough of the agent's whispering to score a considerable amount of success in his guesses. It is therefore reasonable to assume that, *if* the hypnotic agent whispered in this special way, a hypnotised subject, in special *rapport* with him, might hear distinctly at a considerably greater distance without any sign of whispering being audible or visible to third parties. Sidgwick noted that if an observer directed his attention to the *neck and throat* of the whisperer, instead of his mouth and lips, the fact that whispering was taking place became obvious. Accordingly he undertook some further experiments with Smith, concentrating his attention on the latter's neck and throat without telling Smith that he was doing so. He used as percipient Mrs Verrall, who had practised detecting whispering with Mrs Sidgwick and Miss Johnson. The result was completely negative. Sidgwick could see no special movements in Smith's neck and throat, and Mrs Verrall could hear no whispers coming from Smith.

Sidgwick published these results in an important note on "Involuntary Whispering" which he contributed to Vol. XII of the *Proceedings*. After stating his experimental conclusions, he undertook an elaborate statistical analysis of the mistakes which had been made in guessing numbers in the hypnotic experiments and in Messrs Lehmann and Hansen's researches. He claimed to show that the distribution of

mistakes among the Copenhagen guesses closely resembled the distribution of mistakes among the guesses made by the English hypnotised subjects on their *unsuccessful* days, when their scores were plainly due to chance. He ascribed this similarity to a likeness in number-habits between the English and the Danish subjects. Whilst I accept Sidgwick's conclusion that it is most unlikely that the success of the hypnotic experiments can be explained by involuntary whispering, I am not altogether satisfied with his statistical argument and I should like to see the whole subject treated again by modern statistical methods which have become available since Sidgwick's day.

The rest of Sidgwick's activities in connexion with psychical research can be very briefly described. They were concerned with the physical medium, Eusapia Palladino, and the mental medium, Mrs Piper. In the summer of 1894 Myers and Sir Oliver Lodge, in company with Richet, were investigating Eusapia at Île Roubaud. They wrote to Sidgwick stating that they thought they had got physical phenomena under test conditions. He and Mrs Sidgwick therefore went out to France. The phenomena, as usual in their presence, became less striking, but at the time he was almost convinced that some of them were genuinely supernormal. In August 1895 Eusapia stayed at Myers's house in Cambridge and was subjected to an elaborate investigation. The Sidgwicks concluded that her phenomena were fraudulent, and that they were produced by a certain trick which was suspected at an early stage and was worked out in detail by Hodgson. It is significant that Eusapia steadily refused to comply with conditions which would have excluded the use of this method.

The discovery of Mrs Piper in Boston by Professor William James was the beginning of an important new stage in the development of psychical research and the work of the Society. Her mediumship has been of the utmost importance because it gave results which are quite certainly supernormal and which seem, *prima facie*, to be very difficult to explain without going beyond telepathy from the living. It is roughly true to say that Sidgwick's death happened at a transition point in the history of the subject. In the past were the comparatively straightforward problems of the experimental and statistical establishment of the transference of simple concrete ideas and emotions. In the future lay the subtle and complex problems of cross-correspondences, book-tests, and so on, in which we are still immersed. Mrs Piper's mediumship is the connecting link between the two stages, and Sidgwick lived only long enough to participate in the very early phases of the investigation. Myers and others

invited Mrs Piper to England in the winter of 1889, and she stayed until the spring of 1890. Sidgwick took a prominent part in the investigations. He had no success in his own sittings with her, but he was much impressed by the experiences of some of his friends. Subsequent work with Mrs Piper was mainly conducted in the U.S.A. under the direction of Richard Hodgson.

Mrs Sidgwick survived her husband for many years and maintained up to the end her active interest in the Society and her invaluable work on the subject. We have her own authority for stating that, in her opinion, the evidence as a whole provides an adequate ground for believing that human beings survive bodily death. One would give a great deal to know whether the facts which became available after 1900 would have caused Sidgwick himself to accept so positive a conclusion.

Having supplied the reader with a fairly adequate history of Sidgwick's dealings with psychical research, I will now say something about the nature and extent of his contributions to the subject. His own estimate of his capacities is characteristically modest and accurate. It is recorded in an entry in his diary for September 11, 1884. He thinks that he has a mind much better qualified for seeing relations in the history of thought than for suggesting hypotheses in psychical research. " I don't feel the least gift ", he says, " for making a legitimate hypothesis as to the causes of the phenomena, and I am too unobservant and unimaginative about physical events to be at all good at evaluating particular bits of evidence. . . . To tell whether a ' psychical ' experiment or narrative is good or not, evidentially, requires one to imagine with adequate accuracy and exhaustiveness the various possibilities of ' natural ' causation of the phenomenon, and judge the degree of improbability of each. Nora is much better at all this than I am. . . ." This extremely high and very just appreciation of Mrs Sidgwick's powers is reiterated on May 2, 1894, where he says that his only reason for doubting whether she was right in accepting the Principalship of Newnham is that he fears " that she may not find time for the work of the S.P.R.", for which, he says, " I think her uniquely fit—much more fit than I am ". He thinks that in psychical research the one function that he can exercise is the judicial. " I feel equal to classifying and to some extent weighing the evidence, so far as it depends on general considerations."

Making due allowance for Sidgwick's natural modesty and generosity, I think that this estimate of his powers is essentially correct so far as it goes. His main contribution to psychical research did not consist in making ingenious experiments or suggesting fruitful

and far-reaching hypotheses. It consisted in the weight which his known intelligence and integrity gave to the serious study of the subject, in the tact and patience with which he handled the very difficult team which he had to lead rather than to drive, in the extremely high standard of evidence which he inculcated both by example and by precept, in his courage and persistence in face of repeated failure when success seemed almost within reach, and in the general maxims which he laid down in his various addresses to the S.P.R. It will be worth our while to consider in some detail the teachings of Sidgwick's presidential addresses, for they are still highly relevant to contemporary conditions.

The first three are concerned with the *raison d'être* of the S.P.R., with certain criticisms which had been made on the very idea of such a society, and with the nature of the evidence which already exists and the further evidence which is required. When he occupied the presidential chair for the second period he devoted his first two addresses to a survey of the work of the S.P.R. since its beginning, an account of the modification which experience had shown to be necessary in the original plan of campaign, and an answer to certain criticisms to which it had been subjected from various quarters. His third presidential address of this period was explicitly concerned with the *Canons of Evidence in Psychical Research.*

According to Sidgwick the fundamental cause of the characteristic difficulty and controversy which attaches to psychical research is the fact that we are called upon to weigh one improbability against another. We have to balance the antecedent improbability of the events reported against the antecedent improbability that sane and respectable witnesses should be lying or should be deceived in relevant respects. Now there is no rule for estimating the antecedent improbability of such events as the reported physical phenomena of mediumship, hauntings, telepathy, clairvoyance, etc. We have no means of telling what proportion the facts that we know about the " habits " of matter and of mind bears to the facts of which we are still ignorant. As regards the reliability of witnesses and their reports there are some general rules. When normal people, with no obvious motives for deception, testify to quite *ordinary* events we do not raise questions about the probability of their testimony being false. But in the law courts, if the testimony of *two* such persons to a quite ordinary event conflicts, we do raise this question. For a precisely similar reason we must raise it when a witness, however intelligent and respectable, testifies to an *extraordinary event.* Now it is known that the main sources of error are the following : (1) alteration and heightening of a story that passes through a chain

of narrators; (2) errors of memory even in first-hand reports; (3) failure to observe relevant details and tendency to mistake inferences for observations; (4) lastly, if another person beside the witness was present he may have produced an illusion in the witness's mind. Therefore we have to consider (*a*) any facts about the observer which might tend to make him the victim of an illusion, and (*b*) any facts about the second person which make it likely that he was able or willing to produce an illusion in the witness.

In Sidgwick's opinion two important consequences follow. In the first place, it is plain that in every case the probabilities can be only vaguely estimated, and in many cases they must be estimated differently by different people according to their knowledge of beliefs about the character of the persons concerned. Therefore great and irreconcilable differences of opinion are inevitable, and it is useless to bewail them and unreasonable to complain of the slow rate at which the subject progresses.

Secondly, all talk of "crucial" experiments, "knock-down" proofs or disproofs, "completely water-tight" cases, is futile. We must make each individual experiment and report as "water-tight" as we can, and we must go on accumulating more and more such cases. Neither quality alone nor quantity alone will produce conviction; nothing will do so except a constant stream of cases in which the evidence is of the highest quality.

Since this is a matter about which there is still frequent discussion in our Society, and since Sidgwick seems to me to have said exactly the right things about it, I propose to state his views in rather more detail. First, as regards quantity and the need for a constant supply of fresh cases. In no single case, Sidgwick says, can the admissibility of normal explanations be absolutely excluded. This is impossible "even in the case of our own most conclusive experiments, when regarded from the point of view of the outside public. For all records of experiments must depend ultimately on the probity and intelligence of the persons recording them; and it is impossible for us or any other investigators to demonstrate to persons who do not know us that we are not idiotically careless or consciously mendacious." It is sometimes alleged that the S.P.R.'s demand for quantity shows that the quality of the available evidence is poor. This is not true. The quality of much of it is very good; it would be regarded without hesitation as conclusive if the alleged facts were not antecedently so improbable. The need for accumulating evidence is in order to swamp the antecedent improbability of the events to which it bears witness.

There are two other points which may conveniently be treated

under this head of quantity. The first is a warning which Sidgwick gave to the Society soon after the publication of *Phantasms of the Living*. Many members were inclined then, and I suspect that still more are inclined now, to regard the case for spontaneous telepathy as established, and to think that it is not worth while to trouble to collect and investigate fresh alleged instances of it. Sidgwick pointed out the extreme folly of this tendency to be at ease in Sion. Unless a fairly constant stream of well-attested cases is produced sceptics will certainly argue as follows : " On the basis of the statistics which you published in *Phantasms of the Living* and in the *Census of Hallucinations* there should be roughly so many fresh cases in England every year. If there were, you would, presumably, receive and publish a fair proportion of them. Since you do not, it is reasonable to suppose that they do not happen nearly so frequently as the Early Fathers of your Society alleged. And so the statistical case which they built up may be dismissed." No doubt it is *logically* cogent to answer " We don't get such cases, simply because our members, regarding spontaneous telepathy as an established fact, don't bother to look for them, or to investigate those which are brought to their notice, or to report those which they have noticed and investigated ". But, though logically satisfactory, this answer is quite useless for the practical purpose of convincing sceptics.

The other point which may, with a little stretching, be brought under the head of quantity of evidence is the following. Critics in the very early days of the S.P.R. constantly said, as they constantly say now, that no experimental result will satisfy them unless it can be reproduced at will in the presence of any number of sceptical observers. To this Sidgwick makes the obviously sensible answer that, whilst we should all be delighted to have evidence of this kind, we have no right to assume that it must be attainable. (To take an example from important physiological work which has been done since Sidgwick's day, it was only with enormous difficulty that Pavlov was able to secure such complete uniformity in the internal and external conditions of the animals on which he experimented that his experiments gave uniform results. And the presence of a stranger, or even of the experimenter himself, completely upsets the reaction of the animal, although this takes only the very simple form of salivation. It is at least as likely that thought-transference, if it happens at all, depends on a peculiar relation between agent and percipient which is very easily upset, as that the salivation of dogs in Pavlov's experiments is partly dependent on a whole complex of background conditions which can be kept constant only with great difficulty.)

We can now leave the subject of quantity and pass to Sidgwick's views about the quality of evidence which the S.P.R. should demand. He is perfectly clear that a mere accumulation of inferior evidence is of no use. In each single experiment, he says, " we have done all that we can when the critic has nothing left to allege except that the investigator is in the trick. But, when he has nothing left to allege, he will allege that." No evidence should be published until it reaches that degree of cogency. " We must drive the objector into the position of being forced either to admit the phenomena as inexplicable, at least by him, or to accuse the investigators either of lying or cheating or of a blindness or forgetfulness incompatible with any intellectual condition except absolute idiocy." These quotations are taken from Sidgwick's presidential address of July 17, 1882. He returned to the topic in his address of May 10, 1889, and I will quote one sentence from the latter. " My highest ambition in psychical research is to produce evidence which will drive my opponents to doubt my honesty or veracity. I think that there is a very small minority of persons who will *not* doubt them, and that, if I can convince them, I have done all that I can do. As regards the majority even of my own acquaintances I should claim no more than an admission that they were considerably surprised to find me in the trick."

There is one other remark made by Sidgwick which is worth mentioning because it still has constant application. In his presidential address of July 18, 1883, he refers to various normal explanations of ostensibly supernormal phenomena. After detailing these, and saying that every one of them is a *vera causa* which must be excluded before we can seriously consider any claim to supernormality, he proceeds to make the following highly pertinent remark. It is a very common fallacy to put forward a normal explanation which accounts very satisfactorily for nine-tenths of the phenomena of a certain kind, but fails to account for the remaining one-tenth which are equally well attested ; and then either to ignore this recalcitrant residue or to reject the reports of it and claim that one's normal explanation covers all the facts. As Sidgwick says, " It is not a scientific way of dealing with testimony to explain what you can and say that the rest is untrue. It may be common-sense, but it is not science." He cites as an example Faraday's well-known explanation of table-turning. This is a valuable explanation of *most* of the phenomena. But there are well-attested stories of tables moving *without* contact or rising *wholly* off the ground, and, if a single one of these is true, Faraday's theory does not fit all the facts. It seems to me that Sidgwick here puts his finger on a besetting weakness

of the late Mr Podmore, and that that distinguished member of our Society has left spiritual descendants who are with us at this day.

No account of Sidgwick's dealings with psychical research would be complete if it ignored the ethical and religious motives which influenced him in taking it up and in persisting with it. I will therefore conclude my paper with a brief account of this factor in his life.

We must begin by reminding ourselves that Sidgwick was the son of a clergyman, that he was brought up as an Anglican Christian, and that it was not until his twenty-second or twenty-third year that he finally abandoned the idea of taking Orders. Next, we must remember that reports of miraculous events play two parts in Christianity, one being absolutely essential and the other useful but dispensable. Christianity differs from most of the other great religions of mankind in the following way. An essential part of its doctrine consists in propositions about the nature and unique status in the universe *of its own Founder*. Therefore certain miracles, such as his reported resurrection and his subsequent appearances to the apostles, are *parts of Christian doctrine*, and not merely parts of the *evidence for* Christianity. Other alleged miracles, such as those performed by Christ during his ministry or by the apostles afterwards, are in a different position. If every one of them were rejected, no single doctrine of Christianity would need to be modified in the smallest degree. The importance of these miracles, if genuine, is evidential. The fact that Christ was able to perform them, and that he was able to convey to his apostles the power to perform similar, if somewhat less spectacular, miracles, is held to be strong confirmation of the Christian doctrine about his unique nature and metaphysical status. Now in England, when Sidgwick was a young man, enormous stress was laid on the New Testament miracles as evidence for the truth of Christianity. But it is alleged by followers of other religions that similar miracles have occurred in connexion with them, by Roman Catholics that such miracles have continued in their Church without cessation from apostolic times up to the present day, and by Spiritualists that they are happening here and now in mediumistic séances. These allegations placed the standard Protestant argument from the New Testament miracles in an extremely awkward dilemma for anyone as clear-sighted as Sidgwick. Either the New Testament miracles were unique or they were not. If they were unique, they would, no doubt, provide an unique support for Christianity against its rivals. But, in that case, the whole burden would ultimately rest on the trustworthiness of the stories recorded in the New Testament and the untrustworthiness of all the innumerable similar stories told in connexion with other religions and by

contemporary Spiritualists. If they were not unique, it might be much easier to accept them as rare but not unparalleled phenomena. But then they could provide no *special* evidence for the truth of specifically *Christian* doctrine.

Sidgwick's earlier struggles, like those of most of his intelligent contemporaries, were to disengage himself from the first horn of this dilemma. He had been deeply impressed in 1862 by reading Renan's *Études d'Histoire Religieuse*, and he set himself to learn Hebrew and Arabic in order to make a comparative historical study of religion. In a letter to A. J. Balfour written in 1897, describing his position in the sixties, he says that what he then wanted in theology was that the evidences for historical Christianity should be examined with complete scientific impartiality " as a duly instructed rational being from another planet (or, let us say, from China) would naturally weigh them ". Looking back, we can see that the result was a foregone conclusion. By the middle sixties Sidgwick had reached the position that no religion which depended on the correctness of historical statements about mysterious events in a foreign country and a remote period could possibly reach the evidential standard which he demanded. Writing to Dakyns at the end of 1864, he says " I have never before freed my innermost consciousness from the thraldom of a historical belief. Long after the belief had gone the impression remained that it was all-important to have a view on the historical question." Now he has reached the conclusion that there has been enough study of the Bible. What is needed is a comparative study of the mystical and of ecstasy ; the remote past being always subordinated to the present.

In 1869 Sidgwick resigned his fellowship, but he stated at the same time, in a letter to Benson, that he had no desire to leave the Church of England. In 1870 he published a book on *The Ethics of Clerical Subscription*. His mature views on this subject are clearly expressed in a letter which he wrote in May 1881 to J. R. Mozley in answer to a question which the latter had put to him. He said that a layman could conscientiously be a member of the Church of England, provided that he accepted the Apostles' Creed ; but he added that on one could be said to accept this honestly unless he accepted the miraculous birth of Christ. Anyone who proposed to *take an office* which involved membership of the Church of England would be dishonest in doing so if he doubted this dogma, and no bishop could dispense him. If, on the other hand, it were merely a question of taking part in the *worship and the sacraments* of the Church of England as a private individual, a much laxer standard was permissible, and a bishop's permission might be highly relevant.

The religious position which Sidgwick had reached by the age of forty-two is very clearly stated in a letter which he wrote in the summer of 1880 to Major Carey, an old Rugbeian friend. The following are the essential points. It is now long since he could imagine himself believing Christianity after the orthodox fashion. He is not, indeed, inclined to reject the miraculous as such ; but it is clear to him that, if you accept it in Christianity, you cannot reject it offhand in other religions or in modern Spiritualism. For many years past he has not thought of Christianity except as the creed of his friends, fellow-countrymen, etc. As regards Theism, he says " I don't know whether I *believe* or only *hope* that there is a moral order in the universe . . . a supreme principle of Wisdom and Benevolence guiding all things to good ends and to the happiness of the good. I *hope* this. I don't think it can be *proved*. No opposed explanation of the origin of the cosmos—e.g., the atomistic theory—seems to me even plausible. And I cannot accept life on any other terms or construct a rational system of my own conduct except on the basis of this faith." He recognises that his correspondent might well say " The question is, not whether you would *like* to believe in God, but whether the belief is *true* ". To this, Sidgwick says, he would answer by asking the following question : " What guarantee have you for the fundamental beliefs of science except that they are consistent and harmonious with other beliefs that we find ourselves naturally impelled to hold? " And he would continue to argue his case as follows. " This is precisely the relation which I find to exist between Theism and the whole system of my moral beliefs. Duty to me is as real as the physical world, though not apprehended in the same way ; but all my apparent knowledge of duty falls into chaos if my belief in the moral government of the world is conceived to be overthrown."

His position, then, may be summed up as follows. We cannot make an intellectually coherent system out of the data of sense-perception unless we interpret them in terms of certain general principles, such as the Uniformity of Nature and the Law of Universal Causation, which go beyond them and cannot be proved inductively from them. We cannot make an intellectually coherent system out of the data of our moral intuitions unless we interpret and supplement them in terms of Theism, which also cannot be proved from them. Now the latter data are as indubitable as the former, the demand for intellectual coherence is the same in both cases, and the principles required are not self-evident or capable of proof in either case. Logically and epistemologically there is complete parallelism. Either we have no right to make the postulate in either case or we have

an equal right to make it in both. The one position which cannot be defended is to make the postulates demanded by science, and then, in spite of or even because of this, to refuse to make the postulates demanded by morality.

Some further very interesting information about Sidgwick's religious beliefs and doubts is contained in the letter of May 1881 to J. R. Mozley, which I have already quoted, and in another letter of 1890-1891 to the same correspondent, who had showed him some letters from Cardinal Newman. In the former communication Sidgwick says that he is not prepared to admit that the experience of Christians that prayers for spiritual help are often answered is an adequate ground for believing in the objective reality of a sympathising and answering Spirit. He also makes the following important point. We must distinguish, he says, between the following three questions: (i) *Has* Christianity in the past been beneficial or indispensable to human progress? (ii) Is it so *now* and is it likely to be so in future? (iii) Is it true? He thinks that there is very little logical connexion between the second and third of these questions. He would be prepared to admit that, if it could be shown that Christianity *would always* be indispensable to human progress, this would be a *prima facie* ground for thinking it likely to be true. But he sees no reason to believe that it always will be indispensable; though he is inclined to think that, if a general belief in it were to break down now or in the immediate future, the results would be disastrous.

In the second letter to Mozley he sums up his attitude to Christianity as follows. Some form of optimism is indispensable for progressive humanity as a whole, though not for every progressive individual. The theistic form of optimism is the most attractive and intelligible for most people. There is no adequate rational basis for *any* form of optimism; and so the theistic form is, in this respect, no worse off than any other. He thinks that theism will survive, because it is needed; and, if it does so, it will be because of the support which it still obtains among Europeans from the traditional belief in Christianity. For his own part, Sidgwick says, he "has taken service with reason and has no intention of deserting". But he confesses that, if he yielded to his hankerings after optimism, it is likely that the last exercise of his reason would be to submit himself to the authority of the Church of Rome. If he were to submit himself to *any* external authority, he would have no hesitation, on broad historic grounds, in choosing the Roman Church.

One more quotation, this time from a note which Sidgwick wrote in 1895-1896 to Lord Tennyson about his father's *In Memoriam*, must be given as illustrating his reflexions at the end of his life on

the religious controversies of his early manhood. In the sixties, he says, men were absorbed in struggling for freedom of thought in the trammels of an historical religion. Now *that* struggle is over. "Freedom is won, and what does freedom bring us to? It brings us face to face with atheistic science. The faith in God and immortality, which we had been struggling to free from superstition, suddenly seems to be *in the air*; and, in seeking for a firm basis for this faith, we find ourselves in the midst of the 'fight with death' which *In Memoriam* so powerfully presents."

We have now before us a fairly clear picture of Sidgwick's religious struggles and their outcome. Most intelligent and conscientious Englishmen of his generation went through similar struggles, but hardly any of them came out by the same gate as Sidgwick. Some took the path which Sidgwick tells us he was tempted to follow, and elected to "open their mouths and shut their eyes" and swallow whatever the Pope might give them. A few, such as Frederic Harrison, who wanted the jam of Catholicism without the powder of Christianity, joined the Comtist Church, at one of whose reunions Huxley found "three persons and no God". Many more, of whom Huxley himself and W. K. Clifford were the most distinguished examples, found spiritual satisfaction in a kind of revivalistic Agnosticism accompanied by much vigorous banging of the ethical tambourine. (Clifford's solemn excommunication of the eminent scientists who wrote the *Unseen Universe*, and the exquisitely pompous *ex cathedra* pronouncement "The world is made of atoms and ether, and there is no room in it for ghosts", may still be enjoyed as perfect examples of what Jeremy Bentham called "nonsense on stilts" and may still be used as warnings against attaching too much weight to the pontifications of contemporary scientific pundits.) Others, again, contrived to muddle themselves into a kind of Hegelian Christianity, in which everything turned into its opposite, and Materialism and Mentalism were resolved into a higher synthesis in the glow of which one felt it to be crude and ungentlemanly to raise concrete questions about historical events and contemporary phenomena.

It is noteworthy that all these exits, except the ever-open *atri janua Ditis* which Sidgwick was tempted and declined to take, are now utterly out of date and closed to contemporary men. It is inconceivable that any intelligent and instructed Englishman at the present day should find a solution of his religious or philosophical perplexities in Comtism, in revivalistic Agnosticism, or in Hegelianised Christianity. This suggests that Sidgwick's insight was deeper and his purview wider than those of his contemporaries, since

they prevented him from accepting those solutions which satisfied so many of them and which can now be seen to have been delusive. But, it may be said, Sidgwick's own attitude (it cannot be called a "solution") is just as much dated and just as impossible for ourselves as the alternatives which he rightly rejected. I shall make no attempt to discuss this very large question as a whole; but I will conclude my paper with some remarks on that part of it which is specially relevant to our subject, viz., the connexion between Sidgwick's religious and philosophical position, on the one hand, and his interest in psychical research on the other. In what follows I shall be stating my own opinions, and I shall be doing so dogmatically for the sake of brevity; but I believe that Sidgwick would have accepted them, in outline at any rate, and that a good case could be made out for them.

The physical data supplied by normal sense-perception and the mental data supplied by the introspection and observation of normal waking persons are the bases on which the whole system of natural science, including psychology, is built. In this vast coherent system there is not a single fact to suggest that consciousness ever occurs except in intimate connexion with certain highly specialised, complex, and delicate material systems, viz., the brains and nervous systems of living organisms. There are innumerable facts which show that, during the life of an organism, the nature and degree of consciousness associated with it vary concomitantly with the general health and the special physiological processes of that organism. If we confine our attention to this aspect of the case, we receive an overwhelming impression that consciousness is utterly and one-sidedly dependent, both for its existence and for its detailed manifestations, on brains and nervous systems and on processes in them.

When the philosopher comes to reflect on what the scientific specialist tends to ignore while he is engaged in his professional business, viz., the fact that the human individual is not only an object to be observed but is also the experimenter who devises tests and deliberately carries them out and the theorist who speculates and infers, he finds it very difficult to fit the two aspects of the whole into a single consistent picture. He also notices that no scientist, even when occupied in doing his professional work, ever regards himself or his colleagues for an instant as "conscious automata". Plainly there is some very thin ice with some ominous cracks in it.

Still confining our attention to perfectly normal phenomena whose occurrence no one would question, we notice that each of us, beside perceiving, acting upon, and thinking about the external world of

material things, is constantly engaged in reflecting upon the actions, thoughts, desires, and emotions of himself and his fellow-men, and in making judgments about them which involve such predicates as "right" and "wrong", "good" and "evil", "valid" and "invalid", "true" and "false". Such judgments are organised into more or less coherent systems with characteristic principles of their own, which are formulated in ethics and in logic. These facts about human nature are not particularly easy to unify with that "conscious automaton" view of it which seems to be forced on us when we confine our attention to the aspects which are studied by the natural sciences.

At this stage we may, perhaps, be emboldened to put the following question to ourselves. Need we feel the slightest surprise at the palpable inadequacy of the account of human nature supplied by natural science? Natural science is concerned *only* with the data of human sense-perception, and, for most purposes, only with the data supplied by the two senses of sight and touch. Even within this extremely limited region there is a further selection. It deals only with a normalised extract from the visual and tactual sense-data of normal waking men. Quite rightly, for its own purposes, it ignores the peripheral and the abnormal, the sense-data of dreams, of delirium, of persons under hypnosis, and so on. Since men are not merely sensitive beings, and since their peripheral and abnormal sense-data are just as genuine as their central and normal ones, why should one expect that an account of human nature based exclusively on this extract from an extract will be adequate? Echo, so far as I can judge, answers Why?

So far, it seems to me, we can get without going beyond common-sense reflexion on universally admitted facts and without making any appeal to ideals or emotions. The next step is as follows. We all know that in the past claims have been made by various persons to have had *supernormal* experiences, in which they either gained knowledge of ordinary facts under extraordinary conditions or had revealed to them facts about the nature and destiny of mankind which could not be known by ordinary means. We also know that such claims are made by or for some of our contemporaries. If such claims related solely to the remote past, and if there had been no independent reason to question the adequacy of the account of human nature based on natural science, these stories might reasonably have been dismissed with a smile or a sigh. But, in the actual situation, there is a clear call to investigate such claims with scrupulous care when they are made by those of our contemporaries, who cannot be summarily dismissed as knaves or fools. If any such

claims by them should survive investigation, we may have to view certain stories about the past with a less sceptical eye.

Suppose, now, that we should find, as a result of our investigations, that some at least of the claims to supernormal knowledge of ordinary facts are valid. This would not, of course, have any *direct* tendency to show that any human mind existed before or will exist after the death of its present body. Indeed, as members of the S.P.R. know full well, it may weaken the force of arguments for that conclusion based upon mediumistic communications. Nevertheless, it would have an *indirect* bearing on the question of pre-existence and survival. For a great part of the difficulty of any argument in support of this conclusion is the enormous weight of antecedent improbability which it has to overcome. Now this antecedent improbability is largely dependent upon the belief that every known activity of the human mind in life is correlated point to point with some process in the brain and nervous system. If the occurrence of extra-sensory perception were established, we should have positive empirical grounds for doubting this assumption. Our view of the nature of the human mind and its relation to its organism would be profoundly modified, and this modification might well reduce the antecedent improbability of its existing in the absence of its present body.

We come now to the last step, and here, for the first time, there is a reference to ideals and aspirations. It seemed to Sidgwick, and it seems to me, that, unless some men survive the death of their bodies, the life of the individual and of the human race is "a tale told by an idiot, full of sound and fury, signifying nothing". I cannot understand how anyone with an adequate knowledge of physics, biology, psychology, and history can believe that mankind as a whole can reach and maintain indefinitely an earthly paradise. Such a belief is a sign of amiability in the young; but of imbecility, ignorance, or wilful blindness in the mature. I am not suggesting for an instant that survival is a *sufficient* condition of any great good; it is obvious that the world might be infinitely worse with it than without it, as it would be if, e.g., the majority of men survive only to be tortured unendingly in Hell. All that I maintain is that it is a *necessary* condition if the life of humanity is to be more than a rather second-rate farce. I do not desire to survive myself; so far as I can judge, it would be an immense relief to me on purely selfish grounds to be assured of mortality, and I am not altruistic enough to bother very much about the fate of the rest of the human race in my absence. But Sidgwick was a man of noble character and high ideals, with an overmastering sense of duty and the courage to face suffering and unpopularity in doing what he believed to be

right. That such a man should strongly desire survival for himself and his fellow-men, as the *conditio sine qua non* of the seriousness and worth of human life, was inevitable. And it was natural that, in desiring it, he should seek for evidence of it in the one corner in which it seemed to him that evidence might still conceivably be found.

I suppose it is inevitable that some clever fool should triumphantly remark that the fact that Sidgwick approached the subject from this angle and with these desires discounts the value of his work in psychical research. It should be a sufficient answer to point out that in fact Sidgwick reached a purely negative conclusion about the evidence provided by psychical research for human survival. And, if I may pass from the particular to the general, I would conclude with the following observation. A conscientious and critical person who realises the immense importance of human survival is much more likely to weight the scales against *prima facie* evidence for it than to accept such evidence lightly. His desire that it may be true, and that it may be proved to be so, will indeed make him persevere and constantly return to the attack after each set-back and disappointment. This effect it did have on Sidgwick. But he will be so anxious lest his desires should trick him into accepting fairy gold that he will be in some danger of rejecting real gold if it should ever be offered to him.

SOME RECOLLECTIONS OF HENRY SIDGWICK

By Lord Rayleigh, F.R.S.

[These recollections, written some time ago, have little direct reference to psychical research. But, after seeing Professor Broad's paper, it occurred to me that they might help to make up for the lack of personal contact with Sidgwick which he regrets. I am glad to have the concurrence of several prominent members of the Society's council in this view.]

Henry Sidgwick was my uncle by marriage; and he was a familiar figure to me from my earliest years. But sympathetic though he was, I do not think that children particularly appealed to him or he to them. I never heard whether it was a disappointment to him to have had none of his own. The only incident that comes strongly back to me is that we thought his stammer a grown-up affectation and one to be imitated: which we did to his face!

It was, however, when I went up to Cambridge as an undergraduate that I first really got to know and appreciate him. Mrs Sidgwick was at that time principal of Newnham, and they kept house in a flat there. It was always open to me, and I dined with them alone there several times a term. Mrs Sidgwick was by temperament a sympathetic listener rather than a talker, and she often fell into this rôle while he and I discussed things. No doubt at first my ideas were too crude, and my stock of knowledge too small to draw him out to advantage. But this partly mended itself with lapse of time.

Moreover, one of his salient characteristics was to make the very most of whatever companionship circumstances might offer. Thus, at the time I am speaking of, Mrs Sidgwick was "at home" once a week at tea time to the Newnham students. Many of them were extremely shy, and to keep the conversational ball rolling was not altogether an easy matter. Sidgwick was conscious of this, and said that when there was a difficulty in finding a conversational opening with a young lady, the longer the pause the more important he felt the remark ought to be, so that this embarrassing silence tended to be maintained indefinitely. Outside observers, however, formed a very different impression. As soon as Sidgwick entered the room it seemed to them that any embarrassment vanished like smoke. The group within range of him at once became animated. Everyone was at ease and expanded quite naturally with a sense of their own (usually latent) social gifts.

The secret of his success was in great part that he got really interested in the topics which interested his companion and entered into them with zest, however remote they might be from his own pursuits. Thus about the time I am speaking of (1894), the safety bicycle with pneumatic tyres was a novelty and became a fashionable craze. Many guests brought their bicycles for week-end parties. Talk at country houses often turned on the different makes of bicycles, and their constructional details—not that these were discussed with any real engineering knowledge. Sidgwick was not a bicyclist himself, and I remember him saying that he felt left out in the cold when such topics came up : could anyone recommend him a book which would supply these deficiencies?

This trait of universal interest in whatever interested other people was so striking that it once led me to ask him whether anyone ever bored or irritated him. " Well, yes," he said, " if I make my remarks and cannot get anything in reply there is a temptation to that."

The subject of priggishness came up, and Sidgwick committed himself to the statement that he had never met a prig! I attempted to shake this position by citing a fellow undergraduate whom I had heard holding forth in the Sidgwick's drawing-room a few days before in a way that seemed to me singularly priggish. But he would not have it, and insisted that he had enjoyed the youth's conversation enormously.

These were various aspects of his tendency to make the best of people. He was temperamentally anxious that everyone should have a hearing. I implied once that some scientific heretic had not established a claim to be heard. Sidgwick was annoyed by my attitude, which I daresay was crude enough, and replied in a tone which for him was rather severe : " He asks for attention, not to his authority, but to his arguments." The same spirit showed itself when he received from some paradoxer an elaborate attempt to upset the Newtonian system of mechanics. Anyone of note in the academic world is likely to receive communications of this kind occasionally, and the reply is usually (I imagine) of the briefest. In Sidgwick's case the subject was quite outside the line of his own studies, and this circumstance would have given him a perfectly satisfactory and courteous line of defence. But he did not avail himself of it. He asked a distinguished Newnham mathematician to comment on the letter in detail, and forwarded her observations. We may feel pretty sure, however, that his trouble was wasted. Anyone who was sensible enough to profit by a reply of the kind which he sent would hardly have failed to realise that his views were worthless before he submitted them to strangers.

It was during the time when I enjoyed the privilege of his intimacy that Christian Science first began to be talked of. His attitude towards it was one of sympathetic enquiry. That attitude was not (and is not) common in academic circles ; and Cambridge was somewhat scandalised to learn that a lady who had won very exceptional academic honours had become a convert. Sidgwick was, as is well known, a protagonist of the Women's Movement, but it had many opponents ; and one of them attacked him in conversation, pointing to what he considered to be the collapse of one of its boasted successes. Sidgwick was proud of his reply. " Are you aware ", he said, " that the President of the British Association is a Christian Scientist? "

Sir Douglas Galton, the eminent sanitary engineer, was here referred to. Sidgwick attempted, either personally or through the Society for Psychical Research (I am not sure which) to elicit from him some detailed statement of his views ; but without success. Galton replied to the effect that the matter was too personal and sacred to him to admit of discussion in an academic spirit.

This brings me to the subject of Psychical Research.

When the *Memoir of Henry Sidgwick* by A. S. and E. M. S. appeared in 1906, some reviewers noticed that in the part of the book dealing with his later years comparatively little reference was made to psychical research ; and they drew the conclusion that Sidgwick had become disillusioned on this matter, and regretted the time and attention he had given to it. Such was by no means the case. His interest was undiminished, though like other workers in this direction he probably felt that he was not destined to see the problems he had worked at finally resolved. His conviction of the reality of telepathy was the principal definite fruit of his labours. In saying this I am of course speaking from his own subjective point of view. The consensus of learned opinion has certainly not reached this point, though (unless I am mistaken) it is slowly approaching it. It may be remarked in passing that a revolution in thought of this kind is not usually, if ever, accomplished by the pundits of the older generation changing their minds ; but rather by their passing away, and being replaced in the seat of authority by men of the next generation who think differently.

The most discussed psychical topics at the time I have been writing of were the physical phenomena occurring in the presence of the Italian medium, Eusapia Paladino. Sidgwick's great gifts were not such as to qualify him particularly for experimental investigation. He would doubtless be as strong as anyone on the

purely logical side ; but experimenting is a craft, and the physical phenomena in question require for their investigation a kind of cunning in the devising of practical expedients, and an intuition as to the possibilities of deception, which he knew well were not his. His bent was not in any degree mechanical.

I never personally had the opportunity of seeing one of Eusapia's séances. She was entertained at F. W. H. Myers's house at Cambridge, and was, by all accounts, a somewhat difficult guest. It was found important to keep her in a good humour, and Sidgwick took much of the burden of this on his shoulders. Her only language was Italian, and he struggled manfully to make the most of such knowledge of it as he had.

The result of the Cambridge séances was disappointing, and most, if not all, of the experimenters were convinced that systematic fraud was practised by the medium. They themselves were not unprepared to meet guile with guile. Dr Richard Hodgson, an experienced investigator who was skilled in the resources of trickery, was introduced by Sidgwick as " an old friend ", and without explicit statement the idea was subtly suggested that he was valued more in that capacity than for his brains. In this way the medium was put off her guard, with illuminating results.

No more was heard of Eusapia for a time. But presently reports of further marvels occurring in her presence reached this country from the Continent. I remember being present one day when Frederic Myers came in full of eager interest in these, and anxious to resume experiments with her. But his enthusiasm met with a cold douche from Sidgwick. After listening at length to what Myers had to say, he gave his verdict. " I cannot see any reason for departing from our deliberate decision to have nothing further to do with any medium whom we might find guilty of intentioned and systematic fraud." Myers, whose eagerness had by that time been considerably cooled, found little to say in reply.

One of Sidgwick's traits was a pronounced anti-militarist tendency. When after the Omdurman campaign, Lord Kitchener came to Cambridge to receive an honorary degree, some enthusiastic young woman said that he was her hero. When Mrs Sidgwick mentioned this, Sidgwick remarked that he did not think it heroic to mow down savages with machine-guns—it might be necessary, but that was the best that could be said of it. He was not tempted to think of himself as a man of action, " Knowing ", he said, " that I have no physical courage, I always hope that I have moral." His own estimate of his physical courage need not however be taken at its face value.

During the Boer War his attitude certainly verged on the anti-patriotic. He considered the action of this country indefensible, I think on the general ground that the Boers had retreated to the Transvaal in order to get away from British rule, and that if British subjects had followed them there they did so at their own risk, and must put up with such legal and political status as the Boer government chose to accord. I do not remember how he dealt with the rather technical questions about British suzerainty which were involved. Mrs Sidgwick did not see eye to eye with him on this subject, and when he discussed it she was sometimes perceptibly irritated—a rare event indeed with her.

As the campaign proceeded, with very indifferent success to the British army, he took a gloomy view of the ultimate prospects. The following entry in the visitors' book at Terling bears witness to this, and was probably the outcome of a somewhat heated discussion :

Christmas 1899.

" Edward Strutt bets Henry Sidgwick a thousand to one in pennies that there is not an independent Dutch Republic in South Africa within 5 years.

(Signed) Henry Sidgwick.
Edward Strutt."

I do not think Sidgwick went so far as to wish ill-success to the British armies. Certainly he did not, like his friend Oscar Browning, speak of " us " meaning the Boers.

Sidgwick not infrequently told anecdotes about the mentality of his childhood, and the general impression they gave was that he must have combined an earnest sense of duty with complete trust in what he was told by superior authority.

Thus he used to tell the story of how he began to learn Euclid. Impressed with what he had gathered from his immediate seniors of the difficulty of the subject, he determined to grapple with them before it was officially required of him and learnt several propositions by rote, apparently without any conception of what they meant. When his teacher discovered this and explained to him that that was not the way to learn Euclid, he burst into tears at the thought that all his effort had been wasted.

He used to dwell on the rude shock he had received when he learnt that " Benefit of clergy " had meant in effect that clergy might commit crimes with impunity for which laymen would be punished.

Sometimes, too, he would talk of his undergraduate days, but I am ashamed to find how small are the gleanings which I can now set down. He would tell how on one occasion reading mathematics

and classics alternately (he was reading for a double degree) he succeeded in getting in fourteen hours of work in the day. "But", he said, "I could not resist the temptation to spend the next day going round to my friends and telling them about it, so that the average was soon reduced to seven hours."

I gathered from this and other hints that his method of reading had not been of the severely methodical order, with rigidly fixed hours and unflagging attention. "You were with your books," he said. "If you were tired of reading you smoked a cigarette or looked out of the window."

Then there was a glimpse of the formidable Dr Whewell, Master of Trinity, on his way to the university sermon. According to his theory it was the duty of members of the university to attend it, and chancing to meet Sidgwick going in the opposite direction he gave expression to his views. "What did you do?" I asked. "I was not prepared to surrender my liberty of action," replied Sidgwick, "but I tried to look impressed."

There were many stories current in Cambridge of Sidgwick's *bons mots* in conversation. It was alleged, but upon the whole not credibly, that he used his stammer intentionally to give dramatic effect to them. Certainly however it was no disadvantage, for the point gained by being waited for with a moment's suspense. It is greatly to be regretted that there was no Boswell to record his sayings—and the attempt to collect them after so many years cannot have much success. However, here are a few.

It is related that the characteristics of Canon (afterwards Bishop) G. F. Browne were under discussion. Someone remarked that he was not open to the reproach of losing his temper.

Sidgwick: "No, b-b-but he rather obviously keeps it."

Sidgwick on Cambridge values, told me by the late Bishop Charles Gore: "If you want to stand really well with Cambridge, three things are necessary: that you shouldn't be known outside the University; that you should not know anything outside your own subject; and that you shouldn't write your own language gracefully."

It must not be supposed from these examples that he ever allowed himself to be cynical or unkind. They perhaps represent the limit of severity which he allowed himself, unless he felt that condemnation was really called for.

I remember once suggesting as a psychological experiment telling a story without any point to see how people would take it (the suggestion was doubtless crude and stupid, but it must be remembered in excuse that I was only a boy).

Sidgwick: "I have tried it. I remembered when I was half-way through the story that the point was at the expense of a near relation of someone who was there. So I left it out. But they laughed all the same."

Self: "You showed great presence of mind."

Sidgwick: "No, I cannot claim that. If I had invented another point, *that* would have been something to be proud of."

As a matter of fact, however, his readiness was extraordinary. A good example of it is told me by Lord Balfour. Sir William Harcourt lost his seat at Derby at the general election of 1895, and one Green-Price offered, or was said to have offered, to retire in his favour.[1] On this being read out across the breakfast table, Henry Sidgwick remarked immediately, "Well, it remains to be seen which half of his name will be justified this time."

Again, when some purist queried the word *reliable* (perhaps on the ground that it should be *rely-on-able*) Sidgwick remarked: "The prejudice against the word is unaccountable and even laughable: for the word, though not indispensable, should be generally available."

Apart from his spontaneous sayings, which were often so good that they were widely repeated, Sidgwick keenly enjoyed hearing or telling a good story. He was poles apart from the superior person who only sees in such a "chestnut". I give one that I have heard him tell, partly for its own sake, partly because it may help to give an idea of his lighter side.

"The best example of spontaneous wit I ever heard", he said, "was in the Trinity combination room, when a senior fellow was talking in a rather extravagant way, and turning round to one of his juniors said, 'My young friend here will bear me out.' 'Yes', he replied, 'Certainly I will. There is a good precedent for it.' We are told that the young men bore Ananias out!"

At times he did not hesitate to descend to elementary forms of humour. He would repeat a good limerick with keen relish. One story that he used to tell against himself ran thus. He had formed a good resolution that when an idea or inspiration occurred to him, he would not let it slip by, but would write it down at once, so that it should not be lost. In the night he thought he had such an inspiration. Conquering his tendency to slumber, and his natural reluctance to leave the warmth of his bed, he got up and made a note of it. In the morning he remembered the incident vaguely,

[1] The offer does not appear to have been accepted, since he took a seat vacated by Cornelius Marshall Warmington, K.C.

and looked eagerly to see what he had recorded. He read as follows :

Sometimes on one leg,
Sometimes on two.
Something to think of,
Something to do!

He thoroughly enjoyed the theatre, which always seemed to put him in excellent spirits. I remember walking through Piccadilly Circus with him after a performance, and I made some unfavourable comment on the moving illuminated signs displayed there. But he would not agree and maintained that " They add a distinct pleasure to life."

His autumn holiday was usually spent at his brother-in-law's home at Whittingehame in East Lothian. In the circle there his views were always eagerly listened for on any domestic or foreign crisis in national affairs : they were sure to be ingenious and original, even if they did not carry complete conviction.

In the afternoons he appeared in another aspect. Garden golf was in fashion, and though only a very moderate performer, he got wildly enthusiastic and skipped about like a child in his excitement over the game. His long grey beard and the clerical wideawake hat which he always wore made the effect the more ludicrous. Billiards, too, he enjoyed enormously, though he was incredibly bad, having no notion whatever of the game, but no false shame about his incompetence.

It was over a game of lawn tennis, however, that I saw him for the only time momentarily angry. There was some question about whether a ball served was a fault or not. Sidgwick said that he saw the dust fly (from the whitewashed line). His opponent, not clearly apprehending the conclusiveness of this evidence, or perhaps not understanding what had been said maintained his position. Sidgwick may have thought that his veracity was questioned. His eyes flashed with anger, and he said, " Very well, but I shall avoid playing with you again." But he instantly accepted an apology, and I am sure never gave the matter another thought.

I say that this is the only time I saw him angry ; but though I never happened to witness it, I know that any selfish attempt to encroach on public rights moved him strongly to anger. Witness the following told by himself. He was sitting in a railway carriage and two persons of the opposite sex got in. One said to the other in a loud aside, " I am sure the gentleman will have too much good feeling to smoke in the presence of ladies." " Are you aware ", said

Sidgwick, " that this is a smoking carriage? " and the attitude being maintained he deliberately took out a cigarette and lit it as a protest.

Sidgwick's studies in metaphysics and ethics were quite outside my ken, and I knew little of Political Economy. I was, however, contemplating some study in the latter for the Trinity Fellowship examination. Candidates were allowed to name the books on which they wished to be examined, and I asked Sidgwick's advice. He discussed the matter at some length, but somewhat undecidedly. Several books were suggested or dismissed. At last I said, " You have not mentioned your own book." The reply was characteristic. " Well, no, I have recently been going over it for a new edition, and the truth is that I find it so very dull that I cannot honestly recommend it.(!) "

As I recall this conversation I picture his study at Newnham, a smallish square room with walls entirely covered with books. There was a desk at which he would stand upright, reading or perhaps correcting proofs. The pendant electric lamp had been conveniently brought over it by an extemporised arrangement in which Mrs Sidgwick's hand might be traced—one knew instinctively that his was not the mind that had conceived it. The writing table at which he sat was covered with an incredible quantity of papers in disarray. This accumulation periodically overflowed into the drawing-room on the other side of the passage, where it was deposited on the writing table designed for the use of visitors. At this stage Mrs Sidgwick usually took action and a clearance was effected with her help. Apropos of this Sidgwick recalled a discussion which he had heard on how a murderer should dispose of the corpse of his victim. " I should put it among the papers on Sir ——'s writing table " was the suggested solution. " I think there would be cover for a small corpse on mine," he said.

Social intercourse and the exercise of hospitality were to Sidgwick keen pleasures. At the same time he had a definite ethical objection to luxurious expenditure. He used to tell how, at one time, he had under the influence of this feeling, severely simplified the entertainment at his dinner parties, cutting off the champagne or other expensive wine, and generally reducing it below the prevailing standard. But an unforeseen difficulty arose. He felt the need under these circumstances of making it up to his guests by added conversational brilliance ; and the strain of this weighed so heavily upon him that he abandoned the effort and went back to the champagne! I think as a matter of fact that he appreciated good things and enjoyed them when he could do so without a feeling of personal responsibility. The cooking at Newnham was not above criticism,

and I have known him goaded into strong protest on the subject. Mrs Sidgwick said practically nothing in reply.

