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JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

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31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 30th January, 1935, at 5.30 p.m.

WHEN A PAPER ON

“DR. RHINE'S RECENT EXPERIMENTS IN
EXTRA-SENSORY PERCEPTION”

WILL BE READ BY

PROFESSOR THOULESS

[P. 305].

CASE

A PREMONITION

The following dream, the report of which came to us through the President, Mrs Lyttelton, appears to be of the type discussed by Mr J. W. Dunne in his well-known book *An Experiment with Time*, that is to say, the subject matter of the dream is closely related to an incident which occurred shortly afterwards. In experiences of this kind the possibility of chance-coincidence must always be reckoned with and this possibility cannot be exactly assessed in any individual case ; the general lines upon which the evidence may be weighed are discussed by Mr H. F. Saltmarsh in his "Report on Cases of Apparent Precognition" (*Proc. S.P.R.* XLII, p. 49). The names of all the persons concerned are known to the Society, but, except in the case of Mr Miles, pseudonyms are given here.

On 12 June 1934 Mr W. C. Miles wrote to Mrs Lyttelton, reminding her that he had recently attended a lecture of hers in Oxford, and suggesting that she might "be interested in the enclosed account of a dream by a friend of mine. I have known both Miss *Edge* and Miss *Swan* for several months". The enclosed statement, which is signed by Miss *Edge*, the dreamer, and also by Miss *Swan* and Mr Miles, is as follows :

On the afternoon of Thursday, June 7th 1934 Miss *Edge*, after asking her friend, Miss *Swan*, to wake her at 4 o'clock, went to sleep (between 2 and 3 p.m.).

Miss *Swan* woke Miss *Edge* at about 4 o'clock (or just after). Miss *Edge* at once said that she had had a terrifying dream in which she had been to a place where some one was being mauled by lions, and that she herself had only just managed to climb to safety.

Miss *Swan*, on hearing the details, said, "You have been to the Zoo", meaning in the dream experience.

Miss *Edge* had not been thinking of a Zoo or of animals just prior to the dream ; she had, however, been a little excited a day or two previously, but the cause of the excitement had no reference to animals.

The morning following the occurrence of the dream, and its communication to Miss *Swan*, the London papers contained accounts of the mauling of a man by lions at Whippsnade.

This statement is dated "Oxford, June 12, 1934".

From the detailed report of the inquest, which appeared in *The Times*, it would seem that Stenson, the man who was killed, got into conversation with a visitor to the Whippsnade Zoo, named Myhill, when Myhill was having tea "about 4 o'clock". Subse-

quently the two men went together to see the lions and the incident occurred in which Stenson lost his life by falling into the lion-pit. If the times given for the dream and the accident are even approximately correct, the accident must have occurred after Miss *Edge* woke up and told her dream to Miss *Swan*.

NORMAL AND SUPERNORMAL PERCEPTION

BY G. N. M. TYRRELL

THE relation between normal and supernormal perception is a subject which is not much discussed, because people work so much in water-tight compartments that those who are interested in the one kind of perception are not as a rule interested in the other. But, if supernormal perception be an admitted fact, its relation with the perception which we call "normal" must surely be of an intimate and vital kind. It is necessary to take the two together, and it is quite possible that the firm establishment of supernormal perception might lead to a complete revision of our theories of normal perception and so of the entire nature of the physical world.

I am going to attempt to bring out in a very rapid sketch a certain aspect of normal sense-perception which I think puts one's mind in the right attitude for *beginning* to understand the kind of thing that supernormal perception is. It is merely a sketch and a suggestion. I do not wish to be understood as undertaking to prove a thesis: I am only trying very briefly to outline a point of view which I have found helpful, not only as an approach to problems in psychical research, but also to certain metaphysical problems. In the short space at my disposal I shall not be able to go into detail or to discuss questions of evidence. To those of my readers (if such there be) who think that the evidence of psychical research has not yet established anything, I am afraid that this discussion will seem premature.

I will speak first of normal perception, calling your attention to the striking fact that philosophers, though divided in their opinions as to what its nature is, are yet unanimously agreed that the whole subject forms a very difficult problem; and I wish to emphasise the point that this in itself is a very strange fact: the strangeness which it introduces will be found to characterise the whole subject of perception, and to deepen the more one looks into it. For, why should sense-perception be a difficult subject? No layman, unsophisticated in the subtleties of philosophy, ever thinks of the world of his senses as presenting any difficulties at all. If such a man were to come upon

a group of philosophers discussing the subject of sense-perception, his first impression would be, I think, that they were making difficulties out of nothing. The ordinary man looks about him ; he sees trees and houses ; he hears the wind or the roar of the traffic ; he feels the solid ground under his feet, and he cannot see what on earth there is to argue about. Is not the world all given, as plain as a pikestaff—simply *there* ? That, I think, would be his criticism of the philosophers. The world of the sense is *given*—just ultimate, brute fact.

Now, would the attitude of the plain man be right ? Is the sensuous world just given and there, the same for all of us ?

We must glance at the world of sense-perception from the philosopher's point of view, though I cannot go deeply into it, and I think it will be wise at the outset to admit that there is a good deal in the sensible world which is simply *given*. Berkeley's dictum remains true. "When I open my eyes", he said, "in broad daylight, it is not in my power to choose what I shall see." It certainly is not, and that proves that we do not voluntarily create our world in the act of perceiving it and that it is independent of our wills. But that is not the whole story. There is something very peculiar about what *is* given in this world of sense-perception. There is first the question, Why, if it is a simple datum, does it turn out to be such a problem ? Why does it become so unravelled when one tries to look into it and understand it in detail ? For this is just what it does. It starts all right, but becomes unravelled. The fact is that Nature handed the philosophers a problem to solve—and I cannot help regarding it as a piece of irony on her part—she handed them a problem to solve which was solved already : so that when they got to work on it, all they did was to unsolve it. And that surely is an extraordinary thing to start with ; yet I think that it is quite a fair way of stating the truth. I will try if I can to show that it is.

It scarcely seems to need demonstrating that the world of our senses is a solved problem initially, for every percipient being—not only men and women but also young children and animals, live in it without meeting intellectual difficulties so far as their acts of perception are concerned. They do not have to solve problems in order to become aware of the material objects around them. They may, of course, reason *about* these things after they have become aware of them ; but they do not have to reason in order to become aware of them. The sensuous world, in fact, flashes itself upon us as a ready-made whole with a feeling of immediate conviction that it is there, self-consistent and complete. We do not reason on the spot, and I do not think that it can be plausibly maintained that we first got to know our world by solving intellectual problems in extreme infancy,

which have afterwards become telescoped by habit. And can we really persuade ourselves that the cat who becomes aware of a little, grey patch, reasons to herself that it is a mouse? It is, of course, true that for human beings, sensuous stimuli act largely as symbols and start in our minds trains of logical thought, for thinking beings live largely in a world of universals. But this is all subsequent upon the sensuous data. What Berkeley saw when he opened his eyes—what we all see when we open our eyes—comes upon us in an intuitive act—a mystical act if you like—which has nothing to do with reason or logic.

This sense-given world is characterised by a beautiful simplicity and self-consistence, but as soon as the philosopher begins to analyse it, it opens out into a maze of difficulties. It behaves like the thing which in mathematics is called a divergent series, that is to say the kind of series which cannot be summed because its terms go on indefinitely getting larger and larger. The other kind of series, which can be summed, is called a convergent series, because its terms become smaller and smaller, so that, even if there are an infinite number of them, they may yet be summed to a finite total. Any decent, well-behaved problem behaves like a convergent series. A detective novel is an example of one. It begins by presenting the reader with confused and scattered data, but as it goes on, it co-ordinates these data: the clues become clarified; the machinations of the villain are brought to light; everything gradually fits into its place and is satisfactorily explained at the end.

But, the problem of sense-perception does not behave like that; it behaves in exactly the opposite way. Instead of being vague at first, it begins by being perfectly definite; but the more philosophers analyse it, the less definite it becomes; and its air of being a solved problem gives place to an air of being a more and more unsolved problem. Let me try to show this.

Our belief in a world of material objects arises from our two senses of sight and touch alone. Without these, we should have no conception of a world of material things. The other senses fit in with the material concept, but, unaided, they could not give rise to it. Now, let us take some concrete object which we can see and touch, say, for example, a pillar-box. I shall only be able to deal with one sense as my time is very short, viz., the visual sense. We are, then, "seeing" a pillar-box. If we think of analysing the term "seeing" we shall be astonished to find how many complex facts it covers. We look, and immediately we are aware of an entity which exists apart from ourselves, away over there—an entity which is the subject-possessor of a whole string of characteristics, such as shape, size, position, temperature, colour and so forth—qualities which in some sense be-

long to it. How do we get to know all this by merely opening our eyes? The philosopher objects that the term "seeing" is too indefinite. It covers too much. He will object: You say that at a single glance in this act of "seeing" you immediately know of this object and its list of characteristics. You call it a "pillar-box" which is something having an inside as well as an outside; a back as well as a front; a temporal history during which its characteristics may change and yet in some way it itself remains identically the same. Do you claim to "see" all this in one single, visual act?

Now, it is exceedingly hard to say, if we are tackled in this way, exactly how we do get to know all these things. We are not sensibly aware of the inside of the pillar-box as we glance at it; yet even in that glance, we do recognise it as a thing which has an inside. We are not sensibly aware of its back, yet in the same way, we know at once that it has a back. We are not sensibly aware of its objectivity, yet we instantly recognise it as an object, and not as a mere patch in a colour-mosaic. We are not sensibly aware of it as enduring in a more permanent sense than that in which its qualities endure. Yet at a glance we recognise it to be the enduring subject of qualities. Do we *infer* all this from simpler data? But introspection of the perceptual act fails to reveal any trace of inference within the act itself. We do not say to ourselves: That red oblong must be a *thing* having an inside and a back and so forth. We arrive at our result in some way without the use of logic.

The philosopher seeks for the immediate data of the situation. What is it that is immediately given to sense? Surely, astonishingly little when we come to look for it. Just a red, rectangular patch standing out amongst other coloured patches, and seeming, further, to be standing away from us with a certain depth or outness. Philosophers are not agreed as to whether this depth is among the basic data of the situation or whether stereoscopic relief is a secondary feature. But that is not important at present. From the slender datum of this red, oblong, bulgy patch we jump at once in our visual act to complete awareness of a full-blown pillar-box and all that it entails. How do we get from the red patch to the "thingness" of the pillar-box? We do not reason it out; it comes to us in a sort of intuitive flash by the process which philosophers call "External Reference." One can call it an intuition, or a mystical experience if you like. I would hazard that it looks extremely like a case of *suggestion*.