His ethical scruples had no application in cases when he was clear that comfort added to efficiency. I asked if he was in the habit of taking a sleeping car south from Scotland. "Yes," he said with decision, "I always take one. Life is not long enough to allow one to waste the next day by want of rest."

As may be read in detail in the *Memoir*, Sidgwick was a leader in the Women's Education Movement in Cambridge, Newnham College having been in large measure his creation. In 1895–6 it was felt that the time had come to move in the direction of securing the degree for women; it was found in practice (though the opponents of the movement could not or would not believe it) that the lack of a titular degree put women who had passed the standard at Cambridge at a disadvantage compared with women from other universities to whom the titular degree was granted. However, the attempt was not at that time successful. Party feeling ran high on the subject. Among other incidents a committee of undergraduates approached the Vice-Chancellor and asked whether they might present a memorial against granting the degree, and they were given a favourable answer. Sidgwick was much incensed at this. When the subject came up in conversation, and it was suggested that the undergraduate leaders were unjustified in interfering, I remarked that it seemed to me that they were whitewashed by the official reception of their memorial. Sidgwick was silent. When appealed to he said, "I quite agree. The undergraduates ought to have been told it was not their business. If I were to say publicly what I think, it would not conduce to peace. In a few years I shall resign my Professorship and then I shall say some plain words on that and other subjects." The failure of the movement was a deep disappointment to him, and he said wearily that he intended to give up trying to influence university opinion. The work was too much uphill.

Although in this particular matter he was a strong partisan, he was sometimes criticised for sitting too much on the fence. Thus, at a committee meeting on some question of university administration, the chairman observed him twisting his beard and showing signs of mental activity, and said, "Professor Sidgwick, do you wish to say anything about this proposal?"

Sidgwick: "I was p-p-pursuing a train of thought which might lead to an objection."

Sidgwick's sympathetic and unselfish nature showed itself in many directions, but nowhere more strongly than in his interest in the

hopes and aspirations of the younger generation. Any freshman who through old family friendship or otherwise had a claim on his notice was sure to be asked to dine, and cordially received; and if in any case the friendship failed to ripen, the fault certainly was not Sidgwick's.

He had, however, a rather unfortunate shortcoming in never recognising a passer-by in the street, and this was often misunderstood. Many people assumed that he had decided that conventional greetings were tiresome and that it was better to avoid them. However, I reproached him on one occasion with never recognising me, and he clearly failed to realise that there was any truth in the charge.

He always seemed to estimate the capacities of his young friends on a generous scale; and if they seriously failed to make good when it came to the ordeal of a Tripos examination, it was a shock to him. His natural reaction in such a case was to consider whether the university system was not at fault rather than the young man whom it had judged adversely. Perhaps this was partly due to his instinctive sympathy with the weak when at issue with the strong, partly to the vein of unorthodoxy that ran through his whole character.

I do not remember to have discussed religious beliefs with him, but his views are set forth in the published *Memoir*. A few casual gleanings may be mentioned. I have heard the present Archbishop of Canterbury quote in an after-dinner speech his remark: "Bishops individually represent everything that I find most agreeable; collectively, everything that I most detest." That (it seems) was what he thought of bishops, and it will serve to introduce what a distinguished bishop—Charles Gore—thought of him. He spoke of Henry Sidgwick as coming as near as any man he knew to the character described in the text "Blessed are the pure in heart."

Asked whether he regarded Sidgwick as a Christian, he said he did, and referred to the passage in the New Testament[1] on the two brothers, one of whom said, "I go" and went not, and the other said, "I will not go" and went.

In the summer term of 1900 I was in residence at Cambridge as usual, working in the Cavendish Laboratory. I dined with the Sidgwicks quietly once or twice at the beginning of term, but then followed a long interval without my hearing from them. It crossed my mind as a little odd, but I knew of no reason to attach special importance to it. Doubtless, I thought, they have not happened to

[1] St. Matthew xxi. 28–30.

have a suitable day free. Then came a letter from Mrs Sidgwick asking me to come and see them on an appointed afternoon. I did so, and Mrs Sidgwick told me, simply and bravely, the dreadful news. Sidgwick had had symptoms to which he had not attached special importance, but (I think at her insistence) had consulted a specialist, who pronounced that he had a mortal disease. An operation would be necessary almost immediately, but could not avert the ultimate result. Possibly a year or two of invalid life might remain in which he might be able to wind up the literary work which he had on hand.

I saw him for a moment after the interview with her. There was not a shade of difference from his usual cheery manner.

"Well, you have heard how it is with me," he said. "You see I believe in science; I am submitting to this operation though I feel perfectly well."

The next and last time I saw him was a month or two later at Terling, my own home. He had gone there to recruit, as was hoped, after the operation. But the result was far otherwise. I was sent for one day to his room. The change in him was terrible, and it was evident even to my inexperienced eye that the end could not be far off. He said that he had not strength for a long interview, but that he had sent for me because he wished to hear how my scientific work was going on. I told him what I could with a feeling however that the insignificant details of my efforts were quite out of place in face of the awful change that was impending. But it was like him to be thinking of the intellectual interests of another even at that time. He wished me good-bye, and hoped for my success in the immediate object of my ambitions. A few days later he was gone. He lies in the family corner in Terling Churchyard.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 157

THE CONTRIBUTIONS OF PSYCHICAL RESEARCH TO PSYCHOTHERAPEUTICS [1]

By T. W. Mitchell, M.D.

From the earliest times it has been believed or suspected that in the treatment of disease something more than physical or material means is necessary or desirable. Attempts to cure bodily or mental disorders arose in connexion with religious and magical practices. When primitive man was stricken with illness he cried for some divine intervention or some magical gesture that would restore him to health. It was thought that all disease was due to the malign influence of gods or devils and could be counteracted only by prayer and propitiation, or that it was due to the agency of less powerful spirits or other human beings whose evil influence could be annulled by magical rites. The world of primitive man was a world of spiritual agencies which had to be reckoned with in every form of human experience, and it was long before the concept of physical causation in a natural world supplanted the supernaturalism inherent in animistic beliefs.

Up to the eighteenth century the occult influences operating in the cure of disease were always regarded as being of a spiritual nature, but in the Age of Reason men sought more and more for some material or physical source of all such influences whatsoever. But as no likely source could be found on earth men's minds turned back to one of the oldest of beliefs, namely, belief in the influence of the stars on human life. In 1765 a young Viennese student of medicine, Franz Anton Mesmer, wrote a thesis for his Doctor's

[1] This paper was read at a Private Meeting of the Society on October 27, 1938.

degree, on " The Influences of the Planets on the Human Body ". He thought that the heavenly bodies exerted a direct influence on all living creatures by means of a subtle fluid which emanated from them. This fluid he considered to be of the nature of magnetism and for a time he used magnets in the treatment of his patients. Later he believed that a similar influence could be transmitted through his own touch or even by passing his hand over the patient without contact. The force which he believed emanated from his own body in this process he called Animal Magnetism. Strange effects and marvellous cures were brought about by his methods, and he soon found himself opposed by the medical profession and denounced as a charlatan by the whole world of science. But his views and his methods were adopted and successfully applied by his disciples, and to be treated by animal magnetism or mesmerism, as it came to be called, was for a time a fashionable craze.

When James Braid, a Manchester surgeon, in 1845 showed that the mesmeric state could be brought about without the use of any so-called mesmeric passes, and without any belief in a magnetic fluid. the way was opened for the rise of what we now know as the Theory and Practice of Hypnotism. Braid got his first results by asking the patient to fix his gaze on some bright object held in front of his eyes, and when this process was found to induce sleep Braid thought that this was due to neuro-muscular fatigue caused by the fixed gazing. Later he came to realise that the essential feature in his method was the suggestion of sleep implicit in his technique. But the full development of the theory of suggestion as the essential factor in the induction of hypnosis and its accompanying phenomena was the outcome of later researches, mainly those of Liébault and Bernheim, the founders of the so-called Nancy School.

Although we now know that Braid discovered in suggestion what is believed to be the essential factor in the induction of the mesmeric state, acceptance of his teaching was very tardy. His views were opposed, not only by many medical men but also by the mesmerists ; they would not admit that the hypnotic state was the same as the mesmeric state. Moreover they maintained that what were called the " higher phenomena " of mesmerism, e.g., clairvoyance, could not be obtained by Braid's methods.

The whole subject of mesmerism and hypnotism and their relation to one another remained confused and controversial for many years. The old belief that magnetism had some connexion with the phenomena lasted for a long time, and even as late as 1880 Charcot was using magnets to produce effects of various kinds on his hysterical patients. It was in an age of mingled scepticism and credulity con-

cerning these matters that the Society for Psychical Research was founded in 1882. The founders of the Society thought the time opportune " for making an organized and systematic attempt to investigate that large group of debateable phenomena designated by such terms as mesmeric, psychical, and spiritualistic ". A special committee was formed for " the study of hypnotism, and the forms of so-called mesmeric trance with its alleged insensibility to pain ; clairvoyance, and other allied phenomena ". On this committee we find the names of Edmund Gurney, Frederic W. H. Myers. Frank Podmore, Sir William Barrett, and other less well-known men. Most of the research work of this committee was inspired and carried out by Edmund Gurney.

To appreciate fully the importance of Gurney's writings on hypnotism it is necessary to bear in mind the attitude of men of science towards hypnotism and mesmerism in the year 1882 when the Society for Psychical Research was founded. All the well attested facts of mesmerism, the profound anaesthesia induced by Esdaile in India, the many genuine phenomena observed and recorded by Elliotson in England, were derided by the whole scientific world. The reality of the mesmeric state was denied because the existence of the magnetic fluid was thought to be disproved by Braid's demonstration that a similar state could be brought about in the absence of all mesmeric manipulation. Acute controversy arose between the mesmerists and the hypnotists, but in the course of years it died down, and after the death of Braid and Elliotson interest in the subject waned to such a degree that in England there was no scientific investigation into the rival claims of the mesmerists and the hypnotists until Gurney began his work in 1882. As Frederic Myers said of him : " he was the first Englishman who studied with any kind of adequate skill the psychological side of hypnotism, making therein experiments—cut short, of course, by his premature death, but already of the highest value—experiments which, though sometimes concurrent with those of the French School, were yet independently executed ; and which mark, as it seems to me, an epoch in the study of hypnotism in England " (*Proceedings, S.P.R.* IV, p. 365).

In his early work on this subject two problems specially occupied Gurney's attention. The first question he tried to answer was : Is the hypnotic state ever induced by some yet unrecognized agency—some specific influence of operator on subject? Long after suggestion had been recognised as the efficient cause of hypnotic state, " passes " were still used in its induction, and both Gurney and Myers were inclined to believe that there was some evidence found in the course

of their experiments to justify the claims of the mesmerists in this matter.

Myers thought that Gurney's experiments proved " that there *is* sometimes in the induction of hypnotic phenomena some agency at work which is neither ordinary nervous stimulation (monotonous or sudden) nor suggestion conveyed by any ordinary channel to the subject's mind ". The chief form of experiment on which this conclusion was based was described by Gurney as follows : " The ' subject ' is made to put his arms through a thick screen, extending high above his head, and to spread his ten fingers on a table in front of him. The fingers are thus completely concealed from his view, and the operator's hand is held, without contact, at a distance varying from about a third to three-quarters of an inch, over one or another of them—according to my selection—with the result that in a very large majority of cases, the finger so treated, and that finger alone, becomes rigid, and insensible to extremely severe treatment in the way of stabs, burns and electric shocks ".

Although Gurney and Myers stressed the importance of further testing of these results by other observers, little interest in their work was shown ; for under the growing influence of the French School the conviction became general that suggestion was the sole and sufficient cause of all hypnotic phenomena.

Some further experiments, recorded by Mrs Sidgwick and Miss Alice Johnson. in 1890 (*Proceedings, S.P.R.* Vol. VIII) corroborated Gurney's results, but these observers arrived at a different conclusion as to how these results were brought about. They thought that the rigidity and anaesthesia were due to suggestion conveyed telepathically from the operator to the subject. They were further of opinion that their experiments disproved Gurney's hypothesis of a physical effluence directed by the concentrated will of the operator.

This subject was again brought before the Society in 1920 by the late Dr Sydney Alrutz, Lecturer in Psychology in the University of Upsala, and he described some experiments performed by him which convinced him of the reality of a nervous effluence such as Gurney had postulated. After Dr Alrutz's death, his methods were demonstrated, by his assistant, to a group of psychologists at the eighth International Psychological Congress at Gröningen in 1926, and a report of this demonstration by Dr Van Loon and Dr Thouless appeared in *Proceedings* (Vol. XXXVI). These experiments were not successful, and all those before whom they were demonstrated agreed that " the results obtained point to the ordinarily accepted theory of suggestion in hypnosis as an adequate explanation of all the phenomena observed ".

Although this problem of an unrecognised agency in the induction of hypnosis occupied Gurney's mind from the beginning, he soon turned his attention to the psychological phenomena of hypnotism—a sphere in which he did much work of outstanding value. His most important contributions to our knowledge were his papers on " The Stages of Hypnotism ", " The Problems of Hypnotism ", " Stages of Hypnotic Memory " and " Hypnotism and Telepathy ".

The occurrence of different stages or degrees of hypnosis, and the memory relations of these stages to each other and to the waking consciousness, present some of the most perplexing features of trance states, whether arising spontaneously or artificially induced. Although Janet, in France, was at this time occupied with these problems, Gurney's work was begun independently and was set forth in a series of contribution to our *Proceedings*, the first of which appeared a few months after the publication of Janet's first paper. Although so little attention had been paid to hypnotism in England since Braid's time, there had been several investigators on the Continent, but their observations and experiments were little known in this country. Gurney's experiments, therefore, were received with incredulity and few realised that he was laying the foundations on which the psychology of abnormal mental states during the next twenty years was to be based.

Most of the experimental work was devised and carried out by Gurney, but the theoretical implications of his results was more particularly the task undertaken by Frederic Myers. Partly derived from Gurney's experiments, partly from Janet's clinical observations and partly from his own studies on various forms of automatism, Myers put forward a view of the nature of hysteria which was far in advance of the teaching of English clinicians. William James, writing on Myers' service to Psychology, said: " Myers' work on automatism led to his brilliant conception, in 1891, of hysteria. He defined it, with good reasons given, as a ' disease of the hypnotic stratum '. Hardly had he done so when the wonderfully ingenious observations of Binet, and especially of Janet, in France, gave to this view the completest of corroborations. These observations have been extended in Germany, America and elsewhere ; and although Binet and Janet worked independently of Myers, and did work far more objective, he nevertheless will stand as the original announcer of a theory which, in my opinion, makes an epoch, not only in medical, but in psychological science, because it brings in an entirely new conception of our mental possibilities ". (*Proceedings, S.P.R.* XVII, p. 19).

It is interesting at the present day to recall that the first citations

brought forward by Myers in support of his views were from Breuer and Freud's epoch-making paper on " The Psychical Mechanism of Hysterical Phenomena ". After giving a brief abstract of this paper Myers said : " I could not wish for a more emphatic support, from wide clinical experience, of the view of hysteria to which my own observations on different branches of automatism had already, by mere analogical reasoning, directed my thought."

As is well known, Freud's views on Hysteria have undergone a great development since those early days, but there is nothing in his earlier work that is opposed to his theories of to-day. Would that we had to-day a Frederic Myers to evaluate those theories and to incorporate them in a new study of Human Personality.

Besides the early work of Gurney and Myers there were in later years many important contributions on this subject by medical men who were members of the Society for Psychical Research and whose earliest writings appeared in our *Proceedings*. Dr Lloyd Tuckey, one of our members, was one of the first men in England, after Braid's time, to resume the use of hypnotism in medical practice. Dr Milne Bramwell also joined our Society and contributed many important papers to our *Proceedings*. By his forceful personality and his courage he did more, perhaps, than any man in our time to compel the medical profession to consider the claims of hypnotic suggestion as a therapeutic method.

Although the therapeutic side of hypnotism was by no means neglected in the work of the Society—indeed the seemingly mysterious influence implicit in the records of " hypnotic " cures was one of the primary interests of its founders—nevertheless, experimental work was that to which the pages of *Proceedings* were most freely and appropriately open. The original records of one series of experiments, namely, experiments on the Appreciation of Time by Hypnotic Somnambules, are to be found, exclusively I think, in our *Proceedings*.

Although all experimental work on hypnotism has bearings on therapeutics, it is, however, in relation to psychopathology that its results are most enlightening. Knowledge of the peculiarities of hypnotic memory proved very helpful in the study of the conditions known as Double and Multiple Personality. This topic came into prominence through the work of Pierre Janet and other French observers, and although Gurney and Myers had little opportunity of studying such cases at first hand, Myers, after Gurney's death in 1888, made an exhaustive examination of all the available records, and was convinced of the importance of such studies for a true understanding of the workings of the mind. His conclusions were

expounded in many papers in our *Proceedings* and were ultimately incorporated in his great work on " Human Personality and its Survival of Bodily Death ".

Some of these cases of double personality were recorded for the first time in our *Proceedings*, for example the case of Ansel Bourne, reported by Richard Hodgson ; but wheresoever these stories were recorded the earlier cases were very fully summarised and commented on by Myers, and later ones by other members of the Society.

The interest of Psychical Research in these pathological records was largely due to the many similarities observed between the manifestations of multiple personality and the phenomena of mediumistic trance. The main problem was to discover the true nature of secondary personalities, and whether all secondary personalities had the same origin. The secondary personalities of the French cases were obviously formed by a massive splitting of the mind, by what was called a disaggregation or dissociation of consciousness. Restoration to the normal state was achieved by bringing about a reunion of the split-off portions. Occasionally, however, it was found that one of the secondary personalities failed to become incorporated in the restored personality—which was presumably the full normal self. For example, in the Beauchamp case recorded by Morton Prince, there seemed to be no representative of the personality Sally in the restored Miss Beauchamp. As Morton Prince enigmatically said, Sally went back to " where she came from ". Some investigators are tempted to believe that in cases of this kind we have examples of invasion by an extraneous spirit similar to that which they believe occurs in mediumistic trance. Thus, in the Doris Fischer case, recorded so fully by Dr Walter Prince, the personality " Sleeping Margaret " claimed to be a spirit, and Dr Prince and Dr Hyslop were of opinion that her claim was justified.

Many psychologists who have studied such cases deny the necessity of ascribing to these personalities any origin different from that of secondary personalities due to splitting of the self, nor do they admit that the personalities known as " controls " in mediumistic trance have any other origin. They do not admit the spirit hypothesis, even in those rare cases of so-called " personal control " in which possession of the entranced medium by an extraneous spirit would appear to afford the readiest explanation of the observed phenomena. The best critical study of this problem is to be found in Mrs Sidgwick's important monograph on " The Psychology of Mrs Piper's trance " (*Proceedings, S.P.R.* Vol. XXVIII).

It might be supposed that in raising the question of sp rit possession we are passing beyond the territory common to both Psycho-

pathology and Psychical Research ; but in view of the opinion of so acute a critic as Mrs Sidgwick that all mediumistic controls are but secondary personalities or impersonations at the hypnotic level of consciousness, we are perhaps justified in regarding all such phenomena as part of the subject matter of psychopathology. In so doing we imply nothing either directly or indirectly about the related topic of the supernormal acquisition of knowledge, with which we are not now concerned.

In the early years of the twentieth century the part played by Medical Psychology in Psychical Research became so considerable that the Council of the Society for Psychical Research decided to form a Medical Section of the Society and to publish at intervals special medical parts of *Proceedings*. Part 66, Vol. XXVI of *Proceedings*, was the first, and the last, special medical part that was issued ; for in 1914 the war came, and after the war the medical section of the Society for Psychical Research was discontinued for reasons which I will briefly explain.

The revival of psychotherapeutics in England may be said to date from 1905, when a Society for the Study of Suggestive Therapeutics, afterwards called the Psycho-medical Society, was formed in London. From an earlier period there had been in existence an association of Academic Psychologists called the British Psychological Society. During the war some well-known members of this Society who held medical qualifications, such as Dr W. H. R. Rivers, Dr Charles S. Myers and Dr William Brown, had been engaged in the Army Medical Service, and had done much psychotherapeutic work among soldiers suffering from so-called " shell shock ". When the war was over the British Psychological Society decided to form various sections within the general framework of the Society, and a Medical Section, with a large membership, was the first to be formed. It published a quarterly journal, *The British Journal of Medical Psychology*, and as it was thought that all psychotherapeutic and psychopathological interest would now centre in this new organisation, the old Psycho-medical Society and the Medical Section of the Society for Psychical Research were discontinued, as it was felt that they could serve no further useful purpose.

Although the activity of the Medical Section of our Society thus came to an end we still continued to publish in our *Proceedings* contributions from medical psychologists on the various topics to which I have referred, namely, mesmerism, hypnotism, multiple personality and the psychology of trance states in general. But as time went on it became plain that since the war the very foundations of medical psychology, on which we had built for over twenty years, had

been shaken by the work of Professor Freud and his pupils. Interest in hypnotism and its phenomena waned and its use by medical men as a therapeutic agent was gradually abandoned in favour of some form of mental analysis based on the discoveries of Professor Freud.

Although the bearing of these discoveries on Psychical Research was by no means clear, we welcomed any light on our problems that the analysts could give us. It was about this time that Freud's work came to be generally known about in this country, and we asked him to contribute a paper on his use of the word "unconscious" in his writings. He very kindly sent us a "Note on the Unconscious" which was published in *Proceedings* and subsequently in his "Collected Papers". In this paper he told us "what the term 'unconscious' has come to mean in Psycho-analysis and in Psycho-analysis alone".

It is difficult at the present day to recapture the atmosphere in which the early teachings of psycho-analysis were received in this country thirty years ago, but those who can do so will understand why a paper on "The Psycho-analysis of Suggestion and Hypnosis" which we received from Ferenczi, a well-known Hungarian psycho-analyst, was not published in our *Proceedings*. It was thought inadvisable to do so in a lay publication because of the attitude of public opinion at that time towards Freud and his work, and with Ferenczi's consent his paper was handed over to the Psycho-medical Society and was published in its *Transactions*.

With the general adoption of analytical methods in the treatment of psycho-neurotic illness the interrelations of psychical research and psychotherapeutics became less obvious, and at the present time we cannot say that the researches of one department of knowledge have much bearing on those of the other. But there will always remain some common ground—the realm of the unexplained—in which discoveries may be made in one field of investigation or the other. That there is a realm of the unexplained in the history of mental healing cannot be denied.

That there is some unknown force at work in the healing of the sick, in addition to the ostensible means employed, is a belief that can be traced from the beginnings of history up to the present day. This force has been interpreted in various ways, sometimes as divine or magical influence, sometimes as an occult physical or mental influence emanating from the healer, sometimes as merely faith on the part of the healed. Belief in the existence and efficacy of such forces is still widespread at the present day. Although civilised man may have given up the beliefs of the savage in respect to the causation of disease, there still linger in the depths of the mind the deposits of ages of what we presume to call superstition, and a

craving for some manifestation of supernatural power that will cure him as the forefathers of his race were cured.

The advice of the prophet, given at second hand through a messenger, "Go and wash in Jordan seven times" seemed to Naaman a poor way of treating leprosy. He was wroth and went away and said, "Behold, I thought, He will surely come out to me, and stand, and call on the name of the Lord his God, and strike his hand over the place." The leper missed the personal contact with the healer. What he was told to do was too simple, too commonplace; there was no incantation, no magical gesture, no appeal to a divine power. There was no special virtue or charm, he thought, in the waters of Jordan. "Are not Abana and Pharpar, rivers of Damascus, better than all the waters of Israel? ... So he turned and went away in a rage."

Even when material remedies were accepted and used in the treatment of disease, belief in the part played by spiritual agencies was slow to disappear, and when spiritual agencies as understood by primitive man were finally dethroned, agencies of a hidden nature were still believed in. When Socrates was asked by Charmides about his cure for headache, he replied that "it was a kind of leaf, which required to be accompanied by a charm, and if a person would repeat the charm at the same time that he used the cure, he would be made whole, but that without the charm the leaf would be of no avail". Further he said: "If the head and body are to be well, you must begin by curing the soul; that is the first thing. And the cure ... has to be effected by the use of certain charms, and these charms are fair words."

Let us return to modern times and ask if there is any unknown force, any "charm", in the therapeutics of to-day. It is commonly admitted that what is vaguely spoken of as "the personality of the physician" plays an important part in ordinary medical treatment, and it is widely held that the influence thus effected is in some way related to the influence of suggestion in psychotherapy. But how suggestion ever acts to produce curative results is just one of the unsolved problems that we have to consider.

In the days of the later mesmerists much stress was laid on the *rapport* which became established between the patient and the mesmerist, and this was ascribed in some unexplained way to the marvellous attributes of the magnetic fluid. A similar *rapport*, however, was found to exist between the hypnotist and the hypnotised person when no mesmeric methods were used in the induction of hypnosis. There have been many attempted explanations of the peculiar relation of the hypnotised subject to the hypnotiser, but

none of them is quite satisfactory. Dr McDougall ascribes all suggestibility and hypnotic *rapport* to the rousing of the instinct of submission by one who has prestige in virtue of some quality or supposed quality that renders him imposing to the person whom he influences. Freud compares hypnosis to the state of being in love. He says : " The respects in which the two agree are obvious. There is the same humble subjection, the same compliance, the same absence of criticism, towards the hypnotist just as towards the loved object."

Here then, in the ability to arouse feelings of submission or of love, may perhaps be found the " charm " without which suggestion is of no avail. But although we may have found the key to suggestibility, that is to say, *why* a therapeutic suggestion is accepted so as to become efficacious, we still do not know *how* it acts.

The control by suggestion of organic process over which we have no voluntary control is as mysterious to us now as it was in Mesmer's day. It is an aspect of the mind-body relation of which we can give no explanation either physiological or psychological. I do not think anyone understands how such results are brought about. They are a form of obedience or compliance which neither the instinct of submission, nor love, nor fear can account for. Thus notwithstanding all our experience of suggestion and the effects that it can produce we must still acknowledge the activity of some unknown force behind its manifestations.

Suggestion deliberately employed has produced so many seemingly miraculous cures that we are perhaps too ready to ascribe to suggestion all so-called miracles of healing, without being sure that no other force may be at work. There are on record, both in the past and in the present, so many instances of alleged miracles of healing in which, if we light-heartedly dismiss them as being due to nothing but suggestion, we are perhaps missing some unknown factor, the reality and importance of which may some day be acknowledged. No sounder advice was ever given to scientific investigators than the direction to " examine your residues ". If in alleged cases of miraculous healing, whether " spiritual healing ", " faith healing ", or any form of curing the sick that recalls to our minds the so-called superstitions of the past—if, in investigating these cases we find any residuum of unexplained phenomena, it is our duty to search with unbiased minds for any unknown factor that may be involved. To do so is a duty of obedience to the teachings of the history of science, and it is a lesson that has often been taught us in the practice of Psychical Research. We need to avoid the intellectual arrogance which misleadingly assumes that we know far more about the nature

of things than we actually do, just as we must avoid all enthusiasms that would tend to pervert our intellectual judgments. Nowhere do we find the pernicious effects of such enthusiasms more clearly displayed than in the hasty rejection of new truths which happen to be opposed to our beliefs and prejudices, or in the uncritical acceptance of beliefs that fulfil our wishes.

The lesson to be learnt from all our work in Psychical Research, as it seems to me, is this: the need to keep a suspended judgment whenever we encounter new facts or new theories that do not fit into our accepted scheme of things, or that too readily satisfy our hearts' desires.

To many of you, I feel sure, such a Laodicean attitude will not appeal: to me it is the very essence of the scientific spirit. To the man of science there should be but one supreme enthusiasm—the enthusiasm for truth.

REVIEW

Mrs Henry Sidgwick. *A Memoir.* By her niece Ethel Sidgwick. Sidgwick and Jackson, Ltd., London. 12s. 6d. net.

Members of this Society will hardly need to be reminded of the debt it owes to Mrs Sidgwick. Although she did not join the Society until 1884, two years after its foundation, she may be counted one of its founders in all but the narrowest sense of the word, and no one did more than she to shape the Society's course, or contributed more to its achievements during the first 50 years. The earliest of her own papers " Phantasms of the Dead " appeared in *Proceedings* in January 1885; her last was a History of the Society, written on the occasion of its Jubilee in 1932, when she was eighty-seven years old. Between these two dates she contributed constantly to *Proceedings* and took a large share (in collaboration with Henry Sidgwick and others) in such important pieces of research as " Phantasms of the Living " and the " Census of Hallucinations ".

There is no field in which Mrs Sidgwick's personal qualities were more apparent than in psychical research, her human sympathy and desire to help people in any way she could, her meticulous accuracy, her infinite capacity for taking pains, her willingness to embark on any task, however long, dull and immediately unprofitable it might be, if there was the slightest hope that at the end of it the sum of knowledge might be increased, her clear and impartial judgement on matters of evidence, and last but not least, her unswerving determination to follow truth wherever it might lead her.

In Chapter VI of Miss Sidgwick's book, entitled " Newnham and Beyond ", the author traces the gradual development of Mrs Sidgwick's opinions on the question of human survival and the possibility of communication with the dead from the doubt of early days to the conviction publicly announced by her brother, Lord Balfour, at the Jubilee Meeting: " Conclusive *proof* of survival is notoriously difficult to obtain. But the evidence may be such as to produce *belief*, even though it fall short of conclusive proof. . . . I have Mrs Sidgwick's assurance—an assurance which I am permitted to convey to the meeting—that, upon the evidence before her, she herself is a firm believer both in survival and in the reality of communication between the living and the dead."

Where proof is unobtainable, conviction must always be to some extent a personal matter, but it may fairly be said that no one had

a better right to conviction on this particular issue than Mrs Sidgwick, for no one has ever made a more complete and careful study of all available evidence, or is less likely to be led astray by prejudice or emotional bias.

In addition to her contributions to research, Mrs Sidgwick played a large part in the administrative side of the Society's work; she was not only a Council Member for many years and President in 1908 and 1909, but she was Honorary Editor from 1888 to 1897 and Honorary Secretary from 1907 to 1932.

A considerable part of Miss Sidgwick's book is naturally concerned with Mrs Sidgwick's activities in other fields and in particular with the important work she did in the foundation and development of Newnham College. Not only was she Principal of the College from 1892 to 1910, but from the very earliest days of Newnham almost to the time of her death there was hardly any scheme for the advantage of the College in which she did not play a leading part. In this field also, as in psychical research, Henry Sidgwick was her constant collaborator, until his death in 1900.

Several interesting and characteristic quotations are given from some of her public speeches. At Liverpool in 1896 she said that it was the privilege of a University to bestow two gifts: one moral and one intellectual. The moral gift was "the sense of membership of a worthy community with a high and noble function in which every member can take part"; the intellectual gift was the "habit of reasonable self-dependence in thought and study, to whatever end thought and study may be directed. I call it reasonable self-dependence, partly to contrast it with the uncritical acceptance of new ideas . . . to which even persons of strong intellectual interests are liable, if they are given over to miscellaneous reading without ever having made a thorough study of anything. . . . But I equally wish to distinguish it from the undue self-confidence and sweeping dogmatism sometimes seen in persons who have mastered one subject really well, but have never, by living and learning among students who are studying other subjects, imbibed an adequate sense of the limits of their own knowledge." No one exemplified the value of these intellectual and moral gifts more completely than Mrs Sidgwick herself.

Her admirable sense of human values was also shown in the lecture she delivered in the early days of the Great War on "The International Crisis". We were fighting, she said, against the principle that might makes right: "Do not let us, then, indulge on our side in the very vices we are condemning. How can we hope to diminish hatred in the world if we allow ourselves to feel it? . . .

Hatred is not, except in a very limited way, a source of strength but a source of weakness, and if we depend on hatred to enable us to win, we shall lose.... The second duty I want to insist on is that of not being afraid."

There are two delightful chapters on Mrs Sidgwick's childhood and the days before her marriage. It is always difficult to realise that people we have only known in later life were once, like ourselves, very young, and it is pleasant to have glimpses of Mrs Sidgwick sitting in a beech tree on a Sunday afternoon, or joining with her brothers and sisters to initiate guests at Whittingehame into the Balfour customs, which included conducting them " about the more precipitous parts of the grounds by night."

The simplicity, not to say austerity, in her personal way of life which is evident in these early years, remained characteristic of Mrs Sidgwick to the end. Miss Sidgwick mentions an occasion in 1930 when at the age of eighty-five she travelled alone third class from Oxford to Dunbar. When she came to the Society's Rooms, she habitually and in all weathers insisted on departing alone by bus for Waterloo, and I well remember the surprise, not to say consternation, of the Society's officials, when on a bitterly cold day she and her sister, Lady Rayleigh (both of them over eighty), appeared in an open car, because they " liked fresh air ".

It is to be hoped that many members of the Society will read this book, not only for its interest in relation to psychical research, but also for the light it throws on an outstanding character and personality, which will always remain a vivid memory to those of us who were privileged to know and to work with Mrs Sidgwick.

Helen deG. Salter

PROCEEDINGS

OF THE

Society for Psychical Research

PART 158

WILLIAM McDOUGALL, 1871-1938

By Kenneth Richmond

Future historians of mental science will either hold William McDougall in high esteem or leave his name to sink into oblivion. If the human mind, as represented by scientific thought, comes to the conclusion that it is an aimless accident in an aimless world, he will merely be one of the old-fashioned thinkers who stood in the way of that conclusion. The thought of the newer, purposeless thinkers will have passed him by, on deterministic tram-rails which have the peculiarity of being laid by men with no intention of arriving anywhere—since intention will be out of court as a real factor in human procedure.

If, on the other hand, we and the generations that follow us can put it beyond scientific question that we are something more than mechanically-determined aggregates of physical and chemical action, McDougall will stand high among the men of science who maintained the wider view, and upheld it with detailed and comprehensive reasoning. Both in the range of detail, biological and psychological, that he has brought into the argument on behalf of purpose, and in the comprehensiveness with which he has marshalled his material, he has held a place of his own. Men of greater philosophical attainment have built completer structures of reasoning about the necessity for an acceptance of mind as a directive agent, but McDougall remains the chief stalwart in his orderly presentation of facts that strain to bursting-point any of the mechanistic interpretations of life.

His career falls into two divisions, in England until 1920 and in the United States from then until the end of his life, and the space

that separates the two phases of his work is not merely geographical. A Lancastrian by birth and the environment of his boyhood, he graduated at Cambridge, and took his M.B. at St. Thomas's Hospital, London. But for his war service as an officer in the R.A.M.C., his medical training was to be chiefly an adjunct to his general scientific equipment. His Doctorate was in science. His distinguished academic work brought him eventually to Oxford as Wilde Reader in Mental Philosophy. He was elected a Fellow of Corpus Christi College, Oxford, and of St. John's College, Cambridge. His Fellowship of the Royal Society added honour to a position in the scientific world which was becoming increasingly evident in the reception of his published work.

From his first book, the *Introduction to Social Psychology*, onwards throughout his writings, it is clear that his thought as a psychologist took full account of man regarded as a social creature. This is not the place to expand the point that a psychology which fails of attention to the social behaviour of man is an incomplete psychology, but it is relevant to the appreciation of McDougall's wide humanistic outlook that he consistently treated mental life as a fact which has a social context. He also based cogent arguments for the purposive character of life upon the social behaviour of living beings. *Body and Mind*, published when he was 40, was at once received as a book of major importance, and has remained a standard work. In it he maintained that " not only conscious thinking, but also morphogenesis, heredity and evolution, are psycho-physical processes." Among the evidence for a psychic constitution in man he included a chapter on psychical research, in which the following passage occur, on cross-correspondences : " . . . the keenest critics of the view which sees in these writings the expression of the surviving personalities of deceased persons, are driven to postulate as the only possible alternative explanation of some of them the direct communication of complex and subtle thoughts between persons separated by hundreds and even thousands of miles . . . we stand before the dilemma—survival or telepathy of this far-reaching kind. The acceptance of either horn of the dilemma is fatal to the mechanistic scheme of things."

The mind of primitive man next engaged his attention, and was the subject of published work. War and war-service intervened ; and in 1920 he published *The Group Mind*, in which social psychology is extended into the psychology of nations and nationhood. It was in this year that he accepted the Presidency of the S.P.R. and delivered the Presidential address published in *Proceedings*, Vol. XXXI, p. 105. In this he dealt with the common preference of

scientists to remain in ignorance of psychical research, and recognised that they have one motive worthy of respect in the fear of opening doors to public superstition. But he showed this motive to be mistaken: proper research was "our best defence" against superstition; "it is the policy of sitting on the lid of the box that is risky." In personal attitude towards the interpretation of phenomena he professed himself "very decidedly" of the right wing, but spoke of the importance and the value of both wings, and of co-operation between them. The concluding thought in his address, that what may survive is not merely an individual but a social being, reflects the shape of his thought in general psychology, and shows the scientific mind that does not entertain a hypothesis without taking it in the round.

In this same year, 1920, McDougall was offered, and accepted, the Professorship of Psychology at Harvard University which he held for seven years. His translation to the American scene was followed, at first, by a change in his writings that aroused some misgiving among admirers of his thought. He was adapting himself to a new audience and a more public environment than that of English academic life, and his lectures printed in book form showed a certain loosening of texture. There seemed to be some fear of the disappearance of a fine psychologist and the emergence of a popular instructor on human nature and world affairs. But he was meanwhile settling to business, as was presently shown by the appearance of two standard works, *An Outline of Psychology* (1923) and *An Outline of Abnormal Psychology* (1926). These are well enough known to those who make effective use of the S.P.R. library in making sure of their psychological groundwork. His *Character and the Conduct of Life* (1927) was a popular book in the best sense, and a valued contribution to the psychology of education. Meanwhile, his work in experimental psychology included the labour of conducting a prolonged investigation in the biological field, into the inheritance of acquired characters. His plan of experimental breeding, which has now been carried on for 17 years and to the fiftieth generation, has produced important evidence in support of the neo-Lamarckian view. This is perhaps the most concrete example of McDougall's way with a scientific dogma of "It can't happen". He would go to infinite trouble to make sure of his facts and then say, "It does happen".

In 1927 he became Professor of Psychology at Duke University, and it was under his aegis that the formal study of psychical research was instituted there with Professor Rhine as director. For all his positive character, McDougall was not the man to impose his own

rule and method upon the growth of younger men's ideas and experience, and the new institution flourished with its own vitality and is fighting its own battles. He was much engaged with constructive criticism of the innumerable branchings of psychological research and theory that are now claiming attention. This work is hardly represented in his last books, *The Frontiers of Psychology*, notable for its wide treatment of the interrelation between the sciences, *Psycho-Analysis and Social Psychology*, containing a searching criticism of the validity of Freudian "mechanisms" without minimising the great value of Freud's discoveries, and *The Riddle of Life*, the admirable survey of biological theories which was reviewed by Mr Tyrrell in the January issue of the *Journal*.[1]

But his thought in these last years was much preoccupied by the dispersion of psychological progress into so many unrelated channels. He had hoped to see, and to feel that he had played his part in, a massive unification of ideas in psychology. A lifetime of research and reasoning had confirmed his conviction that it is vain to build psychologies upon less than the whole foundation of psychological fact, and that mental aim and striving constitute a psychological fact of the first importance. He saw modern psychologies, one after another, as wasting their energies in the attempt to cram psychical facts, piecemeal, into a dubious framework of physical causation, and as missing the structure of sound theory by disregarding facts essential to structure. It could be said that his last years were weighted by a feeling of disappointment and frustration, and with the idea that the comprehensive outlook which he had offered to the psychological world had been widely refused. But he was no martyr to the thought, and went stoically on with the maintenance of his constructive standard and the shrewd criticism of imperfectly-based theories. Actually, the structure of his psychological thinking stands more securely in the esteem of men than he thought; and as an American colleague has observed, anyone who desires to eliminate what McDougall found it necessary to assume as to the nature of living mind should "see to it that he covers the tremendous range of behavioural facts with which McDougall continually checked his theory.[2]"

We know from the valedictory message that was printed in the *Journal* last December [3] that this distinguished authority upon the mind of man continued to the end of his life to set a high value upon the contribution that psychical research has made and has to make

[1] Vol. XXXI, p. 10.

[2] *The Psychological Review*, Vol. 46, p. 1.

[3] Vol. XXX, p. 294.

towards a comprehensive outlook in mental science. Psychical research workers owe a debt to William McDougall for his life-work in maintaining that outlook, in preserving the essential nature of psychology as an open rather than a closed system, and in constructing an open system of psychological thought upon the widest basis of factual observation.

SHORT EXPOSURE PHOTOGRAPHS OF A JUMPING MODEL

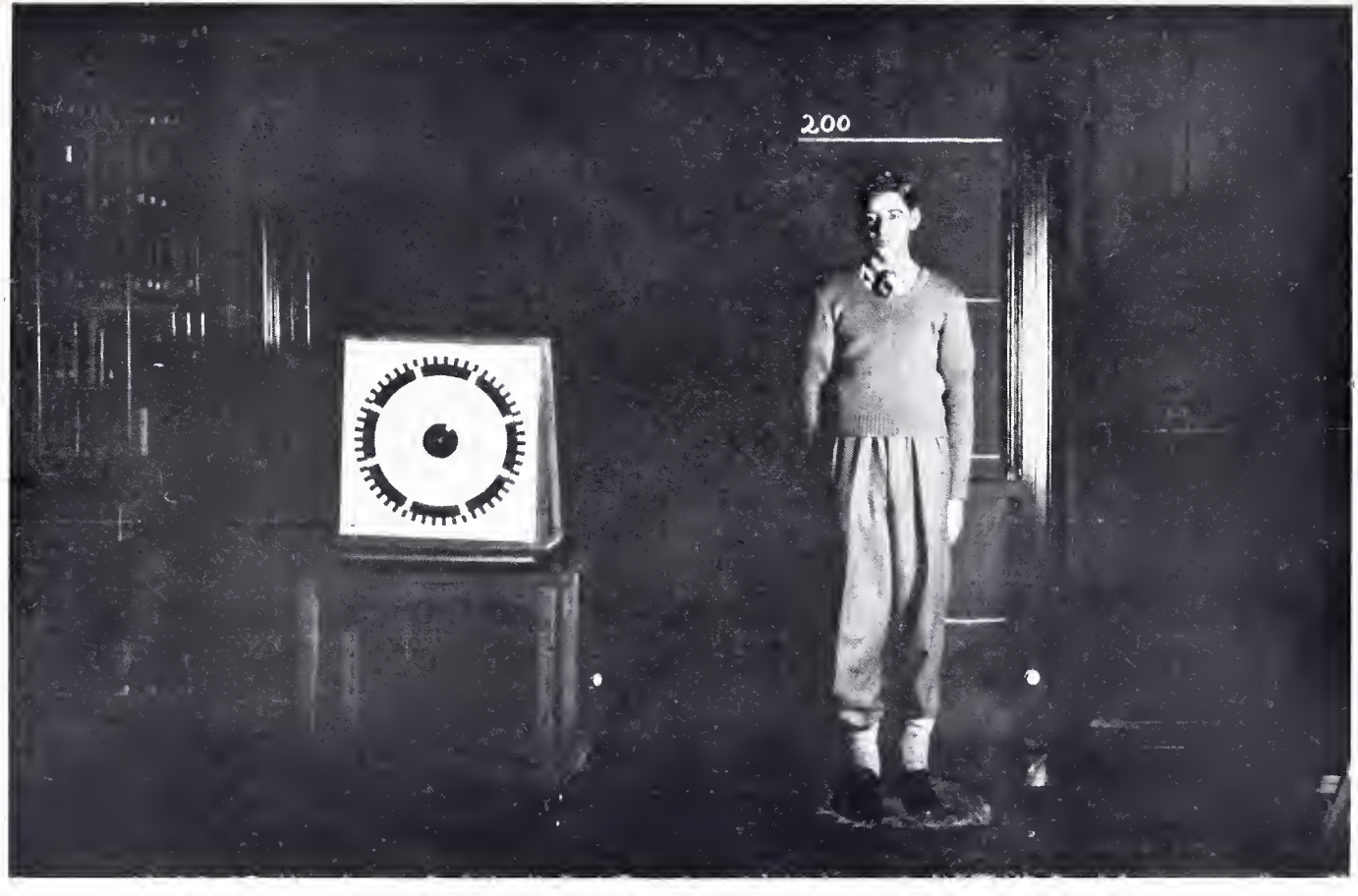
By C. V. C. Herbert

From time to time photographs appear in the press showing a medium at a séance in a state of apparent levitation. These are taken by means of flashlamp bulbs, in some cases the exposure being made by the medium himself by pressing a switch attached to a flexible wire. It has been suggested that these pictures in fact represent nothing more remarkable than the medium jumping from the ground, to which it has been objected that it would be impossible, without very special apparatus, to produce a photograph of a jump which did not show considerable blurring due to the movement of the figure. At the suggestion of the President, Lord Rayleigh, the Society produced a series of photographs of a jumping model in order that comparison could be made with these alleged levitation pictures.

The procedure was as follows :—

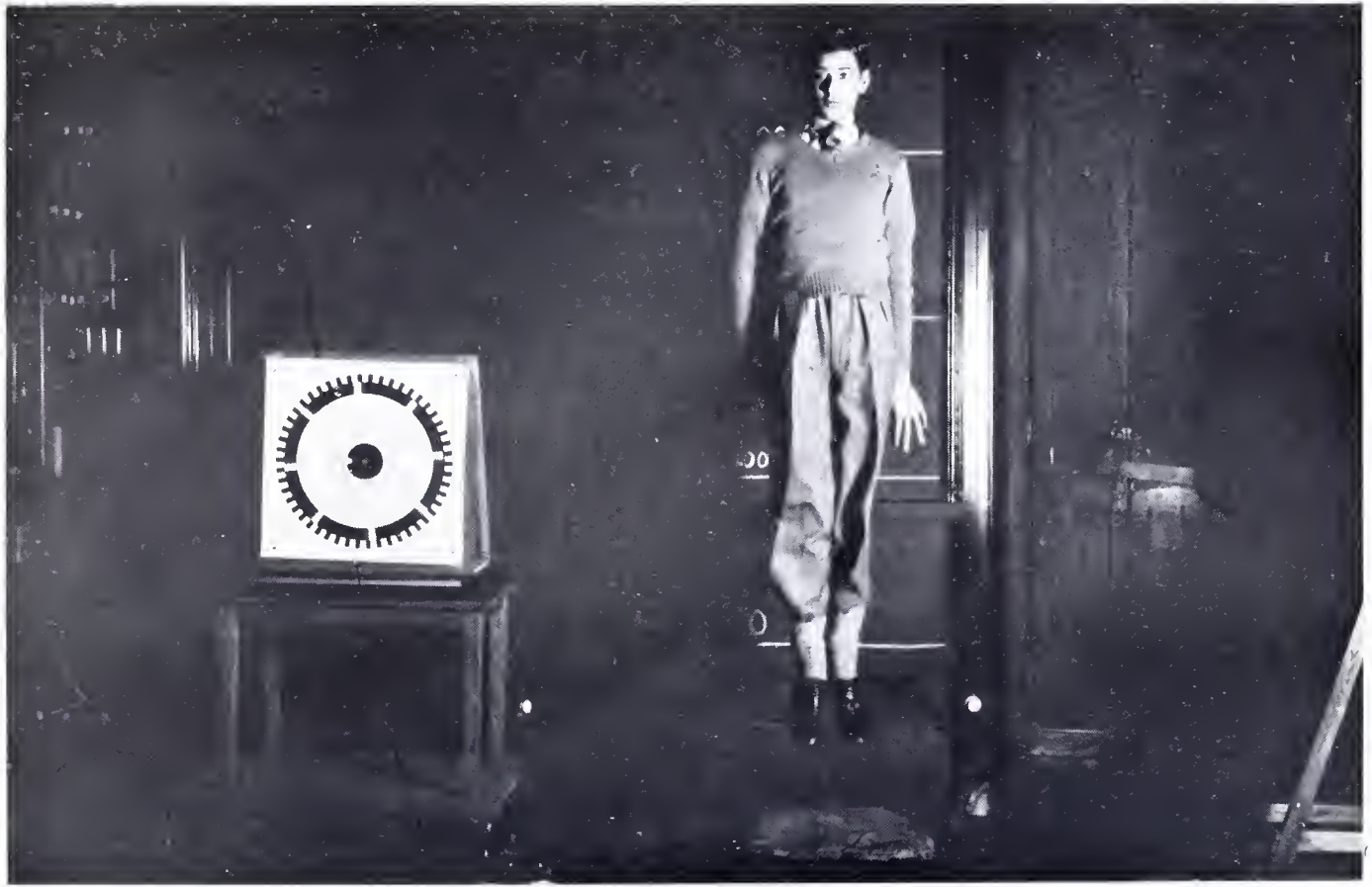
The model was placed against a black background, as is commonly the case in the séance room, immediately behind him being a screen on which horizontal white lines were ruled at 50 centimetre intervals. The distance between the screen and the model was approximately 60 centimetres, though this naturally varied slightly in different exposures. The camera was 6 metres away from the model. To the right of the model was placed the flashlamp, a large approximately parabolic reflector containing a single " Sashalite " photoflash bulb (General Electric Co.), the distance from the bulb to the model being 200 centimetres. The model was faintly illuminated by a red photographic safelight, the exposure being made by means of a telegraph key. To the left of the model and included in the field of the camera was a rotating timing disk, by means of which the length of exposure could be approximately measured. The exposure was made on 35-millimetre film (two frames per picture) in a Leitz " Leica " camera, one operator working the camera shutter and winding the film, while another changed the flash bulbs, checked the speed of the timing disk, and made the exposures by visual inspection by the light of the red lamp. The exposure times varied slightly with the different bulbs, but were approximately 1/100 of a second.

In Plate I the model is seen in position before the jump. Plate II shows the jump in progress, the model's head being about 50 centimetres above the position of rest, as shown by the scale of white lines on the background. Very little movement is discernible. For



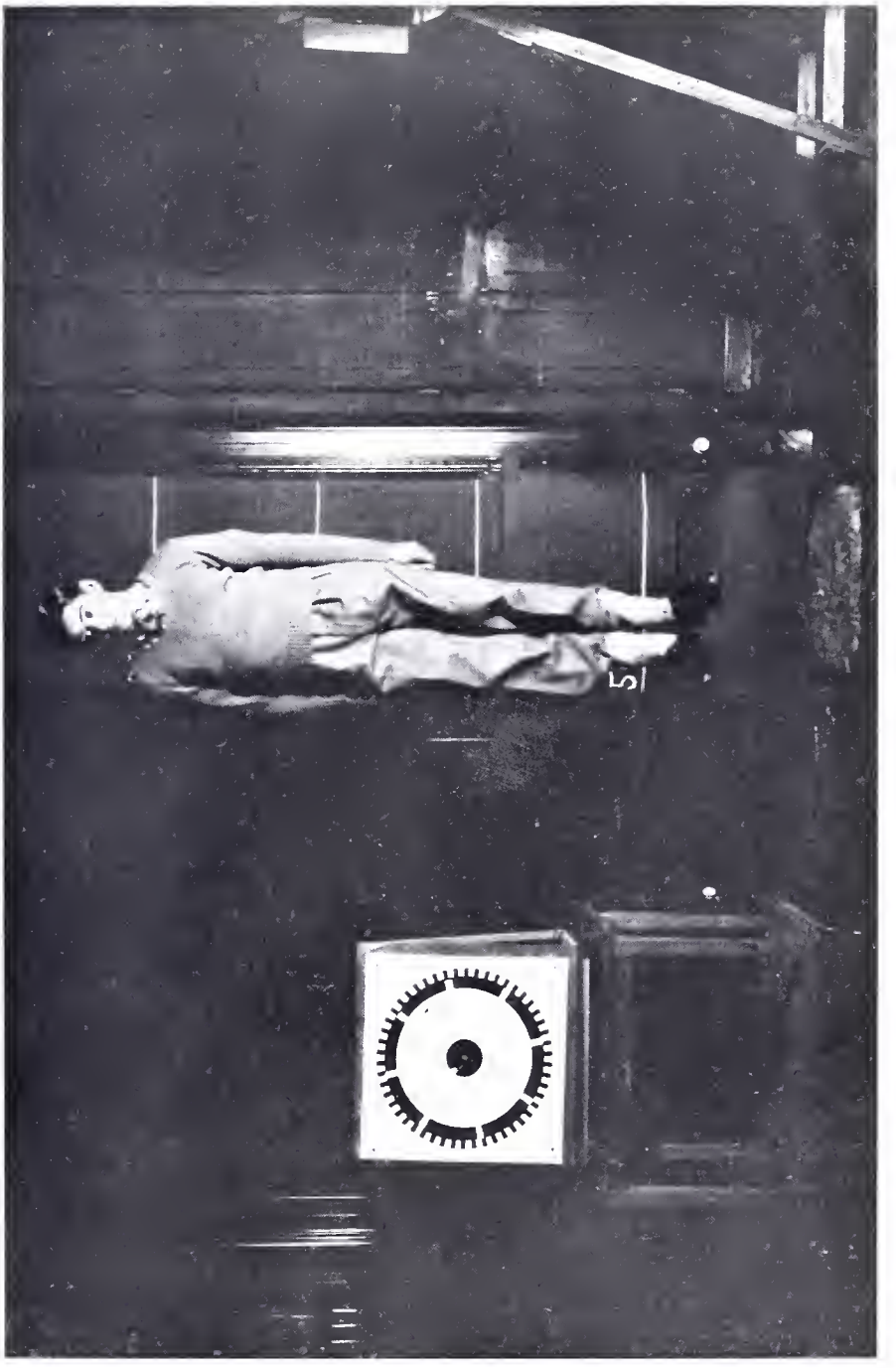
The model in position before the jump

PLATE I

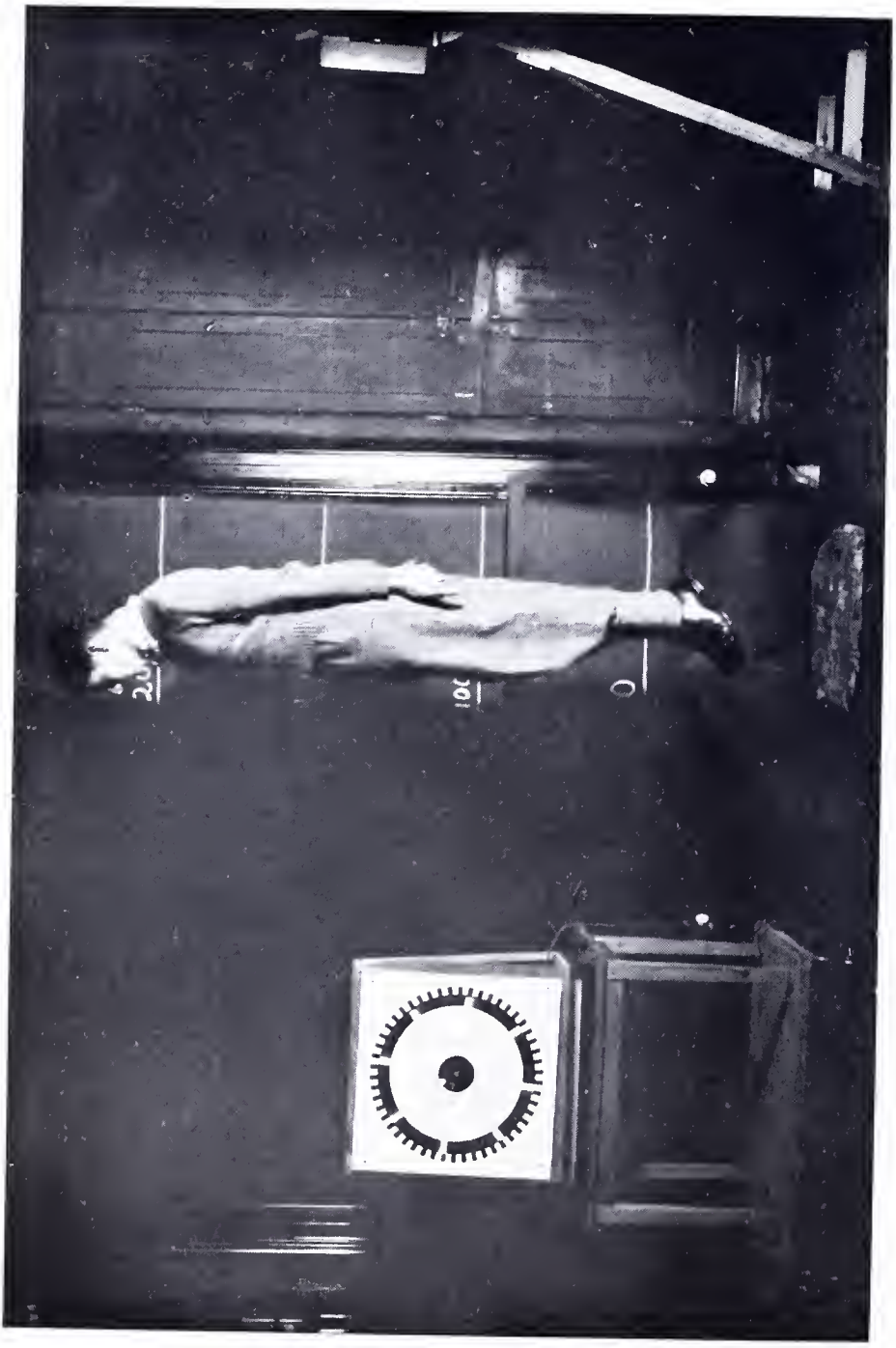


The model at the top of the jump

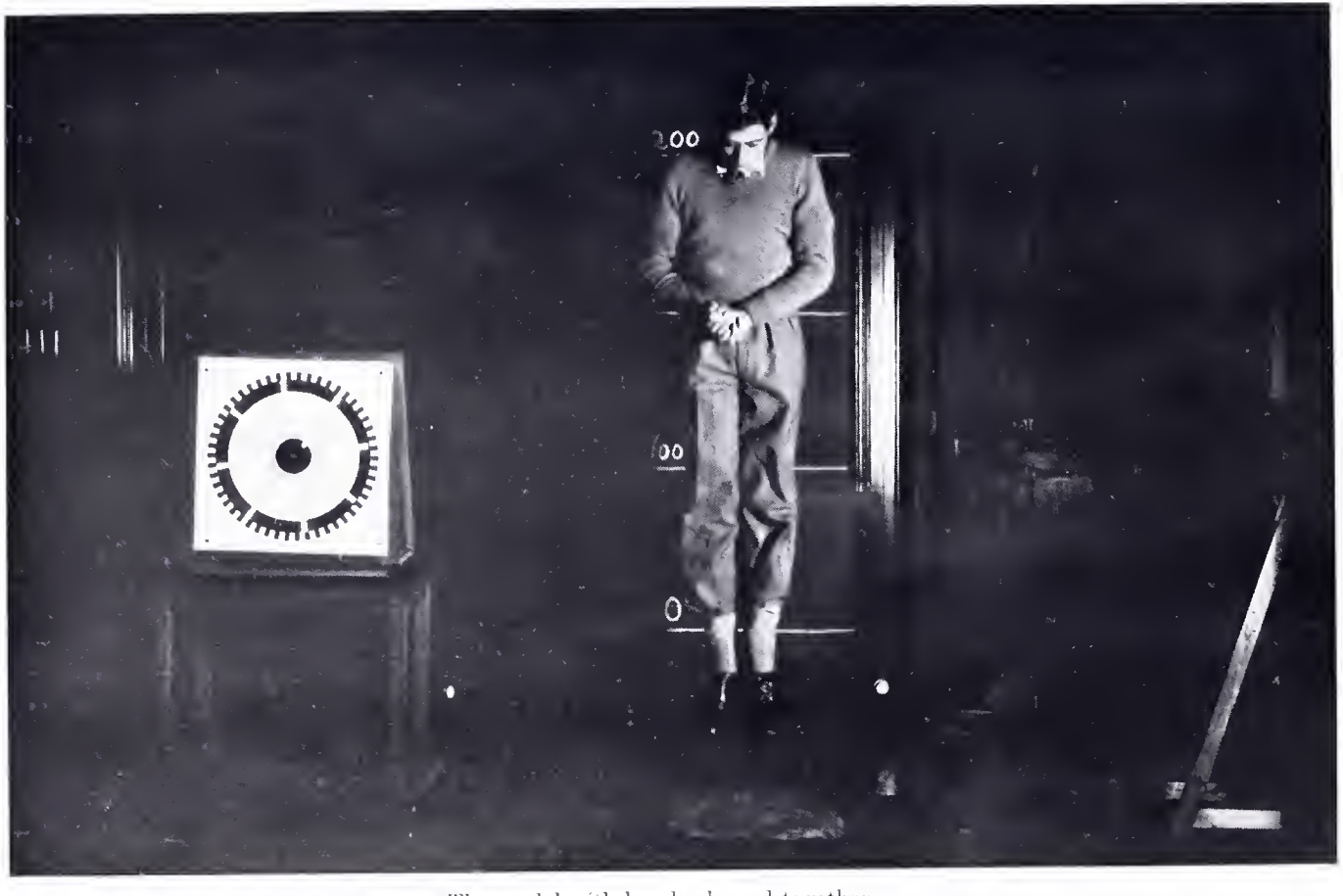
PLATE II



The model with toes drawn up



The model turned sideways to the camera



The model with hands clasped together

Plate III the model was asked to keep his toes well up during the jump, resulting in a very easy position in which he seems to be hanging in mid air with no trace of movement. The exposure here is slightly shorter than the average, being a little less than 0·01 second. In Plate IV the model is turned sideways to the camera, and Plate V shows him with his hands clasped together.

The photographs were all taken on the same day, the results not being seen until some days later. The operators had had no special experience in the photography of moving objects, nor was any special apparatus used, except for the timing disk, the flash bulbs being those commonly used at séances.

It is suggested that these photographs, especially Plate III, are freer of movement and more suggestive of genuine levitation than any of the published séance-room productions. It is therefore concluded that it would be quite *possible* to produce the published pictures by fraudulent means in any case where the flash was ignited by the medium himself or by an accomplice ; though this is not, of course, in any way proof that they were so produced.

The thanks of the Society are due to Mr D. S. Robertson, who kindly consented to act as model, and to Miss J. Robertson, who was one of the operators.

Photographic details, and a description of the timing disk are given in the Appendix.

APPENDIX

Photographic Details :

Lens : Leitz " Elmar :", $f=50$ mm. Aperture $f/5{\cdot}6$.

Distance, figure from Plate, 6 metres.

Distance, figure from background screen, 60 cms. (approx.).

The Plates are enlarged from the negatives $4\frac{1}{2}$ times linear. Negatives on 35-mm. film (two frames per picture) ; Coating, Ilford " Selochrome " special fine grain orthochromatic.

Exposure by single flash bulb, unscreened, in large white cardboard reflector. Bulbs, G.E.C. " Sashalite " photoflash, m.s. fitting, fired by 6-volt (mains transformer) A.C. circuit.

The Timing Disk :

The outer (fixed) scale comprises 100 equal sectors, alternately black and white. The inner (rotating) disk bears 8 radial arms, the width of each, where it cuts the scale, being exactly 1/100 of the circumference. The disk is rotated by means of an electric motor controlled by a centrifugal governor, the speed adjusted to

1 revolution per second by means of a stroboscopic disk illuminated by a neon tube in the 50-cycle circuit. The speed was checked several times during the course of the work, and was found to be very constant.

If n be the number of sectors of the fixed scale covered by the image of one of the radial arms of the rotating scale, the time of exposure in seconds (E) is given by

$$E = \frac{(n-1)}{100}.$$

By examining the image of the disk with the low power of a microscope, the exposure can be fairly accurately measured.

A PRELIMINARY INVESTIGATION OF THE PLATFORM CLAIRVOYANTE MRS HELEN HUGHES

By C. V. C. Herbert

Mrs Helen Hughes has established a considerable reputation as a platform clairvoyante at public meetings, and she has also a large practice of private Sittings. In her platform work she is especially distinguished by giving a large number of names, both Christian names and surnames, a considerable proportion of which are accepted as veridical by the recipients of the messages. This frequent occurrence of names and the general precision of her messages make investigation of her powers comparatively easy, and do a good deal to offset the general difficulties which are always involved in the study of platform mediums. It is probably true to say that investigation of Mrs Helen Hughes is likely to be more worth while than that of any other platform clairvoyante who practises at the present time.

It was therefore felt that the extremely inadequate data relating to Mrs Hughes, which were all that the Society's files contained—one short note by an investigator who had attended one of her public meetings—ought to be very considerably augmented, and that every opportunity should be taken of studying her remarkable powers, and of submitting her claims to supernormal cognition to a critical analysis.

The procedure at a usual platform demonstration is as follows :— The meeting, to which members of the public are admitted free of charge, begins with a speech by the Chairman, which serves to introduce the subject in general and Mrs Hughes in particular to the audience, and gives the medium time to settle down into a state of receptiveness, or, as some believe, of semi-trance. The speech lasts half an hour or so, and as soon as it is over Mrs Hughes begins her clairvoyant " readings " without further preliminaries. She does not appear to be in an abnormal state : her voice is her usual voice, and any condition of trance must be an extremely light one. It is, however, recorded that at a recent meeting held last year, Mrs Hughes was seriously affected by a disturbance which was made by a member of the audience, who was seized with an hysterical attack in the middle of the meeting. Mrs Hughes was unable to continue her performance, and burns or bruises were afterwards found on her person, caused, it is said, by the sudden shock of the interruption.

The messages are given in two forms. In one, Mrs Hughes addresses the audience at large and says, for example, " Is there anyone

here called Wilson? ", or " Is there anybody who knew a Doris Smith?" If a response is obtained, further information is then given. In the second class of message, a specific person is *first* selected. The medium says, " I want to come here—*that* lady " (pointing at a member of the audience). When the person's attention has thus been secured, the message, generally containing names, is delivered. If the recipient disclaims the message, it is sometimes found that it applies accurately to the person sitting on her right or left hand, or in the row behind or in front. In nearly every case somebody is found who is understood to accept the message, at any rate in part. Sometimes Mrs Hughes ends by saying, " Do I know you?" or "could I have known that normally?" and the recipient generally says, "No".

Some of the messages do not prove to be acceptable to anyone, but the general impression obtained by a member of the audience is that by far the largest proportion are more or less successes. Mrs Hughes speaks as though her information was obtained by a combination of clairvoyant and clairaudient perception. She " sees " figures " building up " near the various members of the audience concerned, and she also " hears " messages apparently from an unseen figure standing beside her—she sometimes half turns as though to catch a whispered conversation. The messages are given very quickly with considerable precision. After some twenty messages have been delivered, Mrs Hughes complains that the " power " is running low, and the proceedings come to an end.

At the demonstrations given by the London Spiritualist Alliance, which body has a monopoly of Mrs Hughes' services, the arrangements are the same, except that the audience is of course much smaller than at the meetings held in a public hall.

There can be no question that the vast majority of the messages given at these performances are more or less correct. To what is this correctness due? To chance, to normally acquired knowledge, to supernatural cognition, or to information imparted to Mrs Hughes by discarnate spirits? These alternatives can easily be reduced to two, since chance is clearly not a feasible explanation for such consistent successes, and we have no means of differentiating with any degree of certainty between supernormal cognition obtained through incarnate intelligences and similar information dictated by spirits. Our problem, then, is the following: does Mrs Helen Hughes get her material normally or supernormally? This question can only be answered by studying exact records of what was said by the medium, and by obtaining critical statements from the recipients of the messages. Such an inquiry, so easy at an ordinary

Sitting, is a formidable task when large numbers of people, many of whom are members of the public whose names are unknown, are involved. There is, however, one consolation. By far the most fruitful source of leakage at an ordinary Sitting—the giving away by the sitter of vital information in response to " fishing " on the part of the medium—cannot here be operative to any appreciable extent. The recipients of messages scarcely speak at all. It is clearly impossible that Mrs Hughes could get her material by normal means from the recipients during the meetings. Her information, if normally acquired, must have been obtained beforehand.

As stated above, Mrs Hughes gives a large number of private Sittings in addition to her platform work. At these Sittings, no doubt, much valuable information must be disclosed by the sitters. Even without any conscious " fishing " on the part of Mrs Hughes, it is certain that inexperienced and uncritical sitters will infallibly tell her a great deal about their private affairs, much of which they will not afterwards remember that they have disclosed. If Mrs Hughes is able to remember this material and to associate it with the sitter's appearance, then, if the sitter ever turns up at a platform meeting, this information could be given back. Many people have objected that such a process would necessitate a feat of memory on the part of Mrs Helen Hughes which, in view of the large number of private Sittings which she gives, is altogether impossible. What evidence have we that material brought up at a private Sitting is ever used again after an interval at a platform demonstration? There is, of course, the difficulty that even if, say, a veridical Aunt Jane, mentioned at a private Sitting, again put in an appearance long afterwards at a public meeting, that would be no proof that the second emergence was based on a memory of the first. It might be the real Aunt Jane who had communicated on each occasion, or if the first had been obtained by supernormal cognition from the sitter's unconscious, the second might represent a similar but unrelated supernormal cognition. It would be just as though the two Aunt Jane communications had been obtained through different mediums.

But by a great piece of good fortune we have a well recorded incident in which memory, normal or abnormal, of a private Sitting seems to be the only *possible* explanation of the facts.[1]

It is clear that, in evaluating Mrs Hughes' platform work, we cannot take into consideration messages given to persons who have previously had private Sittings with the medium, unless indeed we have before us a full shorthand record of the private Sittings con-

[1] See page 210.

cerned and are thus able to tell exactly what information was disclosed by the sitter.

The first attempt by the Society to deal critically with one of Mrs Hughes' platform demonstrations was made in connection with a public meeting held at the Caxton Hall on October 24, 1938. It was arranged that a full shorthand note should be taken, and a number of members of the Society attended the meeting and distributed themselves about the hall. These members were instructed to note carefully the people in their vicinity who were given messages, and, after the meeting was over, to engage some of them in conversation and to find out if possible—(1) if they had had previous private Sittings with Mrs Hughes, (2) if they had ever before attended public meetings, and (3) if their friends or relations were known to Mrs Hughes or had had private Sittings.

This last point is of great importance as was shown at a previous attempt of the same kind which was made some time ago by Mrs Goldney. Mrs Goldney approached a member of the audience at a public meeting who had received a remarkable message. This gentleman declared that he had never had a private Sitting with Mrs Hughes, nor had he previously attended a platform demonstration. It seemed as though a very striking instance of supernormal cognition had taken place, as the message was clearly not to be explained by chance, when a lady who had been sitting near the recipient entered the conversation and explained that *she* had recently attended a private Sitting with Mrs Hughes at which her friend had been mentioned (she could not remember whether it was Mrs Hughes or herself who had first introduced the topic of this friend, nor could she see that this had any bearing on the matter). What Mrs Hughes *had* said was that she must be sure to bring the friend to the platform demonstration, which she accordingly did.

The members of the Society who attended the meeting at the Caxton Hall were able to interrogate five out of the eighteen persons who received messages. All five had previously been in contact with Mrs Helen Hughes.

It had been announced by the Chairman that any recipient of a message was invited to leave his name and address with the Secretary of the London Spiritualist Alliance, under whose auspices the meeting was held, so that further particulars could be obtained. The Alliance very kindly consented to communicate to the Society any information thus obtained, and also the answers to a questionnaire embodying the three points mentioned above, which was sent out to those who left their addresses. The questionnaire asked :

1. Have you before attended a public meeting with Mrs Helen Hughes and received a description from her ? If so, with what results ?
2. Have you ever had a private Sitting with Mrs Hughes ?
3. Have you any friends or relations who have sat with Mrs Hughes ?

The London Spiritualist Alliance received and communicated to the Society replies from eight recipients, three of whom had already been interrogated by our members after the meeting. In all, therefore, particulars were forthcoming from ten of the eighteen persons who were given messages. Of these ten, five had had private Sittings with Mrs Hughes, one was personally known to her, and one had received the same message at two previous platform meetings with this medium. The remaining three are therefore all whose messages we can profitably study in connection with our problem.

The first whom we will consider is a gentleman who had not previously sat with Mrs Hughes, though some of his friends had had Sittings with her. The message is recorded in the shorthand note as follows :

Mrs Hughes : " Sir, have you just come or have you ever lived in the Lake District ? "
Recipient : " Yes ".
Mrs Hughes : " Just sitting in the seat against you is a lady who belongs to you. She says, ' Tell him I am here too, but you would almost think Elizabeth was comparing the Lake District to London. Tell him to try again. Get on with your development.' I have to tell you from Dr Hawke to go on, and not turn back. He says, ' He is right this time ' ".

The recipient writes as follows :

" She was correct in thinking I came from the Lake District. I cannot place ' Elizabeth ', but my recollection of that remark was that I was comparing London to the Lake District, and was advised to remain where I am, which is to me quite an evidential remark. I feel sure she said ' Dr *Hall* ', not ' Dr Hawke ', and that he advised my going on ' sitting for development '. This makes sense, as Dr Hall is a near neighbour, who is interested in these subjects, and sometimes discusses them with me. He tells me he knows Mrs Hughes, having ' sat ' with her in Edinburgh. . . . I know no Dr Hawke."

One of the members of the Society who took longhand notes gives the salient features of the message as " Lake District, Elizabeth, Dr. Hall." This supports the recipient's view that it was Hall and not Hawke that Mrs Hughes said.

There was here the obvious possibility that normal leakage might have taken place through Dr Hall, who knew Mrs Hughes and had sat with her in Edinburgh. Through the kindness of Miss Phillimore, the secretary of the London Spiritualist Alliance, I was given permission to write to the recipient in order to obtain more evidence on this point. He answered as follows :

> " Thank you for your letter about Mrs Helen Hughes, which I was only waiting to answer till I heard from Dr Hall.
>
> He has now given me a *definite denial* of ever mentioning my name to her—as I knew he would. He is not my doctor, and I do not know him intimately, but occasionally I have been able to lend him books on these subjects, of which I have a great many."

How, then, was Mrs Hughes able to associate this recipient with the Lake District, and to connect him with Dr Hall? Chance coincidence is obviously out of the question. Out of a large audience it is unthinkable that Mrs Hughes could select someone at random and could by luck correctly place him as connected with the Lake District, to say nothing of Dr. Hall. The only normal explanation would seem to be that some other friend of the recipient had in a Sitting with Mrs Hughes conveyed the information that a person connected with the Lake District and with Dr Hall was likely to attend the meeting. But it must be noted that the information would have to be sufficiently detailed to enable Mrs Hughes to *recognise* the recipient, whom she had never seen, and to identify him from out of a large audience in a public hall. This seems to be highly unlikely.

In our next case the recipient was not selected by Mrs Hughes, but responded to a question addressed to the audience at large. The recipient had had no previous connection with Mrs Hughes, but she has been a spiritualist for the last fifty years, so there is certainly a possibility that she may have been recognised. The shorthand report is as follows :

> Mrs Hughes : " There is an old gentleman here named Levi. Anyone know him ? "
> Recipient : " I know Levi ".
> Mrs Hughes : " There is something strange for him, but he is standing with you. He says, ' Levi is here ', and someone called John too. Have you been suffering lately ? Do you know what Levi says ? Levi says, ' Tell Nellie we have helped to pull her round '. You have been suffering all over your body. Levi says ' Tell Nellie she is all right in our hands, and we will not fail her '. Do you belong to Bournemouth ? "
> Recipient : " I know Bournemouth ".

Mrs Hughes : " There is a lady standing with you who belongs to Bournemouth ".
Recipient : " Yes ".
Mrs Hughes : " Dr Gilbert and Mrs Meddleson ".
Recipient : " Yes ".
Mrs Hughes : " Did she spend her holidays with you ? "
Recipient : " Often ".
Mrs Hughes : " She spends her holidays now over here which is much better. Now I hate to say this to you. . . . Have you twins ? "
Recipient : " No ".
(Another member of the audience said, " She is a twin ".)
Mrs Hughes : " Thank you. Did you know the Wolff family ? "
Recipient : " Yes, very well indeed ".
Mrs Hughes : " I have to say, ' Tell her we want to let her know we can speak to her ! Don't worry about your health will you ? "
Recipient : " No, I won't, thank you ".

The recipient's Christian name is Nellie. She disclaims " John ", and " Dr Gilbert ", but knows Levi, Mrs Mendelssohn (given as " Meddleson ") and the Wolff family. As regards the latter, she notes : " The lady next to me . . . is one of twins . . . her father's name was Wolf." This lady was herself the recipient of a message. She is well known to Mrs Hughes and had had private Sittings. This naturally tends to invalidate the allusions to twins and to the Wolff family, as Mrs Hughes may well have known these facts about the lady sitting next to the recipient. There is also the possibility that Mrs Hughes may have identified the recipient as a friend of this lady whom she knew.

The third case is certainly the best, the gentleman in question never having attended a spiritualistic meeting before, and being quite unconnected with Mrs Hughes. He says that some of his relations had been interested in the spiritualist movement, but that none knew Mrs Hughes.

The message was as follows :

Mrs Hughes : " Sir, did you know anyone called Bromwell ? Mr and Mrs Bromwell have come to your side . . . or is it Bramwell ? Not a relation, but knew you as a boy. They say, ' Ask him if he knew a policeman '. He says, ' Will you tell him Jack is here ', and as Jack comes along, I can just see him as if he has taken a hat from his head and placed it on your knee. He says, ' I want you to know we have a happy home here and one day you will enter therein '. I am touching a soul who was very doubtful, wondering if life went on or not. Jack, Renie, Annie and Sam want you to know it is all true. Do you know Douglas ? Or, you are Douglas ? Renie says, ' Don't forget I am helping my boy and helping you ' ".

The recipient's annotations are as follows :

> " I cannot recall the name of Bromwell, but knew several Bromleys who have passed over. I also had a nephew who served in the police force. Rene was my late wife, Jack our dear doggie who survived her by six months. Sam was an old friend, and Annie, although alive, I believe, his wife. Regret that I cannot fix Douglas. The boy mentioned is my son."

In answer to further inquiries the recipient says that Annie, the wife of Sam, is still living, but that this Annie's mother, who was also called Annie, and was the recipient's Aunt, is dead, so it may be that it was she who was intended.

There are, of course, several discrepancies. If the Jack mentioned was intended for the dog, he can hardly have been seen to " (take) a hat from his head and (place) it on (the recipient's) knee." And he can certainly not have made the speech, " I want you to know we have a happy home here and that one day you will enter therein ", of which Jack seems to have been the author. Nor is " Ask him if he knew a policeman " a very direct way of " getting over " that the recipient's nephew had served in the police force. But for all that there are some striking things in the message. Renie is not a common name, and although Sam and Annie are very usual, they are by no means certain hits.

Bromwell or Bramwell for Bromley is not so good, and Douglas is a misfire. In all we have Renie, Sam, Annie, Bromwell or Bramwell, Jack and Douglas ; six names, of which Renie, Sam and Annie are correct, Bromwell is partly correct, Jack is significant but incorrect in the context, and Douglas is wrong. There is also the attribution of a son to Renie, which is correct. All this would certainly appear to be above chance expectation, and it is very difficult to see how under the circumstances there can have been any normal leakage.

Our examination of this meeting at the Caxton Hall shows that a large majority of the recipients of messages have had previous contact with Mrs Hughes—generally in the form of private sittings ; and that of the three cases examined, where no contact could be found, two certainly provide a *prima facie* case for supernormal cognition. It is impossible to say more ; but there can be no doubt that the investigation proves the desirability of further research into Mrs Hughes' platform work.

On the occasion of a private platform demonstration given by Mrs Hughes at the London Spiritualist Alliance on December 1st, 1938, the Alliance suggested to the Society that we might combine with

them to share the cost of taking a shorthand note, it being agreed that names and addresses should be taken of the recipients of messages, and a questionnaire sent out as before, the replies received to be communicated to the Society. We were naturally very glad to avail ourselves of this kind offer.

At this meeting 19 messages were given, and 14 recipients left their names and addresses. But of the 14 only eight replied to our enquiries. Three of these had had previous private Sittings, leaving us 5 cases for examination.

One of these is so slender that it is not necessary for me to quote it in detail. The name Harold was given which was significant for the recipient's deceased brother, but the recipient did not think at the time that the message was intended for him at all.

Our second example is also weak. Mrs Hughes said :

> " Is there anyone who knows a young man who was killed by aeroplane ? "
> RECIPIENT : " Yes ".
> MRS HUGHES : " Eric. I see a beautiful picture of where he is, I wish I could show it to you—he has got a plane—Eric (or Ulrich) passed out under those conditions, but he is back to tell you he is still going on, and he says ' Tell her I am working hard with scientific researches '. Are you sitting for development at all ? "
> RECIPIENT : " Not now. I have been ".
> MRS HUGHES : " I am very sorry to hear you have given it up, because you are in touch with Eric and with many other souls, and I know you could do a lot of good if you would just give way ".
> RECIPIENT : " I have communicated with him ".
> MRS HUGHES : " You could have gone on further. Eric is very excited because he can come to you ".

The name Eric is correct for a near relative of the recipient who was killed in an aeroplane accident. It must be noted that Mrs Hughes began by asking if there was anybody who knew a young man killed by aeroplane ; but the name Eric was added after the recipient had replied. But there are obviously loopholes for normal leakage here.

The following case is rather better :

> MRS HUGHES : " There is a gentleman standing at my side : does anyone know old Mr Jackson ? "
> RECIPIENT : " Yes ".
> MRS HUGHES : " He is rather an elderly gentleman and suffered very much with chest trouble, this is what he has shown. It was not altogether the chest, it was the heart, he has come along and says, will I tell you that both Mr and Mrs Jackson have come along

to see you, and especially did Mrs Jackson know you in your younger days. Just today in your home you would utter these words, ' If only those you loved and who belonged to you could come back to speak, you would be content and convinced.' I want you to be convinced that Mr and Mrs Jackson have come along to you tonight. Will you tell me, did you post a parcel for someone, if it was not for yourself, it has gone abroad ? Do you know if old Jackson lived abroad ? "

RECIPIENT : " No ".

MRS HUGHES : " He is showing me something of someone abroad, he even speaks of the house he lived in, and is saying, ' Tell her I have met Mary over here and she is not to give way in life '. He means you, Madam. There have been times when you have said these words, ' Is there anything left to live for ? ' I have got to say ' Yes '. Mrs Jackson wants to reassure you, there is a lot to live for if you will only remember they are with you instead of being lost and gone from you for ever. Did you know an old lady who was burned, there is something wrong with her hand ? I do not know whether it was in her late days, or when she was younger, but her hand was scalded. Is there anyone belonging to you named Lizzie in spirit, an Aunt ? William is with her, would you know that, William ? "

RECIPIENT : " I would ".

MRS HUGHES : " It is Lizzie who has brought him, and her arm was burned. Mr and Mrs Jackson ask me will I convey their grateful thanks to you, and be sure to look up to them when you are needing help ".

The recipient writes that she knew a Lizzie, not an Aunt, who had a brother called " Willie ", but that they had no connection with Mr and Mrs Jackson, who were the recipient's Aunt and Uncle. They lived in Ireland and she saw very little of them, but more in her younger days, as stated in the message. As to Mary, Mrs Jackson has a daughter of that name still living, but there may have been a deceased relation of the Jacksons called Mary. The recipient did not know many of the family which was large. She knew nothing about Lizzie's scalded hand. The recipient says, " I don't know anything about the parcel abroad—but I did make a similar remark about ' If only those you loved, etc.' "

Here the recipient, having claimed " Old Mr Jackson ", was given the additional information that she had known the Jacksons better in her young days, which was true ; and it was also suggested that Mr Jackson lived abroad. To this the recipient said, " No " ; but in fact the Jacksons lived in Ireland, so it was not far wrong. She was then given, as associated with the Jacksons, a Lizzie who was said to be her Aunt and who was with William. The recipient had known a

Lizzie who had a brother called Willie, but she was not her aunt and had no connection with the Jacksons, whereas Mrs Jackson *was* the recipient's aunt. As the names Lizzie and Willie are far from uncommon, it is not too much to suggest that chance might be responsible for the partial successes which Mrs Hughes scored in this message.

The next recipient had attended a public meeting with Mrs Hughes, but had not received a message before. Apart from this there was no connection. Mrs Hughes said : " Are you carrying something, either a cross or something in your bag that is gold ? "

The recipient had in her bag a silver cross which had belonged to her deceased brother. She had brought it on purpose in the hope of getting a message. The name Alec was given correctly for the communicator, and also the name Helena as for the recipient herself. This is striking as even if the recipient had taken the cross out of her bag during the meeting, and Mrs Hughes had seen it, that would not explain the correctness of the names.

The last case is the most evidential so far as names are concerned. Mrs Hughes mentioned Annie, Mrs Villma or Gilma, Katey Smith, Mrs Smith, Daisy, Mr French and Mrs French. The recipient says that Annie, Katey Smith, and Daisy are her deceased sisters. Mrs Smith is her mother. She knows no one called Villma or Gilma, but remembered afterwards that she had known a Dr French. (French was mentioned as being a Doctor or a Dentist).

Here again we have at least one message which provides a *prima facie* case for supernormal cognition. It is hoped that by repeating our procedure at future demonstrations, something more positive may be forthcoming, and we have therefore arranged with the London Spiritualist Alliance that a shorthand note shall be taken at both of two demonstrations which they are holding in February. The ideal arrangement, of course, would be to have a special meeting at which the entire audience was made up of people who had had no previous contact with Mrs Hughes. But owing to the great demand for Mrs Hughes' services, this is by no means easy to arrange. In the meantime, it does not seem that we can do better than to continue to collect evidence in the way that I have described.

The thanks of the Society are due to Miss Phillimore and the London Spiritualist Alliance for their kind co-operation ; to those members of the Society who lent their services on the occasion of the meeting at the Caxton Hall, and especially to Mrs K. M. Goldney ; and to Lord Charles Hope, who generously paid the costs of the shorthand writers.

A CASE OF PURPORTED SPIRIT-COMMUNICATION DUE ACTUALLY TO SUB-CONSCIOUS OR TRANCE MEMORY POWERS.

By K. M. Goldney

Report of a clairvoyant spirit-message[1] *given by Mrs Helen Hughes (medium) on February 10, 1938 at a Meeting held at the London Spiritualist Alliance, 16 Queensberry Place, London, S.W. 7.*

Preliminary Statement

Mrs Helen Hughes lives in the provinces (County Durham), but comes to London two or three times a year and gives both public Meetings and private Sittings under the auspices of the London Spiritualist Alliance. I have attended at various times about a dozen of such public Meetings.

Mrs Hughes has an extremely charming personality, and personal contact with her will confirm, I consider, an initial judgment of complete integrity. Her clairvoyant powers at public meetings are, *prima facie*, very remarkable. Whilst giving the messages on such occasions, the medium appears to be in a condition of semi-trance; but in private sittings she also attains a condition of deep trance. Her platform work would appear to be particularly suited to psychical research, inasmuch as she gives specific names (both Christian and surnames), dates, place names, etc.; and it was I who urged upon the Research Officer the desirability of endeavouring to bring about the joint investigation of which the initial stages are here reported.

The disadvantage of public Meetings, which all attending them must feel, is the unlikelihood in the packed hall of obtaining a message oneself, and the impossibility of evaluating the messages given by the medium to others. Since I had never myself received a message in these circumstances, and with the object of finding a case which would bear the strictest scrutiny, I started some four years ago to question a few persons who had received striking messages, and was disappointed to find that each person so questioned happened to have had, at some time or other, a private sitting with the medium or had a relative or friend who had had private sittings. In no instance, and on no occasion, was I lucky enough to find a person who had had no

[1] The terms "spirit-message", "tranee", "communicator", etc., etc. are used for convenience in a general and recognised sense, and their use does not necessarily entail any specific belief regarding them.

contact whatsoever, themselves or through relatives or friends, with the medium.

It is natural enough that the audience on these occasions should be chiefly made up of those already interested in Spiritualism and their friends, and natural also that members of the audience should have made a point of having a private sitting with a remarkable medium. Further, the fact that they had had a private sitting in no way PROVES that a subsequent spirit-message at a public Meeting can be explained away. Nevertheless the lack of any training in research methods makes the average person a hopelessly bad " sitter ", and it would be extremely difficult to establish a claim to para-normality where the receiver of a message at a public Meeting had previously had a private Sitting with the medium at which verbatim notes had not been made. The reader may usefully remember Sir Oliver Lodge's dictum (*Survival of Man*, 10th edition, p. 189) that " hyper-scepticism is more conducive to the development of the subject than hyper-credulity ".

After attending a certain number of public Meetings, I decided to have a private sitting myself with Mrs Helen Hughes. During this Sitting—the only one I have had with her—I carried out an experiment which proved to have interesting results.

Private (Trance) Sitting with Mrs Helen Hughes (booked anonymously) May 15, 1936 at the London Spiritualist Alliance.

At this trance Sitting I myself took down shorthand notes of what was said : not verbatim notes, but notes of *all* the names given and any important rejoinders made by myself, etc. My notes were typed out and filed after the Sitting.

The Sitting was entirely negative ; although I have had some strikingly good sittings, I am by no means a consistently good sitter and this was possibly the worst sitting I had ever had. My notes show that 28 (or more) names were given, only three of which corresponded by chance with names of deceased persons who had been known to me (a remarkably small number even for chance !) and the relationship of these three was not correctly given.[1] Towards the end of this hopeless Sitting I decided to make an experiment (see forward).

[1] I was forcibly reminded of Myer's description of Mrs Piper's Control, Phinuit (*Proc. S. P. R.* Vol. VI, p. 440) : " There were some interviews throughout which Phinuit hardly asked any questions and hardly stated anything which was not true. There were others throughout which his utterance showed not one glimmer of real knowledge, but consisted wholly of . . . random assertions ".

Meeting for Clairvoyance given by Mrs Helen Hughes at the London Spiritualist Alliance on February 10, 1938.

At the time of my private Sitting (May 15, 1936), I do not think Mrs Hughes can have known me by sight. Following this private Sitting and the experiment made during it, I made a point of speaking to Mrs Hughes without an introduction on the few available occasions of her visits to London, but by February 1938 I think it probable she knew my name : in any case she knew me well enough to recognise and greet me in passing.

On February 10, 1938 I attended a packed Meeting at the London Spiritualist Alliance, at which Mrs Hughes was giving clairvoyance. I sat far back in the large room beside my friend Mrs R. During the Meeting Mrs Hughes pointed to Mrs R., and the following conversation took place (approximate rendering).

> MRS HUGHES : " Now I want THAT lady " (pointing). Mrs R. held up her hand. " Yes, that's right, I want you. There is someone here who has come for you : BESSIE—wait—(apparently listening to a spirit communicator)—BESSIE WHITE. Do you know BESSIE WHITE ? "
> MRS R. : " No I don't ; I can't think of anyone of that name ".
> MRS HUGHES : " That's funny. BESSIE WHITE—are you sure you don't know her ? No ; wait ; it is not for you but for the lady sitting next to you ". *I* then held up *my* hand. " Yes, that's right. Bessie White is here for *you*. Do you recognise her ? "
> K. M. GOLDNEY : (After a slight pause) " Yes ; I understand that name ".
> MRS HUGHES : " BESSIE WHITE—and ALEC—ALEC WHITE. Do you know him too ? "
> K. M. GOLDNEY : " Yes ; I understand that also ".

As Mrs Hughes gave the name BESSIE WHITE to me, my mind flashed back to my private Sitting with her of a year and nine months previously. What I dimly remember justified me, I felt, in claiming " I understood " the names given. Nevertheless I had no knowledge of any Bessie White or Alec White in actual fact, nor any clear memory of having heard the names before.

On returning home I immediately looked up the file containing the notes of my private Sitting of May 15, 1936 and read the following commentary, which I had attached to these notes :

> *Sitting with Mrs Helen Hughes* (*booked anonymously*) *May 15, 1936.*
>
> " I had been told (Mrs Hughes) was not so good at private Sittings (as at public Meetings), but determined to have one with her. The attached summary was made from shorthand notes taken at the

time . . . by myself. When I saw what rubbish was coming through, I determined towards the end of the Sitting to give false recognition to a set of names, in order to see whether these would later be given back to me at a public Meeting. I intended to go to such and possibly speak to Mrs Hughes (*i.e.* draw attention to my presence at the Meeting) with this in view. At a given point in the Sitting—THE WHITE FAMILY—I begin to reply as if the communicators were real and the names veridical ".
(That is to say, I claimed recognition of the names and relationship given to me by the medium, although in fact the names were unknown to me.)

Turning then to the given point in my summarised shorthand notes at which I claimed this recognition, I found this :

Mrs Hughes : " BESSIE WHITE is here. Was she a relative ? "
K. M. Goldney : " Yes ".
Mrs Hughes : " Has Mrs White got her son with her ? "
K. M. Goldney : " Yes ".
Mrs Hughes : " All the White family are here—ALEC WHITE ".
K. M. Goldney : " I didn't know him myself—I knew *of* him ".
(Note : The conversation is given in my notes in summarised form : it must be understood that at the time I indulged in a certain amount of dramatisation, suitable to the experiment.)

A description was given by the medium of Mrs White's sudden death by burning. Mrs White then purported to control the medium and communicate directly herself, saying she was doing so for the first time. She spoke with a strong Scotch accent (I had previously agreed with the medium that I had Scotch relations). Her talk was that of a farmer's wife and she spoke of farming conditions, reminding me of a lambing season we had spent together in Scotland (I had never been in Scotland at that time, know nothing of farming or country conditions, and have no Scotch relations except on a grandmother's side some four generations or so back).[1]

Commentary

When making my experiment on May 15, 1936, I wondered whether the false information I was giving and the remarks I was

[1] The reader is again referred to the similarity here to the trance Control of Mrs Piper (Rector) ; and his acceptance of misleading suggestions by the sitter. " Rector ", says Sir Oliver Lodge, (*Survival of Man*, p. 216) " . . . is evidently in the position of receiving ideas by a sort of dictation, and need not always be able clearly to discriminate their source, whether from the ultra-material or from the material side ". Experimental observations of this sort may prove to be of the utmost importance in leading us eventually to an understanding of the trance state.

making to Mrs Hughes whilst she was in a trance would be given back to me at some public Meeting in the near future in the form of a spirit-message. I attended each subsequent Meeting she gave in London and drew attention to my presence by greeting her where possible, but no message came. By February 1938 I had forgotten all the details of my private sitting—had almost forgotten, indeed, the whole experiment—and only the echo of a memory was stirred when the names BESSIE WHITE and ALEC WHITE were given to me. The lapse of time had wiped away all conscious memory of the meaningless details and names given at my private Sitting, and only reference to my files showed that names I had claimed as veridical out of a large number of unrecognised names twenty-one months previously coincided with those given me again so long after.

Mrs Hughes is a very busy and successful medium, booked up (she has told me) for months, even years, ahead. On referring to an official at the London Spiritualist Alliance, I was told that she gave usually 16 private Sittings a week whilst in London, (their rule being that no medium should give more than three Sittings a day); and that if one reckoned an average of 15 Sittings a week for 10 months of the year, it would probably be a fairly accurate surmise (the other two months being free from Sittings on account of holidays, days spent in travelling, occasional sickness, etc., etc. This would of course exclude any unusual year of sickness, which would curtail the number of Sittings given in a year.)

At the rate of 15 Sittings a week for 10 months of the year, we can suppose that Mrs Hughes gave, very roughly, some 1,000 or 1,100 Sittings between the dates of May 1936 and February 1938 (this allows four or five months free of Sittings and sixteen or seventeen months' work out of the twenty-one months).

At my Sitting she produced 28 names. If this can be considered normal, 1,100 Sittings would produce some 30,000 names.[1] A large part of Mrs Hughes' life is spent going from one town to another giving public Meetings and private Sittings to a succession of anonymous or seldom-seen Sitters. Yet in the semi-trance condition pertaining during her clairvoyance, one almost a stranger to her could be correctly given names for which significance had been falsely claimed 21 months before at a single Sitting. This, I consider, goes far beyond any feat of normal memory, and constitutes an example of the striking possibilities of trance memory, presumably akin to the feats of memory (if such it can be called) pertaining to the hypnotic state.

[1] Weeks containing public meetings would probably somewhat *increase* the weekly quota of sitters but *decrease* the weekly reckoning of names.

In making this experiment I had in mind similar experiments made by Sir Oliver Lodge and Richard Hodgson with Mrs Piper, " in order to see what effect on the medium's trance mind was produced by a carelessness or untrustworthiness of Sitters themselves ". For example, Professor E. C. K. Gonner, Lecturer on Economics at University College, Liverpool, and a colleague of Sir Oliver (then Professor) Lodge, was purposely introduced to Mrs Piper as Mr McCunn, another colleague, in order to test the effect on the medium's trance mind. The significant result justifies the means, and I have little doubt that Mrs Hughes herself and students of psychical research will have the good sense to recognise the legitimacy of such an experiment, *provided one is careful to appraise it at its due value and no more.*

Conclusion

It should be unnecessary to point out that the demonstrated fact that Mrs Hughes can store in her sub-conscious mind the minutest details concerning a Sitter and reproduce these months, possibly years, later, does NOT invalidate the hypothesis and claim that she obtains clairaudient messages from the dead. Such a conclusion would be quite unjustified. Here again we can apply to Mrs Hughes's trance the attributes ascribed by Sir Oliver Lodge to Mrs Piper's Control (Phinuit) when he writes : " The attitude is . . . as of one straining every clue and making use of the slightest indication, whether received in normal or abnormal ways ; not, indeed, distinguishing between information received from the Sitter and information from other sources ". (*Proc. S. P. R.* Vol. XI, p. 449).