Now I have only space just to touch on this kind of philosophical discussion. My object is to show you that there are mysteries in the act of perception which the ordinary percipient does not dream of. If the plain man can see no difficulties in sense-perception the philosopher can. And there are worse difficulties yet. What is the nature

and existential status of this red, bulgy, oblong datum which seems to constitute that which is actually given in the visual situation? It is this which is called the sense-datum, or sometimes the sensum. Our perceptual consciousness tells us that it actually *constitutes* the front surface of the pillar-box and that it exists at a distance from us in space. But, on reflection, is not this red datum our own private possession? The red oblong which I sense surely belongs to me alone: the one which you sense belongs to you. How, then, can my private datum or yours be literally a constituent of a pillar-box which exists independently of you and me? And, moreover, my datum does not always exist even for me. It depends on certain conditions, for instance, I must be looking in the right direction. Does the front surface of the pillar-box go out of existence when I look the other way? That is surely nonsense. But, what is the truth, then? Perhaps someone else is looking at the pillar-box. Is that what it is? But if he is, his datum is as private to him as mine is to me: and what if no one is looking at it? Or if it is pitch dark? What happens to its red surface then? Can we say that its red surface is still there, only no one can see it? But what is the sense of saying that a red surface is "there" when there is no possible way of distinguishing it from a blue surface or a green one? In what sense is it red? In what sense, so far as vision is concerned, is it there at all? Perhaps some one will suggest that there is no *actual* red surface there, but that what is there is a *potential* red surface—a surface, that is to say, which *would be red if* certain conditions were realized. But this is leading us into still deeper waters, for it suggests that the front surface of the pillar-box is, after all, a mere potentiality; and if its front surface, why not its back and inside and all the rest of its sensible qualities? And what is the pillar-box if it is not in some sense the sum of its qualities? Can a pillar-box be a potentiality which is only actual at times—at those times, that is to say, when someone is looking at it? That makes its actuality depend upon the observer. It would be intermittently actual, and even then not publicly actual. But our perceptual consciousness tells us that it is publicly existent and always actual whether anyone is looking at it or not: and always red whether anyone can see it as red or not. And this proposition seems perfectly simple and obvious to common sense until we try to analyse it, when it turns into a seemingly hopeless tangle of this kind.

This is what happens when we try to analyse the world of the normal senses. I am not going to attempt to resolve the tangles here. The ground is much too difficult and controversial. What I am trying to do is to justify my statement that the world of the senses starts by being a solved problem and becomes more and more an unsolved problem as one looks into it. I am showing you the

divergent series opening out. In different places in its opening fan the various schools of realist and idealist philosophers have come to rest and remain disputing with one another. Does not this fact alone show how unsolved the problem is? The spell of simplicity which rests on the everyday world of our senses is broken directly we think about that world closely. It reminds one of Mr. Belloc's charming little verse about the water-spider :

“ He glides upon the water's face
With ease, celerity and grace ;
But if he ever stopped to think
Of how he did it, he would sink.”

Our common sense glides like that over the sensible world until we stop to think about it. And then, as soon as our attention is focussed upon it, its self-evident character disappears ; its simplicity vanishes ; its satisfactory feeling of being obvious withers at the touch of criticism ; it opens out into one puzzle after another like the mathematician's divergent series.

If the claim of this sensuous world to be ultimate reality were true, how could it possibly unravel as it does? How can facts which are ultimate facts, or entities which are bedrock entities, ever turn into anything else, however much you examine them? If anything manifests a self-inconsistency when it is analysed, then that thing, as Bradley was so fond of pointing out, is not reality but appearance. Under analysis, the objects of the sensible world do manifest this self-inconsistency. They behave, as Shelley said of the leaves before the West Wind—“ like ghosts from an enchanter fleeing ”.

So that what this philosophical argument is bringing out is that the world of normal perception is not the absolute thing it claims to be, but exists relatively to ourselves. Perhaps I can illustrate this relativity still better by taking the case of colour.

What is the *real* colour of anything? At first sight it seems a simple question, and common sense is quite ready to answer, “ Pillar-boxes and tomatoes are red ; grass and cucumbers are green ; the sky is blue and so forth. There seems to be no problem about that ”. But there is a problem ; it obtrudes itself even in daily life, and especially in feminine daily life. If one ever goes on a shopping expedition with a lady it is pretty sure to turn up. The lady selects her piece of material at the counter, but before buying it, she takes it to the window to find out what its *real* colour is ; because she knows from experience that what looks like a nice shade of beige under the electric light may very well turn out to be an ugly shade of yellow by daylight. Now, if you asked her what colour her piece of material *really* was, she would probably answer, “ Of course its *real* colour is

an ugly yellow ; it only *looked* beige under the electric light". She would be speaking, of course, without the caution born of a training in philosophy. Now, it is not really a silly thing to say that a piece of material *looks* one colour under one set of circumstances, but *is* another colour, which it looks under another set of circumstances ? How is one to define the difference between "looking" a colour and "being" a colour : and in any case, how decide which of the two colours the thing really is and which it only looks ? The lady decides that it *is* the colour it appears to be by daylight, but on what grounds ? Only, apparently, on the ground that she is more interested in its daylight colour. But suppose that the sun suddenly turned the same colour as electric light, what would the *real* colour of the material be then ? She would then say that the material really *is* the colour which she now says it only *looks*. Her point of view is thoroughly anthropocentric and relative. What she calls their *real* colours are the colours things have under the conditions to which she is most accustomed. The colours which things have occasionally are the colours which she says they only "look". But there are no grounds for drawing a distinction between "looking" a colour and "being" a colour. Things "are" all the colours they look, if one can use the word *are* at all. There is no way of defining a *real* colour in the sense of an inherent colour. Colour, in fact, is not an intrinsic property of a thing but is a property which is relative to an observer.

The difficulties increase if we take into account that an object can apparently be two or more colours at once, as in the phrase : "This piece of silk *looks* green to me from here, but at the same time it *looks* purple to you from over there". If it *looks* green and purple at once, it *is* green and purple at once. But I am only illustrating, and I will not pursue this argument further.

A visit to the case containing the fluorescent minerals at the Natural History Museum at South Kensington gives a vivid impression of the relativity of colour. As one turns on the ultra-violet light, the dull, grey minerals glow out into vivid pinks and blues which force themselves on the mind as being the *real* colours of the minerals.

I am aware that in illustrating the point of view which I am trying to suggest by the case of colour, I am laying myself open to attack ; for it will be said that such characteristics of objects as shape, size and position do not share the relativity of colour, but are absolute characteristics of things. I do not believe that this is ultimately true, but have no space in which to vindicate my belief. I must content myself by stating that the relativity manifested by colour is, in my opinion, in the last analysis, general to all the characteristics

of the world of normal sense-perception. Lack of space forces me to be dogmatic. But suppose, as an illustration, that air happened to have a double index of refraction like Iceland Spar, and our visual world was a kind of inebriate's paradise in which everything appeared double. Should we know that physical objects were in reality single? At first sight it would seem as if our sense of touch would tell us so, but as the two senses would have been evolved together, it is much more probable that every feeling of contact would also be double. Rare cases in which things looked or felt single would be put down as illusions.

Relativity permeates the world of our senses, I maintain, through and through, although in our capacity of workaday, percipient beings, Nature carefully arranges to hide this fact from us. She is a great Pragmatist, and puts practical considerations first. Consciousness of the self is, I think one might almost say, *studiously* kept out of the perceptual situation. This is so much the case that it gives one the impression of a teleological factor at work in the staging of the sensible world. In sense-perception, one's perceptual consciousness is projected from one like the light of a searchlight. One is like a man driving a motor-car in the dark, who is barely conscious of himself or the machine, but whose whole attention is projected forward onto the illuminated road ahead. There is, of course, an excellent reason why this should be so in the case of the motorist: but, may there not be an equally good practical reason why it should be so with normal sense-perception. What utter confusion would result if that sense of complete independence were lacking from the external world. Remember that Nature formed our senses for the kind of life in which a man lay down at night with his hand on his spear and his ears alert for the slightest sound of approaching danger. It is an interesting question to ask ourselves whether, after some thousands of years of comparative safety, we are not now witnessing the beginnings of a higher mode of cognition.

But, as I said, although our perceptual consciousness stoutly denies that we have had any hand in shaping the world of our senses, philosophical analysis shows that we have; only the fact is carefully screened from us. This is the teleological factor to which I referred. Does it not look as if we were perceiving, not so much the world as it actually is, as a specially prepared version of the world which somebody or something *wants* us to perceive? Can it be mother Nature, drawing a blackboard picture for her class in simple outlines? That is what I think the facts suggest, although my present little sketch gives a very inadequate hint of it. Nature wants us to believe that the world of our senses is an absolute world, existing in entire independence of ourselves. But in truth it is neither.

I will now leave the philosophical argument and go on to ask what physical science has to say about the plain world of our normal senses. In its early stages, physics was entirely at one with the world of the senses, which it accepted as a going concern. Its business was merely to examine this world as accurately as possible. Its policy was experiment : its philosophy empiricism. After its career of two or three centuries, has it vindicated the account of external reality given by our perceptual consciousness ; or has it followed in the footsteps of philosophy in diverging from it ?

To glance back first at the account which the senses give of colour, can there be any doubt about their verdict ? Colour in perception is given as being spread out over the surface of the coloured object. The red colour of the pillar-box is given as being spread out over its cylindrical surface away from us at a distance in space. I do not see how anyone can possibly dispute this. It is simply a fact. The colour is given as *there*, and there is not the slightest suggestion that we have anything to do with its being there. As given in perception, it is perfectly independent of us. If you were to stop the casual passer-by in the street and ask him if he had anything to do with the pillar-box being red, what would he say ? I think the best you could hope for would be the reply that he was not the Post Office employee who painted pillar-boxes ; but he would be more likely to think that you were insane and hurry on. But the proposition which science maintains is, exactly, that the percipient has everything to do with the pillar-box being red. It states that colour does not exist at all in the external world. Its explanation of colour runs something like this. The surface of the pillar-box consists of molecular groups of bonded atoms, each atom being something like a minute planetary system of electrons circulating about a positive nucleus or proton. The theory of the atom has advanced a long way beyond this now, but that does not affect the present principle. The vibrating systems absorb part of the incoming light-energy and reflect part. That part which is reflected from the pillar-box has the particular frequency belonging to the red part of the spectrum. Some of it enters the observer's eye, and is focussed by the lenses of the eye to form a tiny image on the retina. The rods and cones of the retina under this image are stimulated to send nervous impulses along the optic nerves to a particular part of the cortex of the brain. Certain physiological changes take place in the stimulated portion of the cerebral tract. But now the causal sequence is broken. A hiatus occurs. Without any intelligible link or go-between, a determinate form of conscious awareness is experienced by the percipient—a "sensation" occurs. He *senses* a red, oblong patch. This is the first point at which the redness of the pillar-box appears in the physicist's

explanation. And now that the redness does appear, it is not something in the external world as the redness of perception certainly is. It is *sensation*—a *feeling*.

The whole of this scientific explanation of colour may be summed up in two words. It is a *causal theory*. Science states that the pillar-box is not red, but that it *causes* red. And so with the other qualities of material objects. The ice is not cold; it *causes* cold. The table is not smooth; it *causes* a smooth feeling, and so on. The inevitable tendency of physics is to pass from the claimed independence of the world of the senses to a dependent world relative to the observer. Let me quote a rather well-known passage from Professor A. N. Whitehead's *Science and the Modern World*, which puts this fact in striking terms. He is speaking of Locke and the philosophy of science which arose towards the close of the seventeenth century. "But the mind", he says, "in apprehending also experiences sensations which, properly speaking, are qualities of the mind alone. These sensations are projected by the mind so as to clothe appropriate bodies in external nature. Thus the bodies are perceived as with qualities which in reality do not belong to them, qualities which in fact are purely the offspring of the mind. Thus nature gets credit which should in truth be reserved for ourselves: the rose for its scent: the nightingale for its song: the sun for his radiance. The poets are entirely mistaken. They should address their lyrics to themselves, and should turn them into odes of self-congratulation on the excellency of the human mind. Nature is a dull affair, soundless, scentless, colourless; merely the hurrying of material, endlessly, meaninglessly. However you disguise it, this is the practical outcome of the characteristic philosophy which closed the seventeenth century. . . . It has held its own as a guiding principle of scientific studies ever since. It is still reigning. Every university in the world organises itself in accordance with it. No alternative system of organising the pursuit of scientific truth has been suggested. It is not only reigning, but it is without a rival. And yet—it is quite unbelievable."