The conclusions we *are* justified in making can be summarised as follows :

(1) The experiment undertaken would appear to demonstrate the possession by this medium (and presumably by others) of subliminal powers of memory in the trance or semi-trance state quite beyond the possibilities of conscious memory. As a result it renders null and void the contention often put forward by those unaware of such subliminal powers that it is " absurd " to suppose that a medium can remember facts concerning the hundreds of Sitters she sees in a year.

(2) Admitting that these possibly limitless powers of memory are in force during trance or partial-trance conditions, it becomes obvious that if we are to test the spiritualist hypothesis, only definite research work under test conditions is of value, and Meetings should be arranged of selected Sitters NONE of whom have had any

contact whatever, direct or indirect (through friends or relations), with the medium, and all of whom are willing to annotate, and answer questions regarding, the material given them with the object of obtaining positive results.

As I have already stated, I consider Mrs Hughes's powers remarkable, and with patience and perseverance—modifying one's procedure in the light of experience gained by experiment and practice—I have every hope that we shall obtain conclusive results.

REVIEW

Adventures in Psychical Research by C. E. M. Joad. *Harper's Magazine*, June 1938.

Dr Joad is the Chairman of the London University Council for Psychical Investigation, the Head of the Department of Philosophy and Psychology at Birkbeck College, and a well-known writer on philosophical subjects.

It is therefore natural that an account of his personal experiences as an investigator of psychic phenomena should attract considerable attention and deserve serious consideration. Dr Joad begins his article as follows : " Let me begin by stating my credentials. I have had at different times a certain amount of firsthand experience of what I will non-committally call abnormal phenomena. This experience has been due largely to the facilities afforded in London by the National Laboratory of Psychical Research . . . " " By the courtesy of the Director of the Laboratory, Mr Harry Price, . . . I have had the advantage of sitting with a number of well-known mediums and have witnessed at different times a considerable number of varied phenomena."

" Before I proceed to a description of some of the phenomena I have witnessed I had better state my beliefs in order that the reader may be in a position to discount my bias."

From the above sentences it would appear that all the occurrences mentioned by Dr Joad in this article are described as having occurred under his personal observation, and this impression is confirmed by the frequent use throughout the article of the first personal singular and plural. The occurrences described all relate to what are known as " physical " phenomena, a branch of psychical research in which it is notorious that the debate as to what in fact happened has always been particularly acute, and accordingly first-hand accounts of competent observers are of the greatest value. Sittings with four mediums are described. With one of them, Eva C., Dr Joad had a sitting or sittings at the Institut Métapsychique in Paris. The other three mediums named in the article are Mrs Duncan, Rudi Schneider and Eleanore Zügun. Very full records of the investigation of these three mediums at the National Laboratory have been published by

Mr Harry Price, either in the *Proceedings* and *Bulletins* of the National Laboratory or in book form.[1]

On comparing Dr Joad's accounts of these mediums with the very full accounts given by Mr Harry Price, the student cannot fail to be struck with several discrepancies of the kind which have in the past made the study of physical phenomena so problematical. To take first the case of the Duncan mediumship; after mentioning that Mrs Duncan was investigated for some time at the National Laboratory of Psychical Research, Dr Joad proceeds to describe various incidents in a very positive manner and in terms which, taken in conjunction with the sentences quoted at the beginning of this review, appear to indicate that Dr Joad was a witness of what he describes. He writes, for example, " One could put out one's hand and touch the stuff [*i.e.* ectoplasm], which felt like the white of a hard-boiled egg and was faintly luminous. The whole performance was extraordinarily impressive, and for a time a number of us were inclined to ' believe '. . . . . It was an X-ray photograph which put us on the track of this deception. One day we photographed Mrs Duncan . . . On no other occasion have I seen ectoplasm."

But the 1931 *Bulletin*, " Regurgitation and the Duncan Mediumship ", in the account of each of the five sittings held with Mrs Duncan at the National Laboratory, gives a list of all the persons said to have been present at each sitting, and on no occasion does Dr Joad's name appear in these lists. Later in the same *Bulletin* (App. C., p. 105), there is printed a " List of Sitters who examined the Duncan Manifestations ". This gives the names of 27 persons present at one or more of the sittings and again Dr Joad's name does not appear. In addition to the persons named, the list mentions, without naming them, the presence of an analyst, an obstetrician, and a physician, but there is nothing to suggest that Dr Joad, who we believe does not hold any of the requisite qualifications, was one of these three specialists, nor is his name to be found in the index. Either therefore both the lists of observers at separate sittings and also App. C. of this *Bulletin* are incomplete, or Dr Joad was not present at the Duncan sittings which he describes. If the latter be the true explanation, the language he uses in describing them is certainly

[1] " A Report on the Telekinetic and other Phenomena witnessed through Eleanore Zügun." *Proceedings* of the N.L.P.R., Vol. I (Part 1), January 1927.

" Regurgitation and the Duncan Mediumship." *Bulletin* of the N.L.P.R., 1931.

" Rudi Schneider ", by Harry Price. (Methuen, 1930.)

" An Account of some Further Experiments with Rudi Schneider." *Bulletin* IV of the N.L.P.R., 1933.

unfortunate. For example, the sentence " On no other occasion have I seen ectoplasm ", with which he concludes his account of the Duncan sittings, can hardly be interpreted otherwise than as an assertion that at one of the Duncan sittings at the National Laboratory he *did* see ectoplasm.

There are also minor discrepancies between Dr Joad's and Mr Harry Price's accounts of the Duncan mediumship. When Dr Joad speaks of Mrs Duncan " by some means getting her hands free from the silken sack in which she was encased ", he has failed to understand the nature of the one-piece garment, having sleeves and " gloving " the hands, which the medium wore at the sittings ; as there would be no need for her to free them from this garment in order to use her hands to manipulate ectoplasm as suggested. Then again, Dr Joad writes " One day we photographed Mrs Duncan. This of course was done with her consent but—a fact to which she did not consent because she did not know of it—one of the photographs was an X-ray photograph, and this showed a small safety-pin, cosily nestling in the middle of Mrs Duncan's inside ". This apparently refers to what occurred at the 4th séance on the 28th May 1931, the only occasion when an X-ray photograph was either attempted or carried out. The incident is described at some length on pp. 61–63 of the *Bulletin*. After stating (p. 61) that the medium's husband consented to his wife being X-rayed, but that she was uneasy at the preparations being made for the X-ray, the *Bulletin* continues (p. 62) " and then the unexpected happened, Mrs Duncan *demanded* that she should be X-rayed ! Although we knew it would now be useless, we consented, as the equipment had taken half a day to instal and there was just a very remote possibility that something might be revealed.

" So the medium was X-rayed after all, Dr William Brown being present. We made three radiographs, one of the abdomen, one of the chest or thorax, and one of the skull (reproduced Plate XXV). They were developed immediately after and no abnormalities can be detected. In any case, the depth of the medium would preclude any anatomical peculiarities from revealing themselves. As a matter of fact, the radiographs of the abdomen and thorax are merely blurs."

Nothing is said about any safety-pin.

As regards the phenomena of Rudi Schneider the problems raised by Dr Joad's article are rather different. Dr Joad writes, " the most remarkable of these phenomena [meaning apparently telekinesis] which I have seen was connected with the Austrian medium Rudi Schneider. . . . In séances lasting over two separate years Rudi produced, under strictly controlled conditions, remarkable phenomena.

Of these perhaps the most outstanding seen by me was the raising of a handkerchief without visible agency into the air where it tied itself into a knot."

Dr Joad does not specifically state where the Rudi Schneider séances, to which he refers, took place, but in view of the sentences quoted at the beginning of this review it is a natural inference that, unless there is a definite statement to the contrary, he is throughout his article speaking of séances at the National Laboratory, and that the phrase " séances lasting over two separate years " is meant to indicate the two series of séances held at the National Laboratory from April 1929 to January 1930, described in Mr Price's book " Rudi Schneider ", and from February to May 1932, described in *Bulletin* IV of the National Laboratory. From Mr Price's book and this *Bulletin* it would appear that Dr Joad was present at one sitting in each of these two series, namely that of November 25th 1929 and that of February 23rd 1932, the latter being described in the *Bulletin* as a negative sitting. Of the sitting on November 25th 1929, on the other hand, the report in Mr Price's book (pp. 75–88) says, " the latter portion of it was brilliant ". In addition to the report given in the book, which consists of a transcription of what was dictated into a dictaphone during the sitting, there is reproduced in the book an account contributed by Dr Joad himself to the Press the day after the sitting. Neither the official record of the sitting, nor Dr Joad's contribution to the *Evening Standard*, (26th Nov. 1939) contains any reference to this very striking incident of the handkerchief, although several other phenomena are mentioned as having occurred at the sitting in question.

Dr Joad continues " Nothing could be *seen* to touch the handkerchief ; but, on the view suggested above, that which made contact with it must, presumably, have been something material. Accordingly X-ray photographs were taken with a view to detecting what, if anything, was in contact with the handkerchief ". After a description of the photographic procedure he continues, " As the result of a number of experiments photographs were obtained showing some dark substance making contact with the objects, a substance which, invisible to the human eye, was apparently visible to the X-ray ".

But the reports of the Rudi sittings at the National Laboratory make no mention whatsoever of X-ray photographs being taken at either of the two sittings when Dr Joad was present, or at any other sitting.

If Dr Joad is referring to sittings elsewhere than at the National Laboratory, it is a pity that he does not specify the place, or time, or refer the reader to any contemporary account of the incident.

Nor are Dr Joad's references to Eleanore Zügun free from difficulty. He writes as follows : " A curious incident happened to myself. On the last day of Eleanore's visit to the Laboratory I, together with a number of those who had " sat with " her during the preceding fortnight, went to say good-by. There was, I think, some sort of tea party and a fair-sized crowd of people had assembled. Owing to the press of people, I did not actually speak to Eleanore, but waved my hand to her across the room and left after a short visit. I was going to the country, and in the train took a book out of my bag to read ; it was a new book and some of the pages were uncut. I felt in my pocket for my penknife ; the feel of it was certainly unusual and, pulling it out, I found that a crescent-shaped piece of metal—in point of fact the metal letter C—encircled the knife in such a way that so long as the letter was there it could not be opened. So tightly was the knife wedged into the encircling piece of metal that a mallet and chisel were required to remove it ".

Now, it is a curious fact that whereas Dr Joad is not mentioned at all in the *Proceedings* of the National Laboratory for January 1927, which are entirely devoted to the investigation of Eleanore Zügun, an experience remarkably similar to that described by Dr Joad occurred to the late Professor R. J. Tillyard, and is by him related at length on pp. 57–58 of these *Proceedings*. He also went to say good-by to her and actually shook her hand, and then like Dr Joad took train for the country. His train left Victoria at 8.20 for Rochester and to quote his words, " I had an evening paper, which occupied me about as far as Bromley. Then I took out Huxley's book from my greatcoat pocket and began to read it. By the time we reached Swanley, I had read eight pages and was then interested enough to be considerably annoyed when I found that the next few pages had not been cut. So I opened my greatcoat, put my hand down into the left pocket of my coat, and felt for my knife to cut the pages with. Then a curious feeling came over me. The knife did not feel like my knife at all. I drew it out and found firmly attached to the metal half-ring of the leather case enclosing it a white metallic C which effectively closed the case (see Fig. 4). I realised at once that it was the C which had been lost 11 days before from the notice-board of the ground floor of 16 Queensberry Place and the loss of which had been generally attributed to ' Dracu ', [a control of Eleanore Zügun.] "

It is certainly strange that Dr Joad and Professor Tillyard should both have experiences so strikingly similar on the same occasion.

Difficulties and discrepancies such as these are the despair of the psychical researcher. The general public is very badly informed on matters of psychical research, especially perhaps on matters con-

nected with physical phenomena. It is therefore of the greatest importance that all published accounts of such phenomena should be strictly accurate in detail: otherwise the whole subject is likely to be discredited in the eyes of intelligent enquirers. Dr Joad owes it to his position, both in the academic world and in psychical research to clear up the difficulties we have pointed out.

W. H. SALTER.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 159

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

By Whately Carington, M.A., M.Sc.

NEW SERIES, I

REVISION AND EXTENSION OF THE INTER-MEDIUM EXPERIMENT

ABSTRACT.—To the material derived from Mrs Leonard and Mrs Sharplin, and dealt with in Q.S.T.P. III, further data from Mrs Garrett have been added. Treatment of the whole material by the same general method as before, but avoiding certain pitfalls, shows that there is a significant relationship between the J and E reactions obtained from the different mediums. This is chiefly shown by a negative correlation between the values (J–E) obtained from Sharplin and Garrett, increasing from occasion to occasion. Further investigation shows that the magnitude of this correlation is not independent of the mediums' own reaction times; this supports the hypothesis of Interference, rather than of Interchange, as an explanation of the negative effect found. Collateral evidence is also discussed, and indicates that the true anti-chance odds [1] are considerably greater than the 49 to 1 actually evaluated.

The operation of some kind of external factor or influence is strongly suggested. This does not " prove " the autonomy of the " communicators " concerned, but constitutes supporting evidence in its favour as compared with the implications of a null result.

[1] This term is used as a colloquial convenience: To say that the anti-chance odds are n to 1 is equivalent to the expanded form " A deviation from the expected result of the magnitude observed would occur, as the outcome of chance alone, on the average only once in $n+1$ such experiments."

A man should never be ashamed to own he has been in the wrong, which is but saying . . . that he is wiser to-day than he was yesterday.—SWIFT

If there be no ashes, how shall Phoenix rise?—ANON

INTRODUCTORY.—In the course of my ill-fated Q.S.T.P. III,[1] I rashly opined that, before reaching the conclusions there given, I had "tried all the wrong ways first". This was optimistic, for Dr Thouless had little difficulty in showing[2] that exhaustion of wrong ways had been far from completed; and no one can agree with him more cordially than I in regretting that the aberrations to which he drew attention should have been immortalised in print.

As a matter of fact, so far at least as the work here considered is concerned, there was nothing wrong with the basic method employed, or with the data (except that there were not enough of them), or with the accuracy of the computations as such, so far as I am aware. There was a quite inexcusable error of logic involved in treating the data obtained from Mrs Leonard in two different years as if they had been obtained from two different mediums; and I was altogether at sea in my attempts to deal with the regressions ("slopes") observed and to combine the probabilities derived from these and other quantities into an overall test of significance. It is important to note that these sources of error have been rectified in what follows, not only because I am "wiser to-day than I was yesterday", but because I have enjoyed the benefit of adequate instruction and supervision from appropriate authority in the technical points involved.

As so often before, I must express my gratitude to Professor Fisher and the staff of the Galton Laboratory for many kindnesses; in particular, I am very greatly indebted to Mr W. L. Stevens for putting me right, or preventing me going astray, on innumerable occasions, and especially for looking through the relevant passages of the manuscript as a final check up on the validity of the statistical methods employed. He is not, of course, in any way responsible for the interpretation that I or any one else may tend to place on the results obtained.

I am also under no small obligation, which it is a pleasure to record, to Mr Oliver Gatty, Professor Broad and Dr Thouless for much valued advice, encouragement and help.

MATERIAL.—The material dealt with here consists of the reaction times obtained at four sittings each with Mrs Leonard (October

[1] *Proc. S.P.R.*, Part 149, Vol. XLIV, December 1936.

[2] *Ibid.* Part 150, Vol. XLIV, October 1937.

and November 1935), Mrs Sharplin (October 1935) and Mrs Garrett (September 1936). As stated in my earlier paper, there were actually five sittings with the first two mediums; but as external circumstances prevented Mr Drayton Thomas (to whom I am again indebted for the collection of observations) from obtaining more than four sittings with Mrs Garrett, I have thought it best to ignore the fifth Leonard and Sharplin sittings altogether, in the interests of simplicity and of using a compact block of data. This gives us a total of 2,400 observations from J and E through the three mediums. A total of 800 reactions from Feda were also obtained at the four sittings with Leonard and Sharplin; these led to no results of interest, but the figures are given in Table I B for the sake of completeness.

As explained elsewhere, the order of the words within each group of 25, was varied from occasion (sitting) to occasion; this variation was systematic for Leonard and Sharplin, but randomised by means of shuffled cards in the case of Garrett. Hereinafter the words 1–25 will be referred to as group A, 26–50 as group B, etc., while the occasions will be denoted by the Roman numerals I–IV.

Preparation of Data.—*a. Misheard, etc., words.*—In a few cases the stimulus word had quite evidently been misheard by the subject. For example, when the response "calm" is given to the stimulus "peach", or the response "absolve" to the stimulus "drive", there can be little doubt that these stimuli have been misheard as "peace" and "shrive" respectively. In such cases reference was made to the other occasions on which the same personality had responded to the same stimulus word. The times there recorded were expressed as percentages of the median values of the times of their groups, and the mean of these percentages multiplied by the median value of the times of the group affected gave the value of the time to be used in place of that recorded against the word misheard. Thus, if on occasions I, II and III the word in question had a time of 61 per cent., 75 per cent., and 65 per cent. the group median (mean value 67 per cent.), and on occasion IV, when it was misheard, the group had a median value 12, it would be given a time of 8, this being 67 per cent., to the nearest integer, of 12.

The same procedure, but using occasion instead of group medians, was adopted when working by occasions instead of by groups.

Such interpolation was only used when there could be no reasonable doubt of the mishearing; doubtful cases were allowed to stand. Two or three errors in typing were dealt with in the same way.

b. Over-long times.—As I have explained in an earlier paper, it sometimes happens that the subject's response is so long delayed

that the time elapsing before reply cannot fairly be regarded as representing a normal reaction. Where this has occurred, and an abnormally long time has actually been recorded (or no time, but the note " Long " given by the experimenter) I have substituted, as an upper limit, a time equal to 2·25 times the median of the group (or of the occasion, if working by occasions) concerned. This is, of course, arbitrary; but previous experience has shown that it is appropriate to data of this type, and certainly no exception can be taken to it. It is clearly preferable to allowing fantastically long times to stand or to using some still more arbitrary limit, such as 50 units (fifths of a second) in all cases, which would disregard the prevailing tendency to give relatively long or short times. It appears to err, if anything, slightly on the side of generosity, but a prolonged research would be necessary to determine the precisely optimal value, and there would be little to be gained thereby.

As a result of practical experience, however, Mr Drayton Thomas formed the opinion (which appears to be well justified) that these long delays were not, in general, chiefly due to the effect of the stimulus word, but to other causes such as local distractions or, more particularly, to the communicator or control momentarily " losing grip " of the medium. He accordingly adopted the practice of leaving such stimuli on one side for a minute or two and then giving them again after a short interval. This second attempt almost invariably elicited a reaction time of normal length.

I have carefully examined all such instances and find that in no case does any word show such delay as to require a repetition on more than one occasion for the same personality. It seems clear therefore that these delays are not characteristic of the word and personality concerned, so that, in order to give the fairest possible picture of the personality's reactions, it is better to use the RT obtained at the second time of asking rather than to treat the reaction as " over-long " and write 2·25 times the median for it.

To this rule I have made only one exception, namely a word which had to be *twice* repeated before a reply could be obtained and, moreover, gave a time of 233 per cent. its group median on an earlier occasion. In view of these facts I have had little hesitation in accepting this reaction as a genuine instance of an " over-long " time and substituting 2·25 times the relevant median.

c. Corrections.—In Q.S.T.P. III, pp. 216–7, I expressed some uneasiness lest the apparently significant results there obtained might possibly be due to the effects of practice or fatigue. The fact that the results in question were unreliable, in the sense of being obtained by unsatisfactory methods, does not lessen the necessity

of clearing up this point before proceeding to a re-examination of the material.

Actually, the likelihood of artifacts being generated in this way is small. In the cases of Leonard and Sharplin, the order of the words within each group of twenty-five was varied systematically, and in the case of Garrett was completely randomised by the use of shuffled cards, so that it is very difficult to see how any such effect could be produced. None the less, I thought it wise to examine the point in detail. I accordingly worked the value of the linear regression coefficient '*b*', of the variance absorbed by the regression, of the residual variance, and of the '*t*' used for testing the significance of '*b*', for each of the ninety-six sheets concerned. From the distribution of the *t*'s so obtained we can determine the likelihood of the regressions observed being the result of chance alone.[1]

The ninety-six sheets thus treated yielded one result beyond the ·01 level of significance, with P about ·005; there was another with P almost exactly ·01, another at about the ·02 point, and four at or very near the ·05 level. There is nothing surprising in such figures when so many as 96 sheets are involved, for we should naturally expect one result at the ·01 level, and others in proportion, as the result of chance alone, so that the divergence from expectation is small. As it happens, the most significant results occurred in the Garrett material, and as this was randomised we may fairly say that they 'must' have been due to chance. The distribution of the *t*'s shows that effects of the observed magnitudes and signs would arise from chance alone in more than 85 per cent., 75 per cent. and 50 per cent. of such experiments in the cases of Leonard, Garrett and Sharplin respectively. Finally, it is interesting to note that only one set of four sheets constituting the testing of a given personality on a given occasion, out of twenty-four such sets, shows a regression coefficient of the same sign (*i.e.* a uniform practice or fatigue effect) throughout: we should expect three from chance alone, so that from this point of view the incidence of practice and fatigue effects seems slightly subnormal.

[1] This is standard practice which I need not elaborate here. The "linear regression coefficient *b*" is the measure of the practice or fatigue effect observed, according to sign; the "variance absorbed" is the amount by which the original variance of the reaction times of the sheet is reduced when the effect is corrected: the "residual variance" is what is left when the absorbed variance is subtracted from the total original variance: and the value of *t* is given by $\sqrt{(Absorbed\ Variance)/s^2}$ where s^2 is the residual variance divided by the number of residual degrees of freedom—here 23. Cf. Fisher, *Statistical Methods for Research Workers*, section 26.

There is, in short, no evidence at all for supposing that either practice or fatigue are operative within the groups. And since the result of applying corrections would be to decrease just that variability of the times in which we are interested, it is clear that to use them in these circumstances would be not only supererogatory but pernicious.

I have accordingly used uncorrected data when working by Groups, although, as will be explained in due course, I have applied suitable corrections for inter-group differences when working by occasions.

d. Logarithmic transformation.—It will be understood that since the reaction time cannot be less than zero, and in practice not less than 5 or 4 units, whereas it may be indefinitely prolonged, the frequency distribution of the times will not be symmetrical; that is to say, the times will not be normally distributed. Actually, the deviation from normality is not serious, but in order to reduce possible ill effects from this cause to a minimum, I have adopted a suggestion for which I am indebted to Mr Yates of the Rothamsted Experimental Station and have replaced each time by its common logarithm. This virtually removes all skewness from the distribution and is a precaution which should be adopted in all work of this kind. In order to avoid inconveniently heavy figures I have in practice used the value 100 (log RT − 1), as will be seen from Example I below.

Further Illustration of Differential Method.—The necessity for using the differences between the two sets of measurements for two personalities was emphasised in my Q.S.T.P. III, and was further elaborated by Dr Thouless (*loc. cit.*, pp. 265 sqq.); but it is so important that I may perhaps be forgiven if I use yet another analogical illustration to bring out the point more clearly.

Let us suppose that our only data for characterising a burglar are to be derived from a full-length photograph taken at an unknown distance through a distorting lens. (N.B. The distorting lens corresponds to the medium and the unknown distance to the fact that the absolute mean RT may be affected by the external conditions accompanying the sitting.) We cannot, since we do not know the distance, or degree of distortion, determine his actual height, length of forearm, breadth of head, etc.; but we can find the *ratios* between these quantities as 'rendered' by the particular lens concerned, and these ratios would enable us to identify the same man from another similar photograph taken through the same lens, even though at a different distance, for the only effect of this last factor would be to increase or diminish all lengths in the photograph

in the same proportion. But we could never identify the man from a photograph taken with a lens which distorted in a different manner from the first, for this might elongate instead of broadening, or magnify the head at the expense of the feet, etc.

If, however, we were dealing not with a single burglar but with two in partnership, identification of the *pair* (though not of either separately) would be possible regardless of the different distortion of the second lens. In this case we should have to work with the ratios of the ratios, instead of the ratios themselves, corresponding (for the purpose of the analogy) to the differences between times. To take a single illustrative point:—Suppose that photograph of burglar X taken with lens A showed a total height of 72 mm. and a head-breadth of 6 mm., measured on the actual print, while the corresponding photograph of burglar Y (taken at something like twice the distance) showed a height of 33 mm. and a head-breadth of 3 mm.; then the ratio Height/Head-breadth will be 72/6 = 12 for X and 33/3 = 11 for Y, while the ratio of ratios X/Y will be 12/11. Now suppose that lens B is such that it expands horizontal distances by 50% in comparison with vertical, and the distances (which affect all measurements impartially) are varied so as to reduce X's measured height to 60 mm. and to increase Y's to 44 mm., we should find head-breadths of 7·5 and 6 mm. respectively: this leads to ratios Height to Head-breadth of 8 and 7·33 for X and Y respectively, neither of which is the same as that obtained with lens A; but the ratio of these ratios, namely 8 to 7·33 is the same as before, i.e., 12 to 11. A sufficient number of such X/Y ratios of ratios would enable us, in principle, to identify the pair of burglars XY with just as much assurance as a set of undistorted actual measurements would enable us to identify either of them individually.

The analogy is naturally not perfect—no analogy is, unless it is so literal a transposition of the original problem as to defeat its own elucidatory ends—but it should serve to show both the necessity for the differential method and the manner in which it works.

Moreover, once the necessity for the use of *differences* in RT is grasped it becomes easy to understand the inherent handicap under which the method labours, namely that a much higher degree of accuracy in measurement is necessary than is required for straight forward non-differential comparisons of individuals. An error of one unit is only 5% in a measurement of 20 units, but it becomes 25% if we are dealing with the difference between 20 units and 24. Thus the part played by "error" is, so to say, disproportionately magnified, and may bulk so large as completely to obscure the effect

which it is sought to observe, even though the latter may actually be present.

No one, of course, is to blame for this; and it is just part of the innate difficulty of the problem which no method whatever can eliminate. On the other hand, once the point is understood, we can see how it is that a procedure, which can successfully characterise isolated individuals and discriminate between them, may be altogether inadequate when the greater resolving power required to identify a pair of personalities is called for.

Calculation of the Main Reaction Time Results.—1. *Simplification of the Method.*—Soon after I had begun the analysis of these revised and additional figures on the same lines as before, I discovered that the original procedure as given me by Mr Stevens and discussed in Q.S.T.P. III, Part II, A, could be greatly simplified arithmetically by using the quantity (J – E), where J and E stand for any two corresponding entries for John and Etta respectively, as the working variate, and analysing this in precisely the same manner as was explained in connection with the study of Similarity in Q.S.T.P. I.

It has already been emphasised more than once that our problem can only be dealt with by comparing the *difference* between two communicators as presented by medium A with their *difference* as presented by Medium B. This is what Mr Stevens' original method actually does, but it can be shown that identically the same numerical results are obtained by working

$$z = \tfrac{1}{2} \log_e \frac{S(x-\bar{x})^2}{S(y-\bar{y})^2},$$

where $x = a + b$, $y = a - b$, $a = \mathrm{J}_a - \mathrm{E}_a$, $b = \mathrm{J}_b - \mathrm{E}_b$ and J_a, E_a, J_b, E_b, stand for the reaction times for any word as obtained from J and E respectively through mediums A and B respectively.[1]

This corresponds precisely to the working, in the study of Similarity, of

$$z = \tfrac{1}{2} \log_e \frac{\mathrm{W}}{\mathrm{WP}},$$

where W stood for the sum of the squares of their differences from their mean of the *sums*, and WP for the sum of the squares of their differences from their mean of the *differences*, of the reaction times

[1] The use of the logarithmic transformation mentioned earlier merely means, of course, that we use the difference of the logarithms as the working variate and proceed as above.

for the various words of the two personalities concerned. All that we are doing here is to use the difference (J – E) of two times, instead of a single time, in each case, in making the comparison.[1]

This modification enormously reduces the arithmetical labour involved and has been used throughout the work here described.

Example I (p. 250) shows the full working of the data from Sharplin and Garrett for the first twenty-five words (Group A) and the third Occasion (Sitting) in each case. The left-hand column gives the number of the word in the list: under the heading SHARPLIN, columns J and E give the common logarithms (to two places of decimals, with the decimal point omitted and 100 subtracted) of the reaction times for John and Etta obtained through Sharplin, respectively: the column (J – E) shows the difference between them, which is the working variate. The corresponding data for Garrett are given under the appropriate GARRETT headings. The columns headed SUMS and DIFF^{s} show, as may be verified, the sums and differences of the quantities (J – E). Beneath the various columns the line S(x) shows the total of each. In computing the variances of these sums and differences, we first calculate the sum of the squares, S(x^2) in the two columns, namely 18,364 and 22,620 respectively; from these in each case we subtract the product, $\bar{x}$S(x), of the mean and the total, the remainder, S$(x-\bar{x})^2$, being the sum of the squares of the differences from their means of the quantities concerned. Since there are equal numbers of entries in the two columns, there is no need to divide each of these sums of squares by the number of degrees of freedom in order to obtain the mean variance; we accordingly divide the greater by the smaller, obtaining the ratio (R) between them, which here comes to 1·5381. The logarithm of this quantity to base e is shown in the appropriate tables to be ·4306, and this is what we call Z.

It is easy to see that if the successive quantities (J – E) for Sharplin and Garrett had been of closely corresponding magnitudes, we should have had a tendency towards large quantities with a big variance under SUMS and small quantities with a small variance under DIFF^{s}; this would have corresponded to a positive correlation between the values of (J – E) for the two mediums. As it is, the reverse is the case, indicating a negative correlation, so that the variance of the Differences of (J – E) is greater than that of their

[1] Some readers may find the procedure easier to understand if it is pointed out that the work is formally equivalent to determining the intra-class correlation between the two series of differences (J – E).

Sums. We must accordingly prefix a minus sign to the Z and write it $-\cdot4306$, as shown in the Example.

The actual procedure is quite simple, though the necessary reiteration of the word "difference" makes it sound difficult and confusing. Perhaps it will be easier if we introduce some other term such as "disparity" when talking of the quantity (J – E) and try to bear in mind that our problem is simply one of determining whether the disparities between J and E as shown by one medium agree (positive correlation and positive Z) or disagree (negative correlation and negative Z) with those shown by another medium for the same list of words.

The question of whether the degree of correlation shown can reasonably be ascribed to chance is tested in the usual way by dividing Z by its standard error and looking up the value of the probability required in a table of normal deviates. Since there are 25 words in a group, with 24 degrees of freedom between them, the variance of z is $\frac{1}{24}$, and of Z four times this, or $\frac{1}{6}$, with standard error ·4082. In the particular example worked, the value of the normal deviate obtained by dividing Z by this quantity is only 1·055. Reference to the appropriate table shows that a value of this magnitude or greater might occur as often as about twenty-nine times in a hundred by the operation of chance alone. It is accordingly far from being significant.

The Z's obtained in this way form the raw material, so to say, for the second stage of our investigation, and if John and Etta were autonomous entities transmitting characteristic reaction times undistortedly through the mediums, we should expect the values calculated to be preponderantly positive. If, on the other hand, there is no more than a chance relation connecting the "disparities" (J – E) between John and Etta in one case with those in another, we should expect the Z's to be randomly distributed about a mean of zero and to show no particular features of interest.

2. *Primary results of the Inter-medium comparisons.*—It will be understood that since data for J and E are available from three mediums, there are three kinds of comparison which can be made, namely Leonard and Garrett, Leonard and Sharplin, and Sharplin and Garrett. In the case of the Feda material, only two mediums are concerned, but we can use either the difference between Feda and John or that between Feda and Etta as the working variate.

These main results for John and Etta are given in Table I A, and for Feda in Table I B.

It is necessary to give these results in detail for the sake of com-

pleteness and to assure ourselves that the effect (if any, and of whatever kind) does not vary significantly as between groups, but I do not expect the reader to bestow more than a passing glance on them; it will be more convenient to extract the necessary features for detailed consideration as we go along. It may be noted, however, that the grand total (0·2412) for the Leonard-Garrett comparisons is faintly positive; that for Leonard-Sharplin (−0·7790) moderately negative; and that for Sharplin-Garrett (−3·1239) very heavily negative: also that the occasion totals (in the extreme right-hand column) for these last data show a clear tendency to become more negative on the later occasions.

I shall discuss this curious negativity in considerable detail below; at the moment we need only concern ourselves with the question of whether the results obtained can reasonably be attributed to chance causes or not.

For this purpose I shall concentrate on the Sharplin-Garrett Z's of Table I A, ignoring the Leonard-Sharplin and Leonard-Garrett values as evidently non-significant by inspection.

It is naturally impracticable to give here a full account and explanation of the methods used for testing the significance of the figures, for which the reader must refer to the appropriate text-books; the best I can do is to reproduce the actual working, so that it can be verified by anyone interested, and to add a few words of elucidation for the benefit of the semi-statistical reader.

Ignoring the differences between groups as insignificant by inspection, and concentrating on the occasion totals, we have

Occasion	Z	ξ_1	$Z\xi_1$
I - -	·4216	−1·5	− ·63240
II - -	− ·4386	− ·5	·21930
III - -	−1·1771	·5	− ·58855
IV - -	−1·9298	1·5	−2·89470
Total -	−3·1239		−3·89635

Here the Z's are copied from the Table, the use of ξ_1 and the entries under that heading follow Fisher's technique for the determination of regression coefficients, and the values in the last column are the products of those in the first two.

Dividing the two totals by the number of entries and the sum of the squares of ξ_1 respectively, we obtain the mean and the linear regression coefficient of Z on occasion; these are found to be −·781 and −·779 respectively. Multiplying each of these by its corresponding total, we obtain the amount of variance 'absorbed'

as the term is, by the mean and the regression; these quantities divided by the theoretieal varianee, whieh for a total of four Z's will be $\frac{2}{3}$, give us two χ^2's with one degree of freedom eaeh. The values of these are about 3·660 and 4·554 respectively, and referenee to the appropriate Tables shows that the first is very nearly signifieant ($P<\cdot06$) and the seeond quite definitely so ($P\sim\cdot03$).

To obtain the probability that both the mean and the regression should differ from their expeeted values of zero to the observed extent, or more, as the result of ehanee alone, we sum the two χ^2's and obtain another χ^2 with 2 degrees of freedom; this eomes to 8·214 for whieh the value of P is only about 1 in 61 or ·0165.

In other words, the results of the Sharplin-Garrett eomparisons deviate from expeetation to an extent whieh would oeeur on the average only onee in rather more than sixty sueh eolleetions of 1,600 original observations, if no faetor other than ehanee were operative upon the data.

This is a very reasonably handsome degree of signifieanee, and it would still remain just above the eonventional level of ·05 even if we felt eompelled, whieh is very doubtfully neeessary as we shall see below, to treat the Sharplin-Garrett eomparisons as the best of three anteeedently equal sets. Actually, the probability of the results being ehance-determined is very mueh less than this, as we shall shortly see.

In ealeulating the foregoing signifieanees we have retained the division of the data into groups—a division whieh is quite unneeessary onee we have satisfied ourselves that there are no important differenees between them from the point of view of the distribution of Z. On the eontrary, it leads to an unnecessarily insensitive test of significanee, beeause, speaking teehnieally, we lose the three degrees of freedom between the four groups into which each oeeasion is divided. We shall aeeordingly obtain a better estimate of signifieanee by pooling the groups for each oeeasion and re-working the Z's for each oeeasion as a whole.

Before doing this, however, I readjusted the data in respeet of over-long times, misheard words, etc., using the oeeasion medians, as indicated on p. 225, above. I also eomputed suitable eorreetions to be applied equally to eaeh member of any group, so as to deal with the faet that although the intra-group regressions on order are negligible the inter-group differenees for a partieular oeeasion are not always so.

Taking the Sharplin-Garrett figures only and testing signifieanee preeisely as before, we have

Occasion	Z	ξ_1	$Z\xi_1$
I - -	·0961	−1·5	− ·14415
II - -	− ·1403	− ·5	·07015
III - -	− ·3348	·5	− ·16740
IV - -	− ·4642	1·5	− ·69630
Total -	− ·8432		− ·93770
Mean -	− ·21080	*b*	− ·18754
Absorbtions	·17775	and	·17586
Theoretical Variance		4/99	
χ^2 - -	4·3993	and	4·3525
P - -	< ·04	and	< ·04
Total absorbtion - -		·35361	
χ^2 - - - -		8·7518	
1/P - - - -		79·52	
P - - - - -		< ·02	

The procedure may be followed by reference to the first evaluation. Note that the theoretical variance is now 4/99, because the occasion as a whole has 100 words with 99 degrees of freedom between them. The value of P now comes to no more than about 1 in 80, which means that even if we take the result as the best of three the odds are better than 25 to 1 against the outcome being fortuitous.

Interpretation of Negative Z's.—We shall see below that even the distinctly high significance just found considerably underestimates the improbability of the Sharplin-Garrett results being due to chance; but I propose to leave this question on one side for the time being and to attempt some discussion of the interpretation to be given to the occurrence of negative Z's.

Statistically speaking, of course, and on the hypothesis that chance alone is operative, negative Z's are just as likely or unlikely to occur as positive, and if the results could fairly be ascribed to chance there would be nothing to occasion surprise. But this is not the case and it is accordingly our duty to try to ascertain what interpretation, if any, can reasonably be given to this very curious outcome.

At one time it was contended that "no intelligible meaning" could be attached to a negative Z. This is definitely incorrect, for there are certainly two, though I think not more than two, perfectly intelligible if not equally plausible explanations that can be given. I shall refer to these as the hypotheses of *Interchange* and *Interference* respectively, and our next step must be to state them as clearly as we can and to explain what kind of mechanism must be postulated in connection with each.

I should point out here that since both these hypotheses lead to the same main result, namely negative Z's, they cannot be distinguished by the methods so far described. They appear to me, however, to be conceptually distinct, even if it be argued that the only effect of Interference is to produce Interchange; and we shall see below that there is some experimental reason, apart from general considerations, for preferring the one to the other. I have therefore thought it proper to discuss them separately.

a. Interchange.—The hypothesis of interchange has at any rate the merit of simplicity. It postulates that, for the occasion or group concerned, the parts of J and E are interchanged by one of the mediums (but not by both) either throughout or preponderantly, so that when we think we are getting J's times we are really getting E's, and *vice versa*.

The way in which this would operate is easily understood by reference to Example I. Suppose that in this group there has been interchange in the case of Sharplin, but not in that of Garrett. Then to rectify matters we must write all the Sharplin E figures in the J column and the J figures in the E column. This will have the effect of reversing the signs of the Sharplin (J – E) column. Then, since the Garrett figures remain unaffected, the result will be to interchange the figures under SUMS and DIFF^s. This can readily be seen by considering the first line which will now read (the headings being just as before)

1.	18	0	18	0	18	−18	0	−36

The process being continued all down the columns the total effect will be to interchange the figures for SUMS and DIFF^s throughout, so that we should end with a sum of squares of 22,485·44 for the former and of 14,618·56 for the latter, leading to a *positive* Z of the same numerical value as before.

It should be noted in passing that a purely random substitution of J for E and E for J times would not produce the required effect, but would merely tend towards the production of Z's approximating to zero.

If we had any external reason for supposing that interchange of this kind actually took place in the case of Mrs Sharplin, we should have some justification for changing the signs of the Z's in all comparisons in which she is concerned and claiming that the experiment as a whole yielded a triumphantly positive result.[1] Unfortunately, so far as I can see, we have no warrant for this at all; indeed such

[1] I say "Sharplin" here, because inspection of Table I A will show at once that it is she who appears most closely associated with negative Z's.

trivial evidence as is available seems to contra-indicate the supposition. To test the point, I asked Mr Drayton Thomas to go through the Sharplin material and see whether there was any appreciable tendency for characteristic J responses to appear in the E lists, or *vice versa*; because, if interchange were taking place, one would expect this to happen to an appreciable extent. He could only find four very doubtful instances, occurring in two negative and one positive groups, whereas he had previously noted four reasonably characteristic J words in their proper place. Since there are 11 negative groups out of 16 in the Sharplin-Garrett material, the first of these facts is evidently negligible, while the second tends against interchange having occurred.

There seems, therefore, no collateral evidence to support the hypothesis of Interchange, while it is very difficult to imagine such a thing taking place systematically and on the extended and increasing scale required to produce the observed effects.

b. Interference.—This hypothesis is perhaps less artificial than that of Interchange, but it is not quite so simple and is correspondingly more difficult to explain.

For those who are familiar with the notions and terminology of the Analysis of Variance the point may be most easily dealt with by recalling that in the original method we were comparing the variance due to the first order interaction of communicator and word (CW) with that due to the second order interaction of medium-communicator-word (MCW)[1]: alternatively, in the simplified method, we are comparing the variance of the differences (J – E) due to the different words with that due to the interaction of word and medium; the fact that the last-named (MW) comes out unduly large indicates that a considerable degree of medium-communicator-word interaction is occurring.

In more ordinary language, the difference between J and E depends not merely on the word but on the medium, and not merely on the medium but on the word; that is to say on medium and word taken in conjunction. Alternatively we may say that the difference in reaction time between the two mediums (and we should never forget that it is from these that the data are actually obtained) depends not only on the word used as a stimulus but also on whether they are in the J or the E state.

Approaching the problem from a slightly different angle, we may reflect that, since we are using the difference between the two states J and E as the working variate, no relationship between the two mediums in trance as such, whether of positive correlation or of

[1] Cf. Q.S.T.P. III, Part II A.

negative, can affect the result, because in so far as there may be anything characteristic of either medium common to both states of that medium it will disappear in the subtraction. It follows that for a positive correlation to be obtained it is necessary for one state or the other or both to affect both mediums in the same way; and for a negative result it is necessary that one or the other state or both should produce opposite effects on the two mediums.

Possibly the point will be easier to understand if we think of it in terms of J and E as two external sources of some kind yielding different but characteristic sets of reactions to the various words of the list. Then the differences (J – E) between the two series obtained through the two mediums must be positively correlated, regardless of the influence of the mediums, provided this influence is impartially applied; provided, that is to say, that in respect of any given word each medium produces the same effect on J as on E, or alternatively that the effect of both mediums on J (or E) is the same. But it is fairly easy to see that if the two mediums produce opposite effects on one of the communicators, or if one medium produces opposite effects on the two communicators, the resemblance of the two sets of differences (J – E) must be diminished and the difference between them increased; and if this is taken far enough a negative correlation will result.

The process is, however, difficult to translate from the mathematical to the verbal form, so it will probably be better to think of the negative results merely as being generated by the not detailedly specified "interaction" of mediums, communicators and words operating in a fashion somewhat similar to that in which different subjects might be expected to give somewhat different reactions in the presence of different witnesses. It will be of more interest to enquire whether there is any positive evidence for the operation of interference of this kind.

Regression of Z's on Mediums' Times.—In considering the possibility of obtaining such evidence I reflected that, if either medium 'interferes' with the reactions, she is most likely to do so in respect of those words of greatest emotional significance to her in her normal state; she is likely to be neutral in respect of words which strike no complex, arouse no interest and create no disturbance.

If this is true, then, since prolongation of reaction time is a sign of emotional significance, it is likely that the signs of interference, namely negativity of Z's, will be most marked among words for which the medium's own reaction time is longest, and least among those for which it is shortest.

To test this we will select whichever medium we like—Garrett,

say—to start with, arrange the words in the order of the reaction times obtained from her when tested on the occasion concerned in her normal state, break this series of 100 up into blocks of 10, compute Z for each such block, and see whether there is any significant tendency for the value of Z to vary from block to block.[1] We must also, of course, do the same thing again using Sharplin as 'indicator' so to speak, and must repeat for all occasions. As an example I give below the values of Z obtained from Garrett and Sharplin, and the working of the regression coefficients, for the first occasion :

TABLE I

		Sharplin		Garrett	
Block		Z	Z	Z	Z
1	- −4·5	·8932	−4·01940	− ·6250	2·81250
2	- −3·5	·4328	−1·51480	− ·2080	·72800
3	- −2·5	·2209	− ·55225	−1·4426	3·60650
4	- −1·5	·7607	−1·14105	1·0097	−1·51455
5	- − ·5	·2348	− ·11740	·3035	− ·15175
6	- ·5	− ·4714	− ·23570	− ·0370	− ·01850
7	- 1·5	·3403	·51045	− ·1090	− ·16350
8	- 2·5	·0647	·16178	·7324	1·83100
9	- 3·5	− ·6603	−2·31105	·6533	2·28655
10	- 4·5	− ·4299	−1·93455	− ·1467	− ·66015
Total		1·3858	−11·15400	·1306	8·75610

Dividing by 82·5 gives b − ·1352 and ·1061;
$1/\sigma_b$ is easily shown to be 13·6245, whence we have
b/σ_b 1·842 and 1·446,
of which the first is not far from significance (P ∼·065).

There is no need to give these details in full throughout, but Table II shows the values of b obtained for Sharplin and Garrett by applying this treatment to each of the four occasions.

TABLE II

Occasion	Sharplin		Garrett	
	b	b/σ_b	b	b/σ_b
I - -	− ·1352	1·842	·1061	1·446
II - -	·0376	·513	·1193	1·668
III - -	·0349	·475	− ·1698	2·313
IV - -	− ·0166	·226	− ·0239	·326
$S(b^2/\sigma_b^2)$ -	3·933		10·329	
P - - -	∼ ·4		·0353 or 1/28·47	

[1] In other words, we test the regression of Z on block order. I am indebted to Mr Stevens for this method, which is a great improvement on the far cruder plan I first thought of using.

It is to be noted here that, since the expected value of b is zero, $b^2/\sigma_b{}^2$ will be a χ^2 with one degree of freedom in each case, so that the sum of these quantities will be, for each medium, a χ^2 with four degrees of freedom. This is negligible in the case of Sharplin, but gives a significant value of 10·329 in the case of Garrett. In other words, the tendency for Z's to be negative is *not* independent of Garrett's own reaction times.

The result is slightly surprising inasmuch as it is Sharplin, as we have seen, rather than Garrett, who seems most closely associated with the production of negative Z's. But the point is not of major importance, for the test we have applied is, so to say, an extremely naive one. It seems likely on general grounds that the process of interference which we are considering is some more complex function of the medium's own time than its actual magnitude, and still more likely that it is some not altogether simple function of the two times (that is to say, of Sharplin's own time and Garrett's own time) taken in conjunction. In the circumstances, the surprising thing is that the test should have given a worth-while result at all rather than that its outcome should not conform precisely to anticipation.

It is perhaps worth noting that there is here, for those who care to think in such terms, a strong suggestion that successful mediumship may consist not so much in receiving the appropriate impressions as in the ability to abstain from interfering with them. In this case, we can almost see the process of distortion by the medium's own mental content going on; and it may well be that the chief difference between good mediums and less good is to be found in the degree to which this distortion is avoided.

Combination of the Probabilities.—The treatment of the main RT and that for the regression of Z on Medium's own time are completely rigid and yield anti-chance probabilities which may be combined in the usual way.[1]

The two contributions are

	P	1/P
From main RT results, combining Mean and regression on occasion	·0125	79·52
From regression of Z's on Mediums' own times, taking Garrett's result as the better of two	·07	14·49

Then we have

$\log_e$ 79·52		4·37601
,, 14·49		2·67347
Sum		7·04948
Multiplying by 2 gives		14·09896

[1] *l.c.* by negative logarithms: cf. Fisher, *loc. cit.*, section 21, 1.

which is a χ^2 with four degrees of freedom. Interpolating in the appropriate tables [1] we find P to be about ·007 ; that is to say, the odds are more than 140 to 1 against the results of the Sharplin-Garrett comparisons being the outcome of chance alone.