Now, whether it is believable or not, the important point is that if we take these two accounts—the sensuous account and the scientific account—at their face values, they are in flat contradiction with one another. There can be no question at all that our perceptual consciousness places the scent in the rose, the song in the nightingale, and the radiance in the sun; and, for that matter, the red colour on the pillar-box. But physics denies that they are there, and places them in our minds instead. Is there any way of reconciling these two diverse points of view? As far as I can see there is none, so long as both these explanations claim to be *ultimate*. But, if one or both of

them, will agree to accept a relative status, then the two might be reconciled.

Space will not allow me to consider the alternatives in which : (a) the world of the senses is taken to be ultimately real while the world of physics is the appearance of it which the scientific observer attains, or (b) the world as disclosed by physics is ultimate reality, while the world of the senses is a relative appearance of this to the ordinary observer. I have given reasons for rejecting (a) ; and, as for (b), how can a world-outlook which is in such a state of flux as modern physics pose as ultimate and unchangeable truth ? The only alternative remaining is that neither our senses nor the physical scientist is putting before us the world of being as it ultimately is. Both these views are strictly relative to man, who, if he is shaped by the universe he inhabits, in turn shapes that universe in the act of cognising it, in the sense that, what is present to his consciousness is not identical with the character of the thing itself, but is a picture which represents it, and is peculiarly adapted to him.

Physics does not support the given character of the world of sense-perception any more than philosophy does.

It shows it as infected through and through with relativity. We need not consider the modern developments of the subject ; they have only left the world of the senses still further isolated. In the words of Professor Max. Planck, " as the view of the physical world is perfected, it simultaneously recedes from the world of sense ". The one world is now no longer recognizable in the other. And this mutual divergence is a sure indication that both of these worlds are abstractions.

Let me briefly recapitulate. I have been speaking so far of the world of normal sense-perception, and I have rapidly glanced at it from each of three different points of view. First, from the point of view of the entirely unsophisticated person whose world is the one which he sees, and feels, and hears by his unaided senses. His world was entirely satisfactory, succinct, clear-cut, without problems or difficulties for him. It claimed to exist in entire independence of him and to be absolute and final. Secondly, I looked at the same world from the point of view of the philosopher. We saw the philosopher take it up as an already solved problem and try to analyse it, and we saw it disintegrate under his criticism and open out like the mathematician's divergent series. I illustrated this process by taking the case of colour, although I maintained that the principle was quite general. We found that the world of sense lost its independence under analysis and became riddled through and through with relativity, involving the percipient as an efficient and indispensable factor in determining the nature of that world itself.

Thirdly, I looked at the same world again through the eyes of physical science, and found that its examination did the same kind of thing that philosophy had done, viz. unravelled the world of the senses and fastened its qualities upon the observer—in a word, introduced the same element of relativity, which reduced the sensible world from being the universe as it *is* to being a picture of the universe as it *seems*.

Now, I want to make it quite clear that in advocating this relative view of the world of normal sense-perception I am not advocating any form of Subjective Idealism. I am not asserting that the mind *creates* its world. My point of view is fundamentally realist. I am saying that both science and philosophy, if we interpret their dicta rightly, point to the world of our senses as being an *aspect* of something which lies behind it—an aspect which has been peculiarly adapted by nature to the practical needs of humanity, and indeed of all living animals. The idea, of course, is a very old one, which has been familiar since at least the time-honoured illustration of Plato's cave, wherein the chained men watched the shadows of real events passing along the back. And I shall be reminded of Kant's noumenal and phenomenal worlds; and also, perhaps, of the saying which Dr Inge often quotes from the Quaker Mystic, Isaac Pennington: "Every truth is Substance in its own place, though it be but Shadow in another place. And the Shadow is a true Shadow, as the Substance is a true Substance". But I believe that this view has a special relevance to the subject which I am now going to discuss, viz. that of Supernormal Perception.

I am not, of course, in my present argument making any attempt to disclose the nature of the ultimately real—the Substance which we see as Shadow. I am merely maintaining that, whatever the nature of the ultimately real may be, the world of our normal perceptions is not identical with it. And I suggest that this point of view makes the facts which we already know about supernormal perception most nearly intelligible to us, and it should tend to mitigate the attitude of *a priori* scepticism with which these facts are generally met.

I now turn to supernormal perception, using the terminology with which we are all familiar. This is subdivided into three headings, viz. Telepathy, Clairvoyance and Precognition. I am not sure that I should not add a fourth heading, viz. Retrocognition. But genuine retrocognition (as distinct from revival of lost memories) is a very difficult subject, and I am not even sure whether it is possible in principle to obtain any evidence of it, so I will not speak of it. I need not define these three types of cognition here, and as I am treating the subject from a theoretical standpoint, I shall say very little on the question of evidence. There is, of course, evidence in

the Society's records, as well as elsewhere, of all these three types of supernormal perception. A good deal of that for telepathy and clairvoyance is undifferentiated. An illustration of this undifferentiated evidence is provided by the Ossowiecki case reported in the *Proceedings* for December, 1933. This might have been a case of telepathy, or it might have been a case of clairvoyance, or it might have been a case of both together. The usual tendency to give telepathy the preference in such cases as this is, as far as I can see, no more than a prejudice. Thus there is a great deal of evidence for telepathy and clairvoyance combined, but we do not know how to apportion most of it between the two.

The evidence for precognition, though less in amount, is nevertheless considerable. This evidence, as far as this Society is concerned, was recently collected and sifted by Mr H. F. Saltmarsh and published in his important paper in the *Proceedings* in February, 1934. It will be remembered that, after rejecting the weakest cases, the author was left with a total of 281 cases, which he classified into grades of different evidential value. No one has yet taken all these cases and shown that any alternative explanation to that of precognition disposes of them more reasonably; and as this is the only way in which the *prima facie* explanation of precognition can be displaced, this explanation so far stands unchallenged.

I will assume, then, that Supernormal Perception exists in all three of its branches. Can we explain it?

Now, explanation means description in terms of the familiar. If we can explain supernormal perception, we have established this very important fact, viz. that normal and supernormal perception belong in some way to the same order of things—the order with which we are familiar. Both can be described in the category of familiar concepts. If, on the other hand, we cannot explain supernormal perception, although the converse need not be true (for our failure might be due to insufficiency of data and ignorance) yet it *might* be true. That is to say, the failure to find an explanation might mean that normal and supernormal perception belong to two entirely different orders of existence.

Now, with regard to clairvoyance, is there any way in which we can explain it in terms which are familiar to us? Some percipient becomes cognisant of the design on a card as it lies in the middle of a shuffled pack, or perhaps of words on a certain page in a book as it stands closed amongst other books on the library shelf. Can we conceive how such a thing can happen?

The first thing towards which our mind turns is some form of radiation hypothesis, because science explains certain of our modes of normal, sensory perception by means of radiation. We become

aware of a distant star, of the warmth of a fire or the ringing of a bell by one or other kind of radiation which travels through space from the cognised object to ourselves. Can it be that some form of radiation passes from the cognised design on the card or from the print in the book, through the intervening objects to our brain or nervous system and there in some way records itself and affects our consciousness? A little reflection is, I think, sufficient to dismiss such an explanation entirely.

Suppose it is a book-test, for example in which telepathy is excluded. The percipient gets to know certain words on a particular page in a closed book in a shelf of books. Some sort of radiation must be given off by the ink. It must pass through the paper, the binding, the wood of the book-case and the walls of the room if these happen to intervene between the print and the percipient. If this was what was happening, it would first of all make a difference in what direction the percipient stood. In line with the page he would get next to nothing: at right angles to the page he would get the best result. The nearer he approached the book, the stronger the impression would become; and if we ever came across a percipient who could read a page of a book a mile away, we could be pretty sure of getting amazing results by holding a book against the back of his head. None of these things show any signs of happening. But the worst difficulty a radiation theory has to meet is that if the particular letters in which the percipient was interested were giving out this radiation, every other letter on every other page, not only of that book, but of all the other books in the book-case, would be giving it out too, and as Professor Rhine says in his book on *Extra-Sensory Perception*, the result would be "one great, unanalysable splotch".

Then there is the question of distance which applies both to telepathy and clairvoyance. The intensity of any physical transmission varies inversely with the square of the distance from the source. This is the only conceivable way in which energy could spread from a given centre through three-dimensional space. Let us try to realise what this inverse-square law means. Take the most powerful lighthouse on the English coast—the Lizard. Its light is directly visible to a distance of about eighteen miles. Imagine its power increased until the light could penetrate 1800 miles; what would it be like to come up and look into the lens? Or if the steam whistle of an ocean liner were powerful enough to be heard all across the Atlantic, what would it be like to stand about five feet away from it? Now, there are many cases in which telepathy has taken place across the world. If telepathy were a kind of "mental radio," as the popular mind seems to believe, then an agent who could transmit a telepathic message, say, from England to Australia would be a truly terrible member

of society. No one who lived in the same county with him—not even the most unpsychic person—would be able to keep his telepathic messages out; and to enter the same room with him would very likely shatter one's nervous organisation and prove fatal.

The extraordinary penetrability of radiation, which passes through so many obstacles on its way to the percipient, raises the question why such radiation should not also pass through the nervous substance of the percipient without producing any effect.

It seems scarcely worth while to continue the argument against a radiation theory of telepathy and clairvoyance, but there is another argument against it, which is sufficient by itself to put the theory out of court. All meanings which are transmitted by physical means have first to be translated into a code of symbols, which are either a series of noises, or visible marks or gestures. These are radiated by quantitative changes of energy—light or sound or electro-magnetic energy in the case of wireless. These purely arbitrary symbols are then reinterpreted by the percipient who uses his knowledge of what the symbols mean. But there is not the slightest trace in telepathy or clairvoyance of such symbolical encoding or decoding processes. Nor, one may add, is there any trace of the necessary transmitting and receiving organs. The radiation theory may surely be dismissed once for all.

Is there any other theory of physical transmission which will meet the case, which is not a radiation theory? I have never heard of one and I cannot conceive of one.

We are therefore baffled in our attempt to explain telepathy and clairvoyance in physical terms; and that is tantamount to saying that we are baffled in our attempt to explain them at all.

But now let me recall the final result of our examination of the world of normal sense-perception, which, put in a nutshell, stated that the world of the perceptual consciousness is not so much *the* world as *our* world. What assumption have we been making in trying to explain clairvoyance? We have been assuming that the clairvoyant is getting in touch with a physical object, the physical card or the physical book. But the conclusion summarised above suggests that we are on the wrong track in doing this. The card and the book are sensible objects belonging to *our* world: they are not bedrock entities at all. They are highly specialised *pictures* of something else, and it is not in the least probable that the percipient is getting in touch with what we call “physical objects” themselves. He is getting in touch with whatever it is that lies *behind* physical objects, and if he gets a physical picture of the card, etc., it is because the impulse has taken this form on emergence. The clairvoyant's original cognitive impression is not derived from a physical object.