In the interests of the strictest conservatism we will consider this as the best of three comparisons (*viz.* Leonard-Garrett, Leonard-Sharplin and Sharplin-Garrett) of equal antecedent status, although, as I have already indicated and shall later explain, this is probably by no means obligatory. Using exact figures and proceeding in the usual way [2] we obtain $P' = \cdot 02$ or 1 in 50, very nearly, for the probability that the data as a whole represent a randomly related collection. This is definitely significant, and the null hypothesis that there is only a chance relation between the times given in the J and E states by the three mediums may be regarded as virtually disproved.

Interim Discussion of Foregoing Results.—Without in any way wishing to overcall my hand, I think we may regard the outcome of the study up to this point as remarkably satisfactory. It is true that anti-chance odds of 49 to 1 are not overwhelming ; but the desire for astronomical figures is no more than a symptom showing that the function of tests of significance has been imperfectly understood. This function is not to '*prove*' this hypothesis or that, but to help us to distinguish chance generated effects from real ; that is to say, to tell us how likely it is that the effects observed are due to uncontrollable, or at least uncontrolled, chance variations of circumstance rather than to systematic causes relevant to our enquiry. We can never say with complete finality that any given result, no matter what the anti-chance odds may be, is certainly *not* due to chance ; but we can tell whether it is so likely to be so that further enquiry on the lines concerned will probably be waste of time. The conventional and purely arbitrary criterion usually adopted is that of one chance in twenty ; that is to say, it is commonly (if a trifle pusillanimously) agreed that a result is to be discarded if there is more than one chance in twenty of our wasting our time in following it up. The odds of 49 to 1 here obtained are handsomely beyond this point, and, if we had obtained very much longer odds, the only additional conclusion would have been that we had collected many more data than were necessary for our purpose.

But actually we have done a good deal more than merely show that

[1] Fisher and Yates, *Statistical Tables*, p. 27.

[2] *I.e.* by evaluating $1 - q^3$, where q is $1 - p$ and p has the value ·006944 just found.

errors of random sampling constitute an improbable explanation of the relationships observed between the figures; we have made quite appreciable progress towards discovering what sort of processes seem to be involved.

In dealing with material of this kind, three stages are clearly distinguishable. First we must ask whether the observational data are consistent with the null hypothesis of a chance determined origin; if they are not, we must try to frame some reasonable explanation for the features they are found to present; finally, we must conduct some further test, either on the same data or on others collected specially for the purpose, to ascertain whether this explanation is correct. In this case we have successfully carried out all three stages of the work. Taking the Sharplin-Garrett figures, we find odds of more than 100 to 1 against their being ascribable to chance alone; we then envisaged two possible explanations of the most striking feature (negative correlation between the J – E differences) which they presented, namely Interchange and Interference, the latter being by no means implausible antecedently, but rather the reverse; we went on to surmise that, if this second explanation were correct, it would be unlikely for the degree of correlation to be independent of the mediums' own times; finally, on testing this, we found significant anti-chance odds (in the case of Garrett) against such independence.

To all intents and purposes this is tantamount to saying that we have shown (*a*) that the relationship between the J and E data obtained through the different mediums is not a chance relationship, (*b*) that the curious form of negative correlation which this relationship takes in the case of Sharplin-Garrett is due to interaction between the mediums and communicators (whatever these may be), and (*c*) that this interaction is of the nature of interference due to excessive influence of the mediums' own mental make-up on the natural J and E reactions. Further than this we cannot at present go; but to have gone so far is as much as we could look for from an experiment of this kind, short of an out and out positive result, such as no prudent man would expect to obtain in Psychical Research.

Collateral Evidence.—I have said above that the process of treating the anti-chance probabilities evaluated as if they were the better of two attempts in the case of the association of mediums' times with degree of negative correlation, or as the best of three in the case of the Sharplin-Garrett comparison as a whole, is probably unnecessary. The reason is that this treatment is, properly speaking, only called for when the antecedent probabilities of obtaining

the effects observed are equal in the various cases considered ; and this does not seem to be true here.

If, for example, we were concerned with tests of gustatory acuity and wished to ascertain whether two solutions were identical or not, relying on tests involving the sense of taste alone and using three subjects picked at random for the purpose, we should be quite right in treating any intrinsically significant result shown by one of them as the best of three attempts, and in discounting the results accordingly ; but if it were found that one of our subjects was a professional tea-taster while another had had some experience of the kind, this would cease to be correct ; we should then attribute differences in successful discrimination not to the effects of random sampling (*i.e.* to chance alone) but to the known differences of ability between our subjects, and should take the most significant result (presumably from the professional) at its face value. Very much the same kind of thing applies here : we know that Mrs Leonard has had many years of experience and practice at whatever the process is that we describe as " communication by J and E " ; Mrs Garrett, though without previous experience of J and E themselves, has made something of a specialty of " direct control " in general ; while Mrs Sharplin, I understand, has devoted comparatively little attention to this branch of mediumship. Consequently, where direct control by J and E is concerned, the antecedent status of the three mediums is very far from equal ; on the contrary, we should expect them to rank quite definitely in the order of merit Leonard, Garrett, Sharplin ; and we should expect any phenomenon of interference to be much more marked in the case of the two last-named ladies than in that of the first. And this order, as a glance at Table I A will show, is precisely that in which the study of the reaction times ranks them.

Now it is true that the probability of their ranking in this order as the result of chance alone is as great as 1 in 6 ; and it is also true that, since this probability cannot, *a priori*, assume any value between 0 and 1, it is not practicable to combine it with the previously found overall value of 1 in 50. But it is perfectly legitimate to maintain either that this ranking, which is confirmed by other evidence given below, destroys the assumption on which the treatment of the Sharplin-Garrett comparison as the best of three depends, or to say that it constitutes additional evidence of the non-chance character of the results, even though we are not in a position to assume the exact force of its contribution. These are really no more than two different ways of expressing the same argument ; but, whichever way we look at it, it is clear that the

aforementioned anti-chance odds of 49 to 1 constitute a considerable understatement of the facts of the case.

This matter of the ranking of the three mediums is well supported by a numerical study by Mr Drayton Thomas (unpublished at the time of writing) of the verbal responses obtained. For the present purpose the following abbreviated account will be sufficient.

Mr Thomas recognises, in effect, three criteria by which the relative merits of the mediums are assessed.

(1) The first is the likeness of J to E as judged by the number of identical responses to the same stimulus word given by them but by them alone; this is based on the assumption that the two communicators, being father and daughter and closely associated both during life and after death, would have more associations in common, but not shared by others, than would two personalities not so related. The system of marking was as follows: "When J and E (or any other pair of personalities) give an identical response in reply to some stimulus, and no other personality gives it, then I give one mark for identity to that pair; should they *both* repeat that reaction to the same stimulus a further mark is given. Sometimes they both repeat this reaction word a second or third time, which gains a second or third mark. It must be understood that no reaction is counted unless given by the one pair and *by them only*." Mr Thomas applies this procedure to each of the six pairs formed by the four personalities (medium, control, J, E) in each "manifold". Since there were only four Garrett sittings instead of five as with Leonard and Sharplin, it will be best to give the results in terms of percentages of the total scores gained by the JE pair as compared with the mean percentage for the other pairs. The figures are:

TABLE III

	Identical Responses	Repetitions	Total
Leonard			
% Total score for J and E	43·2	43·5	43·3
Mean % for other pairs	11·4	11·3	11·3
Garrett			
% Total score for J and E	32·4	32·9	32·6
Mean % for other pairs	13·5	13·4	13·4
Sharplin			
% Total score for J and E	20·0	14·9	18·0
Mean % for other pairs	16·0	17·0	16·4

It is evident that this test ranks the mediums Leonard, Garrett, Sharplin in the order given.

(2) The second criterion is that of "characteristic" responses, that is to say, replies judged by Mr Thomas to be determined by interests and sentiments known to have been of importance to the communicators, such as ministerial activities for J and painting for E. The figures here are :

	J	E	Total
Leonard - - -	38	54	92
Garrett - - -	37	45	82
Sharplin - - -	4	0	4
Total - -	79	99	178

and the ranking is clearly as before.

(3) Finally there is the occurrence of unique reactions, *i.e.* responses given by J or E but by no other personality at all. This criterion is based on the supposition that the bare fact of a response being "unique" in this sense indicates that the communicator is getting through something of his own as opposed to something of the medium's. The figures are :

	J	E	Total
Leonard - - -	12	9	21
Garrett - - -	6	4	10
Sharplin - - -	2	5	7
Total - -	20	18	38

Here again, though with rather less certainty, the mediums are ranked in the same order.

Of these results, those given by (2) are, I think, the most important. None, however, is as unambiguous as that of the RT analysis, for it would probably be possible to attribute them all to differences of histrionic ability without much strain. Still, I think it fair to claim that they appreciably strengthen the view that something characteristic of the communicators is being transmitted with varying degrees of success—in other words, that the order of merit noted by Mr Thomas and given objectively by the RT analysis is a real one, with the implication that some process akin to "communication" is going on. At the very least, there can be no doubt that they are entirely consonant with this hypothesis.

Another fact, which may fairly be reckoned as collateral evidence, though not of very great force, is that, in the case of all three

mediums, J's mean reaction time is significantly longer than E's. I compute these from the group totals and find

	Leonard		Garrett		Sharplin	
	John	Etta	John	Etta	John	Etta
Mean group total -	400·2	325·0	284·0	262·6	317·8	287·6
Diff^{ce} of means -	75·2		21·4		30·2	
s - - - -	57·35		23·93		40·14	
t - - - -	3·709		2·529		2·128	
P - - -	$<$ ·001		$<$ ·02		$\sim$ ·04	

It will be noted that here again the three mediums appear in the same order in the sharpness with which they discriminate the two personalities.

The actual effect, though intrinsically significant in each case, represents, of course, no more than a single character of the personalities, as it might be a single item in a set of Bertillon measurements; it is not therefore of great contributory weight, though it seems worth noting as a matter of general interest. On the other hand, the chance of all three showing a difference of the same kind is ·25, and that of their appearing in the same order as before is ·17; so that these facts constitute an appreciable, if again inassessable, makeweight to the overall anti-chance odds.

General Discussion and Interpretation.—The interpretation which will, in fact, be given to the results described above will depend so largely on the predispositions of the interpreter that discussion of them here is almost foredoomed to be a waste of time. We can, however, at least clarify the issue to the extent of deciding what we can be sure about and what not, and of examining with some care, if not altogether exhaustively, the alternatives that present themselves.

The *fact* to be discussed is that of finding a significant negative correlation between the values of (J – E) for the reactions obtained on different words for the two mediums Sharplin and Garrett. The alternative *explanations* are (*a*) use of a faulty method, (*b*) computing errors, (*c*) chance, (*d*) some direct causal connection between Sharplin and Garrett, (*e*) some external cause or source of the reactions operative upon both.

Dealing with these in order, we may at once dismiss the first, for no well-advised critic will attempt to dispute the methods of determining the correlations and their regression on occasion, the combination of these, or the estimation of significance, which have been employed; nor does it need more than a glance from an instructed eye to see that I have scrupulously avoided those vagaries

which beset my errant if well-intentioned feet in the preparation of Q.S.T.P. III. The methods in question are completely orthodox and may be regarded as being almost as well established as the multiplication table or the use of logarithms. Moreover, as indicated in my preamble, I have had the advantage of consultation with the highest authorities throughout the work.

As regards errors of computation, the work is not of a kind in which the outcome is liable to be seriously falsified by the occurrence of a few undetected slips. Actually, of course, all the calculations have been carefully checked; but perhaps the best assurance on this point is to be found in the fact that when, at an earlier stage, I worked the whole of the main calculations using corrected times not converted to logarithms, I obtained results substantially identical in sense and magnitude with those here given. We may therefore take it that no serious error has been introduced in this way.

In any ordinary sense of the words, moreover, we may regard chance also as eliminated; the minimal odds against this hypothesis are 49 to 1, and actually they must be quite considerably, if indeterminably, greater than this. For all practical purposes we may count "chance", which in any event is but the last refuge of the destitute, as out of it.

To sum up thus far, in colloquial language for the sake of emphasis: We are confronted with a real effect, which is NOT due to chance, to errors of method or of calculation, and must therefore be explained either by the invocation of "normal" causes or by the admission of some cause which, in the present state of our ignorance, we must be content to regard as "paranormal".

The difficulty of producing any reasonably plausible explanation on normal lines is very great. It would be bad enough if all we had to account for were a negative correlation between the two mediums themselves, whether in trance or not; for we could not get over it, as we should do in the case of a positive correlation, merely by assuming that the list of stimuli used had inadvertently been allowed to contain a number of words having a more or less universal importance; we should be compelled to suppose in addition, and quite gratuitously, that it contained a considerable proportion of words of two types, X and Y say, present in roughly equal numbers, of which one type caused prolongation of time in Sharplin but not in Garrett, and the other in Garrett but not in Sharplin [1]; in other terms, that Sharplin and Garrett were strongly

[1] X and Y may be supposed to refer to any pair of antithetic qualities such (just for the sake of illustrating somehow) as "masculine" and "feminine", or "intellectual" and "athletic".

contrasted or antithetic types, one giving delayed times on about one-half of the words used (or some considerable proportion thereof) and the other on the other.

This idea, though it may sound simple enough on paper, is actually almost incredible. It is, of course, perfectly possible to argue that any word must have, in the last analysis, predominantly X or Y connotations, whatever X and Y may stand for; but it is only possible to do this from what I may term a "public" or general standpoint, and it does not in the least follow from any such argument that the words concerned have predominantly the *same* X or Y connotations for both Sharplin and Garrett; yet this further assumption of an extensive degree of word to word correspondence is necessary in order to explain any observed correlation.

But the effect actually observed is very much more difficult to explain than this, because we have carefully employed a differential method, taking the value (J – E) as the working variate, in order to eliminate any correlation between the two mediums, whether positive or negative, which might result from or remain in the trance state as such. Consequently, we cannot suggest, as otherwise we might be tempted to do (though we should not thereby evade the difficulty just mentioned) that the process of going into trance automatically brought out latent characteristics of the mediums not observable in their normal states. It must be something to do with the intrinsic J-ness or E-ness of one or other of the two states or both that is responsible for the effect, and I see no way of attaching any kind of intelligible meaning to such terms except by postulating *some* kind of external source or factor capable, so to say, of setting its own individual imprint on the reactions of the trance states concerned, while the fact that this factor interacts with the two mediums in such a way as to lead to a negative instead of the more natural positive correlation does not make it less necessary to conclude that it is in some sense external. In fact it makes it, if anything, more necessary, because, while we might by a sufficient stretching of the imagination suppose that a diligent getting up of the parts of J and E by two adequately accomplished actresses might conceivably result in a positive correlation of the differences between the two renderings, even in so recondite a respect as the reaction times given to a particular list of words, we really cannot plausibly suggest that a significant negative correlation could proceed from any such process, however far it might be carried.

I should feel happier if the whole experiment could be repeated with an entirely new list of words, but particularly the Sharplin-Garrett part of it; indeed, one might say that this last is definitely

necessary in order to consolidate our results, since we have no satisfactory replication so far. I should also like to see the same experiment tried out with two or three good stock-company actresses who had studied the characters of J and E so far as published and known material permits, and were trying to project themselves into those characters while under test.

Such remarks, however, referring respectively to a confirmation and a sidelight, should not be taken as minimising the importance of the results actually obtained, which I believe to be considerable.

This is not to say that I regard the autonomy of J and E to be "proved" or even "established" by this experiment. So far as this work is concerned, the spirits, if I may put it so, remain a good many degrees below proof. On the other hand, I think that only ignorance or bigotry could deny that the case for the operation of some non-chance non-medium factor, that is to say an external factor, and therefore for the "autonomy" in question, is very greatly strengthened by the results obtained, as compared with what would have been the case if we had obtained insignificant results conforming to the null hypothesis that there is no connection between the mediums other than chance alone.

The natural and obvious explanation of the phenomena as a whole, from the orthodox standpoint, would be that the entranced mediums are highly suggestible and need little inducement to adopt histrionic poses representing, as best they may, the personalities of the supposed "communicators". If this were so, we should expect, it is true, that the general behaviour of the mediums in the relevant states of control would be broadly coloured in conformity with known characters of the persons concerned, but we certainly should *not* expect this to be carried out in such minuteness as is involved in differences of reaction times to common words of no particular public interest; such details, we should certainly imagine, would be filled in in accordance with the personal idiosyncrasies of the mediums. In other words, we should confidently expect that sets of reaction times obtained from the "communicator" states would be as randomly related as those from different people. This is emphatically not the case, and to this extent the "natural and obvious" explanation of the phenomena is quite definitely contra-indicated.

EXAMPLE I

Working of Z (= 2z) for Differences (J – E)

SHARPLIN-GARRETT: Occasion III, Group A

Word No.	SHARPLIN J	SHARPLIN E	SHARPLIN (J – E)	GARRETT J	GARRETT E	GARRETT (J – E)	(J – E) SUMS	(J – E) DIFFS
1	0	18	–18	0	18	–18	–36	0
2	20	–10	30	15	15	0	30	30
3	0	4	–4	18	–5	23	19	–27
4	20	20	0	15	18	–3	–3	–3
5	4	8	–4	4	11	–7	–11	3
6	4	8	–4	11	–15	26	22	–30
7	11	–30	41	–10	15	–25	16	66
8	8	23	–15	–5	0	–5	–20	–10
9	11	4	7	0	–5	5	12	2
10	8	4	4	18	0	18	22	–14
11	18	–5	23	–15	8	–23	0	46
12	–5	–22	17	40	–15	55	72	–38
13	15	–15	30	–15	–5	–10	20	40
14	23	11	12	–10	–15	5	17	7
15	15	4	11	34	11	23	34	–12
16	8	32	–24	40	–5	45	21	–69
17	0	11	–11	0	23	–23	–34	12
18	18	23	–5	–10	8	–18	–23	13
19	15	11	4	11	18	–7	–3	11
20	4	11	–7	38	0	38	31	–45
21	15	–22	37	–5	–5	0	37	37
22	0	–15	15	15	0	15	30	0
23	11	–10	21	–5	11	–16	5	37
24	8	0	8	18	15	3	11	5
25	4	–10	14	18	–5	23	37	–9
S(x)	235	53	182	220	96	124	306	58

CALCULATION OF THE VARIANCES

	Sums	Differences
$S(x^2)$	18364	22620
$xS(x)$	3745·44	134·56
$S(x-x)^2$	14618·56	22485·44
R	1·5381	
Z	–·4306	

TABLE I A

VALUES OF Z(2z) FOR INTER-MEDIUM COMPARISONS OF (J – E)

LEONARD-GARRETT

Occn.	Group A	B	C	D	Total
I	– ·2771	·5199	·3962	– ·5018	·1372
II	– ·0272	– ·2192	·1630	– ·2458	– ·3292
III	– ·3793	·1985	– ·0734	·0236	– ·2306
IV	·0420	– ·4275	·6797	·3696	·6638
Total	– ·6416	·0717	1·1655	– ·3544	·2412

LEONARD-SHARPLIN

I	– ·1852	– ·3729	·5923	– ·3649	– ·3307
II	·5076	– ·4039	– ·2096	·1808	·0749
III	·7372	– ·3458	– ·2644	– ·8310	– ·7040
IV	·0250	– ·1596	·4932	– ·1778	·1808
Total	1·0846	– 1·2822	·6115	– 1·1929	– ·7790

GARRETT-SHARPLIN

I	– ·0546	·3453	– ·0124	·1433	·4216
II	– ·7722	– ·1230	– ·1148	·5714	– ·4386
III	– ·4306	·0572	– ·4448	– ·3589	– 1·1771
IV	– ·7487	·3746	– ·7881	– ·7676	– 1·9298
Total	– 2·0061	·6541	– 1·3601	– ·4118	– 3·1239

TABLE I B

VALUES OF Z FOR INTER-MEDIUM COMPARISONS OF FEDA: (F – J) AND (F – E)

LEONARD-SHARPLIN

1. Feda and John

I	·1956	– ·1526	– ·1774	·5874	·4530
II	·1692	– ·6260	·1422	·0712	– ·2434
III	·6341	·1832	– ·4927	·2365	·5611
Total	·9989	– ·5954	– ·5279	·8951	·7707

2. Feda and Etta

I	·0432	– ·5669	·1491	– ·4887	– ·8633
II	– ·2290	·2933	– ·1489	·0813	– ·0533
III	1·0501	– ·2677	– ·6201	– ·2526	– ·0903
Total	·8643	– ·5413	– ·6199	– ·7100	– 1·0069

N.B.—The results for Feda are completely insignificant, and are given only for the sake of completeness. They will not be further discussed.

REPORT ON GLASGOW REPETITION OF DR RHINE'S EXPERIMENTS ON EXTRA-SENSORY PERCEPTION

By Robert H. Thouless

Some years ago I attempted a repetition of Dr Rhine's experiments on extra-sensory perception. I have hesitated to publish the results since the numbers were small and I knew that Dr Soal was carrying out a much more complete experiment in London. Since, however, my experiments have sometimes been quoted and since they throw some light on statistical questions raised in connection with Rhine's work, it seems worth while to publish a short report of the results.

The experiments of Rhine are now so well known as not to need detailed description. There is a pack of 25 cards with five different designs (circle, star, rectangle, waves and cross) and five specimens of each design in the pack. The subject is required to guess which of the five designs is printed on the face of each card in the pack, the face of the card being invisible to him. If he is merely guessing, it is expected that he will, on the average, get five right in each pack. If he gets significantly more than five right under conditions which preclude any normal means of gaining knowledge of the design, this is taken to be evidence that he has some power of extra-sensory perception (E.S.P.), that is of telepathy or clairvoyance or both according to the conditions of the experiment.

My experiments were carried out under various conditions. Generally the face of the card was visible to the experimenter and the back visible to the subject if he cared to look at it. Some experiments were witnessed but the majority were not. The subject was sometimes not informed of successes until the pack was finished, but more generally he was told after each successful guess. One series (with myself as subject) was done by the ' pure clairvoyance ' method. The conditions were generally not as rigid as would be required for crucial experiments but my intention was to introduce rigid control of conditions if positive results began to appear. At no stage was there any sign of positive results, so the experiments are here reported all together without any distinction of conditions.

In the Table below, I show the number of times each subject gets 0 right in a series of 25, the number of times he gets 1 right, etc. This seems better than Rhine's method of merely reporting total results, since this method would show if the capacity appeared sporadically and rarely. Also it demonstrates the actual distribution of successes.

TABLE OF RESULTS

Subject	Number Right												Total	Expected Total
	0	1	2	3	4	5	6	7	8	9	10	11		
S.T.	0	0	0	1	3	3	2	1	0	0	0	0	49	50
M.D.	0	0	0	1	1	0	1	1	0	0	0	0	20	20
D.D.	0	0	1	0	2	1	0	0	0	0	0	0	15	20
P.T.	0	0	0	5	5	8	0	2	1	1	0	0	106	110
J.S.	0	1	0	2	2	3	3	4	1	0	0	0	84	80
M.L.	0	0	1	1	1	0	0	1	0	0	0	0	16	20
E.L.	0	0	1	1	0	0	1	1	0	0	0	0	18	20
W.B.	0	1	1	5	3	7	4	3	0	0	0	0	110	120
E.T.	0	0	4	4	7	6	5	3	3	2	0	0	171	170
A.C.	0	4	7	7	22	19	18	13	5	4	1	0	507	500
R.T.	0	1	3	3	2	5	1	4	1	0	0	0	91	100
W.	0	0	0	1	1	2	1	1	0	0	0	0	30	30
X.	0	0	1	2	1	1	0	0	0	1	0	0	26	30
Y.	0	0	2	0	0	1	2	0	0	0	0	1	32	30
Z.	0	0	1	2	0	0	2	0	0	0	1	0	30	30
Totals	0	7	22	35	50	56	40	34	11	8	2	1	1305	1330

If nothing but chance were operative in producing the above results, we should expect that the numbers in the bottom row would be distributed in close agreement with the numbers obtained by the expansion of the expression $N(4/5+1/5)^{25}$ (where N is the number of series of 25 guesses). The numbers observed and expected on this hypothesis are shown below.

	<2	2	3	4	5	6	7	8	9	>9
Observed -	7	22	35	50	56	40	34	11	8	3
Expected -	7·3	18·9	36·1	49·7	52·2	43·5	29·5	16·6	7·8	4·6
Deviation from expn.	−·3	+3·1	−1·1	+·3	+3·8	−3·5	+4·5	−5·6	+·2	−1·6

These are only such deviations from expectation as might be expected to occur by chance. This may be tested by summing the squares of the deviations divided by the expected number in each class and so obtaining the quantity χ^2 which turns out to be 4·25, a value which will be exceeded by chance alone in about nine observations in ten. Agreement with the expected logarithmic distribution on the hypothesis that no subjects were showing extra-sensory power is, therefore, perfectly satisfactory.

The same conclusion emerges if we examine the mean score and standard deviation. If chance alone were operative, the expected

mean number of successes would be, of course, 5, and, if the scores were distributed on the expected logarithmic curve, the standard deviation should be exactly 2. The observed mean is 4·906 and the observed standard deviation is 1·93. The standard errors of these are ·23 and ·16 respectively, so in each case the observed deviation from expectation is a little less than half the standard deviation and the probability of chance occurrence is between ·6 and ·7.

Agreement with the chance hypothesis is unquestionable, therefore, either when the results are treated in mass (as in the last paragraph) or when we consider (as in the earlier calculation) expectations of given numbers right in separate series of 25 guesses.

The advantage of considering expectation in separate series of 25 is that it might reveal the presence of extra-sensory perception even if it occurred so rarely as to be swamped by chance results in the total. It remains possible that E.S.P. occurs sporadically over periods not long enough appreciably to affect whole series of 25, but long enough to affect shorter sequences. If this were the case, neither of the previous tests might reveal its presence but it might be shown by a tendency for successive guesses to be right more often than one would expect in a random distribution. I, therefore, counted the number of single right guesses, of two successive right guesses and so on.

The mean chance expectation of k successive right guesses can be easily shown to be $(26-k)/5^k$ in each series of twenty-five guesses if the probability of each single guess being correct is 1/5. From this formula, we can find that the mean chance expectation of series of *only* k successive right guesses (*i.e.* of a series of k right guesses which is not part of a series of more than k right) is

$$(424-16k)/5^{(k+2)}$$

for all values of k up to 24.

From this formula, we can compare observed and expected numbers of single right guesses, of pairs of right guesses, etc., in any number of series of 25 guesses. I have counted these for the first 241 series of 25 with the following result :

Consecutive right guesses	1	2	3	4	5	>5
Observed	778	150	26	5	1	0
Expected	786·6	151·2	29·0	5·55	1·06	·25

It is clear that there is no tendency for an excess of sequences of right answers such as might be caused by occasional bursts of E.S.P. capacity.

All tests, therefore, show that these results are such as are to be expected on the hypothesis that no E.S.P. power was shown by any of the subjects. I do not wish it to be supposed that I am claiming that these are evidence against the reality of Rhine's results. A negative result by one worker cannot be regarded as a disproof of a positive result obtained by another if the positive result has been obtained under satisfactory experimental conditions.

I think that the results do show that the hope that Rhine has discovered an easily repeatable technique for demonstrating E.S.P. is not fulfilled. It is true that the number of cases is not large, but in the preliminary experiments reported in Rhine's first book, some indication of positive results emerged from shorter series than this. The question of the repeatability of Rhine's experiments depends much more on Dr Soal's extended series of experiments than on my own short one.

On one important point, I think I can give some support to Rhine. Some of his critics have doubted whether he was correct in assuming $2/5 \times \sqrt{N}$ as his standard error in a random distribution. One critic, at least, even doubts whether 5 is the mean chance expectation of number right per pack of 25. My results appear to be due solely to chance and both the mean of 4·906 and the standard deviation of 1·93 are not significantly different from the expectations assumed by Rhine of 5·0 and 2·0 respectively.

It is true that the fact that there are 5 cards of each kind in each pack will tend to reduce the standard deviation unless subjects guess exactly five of each kind also (obviously if subjects always guessed the same card throughout a suit, the standard deviation would be zero, although the mean chance expectation of five right would remain unchanged). The formula for allowing for this effect has been given by Dr Stevens in a recent publication. Since, however, this effect would reduce the standard deviation and, therefore, the standard error, the ignoring of it is a mistake on the right side since it causes the significance to be underestimated. In my experiments, at least, the effect of a non-equal distribution of guesses between the five suits seems to be negligibly small. In any case, this effect would reduce the standard deviation and, therefore, the standard error, so, in ignoring it, we are making a mistake on the right side, which will lead only to an underestimation of the significance of the evidence.

That my failure to reproduce Dr Rhine's result may have been due to failure to reproduce his conditions, I am, of course, willing to admit. The necessity for certain conditions not reproducible by a conscientious and experienced experimenter is, however, a factor

interfering with repeatability of the experiment. If E.S.P. is a genuine phenomenon, it is to be hoped that investigators of it will so standardise the conditions, that the results can be repeated in any laboratory.

There is one point about the conditions which I should like to emphasise. It was always my wish to keep the subjects in a state of believing that they were getting positive results. Some were, from the beginning, unbelieving, but, in many cases, this condition appeared to be fulfilled. It is made easy by an important psychological factor, that people generally expect deviations from mean chance expectation in a random series less frequently than they actually occur. The most naive subjects were strongly impressed by their success in getting even five right out of twenty-five and the less naive were strongly impressed if they got nine or ten right and such an experience would keep them encouraged through many series in which less than mean chance expectation was obtained. It was indeed easy to encourage subjects to believe that they had 'good' and 'bad' days, although the alternations of several successful series with several unsuccessful ones turned out, on critical examination, only to be such as would occur in a random series. I may have failed to reproduce some essential condition for successful demonstration of E.S.P. but I do not think it was lack of faith in my subjects. With a little care, this is one of the easiest conditions to produce in most subjects who do a number of sets of guesses.

A PROXY EXPERIMENT OF SIGNIFICANT SUCCESS

By C. Drayton Thomas

The proxy case here described is probably unique. Its special interest lies in the fact that an intermediate person so arranged it that the applicants had no link with sitter or medium until the experiment was completed.

Despite this absence of contact the desired communicator produced substantial evidence of his identity.

Two later experiments yielded interesting confirmation of the first.

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Frederic William Macaulay

In June 1936 Professor E. R. Dodds suggested that I should try to obtain information at my sittings for a scientist friend of his and sent me the following information to work upon :

> Wilfred Stanley Lewis of Exeter requests a communication from his father-in-law.

Finding that it would be some time before I could undertake this experiment at my sittings with Mrs Leonard I promised to try with another sensitive.

Professor Dodds (unknown to me) wrote to Professor Lewis as follows :

" The friend who undertook to try for a proxy sitting on your behalf has arranged one for Tuesday next, June 16, with a new medium, not Mrs Leonard. The medium is not to be told the purpose of the sitting, but I have asked my friend to concentrate before the sitting on the following ' request ',—Wilfred Stanley Lewis of Exeter requests a communication from his father-in-law. My friend has been told nothing whatever about you except that you are a distinguished man of science and that your wife has had supernormal experiences and has had successful sittings with other mediums. It seems to me that it might help, and could do no harm, if your wife put up a ' request ' on her own to one or both of your fathers-in-law to communicate at this sitting."

That sitting was a complete failure and in reporting this to Professor Dodds I wrote :

" I will try if possible to get something with Mrs Leonard presently ; could you get me a rather better clue meanwhile. Merely to ask my people to find the father-in-law of Mr Wilfred Stanley Lewis of Exeter does not help much, as it omits the name of the required person. Suppose *you* were asked to find the father-in-law of anyone named as living in London, and the required individual were said to be resident in Birmingham, you would be puzzled how to proceed!

" Perhaps you could get me the name and the month of decease. That might suffice."

Note.—I was unaware that the desired communicator had been connected with Birmingham, and named that city in the above illustration only because that was where Professor Dodds was then residing.

To this letter he replied on June 21, 1936 :

" The person from whom a communication is desired in the Lewis experiment is Frederic William Macaulay, who died May 20, 1933. He was for the greater part of his life associated with Birmingham (but was not known to me). It is suggested that the ' request ' should be presented as coming from his daughter ' Emma ' (Mrs Lewis), rather than from his son-in-law, whom he did not know so intimately. They suggest also that if a communicator emerges who *might* be identified as this F. W. Macaulay, the following question might (if opportunity offers) be put to him at the sitting : ' Can you tell me anything of the two men with whom you were so closely associated in your last unfinished piece of work? ' "

It is important to remember, while reading the following account, that the above includes everything known to me up to the end of the fourth sitting ; also that neither Mrs Lewis nor her husband had had any intercourse with Mrs Leonard. Their earlier sittings with Mrs Brittain, to which reference is made below, have never been published.

No mention of the proposed experiment was made, either to Mrs Leonard or to Feda her Control. The method I adopted was one which had often led to success in proxy cases, and included an appeal to the desired communicator in my study at home.

My sittings for the first part of the experiment were taken on the following dates :

June	26,	1936,	comprising	paragraphs	1–22	in the record.
July	24,	,,	,,	,,	23–44	,, ,,
Aug.	21,	,,	,,	,,	45–51	,, ,,
Sept.	25,	,,	,,	,,	52–80	,, ,,

At the first of these sittings Feda introduced a new communicator *without any leading on my part.* When she announced a stranger's presence I remarked that I had invited someone. After the stenographer had taken down 14 items of an apparently evidential character I named Frederic William Macaulay as the person from whom I was hoping to hear. This I did to make sure of being on the right track ; for it seemed unwise to waste time in uncertainty. Feda stoutly insisted that we had the person I desired.

Mr Macaulay had been a water-engineer interested in hydraulics. From messages given in the above sittings I select some references to this occupation, as also to other of his interests and to certain articles which he had been in the habit of using.

REFERENCES TO PROFESSIONAL WORK

(The annotations of Mrs Lewis, his daughter, are within brackets. Figures in left margin denote the place of each item in the full Leonard record : the first figure being the number of the sitting.)

Date of sitting : June 26, 1936.

FEDA : 1/5 He pictures big stones built one on the top of the other. This refers to the work he was doing before passing. The letter " M " is shown over these stones, it is a name very much linked with this work.

(Quite interesting ; he was constructing a very large dam at the time of his death. The " M " might stand for the county of Monmouth in which his work was being done.)

FEDA : 1/6 There is also a big initial " H ", another word connected with the same thing.

(Possibly indicating hydraulic works. He was specially interested in hydraulic engineering.)

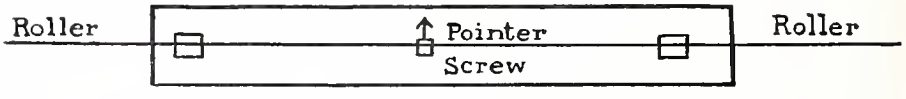
FEDA : 1/7 He had completed a period of service a little while before he passed. A circumscribed period of time came to a finish not long before he passed, and January has to do with the finish of the period.

(He retired into private practice just under three years before his death. The certificate of safety for a reservoir, the contruction of which he had begun before his retirement, was given in the January before his death.)

Date of sitting : July 24, 1936.

FEDA : 2/23–25 An instrument is described as " steel and rather long shaped ". He holds it in his hands. At one end there is something that screws on to it and can be slid along. He holds this instrument carefully, measuring something on it, and holds it flat and then holds it up as if looking for a mark.

(Might be a 2 ft. steel ruler, mounted on rollers and with adjustable pointer that could be screwed into position which my father always used (see sketch).)



FEDA : About the two men he was working with ; one was much linked with him over this instrument which they used in doing special work together.

(One of the two men in particular was often in my father's drawing office with him.)
He shows something like a table, long and rather narrow for its length.
(The drawing office had a long narrow table.)
They don't seem to be sitting, but standing near this bench, bending over it a little.
(The table was built to stand at and not to sit at.)
Artificial light hangs over it and is adjustable.
(There were adjustable lights over this table.)
And I hear glass; he makes me hear the tinkle of glass and metal.
(The glass might be his glass tracing table. I can still mentally hear the click of his steel ruler on this.)
I hear a door closing with a sound as if not an ordinary door. Something to stop it making a loud click as it closes.
(In his office there was an automatic door-closing arrangement.)
This room was frequently used by him. It had many books connected with his work.
(There were many books on engineering and meteorology in the room.)

Two further items, both said to be wrong, are discussed later in this paper under the heading " Inaccuracies ".

FEDA: 2/41 (*a*) I don't think he expected to pass over. He speaks about a journey he was thinking of taking, and had made some preparation for, only a little while before he passed. He would like to have taken that journey but did not.
(I think the reference must mean Southend where the Water Engineers were to meet. I have ascertained that my father, who was for long a most active member of that body, did intend to go to the Meeting.)

On inquiry at the Institution of Water Engineers I learn that the date of their Southend Meeting was May 10, 1933; this was ten days before Mr Macaulay's death and when his illness had become troublesome.

FEDA: (*b*) A place beginning with the letter " S ", very much connected with him and his work. A place with a good many letters in its name.
(When he came home from the West Indies he and his family lived in Southampton for many years.)

At the time of the sitting I supposed that both the above referred to the same place, but it now seems probable that recollection of the intended visit to Southend, or possibly similarity in name, brought up memory of Southampton, which place the British Waterworks Association had visited during its Meeting at Bournemouth in June 1932. Mr Macaulay had attended that meeting.

Date of sitting, September 25, 1936.

FEDA : 4/55 He shows me some books, not ordinary printed or reading books, but books with his own handwriting in, as if he had made an awful lot of notes. I am seeing the " R " again and I think the notes would be in connection with this name " R ".

(He did leave a mass of written notes about his work on reservoirs.)

FEDA : 4/61 To do with his work. I feel a very strong light and a counter or bench and a peculiar looking instrument which I should think his daughter would know. It has a long arm to it through which a little piece is sticking out, and that piece goes up and down a little like a balance. It feels very much like a weight thing, but not a big thing like people stand on. He doesn't put weights on though, he seems to put on it glasses and bits of metal. It is important.

(When I began to read the description of this instrument I at once thought of an appliance which he frequently used in his earlier work to test the strength of concrete. Blocks of concrete were put in at *a a* and subjected to an increasing stress till they fractured. He would examine the pieces by a good light after breakage. Glass played no part in the process.)

Mrs Lewis added the accompanying sketch :



FEDA : 4/63 There is another thing he does : he makes funny figures and writes things down, not like 1, 2, 3, but different figures that mean something, and he writes them in funny

ways. They are like the Signs of the Zodiac, like the writing on the parchments in the East.
(My father did a good deal of research in hydraulics, in which subject Greek letters are used as well as figures. This may be an attempt to convey the notation of the subject. He often discussed his work with my brother and myself, jotting down formulae and so forth on odd scraps of paper as he talked.)

Personal Memories

Date of sitting, June 26, 1936.

Feda : 1/3 He went rather quickly, and didn't think he was going ; as if he felt " I have things to do and hope to live on and do them ". Disappointed at having to go.
(He was advised to have an operation for the removal of gall stones. Was working as usual on May 18, 1933, and was dead by the morning of May 21. The attitude described above echoes a hope he expressed to me that he still had many years to go, as there was so much interesting work to be done.)

Feda : 1/4 Interrupted when he was writing important instructions just before he passed.
(Written instructions about work and the general disposition of his affairs were left unfinished.)

Feda : 1/8 He had a presentation not long before he passed over, with a name and certain details inscribed on it.
(He had a presentation, accompanied by an illuminated address ; this was in 1930, exactly three years before his death.)

Feda : 1/9 He was liked in two ways, in his work and in a hobby. This second string to his bow he did not do for money. It was good work for which people were grateful. When he passed there was public mention made of this.
(He used to say that he had two hobbies,—his work as an engineer and the work of helping young engineers. He had many tributes from young men whom he had assisted in a variety of ways.)

Feda : 1/10 Has his daughter a special cup that he drank from and that he could show to people, or people might ask to see? Not an ordinary thing.
(He had several silver tankards which he won shooting at

Bisley. He drank from these. Each of his five children has one now.)

FEDA : 1/11 He had not been well for two or three years, but did not regard it seriously.
(Some two years before his death my father began to have attacks of pain in his back and side. His doctor dismissed them as lumbago until shortly before his death, when a specialist was consulted and diagnosed the presence of gall stones. My father did not worry much until the last severe attack when the specialist was called in.)

FEDA : Two months before the end he had disquieting symptoms that seemed serious, but it was only a short time before his passing that he realised its seriousness. He says,—" My heart seemed very tired," something affected it.
(Two months before the end he spoke rather despondently to his son-in-law about " always feeling tired ", and said that it was a great happiness to know that, should he not have much longer to live, his children were all settled and happy.)

FEDA : 1/19 He began life in one place, then spent the rest of it in a very different kind of place. Very different conditions between them.
(He spent his boyhood in the West Indies and the rest of his life in England or Ireland.)

Date of sitting : July 24, 1936.

FEDA : 2/33 His daughter has been a little surprised, hasn't she, about something in a material way? Something he had not got? Something financial that he had not got, which she thought he had got,—that he had not left something. As if she said, " Why hasn't he got it? " One particular thing, not quite so much of it as they expected. But in another way there was. They would not be unduly worried because other things made it balance, but they would be puzzled.
(Correct. We did wonder where money which had come from one particular source had gone. But things did " balance ". My father had inherited about £6,000 from a great aunt. We knew that he had had certain losses during the Great War from money invested abroad ; but this particular sum was invested at home and we did not know, until after his death, that he had realised the capital and used it—chiefly to help needy friends and relations.

At the same time " Things *did* balance " in the sense that my mother, though she could not enjoy the income from this sum, was left very comfortably off. And the family felt it was quite fair as much of the money had been spent on a relative of hers, and some to pay off her own debts.)

FEDA: Sets of boxes. He had what he calls a nest or set of boxes
2/36 that he was very interested in, and he used to keep things in them. Several of them and they all seem to fit into each other, to fit in together into something that looks like a case over them. It seems important to him. I don't think they were all full.
(His treasured tool chest was a nest of drawers. He delighted in tinkering about the house and garden, and always kept all tools, nails, screws, and so forth in perfect order.)

Date of sitting: August 21, 1936.

FEDA: There is also a John and a Harry, both with him. And
3/47 Race . . . Rice . . . Riss . . . it might be Reece but sounds like Riss, and Francis. These are all names of people who were connected with him or linked up with him in the past, connected with happy times. I get a feeling of an active and busy home in which he was rather happy.
(This is a very curious passage, taken in conjunction with " they are connected with rather happy times. I get a feeling of an active and busy home in which he was rather happy." Probably the happiest time of my father's life was in the four or five years before the war, when we, his five children, were all at school, and the home was packed with our friends during the holidays. John, Harry and Francis could be three of these. Francis is certainly dead —I do not know about John and Harry. But the most interesting passage is " It might be Reece but it sounds like Riss ". This carries me back to a family joke of these pre-war days. My elder brother was at school at Shrewsbury and there conceived a kind of hero-worship for one of the " Tweaks " (sixth form boys) whose name was Rees. He wrote home about him several times and always drew attention to the fact that the name was spelt " Rees " and not " Reece ". In the holidays my sister and I used to tease him by singing " Not Reece but Riss " until my father stopped us, explaining how sensitive a matter a young boy's hero-worship was. I think Rees was killed

in the Great War. He was never at our house, but we had him carefully pointed out to us whenever we were at Shrewsbury. This Reece-Riss reference is quite characteristic of other sittings I have had in which have been made quaint little references to small matters that yet are important with reference to my father.)

Date of sitting : September 25, 1936.

FEDA : 4/56 This gentleman would have had pains in his limbs? I get rather a stiff feeling and aches in the limbs. Something he suffered from in later years.

(These were symptoms of his last illness.)

FEDA : 4/57 Also a peculiar feeling in one hand too. Will you ask his daughter if there was something about one hand that made it uneasy sometimes. Something not quite right with one hand. I feel he had done something to one hand that would make it a little different from an ordinary person's hand.

(About a year before his death he had severe blood poisoning in one hand. I believe it was always tender afterwards.)

FEDA : 4/67 I get a funny word now . . . would he be interested in . . . baths of some kind? Ah, he says I have got the right word, baths. He spells it B A T H S. His daughter will understand, he says. It is not something quite ordinary, but feels something special.

(This is, to me, the most interesting thing that has yet emerged. Baths were always a matter of joke in our family—my father being very emphatic that water must not be wasted by our having too big baths or by leaving the taps dripping. It is difficult to explain how intimate a detail this seems. A year or two before his death my father broadcast in the Midland Children's Hour on " Water Supply " and his five children were delighted to hear on the air the familiar admonitions about big, wasteful baths and dripping taps. The mention of baths here also seems to me an indication of my father's quaint humour, a characteristic which has hitherto been missing. I should add that, as Water Engineer to a city where the water consumption was very great and at times in excess of the supply, my father was very interested in baths, taps and all distribution fittings. He was a member of a Committee which standardised the two latter, and persuaded his Water

Committee to adopt these standardisations. He also advocated the installation in new houses of *small* baths.)

Mrs Lewis wrote later : " I think the missing characteristic, his whimsical humour, emerged in this BATHS reference." This refers to a remark in one of her previous letters to Professor Dodds, that, in the first sittings, she missed one of her father's characteristics. What this characteristic was she did not state until writing the above comment.

FRIENDS AND ACQUAINTANCES

Date of sitting : June 26, 1936.

FEDA : 1/15 The man whose daughter wants to hear about him is here and someone called Alfred is linked up with him and is very important to him. A " T " and a " J " connected with him. There is something about this man Alfred that I think will be very interesting.
(Alfred, who died many years ago, was probably his closest young friend after his own relations. Alfred's second Christian name began with " T " but I have no means of finding out if " J " applied to him also.)

Date of sitting : July 24, 1936.

FEDA : 2/26 There is a man James near him, not on the earth. James passed over some time before him. He likes being with him very much.

FEDA : 2/30 James again, and with him comes a lady called Mary.

FEDA : 2/31 You said his name was Macaulay. Well, he is giving me another Mac name, but not that. Somebody he was very close to on earth, very near indeed, and his daughter will know whom he means. She has been thinking something about this Mac person a little while ago.
(James McCance had a daughter Mary who died a few months ago.)

Mrs Lewis wrote more fully at a later date, *i.e.* November 23, 1937, when the whole experiment had ended, regarding her father's friendship and affection for his cousin James McCance :

> (I do not think I have sufficiently emphasised the very close friendship that existed between him and James McCance. After his father, mother, grandmother and brother, James would have been by far the most likely

person for him to seek out. They were, indeed, more at one in their tastes than my father was with the other relations I have mentioned; the tie with them was more purely affectionate. Next to James and Ernest would probably come Alfred who was always called "Fred" or "Freddie" by my father. These two are both friends of his youth and middle age.)
For the names Alfred and Ernest see paragraphs 1/15, 5/91 (pp. 267, 271).

Date of sitting, September 25, 1936.

FEDA: 4/66 There is a very old man with him who passed a good bit before he himself did, one much linked up with him and who would be interested to meet him again. Quite an old man and very white.
(The description could apply to my father's first "chief" James.)
Before he passed he would have been what you call a bit doddery. This old man would have been in bed for some weeks before passing; some people would find him rather difficult, but he was very clever and rather an unusual sort of man, rather peculiar. This F. W. gentleman knew him very well. Now they have many things to talk about.
(I have heard my father say that James was becoming "doddery" before his death. He also said how very "difficult" some people found him. Mr Drayton Thomas spoke my father's name the wrong way round, but Feda has the initials in the correct order. I mention this because my father hated to have his name incorrectly arranged.)

Yes, I had inadvertently given the name as William Frederick Macaulay. It may be of interest to remark that the full name had been spoken by me to Feda only twice, and that was during the first sitting three months before.

FEDA: 4/68 "Minster" or something-minster. Please ask the daughter if she remembers a name that would be familiar to him, and it is something-minster. Minster isn't all the word, I feel I am missing part of it. Sometimes there is a connection between what they have just said and what they go on to. If people took the trouble they could always trace the possible connections.
(The James referred to above, and no connection with

James McCance of 2/31, had his offices in Victoria Street, Westminster. My father worked there for a few years and was later his assistant engineer in the provinces for many more years; moreover, when my father retired from his post in Birmingham, he became partner in the firm of Rolf & Rafferty of Westminster.)

FEDA: He is trying to give me something to do with eyesight or eyes. It wasn't so bad, but there was worry over it.

4/69–70 Godfrey; will you ask the daughter if she remembers someone called Godfrey. That name is a great link with old times.

(My father's most trusted clerk, one who specially helped in the hydraulic research, was called William Godfrey. He was with my father for years and I remember him from almost my earliest childhood. He has recently been gravely ill and my father would certainly be interested in his progress. The reference to eyesight might possibly refer to this Godfrey who did have great trouble with his eyes latterly. At the same time my father also complained, towards the end of his life, that reading and close work tired his eyes excessively and gave him headaches. It was a very common gesture of his at this time to take off his glasses and rub his eyes, a thing I had not seen him do before though he had worn glasses for years.)

FEDA: What is that? . . . Peggy . . . Peggy . . . Puggy . . . he is giving me a little name like Puggy or Peggy. Sounds like

4/79 a special name, a little special nickname, and I think it is something his daughter would know. Poggy, Puggy or Peggy. I think there is a " y " on it.

(My father sometimes called me " pug-nose " or " Puggy".)

Result of First Four Sittings

Out of the 94 items given during four sittings this communicator was successful 70 times. Such a proportion of accuracy—74·4 per cent.—places the result beyond anything attributable to chance coincidence.

Professor Dodds suggested an original system for marking these records. We used his method independently and then compared our results. We were in agreement save that he had given a slightly lower mark for items relating to the communicator's interest in engineering. This he did on the assumption that such interest might have been assumed by the medium or Feda after the first

reference to engineering. I did not lower the marking for these because, at the time they were given, neither Feda nor I had any inkling that they were true to fact. The net result of this small difference in our valuation is, however, almost negligible. Hence it seems simpler to use, for this paper, my own marking in which "Right, Good, Fair" are counted as successful, while "Poor, Doubtful, Wrong" are failures.

The numerical analysis of the four sittings is :

Right -	36	Poor -	2	
Good -	10	Doubtful	19	TOTAL NUMBER OF ITEMS 94
Fair - -	24	Wrong -	3	Successes, 74·4 per cent.
	70		24	

Such a result, in the absence of any link between applicants and sitter, strongly supports the argument for telepathic communication by the "deceased" F. W. Macaulay.

SECOND PART OF THE EXPERIMENT WITH MR MACAULAY

January 22, 1937

After I had seen Mrs Lewis's annotations to the foregoing it was suggested by Professor Dodds in a letter dated December 2, 1936, that I should ask the same communicator to say something about his daughter's recent visit to Ireland. He added that this request had reached him "some time ago". No further clues about this visit were given me.

I decided to try. Mentally, while at home, I asked Mr Macaulay to attend my next sitting and to speak about his daughter's Irish visit. It was thus some two months after Mrs Lewis' request to Professor Dodds that I made the attempt, and it was fully seventeen weeks since the last Macaulay communication, during which interval I had taken seven sittings. Yet Mr Macaulay was now introduced by Feda *without any inquiry or remark by me*, save that I gave his name when informed that a man was present about whom Feda asked whether I wished to give him an opportunity of speaking. Here is the verbatim report of that introduction :

FEDA : Now . . . Just wait a minute. . . . I don't want to waste time on him if he is not wanted, but were you expecting a man?

C. D. T. : I was. I want Mr F. W. Macaulay. We have had him here before and I have asked him to come to-day.

FEDA : Well, he is here, and has he got a daughter on the earth?
C. D. T. : Yes.
FEDA : That is the one then, beeause he says, " Daughter wants more and friend also. Yes, friend . . . friend . . . a man wants to know something as well."
(This last remark possibly referred to the faet that Professor Lewis had suggested my asking for messages from his first wife ; I had not intended to introduee that request until the elose of this sitting.)

Of the 30 evidential items given in this sitting 10 referred to the Irish visit, and the others to the eommunieator's interests.

If I quote a few of these items it will enable readers to follow the allusions made by Mrs Lewis after studying the reeord.

FEDA : I keep getting the name Ernest, but I don't know whether he is on the earth or passed over. I have had it two or
5/91 three times but didn't give it before beeause I don't know whieh side he is on. But I feel something is happening about Ernest that is rather important.
(My father's very elosest friend was ealled Ernest. They were at the University together and were never long without seeing one another. Ernest outlived my father. He was still alive during the early sittings.)
FEDA : He wishes to say to his daughter, " Glad Tidings ". I don't know what he means but it is something happy
5/92 relating to what he is helping from the other side to bring about.
(Shortly after the date of this sitting I reeeived news whieh was real " Glad Tidings ",—namely that my favourite brother, who had been in Canada for ten years and whom we all, my father ineluded, longed to have in England again, was at last in a position to return home. . . . One reason why my father so wanted my brother home was that he has a son. My father used to say : " I don't want the Maeaulays to beeome a Canadian family. If Hugh (the brother) were here, young Diek (the son) might one day ' retrieve the family fortunes ' and buy baek Ben Neagh " (the Irish House).)
C. D. T. : Your daughter wanted to know if you would like to say anything about her reeent visit to Ireland.
FEDA : He says, Yes, that has some eonneetion with the glad tidings of whieh he just spoke. He says that he got your message. He doesn't say his daughter's but yours.

As previously remarked I had, two days before this sitting, mentally invited Mr Maeaulay to be present and give more information. It was the first time I had done so sinee the previous series ended four months before. Mrs Lewis was not informed that I proposed to resume the attempt at this date, but merely that I would try again. I did not mention to anybody that I proposed to do so just now.

FEDA : 5/93 While his daughter was in Ireland there was an anniversary in whieh he was interested.
(Whilst in Ireland my husband and I eelebrated the anniversary of our engagement. The anniversary of my father's marriage also fell within this period.)

FEDA : 5/95 She thinks of a name Whit-something or White. . . . Was that linked with her being in Ireland?
(We have relations ealled White in Belfast.)

FEDA : 5/96–7 At a place " B ", was she interested there in a publie building or publie institution that he would be interested in also? He feels she was.
(This is interesting. The son of the arehiteet of several of the best buildings in Belfast was a great friend of my father. When I was a ehild these two took me to see these buildings. I, in my turn, showed them to my husband on this visit, telling him all I eould remember of my ehildhood's experienee.)

FEDA : 5/97 (*a*) Part of the time she was in town and part in countrified surroundings.
(I was partly in Belfast and partly in the countryside of Antrim.)

FEDA : 5/98 He felt she was in the midst of trams and doing something that made her think of him.
(This is another interesting point. I was really nervous driving our ear amongst the Belfast tram lines and the speeding trams. Several times I told my husband how my father had hated being driven where there were trams. Indeed he would go miles out of his way to avoid them.)

FEDA : 5/99 A public building and trams, rather a wide spaee or square, big and open near it or round it. He gives the word " memorial ".
(We visited the Belfast Memorial and I thought there of my father.)

Having been there myself I can say that the above deseription fits it admirably.—C. D. T.

FEDA: He thinks his writing need not be wasted, something
5/83 might be got out of the writings he left.
C. D. T.: Who could do that?
FEDA: There is a friend on the earth whom he could impress.
C. D. T.: Would that be his son-in-law?
FEDA: I think so. He says, " Not a relation exactly ", but one linked with him on whom he looks as a friend.
(This might possibly refer to a great mass of meteorological observations which my father had made over a certain catchment area in Central Wales. He had, as a matter of fact, discussed these with my husband who is, as a geographer, also interested in meteorology. My husband says that he could indeed handle this material; but that he does not consider himself competent to deal with the hydraulic work.)
FEDA: This man is somewhat psychic and gets impressions on
5/84 which he often acts although he has a well balanced mind.
(This is certainly true of my husband. His " hunches ", as he calls them, were a source of amusement and interest to my father.)
FEDA; He gives the letter " S " to do with that person; it may not be the proper name, but yet quite fitting; it may
5/85 be some special name, but it is one he knows about and you may not.
(This is curious. When I first met my husband I always called him Stanley and so introduced him to my people. On the eve of my wedding, at the instance of his mother, I began to call him Wilfred. My father died before he had become fully accustomed to the name and frequently reverted to Stanley. In a sitting which my husband and I had with Mrs Brittain in June 1933, I made the following note: The Control said, " The lady's Daddy speaks of the gentleman; but he says Stanley, Stanley—not Wilfred, like the lady says.")
FEDA: Was this Mr Macaulay a researcher of some kind? Not a psychical researcher but of something else. He always
5/87 wanted fuller information. He read much about it and went somewhere where he could study this " M " subject.
(" M " is possibly a reference to Meteorology.)
FEDA: Drawings and diagrams had to do with his work. He could
5/88 make them himself but had people to do them for him.
(Correct: diagrams, maps and plans of the region in Monmouthshire where he was constructing a reservoir.)

FEDA : 5/89 (*a*) He refers to this because he was studying a special set of diagrams not long before he passed.
(He did leave, on a certain table in his study—quite apart from anything else—a plan upon which he had himself drawn and coloured-in certain sections and made notes.)
(*b*) He wished to complete it before he passed and a name " G " was much linked with these studies and work.
(He was helped in all his work, and greatly helped, by a former governess in the family whom we all call Gibbie.)
(*c*) I had, he says, copied with my own hand parts of the plan and made notes which I left a little separate from my other work. It was more than plans, the work was actually being done, going on, which he wished to complete when he passed over.
(As I have already said, he died while this work was being done and was still engaged upon it four days before his death.)

FEDA : 5/106 He says " Institution ", as if he had been linked with a special institution, but not always the same one.
(He was closely associated with the Institution of Civil Engineers, and with the Institution of Water Engineers, being on the Council of the former and a President of the latter.)

Whereas many such societies are called Institutes, the two to which Mr Macaulay belonged are termed Institutions, and that is the word used by Feda.

Mrs Lewis made comment on this sitting :

" In view of the idea that mediums may be telepathically influenced by the sitter, it is interesting to note that, though Mr Drayton Thomas, at this sitting, knew quite a number of details about my father, no more definite information is given on points about which he was fairly well informed. Instead, quite new ground is broken about the subject beginning with " M ", which my husband might be able to handle.

" It seems also, in my opinion, that the references to the ' public buildings ' and the ' trams ' in connection with Ireland are striking. Both brought my father very forcibly into my mind."

Ten items related to this Irish visit ; of these but two were wrong. One, apparently some slip in transmission, reads that his daughter was not thinking of him while in Ireland ; the other referred to some gathering of people at 11.30 one morning at which he was being spoken of.

For the whole sitting there were twenty-five successes and five failures. The detailed valuations being :

	Irish References	Other References	Totals
Right - -	5	10	15
Good - -	0	2	2
Fair - -	3	5	8
			—
			25 successes.
Poor - -	0	0	0
Doubtful -	0	3	3
Wrong - -	3	0	2
			—
			5 failures.

Number of items, 30 ; Successes, 83·3 per cent.

To what shall we attribute this increased percentage of success? I had in the meantime seen several letters from the applicants which were forwarded to me by Professor Dodds. Did the sight or handling of these letters establish a link whereby the applicant's knowledge passed telepathically to me or to the medium? I do not think so, but attribute the improvement to practice in the preceding four sittings. This may have familiarised the communicator with telepathic procedure so that he was now better able to transmit his prearranged material. I think we may postpone this question for awhile, merely noting that, since this fifth sitting attained a higher degree of success than the previous four, it might be expected that the same communicator would do better still if the sittings were continued. They were continued, but a different communicator was asked for. What was the result? We shall presently see that the falling off in evidential quality was immediate and definite, a result suggesting that we now had a different person originating the messages.

Inference from Coincidences with Previous Sittings

Appended to this account, see Appendix 2, is a list of items which were given some years earlier by Mr Macaulay when communicating with his daughter through other sensitives shortly after his death.

It was only by a happy chance that I noticed these coincidences when Mrs Lewis kindly allowed me to see records of her earlier investigations in mediumship. It will be observed that no fewer than ten matters which the Macaulay communicator had given three

years earlier through other sensitives he now repeated through Mrs Leonard when I took proxy sittings on behalf of his daughter at the request of Professor Dodds.

This seems fairly conclusive evidence that both sets of messages originated in the same mind. If Mr Macaulay gave them at his daughter's earlier sittings, as she found reason to believe, then it was he who repeated them through Mrs Leonard. (See final note at end of Appendix 2.)

Eight of these coinciding references occurred during my first four sittings, that is to say, before I had seen Mrs Lewis's handwriting or heard of her verifications.

That telepathy from Mr Macaulay's daughter or son-in-law played no part in the Leonard sittings is further indicated by the fact that, *after* my seeing their letters to Professor Dodds, the four further sittings produced *only two more* of these previously given items.

As before remarked, neither Professor Lewis nor his wife had met Mrs Leonard.

Concerning Inaccuracies

In the first 4 sittings there were—

	Wrong	3 ;	Doubtful	19 ;	out of 94	Items.
In the 5th	,,	2 ;	,,	3	,, 30	,,
		5		22	124	

If one were asked to write 124 statements about a person of whom no more was known than I and Mrs Leonard knew of Mr Macaulay, it is improbable that the inaccuracies would be limited to five, or the remarks of doubtful applicability so few as twenty-two.

It is further highly probable that the *quality* of the wrong and of the doubtful statements in such fictitious experiment would be glaringly worse than that of the Macaulay sittings.

A brief examination of the failures is appended to this paper (see Appendix I). It reveals the fact that errors were mostly trivial, while a large proportion of the doubtful items were attempts at personal names. In Leonard sittings there is frequent difficulty and partial failure in the transmission of names.

Reviewing these five sittings as a whole we find that the information offered as evidence of personal identity is strikingly applicable to Mr. F. W. Macaulay. This is his daughter's verdict ; and it is confirmed, for those of us who did not know him, by the information about his work, his friends, and his personal belongings, which is provided by his daughter's annotations.

The messages have all the appearance of coming from Mr Macaulay. Making allowance for occasional blurrings in transmission, it *looks just as if* the material had been selected and given *by him* for *the purpose* of indicating his identity.

Those to whom it seems incredible that dead persons survive in some other state of being, or that, if surviving, they can make temporary use of psychic channels to resume intercourse with earth, —for such minds there is an urge to suggest alternative explanations.

What are the most hopeful of such alternatives? Perhaps to doubt the veracity of those taking part in this experiment, or to think that all might be explained by telepathy from Mrs Lewis to Mrs Leonard.

We may profitably address ourselves to a consideration of this latter alternative. Let us examine certain features of the messages and note their bearing on the telepathic hypothesis.

Experiments to Discover if Mrs Lewis could Influence the Macaulay Communicator

After the second Macaulay sitting Professor Dodds wrote to Mrs Lewis :

" It might be interesting if you tried to convey to your father a request that he should refer at the next Leonard sitting to some specific topic of your own choice (some interest of your childhood, for example). It might be advisable to write down a short statement of this topic and post it to me."

Mrs Lewis chose such a topic and sent a note of it which Professor Dodds received on August 21, 1937, which was the day of my fourth sitting. Neither then nor later was the matter in any way referred to at the sittings.

Mrs Lewis made a further attempt on December 13, 1937, when asking her father to send me a message which I should feel compelled to forward to her. A copy of the request was sent to me in a sealed envelope which I was not to open. It therefore remained unopened until December 1938, when I noticed it while finally examining all material for purposes of this paper.

The note reads : " I have asked my father to try and send me a message which you will feel compelled to forward to me. I leave the subject matter to him ; but he is to be urgent about it. I have also asked that he shall not mention this sealed envelope to you, but shall be so emphatic about the message that you will open it. Eve Lewis. December 13, 1937."

My records of sittings contain no message from Mr Macaulay later than his communications of January 22, 1937, about the Irish visit. I have never been aware of any urge to open the envelope and had forgotten its existence.

In neither of these minor experiments do we find any evidence that Mrs Lewis could influence the messages given in Leonard sittings or impress my consciousness.

Was there Telepathy from Mrs Lewis?

Such successes as have so far attended experimental telepathy show that ideas caught by the recipient were sometimes those which the person transmitting desired to send; but at other times the received ideas had not been in the forefront of the agent's mind although familiar to him.

We cannot be sure that thoughts about her father would pass telepathically from Mrs Lewis more easily while she was thinking of my proxy sittings: nor do we know that, while she was engrossed in other matters, her subconscious memories about him could more easily reach me or Mrs Leonard. Indeed we know so little of the laws governing the telepathic processes that dogmatism is entirely out of place.

What we actually discover in these records is that, while much referred to matters within the daughter's knowledge, the communicator was able to insert three items based on information which she did not share: I shall refer to these items under the titles of "Gibbie", "Disappointment" and "Glad Tidings" (see pp. 283–285). Let us first consider the absence of reply to a request for information about Mr Macaulay's colleagues. The answer to this might have been given in various ways, the simplest being the statement "both have died".

I presume that Mrs Lewis hoped for this and possibly expected it; the answer was certainly in her mind both consciously and subconsciously. Had she been capable of influencing my sittings telepathically it is exactly here that we might look for evidence of that ability. Inference from silence is unreliable, but, for what it is worth, it here tells against telepathy from Mrs Lewis.

The following extracts contain everything said, whether by me or by the communicator, on this subject.

Professor Dodds, when informing me of the applicant's wish for a message from Mr Macaulay, wrote, "If a communicator emerges who *might* be identified as this F. W. Macaulay, the following question might (if opportunity offers) be put to him at the sitting: 'Can you

tell me anything of the two men with whom you were so closely associated in your last unfinished piece of work?' "

Sitting 1, *paragraph* 4.

C. D. T.: Your daughter is very anxious to have some evidence of your identity. She asks if you remember, and would care to say something about, two men who were connected with you in the last bit of work that you did and which you were not able to finish?

To this Feda replied that he recognised about the daughter and about the two men; she then passed on to descriptions of the communicator's work.

Sitting 2, *paragraph* 23.

C. D. T.: Your daughter will be so glad to hear from you, and she thought if you had any difficulty about proving your identity you could easily do it by giving information about those two men with whom you used to work when you passed over and left your work unfinished.

FEDA: He knows what you mean.

There immediately followed the description of an instrument which is recognised as one regularly used by Mr Macaulay in his office. The message then continued:

FEDA: You said two men he was working with, didn't you? Well, there was one who was very much linked up with him over this instrument. They were doing some special work together in which they used this particular instrument. (Mrs Lewis writes: He and one of the two men in particular were often in my father's drawing office.)

Feda next gave several particulars about this office and added:

> One of the two men—both the men he worked with seem to be on earth still; because he is talking of one man of the two who seems to be away somewhere. One—but where is the other one then? I think I have got this right. There is a man who is on the earth who worked with him, but he seems to be going away. There is some change round this man.
> (The two men I had in mind when asking the question are both dead. Of his many other colleagues I cannot speak.)

Such a reply cannot be regarded as due to the daughter's telepathic activity.

Sitting 3, *paragraph* 45.

On Feda's remarking that the communicator was present I read aloud a message sent for that purpose by Professor Dodds. It included the following :

C. D. T. : It might be useful to say that the two men about whom we asked were Dr L. and Major L. and may I say that the Doctor's Christian name begins with P.—Dr P. L. And the Major's name begins with two L's. Your daughter thinks that the lady of whom you spoke is Gibbie. She is very much worried about the Gibbie situation and can you advise her about this?

It was probably unwise to put more than one question at a time. The reply dealt with the last one only and did not allude to the two men.

Sitting 4, *paragraph* 53.

C. D. T. : His daughter says . . . if her father felt inclined she would like to hear a little more about the two men he used to work with, Dr L. and Major L. that she would know . . . Anything you like to say to your daughter she will be very pleased to hear, also anything for your friends.

FEDA : Well, he would very much like to do that. Will you please leave the suggestions with him and he will deal with them as he can in between other things.

No further allusion was made to the two men and I reproach myself for omitting to renew the request at the fifth sitting.

Whatever the originating mind from which came so many intimate references to Mr Macaulay's professional work, his personal memories and other matters, we can scarcely credit that it was wholly ignorant of these colleagues with whom he was working at the time of his death.

That this reiterated question was overlooked, or disregarded in favour of other evidential material, seems to me the most probable explanation of the silence. And it does, at least, find some support in the obvious difficulties of the transmission process. We find hints of those difficulties in the following passages :

Sitting 1, *paragraph* 9.

C. D. T. : That seems to suggest Free Masonry.

FEDA : Well, don't suggest at all ; he says you might lead him off on a wrong track that he doesn't want to go on. Don't suggest, he says.

Sitting 3, *paragraph* 52.

FEDA : He is just waiting a minute because there was something he wanted to think about : so while he is thinking about it I will go on to something else.

Another communicator was then introduced. Towards the end of the sitting Feda reverted to Mr Macaulay.

FEDA : Oh, that gentleman hasn't waited. I thought he was only just going to step back a bit, but I suppose he gave all he could.

C. D. T. : He has not told us about . . . those two gentlemen.

FEDA : No. I left that with him, he is going to think that over. He gave some new things to-day. They can't always elucidate things they are wanted to; They can't do anything just to order, they have got to prepare it themselves and then get it through.

Sitting 4, *paragraph* 58.

The following paragraph suggests difficulty and is also remarkable because of the direct voice which occurred no less than six times. By that I mean that the words printed in italics in the extracts that follow were heard by me and the stenographer in a loud whisper which was strikingly different from the Feda voice. It sounded on each occasion as if produced some twelve or more inches from the medium's lips. This repetition of direct voice is the more striking as there was but one other instance of it in the whole sitting.

FEDA : *Ideas.* . . . He says his ideas have altered very much about his work since he passed over. He is still interested in his work, but he has got such different ideas about it that if he came back he would want to continue. . . . Wait a moment. . . . He is giving me such a strong look —He would want to continue the same work on entirely *different* methods. The methods were wrong ; he says, I am going to try and help the people with whom I was working on earth. I will try to tell you some more as I go on, but it is difficult to express myself in this way— in—something *terminology*—not the right *terminology*. He says the methods were leading on a wrong line, but we were following them up because they seemed to lead somewhere. But later on, if we *persisted*, we shall find we have got to *retrace* our steps.

Sitting 4, *paragraph* 80.

FEDA : He is showing me like a place where he would go up hill,

a place he was very, very interested in. (Pause.) I feel that that is it, do you see, because though he shows me a little quick picture he didn't show me going anywhere; He just said it was a place that he was very much interested in. Now what are you showing me?—He is stopping. I don't know whether he is going away.

What of this failure to answer the question? I have not found that questions about facts unknown to the person taking a proxy sitting are usually unanswered. On the contrary they sometimes elicit quite good replies indicating knowledge which the deceased was known to possess. When questions are put by relatives who are themselves present at a sitting their deceased friend does not invariably reply. It may be that he prefers to continue with his prepared material rather than risk forgetting to give it later; or it may be that his memory has different associations with the subject and he replies in words which at first seem inaccurate, but are later discovered to be a true reply, although not the one expected by the questioner.

On the general subject of replies to questions there is a further possibility too important to be overlooked. It is that the communicator may have forgotten the incident and may even deny that it ever happened. Such blanks in memory certainly occur in our present life. I recollect visiting Bideford and being taken by my friend Dr A. T. Shearman to see what was locally termed a "paint mine". This remained a vivid memory owing to my interest in geology. Thirty years later I incidentally asked Dr Shearman (whose interests were classical studies and psychology) if he had visited this paint mine since those days. To my astonishment he disclaimed any knowledge of it and I failed to evoke in him any recollection of the occasion. Again, I listened to two ladies who had both known Sandown, I.O.W., in their youth. In comparing their recollections of the place one recalled a military incident, the other a tragedy which had occurred there; but neither remembered the outstanding event which the other so strongly associated with the place.

In my opinion no strong argument can be based on the failure to answer this question about the two colleagues.

The Communicator had knowledge of three facts which were unknown to Mrs Lewis.

I. GIBBIE

Sitting 2, *paragraph* 29. *July* 24, 1936.

FEDA : I get a feeling of another lady, not his daughter, closely related to him, I feel it is rather like a sister, and she is on the earth. He is helping—not her exactly—but he is helping about her, something he wants to do in connection with her, influence her and help her, and he will do it. He would like you to tell his daughter ; he seems to think she will know what he means.

(Mrs Lewis wrote to Professor Dodds, " This strongly suggests reference to a particular situation that is causing me great anxiety at the moment.")

Sitting 3, *paragraph* 45. *August* 21, 1936.

I have the following message as suggested by Professor Dodds :

C. D. T. : Your daughter thinks that the lady of whom you spoke is Gibbie. She is very much worried about the Gibbie situation and can you advise her about this?

FEDA : Yes, yes, it is Gibbie. Gibbie is not making it very easy for anyone to help. It is not just the situation around Gibbie that is difficult, but Gibbie is not making it easy, is not helping others to help. He doesn't want his daughter to try and do anything just at the moment. He has a very strong impression—and here he would like to be careful and say he has the impression that in about a fortnight from now there will be an easing of the situation, an opportunity for his daughter to see if something can be done, but not to move till then. There are some difficult conditions round Gibbie, not her own entirely, but somebody's difficult condition, something that has to be fought down.

(Since the sitting was held I have heard news which makes it seem that the difficult situation, which has existed unchanged for nearly two years, may be eased within the next week or two. The phrase " something that has to be fought down " is most appropriate.)

(In a later letter of October 6, 1936, Mrs Lewis added— " You may remember that I asked whether I could do anything about the Gibbie situation and was told to leave things alone as there were signs that it was going to improve by itself within the next few weeks. Frankly

I did not think this possible, but actually the miracle has come to pass.)

Full details were given in substantiation. Having read them I can vouch for the relevancy of the allusions and the inside knowledge of family matters they imply. The name Gibbie was purposely given by Mrs Lewis in order that I might introduce it at the sitting; there remains therefore no uncertainty as to the person referred to in the subsequent messages.

We thus have the significant fact that, at the third sitting, the communicator expressed the opinion that a certain situation would shortly change for the better. This was regarded as impossible by Mrs Lewis, but seven weeks after the forecast was made she wrote of it that "the miracle has come to pass". We may therefore conclude that *the communicator knew something which his daughter did not know;* his forecast being based on information to which Mrs Lewis had no access at the time it was given. It neatly evades the telepathic hypothesis; for to suggest that Mrs Lewis subconsciously knew the facts and drew the inference would be to credit her with imaginary powers for which there is no warrant in anything known about telepathy, whether spontaneous or experimental.

II. Disappointment and Uncertainty which will end Satisfactorily

Sitting 2, *paragraph* 43. *July* 24, 1936.

Feda: Her father says that his daughter has been rather disappointed about something not coming to a head for her husband. There is something being kept back or delayed for him and about him, as if they are being kept in the dark a little, so that she wonders what is going to happen. It is rather an important thing for them and he thinks it will be all right. It is only a delay and it will work itself out.

(Mrs Lewis wrote on November 3, 1937—This is so personal that I really cannot expand it, except to say that it exactly described my feelings at the time of the sitting. Now the situation has, within the last six weeks, absolutely "worked itself out".)

The communicator's statement indicates that he had knowledge of contemporary events or mental processes *of which his daughter knew nothing.*

Disappointment and uncertainty are so frequent in the life of most

people that such references are worthless as evidence unless accompanied by definite statements. In the above instance these were included, viz. (1) For her husband, (2) something kept back or delayed, (3) about which they are kept in the dark, (4) and wondering, (5) it will be all right, it is only a delay.

The matter being private we cannot judge for ourselves exactly how pertinent to the facts this forecast may have been. But Mrs Lewis has recorded her opinion quite clearly.

III. Glad Tidings

Sitting 5, *paragraph* 92. *January* 22, 1937.

Feda: I don't know what "glad tidings" means, but he is bringing in these words. Something for you to say to his daughter, "glad tidings". He wants it put that way and it seems to lift me up a little. It is pleasant and happy and will affect the conditions that Mr Macaulay is helping to bring about from the other side. He wants her to know that he is trying to help.

C. D. T.: Your daughter wished to know if you would like to say anything about her recent visit to Ireland.

Feda: Yes, yes, isn't it funny? He is a bit linking that up with what he has just been saying.

C. D. T.: I don't know anything about it.

Feda: He is glad that you don't know. But now that you have brought up the subject he says, "That has some connection with what I have been telling you and which I entitled 'glad tidings'."

Mrs Lewis wrote—"shortly after the date of this sitting I received news which was real "glad tidings"; namely that my favourite brother, who has been in Canada for ten years, was at last in a position to return. He actually took the decision on Christmas Day 1936 and came back in March 1937. I heard of it in February 1937.

"One reason why my father so wanted my brother home was that he had a son. My father used to say, 'I don't want the Macaulays to become a Canadian family. If Hugh (the brother) were here, young Dick (the son) might one day retrieve the family fortune and buy back Ben Neagh' (the Irish house)."

Here we see a further instance in which the communicator had obtained information which was unknown to his daughter. There was, as he distinctly stated, a connection between "glad tidings"

and Ireland. Tidings shortly arrived which held possibility of the fulfilment of his remembered hope that the grandson might some day buy back their ancestral home in Ireland.

Thus the message was presently understood and, as I think, correctly, notwithstanding that certain phrases, such as "some connection with", "a bit of linking up with", "will affect the conditions" were vague. In general the use of terms like these is exasperating, because they merely hint at a possible meaning undefined. In such cases the communicator's thought may have been clear, but the Control's transmission of it infelicitous. If a sitter finds it exasperatingly vague, how much more must the communicator, that is if he realises the form in which his thoughts are being translated. He may well feel as one who, wishing to express himself in a foreign tongue, is compelled by limited vocabulary to circumlocutions which but partially and imperfectly represent what he desires to say.

In the above three instances we find that the communicator was aware of matters which were quite unknown to his daughter. This tells against the hypothesis that these communications are attributable to telepathic activity on the part of Mrs Lewis. Her own opinion is seen in the following letter :

"I do not think that I am mentally specially accessible. I have sat with two mediums, both supposed to be excellent, who gave me nothing. I was alone with them on every occasion. When I have had good results my husband has been present ; so the inference would be that the telepathy, if any, was with him. As an argument against the telepathic theory I may remark that, in those good sittings, most of the material given was quite outside my husband's knowledge."

These three forecasts are proved, by their fulfilment, to have been based on information about thoughts or purposes, which were at the time in some mind or minds of people other than Mrs Lewis. This ability to notice active thought in the mind of some person or persons which will ultimately affect the sitter, although entirely outside his present awareness, has been shown from time to time by my chief communicators. The results warranted the forecasts based on that asserted knowledge. Are we to reject this claim of the communicators? If so, what is the alternative explanation? It would be that the medium possessed the ability which, in the sittings, is attributed to the communicators. On considering the implications of this hypothesis we find that it assumes the possession by the medium of a faculty which lies beyond anything attributable to brain and mind. Such transcended faculty suggests the operation

of something which loss of the physical body would leave unimpaired, an activity of the soul acting independently of the body. If that can be achieved during earth life it is little surprising to find it being done posthumously.

I have deemed it necessary to deal at some length with this question of telepathy because of the tendency in some minds to suppose that, even in successful proxy sittings, it is preferable to refer the given information to some distant mind on earth rather than to the ostensible discarnate speaker.

Third Part of the Experiment

The First Mrs Lewis

In the letter from Professor Dodds, dated December 2, 1936, which suggested inquiry about an Irish visit, it was also asked if I would try to obtain messages from the first wife of Professor Lewis. The letter ran thus :

" Professor Lewis would like to try, if there is ever an opportunity, whether a communication can be obtained through Mrs Leonard from his first wife. This might prove, I think, of some evidential value, as the present Mrs Lewis knows (I am told) exceedingly little about her predecessor, and telepathy between Mrs Lewis (herself psychic) and Mrs Leonard would thus virtually be ruled out, in a way in which it is not ruled out in the Lewis-Leonard experiments up to date."

On seeing a copy of the above, Professor Lewis wrote me that his present wife knew " practically nothing of her predecessor and had never consciously seen her ".

In the course of the fifth sitting on January 22, 1937, Feda had said, " He is very anxious for me to describe someone he has with him." She then gave ten details of face, form and dress. Commenting on this Professor Lewis later wrote, " This is a very fair description of my first wife as regards stature, shape of face, colouring and style of hairdressing." Having at that time no suspicion that this referred to the first Mrs Lewis, and with the above request in mind, I presently took opportunity to ask if Mr Macaulay would bring the first Mrs Lewis. He agreed to do so.

Other interests occupied me during the next four sittings and it was not until just before my sitting of March 5, 1937, that I mentally asked the first Mrs Lewis to be present and give messages for her husband. Three months had elapsed since she had been mentioned in Mrs Leonard's presence, yet she was now introduced as follows :

FEDA : Oh, what does she want?
C. D. T. : Have they brought a lady?
FEDA : Yes, I think this is a case.
C. D. T. : I have asked for the first wife of Professor Lewis. Does this lady know him?
FEDA : I think this is the one.

From the items given by this communicator the following were the best. (The annotations are given by Professor Lewis.)

Date of sitting : March 5, 1937.

FEDA : 6/111 Something important happened about twenty years ago, a link with them both, it caused them to be together. (We were married in 1918.)

I ascertained later that the marriage was in June 1918 ; a date only fifteen months short of twenty years before this sitting.—C. D. T.

FEDA : 6/136 The name Platt is a link with the old days, not important but connected with a time when she was on earth. (A Mr Platt, known to her, was in College with me in 1910–13.)

FEDA : 6/137 She mentions this because her husband has quite recently been reading something in which the name Platt figures very prominently, but no connection with the Platt of long ago. (Another Platt, not known to her, but known to me and a later student at the same college, which she knew well, has written a book on geological maps. I have been recently preparing lectures involving reference to that book.)

Date of sitting : April 2, 1937.

FEDA : 7/141 She has very happy memories in connection with him (i.e. Professor Lewis) and a place " S ", quite away from his present locality. A place sounding like Soam . . . Sum . . . I get an " M " sound in it. I had better not try to get more than the " S ". (We became engaged at a small village called Swindon outside Cheltenham.)

Date of sitting : June 18. 1937.

FEDA : 8/147 I am going back to a bridge, a good many years ago, in the evening, in April, the name William comes much in memory, a special time and circumstances that were important.

(I proposed to my first wife on a bridge, on an April evening, twenty-five years ago. There were certain difficulties connected with her father which delayed her acceptance. and the father's name was William.)

It will be noted that the above refers to five facts all of which were closely connected with a single occasion, and all are recognised as correct.

Notwithstanding the successes quoted above. the whole result was poor.

Analysis of Results

Items given by Mrs Lewis in three sittings :

Right	14	Poor	0	Number of Items	47
Good	4	Doubtful	19		
Fair	6	Wrong	4	Successes, 51 per cent.	
	24		23		

It is evident at a glance that this communicator was much less successful than the former. The impression conveyed by the full records is even more unfavourable to the second communicator than these figures show. Comparing them for quality of successes, and making deductions for each failure, the superiority of Macaulay over Lewis is in the proportion of 6 to 1.

In order to make this comparison I take the same number of items, viz., the first 47 of each, and calculate the percentage.

Macaulay's first 47 obtained 74·4 per cent. successes.

Mrs Lewis's first 47 obtained 51 per cent. successes.

But this does not reveal the wide difference in *quality* of the two results. To obtain this I devised the following method :

For each " Right " add 2 ; for each " Good " add one.

For each " Wrong " subtract 2 : for each " Doubtful " subtract one.

Thus—Macaulay—Right	18	18 and 36 are 54			
Good	5	5 and 5 are 10	Total	76	
Fair	12	12			
Poor	1	1			
Doubtful	9	9 and 9 are 18	Total	25	
Wrong	2	2 and 4 are 6			
				51	

Lewis—	Right - 14	14 and 28 are 42		
	Good - 4	4 and 4 are 8	Total -	56
	Fair - 6	6		
	Poor - 0	0		
	Doubtful 19	19 and 19 are 38	Total -	50
	Wrong - 4	4 and 8 are 12		—
				6

Treating the records by this method the 47 items yield figures which seem to express more accurately the difference in quality of the two sets. Mrs Lewis with 6 is seen to be not quite one-ninth so successful as Mr Macaulay.

How are we to account for this disparity? Conditions should have favoured the second communicator; for by the time her three sittings commenced a considerable correspondence had passed between me and the applicants.

I think we may assume that the evidence points to two distinct communicators. Had these purported communicators been fictions of the medium's subconscious, and had the information been obtained by her own excursive or telepathic faculties, there should have been no falling off in the result. But the fall was sudden and decisive.

In comparing my numerous records of what different communicators have given through Feda it is seen that some are more concise than others. The record of Mr Macaulay's first 47 evidential items occupies 207 lines, while that of Mrs Lewis's 47 occupies 278 lines; the average for each item being 4·4 and 5·9 lines respectively.

This again favours the assumption that different minds originated the two sets of messages, the same stenographer having made and typed these verbatim records.

Final Remarks on the Whole Series of Sittings

It may be objected that the facts given by the Macaulay communicator as evidence of his identity were known to his daughter and therefore open to the suspicion that they were obtained by the medium from her memory. To this there are three answers:

1. The communicator would naturally wish to select from his memories such facts as his daughter could verify; for the giving of unverifiable evidence would be pointless.

2. The Gibbie forecast revealed knowledge of fact which was unknown to his daughter. Two other forecasts did likewise, although these are perhaps less convincing than the former.

3. No fewer than 22 statements are marked doubtful because

Mrs Lewis had no means of ascertaining whether or not they were true. Some of these may have represented information not shared by her.

The final refuge of the sceptic is the hypothesis that no statement need be attributed to disearnate intelligence if it refers to anything known at the time by anyone anywhere on earth.

As to this, let those believe it who can!

The first four Macaulay communications were on a high grade of evidential value. No relative of the deceased approached me : the experiment was arranged by an intermediary who was careful to give only the necessary clues.

The proportion of accurate statements puts the result outside the scope of chance coincidence. The nature of the statements is also signifieant ; for they chiefly related to memories held in common by the deceased and the applicants. Not only were these suitably selected, but three of them indicated an awareness of facts which the daughter did not know till later. Telepathy from her is thereby discredited. Is there any logical alternative to acknowledging that her father was the eommunieator?

Previous proxy cases have shown similar instances where a communicator revealed knowledge which was unknown to the applicant. One such was " the pipes " incident reeorded in the Bobby Newlove case. (*Proc. S.P.R.*, Vol. XLIII, 1935, pp. 481–483.)

As is well known sitters often receive information unknown to them at the time ; proxy sittings prove that the same happens when a person taking the sitting is unaequainted with either the communieator or his relatives. We may therefore conelude that, should a communicator know something which his relatives do not know, the absence of the latter does not afford his ability to transmit that information through a medium.

The hypothesis that in these cases the information is derived telepathically from some distant person should not be easily assumed ; especially when, as is usually the case, the distant person who knew the fact in question has never heard of either sitter or medium. What, one may ask, is the supposed prompting which could start this telepathic action? Whose mind is it which makes so judicious a seleetion of the appropriate time and message contents?

If the knowledge emerging in these proxy sittings is attributable to a purely human faculty, then that faculty differs from the telepathy which manifests in spontaneous and experimental cases. If human, it is *sui generis* ; but I know of no adequate reason for supposing it to be a human faculty. The evidence points in another direction.

In the third experiment messages from the first Mrs Lewis were less successful, despite the fact that she had given good evidence to her husband at earlier sittings with other mediums.

Before the date of this third experiment I had learnt a little about the present Mrs Lewis and her father ; for I had seen her annotations to the Macaulay communications. Imaginative critics have suggested that if I see an applicant's handwriting I have thereby established a link which facilitates the transmission of ideas from him to me or to the medium. The result of the third experiment does not bear out this suggestion ; for the success was strikingly less.

It seems significant that Mr Macaulay should have produced a much higher proportion of success than did the first Mrs Lewis. Why should this have been? The conditions were the same, excepting that a different communicator had been asked for and the material given had to be annotated by Professor Lewis and not by his wife. It may be granted that annotators differ in their estimate of what constitutes evidence. Let us assume that Professor Lewis was more exacting than Mrs Lewis ; would this account for the striking difference in our numerical findings for the two communicators? I greatly doubt it.

That communicators in proxy sittings reveal a wide difference in ability to select and transmit appropriate evidence I had previously noticed while examining some fifty proxy cases. The same wide difference was a conspicuous feature in Book Tests.

Granted a good medium and a sympathetically understanding sitter the result would seem to depend entirely upon the ability and perseverance of the communicator.

From Mrs Lewis I learn that her father had proved himself an excellent communicator in sittings which she took shortly after his death. He then gave accurate and detailed evidence of identity with but a small percentage of error. This is confirmed by Professor Dodds who has examined those records. Moreover, Mr. Macaulay was " endlessly patient with children and immature minds ", and this is an important point ; for, if the process of communication be as difficult as Controls and communicators assert, much patience and persistent effort are essential for the best results.

Professor Lewis tells me that his first wife was " most painstaking and precise ", also " able to combine precis and clarity ". He thinks, however, that she might be disinclined to help a control who did not easily grasp her meaning, since it was not her habit to " suffer fools gladly ". Now it is often noticed that communicators reveal traits of their earth-life character : hence it may be that Mrs Lewis,

even if noticing that much of her material was being distorted, would not trouble to elaborate messages incorrectly transmitted, but hoped that some at least might get through. So does an inexpert golfer continue driving his balls into the water-hazard trusting that presently one will reach the other side.

Here is an instance of such refusal "to be bothered explaining". It relates to a regular communicator who gave first class evidence of identity at many sittings. I rarely failed to recognise her statements, although a few of them were obscured in transmission and only explained at later dates. On this occasion she gave through Feda thus:

FEDA: Ash—Ash, a name.
C. D. T.: Was it one of the maids?
FEDA: No, not in the house or the family, but one very much connected with the house.
C. D. T.: Sorry, but I do not trace that.
FEDA: Leave it. She is not one who would care to be bothered explaining.

Months later it occurred to me that the name, given by Feda as Ash, was intended for Nash. This explanation was accepted at a subsequent sitting and followed by considerable information about this Mrs Nash, some of which I knew to be correct, and some of which I was able to verify afterwards.

Here then was an instance of a name being wrongly given and the communicator either unable, or not troubling, to correct the mistake.

Mrs Lewis, like the above, may have been unwilling "to be bothered explaining" these messages which failed to get through correctly at a first attempt.

A further point of interest is that Professor Lewis, for some time after his wife's death, had excellent sittings, but as time passed the quality of her communications diminished. In my twenty-two years of investigation I have noticed with some communicators a similar decline; they did best in the first year or so, then their interest in providing evidence seemed to slacken. It may be that they became engrossed in activities of the new life and no longer felt strong interest in earthly contacts. This diminution of interest is by no means universal; some of my very best communicators left earth more than twenty years ago. Much would seem to depend on strength of affection or the conviction that mediumistic intercourse with us can be of benefit.

I was happy to accede to the suggestion by Professor Dodds that

this experiment should be undertaken: for he and I represent two divergent opinions held by members of the S.P.R. These are (1) the ultra cautious, which relies on the hypothetically unlimited scope of telepathy between persons widely separated and even unknown to each other. And (2) the whole-hearted conviction of the actuality of intercourse with those departed this life.

We agree on the possibility of telepathy between minds on earth. Our differences would seem to hinge on the dubious extension of such telepathy; is it practically unlimited, so that it can be assumed as a *vera causa* for everything met with in mediumistic communications? Or do the latter sometimes go beyond anything explicable by telepathy?

I am confident that they do. This conviction is based on oft repeated and varied observations. Of these the proxy sittings form a part. One such case was reported in the *S.P.R. Proceedings*, Vol. XLIII, 1935, entitled "A Proxy Case extending over eleven sittings". The present case is a degree further removed from explanation by telepathy because of results obtained under conditions entirely excluding contact between sitter and applicants.

Note by Professor E. R. Dodds

Mr Drayton Thomas has asked me to state my personal view of the Macaulay-Lewis experiment, as I was instrumental in arranging it and have examined the whole of the material fairly closely.

It appears to me that the hypotheses of fraud, rational inference from disclosed facts, telepathy from the actual sitter, and coincidence cannot either singly or in combination account for the results obtained. Only the barest information was supplied to sitter and medium, and that through an indirect channel. Until after the first sitting (at which a substantial amount of veridical matter emerged) the medium had no opportunity of initiating enquiries; and although she might then conceivably have had recourse to obituary notices and to a private enquiry agency, I cannot imagine how she could have obtained such items as "Reece-Riss" (47), "Puggy" (79) or the reference to Professor Lewis's private conversation on a bridge 25 years earlier (147). It is equally incredible to me that such items were all of them mere lucky shots.

If these hypotheses are ruled out, the experiment seems to present us (and this is its importance) with a clear-cut "either-or": Mrs Leonard had supernormal access on this occasion *either* (*a*) to some of the thoughts of a living person or persons who had never held any

communication with her *or with the sitter* ; or *else* (*b*) to some of the thoughts of a mind or minds other than that of a living person.[1]

I see at present no plausible means of escape from this staggering dilemma. Nor do I see any valid ground for embracing one horn of it and spurning the other, as Mr Thomas does. In the present state of our knowledge—or rather ignorance—about the mechanism of telepathy, it seems to me impossible to specify the limits of its operation, though no doubt such limits exist and will one day be determined. In the meantime I can only state my conclusion in the form of a disjunctive proposition.

Three remarks, however, seem worth making, in answer to certain arguments of Mr Thomas's :

(*a*) All the veridical statements about the past or the present attributed by Feda to " F. W. Macaulay " concerned matters within the knowledge of the present Mrs Lewis : all those attributed to " the first Mrs Lewis " concerned matters within the knowledge of Professor Lewis. (Three statements about the future, attributed to " F. W. Macaulay ", Nos. 43, 45, 92, are claimed by Mr Thomas as veridical (see above, pp. 283 ff.) ; but they are unfortunately so vague that with a little goodwill I could read all three of them as referring to recent incidents in my own life. Such optimistic babblings are a stand-by with all mediums : to build any argument on them seems to me more than rash.)

(*b*) Between the " Macaulay " and the " Lewis " sittings there were thus *two* changes : a change of " communicator ", and also a change in the person who was able to verify the communications and must on the telepathic hypothesis be regarded as their ultimate source. On this hypothesis the qualitative inferiority of the " Lewis " communications might simply mean that Professor Lewis proved a less effective telepathic agent than his wife (as I should in fact expect from my knowledge of them).

(*c*) I cannot agree with Mr Thomas that " there is nothing in the whole experiment to support the suggestion that the Macaulay communications owed anything to telepathy from minds on earth ". Apart from the negative fact that " F. W. Macaulay " made no verifiable reference to matters outside his daughter's knowledge, there is his positive awareness of his daughter's recent movements (96–99) and even (it would seem) of her recent thoughts (43, 98).

[1] I put the second alternative in this negative way because I have no means of defining the character or status of such minds, if they exist, or of determining how many such minds might, singly or between them, possess the veridical information which was given. Even the use of the word " mind " perhaps assumes more than is strictly justified.

" F. W. Macaulay " stated that he was " with her " (whatever that means) on the day described in items 98–9, and I am in no position to dispute the assertion ; but in such a case the telepathic hypothesis has at least the relative merit of postulating one miracle instead of two.

It is, I think, worth adding that I made an earlier attempt, on behalf of a different enquirer, to obtain messages through Mrs Leonard without any communication either between the enquirer and Mrs Leonard or between the enquirer and the actual sitter (in this case Miss Nea Walker). In 1934 Miss Walker kindly consented at my request to take a sitting on behalf of an *unnamed* friend of mine who wished to receive a message from an *unnamed* near relative who had been an active member of the S.P.R. and had believed in survival. No other information was supplied to her. The resultant sitting (date of which was not known in advance either to me or to the enquirer) yielded a few relevant statements, but nothing which either in the enquirer's judgment or in mine exceeded the normal range of coincidence. The failure may be variously explained : without further experiment it would be rash to conclude that it is an indispensable condition of success in proxy sittings for the actual sitter to know the names of the enquirer and the desired communicator. I should like to see other experiments tried on these lines.

May I urge, in conclusion, that serious students of psychical research should acquaint themselves with the complete annotated record of the Macaulay and Lewis sittings, which may be seen at the Society's Rooms?

APPENDIX I

Characteristics of the Messages Marked " Doubtful "

More than half of these were attempts at personal names ; those accustomed to sit with Mrs Leonard will agree that Feda finds names difficult and that they are frequently given in distorted form. To listen while she struggles for the correct name is exactly like hearing a rather deaf person try to repeat some difficult word which is but half heard.

The following examples will suffice to indicate the character of these " doubtful " name messages.

Feda : Foreman . . . Foreman. No, that is not right. A name sounding For-something, like Forcman. It is awful like

4/30 Foreman. For- For, just leave it for the moment. I am afraid to stop too long.
(Foreman not identified.)

FEDA: 1/14 Would there be a name Abby, Abby? (This was not proceeded with and is not identified.)

FEDA: 1/17 He is trying to get another name now beginning with a "G". I feel this is a lady's name, there is a feminine feeling. Somebody he was very interested in.
(This cannot be traced.)