The truth is that the physical object and the clairvoyant impression are both derived from the same source. When we ask how the clairvoyant's information is derived from the eard, we are asking the same sort of confusing question as : 'How can the back view of a house be derived from the front view?' The answer is : 'it cannot'. The front view and the back view are independent of one another, but are both derived from the same source, which is the house itself. So that, if supernormal perception be a fact at all, it looks as though we are not dealing with any mere extension of normal sensory faculty on the lines of hyperaesthesia or some extended sensitiveness to electromagnetic or corpuscular radiation or anything of that sort. Normal sensory perception is a highly specialised kind of thing. What we are dealing with lies outside it altogether. In the long run, if this view is substantiated by experiment, it looks as if there can be only one result. The tendency will be more and more to show the world of normal sense-perception as something highly specialised, particular, abstract and relative instead of as we always tend to regard it, universal, concrete and absolute. If this latter were the truth, then the relation between the normal and supernormal worlds would show itself as that of part to whole, and the two would blend and show continuity. But they do not. We pass in a single step from the fully known world of the senses to something entirely inexplicable : and what can this mean except that the relation between the two worlds is not that of part to whole but that of aspect to entity, or, if you prefer it, of appearance to reality ?

I need scarcely point out that if clairvoyance does not take place from card to brain, neither does telepathy take place from brain to brain. If the eard is only the sense-version of something else, then the brain is only the sense-version of something else too—and what can that something else be except some grade of the complex human personality ? The reason why the messages seem to ignore space is because space itself is only a part of the sense-picture.

Of precognition I have no time to speak. It plunges us, of course, at once into the controversy between determinism and libertarianism. And if we seek to delve into that, we find ourselves involved in the most extraordinary welter of paradoxes. But this I would say, that it ought to be possible to prove the existence of clairvoyance and precognition quite definitely by means of fairly simple experiments, and that to establish them as facts would be of the greatest importance. In fact, psychical research has an advantage over all the physical sciences in that they are entirely dependent on the data of normal sense-perception : nothing they discover can be truer than the data of the senses. But psychical research has access to another mode of cognition, and this is like getting a cross-bearing on the

whole range of scientific knowledge. It stands in a unique position among the sciences and at the same time enters the metaphysical problems of philosophy. But it does not approach these from the speculative point of view alone, for psychical research always retains its empirical basis. It is so important, occupying as it does, the crucial centre from which the most important human problems radiate, that one wonders why all the universities in the world have not long ago pooled their resources in an attempt to exploit its possibilities. I suppose that if the pursuit of knowledge were organised on a speculative and not on a commercial and utilitarian basis, this would have been done. But even as it is, one would think that a society for psychical research which strenuously pursues the experimental side and as strenuously advertises the theoretical importance of its results, ought to have great opportunities before it.

REVIEW

LIEUT.-COLONEL ROBERT HENRY ELLIOT, M.D., Sc.D., F.R.C.S.,
The Myth of the Mystic East. 8vo, pp. 301. London, Wm.
Blackwood & Sons, Ltd. 1934. Price 7s. 6d. net.

Colonel Elliot has written an extremely interesting book, the least fortunate part of which is perhaps the title. His long residence in India as an Officer of the Indian Medical Service and his familiarity with conjuring in Europe have made him very critical of claims that there exists in India a "magic" of a different kind from that practised by conjurers in European countries, a "magic" depending on "supernormal" control of natural forces.

At several points he touches matters which have come under the consideration of our Society, notably the "Indian Rope Trick". He emphasizes the fact, well known to our members, that it is impossible to get first-hand evidence, which will bear examination, of the trick ever having been performed as described, and rightly adds that until such evidence is forthcoming it is idle to theorise as to how a thing happened which there is no reason to believe ever happened at all. He subjects to detailed criticism various testimonies in favour of the trick having been performed, and suggests that, where the bona fides of the narrators is beyond suspicion, they are confusing in their own minds traditional descriptions of *this* trick with memories of a quite different feat, which they had themselves observed, namely, balancing on the top of a long bamboo pole. The whole of Chap. III of this book is devoted to this subject.

Other famous tricks, such as the mango trick, receive similar

attention. Colonel Elliot draws attention to the very different conditions under which Indian and European magicians work, and particularly to the fact that whereas in the West great value is paid to novelty, in the East the magician simply seeks to do the old tricks over and over again with increasing slickness. This, coupled with the fact that the practice of magic, as so many other things in India, is hereditary, may very likely result in the Indian magician attaining by normal means a greater degree of manual dexterity than his European rival. He is not, however, subjected to as close scrutiny as the European, and will generally refuse to perform the same trick twice before the same audience.

Colonel Elliot is also incredulous as to the existence of "were beasts", of which creatures reports from time to time reach the S.P.R. He has, in connection with medical work, made a special study of venomous snakes, and the latter part of the book contains a mass of information, which, if not very relevant to our subject or to the earlier portion of the book, is of considerable interest about snakes, British and foreign, venomous and non-venomous.

The reader who approaches Colonel Elliot's book from the angle of psychical research may occasionally be irritated by his dogmatism on one or two matters where an open mind is desirable, e.g. the possibility of mass hallucination, but taking the book as a whole he will find a great deal to enjoy.

W. H. S.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION.

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

The Annual General Meeting of the
Members of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 27th February, 1935, at 5.30 p.m.

Tea will be served from 4.45 p.m.

FORTHCOMING MEETINGS

March 27. "Further Studies in Trance Personalities." By Mr W. Whately Carington.

May 29. "Useful Ghosts." By Miss Ina Jephson.

Owing to the date of Easter there will be no meeting at the end of April. A meeting is being arranged instead for early in May, and a further announcement of this will appear later.

THE INVESTIGATION OFFICER

AT Mr Besterman's request the Council have agreed to treat his appointment as an Officer of the Society as terminated as from the 22nd January. The Council wish to place on record their appreciation of the many services rendered by Mr Besterman to the Society since his appointment in 1929.

NEW MEMBERS

Birley, Mrs Oswald, Corner House, 62 Wellington Road, London, N.W. 8.

Crocker, Lieut.-Colonel H. E., C.M.G., D.S.O., 7 Wrights Lane House, Kensington High Street, London, W. 8.

Harding, Norman, 18 North Common Road, Ealing, London, W.5.

Mace, C. A., M.A., 50 Avenue Road, London, N.W. 8.

Miller, G. B., Brentry, Romsey, Hants.

MEETINGS OF THE COUNCIL

THE 325th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 9 January 1935, at 3 p.m., THE PRESIDENT in the Chair. There were also present: Professor E. R. Dodds, Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., Mr W. H. Salter, Mrs W. H. Salter, and the Rev. C. Drayton Thomas; also Miss Isabel Newton, Secretary.

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

Five new Members were elected. Their names and addresses are given above.

The Report of the Council for 1934 was considered and adopted as amended.

It was decided to hold the Annual General Meeting on Wednesday, 27 February, at 5.30 p.m.

THE 326th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 30 January 1935, at 3.30 p.m., THE PRESIDENT in the Chair. There were also present : Professor C. D. Broad, Sir Ernest Bennett, M.P., Professor E. R. Dodds, Mr Oliver Gatty, Mr Gerald Heard, Miss Ina Jephson, Sir Lawrence Jones, Bart., Mr G. W. Lambert, Mr W. H. Salter, Mrs W. H. Salter, the Rev. C. Drayton Thomas and Miss Nea Walker ; also Miss Isabel Newton, Secretary.

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

Two new Members were elected.

The Council at Mr Besterman's request agreed to treat his appointment as an Officer of the Society as terminated as from the 22nd January. The Council wish to place on record their appreciation of the many services rendered by Mr Besterman to the Society since his appointment in 1929.

Mr W. Whately Carington was appointed Myers Memorial Lecturer for 1935.

ANNUAL REPORT OF THE COUNCIL FOR 1934

(1) *Research : Parapsychical Phenomena.* Mr Whately Carington has actively pursued the investigations with word association tests referred to in the last Annual Report. With the co-operation of the Rev. C. Drayton Thomas and the Rev. W. S. Irving he has obtained further material from Mrs Leonard, her control "Fedra", and various "communicators", and has analysed this. Some members of the Society have also kindly consented to be subjected to these tests in their normal state, and in one case also in trance condition, and Mr Carington has analysed the results.

In view of the novelty of these experiments, as applied to psychical research, and the importance of the results which may very possibly be obtained from them, the Council has purchased an efficient Calculating Machine, without which the analytical work would be impossible, and has lent it to Mr Carington for use by him in the further work which he is actively pursuing on the same lines. Mr Carington in April read a report (published in Part 136 of *Proceedings*) on the work he had so far done, and further reports from him are awaited with interest.

Several of our members have been conducting experiments in paragnosis of various kinds. The work of Dr J. B. Rhine, now the Honorary Research Officer of the Boston Society for Psychic Research and a member of our Society, on extra-sensory perception is

of great interest. In it he has emphasised the importance of conducting experiments in such a way that results attributable to any one particular form of paragnosis shall be distinguishable from results attributable to other forms, and this is being borne in mind by the experimenters of our Society.

(2) *Paraphysical Phenomena.* The sittings with Rudi Schneider mentioned in the last Report were continued after the New Year until Easter. A feature of the experiments was the use of various new types of appliances. This was rendered possible by the generosity of Mr Oliver Gatty, who took a large part both in financing and in organising the 1934 sittings. The appliances used were described by Mr Gatty at a meeting of the Society in May, and a report on the sittings by the Investigation Officer and Mr Gatty was published as Part 137 of *Proceedings*. No definitely supernormal phenomena were observed at the sittings, but the careful application of a rather elaborate mechanical system of control and recording must be of assistance in future experiments with this type of mediumship.

After the medium had completed his sittings for the Society, he gave some sittings to Lord Charles Hope, who has reported on them in Part 138 of *Proceedings*.

(3) *The B.B.C. Lectures.* Towards the end of 1933 the British Broadcasting Corporation invited several members of the Society to take part in a series of wireless "talks" entitled "Enquiry into the Unknown", which the Corporation was organising. Ten members of the Society accepted the invitation and "talks" were delivered by them in January to March 1934. They aroused great interest among the general public and, in response to invitations issued by some of the lecturers, many spontaneous cases of phantasms of the living, haunts, precognition, etc., were brought to the Society's notice. The "talks" have been reprinted in an expanded form, *Enquiry into the Unknown* (Methuen & Co.), and Mrs Salter has also published some of the cases received by her under the title *Evidence for Telepathy* (Sidgwick & Jackson, Ltd.). The President and Mrs Salter read papers in March and September, giving an account of some of the cases of precognition and telepathy that they had received.

(4) *Donation to the Society.* In February a member, who wishes to remain anonymous, presented to the Society through Mr Gerald Heard a donation of £1,000, to be expended by the Council on research. The Council wish to record their most hearty thanks for this donation, which is the most substantial donation towards research that the Society has received for many years.

The donor authorised Mr Heard to discuss with the Council the

particular lines of research in which he was most interested and which he thought could profitably be pursued, and a programme to be undertaken as soon as possible was accordingly drawn up by the Council in conjunction with Mr Heard, who was co-opted a member of Council.

(5) *The Investigation Officer's Visit to America.* It was part of this programme that the Investigation Officer should visit the United States and Canada for the purpose of observing and reporting on several pieces of research being carried on in those countries, especially Dr Rhine's experiments in extra-sensory perception at Duke University, North Carolina, and Dr Glen Hamilton's investigations at Winnipeg. The Investigation Officer suggested that the visit might conveniently be extended to Brazil, so that he could have an opportunity of visiting the Brazilian medium, Mirabelli, members of the Society having offered to give him an introduction to that medium and to contribute to the expenses. The Council accordingly authorised the Investigation Officer to undertake a tour first of all to Brazil to investigate Mirabelli, then to the United States and Canada. An estimate of the cost of the tour was made, and with the donor's concurrence the sum of £350 out of the donation of £1,000 was allowed by the Council for the expenses of the tour.

Mr Besterman left England for Rio de Janeiro on the 13 July. Through the kind introduction of a member of the Society residing in Brazil, he was able to obtain several sittings with Mirabelli at which he observed interesting phenomena. Towards the cost of this investigation Miss May Walker kindly gave £25. The Council wish to take this opportunity of thanking her for her generous gift.

From Brazil Mr Besterman went to New York, where he stayed about three weeks. From there on the 18 September he wrote to the Hon. Secretary announcing that he had been seriously ill and that he was returning to England in a few days' time. He arrived in England about the end of September, and on the 8 October wrote to the Hon. Secretary tendering his resignation of the post of Investigation Officer as from the expiration of the six months' notice required by his agreement with the Society.

The Council felt obliged to accept Mr Besterman's resignation as from the expiration of six months from the date of the letter in which he tendered it, also, in view of a recurrence of his illness, to grant him temporary leave of absence.

(6) *Research Committee.* The resignation of the Investigation Officer necessitated a revision of the Council's arrangements for research. The Council decided to appoint a Research Committee to advise them as to the future organisation of research and to initiate

ACCOUNTS OF RECEIPTS AND PAYMENTS FOR THE YEAR ENDED 31 DECEMBER 1934.

Dr.

GENERAL FUND.

Cr.

<i>To Subscriptions:</i>				
Members (1931)	-	-	£2 2 0	
(1932)	-	-	4 4 0	
(1933)	-	-	11 3 0	
(1934)	-	-	820 7 9	
(1935)	-	-	44 2 0	
	-	-	5881 18 9	
Associates (1933)	-	-	£3 18 6	
(1934)	-	-	228 3 6	
(1935)	-	-	3 3 0	
	-	-	235 5 0	
Life Members	-	-	43 19 6	
Donations:				
Towards Printing Expenses	-	-	13 12 0	
General	-	-	4 1 10	
	-	-	17 13 10	
Sale of Publications:				
Per Secretary to Members	-	-	25 0 11	
Secretary to Public	-	-	68 1 7	
F. W. Faxon Co.	-	-	31 4 6	
	-	-	124 7 0	
Rent from Society's Tenant	-	-	250 0 0	
Interest on Investments	-	-	219 11 0	
Sale of Transactions of International Congress	-	-	0 13 9	
Reproduction of Drawings from Proceedings	-	-	2 2 0	
Legacy	-	-	100 0 0	
Sale of £161 3½% War Stock	-	-	165 0 0	
Library Fund Account Transferred	-	-	43 16 5	
	-	-	5881 18 9	
	-	-	158 5 2	
	-	-	331 5 10	
	-	-	489 11 0	
Binding Proceedings and Journal	-	-	-	18 2 2
Postage	-	-	-	95 0 2
Salaries:				
Secretary	-	-	-	275 0 0
Assistant Secretary	-	-	-	141 15 0
	-	-	-	416 15 0
Pension to Miss Alice Johnson	-	-	-	129 0 0
Balance of Grant to Research Fund 1933	-	-	-	20 2 0
Library, Purchase of Books and General Upkeep	-	-	-	20 17 4
Rent	-	-	-	175 0 0
Rates	-	-	-	35 2 7
Fuel and Lighting	-	-	-	57 4 2
Caretaker's Wages and Uniform and Cleaning Expenses	-	-	-	145 18 7
Repairs	-	-	-	52 9 7
Furnishing	-	-	-	1 0 11
Expenses of Meetings	-	-	-	13 13 5
Stationery	-	-	-	23 0 4
General Printing	-	-	-	10 2 6
Insurance	-	-	-	16 13 6
Telephone	-	-	-	15 6 1
	-	-	-	576 17 6

MEMORANDUM OF ASSETS.

GENERAL FUND.

£58	11	2	2½%	Annuities.
£62	19	0	24%	Consolidated Stock.
£219	8	7	31%	Conversion Stock.
£588	17	8	34%	War Stock 1952.
£800	0	0	York Corporation 3%	Redeemable Stock 1916-41.
£250	0	0	New South Wales 5%	Stock 1935-55.
£1,200	0	0	Nigeria Government 3%	Inscribed Stock 1955.
£937	0	0	London, Midland and Scottish Railway 4%	Debenture Stock.
£562	0	0	London, Midland and Scottish Railway 4%	Preference Stock.
£1,540	0	0	East Indian Railway Irredeemable	Debenture Stock.
£520	0	0	East Indian Railway Deferred	Annuity.
£100	0	0	Prescot Gas Co. 4%	Preference "C" Stock.
225	Consolidated Ordinary Shares of £1 each fully paid of the Prescot Gas Co.			
300	Deferred Shares of 5s. each fully paid of the South Staffordshire Tramways			
	(Lessec) Co. Limited (in voluntary liquidation).			

ENDOWMENT FUND.

£1,460	0	0	0	4% Consolidated Stock.
£908	0	11	India 3½%	Stock 1931.
£550	0	0	India 4½%	Stock 1958-68.
£161	11	6	New South Wales 5%	Stock 1935-55.
£1,797	0	0	London and North Eastern Railway Co. 4%	Debenture Stock.
£1,055	0	0	Great Western Railway Co. 5%	Rent Charge Stock.
£800	0	0	Great Western Railway Co. 5%	Guaranteed Stock.
£2,258	0	0	London, Midland and Scottish Railway Co. 4%	Preference Stock.
£514	0	0	London Passenger Transport Board 4½%	"T.F.A." Stock.
£260	0	0	East Indian Railway 3½%	Debenture Stock.
£1,260	0	0	East Indian Railway 4½%	Irredeemable Debenture Stock.
£700	0	0	Great Indian Peninsular Railway 4%	Debenture Stock.

F. W. H. MYERS MEMORIAL FUND.

£750	0	0	5%	Conversion Stock 1944-64.
£250	0	0	3½%	Conversion Stock 1961.

We have examined the above Accounts and compared them with the Society's Cash Books, Receipt Books, and Vouchers, and certify that they are in accordance therewith. We have also verified the investments of the General, Endowment and Myers Memorial Funds as set forth in the above Statements.

70A Basinghall Street, London, E.C. 2, 29 January 1935.

HARDENS, WILKINS, AVERY & FLEW, Chartered Accountants.

and carry out such research work as was practicable in the immediate future. The new Committee consists of the President (*ex officio*), the Hon. Editor, Dr T. W. Mitchell, Mr Whately Carington, Mr Gerald Heard and Mr Oliver Gatty. This Committee has met and has reported to the Council that it was desirable that a salaried official, to take office from the expiration of Mr Besterman's appointment, should be appointed, to work in conjunction with a Research Committee. After consulting leading psychologists at the Universities, and other persons whose opinions might prove of value, they have considered the names of possible candidates, and also the lines of research which could be most advantageously followed up at the present time.

(7) *The Presidency.* The Council elected the Hon. Mrs Alfred Lyttelton, G.B.E., for a second term of office.

(8) *Changes among the Hon. Officers and Officers of the Society.* In May the Council appointed Miss Nea Walker as Hon. Editor of the *Journal*. The Council wish most cordially to thank Miss Walker for having undertaken this considerable task. Although Miss Walker gave her consent to the appointment on the understanding that it was a temporary arrangement, they hope that it may be a long time before she asks for other arrangements to be made.

Miss Newton, who has been with the Society thirty-one years, informed the Council that since her illness she was beginning to find the office hours rather long. After discussion with her and Miss Horsell the Council arranged that Miss Horsell, who has consented to revert to full-time work, should take over the routine work of the office, Miss Newton remaining Secretary of the Society and attending at the office for half-time only. The Council wish to place on record their warmest appreciation of the services already rendered by Miss Newton.

(9) *Changes on the Council.* In April Mr Oliver Gatty, Mr Gerald Heard, and the Rev. C. Drayton Thomas were co-opted as members of Council.

(10) *New Hon. Associate.* In February the Council, in recognition of the many services which the Rev. W. S. Irving had performed for the Society, especially in connection with his investigations of Mrs Leonard, elected him an Hon. Associate of the Society.

(11) *Professor Driesch's Lecture.* In October the Society had the good fortune to hear a paper from Professor Hans Driesch, who was visiting England to lecture at several universities. It is hoped that the paper, the subject of which was "Memory in Relation to Psychical Research", will be printed in *Proceedings*.

(12) *Library.* During the year 83 Members borrowed 431 books,

and 59 books were borrowed by the National Central Library. Several Members availed themselves of the privilege of borrowing books from other libraries through the Society and the National Central Library.

(13) *Obituary*. During the past year the Society has sustained a heavy loss by the death of Dr Walter Franklin Prince, Research Officer of the Boston S.P.R. and a Corresponding Member and ex-President of our Society. Memorial Notices were published in Part 138 of *Proceedings*. Other members of long standing, whose deaths must be recorded with regret, are : Mrs Barkworth, Lady Diekinson Berry, Mrs Burrard, Professor Chattoek, Dr Bernard Hollander, Mrs Monypeny, Judge R. E. Moore, Mrs T. S. Perry, the Dowager Lady Rayleigh, Mr R. G. Shewan, Mrs H. C. Tait, Princess Mary of Thurn and Taxis, Mrs Walker-Munro and Mr Richard Wood. The total number of deaths was eleven Members, nine Associates, one Honorary Member and one Corresponding Member.

(14) *Membership of the Society*. During the year 47 new Members and two Student Associates were elected. The total loss in numbers from deaths, resignations and other causes was 43 Members and 23 Associates, leaving a net increase of five Members and a decrease of 21 Associates. The total membership of the Society now stands at 743.

(15) *Publications*. Six Parts of *Proceedings* were published during the year : Part 133 in January, Part 134 in February, Part 135 in May, Part 136 in July, Part 137 in October, and Part 138 in December.

The Secretary's sales to the general public amounted to £68 1s. 7d., and to members of the Society £25 0s. 11d. The returns from the Society's agent in America amounted, after deduction of commission, to £24 16s. 8d.

(16) *Meetings*. The following meetings have been held during the year :

- 31 Jan. "A Proxy Case extending over twelve sittings with Mrs Leonard," by the Rev. C. Drayton Thomas.
- 28 Mar. "Telepathy and the Listener-in," by Mrs W. H. Salter.
- *25 April. "The Quantitative Study of Trancee Personalities," by Mr Whately Carington.
- 30 May. "An Informal Report on the Society's Investigation of Rudi Schneider," by Mr Oliver Gatty.
- 27 June. "The Fire Walk," by Mr Ernest S. Thomas.
- 25 July. "Experiences in Reciprocal Telepathy," by Dr Carl Bruek.

- 26 Sep. "The Response to a Broadcast Talk on Precognition,"
by the Hon. Mrs Alfred Lyttelton, G.B.E.
- *24 Oct. "Memory in Relation to Psychological Research," by
Professor Hans Driesch.
- 28 Nov. "Normal and Supernormal Perception," by Mr G. N. M.
Tyrrell.

* General Meetings.

REFLECTIONS ON THE MEDIUMSHIP OF RUDI SCHNEIDER

BY THEODORE BESTERMAN

THERE is practically nothing I disagree with in Lord Charles Hope's comments on our recent report on the S.P.R. sittings with Rudi Schneider, beyond the minor points already referred to by Mr Gatty. Our report merely set out facts in connection with a particular investigation, while Lord Charles Hope takes a wider view. His remarks have, however, led me to reconsider once again, from a still wider angle, the whole situation brought about by the history of Rudi's mediumship. My own experience of it goes back to 1928, and I have had sittings with Rudi in the *séance*-rooms of Schrenck-Notzing in Munich, of the Institut Métapsychique in Paris, of Herr Schickl at Weyer, and of the S.P.R., as well as sittings in Rudi's late home at Braunau. In addition I have carefully studied the whole of the large literature produced by this mediumship, together with much unpublished material. I have also had opportunities to question a large number of people of all kinds who have had sittings with Rudi under all sorts of conditions. My reflections on this mediumship may therefore be of some value.