FEDA: 2/28 Willow, Willow, Willow. It must be the name of a place. It is something he has been thinking about lately. His daughter? Has he a sister do you know? I get a feeling of another lady, and it is not his daughter, closely related to him. I feel it is rather like a sister and she is on the earth.
(I wish Feda could be pressed about "Willow". Taken in conjunction with the next paragraph it suggests an attempt at a place name which, if correctly given, I should consider to be evidential. The place name is Ludlow.)

FEDA: 5/90 (After giving a description which Professor Lewis recognised as a very fair description of my first wife as regards stature, shape of face, colouring and style of hairdressing.) A name comes to her Ellen, Nellie; she may be speaking of a relative.
(Professor Lewis continued: "I know of no Ellen or Nellie likely to be closely associated with her. A grandmother of mine, now passed over some years, knew her and was fond of her, but their contacts were so few and short, a total of hours in all at long intervals, that this seems unreliable as evidence, even though the grandmother's name was Ellen.")

Other of these "doubtful" items were as follows:

FEDA: 2/40 Batten, Button. Just put it down. Not long before he passed over he had been wanting to loosen something, he calls it a batten; something he wanted to do and he remembers speaking about this batten, something to do with his work. He wanted it done specially. And hooks to put things on, hooks, hooks, hanging things on hooks. Not ordinary clothes places, but as if he got things and hung them up in places.
(I have heard this word in general engineering talk, but cannot recollect any particular occasion.)

3/47 In the midst of a reference to happy times in the past,

which is recognised as correct, three names were mentioned as being now with him, John, Harry and Francis.
(John, Harry and Francis could be three of these. Francis is certainly dead, I do not know about John and Harry. In view of this uncertainty both John and Harry are counted among the " doubtful " references.)

FEDA : 4/62 Oh, does he hold something to his eye? Not an ordinary glass, but something he holds up right close to his eye like looking down a peep-hole. This is like a sort of little tunnel that he puts to his eye.
(He frequently used a magnifying glass, but I do not recollect any " sort of little tunnel ". It may be a confusion with his telescope. This he used a great deal on the rifle range. In a sitting with another medium I was told that a telescope was very significant in connection with him.)

FEDA : 4/80 He went up a hill somewhere, a place he was interested in. He just showed me a little quick picture and said it was a place he was very interested in.
(Too vague, though I can add that all my father's reservoirs, excepting one covered storage tank, were made by damming valleys in hilly country.)

FEDA : 5/105 I get shipping too. I don't feel he had much to do with that, but he is giving me the place.
(He was always interested in shipping, as his father was a naval man.)

Eight items only seem to be definitely inaccurate. They are as follows :

1/25 A Description of the office in which he worked contained nine points ; of these all were correct save the two here given.

FEDA : He gives me the idea of a smell that I should not like, that would make me cough, dangerous to stay in it too long, as if it would make one unconscious. It is in this room near the wall, about 3 feet up. Both the men he worked with seem to be on earth still.
(The " smell " is completely wrong. The two men I had in mind when asking the question are both dead. Of his many other colleagues I cannot speak.)

Date of sitting, July 24, 1936.

FEDA : She was thinking very much of him last week, something had been happening last week that reminded her of him

3/34 in his early life. Some kind of anniversary in last week, some date that had come round again that she was thinking of and that was connected with him in his early life. August is rather important for him. There seems to be another kind of anniversary, a rather important kind, in August.

(I cannot recall any particular anniversaries in July or August, excepting that I always think of him during the Bisley Meetings, because he so enjoyed them ; and again think of watching cricket matches with him when I was on holiday during those two months. But then most things remind me of him.)

FEDA : An elderly woman with him who belongs to his daughter's husband. Did you say her name was Emma? There is
2/44 an Emma with him. I get Mary again now quickly, as if there must have been a link between them.

(If Emma is the woman connected with my husband we don't know her. But my aunt of that name, who was a Spiritualist, died recently. I don't know a Mary in connection with her.)

4/61 An instrument is described as one used by Mr Macaulay in his work. This is recognised by his daughter, except for the following point :

FEDA : He seems to put on it glasses and bits of metal. Something with glasses he used.

(Glass played no part in the process. He frequently used a magnifying glass.)

FEDA : It was rather an interesting time when she was in Ireland,
5/94 not that she was thinking of him while there.

(I was thinking constantly of him as we were visiting a village where the Macaulays had lived for three hundred years.)

It is obvious that the remark " not that she was thinking of him while there " was an error, either in transmission or by the stenographer ; for in 5/98 Feda said, " He felt she was in the midst of trams and doing something that *made her think of him.*"

FEDA : Half-past eleven in the morning I was with her, a special time ; she will know why. Interview—gathering—seeing
5/100 people later in the day and again next morning by arrangement. Talking about a man passed over as well as of himself.

(This is not recognised.)

APPENDIX II

Coincidence of Allusion between Leonard Proxy Sittings of C. D. Thomas in 1936–37 and Sittings taken by Mrs Lewis in 1933 with a different Medium

			Number of Paragraph in Leonard Records
Water and Reservoirs	Abbott and Leonard		5, 6, 7, 54–5
Sister Anna	Brittain and Leonard		75–7
Margaret and Sarah	,,	,,	18, 77
Very Tired before Passing	,,	,,	11 *b*
Reference to Son-in-law	,	,,	85
Telescope	,,	,,	62
Has met the first Mrs Lewis there	,,	,,	89 *d*, 100
Warnings about Health Condition	,,	,,	11, 12
Active to the Last	,,	,,	3, 65
Attitude to Post Mortem Communication	,,	,,	38

Macaulay

Abbott. *December* 15, 1933. Professor and Mrs Lewis Sitters.

Water and Reservoirs

"The old gentleman says that the bee in his bonnet is all about water. Water, water, so that there shall be no shortage."

"Birmingham, big things are going to be done for the place that he loves. They are going to store up the waters, and make it practical. They went nearly thirsty with you a few months ago. Why can't they push forward with his pet theory?"

(True of all England. "Pet theory" must be his idea of a National Water Board, and a division of England into "Water regions".)

Leonard. 1/5, 6, 7.

"Big stones built—the work he was doing. Shows "M" over the stones, a link with his work, also "H" connected with it too."

(Suggests his reservoir in Monmouthshire and Hydraulic Engineering.)

4/54–5 " Interested in some work ' R ', almost up to the time he passed over. Left mass of handwritten notes about this name ' R '."

(Yes, notes about Reservoir work.)

Sister Anna

Brittain. *June* 22, 1933.

Mr Macaulay had died four weeks previously, May 20, 1933.

Mrs Lewis writes : " I went in a distinctly antagonistic mood, as my father had always been very critical of all spiritualistic matters."

Brittain : " A little sister who died very young, the Daddy's. She is grown up, now with Daddy : It is Anne."

(My father had one sister who died at the age of three. The child's name was Anna.)

Leonard : " I am getting the name Anne too, Anne or Annie, is
4/75–7 passed over."

Feda then described with much detail an old lady " who passed years ago ", and " very near her " a Sarah, also described.

(This might be his sister Anna, especially as he goes on to describe (76) someone who might be his grandmother, for whom this child is named. The suppositions made above are strengthened by the immediate mention of Sarah—his mother's name and the description applies well to Sarah.)

Leonard : " I see standing with him there a rather young lady, I
3/48 think she passed over quite young some time ago. Will you ask if he had a sister a great many years younger than he was? "

(Anna, his only sister, died as a child.)

Margaret and Sarah

Brittain. *June* 23, 1933.

" Margaret and John are with him, so is his mother. Her name is Sarah."

(This is correct. In regard to Margaret and John these names conveyed nothing to me ; but some weeks later,

my sister, to whom I sent the notes of the sitting, wrote to say that she had been reading up an old family book and found that these were the names of Sarah's brother and sister. All three had died before my father was sixteen and he rarely mentioned his uncle and aunt. When he spoke of the latter he always called her Aunt Mary.)

LEONARD. 1/18, "*Margaret*"; *and* 4/77 "*Sarah*".

LEONARD : "Among names of those now with him given for identi-
1/18 fication" is Margaret.
4/77 Sarah named and described, together with old lady whose description fits her mother; thus mother (Sarah) and grandmother spoken of as with him now.

VERY TIRED

BRITTAIN (*continued*).

BELLE : "Very happy, happier than he has ever been. He was so tired, so very tired. It was rather a sad end to his overcoat—he means his body, but now he doesn't want to see it any more."

LEONARD : "My heart seemed very tired, something affected it."
1/11 *b*

SON-IN-LAW

BRITTAIN.

BELLE : "He is asking about Stanley now. He means the gentleman, but he doesn't say Wilfred like the lady does. He says Stanley—Stanley."
(I called my husband Stanley when we were first engaged and my father had hardly got used to calling him Wilfred when he died.)

LEONARD, 5/85. Following indirect allusion to his son-in-law, Professor Lewis :
"He gives the letter 'S' to do with that person : it may not be the proper name but yet quite fitting, etc."

TELESCOPE

BRITTAIN (*continued*).

BELLE : "Now he will show something very important so that you will know it is really him. It is a black thing—a telescope."

Mrs Lewis : I don't recognise that.

Belle : " He says ' doesn't it remind you? ' Is it a flute? He is shaking his head. In a green baize bag. He says Marian." (This is a very curious episode. The control kept insisting that the telescope was very important and I could not place it at all. After about five minutes the matter was given up, the control saying that Daddy was very surprised. When I read over the notes afterwards the name Marian recalled my father's stepmother and led me to wonder whether he had a telescope that had belonged to his father, who was a naval man. Three months later I asked my mother if this was so. She said she believed there was one somewhere but it was not with Daddy's telescope. I said, " Had he one then? " She looked at me as if I were mad, and then, for the first time, I remembered that my father was, until about five years before his death, a most enthusiastic rifle shot. He went to Bisley every year and on two occasions got into the King's Hundred. We have at least twelve cups that were won at Bisley and elsewhere. I can't understand this complete lapse of memory on my part. He used the telescope on the range.)

Leonard : " Does he hold something to his eye? Not an ordinary
4/62 glass, but something he held up right close to his eye, like looking down a peep hole—sort of little tunnel."

Mr Macaulay has met the First Mrs Lewis there

Brittain (*continued*).

Prof. Lewis : " Is Connie still there? " (The name of his first wife.)

Belle : " Oh yes, rather, but she doesn't want to barge in and be a nuisance." (A very characteristic expression of Connie's.)

Mrs Lewis : Has she met Daddy?

Belle : Oh yes, rather. She's been with him quite a lot, etc., etc.

Leonard : Description of a lady recognised by Professor Lewis as
5/89 *d* very fair for his " first wife as regards stature, shape of
& 100 face, colouring and hair-dressing ". This was prefaced by : " He is very anxious for me to describe someone he has with him," and concludes " She is with him ".

100 Unaware of the identity of the described lady, I later

asked if he would bring the first Mrs Lewis to a sitting. Reply was, " He would like to do that and I think there is a reason why he can."

Warnings

Brittain (*continued*).

Belle : Do you know he had little warnings before he went? He had a little fall, sometimes he felt faint just for a moment. When he had his bath he didn't have much water. It was his heart or perhaps blood pressure. But perhaps you didn't know that.

Mrs Lewis : He never told me anything.

Belle : No, he pretended he was all right. He never opened his silly mouth. (A very common expression with my father.)

Active to the Last

But it would not have made any difference and he went to the eve of his passing without giving any trouble, and he was able to do all his work.

(Correct, he worked until the evening of June 17, 1933, and had his operation on June 18. My father used to take his bath in the mornings.)

Leonard, 1/11 and 12, for " Warnings ", and 1/3 and 4/65 for " Active to the last ".

1/11 " He had not been well for two or three years but did not regard it seriously. Two months before the end he had disquieting symptoms that seemed serious, but it was only a short time before his passing that he realised its seriousness."

1/12 " I get a feeling of something happening early in the morning, something rather like a warning kind of thing, like—oh, now something is happening."

1/3 " I don't think he thought he was going. It is a feeling ' I have things to do and I hope I shall be allowed to live on and do them '."

4/65 " Busy up to a short time before he passed."

Re Communication after Death

Brittain, 1933.

Mrs Lewis : Has Daddy anything to say ?

BELLE : We are waiting for him. You see he didn't pass over very long ago and he isn't quite sure. He didn't believe in this sort of thing when he was on earth. He is laughing ; there is such a twinkle in his eye.
And he has such funny hair ; it is two colours—white in front and on top and very much darker at the back. And he has a very funny mouth (giggling a good deal).

PROF. LEWIS : What is funny about his mouth?

BELLE : It is all crooked, one corner goes up. (Twisting her mouth up at the right corner.)
(This description of my father's hair and mouth are quite correct. It would also be true to say that he usually had a twinkle in his eye.)

BELLE : He is very reserved.

MRS LEWIS : True.

BELLE : He says he was not quite sure about the after life, he didn't think much about it on the earth. He didn't really think there was anything.

MRS LEWIS : True.

BELLE : Mostly he thought about people. He had a lot to do for them.

LEONARD : " He had heard something about this subject before he
1/38 passed over. I don't think he knew very much but I feel he had talked about it. He knew somebody—not his daughter—with whom he had talked about it, and had learned just a little that way."
(He used to fight with Sir Oliver Lodge about Spiritualism.)

Having seen these striking coincidences let us ask what they tell us. There are two possible explanations, although one of them fails to explain the whole and is, in my opinion, wholly imaginary. It assumes that the above coincidences were due to the memory of Mrs Lewis from which, three years after the Brittain sittings, some of the recollections given there were now " read " by Mrs Leonard. That hypothesis is weakened by the fact that special precautions were taken to ensure that neither sitter nor medium should have any mental link with Mrs Lewis. We had nothing beyond the clue necessary for indicating the person from whom communication was sought ; that clue being in the handwriting of Professor Dodds and not in that of Mrs Lewis. The above hypothesis is further discredited by its failure to explain the inclusion of messages which revealed awareness of facts which were unknown to Mrs Lewis.

To me the coincidences between Brittain and Leonard sittings indicate that the Macaulay communicator from whom I received so much accurate information was not a fiction created by Mrs Leonard's mind, but an independent person who, after the lapse of three years, offered as proof of identity some of the same material which he had previously selected for the same purpose and given through other sensitives and to a different sitter.

While going to Press the suggestion was brought to my notice that information about Mr F. W. Macaulay could presumably have been found in published notices. I accordingly inspected *Who's Who* for a year previous to Mr Macaulay's decease but found no mention of his name. I next wrote to the Editors of all the Birmingham daily and weekly papers inquiring about obituary notices, and on learning that two had appeared I procured copies.

The Birmingham Mail for May 22, 1933, gives the following particulars among much else which has no bearing on Leonard sittings: Mr F. W. Macaulay died on May 20, 1933, after an operation. He was in his 69th year and had been Chief Engineer to the city Water Department and a recognised authority on water engineering. A member of the Institute of Civil Engineers he entered the city's employment on the recommendation of the late James Mansergh. The date of his retirement was 1929.

This notice includes a photograph which shows that he wore glasses.

The Birmingham Post repeats much of the above and states further than Mr Macaulay was born at Southampton, and had been associated with James Mansergh in big engineering schemes in the North of England.

While some of these facts appeared in the Leonard sittings it is obvious that they form but a minute portion of the information there given.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 160

PRESIDENTIAL ADDRESS

HAUNTING AND THE "PSYCHIC ETHER" HYPOTHESIS: WITH SOME PRELIMINARY REFLECTIONS ON THE PRESENT CONDITION AND POSSIBLE FUTURE OF PSYCHICAL RESEARCH

BY

H. H. Price

When you did me the very great honour of inviting me to be your President, I am afraid I did not reflect sufficiently upon the obligations of the office. The long series of exceedingly distinguished men who have held this position before me have set a standard to which I cannot hope to attain. Moreover, though I have had the good fortune to be a member of the Society for several years, I must confess that my knowledge of Psychical Research has been derived almost entirely from reading, and hardly at all from personal investigation. Nor has my reading been very extensive. As a professional philosopher, I am naturally interested in a subject which seems likely to throw entirely new light upon the nature of human personality and its position in the Universe. (Indeed I believe it may do more: I believe that in time it may transform the whole intellectual outlook upon which our present civilisation is based.) But I cannot claim any wide or detailed knowledge of the results hitherto achieved. So if I venture to make some suggestions which have a bearing upon the future development of the subject, you must understand that I speak as an onlooker—an outsider if you like—and you must forgive the combination of ignorance and temerity which I shall probably display.

It seems to me that there are two main obstacles which are at present holding up the progress of our enquiry. They are quite different, and it may seem at first sight that there can be no connection between them. But if I am right, there *is* after all some connection, and the removal of the one will help to remove the other as well.

The first obstacle is this. We do not yet know of any method by which supernormal phenomena can be produced and repeated at will by anyone who is prepared to take the requisite trouble. We are still too much dependent upon the occurrence of "spontaneous cases". There was of course a time when we were almost wholly dependent on them. It was as if one could only study electrical phenomena by waiting for an occasional thunderstorm. Indeed it was worse. For the phenomena (for example telepathic phantasms) were very seldom, if ever, observed directly by the investigator himself. They were reported to him by other people, and it was necessary to establish the honesty and the accuracy of the reports. With regard to a great many supernormal phenomena —and those perhaps the most interesting ones—we are still precisely in that position. It is true that we *have* developed experimental methods for investigating Telepathy and Clairvoyance, and we know how to analyse the results by means of an elaborate statistical technique. The devising of these methods was a most important step forward. Nevertheless, they do not give us all we want. Broadly speaking, they enable us to detect the presence of "extra-sensory" powers in any given person, and to measure the degree in which they are present. But if our study is to become an experimental science in the full sense of that phrase, we must be able to *produce* the phenomena whenever we like; or if you prefer, we must be able to *ensure* that they will happen. It is not enough to be able to detect and measure them when they do happen. We must be able to arrange artificially a set of conditions, given which a specified sort of phenomenon (*e.g.* the clairvoyant reading of a sealed letter) is reasonably certain to occur. We can then proceed to vary these conditions one by one, and to introduce new ones, and notice the results. Moreover we must be able to *describe* these conditions fully and unambiguously; so that any other investigator, no matter who, can repeat the process whenever he likes and verify our results.

It would be still better if the investigator could produce in himself (not merely in the other persons whom he studies) the phenomena which he desires to investigate. We need to know from within, by personal and first-hand experience, what it is like to see an

apparition in a haunted house, or to have a clairvoyant vision of an event happening at a distance. If every trained psychologist—dare I add, every trained philosopher?—could have these experiences himself, whenever he liked, under known and repeatable conditions, we might be able to find out what sort of " seeing " or " vision " it is, and how much or how little analogy it has to the normal experiences which go under these names. At present we have to depend upon the testimony of persons who are no doubt honest and intelligent, but do not know what to look for, or how to describe it. Thus we have had to invent a number of technical terms—such as " Telepathy " and " Clairvoyance "—without any really clear notion of the meaning we intend to attach to them.

This ideal which I have sketched, the ideal of a genuinely experimental science, based upon a direct and first-hand experience of the phenomena, may well appear utterly unattainable. Certainly we are a very long way from it at present. But I do not think we should despair of achieving it, and I should like to suggest one or two lines of thought which may help us to find the way.

In the first place, we might appeal to the Physiologists and Biochemists. It is not at all inconceivable that some drug may be discovered which will give us what we want ; it is well known that certain drugs, for example mescal, have the power of inducing sensory hallucinations. Indeed, the required drug may already have been discovered or artificially produced. If it has been, the fact might easily have escaped our notice. In the present state of extreme scientific specialisation, its discoverer would probably not even ask himself whether it might be useful to Psychical Researchers. (We must remember too that even to-day our subject is viewed with very considerable suspicion in scientific circles.) We have some reason to think that supernormal powers manifest themselves more easily when the normal functioning of the sense-organs is inhibited in some degree ; and I should imagine—though of course I speak in complete ignorance—that quite a number of chemical agencies might have such an inhibiting effect.

Perhaps this suggestion has already been followed up, with no positive result. If so, I apologise for making it, and I will now make another which is on somewhat similar lines. Is it not possible that prolonged *fasting* may lead to, or at least facilitate, the manifestation of supernormal cognitive powers? I suppose that the effects of fasting have been carefully studied by physiologists ; but have they ever been studied with this question in mind? It is surely significant that a number of religious traditions, Christian and non-Christian, lay great stress on the importance of fasting, and hint

very strongly that there is a close connection between fasting and "visions" of one sort or another. I need only refer you to the biographies of the Christian hermits and ascetics who flourished in fourth-century Egypt. Can we suppose that a practice which is so utterly repugnant to ordinary human nature would have been adopted so widely, and persisted in for so long, if it had not led to some pretty striking experiences? You may say that the experiences were "spiritual" rather than "psychic". But I think that the stories which have come down to us, for instance in the legend of St. Antony, suggest the contrary. When all necessary allowance has been made for hagiographical and theological enthusiasm, I think there is a residuum left which deserves our serious consideration. The Reformation did many disservices to mankind. Perhaps one of the greatest was this, that it made fasting unfashionable—not to say socially impossible—among the more scientifically minded peoples of Europe. Before leaving the subject I might mention the reports that members of the Everest expedition and other similar expeditions experienced occasional hallucinations. This might have been due to semi-starvation; but of course it might also have been due to reduced atmospheric pressure, and perhaps that is another possibility which is worth investigating. (The *physiological* effects of reduced atmosphere pressure have of course been pretty thoroughly studied already.)

Thirdly—though I am afraid this suggestion will shock some of you—I do not think that we should be too proud to take any hints we can get from the mystical and occult traditions of the Far East, particularly of India. I am not suggesting for a moment that we should accept their conclusions—unless and until we succeed in verifying them for ourselves. What I have in mind is their *methods*, the assemblage of physiological and psychological exercises which are roughly included under the name "Yoga", and the corresponding ones which are practised in China and Tibet.[1] Though some of these methods aim at inducing religious experiences of a mystical kind, there are others which profess to develop the "psychic powers" of the subject, including the powers of Telepathy and Clairvoyance; for it is assumed (not altogether unreasonably, I think) that these

[1] I think it is very much to be desired that a party of qualified Psychical Researchers should visit Tibet; for in that country the traditions which I have in mind remain in full force, almost untouched by Western influences. I know that the difficulties—financial, linguistic, and even political—would be enormous. But mountain-climbers and naturalists can manage to overcome them. New light on the human mind is far more worth having than a Giant Panda.

powers are present in every normal human being, though in a latent state. These methods may appear to us peculiar, or even repellent. Nevertheless, it is claimed that their effectiveness can be empirically verified by anyone who is prepared to take the requisite trouble. And I think that this is a claim which ought to be seriously investigated.

Before I leave this part of my subject, there is one other suggestion I have to make. It may appear naïve to the point of childishness. One has the impression—I do not know how far statistics support it—that supernormal experiences are relatively uncommon among highly educated persons. (There are of course striking exceptions : for instance Swedenborg and perhaps Socrates. But I think they are not numerous.) There is some reason for supposing that these experiences were more frequent in earlier and less civilised ages ; and that at this day they are more common among the less advanced peoples of the world than they are among ourselves. There is also some evidence that experiences of this sort are more likely to occur when intellectual processes are somewhat in abeyance : as in states of fatigue or illness or at the point of death. The appearances, then, do at least suggest that supernormal cognitive powers tend to be feeble or non-existent where the power of abstract thinking is great. Now there is a possible explanation for this, which it may be instructive to consider. The more abstract our thinking is, the more it has to be carried on by means of *words* ; or by means of other conventional symbols, for instance algebraic ones, which for our present purposes may be taken as a sort of words. But it is also possible to think by means of *images*. For highly abstract topics images are useless, but they serve well enough for relatively concrete ones ; and it is possible, and it may be relevant, that they are more closely connected with the emotional side of our nature.

Now almost the whole of our present higher education, in so far as it does not consist in imparting information, is designed to increase our capacity of *verbal* thinking (including under that head the manipulation of mathematical symbols.) And this is perfectly natural. For speaking broadly, this education has two main aims. If it is of the scientific sort, its aim is to produce scientists and scientific technicians. If it is of the humanistic sort, its aim is to produce administrators in a wide sense of the word. These are the two most important classes of persons in our present civilisation : the scientists and technicians on the one hand, the administrators on the other. And it is obvious at a glance that neither of them could carry on their work for more than a few minutes by means of image-thinking alone. Thanks to the very advanced state of our scientific technique, and the unparalleled complexity of our social

organisation, we probably live in the most *word-ridden* age that the world has ever seen. Image-thinking, on the other hand, is systematically repressed in youth by our whole system of higher education, and by the whole weight of the " educated attitude " of those among whom the adult subsequently lives and moves. Not that it is eradicated altogether ; it turns up again in moments of fatigue or disease or relaxed attention, in dreams, in the hypnagogic and hypnopompic experiences which are intermediate between sleeping and waking. It has only been more or less completely inhibited in the sane, waking, normal life of highly educated persons.

Now I want to suggest that this exclusive cultivation of verbal thinking may be adverse to the development of supernormal cognitive powers ; that such powers manifest themselves more readily in persons whose thought is mainly conducted in images (persons of the " intuitive " rather than the " abstract " type [1]) ; and that this is the reason why such powers are relatively more common among uneducated persons, children, and primitive races ; and moreover that it is the reason why they tend to emerge more easily in sleep and in states of relaxed attention or mild dissociation.

If I am right in this suggestion (which I admit is something of a shot in the dark) an important practical consequence follows. Investigators of supernormal phenomena ought to make a deliberate effort to cultivate their own powers of image-thinking. In this way they should increase their chances of obtaining first-hand supernormal experiences in their own persons. And of course they should encourage other people to do the same. But indeed the word " cultivate " perhaps gives a false and unduly disheartening impression. I think it is not really a case of *increasing* a power which one has in a feeble degree : but rather of attaining or recovering *conscious control* of a power which is already functioning abundantly. For it is plausible to suggest that image-thinking is going on in all of us all the time, but that in highly-educated persons it has got dissociated from the main stream of their daylight waking consciousness. (Cf. the suggestion that we are dreaming all day long as well as at night, but only notice it when we are asleep.) In fact, the intellectual and abstract-thinking man is something of a split personality, a little mad if you like : a conclusion which need not surprise us! As some philosopher has said, " nous vivons sur la

[1] The word " intuitive " is one which the professional philosopher dislikes, owing to the many different meanings which it has borne in the course of the history of Philosophy. But I think that in ordinary life and conversation it is sometimes applied to the image-thinking as opposed to the abstract-thinking type of person.

surface de notre être". The thing for us to do, then, is to re-associate our image-thinking with the main stream of our waking consciousness: to break down a barrier which cuts off a fully active faculty from our view, rather than to revive by toilsome exercise one which has become atrophied through long disuse. Not that even this removal of barriers is an easy task; but experience shows, I think, that it is a practicable one. And perhaps the other measures which I suggested may assist us in it. Perhaps that is the point of them, if they do have the efficacy which I tentatively attributed to them. We all know that the emergence of images into consciousness is facilitated by certain *physiological* states, such as fatigue. Perhaps the drug we are looking for is one which will induce the required physiological condition artificially; possibly abstinence from food may also favour the free emergence of images, and the breathing exercises and bodily postures of the Yogis may do the same. Perhaps this is also the point of the auto-hypnosis which occurs in crystal-gazing and similar practices. Its immediate effect may be to facilitate the emergence of images, and thus indirectly set free the subject's clairvoyant powers. I have of course been assuming throughout—and it is an old and a reasonable assumption—that supernormal cognitive powers are in fact possessed by everyone and even that they are active in everyone to some extent. I have been assuming, following F. W. H. Myers, that the difference between "sensitives" and the rest of us is just a difference in the normal position of the threshold of consciousness. If this is indeed the difference, it should not be beyond the wit of man to remove it by one means or another. That is the point of all the suggestions I have been making. If I am right, the obstacles are specially great in the case of highly-educated persons. But I see no reason why we should despair of overcoming them even there. And if once they were overcome, the way would be open for an enormous advance in the investigation of supernormal phenomena.

One more remark before I leave this part of my subject. It may well be that the supernormal powers function on some "deeper" level than the level of image-thinking. But even so, images might be their *proximate* manifestation; whereas their manifestation at the level of verbal thinking might be far less direct, and inhibited by all sorts of additional counter-forces. We may compare the fact that dreams (which are a kind of image-thinking) seem to be the proximate manifestation of our unconscious wishes; and that these wishes "get through" into ordinary waking life only occasionally and as it were furtively, in slips of the tongue, apparently accidental actions and the like.

So much then for the first main obstacle which obstructs the progress of Psychical Research at the present time: the lack of any reasonably certain method by which the phenomena may be obtained at will, and repeated, by anyone who takes the necessary trouble. I turn now to the other main obstacle which confronts us. It is equally serious, and even more obvious—indeed it is perfectly familiar to us all. Dare I suggest that it may become even too familiar? that one is in some danger (I speak for myself) of getting used to it and acquiescing in it, as in something permanent and inevitable?

The obstacle I refer to is this. We need, and have not got, a *comprehensive hypothesis* which will bind together all our phenomena, or as many of them as possible, in one unified intellectual scheme. We have now collected a very large mass of well-attested facts. Most people who have examined the evidence are now agreed that the occurrence of Telepathy, Clairvoyance and Haunting is pretty firmly established. We can say the same of the *cognitive* phenomena of Mediumship. I do not think we can say it of the *physical* phenomena at present; though here we must remember that in Poltergeist phenomena, the evidence for which is quite good and fairly abundant, we find something which is anyhow analogous to the alleged performances of "physical" or "telekinetic" mediums. Further, it seems to me that the evidence for Precognition is also fairly good. But here there is a special difficulty. Some investigators seem to think that Precognition is *logically impossible*, that the word stands for a self-contradictory concept. I do not myself take that view. But if they are right, of course no amount of evidence could establish the occurrence of Precognition, just as no amount of evidence could establish the existence of a square table which is also circular. Lastly, with regard to the "great question" of Survival we know that there are great differences of opinion among those who are best qualified to speak. But I think most of us will agree with Professor Broad that the phenomena which have been established greatly lessen the *antecedent improbability* of Survival, by showing that the embodied human mind has cognitive powers which to all appearances do not depend upon processes in its sense-organs and central nervous system. And I think most of us will also agree that such evidence as the Willett scripts provide at any rate confronts us with this dilemma: either discarnate minds exist and can communicate with the living, or else some incarnate human minds possess telepathic and clairvoyant faculties of a staggeringly extensive kind.

So much for the reasonably well-established facts. But as to the explanation of them, we remain almost as much in the dark as the

pioneers of our enquiry fifty years ago. We can see, as they did, that all these phenomena have their seat in the "subliminal" stratum of our personalities. But we can see very little more. We can form no conception, or hardly any, of the *modus operandi* of Telepathy and Clairvoyance. We have good reason to think that Mediumship has some very close connection with the phenomena of dissociated and alternating personality discussed by psychologists and psycho-therapists; but we cannot go much farther than that. Again, if discarnate minds exist, we cannot conceive what manner of existence they enjoy, without physical sense-organs, or physical organs of action such as incarnate minds possess. For the whole of mental life as we know it here is based upon two sorts of experience: the experience of sense-perception on the one hand, the experience of action on the other. And how could either of these occur in the absence of a body and a nervous system? I hazard the suggestion indeed that the real difficulty about the Survivalist Theory at present is not so much the lack of evidence—there is quite a lot of evidence which favours it—but rather the apparent unintelligibility of the theory itself. What I said about Precognition just now applies to Survival too. If the very notion of an unembodied mental life is self-contradictory (and some persons of the highest intelligence have thought so), then no amount of evidence, however great, will do anything whatever to support the Survivalist hypothesis; for it will not really be a hypothesis at all, but just a meaningless combination of words, which cannot even be called false. I do not myself believe that the notion of an unembodied mental life *is* self-contradictory, but I do think it is an extremely puzzling and difficult one.

If I may venture to speak as a professional philosopher for a moment, and offer as it were professional advice, I would suggest that those who incline to the Survivalist hypothesis should spend less of their time collecting evidence for it, and should rather turn their attention for the present to the clarification of the hypothesis itself. If they can succeed in shewing that it is an intelligible and self-consistent hypothesis, in short, that it *is* a hypothesis and not a meaningless combination of words (and I am inclined to think that they could), they could then return to the task of collecting evidence for it with the assurance that their labour would not be wasted.

I have now offered a very brief survey of the present condition of our subject (so far as an onlooker may); and I am afraid that in spite of all the excellent work which has been done, it is still a scene of twilight and confusion, so far as the *understanding* of the phenomena is concerned. Our situation is rather like that of Physics and

Chemistry in the sixteenth century, before it had yet occurred to people that the large-scale properties of bodies could be explained by the motions and configurations of their minute parts : or rather, before that suggestion had been taken seriously, and while it was as yet a mere metaphysical speculation, put forward by one or two singular men two thousand years before. Until this obstacle has been overcome, it is obvious that our subject cannot become a science in the full sense of that word. And it is also obvious that this obstacle *is*, after all, closely connected with the other one which we discussed previously. Until one has a fairly comprehensive theory, however inadequate, one cannot use the experimental method with much profit. For that method, as Kant said, is eventually a way of forcing Nature to answer our questions, and before we can employ it, we must have a reasonably clear idea of the questions which we wish to ask.

In this situation, I think that our only safety lies in boldness. In the collection of facts, one cannot be over-cautious. But in the invention of theories, especially in a field so peculiar as ours, where analogies drawn from the existing sciences are almost useless, a canny and sober circumspection would be the greatest mistake. If people accuse us of being speculative and even " metaphysical " we must refuse to be frightened. We must postulate unverifiable entities and processes if we cannot get on without them. The task of philosophical *deflation*, of removing unnecessary metaphysical entities, comes at the end of a science's progress, not at the beginning ; if such writers as Hume and Mach and the modern Logical Positivists had lived in the early seventeenth century, Physics would never have got itself started. In short, we must not be deterred by the fear of talking nonsense. If this maxim applies to the early stages of any science whatever, it applies with a quite special force to Psychical Research. The phenomena with which we are concerned are so peculiar, and so unlike those visible and tangible facts which ordinary language is designed to deal with, that the right theory of them is *bound* to seem nonsense when first propounded. If we are still frightened, we may take comfort from the history of Psychology in the last thirty years. The statements " Smith is two different people at the same time " or " Smith is one person on Thursday and another on Friday " are on the face of them sheer nonsense : they contradict our ordinary rules for the use of the word " person ". Yet nobody (except a few very old-fashioned philosophers) would now object to the conceptions of Dual and Alternating Personality, or deny that they have thrown great light on some of the most obscure phenomena of the human mind.

Moreover, in our search for a comprehensive hypothesis we must

not mind taking hints from quarters which are accounted scientifically disreputable. I am thinking again of the occult and mystical traditions of the Far East. It is well to remember that in India and the Buddhist countries men not necessarily inferior to Europeans in intelligence have been devoting themselves for very many centuries to the deepening and extension of human consciousness. In fact, in a rather unscientific way they have been practising a kind of Psychical Research for well over two thousand years. The theories which they have been led to frame may have got mixed up with all sorts of dubious theological and cosmological dogmas. Nevertheless they may give us some help in framing a more adequate and genuinely scientific theory for ourselves. We cannot afford to despise any useful suggestion, from whatever quarter it may come. I even think that the humble savage may have something to teach us. It is greatly to be wished that more Anthropologists should be trained in Psychical Research, and more Psychical Researchers in Anthropology. Even the most cursory reading of Anthropological literature is sufficient to shew that Anthropologists have collected a whole mass of material which falls within our province, though their scientific orthodoxy has usually led them to assume that it *must* somehow be explained away as fraud and delusion.

But it is time for me to leave these general considerations and try to practise what I preach. There is one fairly comprehensive hypothesis which has commended itself in various forms to a number of enquirers, and I shall devote the rest of this paper to the discussion of it. If it is tenable it enables us to connect together quite a wide range of supernormal phenomena. This is the hypothesis of a something intermediate between mind and matter as we ordinarily understand them: something which is in *some* sense material because it is extended in space (though not necessarily in Physical Space) and yet has some of the properties commonly attributed to minds. This something was called by Frederic Myers "the Metetherial". More recently Mr. C. A. Mace, in a very interesting address to this Society, has spoken of a "Psychic Ether". In the mystical literature of the Far East we meet the same thing under the name of *Akasa*, which again is usually translated "ether". Here I may mention a noteworthy point about these same Eastern traditions. In the *Sankhya* philosophy,[1] one of the six classical

[1] The word *Sankhya* appears to mean something like "enumeration". Cf. the dictum—I think it is the late Professor Alexander's—that the task of philosophy is "to make an inventory of all the main types of entity in the Universe".

Indian systems, we find a very sharp dualism between *Purusha* (self or knowing subject) on the one side, and *Prakriti* (usually translated "matter") on the other. At first sight this reminds us of the equally sharp dualism of Descartes between mind and matter. But on further examination we are astonished to find that the line is not drawn at all where Descartes drew it. Very much of what we are accustomed to call "mind" is in the Sankhya system regarded as material. Indeed *everything* that we call mental, except only pure awareness, falls on the material side; of course it has then to be added that there are other forms of matter besides those revealed to our ordinary senses. This idea, or something not unlike it, has not been wholly unknown in our own philosophical tradition. Some Western philosophers have rejected the familiar two-fold division of matter and mind, and have preferred the threefold division of matter, soul and spirit. Here "spirit" corresponds to the *Purusha* of the Sankhya system, while "matter" and "soul" together would correspond to *Prakriti*. At any rate in both cases we have something intermediate between spirit and ordinary visible or tangible matter; and whether we reserve the special name "soul" (*ψυχή*) for this something, or say that it is a "higher" sort of matter, does not make so very much difference.

However this may be, let us assume for the moment that there *is* such an intermediate something and let us follow Mr. Mace in calling it the "Psychic Ether". In the remainder of this paper I shall first try to make this elusive conception a little more definite; and I shall then try to show how it might be used for the explanation of certain supernormal phenomena, notably the phenomena of Haunting.

I want to start by returning to a topic which I have already touched on in quite a different connection, the topic of *mental images*. Philosophers and Psychologists have always supposed that mental images are "subjective": that is, that any given image is wholly dependent for its existence upon the mind, and perhaps also upon the brain, of the person who is aware of it, and moreover that it is private to that person. They have also usually supposed, though not quite always, that images are *evanescent* entities, which have no existence (not even a mind-dependent existence) either before or after the date at which we are aware of them; so that I cannot be aware of the same image to-day as I was aware of yesterday—an assumption which of course does not necessarily follow from the previous assumption of privacy and mind-dependence. Now what is the evidence for these two assumptions? I do not think

there is any conclusive evidence for either of them. The mere fact that images are commonly called "mental" is no evidence. The reason why we call them so is merely the fact that they are not apprehended by means of the ordinary physical sense-organs, such as the eye and the ear. They might well be mental in *this* sense without being on that account mind-dependent, or private, or evanescent. Perhaps it will be said that they are obviously "in" the mind which is aware of them. But this sort of argument, so familiar in the History of Philosophy, will now deceive nobody. It is either repeating in different words the very thing which has to be established—"in" may be just a *synonym* for "dependent on". Or else it is only saying that they are entities which we are directly and immediately aware of. Certainly we are, but nothing follows from this about their *nature*. Something which is "in" the mind in this latter sense might perfectly well also exist "out of" it.

I think then that we are entitled to deny these assumptions if we like. Let us make the experiment of doing so. We will indeed concede that every image is *originated* by a mental act—not necessarily a conscious one—and that this act has its physiological correlate. But we will suppose that, once it has come into being, the image has a tendency to *persist* in being; and that it is not dependent upon the mind for its continuance, as it was for its origination. The mind which originated it might be aware of it from time to time thereafter; it might be "summoned up into consciousness" occasionally, or pop up of its own accord. But conceivably its whole career from start to finish might be passed in the Unconscious. We will also suppose that it is not necessarily *private* to the mind of its original author, but is capable of presenting itself in suitable circumstances to other minds as well.

How should the persistence of an image be conceived? I should suppose that it is more analogous to the persistence of a *process* (*e.g.* a prolonged noise) than to the persistence of a *thing*, such as a brickbat. But I think this is a question of purely philosophical interest, which need not concern us here. For our purpose no great harm will be done if we speak of a persisting image as a kind of *thing*, even though this is not strictly accurate.

I wish now to take a further step. Let us suppose that images are not only persistent entities, but are endowed with *causal properties*. If you prefer to put it so, we will say that they are "dynamic" rather than "static" entities, endowed with a kind of "force" of their own. I am not referring only to what Psychologists call Ideo-motor Action, though that is part of what I mean; nor even

to what is called Association of Ideas,[1] though that again is part of what I mean. I am thinking primarily of Telepathy. I know this sounds absurd. Telepathy is commonly regarded as a relation between two minds or personalities. But is it not possible that the relation between the two *minds* is derivative, and that the primary and fundamental relation is one between two *mental contents*? Perhaps the primary fact is that one mental content is capable of modifying or even of generating another; and when the two contents happen to be contents of two different minds, we call the result Telepathy. Now images are one important sort of mental contents. Let us suppose then that every image is endowed with a kind of *telepathic charge*, enabling it to modify or even perhaps to generate other mental contents, which need not necessarily be contents in the mind of its original author. This charge might vary in all sorts of ways as between one image and another. It might also tend to decrease with lapse of time, and we might suppose that when it finally vanishes, the image itself ceases to exist. Thus some images might persist in being, and retain their causal properties, for a very long time; while others might fade away quite quickly, and some perhaps would hardly outlast the act by which they were first originated.

Now if it is legitimate to think of images in this way—as persistent and dynamic entities independent of the mind of their original author, and able to escape as it were from his control—we might be able to form some more definite notion of the Psychic Ether. It might be an *ether of images*. If it were, I think it would have the kind of properties which we require it to have. For an ether of images would be something intermediate between mind and matter as we *ordinarily* conceive of them; while if we were prepared to stretch these conceptions a good deal, then we could either call it mental or call it material, as we liked. I will now try to explain this.

We are all familiar with the hypothesis of a Collective or Common Unconscious. The suggestion is that although our conscious personalities are isolated, yet in the deeper levels of the Unconscious the distinction between *I* and *you* no longer exists. What does this suggestion really come to? No doubt we picture the Common Unconscious as a kind of continent, out of which our conscious personalities arise like isolated mountain peaks. But this is after all only a picture, useful as it may be. The unity of the Common

[1] Is it possible that Association by Resemblance might occur without unity of consciousness, so that an "idea" in my mind could be associatively linked, by resemblance, with an "idea" in yours? Or is this suggestion *too* nonsensical?

Unconscious cannot really be very like the spatial continuity of a continent. I think that the hypothesis of a Common Unconscious is to be regarded as a *causal* hypothesis. Suppose that a content in my unconscious could directly affect a content in yours, and *vice versa*, without any physical intermediary: for example, suppose that a suppressed wish in me could directly cause a dream in you, and conversely. Suppose that this happened with *every* content of my unconscious and every content of yours. If this were so, what sense would there be in speaking of the one unconscious as "yours" and the others as "mine"? Clearly there would be none. For the unity of *any* unconscious can only be defined in causal terms. It is a unity of law, or of interaction, since it cannot be a unity of space. Thus the hypothesis of a Common Unconscious is only another way of saying that at their deeper levels all personalities are in complete and continuous telepathic rapport. It is not an *explanation* of such rapport, as it is sometimes thought to be, but only another way of describing it. (There are further complications in this hypothesis, which I will mention but not discuss. This telepathic rapport might have different degrees of extensiveness, and perhaps different degrees of intensiveness as well. It might be that each person's unconscious interacted with everyone else's: or only with some other people's. Again, as between Smith and Mrs. Smith it might be complete—every content of the one might affect every content of the other all the time—whereas between Smith and Robinson it might be incomplete and intermittent. Further, it might be either unilateral or reciprocal. A's mind might affect B's, whereas B's mind did not affect A's, or not so much; or both might affect each other equally. When we take account of all these possibilities, we see that the structure of the Common Unconscious might be exceedingly complicated. Considered as a whole, it might have a very weak and washy sort of unity, whereas there might be a number of very strong and intimate unities within it. We could not then ask "Are personality A and personality B united or separate?" We should have to ask instead "How *much* of a unity is there between them?")

Now I can return to the Ether of Images. When I suggested that images might be dynamic entities as well as persistent ones, and that each was endowed with a telepathic charge, this was only a special and limited form of the hypothesis of a Common Unconscious: limited, because images are only one sort of mental contents. The Ether of Images could equally well be described as a certain level or range (perhaps rather a superficial one) within the Common Unconscious: that level or range of it at which images persist and interact

with each other more or less freely, no matter whose mind they began their career in. For the laws of their interaction, and the causal properties manifested in it, are *psychological* laws and properties, though they considerably transcend the perview of Orthodox Psychology, since Telepathy is included among them. I hope I have shewn, then, that the Ether of Images has mental properties; we could even say, with a certain stretching of language, that it actually is a certain level within a common unconscious *mind*.

It remains to shew that the Ether of Images has material properties as well; naturally they will not be the same properties as we ascribe to ordinary matter, but they might be somewhat like them. Here the fundamental point is that images are *spatial* entities. But they are spatial in a curious half-way-house kind of fashion, which disconcerts us at first because we seldom reflect on them (our ordinary tendency is to think by means of them rather than about them). I will consider visual images mainly, but what I have to say applies to tactual and kinæsthetic images too, and perhaps to the other sorts. An image, then, has *extension*; it has a shape, sometimes even a stereoscopic shape. But it does not necessarily have *location*. Of course the parts of a given image A are located in relation to other parts of that same image. One part is above another or to the left of another. This follows from the fact that A is an extended entity. Again the image A may happen to form part of an image-field (the image of a cat may form part of the image of a drawing-room): and in that case we can say that A is located in relation to the other members of the same image-field. But we cannot say that the image-field *as a whole* is located anywhere. It is spatial, in that it is extended and has spatial relations within it, but it forms as it were a spatial world of its own. Likewise if A is an isolated image, as it may be, we cannot say that A as a whole is anywhere, though we can say where every part is in relation to other parts. (We may either regard the isolated image as a particularly simple image-field; or we may regard the image-field as a particularly complex single image.) I think that similar considerations apply to size as well. If I have an image of a cat on a hearthrug, I can say that the hearthrug-like part of the image is *larger than* the cat-like part, and within the cat-like part the tail is *longer* than the left-hand ear. But I do not think it is even intelligible to ask how large the image as a whole is: (for example, is it larger or smaller than someone else's image of Mont Blanc?). What I have said about location obviously applies to motion too, and what I have said about size applies to changes of size. An image can move within its own image-field, and can change its size in relation

to other images in the same field. But it cannot intelligibly be said to move from one image-field to another, nor to have grown larger or smaller on the way. Or if we obstinately insist on saying that all image-fields *must* be " somewhere ", *i.e.* that there is a single space within which all images whatever are located, then we shall have to say that an image can move from one place to another without passing through the intermediate places. But I think it is better to say, as I have, that different image-fields are not interrelated spatially at all, though each in itself is spatial ; and that the unifying factor which unites them all into one single Ether of Images is not spatial but causal.

If this still does not satisfy you, I will make a further suggestion. Although there is nothing in the image-world which resembles the relation of distance in the physical world—since one image-field is neither near to nor far from another—yet there might be something which has a faint analogy to it. It might be that in the Ether of Images there are greater or lesser degrees of *telepathic affinity* as between one image and another ; and there might be some degree of telepathic affinity, however slight, between every image and every other. And if such relations of affinity should happen to constitute an order having dimensions, we might then be able to say that there *is* an all-embracing image-space after all. But if so, it will be a different *kind* of space from the one which we find within any one image or any one image-field. It need not have three dimensions. And even if it had, there would be no detailed correspondence between it and the space of the Physical World. A's body and B's body might be very close to each other in Physical Space, and yet A's images and B's images might be very distant from each other in the image-world. For there might be very little telepathic affinity between A's images and B's, despite the relation of physical proximity between their respective bodies ; conversely, there might be the most intimate telepathic rapport between A and B, though their bodies were a thousand miles apart.

I have now tried to show that an Ether of Images would have both mental and material properties. It is either a queer sort of mental world or a queer sort of material world, as we like, though neither the word " mental " nor the word " material " can be applied to it without a certain misfit and discomfort. Indeed all this talk of an Ether of Images, or of a Psychic Ether at all, may well seem like sheer nonsense when considered in cold blood. But as we saw earlier, this is only to be expected. Any theory of these difficult matters is bound to give our ordinary language-habits a pretty violent tweak. If it does not, we can be sure that there is something wrong with it.