The main point, and the only one I propose to touch on, is this: has Rudi ever produced parapsychical phenomena? It seems astonishing to outside critics that this question has not yet been answered in a definitive way; but there are excellent reasons for this uncertainty, reasons familiar to anyone who understands the conditions under which parapsychical sittings are held. When I am asked whether I have myself seen any phenomena at Rudi's sittings the genuineness of which seems to me beyond doubt, I answer without hesitation "No". But the subject does not lend itself to such categorical affirmatives and negatives. I should prefer to state my feelings in these terms: "While I am not prepared to vouch for the genuineness of any phenomenon I have seen, there are several I am unable to explain in normal terms."

It is of some importance to elucidate the implications of this statement. The obvious answer, an argument I have often used myself, is to say that ignorance of a technique does not imply its non-existence. Because, for instance, I do not know how a fraud is carried out, it does not follow that fraud was not present. This is true, of course, as a general statement, but is again far too sweeping. It would be absolutely true of a conjurer's performance on the stage, where the conditions are entirely in the performer's hands within the obvious limits. It is far less true of a sitting on my premises, under my control, and with a medium unprovided with tools. In these circumstances the obvious limitations are much greater, and it becomes possible to inquire whether inability to imagine how fraud could take place does not conceivably carry with it the extreme improbability of such fraud. Has this limit been reached in the sittings with Rudi? But for one factor I should be somewhat inclined to think that it has. This factor is the extreme fallibility of human testimony. If human testimony, I mean of course the testimony of honest and intelligent people, could be accepted at its face value, doubt about parapsychical phenomena could not possibly exist. This would be equally true if such testimony could be accepted even to the extent of a mere fifty or even twenty-five per cent. But we know that it cannot under the special conditions of a parapsychical sitting. Yet the testimony remains so good and so voluminous that I feel sometimes that a residuum of only five per cent. would clinch the matter.

My final conclusion, therefore, is that the reality of parapsychical phenomena must remain in doubt until further and more elaborate investigations into human testimony are carried out. It may be objected that direct investigation is a more sensible course; in theory, yes, but in practice, I very much fear not. The principle of indeterminacy seems to be of paramount importance here; if parapsychical phenomena exist, nothing seems more certain than their failure to appear when subjected to stringent tests.

CASE

P. 306.

A PREMONITION

Mrs Dunlop, 50B Compton Avenue, Brighton, wrote as follows to the President in February 1934:

“On the night of October the 17th 1931 I dreamed that a man died on the couch of my sitting-room. I did not recognise him altho' I saw him very plainly. I related my dream at breakfast to

members of my family, which was a Sunday. My father-in-law came to supper that night and when starting for his home, he was taken ill and we were unable to move him to bed. *He died on the couch in my sitting-room* on Oct. 19th. He had not been attended by any doctor and apparently only suffered from indigestion. It was totally unexpected."

Mr Besterman wrote to Mrs Dunlop asking for corroborative statements and for independent evidence of the death of her father-in-law on the date in question. Mrs Dunlop, in reply, kindly sent statements from her mother and her husband, and gave permission to use her account. Mrs Dunlop's mother writes as follows :

" March 21 1934
6 Queens Park Terrace
Brighton

" It is quite true that my daughter Mrs Dunlop who was living with me at the time at 45 St Lukes Terrace, Brighton, told me of her dream. I was with her Father-in-law at the time he died on the couch the next day.

(Signed) E. WALTER."

Mr Dunlop writes as follows :

" 50 Compton Avenue
Brighton
22. 3. 1934.

" This is to confirm the dream my wife had respecting the death of my Father. She related it to me the next morning at breakfast and the following day my Father died quite unexpectedly on my couch. We were then living at 45 St Lukes Terrace, Brighton, with my wife's Mother.

(Signed) G. L. DUNLOP."

Mrs Dunlop also stated that the *Brighton and Hove Herald* for 24 October 1931 contained an obituary of her father-in-law. This has been verified ; the notice reads :

" John Dunlop, late of Wilbury-gardens, passed away at address of his son at 45, Luke's-terrace, suddenly, October 19th."

CORRESPONDENCE

CRITICISM OF A RADIATION THEORY AS AN EXPLANATION OF
TELEPATHY OR CLAIRVOYANCE.*To the Editor of THE JOURNAL*

DEAR MADAM,—I should like to express my appreciation and thanks for the very interesting and thought-compelling article on “Normal and Supernormal Perception” by Mr G. M. N. Tyrrell in the January issue of the *Journal*. I only regret that considerations of space necessitated so much compression. Might I suggest that Mr Tyrrell would be rendering a great service to Psychical Research in general and our own society in particular were he to expand his sketch into a lengthier thesis. I sincerely hope he will find it possible to do this and I am sure he would earn the grateful thanks of all our members.

While entirely agreeing with him that telepathy or clairvoyance cannot be “explained” by any “radiation theory”, I do not think that the second argument he advances against such a theory is entirely sound in the way he has stated it. I refer to his point about the intensity of radiation varying inversely as the square of the distance. In some types of radiation other factors than mere distance may intervene and quite mask the effect of distance as such, and so, apparently, torpedo the Law of Inverse Squares completely. Take, for example, the phenomenon of what is generally known as “short-wave wireless”. It is by no means true that the strength of the signal received from a short-wave transmitting station varies inversely as the distance. I did some experimenting in this field a few years ago and the account of one test I made will illustrate my point:

My transmitting station was in Tientsin, China, and using low power (about 50 watts) and a wavelength of 30 metres the same test signals were heard and reported by wireless stations in the following places:

Chefoo	-	-	180 miles distant	No signals audible.
Shanghai	-	-	800 „ „	Signal strength <i>r</i> 6
New South Wales	6,000	„	„ „	„ „ <i>r</i> 3
New Zealand	-	7,500	„ „	„ „ <i>r</i> 5
Argentine	-	-	15,000 „ (antipodes)	„ „ <i>r</i> 7

(Signal strength is roughly graded in wireless by the numerical code *r* 1 to *r* 9. *r* 1 is barely audible. *r* 9 blasts the 'phones.)

Thus the strengths of the signals received in different places seem to contradict entirely the “inverse square” law. The nearest

station got no signals at all (known as "skip distance effect".) The loudest signals were actually received at the furthest possible distance on the earth's surface from the transmitter.

The explanation is, of course, that there are other factors that determine the strength of wireless signals, *e.g.* the wave-length used, the weather, the time of day, the varying absorption effects of different kinds of earth surface, the reflection from the "Heaviside Layer" and other ionisation effects in the upper atmosphere, etc.

Now I imagine that a protagonist for a radiation theory for telepathy or clairvoyance could claim the existence of unknown factors that would, in an analogous way, lead to very contradictory results when the effect of distance was being considered. There might even be some sort of "skip distance effect" or recipients might show sometimes better results when they were further away from the agent or transmitter. I am by no means such a protagonist. After reading Mr Tyrrell's paper I do not think a radiation theory has a leg to stand on. All the more therefore I do not wish there should be an opportunity presented for anyone to rig up an artificial leg because of a too-easy theorising of the probable effect of distance between the Agent or Object and the Percipient.

I am, Yours, etc.,

G. W. FISK.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION.

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 27th March, 1935, at 5.30 p.m.

WHEN A PAPER ON

“THE QUANTITATIVE STUDY OF
TRANCE PERSONALITIES” (CONTINUED)

WILL BE READ BY

MR WHATELY CARINGTON

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

NEW MEMBERS

Howden, Richard A., 27a Cornwall Gardens, London, S.W. 7.

Machin, Mrs., Kenora, Ontario, Canada.

Thurn and Taxis, H.I.H. Prince Alexander of, Loucen, Czecho-Slovakia.

MEETINGS OF THE COUNCIL

THE 327th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 27 February 1935, at 4 p.m., THE PRESIDENT in the Chair. There were also present : Professor C. D. Broad, Professor E. R. Dodds, Mr Gerald Heard, Miss Ina Jephson, Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh and the Rev. C. Drayton Thomas ; also Miss Isabel Newton, Secretary.

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

Three new Members were elected. Their names and addresses are given above.

THE 328th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, immediately after the Annual General Meeting, the retiring PRESIDENT in the Chair. There were also present : Professor E. R. Dodds, Miss Ina Jephson, Mr W. H. Salter, Mrs W. H. Salter and Mr H. F. Saltmarsh ; also Miss Isabel Newton, Secretary.

Professor C. D. Broad was elected President for the year 1935.

Mr W. H. Salter was re-elected Hon. Secretary, Mrs W. H. Salter, Hon. Editor of the *Proceedings*, and Miss Nea Walker, Hon. Editor of the *Journal*. Admiral the Hon. A. C. Strutt was elected Hon. Treasurer.

Committees were elected as follows :

Committee of Reference and Publication : The Earl of Balfour, Professor C. D. Broad, Mr Whately Carington, Sir Oliver Lodge, Dr T. W. Mitchell, Mr J. G. Piddington, Mr W. H. Salter, Mr H. F. Saltmarsh, and Mrs Henry Sidgwick.

House and Finance Committee : Miss Ina Jephson, Mr G. W. Lambert, Mr W. H. Salter and Admiral the Hon. A. C. Strutt.

Research Committee : Mr Whately Carington, Mr Oliver Gatty, Mr Gerald Heard, the Hon. Mrs Alfred Lyttelton, G.B.E., Dr T. W. Mitchell, and Mrs W. H. Salter.

Library Committee : Professor E. R. Dodds, Sir Lawrence Jones, Bart., Admiral the Hon. A. C. Strutt and the Rev. C. Drayton Thomas ; also the Secretary, *ex officio*.

Corresponding Members and Hon. Associates were elected for the year 1935 as follows:

Corresponding Members : Professor Henri Bergson, President Nicholas M. Butler, Dr Max Dessoir, Professor Dr S. Freud, Professor Pierre Janet, Dr C. G. Jung, Count Carl von Klinekowsk, M. Maurice Maeterlinck, Professor T. K. Oesterreich, Dr Eugène Osty, Professor Charles Richet, Dr Rudolph Tischner and Dr Elwood Worcester.

Honorary Associates : Miss H. A. Dallas, Rev A. T. Fryer, David Gow, J. Arthur Hill, Professor R. F. A. Hoernlé, Rev. W. S. Irving, Professor J. H. Muirhead, Professor Charles Sage, Miss F. Melian Stawell, Dr A. Tanagras, Dr Th. Wereide.

ANNUAL GENERAL MEETING

THE Annual General Meeting of Members of the Society was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 27 February 1935, at 5.30 p.m., THE PRESIDENT in the Chair.

The following members were present : Dr Margaret Balfour, Mr E. Batchelor, Miss Carruthers, Mr H. S. Collins, Dr E. J. Dingwall, Professor E. R. Dodds, Mr G. W. Fisk, Miss Fleming, Miss Foot, Professor D. F. Fraser-Harris, Mrs D. F. Fraser-Harris, Mrs Goldney, Mr N. Harding, Miss Jephson, Miss G. M. Johnson, Mr I. B. Nicholl, Dr J. V. Rees-Roberts, Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Mr S. G. Soal, Mr G. M. Thomas, Mr G. N. Tyrrell ; also the Rev. H. Thurston (Associate).

The Secretary having read the Notice convening the Meeting, the Hon. Secretary moved the adoption of the Annual Report, which was seconded by Professor E. R. Dodds and carried unanimously.

In presenting the Accounts for the year, Mr Salter, Acting Hon. Treasurer, dealt with the financial position of the Society. For the second year in succession he had to report a slight increase in the number of members, which indicated that there was a real recovery from the "slump" of 1931-1932. The number of Associates was of course declining, as no new Associates, except Student Associates, were being elected. There was a small shrinkage in the Society's income from investments, and on the American subscriptions owing to the rate of exchange. So far as its ordinary income and expenditure were concerned the Society was holding its own.

It remained true that the Society's ordinary income (mainly derived from subscriptions and investments) after payment of over-

head expenses, printing, etc., left only a small margin for research, and that for any research on a large scale the Society was dependent on special donations. They had received in 1934, several generous gifts, among which he might mention Mr Gatty's gift of money and appliances (and also of his own technical knowledge) and the anonymous donation of £1000. The Hon. Secretary referred to Mr Besterman's American tour, and to the programme of Research undertaken by the Council when Mr Besterman had resigned his post. He also mentioned the redistribution of work in the office and paid a tribute to Miss Newton's work over so many years, congratulating the Society on being able to retain her services, though no longer for full time.

In the discussion that followed Mrs Goldney and Dr Dingwall also paid warm tributes to Miss Newton. Dr Dingwall praised Mr Whately Carington's work and said he was very glad the Council were supporting it. The work was difficult and no sensational results were to be expected, but it was really scientific in aim and method. He also spoke warmly of Mr Besterman's work in re-organising the Library : he had not approved of the American tour, but would like to know what report Mr Besterman had made on Mirabelli.

The President, Mrs Salter, and Mr Saltmarsh gave an account of some of the research work in progress, particularly the psychological study of Leonard records undertaken by Mr Kenneth Richmond, who had great practical and theoretical knowledge of psychology, and the experimental work in extra-sensory perception which Mr Tyrrell was carrying out with appliances specially designed. Hearty approval followed on a reference to the great amount of time and trouble which the retiring President (Dame Edith Lyttelton) had, notwithstanding her important public engagements, devoted to the Society's work during two difficult years. The meeting closed with a vote of thanks to the Hon. Secretary, proposed by Mr Saltmarsh and seconded by Dr Dingwall.

The Chairman announced that there were no candidates for election to membership of the Council other than the six members who retired by rotation. The following six members were re-elected accordingly : Mr W. R. Bousfield, Professor E. R. Dodds, the Hon. Mrs Alfred Lyttelton, G.B.E., Miss Ina Jephson, Dr F. C. S. Schiller and Dr M. B. Wright.

Messrs Hartleys, Wilkins, Avery and Flew were re-elected Auditors for the forthcoming year.

CORRESPONDENCE

To the Editor of THE JOURNAL

MADAM,—May I thank Mr G. W. Fisk for his very kind and appreciative remarks about my paper on Normal and Supernormal Perception ?

The point he raises about the inverse-square law of radiation seems to me to be based upon a misapprehension of what that law actually means. The interesting experience he describes shows how irregularly wireless energy can be distributed about the earth's surface : but surely this does not entitle one to say that this "torpedoes" or "contradicts" the law of inverse-square distribution ! That law does not state that radiant energy *always* varies in intensity inversely as the square of the distance from the source. It means (whether this is explicitly stated or not) that it does so only when the *medium is uniform*. Very often it is not uniform : there are reflecting, refracting or absorptive substances within the space relevant to the transmission. This is the case inside a telescope or a speaking-tube, and no one expects the inverse-square law to be obeyed in such cases as these. And it would be a complete misapprehension of the meaning of the law to say that telescopes and speaking-tubes "contradict" it. The erratic distribution of wireless energy which Mr Fisk describes is only another example on a larger scale of what is happening in an optical or acoustic instrument : and I do not suppose that he or anyone doubts that wireless waves obey the inverse-square law when the medium is uniform.

What Mr Fisk means to suggest in the present case, if I understand him rightly, is that telepathy and clairvoyance may be transmitted by some form of radiant energy for which there exist distorting factors about the world of a similar kind to those which exist for wireless energy. Such an assumption, being an unsupported postulate, cannot be disproved ; but I think the following considerations will show that there is not much in the suggestion.

Set up a short-wave wireless receiver at distances of, say, 10, 100, 500 yards and a mile from the transmitter, and measure the energy in the receiving aerial. The inverse-square law will be approximately verified in practice in spite of the Heaviside layer and other long-distance distorting factors. So with telepathy. Whatever factors there might be which distorted it at long distances, it is pretty safe to say that we should get an unmistakable gradation of effects at quite short ranges, if the transmission were physical. When agent and percipient were sitting with their heads a few inches apart, there would be a far more striking result than when they were in adjoining rooms ; and this would be more striking

again than when they were at opposite ends of the town. In fact, if telepathy were a physical effect, I think it would have forced itself upon the attention of mankind long ago, even though unsought. And the same with clairvoyance. Yours etc.,

G. N. M. TYRRELL.

REVIEWS

J. W. DUNNE. *The Serial Universe*. Pp. 242. London: Faber & Faber. Price 10s. 6d.

In this book Mr Dunne develops further the theory which he originally propounded in his earlier work, "An Experiment with Time". It falls into three parts; in the first he gives a comprehensive and lucid exposition of the theory of Serialism and sets forth the general grounds on which it is based. In the second Mr Dunne further elaborates his thesis and subjects it to the test of general application, he illustrates his points by means of a system of diagrams which certainly add greatly to ease of comprehension.

In the third part he applies his theory to several of the concepts of modern physics, viz. relativity, the quantum theory, Heisenberg's principle of uncertainty, etc. He claims that the view he advocates, viz. an infinite regressive series of times in which the time of one stage becomes an extra spatial dimension of the next, together with an infinite series of Observers, not only fits in perfectly with these concepts but also affords an explanation of them.

It seems to me that it is this section of the book which will give most difficulty to the ordinary reader. Unless one is fairly well versed in the more recent advances of mathematical physics many of the arguments must remain obscure in spite of the really excellent explanations which Mr Dunne gives.

From the special point of view of psychical research the main interest is the author's claim to demonstrate the immortality of the Observer. I cannot help feeling, however, that very few readers will be able to rest a solid belief in survival on the sole foundation of what is, after all, a finely spun web of metaphysical argument. For myself I always have a certain feeling of mistrust for proofs of what "must be"; what I want to know is "what is". Metaphysical argument is always a fragile thing and terribly open to destructive criticism.

As illustrating the uncertainty of conclusions resting thereon it is curious that Mr Dunne in one place employs a line of reasoning which had already been used by McTaggart, but derives therefrom a conclusion diametrically opposite to his, viz. that there is an infinite series of times, whereas McTaggart concludes that there is no time at all.

As regards the main point of all Mr Dunne's work, viz. the theory of Serialism, this is not the place to enter upon any discussion of his reasoning, though I think that I should say that I cannot find myself able to accept it.

However the book is worthy of study by all interested in the more philosophic side of psychical research, and even if lack of special mathematical training may prevent them from appreciating some of the arguments, the main part of the book is so clearly written and the various points so well illustrated that they should have no difficulty in understanding the theory and forming an opinion as to its validity.

H. F. S.

MISS GERALDINE COSTER, *Yoga and Western Psychology, A Comparison*. Cr. 8vo. Pp. 248. London, Oxford University Press. Humphrey Milford. 1934. Price 3s. 6d. net.

It is clear that Psychical Research remains the most refractory of sciences because it is possessed of a double nature. To-day most sciences are becoming unpleasantly aware that their own natures are not pure or single. The observer cannot be kept out of the picture which physics now tries to give of the universe, and those sciences which try to prevent his shadow falling on their virgin whiteness are, it would seem, attempting the impossible. Sooner or later they will all have to confess to a relativity in their descriptions which will cast a penumbra of doubt over their first bright confident certainties. This, however, is little comfort to psychical researchers—to know that even the most detached sciences are getting involved with subjective problems. The intention of those who determined to explore this border-land subject was to bring it under the dominion of science, and they would hardly feel compensated for this failure by seeing all the other sciences lapse toward the border-land from which they hoped to rescue their chosen study.

Yet this seems the present trend of all science, and if it continues it looks as though it will end by making all science as difficult as psychical research, not an alternative which the first researchers in that subject hoped.

Is there any way out? There does seem one possible path, a path in the true tradition of science and yet one of great difficulty. We need not, however, think that it is impossible, for it is the path which all advanced researchers in the other sciences are beginning to survey as the route which may well lead them out of their present impasse. It is not impossible that the same method of detached examination which made the first period of objective science so fruitful, may be

applied with equal success to the instrument of observation as it has been to the subject of observation. We now realise that our psycho-physical organism is such that it cannot present us with data but rather with constructions, with instantaneous concepts rather than with direct unvarnished percepts. As Mr Tyrrell pointed out in his paper read before the Society, the problem of apprehension to-day is far better understood than formerly, and we are beginning to analyse not merely the "objective fact" but how that "fact" was carried into and conveyed to our consciousness. In earlier times people knew, with the assurance of ignorance, that there was only one way of taking in the world, and that any other impressions which might come into consciousness must be hallucinatory, purely subjective. Now, however, with a new knowledge of the capacities of the instrument which we have to employ to give us impressions of the outer world, we are beginning to realise that whereas no impression of the outer world can be free of an element of subjective construction, so no impression in the mind can be dismissed as being wholly caused by and wholly explicable through subjective elements. As the astronomer when he wishes to see other stars than those at present in the telescope's focus alters the aperture, so it is possible that we may alter the aperture of the mind if we wish to concentrate on certain conditions which are not those observed, and only necessary to be observed, in the economic world, in the task of getting daily bread. Indeed it seems as though we must explore how this may be done if we are to advance any further in our subject.

It is this consideration which makes Miss Coster's book of importance to psychical researchers. Here is an attempt to link on to our present knowledge of the working of the mind the "field work" done for centuries by Indian introspection. The Indian work could not be seriously considered until our own psychology by its discovery of the subjective mind had found out how deep lie the sources of action in the psycho-physical organism, how powerful they are and what an elaborate, patient and strange technique is required if the doors are to be opened and access to the power house obtained. This book is short, clear, concise, though dealing with a subject which all who have read in it may be forgiven for feeling could never attain to any of these desiderata. It is first of all addressed to those who wish to understand themselves—those who go to the psycho-analyst not to understand abstractly how the world is apprehended but how to manage their own lives—but, naturally, in this subject the other side, how reality is apprehended, cannot be disregarded. There must be a converse to *gnothi seauton*—"understand the outer world fully and you will under-

stand yourself". It looks then as though when psycho-analysis becomes less concerned with the individual's problems and more with the task of widening and varying the aperture of apprehension not only may therapeutics enter on a new and more fruitful development, but psychical research and indeed all science gain a new and essential instrument. No one can read this book without feeling that this is at least possible, that this path should be explored and that the methods here recommended are both scientific and promising.

G. H.

WHATELY CARINGTON, *Three Essays on Consciousness*. (Reprinted from the London Forum.) 30 pp.

There is one thing upon which one can rely when reading anything from the pen of Mr Whately Carington, and that is that one will not have served up a rehash of stale topics and arguments, but good, fresh, stimulating food for thought, rendered all the more palatable by the dash of "sauce piquante" which is infused by what he himself once described as verbal caracoles.