I now want to illustrate the explanatory value of my hypothesis by applying it to the phenomena of Haunting [1]; perhaps it will have to be modified somewhat in the process, and will emerge at the end looking even queerer than it did at the beginning. We may divide the phenomena provisionally into two classes: haunting *without* physical effects, and haunting *with* physical effects. The second class would often be lumped together under the head of "poltergeist phenomena". But I think this is inadvisable, at any rate as a first step. For sometimes the physical effects have at any rate the *prima facie* appearance of being produced by a discarnate mind, whereas poltergeist phenomena proper seem to be produced "mediumistically" by the agency of an incarnate one. However, it is haunting *without* physical effects which I wish to consider here: that is, cases in which the haunting consists solely in the repeated occurrence of phantasms or apparitions in a certain locality, including the occurrence of phantasmal smells or noises or touches. And let us suppose, for the sake of definiteness, that the haunted locality is a certain room in a certain house.

Now it has often been suggested that such apparitions are due to some sort of localised trace or vestige or impress left in the matter of the room. These traces would be the quite automatic result of the emotions or other experiences of some person who formerly inhabited the room, much as finger-prints result automatically from our handling of a wine-glass or a poker. Thus on this view the apparition is not a *revenant*, as popular superstition supposes—not a deceased personality revisiting the scenes of its former experience nor yet an "earth-bound spirit" lingering on in them—but is something more like a photograph or a cinematograph picture. (The physical trace would correspond to the photographic negative; and it would be as it were "developed" when anyone with a suitable mind and nervous system enters the room.) This is what Signor Bozzano calls "the Psychometrical Theory" of Haunting. For in Psychometry too we seem to find that a material object retains traces of the past experiences of a person who was formerly in physical contact with it. Signor Bozzano himself holds that this Psychometrical Theory fits only some cases of haunting, indeed only a relatively small number, and that the majority must be explained by the activity of surviving and discarnate personalities, an activity which may either be of the automatic and somnambulistic sort, or of the conscious and intelligent sort. In this he may be right. But

[1] I should like here to express my great obligation to Signor Ernest Bozzano's book *Les Phénomènes de Hantise* (Alcan, Paris, 1929).

it seems a good methodological principle to push the Psychometrical Theory as far as ever it will go.

Now if we attempt to work out this theory in detail, I think we are driven to combine it with the Psychic Ether hypothesis. For these traces, or whatever they are, are not of course independently observable in the physical matter of the room, for instance in the walls or furniture. (Contrast the traces on a gramophone record. These *are* independently observable, *as well as* the sounds which they enable us to hear.) If they are indeed *physical* traces, they must consist in some more or less permanent mode of arrangement of the molecules or atoms or infra-atomic particles, of which the walls, furniture, etc., are composed. And in that case, it ought to be possible to verify their existence by the ordinary methods of Physical Science—by physical or chemical tests of some sort or other. But so far as we know, this cannot be done. It is therefore natural to suggest that the seat of these traces is something which is not material in the ordinary sense, but somehow interpenetrates the walls or the furniture or whatever it may be : something which is like matter in being extended, and yet like mind in that it retains in itself the *residua* of past experiences. And this is just what the Psychic Ether is supposed to be.

We have tried to conceive of the Psychic Ether as an ether of images. Will this conception of it fit in with the requirements of a psychometrical theory of haunting? I want to show that it will. But I admit that there are difficulties ; and in order to surmount them we shall have to introduce fresh complications.

The essential point in our previous discussion was of course the suggestion that images are persistent and dynamic entities, which when once formed may have a kind of independent life of their own, and may escape more or less completely from the control of their author. Let us now suppose—despite of what we said earlier about their spatial properties—that in certain special circumstances an image or group of images might get itself localised in a particular region of Physical Space. (What circumstances, we shall see later.) Once localised there, they might continue to be so localised for a considerable period, retaining the telepathic charge which they had at first, though this might gradually diminish in intensity. Suppose that a human being now enters the room ; and suppose there is a telepathic affinity between the contents of his mind on the one hand, and these persisting and localised images on the other. A telepathic process then occurs. The result of this might only be that the visitor feels a feeling of emotional *malaise*—which is not uncommon in haunted places—accompanied perhaps by what is called " a sense

of presence". Or again it might be a dream. Or finally, in the most favourable cases, it might be the generation of a phantasm located in his ordinary waking visual field (or tactual field as the case may be). This apparition might be related to the persisting image in much the same kind of way as the visual sense-datum of a chair is related to the physical chair. For it would certainly be generated by a process originating in the image (a purely psychical one, to be sure, not a psycho-physical one) and it would be located in approximately the same place; its shape might also be a perspectified aspect of the shape which the image has. When a man is aware of an ordinary sense-datum which is related to a chair in this sort of way, we say he is *perceiving* the chair. And so we should be entitled to say here that he is *perceiving* the persistent and localised image. Moreover, if there were other persons in the room whose mental contents had the required telepathic affinity with the persisting image, we could say that these other persons too were perceiving the persistent image as well as he. Likewise he or others might perceive it again on some later occasion. Thus the persisting image would be a kind of "public object", as the chair is; except that it would be "public to" a restricted class of persons—namely all those, and only those, whose mental contents had the requisite minimum [1] of telepathic affinity with it—whereas the chair is public to all percipients with normal eyesight. On the other hand, though public to different people, it would not necessarily be public to different *senses*. It might be visible but not tangible, or tangible but not visible; or perhaps it could only be heard or smelt.

There is a further point which may be worth mentioning. If we now take into account the fact that the persisting image did originate in a living human mind, perhaps a long time ago; and if we prefer to conceive of telepathy as primarily a relation between *mind* and *mind*, and not just between mental contents: then we may say that haunting is a kind of *deferred telepathy*, resulting in the production of a *post-dated telepathic phantasm*. It will be a telepathic transaction between Smith as he was ten years ago, when he lived in this room, and me who am in it now. The telepathic impulse from him will have been stored up, as it were, in the persistent and localised image which he originated and left behind him long ago, and the impulse only reaches me to-day when I come into the room.

This line of reflection suggests another. The person who originated the image may still be alive at the time when the apparition

[1] I say "requisite *minimum*" because we have evidence that the same ghost may be perceived more clearly by some people and less clearly by others. But cf. also pp. 339 *et seq.*, below.

occurs. (The theoretical possibility of "haunting by the living" seems to be actually confirmed in some of the records.) But it is usually found that he is dead, and even that he died many years before. Let us suppose that he *is* dead. Let us also suppose that the haunting is of a fairly complex sort, though still without *physical* effects. For instance, we will suppose that the phantasm is seen in a number of different rooms in the house, and it is seen to move from one room to another, so that the phenomena are "cinematographic" rather than just "photographic". Here then there is a *group* of persisting images, interrelated in a fairly complex way. Now since the original author of these images is dead, Anti-Survivalists will of course wish to maintain that his mind has ceased to exist. But can they quite maintain this, if our explanation of the phenomena is the correct one? For, to put it crudely, a *bit* of him does still survive, even though his body has long since disintegrated. This set of interrelated images is something like a very rudimentary secondary personality. It was split off from his main personality at the time when he lived in this room; it escaped from his control and acquired an independent existence of its own. And it has succeeded in "surviving" the disintegration of his body, even if we say that his main personality has not. To be sure, it need not survive for ever. Eventually the images may lose their telepathic charge and fade away. The fact remains that it has succeeded in surviving for quite a long period, possibly for many years. Of course it is very far from possessing *all* the attributes of a personality. To call it even a secondary personality, even though the adjective "rudimentary" be added, is very likely an indefensible stretching of language. All the same, it is an interrelated set of mental contents, endowed (if we are right) with a certain telepathic power. Moreover, it is a "cinematographic" phantasm which we are now considering: there is the appearance of *movement* and of changes of posture. It is a *series* of visible or quasi-visible shapes. And in the manner in which the series is interrelated there may be, and there often is, the appearance of a rudimentary purpose. The complex of persisting images is dominated as it were by a kind of *idée fixe*. If we did not know that we were seeing a mere apparition, we should say "here is a human being who is behaving in a curious somnambulistic way". Thus, though it may be unjustifiable to call the set of persisting images a rudimentary secondary personality, such language is not without excuse. Perhaps in the circumstances, the crudest terminology is the best. Let us repeat then that a "bit" of the deceased personality has succeeded in surviving.

But once we admit this much, I think we have to go farther. If a

bit of his personality has managed to survive, if something which is at any rate *quasi*-mental has managed to carry on its existence for years quite apart from a brain and nervous system, the survival of a complete personality is not impossible; the antecedent improbability of a complete or integral survival is at any rate diminished. This conclusion is a somewhat curious one. For the Psychometrical Theory of Haunting has seemed acceptable to many people precisely because they thought it was an *alternative* to the Survivalist explanation. And so in a way it is. But the alternatives are not to clear-cut as they look. In the first place, there is actually a Survivalist element in the Psychometrical explanation itself, as I have just shown. It is a question of *how much* survival we must postulate in order to explain the phenomena of Haunting; we have in any case to admit the survival of something, and of something quasi-mental. And secondly, as I have also shewn, though the phenomena of Haunting do not in themselves require the hypothesis of complete survival (since something very much less will suffice to explain them), they do indirectly weaken the most important objection against that hypothesis, by shewing that something which is at least quasi-mental can exist in the absence of a brain and nervous system.

The account which I have given of Haunting, in terms of persisting and telepathically-charged images, is exposed to certain difficulties, which I must now try to meet. Especially I have to make it consistent with what was said earlier about the spatial properties of the Psychic Ether. I have of course maintained all along that this Ether is an Ether of Images. But I suggested above that the Ether of Images is not a single spatial continuum. An individual image, I said, or again an individual image-field, does have spatial extension, and the notion of spatial location applies *within* it. But I also insisted that *between* one image-field and another there are no spatial relations in the ordinary sense, though there may be relations of telepathic affinity, and these may conceivably be arrangeable in an order having dimensions. But in the account which I have just given of Haunting, it is of course essential to maintain that an image or set of images can be quite literally *localised* in a certain region of Physical Space, for example in a certain room. But if the Psychic Ether as a whole is not a single spatial continuum, how can a part of it be located in Physical Space which *is* a single spatial continuum? There is a further difficulty which arises from the suggestion that Haunting is a sort of deferred Telepathy. For normal Telepathy —including the sort which results in the production of a telepathic

phantasm—seems to be independent of the spatial position of the percipient's body. There may be a telepathic relation between A and B when their bodies are many hundreds of miles apart; and there may be none—or none that we know of—when they are only a yard from each other. But Haunting, whatever theory we may hold about it, seems to *require* a certain sort of spatial relation between the percipient's body and the haunted place or object: moreover it must be a relation of spatial proximity. (Has a ghost ever been seen at even two hundred yards' distance? The usual range seems to be only a few feet.)

There is, however, a normal and fairly familiar phenomenon which may help us here. It is possible to "project" a mental image into space. Thus, with a certain effort, I can now project a visual image of a black cat on to the carpet which I see before me. The cat-like image is then located in my ordinary visual field. Much the same can be done with auditory images. An auditory image resembling the sound of a gramophone can be projected into space, so that it is somewhat as if one were hearing a gramophone in the next room. Some people will perhaps say that they cannot do these things at all, and do not know what I am talking about; others, that they can do them easily; still others, including myself, that they can do them occasionally and only with a special effort. I would suggest, however, that this projecting of images is a process which can and does go on in all of us automatically and perhaps frequently; and that these differences between one person and another are only differences in the degree of consciousness which they have of it, and in the degree of voluntary control which they have acquired over it. I have already suggested that image-*formation* is going on in all of us all the time, whether we are aware of it or not; and the same might be true of image-projection.

We may notice that the projection of images is in any case a very peculiar process, quite unlike anything which goes on in the Physical World; and this despite the fact that the image is a spatial entity, having the properties of extension and shape. When the image is "put" into my visual field, it is not at all like putting a book on to the table or "projecting" a tennis ball into the street. The image does not *pass* into my visual field from somewhere else, for it was not located anywhere to begin with; nor does it pass through other places on the way. It simply changes instantaneously from a state of being extended but *un*located to a state of being extended and located. I know this sounds like nonsense. But I am simply trying to describe a fairly familiar empirical fact. At least it is familiar to myself and to a number of other people. If there are

any among you who do not know from personal experience what I am talking about, they must just take my description on trust; perhaps their faith may be strengthened by a consideration of the phenomena of Hallucination.

Now we have been assuming—this is the basis of the whole argument—that images, once formed, can persist in being for a long period, independent of the will or knowledge of their author. If so, it is reasonable to suppose that once an image has been projected into a certain region of space, it will remain there as long as it continues to exist. We should have to admit that the images which are responsible for haunting were probably projected *unconsciously*. But I have already suggested that there is no difficulty in admitting this. Thus even though the Ether of Images as a whole is not a single spatial continuum, it appears that a certain bit of it (a certain image or group of images) can come to be as it were " earth-bound " and tied to a particular place in the Physical World, by means of the mechanism of image-projection. And then any telepathic charge which the image may have can only take effect from that particular place as a centre; so that in this special case, though not in others, a " radiation theory " of Telepathy might be feasible—a point which I will consider later.

I now turn to a second difficulty. The haunting apparition is normally a more or less exact copy of the body of some person who formerly lived in the haunted place. That indeed is the point of the preposition " of " when we call it the ghost *of* Smith or whoever it may be. (We ourselves have already compared it to a photograph or cinematographic picture.) But is this at all what we should have expected if the ghost is a persistent and projected image originating in Smith's mind? Is it not most uncommon to form an image of one's own body—especially an accurate one? The puzzle is increased by the fact that the image would have to represent the visible appearance of one's own body *as seen from without*. If the image was formed and projected by Smith in some period of intense emotion, surely the last thing he would be thinking of at such a time would be the outward aspect of his own body—something which he has only seen occasionally in a mirror? Surely the ghost ought not to be the ghost " of " Smith himself: it ought to be the ghost " of " some other person to whom his thoughts were turned at the time?

The difficulty is a serious one, and I think it applies not merely to my theory, but to any theory which tries to explain the phenomena of Haunting on " psychometric " lines. There seem to be two ways of getting over it. First, it has been thought by some people that there is such a thing as the *mind of a place*. They say it is a

mistake to suppose that memory is exclusively a property of persons and animals. They suggest that walls also have memories (as they are said to have ears); or perhaps that a region of physical space, or some extended something interpenetrating it, may retain a memory of events which have gone on in it. This reminds us of the old and queer speculations concerning an *Anima Mundi*, or soul of the world; only that here it would be an *anima loci*, the soul of a place. I do not myself think that it is necessary to have recourse to anything so strange as this; I hope to show presently that the difficulty can be met in a less extravagant way. But in case I am wrong in thinking so, I should like to point out in passing that the hypothesis of an *anima loci* is not really quite so queer as it looks at first.

At least it begins to look much less queer if one adopts a certain theory of sense-perception and of the constitution of the material world. This theory, which I think is quite plausible on other grounds, is the one put forward by Bertrand Russell in certain works of his middle period, notably *Our Knowledge of the External World*, *Mysticism and Logic*, and *The Analysis of Mind*. According to it, a piece of matter is not the relatively simple and tidy object which we commonly think it to be, but is a vast and complex *group of sensibilia*. Sensibilia are such entities as colour-expanses, sounds, tactual pressures and the like. In fact they are just such entities as are called *sense-data* or *sensa*, only that they persist in being whether anyone is sensing them or not; and the sense-data actually sensed by human and other percipients are short temporal slices of such persisting sensibilia. This theory has never yet been worked out completely, and it is obviously exposed to serious objections from both the Physical and the Physiological side. I think myself that these objections can probably be overcome by making the theory slightly more complicated, but it would not be relevant to discuss the matter here. What concerns us now is the mode of spatial location which the sensibilia are said to have. Each of them is not only *at* a certain place but also *from* a certain other place, and until we have mentioned both places, we have not completely specified the sensibile's location. Thus a certain mountain, say Skiddaw in Cumberland, is a vast and complex assemblage of "views" which go on existing whether or not anybody is viewing them. One of them will be a view of Skiddaw *from* the top of Helvellyn, another will be a view of it *from* Keswick railway station, others will be *from* various points in Derwentwater, and so on. And when we say that they are views "of" it, each existing from its proper place, we mean that they are members of the group which collectively *is* Skiddaw.

Now this theory may well be combined (as Lord Russell has in fact combined it) with another theory which philosophers call Neutral Monism.[1] According to this, both mind and matter are composed of the same constituents. Such entities as colour-expanses, sounds and the like are not merely objects of a mind's awareness, but actually constituents of that mind. If they are also constituents of the material world, as the previous theory says they are, then mind and matter overlap, and any case of sense-perception is a case of such overlapping. We can now return to the hypothesis of an *anima loci*. Consider any place P, say the middle of a certain drawing-room. Since colour-expanses and other sensibilia are continuously existing from the place, whether or not any percipient organism occupies it, we can regard these sensibilia as forming a group—a group united by the fact that all the members exist *from* the same place P. And if the Neutral Monist theory of mind be adopted, we could regard this group of colour expanses existing from P as constituting a kind of rudimentary mind, the "mind of" place P, an *anima loci*. To be sure, we must now take a further and even more dubious step if we are to get the particular sort of *anima loci* we want. We shall have to endow it with a rudimentary memory as well. We shall have to say that when there are sensibilia existing from a place, then memory-images also come into being which exist from (or at?) that place, and which are more or less accurate reproductions of these sensibilia. These images, we shall have to suppose, remain there permanently, and collectively constitute "the memory of the place". This is certainly a rather extravagant suggestion. We ordinarily think that memory-images can only be *generated* in connection with some sort of brain or nervous system; and we are still disposed to stick to this view about their origin, even if we suppose—as I have been doing—that they acquire a more or less complete independence afterwards. But of course many people also think that such entities as colour-expanses only exist in connection with a brain or nervous system. This is the familiar assumption of the psycho-physiological "subjectivity" of sensible qualities, an assumption which was taken over from the Cartesian philosophy of the seventeenth century, and has been part of Orthodox Science ever

[1] Cf. Russell, *The Analysis of Mind, passim.* The theory of Neutral Monism was first worked out by Ernst Mach in his *Analyse der Empfindungen* (an English translation has been published by the Open Court Company under the title *The Analysis of Sensations*). But the theory had already been suggested by Hume in his *Treatise of Human Nature* (p. 207 in Selby-Bigge's edition) and in Berkeley's *Commonplace Book*, though not in his published works. If I am not mistaken, there are also traces of the theory in some forms of Buddhist metaphysics.

since. If once we reject this assumption and say that such entities as colour-expanses are actually constituents of the Physical World, perhaps it is less difficult to say something similar about memory-images, and to hold that they come into being automatically at any place from which sensibilia exist, whether a nervous system occupies that place or not.

So much then by way of showing that the notion of an *anima loci* or *place-memory*, fantastic though it be, is not quite so fantastic as it seems at first sight. If a place can retain memory-images of the views which formerly existed from that place, and if some of these were views of Smith's body from without, and if these memory-images are what *we* perceive now when we see Smith's ghost: then it is very natural that the ghost resembles Smith's body as it would appear to an external observer.

However, I do not think that we are compelled to accept this queer theory. The difficulty which led us to discuss it can be solved in another and simpler way. The difficulty, it will be recalled, was this: if the external source which causes us to see the apparition is an image which originated in the mind of Smith himself, why should the apparition resemble Smith's own body as it would appear from without? Indeed why should it resemble his own body at all? Now fortunately there is an analogy which we may appeal to, which at least shows that the thing can happen. I refer to the classical telepathic phantasm. This seems to represent the agent's body as he habitually thinks of it, and clothed in the clothes which he thinks of himself as wearing (there is the well-known case of a telepathic phantasm with a patch in the skirt). Sometimes there are additional elements represented, as wounds or injuries, and sometimes the phantasm appears dripping wet. These again are features which the agent would *think* of as characterising his body at the moment, though again he would not have *seen* them as they would look from without. These considerations suggest that a mind *can* form an image of its own body as it would appear from without. Presumably such an image would be formed unconsciously, in accordance with one's likewise unconscious beliefs about the visible characteristics of one's own body. If this is indeed so, there is no reason why Smith should not unconsciously *project* an image of his own body into the space of the room in which he is; and this image, according to our previous argument, will then persist there, to become in due course the ghost " of " Smith. Perhaps such images of one's own body are more liable to be formed and projected in periods of strong emotion. Or perhaps they are formed and projected constantly (though unconsciously) but in moments of intense emotion they may have a

stronger telepathic charge, or a greater power of persistence, or both.

So far we have considered the phenomena of Haunting from the side of the object perceived. (The sense in which I am using the word "perceive" has been explained on p. 326 above). This object, we suggest, is an image or set of images originating in the mind of someone who formerly lived in the place; projected by him into space, probably unconsciously; endowed with a certain telepathic charge, deriving perhaps from the emotions he felt at the time; and persisting in that place thereafter, independent of the mind or will of the original author. But we have still to consider the situation from the other side, the side of the percipient who "sees" the apparition; and we have to try to understand, as far as we can, the process by which the seeing comes about.

This process can hardly be analogous to the physical and physiological processes which underly our normal visual experience. The ghost which I see may be in the middle of a room which I also see; but the two seeings must be caused in different ways, even though the final results of the two causal processes are similar. For on any theory, and whatever the external factor in ghost-seeing may be, it can hardly be something which emits or reflects ordinary light-rays. Otherwise it would be an ordinary physical object, whose presence could be detected by the ordinary methods of Physical Science. Moreover, it would not then have the *restricted publicity* which ghosts do have, but the unrestricted publicity of an ordinary physical object, and it would have to be tangible as well as visible. Could it then be analogous to a rainbow or mirage, which is visible but not tangible? No, for even so its publicity would not be sufficiently restricted. Anyone with normal eyesight would then be able to see it, provided he stood in the right place; and this is not found to happen. And of course if the external factor in ghost-seeing is what I myself have said it is, namely a persistent and localised mental image, it certainly cannot emit or reflect light-rays, even though it is located in the space of the physical world: it is *in* the physical world, but not *of* it. We have accordingly suggested that the process which enables us to perceive it is not physical but telepathic—a kind of deferred Telepathy.

But if it is telepathic, there is a serious difficulty to be faced. For it appears that *this* telepathic process, unlike all others, must be subject to spatial limitations. If it were a case of ordinary Telepathy, ought I not to see the ghost equally well whether I am here, or in the next street, or in America, provided that I have the right

sort of telepathic receptiveness? But actually of course I shall only see it if I am physically present in the haunted room. Shall we then be driven to say that in this type of case, though in no others, a " radiation theory " of Telepathy is correct? Shall we say that the persistent and localised image emits a radiation of a sort unknown to Physical Science, and that this affects the brain and consequently the mind of the percipient? I do not think we could hold in any case that the radiation affects his *eyes*. For, if so, it ought to be subject to the same kind of laws as ordinary light : and the shape, size and position of the apparition ought to be altered by the interposition of lenses, prisms or mirrors, which does not apparently happen. We have to remember too that sometimes the ghost is only " seen " in a dream, when the percipient's eyes are shut ; and sometimes he does not see it at all, and yet he may still experience a vivid " sense of presence ". Thus I think we should have to say that these radiations, if such there be, affect the central nervous system directly ; not indirectly, by way of a peripheral sense-organ, as light-rays or heat-radiations do. Perhaps this *is* what happens. But if we are inclined to accept this account of the matter, we must face the consequences. The central nervous system is nothing but a physical object of a very complex sort ; and any radiations which can cause changes in it must surely be *physical* radiations, detectable by the methods of Physical Science (even if not yet detected) and able to be diverted or modified by purely physical means. Moreover, if I may repeat it again, the entity which emits them must then itself be a *physical* entity, and must itself be detectable by these same methods : for example, by electrical methods of some very refined sort. Are we prepared to accept these consequences? We certainly cannot say that at present there is any empirical evidence for their truth ; and if they are false, the radiation hypothesis must also be false.

I think then that although the process which results in the seeing of a ghost is undoubtedly *spatial* in some sense—as the very meaning of the word " haunting " implies—yet it is not helpful to conceive of it as any sort of radiation, at least in our present state of ignorance. And if it be a telepathic process, we have got to account somehow for the spatial limitations to which it is subject. Can we find any other way of accounting for them, once the radiation theory is rejected? The answer I am going to offer is perhaps the most unplausible of all the unplausible things I have suggested in the course of this address ; you may think that in putting it forward I am surrendering to the wildest superstitions of the Occultists.

I want to suggest that there may be some truth in the theory of an " aura " or " psychic atmosphere " surrounding the body of a living

person. This theory, if you like, is just another application of the Psychic Ether hypothesis which is the main theme of this address. For this psychic atmosphere would have some of the properties of matter (namely spatial extension and location in Physical Space) and some of the properties of mind. It would be a portion of this Psychic Ether localised in and around a certain place, the place where a living human body now is; just as a ghost is a portion of this same Psychic Ether localised in a place where a living human being formerly *was*. And I suggest, though I do not quite know the meaning of what I say, that the "stuff" of which this psychic atmosphere is composed is the same as the "stuff" of which images are composed. I will even say, if you prefer, that it actually consists of a vast mass of unconsciously and automatically projected images —images which form the habitual mental content of the person in question—all mixed up together, and having so to say a certain "mass-effect" characteristic of that particular person. I think this suggestion, peculiar as it may seem, will fulfil our requirements in a way in which the Radiation Theory would not. The psychic atmosphere surrounding the percipient is spatial in quite a literal sense. Yet it is not physical, as his nervous system is, and no physical instrument could detect its presence.

We may then proceed to suppose that the thing which the ghost-image primarily affects (by means of the telepathic charge inherent in it) is not the percipient's central nervous system, and still less his eyes or other peripheral sense-organs, but rather this psychic atmosphere which surrounds and perhaps interpenetrates his body. This does at least enable us to say that the two terms involved in the transaction are *in pari materia*; for the one *is* a mental image, and the other is either an assemblage of images or is at least composed of "imagy" stuff. We do not have to hold that the one term (the ghost-image) is non-physical and the other physical, as we should if we said that the recipient of the telepathic transmission is the central nervous system. Nor do we have to hold that the one is spatial and the other non-spatial, as we should if we supposed that the recipient is the knowing subject or Pure Ego, which is not literally in space at all. *Both* terms—the persisting and localised image on the one side, the percipient's psychic atmosphere on the other—are alike in being spatial, yet neither is physical. Thus it is at any rate somewhat easier to conceive of a direct causal transaction between them.

Before going farther, we must turn aside to consider another difficulty which is at first sight entirely different from the one which concerns us at present. But I think we shall find in the end that it will help us to form some notion of the causal process by which

ghost-seeing is conditioned. It is a difficulty which applies to any theory of Haunting. It is this. Why is it that so *few* places are haunted? If the haunting of a house (anyhow the type of haunting which we are discussing) is ultimately caused by the emotions or other experiences of persons who formerly lived there, surely any house which has been inhabited for twenty or thirty years ought to be haunted, and indeed haunted by a number of different ghosts? In a town of any age, almost every street corner ought to be packed with apparitions. Even a new house ought soon to be haunted by ghosts of its still-living inhabitants. (As we have seen already, " haunting by the living " is not unknown. But why is it not a great deal more frequent?)

You may reply perhaps that it is not enough that Smith should have lived in the house, nor even that he should have had emotions of certain *sorts* there ; what is required is that he should have had emotions of very great *intensity*, and these after all are not so very common. Even so, in any house which has been inhabited for a century—no great age for a house—such intense emotions must surely have occurred quite a number of times. Births, deaths, serious illnesses, accidents, are bound to have occurred in the household. And what about prisons and law courts, or places where religious manifestations of the more extreme kind have been systematically promoted? What about railway stations? Arrivals and departures often cause extremely strong emotions. So even if we say that great intensity of emotion is a necessary condition of haunting, even so it would seem that there ought to be a great deal more haunting than there actually is. It is of course true that a special type of *percipient* is required. It is not everyone who can see a ghost, even granting that the requisite conditions are present on the objective side. But even so, such percipients do exist. Should we not expect *them* to see vastly more ghosts than they do see? For the *objective* conditions, it would seem, must be fulfilled in a very large number of cases.

The solution I would myself suggest is this. I think we should boldly agree that, so far as the objective conditions go, every place which has been inhabited for any length of time *is* haunted by a large number of ghosts. The trouble is, I suggest, that as a rule it is haunted by *too many*, so that their effects obliterate each other. I will explain myself. In any long-inhabited place there will be quite a large number of persistent and localised images, unconsciously produced and projected by the minds of those who have inhabited it. I suggested just now that the " psychic atmosphere " surrounding a person might be a kind of mass-effect due to a large number of

different images which he unconsciously projects. Perhaps in the same sort of way every room which has been inhabited for some time has *its* psychic atmosphere, and likewise every law court and railway station. This psychic atmosphere of the room will be a kind of blending of all the persistent images which have been unconsciously projected into it from time to time. These different images will have different telepathic charges—corresponding to differences of type and intensity between the emotions with which they were originally associated. The result will be a kind of confused amalgam of the whole lot. Consequently, the percipient, even though he has all the requisite subjective qualifications for ghost-seeing, will only be able to say that the room has a characteristic "feel" about it; he will not be able to *see* anything. It might even be that the telepathic charges of the different images cancel each other out, so that nothing is either felt or seen.

I am much inclined to think that the same kind of thing may happen in ordinary Telepathy: that telepathic "impulses" in great numbers are continually reaching everyone, but that normally they inhibit each other, just because they are so numerous and so diverse. If so, the reason why most of us appear to receive no telepathic impressions is that we receive too many, so that no one of them makes any distinct or individual mark upon our minds. If this parallel is not acceptable, I will appeal to a frankly materialistic analogy, drawn from the more familiar world of the Detective Story. When a certain object, say a poker, has been handled by a great number of people, it will be useless to look for thumb-prints on it: not because there are none, but because there are too many, and they are all blended together into an undifferentiable mess. So it may be with the persisting images which previous occupants of a room have left behind them. Just because they are so numerous and so different, no one of them makes any individual impression upon the mind of the percipient. The place is so much haunted that it seems not to be haunted at all.

We can now turn back to our previous question concerning the causal process which results in ghost-seeing. I suggest that the first thing required is the overlapping or interpenetration [1] of two "psychic atmospheres", the one which surrounds the percipient's

[1] We have no reason to suppose that images are impenetrable to other images, as material particles are impenetrable to other material particles. So far as we can tell, two localised images could be in the same place in Physical Space. However, if images *are* mutually impenetrable, the process which I have called interpenetration ought rather to be described as a blending or mixture, analogous to the blending of two liquids or gases.

body and the one which pervades the room. This interpenetration of the two psychic atmospheres will be the *spatial* condition which has to be fulfilled if the ghost is to be seen. This is the substitute which I would offer for the Radiation Theory discussed and criticised above. For we may suppose that this interpenetration will cause changes in the psychic atmosphere of the percipient. *Ex hypothesi* a man's psychic atmosphere is affected by processes in his mind. I now want to suggest that the causal relation between them may be two-way, so that his mind is in turn affected by changes in his psychic atmosphere, which after all consists of mental contents belonging to him; for images, though projected into space, are still mental entities. If this be granted, a man's psychic atmosphere will be a kind of secondary body, related to his mind in the same kind of way as the ordinary physical body is, though perhaps more intimately. (The notion of a "spiritual body" additional to the "physical body" is of course an old and familiar one; what is new is only the suggestion that it is composed of unconsciously projected images, or at any rate of image-like stuff.)

But obviously this spatial interpenetration of two psychic atmospheres—the percipient's and the room's—is not a *sufficient* condition of ghost-seeing, but only a *necessary* (indispensable) condition. As I have already said, I think that a telepathic process is also required. I have suggested that every persistent and localised image has a telepathic charge. We may further suppose that telepathic charges can differ in at least two distinct ways: in *quality* and *intensity*. We will next re-introduce the notion of *telepathic affinity* which was mentioned some time ago. This, it will be remembered, was a relation between two psychical contents which makes it possible for the one to have a telepathic effect upon the other.

Now the localised and persistent image, which is the external or objective factor in Haunting, may have a greater or lesser degree of telepathic affinity with the contents of the percipient's psychic atmosphere, or perhaps even none at all; whether it has much, or little, or none will depend on the *quality* of the telepathic charge inherent in it. If the affinity is slight or non-existent, nothing will happen, even though the *spatial* conditions for ghost-seeing are completely fulfilled. And even though the telepathic affinity is great, still nothing will happen if the intensity of the telepathic charge is too low. But let us suppose that the affinity is great and the telepathic charge very strong. Then the localised image *will* have a telepathic effect upon the percipient's psychic atmosphere, and that again upon his mind. In consequence, he will produce and project a phantasm. And if this phantasm corresponds pretty

closely in shape, size and location to the persistent image which started the process, then we may say that he is "perceiving" the persistent image itself. (How shall we discover whether the phantasm *does* correspond to the persistent image? We discover this indirectly, by finding out whether it has a sufficiently close resemblance to the body of some former inhabitant of the room.) Thus the final stage of the process will be the same as it is in the case of the classical telepathic phantasm. The difference will be in the earlier stages. For the occurrence of the classical telepathic phantasm is not dependent upon any particular spatial relation between the agent and the percipient. Moreover, in haunting the *immediate* agent is not a mind, but only an image: though the *ultimate* agent is the mind which originated and projected the image long ago. So it is a case of *deferred* telepathy, resulting in the production of a "post-dated" telepathic phantasm. I have indeed myself suggested that in all telepathy, of whatever sort, the immediate and primary source of the telepathic impulse is a psychical content rather than a mind. Even so, the difference between Haunting and Classical Telepathy still remains. For in Classical Telepathy the telepathically-active psychical content is actually contained in a complete and living personality; whereas in Haunting it has long since got dissociated from the mind which originally owned it, and indeed that mind (if the Anti-Survivalists are right) may long ago have ceased to exist.

We were asking a few pages back why ghost-seeing is a comparatively rare occurrence. We can now add something to our answer. The reason we gave was that most places are haunted too much and as it were too promiscuously. If an apparition is to be seen, there must be a certain constituent in the psychical atmosphere of the room which stands out, so to speak, from the rest. We can now see that it must stand out in two different ways, *both* in respect of quality *and* in respect of intensity. To use the same sort of analogy as before: if a lot of signatures were written all over a small piece of paper, one on top of the other, you could not read any of them. You would see only a confused blur. But if one were written in red ink and all the rest in black, you might be able to read the red one quite well. And you would be more likely to do so if the ink in which it was written had been particularly strong and lasting. Even so, you would not succeed if you happened to be colour-blind.

It may, however, be that great intensity in the telepathic charge can compensate for otherwise unsuitable *quality*. Conversely, if the quality is exactly right (if there is the maximum degree of telepathic

affinity between the persisting image and the mental contents of the percipient) this may compensate for feeble intensity. In terms of our analogy: one might still be able to pick out the red signature from the confused black ones, even if it had faded; and even if a man was colour-blind he might still be able to pick out one which was written in much brighter and fresher ink than the rest. These conditions as to quality and intensity are perhaps not likely to be fulfilled so very frequently. And this may incidentally account also for the many instances in which a man sees just one ghost in his life, without giving any other evidence of supernormal powers, telepathic or otherwise.

I must now bring my remarks to a close. I am well aware that the theory of Haunting which I have sketched is full of loose ends. For one thing, it is much too narrow, in that I have spoken as if images were the only important sort of psychical contents, which is far from being true. Again, the theory has in any case been restricted to one special type of Haunting, the type in which there are no physical effects; it could only be extended to cover other types by introducing additional assumptions, which might have to be very outrageous. Worst of all, my whole explanation may be sheer nonsense from start to finish. Certainly I tremble to think what a hash might be made of it by an even moderately competent second-year student of Philosophy. And yet the initial step, upon which everything depends, the suggestion that mental images may persist in being apart from the mind of their author, is not so utterly extravagant, but only unfamiliar. The current view, that they exist only so long as the act of "imaging" goes on, has been simply taken for granted without any solid argument. And once this initial step is taken, the additional assumptions I have made are, I do not say easy, but at any rate not so very difficult.

However this may be, the risk of nonsense has got to be taken. Unless we are prepared to take it, our subject will never advance out of the fact-collecting stage into the maturity of a genuine science. For, as I have pointed out already, the phenomena which concern us are so unlike those which ordinary language is designed to describe, that the right theory of them, when found, is bound to seem nonsensical at first. We may safely predict that it will be the timidity of our hypotheses, and not their extravagance, which will provoke the derision of posterity.

Appendix

There are two rather curious questions which are worth mentioning, though they lie somewhat outside the line of my main argument.

1. According to the "psychic atmosphere" theory sketched above, one would expect that *persons* would sometimes be haunted as well as places. For the psychic atmosphere of a person, like that of a place, consists of a mass of projected images, and they will presumably be telepathically charged. We should expect that sometimes some one of them would stand out from the rest in respect of its quality and intensity, especially if it is connected with some strong and prolonged emotion. In that case it ought to be perceptible to a suitably-qualified percipient. I think there is evidence that this does sometimes happen, though I do not know how good the evidence is.

2. Does anyone ever see the ghost *of himself*? When a man comes into a room which he has inhabited for years, ought he not sometimes to see an apparition of himself, sitting in his favourite chair? Haunting by the living is not unknown, as we have seen. One would almost think that "self-haunting" would be the most common case of it, and indeed that it ought to be quite frequent. The best condition would be that in which the percipient is the *sole* inhabitant of the room and it has never previously been occupied by anyone else. Perhaps this does not happen so very often; even so, it must happen sometimes.

If, however, self-haunting *never* occurs,[1] this is a serious difficulty for my theory. Surely an image projected by myself would have the maximum degree of telepathic affinity with my present mental contents? It would seem then it ought to cause *me* to see an apparition, even if it has no effect on anyone else.

The only solution I can suggest is that the telepathic affinity may be too perfect. Perhaps telepathy only occurs when there is some degree of shock or intrusion. Perhaps a telepathic charge behaves like an electric charge. If two neighbouring bodies have exactly the same electric charge, there is no electrical discharge from the one to the other.

Even so, we should expect that when an adult man revisits the home of his childhood, he would sometimes see the ghost of himself as he formerly *was*, even though he never sees the ghost of himself

[1] Signor Bozzano (*Phénomènes de Hantise*, p. 184) says that it did once occur to Guy de Maupassant. "En rentrant chez lui il se voyait assis dans son fauteuil." The authority for the story is said to be Paul Bourget, but unfortunately Signor Bozzano does not give the reference.

as he now *is*; for his childish emotions were presumably different from his present ones, and so the images which he then projected would have a different telepathic charge from the images which make up his psychic atmosphere now. Or shall we reply, taking a hint from the Psycho-analysts, that he still retains his childish emotions to this day, in undiminished strength, though they are now repressed into the Unconscious?

OBITUARY

Professor Sigmund Freud

Professor Sigmund Freud died in London on September 23, 1939, at the age of 83, after a long and painful illness. He was born in Moravia in 1856, but from the age of four he lived in Vienna until 1938, when, after the annexation of Austria by Germany, he found refuge in England. He studied medicine in Vienna and at first his interests lay in the field of cerebral anatomy and physiology. The various branches of medicine proper, apart from psychiatry, had no attraction for him and after a short period of study under Charcot in Paris, he settled down in 1886 as a specialist in nervous disorders. Abandoning the treatment of organic nervous diseases he confined his work to the treatment of the neuroses. At first his principal instrument of work, apart from haphazard and unsystematic psycho-therapeutic methods, was hypnotic suggestion. From the first he used hypnotism not only as a means of giving therapeutic suggestions but also for questioning the patient about the origin of his symptoms. The value of this procedure he had learnt from Dr Josef Breuer, a Viennese physician, and its further extension by Freud led in the end to the development of the methods of Psychoanalysis in which hypnotism is not used.

The momentous nature of Freud's discoveries might have been suspected from the storm of opposition and obloquy which they encountered on first being disclosed; and nothing in Freud's life-history is more impressive than the courage and persistence with which he devoted himself to work which was not only unappreciated by his fellow physicians but was derided by the greater part of the world. Only after more than ten years of solitary work did his teaching begin to get a hearing among psychologists. Then gradually a small band of pupils from other countries, attracted by his teaching, gathered around him in Vienna and in time helped to spread a knowledge of his doctrines throughout the world.

The first noticeable recognition of Freud's work by academic psychology was an invitation from Professor Stanley Hall to deliver a course of lectures on Psycho-analysis at Clark University in the United States. Freud was greatly heartened by his reception in America. "In Europe," he said, "I felt as though I were despised; but over there I found myself received by the foremost men as an equal."

Interest in psycho-analysis was greatly stimulated by the War of 1914-1918, for during its course psychologists and medical men were forced to admit the part played by mental factors in the production and treatment of war neuroses. Since those days acknowledgment and understanding of Freud's work have steadily increased and at the present time it is recognized throughout the world as the most important contribution to psychology and to therapeutics that has ever been made by one man. And it is not only in these two spheres that its effects are seen. There is no department of knowledge related to man's life and behaviour that has not been influenced by Freud's teaching.

Freud was a corresponding member of our Society and contributed to *Proceedings* a paper entitled a "Note on the Unconscious in Psycho-analysis". It was indeed in his prolonged researches on the unconscious that his work approached most closely to the interests of Psychical Research. He was at all times sceptical of alleged supernormal phenomena but he appeared to have an open mind on the possibility of the occurrence of telepathy. At the end of a paper on "Dreams and Telepathy" written in 1922, he said: "Have I given you the impression that I am secretly inclined to support the reality of telepathy in the occult sense? If so, I should very much regret that it is so difficult to avoid giving such an impression. In reality, however, I was anxious to be strictly impartial, I have every reason to be so, for I have no opinion; I know nothing about it."

Such a statement is entirely in keeping with Freud's attitude towards all judgments of belief or unbelief. Some of his own discoveries were at first sight as unbelievable as those of psychical research and the advice he gave to the detractors of Psycho-analysis is equally applicable to the critics of Psychical Research: "If owing to ignorance of the subject you are not in a position to adjudicate, then you should neither believe nor reject . . . no one has a right to conviction on these matters who has not worked at the subject for many years, as I have, and has not himself experienced the same new and astonishing discoveries."

Those who knew Professor Freud best are at one in their judgment that he was not only a great psychologist but also that he was a

great man. His undeviating search for truth in spite of all opposition and opprobrium has never been surpassed in the history of science. His equanimity under neglect and misrepresentation, his serenity under trial and affliction, his bearing under cruelty and injustice, are the marks of a truly great man.

T. W. M.

REVIEW

SIGMUND FREUD. *Moses and Monotheism* translated from the German by Katherine Jones. The Hogarth Press. Pp. 223. Price 8s. 6d.

Historical research has traced the origin of monotheism to Egypt, and according to Freud the monotheism of the Jews was but a continuation and development of the beliefs of the worshippers of the Sun God Aton during the reign of the Pharaoh Ikhnaton of the Eighteenth Dynasty: Moses was an Egyptian, probably of high rank, and an enthusiastic adherent of the Aton religion in which the sun was worshipped not as a material object but as a symbol of a Divine Being whose energy was manifested in his rays. When Ikhnaton died the Aton religion was abolished and Moses attached himself to the Israelites settled in Egypt, became their leader and converted them to the religion of his teacher Ikhnaton. He led the Israelites out of Egypt and for a time they accepted his teaching, but when they found the hardships of the Exodus more than they could bear they rebelled against him, murdered him and relapsed into idolatry.

The murder of Moses by the rebellious Israelites is the central point in Freud's theory of the revival and final victory of monotheism in Jewish religion. In this book he applies the views on the origin of religion which he had put forward a quarter of a century ago in his book *Totem and Taboo*. Totemism, in which Freud finds the beginnings of all religion, arose in prehistoric times after the murder of the "primaeval father" by the sons of the "father-horde". The sons loved the father, but they also hated him. They were jealous of his power and his possession of all the desired things of life; so they banded together and killed him. After his death the sons suffered remorse, and the love and reverence towards the father was displaced on to the totem animal which was treated as a father-substitute. The hate towards the father found vent in the totem feast in which the totem animal was killed and eaten.

This crime, the murder of the primaeval father, has lain heavy on humanity; it is the "original sin" of mankind. Every god that

man has worshipped has been a personification of some aspect of the father, every act of worship has been a plea for condonation of the sin and an appeal for the continued love and protection of the all-powerful father. Man's need for God arises when he feels his helplessness against the forces of nature that threaten his existence and cause him anxiety. His ideas of God are based upon his childhood experience of his father—one who was powerful to protect him from danger and to whom he could turn in time of trouble. And just as in childhood he turned to his earthly father for protection, so, when grown up, he turns to his heavenly father when no earthly help can be found.

The murder of Moses by the Israelites was a repetition of the murder of the primaeval father, and stirred up and intensified the feeling of guilt associated with the original sin; and although the Israelites for a time tried to gain reassurance by straying after strange gods, in the end they came back to the one and only God whom Moses had taught them to worship.

The influence of events of early childhood on later life is one of the cardinal tenets of Freud's teaching, and he applies to the life of the race the same principle which he found to be so important in the individual life. In the life of the individual such influence is most strikingly shown in the development of the neuroses. Some painful experience in childhood is repressed and forgotten; after a period of latency it may, for one reason or another, get stirred up again and give rise to mental conflict; in the end it forces its way into consciousness in a distorted form as a neurotic symptom.

Painful experiences in the life of the race are also forgotten and after, it may be, centuries of latency may give rise to racial symptoms. According to Freud the rise and development of religion is such a symptom which has its roots in the murder of the primaeval father and his later representatives. But while we may speak of the memory of events in the individual life being repressed in the unconscious, it is not easy to conceive of a racial unconscious in which the memories of repressed racial experience are conserved.

The possibility of something of this kind that would account for what appear to be racial memories has been suggested many times in Freud's writings and he considers the evidence on which his conclusions are based so strong that he now ventures to assert that "the archaic heritage of mankind includes not only dispositions, but also ideational contents, memory-traces of the experiences of former generations".

In advocating this doctrine Freud is well aware that he is, as often before, in opposition to orthodox opinion. He is accepting the trans-

missibility of acquired characters and the existence in the individual of innate ideas.

" Though we may admit ", he says, " that for the memory-traces in our archaic inheritance we have so far no stronger proof than those remnants of memory evoked by analytic work, which call for a derivation from phylogenesis, yet this proof seems to me convincing enough to postulate such a state of affairs. . . . It is bold, but inevitable."

If we accept this view it may be found to have an unsuspected bearing on some of the problems of Psychical Research. For not only would our archaic inheritance provide memory-traces of racial experience but it might also account for the occasional manifestation by human beings of what appear to be supernormal powers. It has sometimes been suggested that what appears to be supernormal faculty in man is often analogous to some of the normal capacities of the lower animals which have in the course of evolution been lost to the human race. For example, many careful observers of animal life have expressed the belief that there is telepathic communication between members of the same species, that their instinctive activities sometimes appear to be guided by foreknowledge, and that the acuteness of some of their special senses is comparable to the hyperacuity displayed by human beings in certain trance states. Indeed the ability or tendency to go into trance may be derived from the same source. A scientific friend of Freud assured him " that the Yogi by their practices of withdrawal from the world. concentrating attention on bodily functions, peculiar methods of breathing, actually are able to produce new sensations and diffused feelings in themselves which he regards as regression to primordial. deeply buried mental states ". Freud comments : " There would be connections to be made here with many obscure modifications of mental life such as trance and ecstasy."

Although belief in archaic inheritance such as Freud postulates is requisite for a full appreciation of this book, the difficulties in the way of acceptance of such belief and the evidence in support of it are not discussed at any length in the text. The whole of the argument is based on the results of psychoanalytic research, and it may be that only readers who are conversant with these results will fully grasp the significance of Freud's speculations. Some of the conclusions arrived at are dependent upon the interpretation of data provided by ethnological and historical research, and there will be criticism of this work by those who do not accept the views of the writers on whom Freud relies for his interpretations. Thus, for example, he has been reproached for adhering to the description of

the totem feast given by Robertson Smith (a man, by the way, who suffered for his heretical opinions very much as Freud has suffered for his), since later ethnologists have unanimously rejected Robertson Smith's theories. But, Freud says: "A new theory does not necessarily denote progress. . . . It was my good right to select from ethnological data what would serve me for my analytic work." Convinced as he was of the truth of his own psycho-analytical findings, he felt justified in adopting any views of competent investigators in other fields that are consonant with his own, whether these views are generally accepted or not.

Moses and Monotheism is a characteristic example of Freud's work both as a thinker and a writer. It is well translated and has an adequate index. T. W. M.

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