These three essays constitute a courageous and valuable attempt to tackle some of the problems arising from consciousness; they are not, and of course could not be, intended to deal with the subject in any exhaustive fashion. Mr Carington has simply picked out three outstanding points and concentrated on them. His fundamental datum is stated in the proposition. "I am conscious", the truth of which will be allowed by all but the extreme behaviourist. He then argues that, because it has been found impossible to fix the line between living and non-living matter, consciousness is universal. It seems to me that there is an hiatus in his reasoning here; he assumes without discussion that consciousness is always concomitant with life. I do not say that he is wrong, but I certainly think that the gap should be filled. An unconscious living being may be an impossibility, but the assumption that it is so should not be made without some comment.

I am afraid that I must also attempt to pick holes in his main argument. He says, "If life is closely connected with chemicals, then chemicals are closely connected with life". But surely this should be, "Life is closely connected with *some* chemicals". The only conclusion to be drawn from this is that *some* chemicals are closely connected with life. This is a particular proposition and, the undistributed term being chemicals, it follows that no conclusion in which this term is distributed can be drawn from this premise. Mr Carington's conclusion is an universal proposition.

In his second essay Mr Carington seeks to establish the theory of an Universal Consciousness or World Soul. I think that it is a little unfortunate that the same term "universal" should be used in two distinct senses. He tries to show in the first essay that consciousness is universal in the sense that all that exists, down to the ultimate constituents of being, is to some extent conscious. What he now means by "universal" is that all these "disconnected individual" consciousnesses are really in some way one. These two meanings do not imply each other.

He develops several lines of argument, notably from community of experience, from paragnostic phenomena and from mysticism, and he undoubtedly succeeds in putting up quite a good case; though, as he himself admits, the alternative theory of an external world can never be disproved *in toto*.

At the very outset of these essays Mr Carington justly points out the necessity of examining our preliminary assumptions. I think that perhaps in this second essay he has allowed at least one assumption to escape him, viz. that any terms which we can employ, any mental machinery, processes of reasoning, concepts, etc., are adequate to deal with ultimate reality. It may be that the only answer available to us is that of the Vedantist, "Neti, Neti" (Not so, Not so)—that is to say, complete and total agnosticism.

Moreover, I personally can form no clear idea of what is meant by an universal consciousness. I can construct a formula which seems, *prima facie*, intelligible, such as an unitary consciousness (unitary in somewhat the same way as I normally feel myself to be an unit) which embraces and includes all other individual consciousnesses without destroying their individuality. But when I come to examine this concept more closely I find that it evades my understanding, for the reason that it seems to transcend the categories of number, sameness and difference and to confuse the distinction between the one and the many.

It is quite possible, in fact it seems to me probable, that these categories are transcended in ultimate reality, but I cannot reach such heights myself. I believe that some of the advanced mathematical physicists—I think it was Dirac—have put forward a theory which might be called the indeterminacy of individuality. Thus, though I admit that it is quite possible that Mr Carington is correct, I feel quite lost when I attempt to think of myself as I know myself as being only a part of another larger self which I do not know.

However the fact that I cannot grasp the idea is no reason whatsoever for supposing that it is wrong or that others cannot grasp it.

In his third essay Mr Carington sketches an outline of the evol-

ution of the individual consciousness by successive differentiations and traces a parallel between it and the evolution of the physical universe as now conceived by modern physics.

His suggestions remind me somewhat of Herbert Spencer's famous formula.

If what I have said above may seem to be mainly criticism it is not because I do not appreciate these essays. I consider that they are a most valuable contribution towards the understanding of a very difficult subject, the most difficult perhaps because the most familiar as well as the most fundamental of all with which science and philosophy have to deal. I hope Mr Carington will carry his analysis further and elucidate the meaning of the two remaining terms of his fundamental datum, viz. "I" and "am."

H. F. S.

La Revue Belge, 15 October 1934 and 1 January 1935.

The earlier of the numbers contains an article on haunted houses by M. Paul Heuzé. This, at any rate, is what the title of the article is intended to convey, though in fact the writer states his disbelief in telepathic apparitions, poltergeists, etc., with hardly a word about haunted houses. M. Heuzé refers with gusto to an invitation extended to him by the owner of a Scottish house to come and see a phantom who is alleged regularly to appear on a given date; M. Heuzé answered, so he says, that he would certainly refuse to put himself out to see a phantom cross a dining-room. This answer he seems to regard as witty and sensible; we have to inform M. Heuzé that it is, on the contrary, foolish and inept.

In the later issue Count Perovsky has the congenial task of administering a courteous reprimand to M. Heuzé, which he does with uncompromising thoroughness. At the end of this article M. Heuzé adds a few words which can be best described as impertinent, in both senses of the word.

TH. B.

JOSEPH J. WILLIAMS, S.J., *Psychic Phenomena of Jamaica*. 8vo, pp. [iii]. 309. New York: Dial Press, 1934. Price \$2.50.

Father Williams's book is valuable chiefly to those interested in African influences and traditions among the natives of Jamaica. Nevertheless his book deserves attention from students of psychical research. The writer has collected a number of local ghost stories, mainly of the poltergeist type, some of which attain a fairly respectable evidential standard.

The tone of this book is the more remarkable in that Father

Williams gives not the slightest sign of having ever heard of psychical research. He concludes (p. 263): "Taking them all together as a composite whole, it is my unhesitating conclusion that there are times in Jamaica when phenomena occur that transcend the forces of Nature and must be attributed to spirit control, which, judged from the consequences, are of diabolical origin." Such a conclusion from a priest of culture and ethnological training is worthy of notice. An S.P.R. expedition to the West Indies seems more than ever desirable!

TH. B.

ALEXANDER CANNON, *Powers that Be*. 8vo, pp. xv. 221, portrait.
London: The Francis Mott Co., 1934. Price 5s. net.

Dr Cannon is, or was until recently, associated with one of the L.C.C. lunatic asylums. He is also, so he tells us on his title page, Kushoo Yogi of Northern Thibet and Master of the Fifth of the Great White Lodge of the Himalayas. A work entitled *Powers that Be* and written under such auspices must attract, even command, attention.

TH. B.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION.

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL
OF THE
Society for Psychical Research

NOTICE OF MEETING

A General Meeting of the Society

WILL BE HELD IN

THE CONWAY HALL,

RED LION SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 1 May, 1935, at 5.30 p.m.

WHEN

THE PRESIDENTIAL ADDRESS

WILL BE DELIVERED BY

PROFESSOR C. D. BROAD, LITT.D.

N.B.—Members and Associates will be admitted on signing their names at the door. Visitors will be admitted on production of an invitation card signed by a Member or Associate.

MRS SIDGWICK

OUR Members will be interested to learn that on the 11 March Mrs Sidgwick attained the age of ninety. To her services in research, in administration, and in counsel ever since the Society's foundation, the success, and even the continuance, of the Society has been largely due. At their meeting on the 27 March the Council passed an unanimous vote of congratulation to her.

THE FIFTH INTERNATIONAL CONGRESS FOR PSYCHICAL RESEARCH

ON the invitation of the Norwegian Society for Psychical Research, whose activity and enterprise are well known, the 5th International Congress will take place at Oslo from August 26th to 31st. It is to be hoped that our Society will be well represented at Oslo, and any of our members who may be able to attend may be sure that they will receive a very warm welcome.

Members wishing to read papers at the Congress are reminded that, under the rules governing the Congresses, papers have to be submitted in advance to the National Committee concerned. The English National Committee consists of Dr William Brown, Professor C. D. Broad, Dr F. C. S. Schiller and Sir Ernest Bennett, M.P.

Will any member who wishes to attend the Congress or read a paper kindly notify Miss Newton at the earliest opportunity, and also submit the typescript of the paper proposed to be read, so that it may be circulated to the members of the National Committee. As a large number of papers are read from many different countries, it is essential that papers should be brief.

W. H. S.

APPOINTMENT OF HON. LIBRARIAN

MISS K. E. WATKINS, a member of the Society, and a trained librarian, has very kindly offered to supervise the administration of our Library, and to do the more technical part of the library work, the issue of books, etc., being done, as hitherto, by the Assistant Secretary. The Council are particularly grateful to Miss Watkins, whose ordinary professional duties leave her little leisure, and consider themselves fortunate in having secured her as Hon. Librarian.

APPEAL TO MEMBERS

THE Research Committee has decided to carry out a series of experiments on Extra-Sensory Perception, using cards as well as other

methods, and would be glad to have the names of any Members or their friends who would co-operate by offering themselves for test as percipients. These tests will be carried out at the Society's Rooms, and times will be arranged as far as possible to suit the convenience of Members. Names should be sent to Mr G. N. M. Tyrrell, either at 6, Carlton Hill, N.W. 8, or at 31, Tavistock Square, W.C. 1.

NEW MEMBERS

Cole, Lady Eleanor, Fishers Hill, Woking.

Evans, Laurence A., Mitre House, King's Road, Chelsea, London, S.W. 3.

Power, F. Danvers, 25 Woodside Avenue, Burwood, New South Wales, Australia.

Sorabji, K. S., 175 Clarence Gate Gardens, Regent's Park, London, N.W. 1.

MEETING OF THE COUNCIL

THE 329th Meeting of the Council was held at 31 Tavistock Square, London, W.C.1, on Wednesday, 27 March 1935, at 4 p.m., THE HON. MRS ALFRED LYTTTELTON, G.B.E., in the Chair. There were also present : Sir Ernest Bennett, M.P., Miss Ina Jephson, Sir Lawrence Jones, Bart., Professor W. McDougall, Mr W. H. Salter, the Rev. C. Drayton Thomas and Miss Nea Walker ; also Miss Isabel Newton, Secretary.

The Minutes of the last two Meetings of the Council were read and signed as correct.

The Minutes of the Annual General Meeting were taken as read and signed as correct.

Four new Members were elected. Their names and addresses are given above.

The following were co-opted as Members of Council for the year 1935-1936 : Professor C. D. Broad, Dr William Brown, Mr Whately Carington, Mr Oliver Gatty, Mr Gerald Heard, Lord Charles Hope, Professor Julian Huxley, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, Rev. C. Drayton Thomas and Miss Nea Walker.

Miss K. E. Watkins, B.Sc., was appointed Hon. Librarian to the Society for the year 1935.

By an oversight the name of Mr Carl Vett was omitted from the list of Corresponding Members printed in the March *Journal*. Mr Vett, who has been a Corresponding Member since 1924, was duly re-elected at the meeting on 27 February.

PRIVATE MEETINGS

THE 134th Private Meeting of the Society was held on Wednesday, 30 January 1935, at 5.30 p.m., PROFESSOR C. D. BROAD in the Chair.

PROFESSOR R. H. THOULESS read a paper on "Dr Rhine's Recent Experiments in Extra-sensory Perception", and an interesting discussion followed, in which Mr Bartlett, Mr Whately Carington, Dr E. J. Dingwall, Mr J. Hettinger, Mrs Maeky, Mr I. Nicholl, Mrs W. H. Salter and Mr S. G. Soal took part.

THE 135th Private Meeting of the Society was held on Wednesday, 27 March 1935, at 5.30 p.m., Mr W. H. SALTER in the Chair.

MR W. WHATELY CARINGTON read a paper on "The Quantitative Study of Trance Personalities", continuing and extending the methods of the Word Association Test employed in the first paper on this subject, published in *Proceedings*, Part 136. Mr J. C. Maby and the Rev. C. Drayton Thomas spoke in the discussion which followed.

Mr Whately Carington's paper will, it is hoped, shortly be published in *Proceedings*.

SOME EXPERIMENTS IN UNDIFFERENTIATED
EXTRA-SENSORY PERCEPTION

BY MR G. N. M. TYRRELL

THE following experiments form a belated sequel to the experiments with cards which were carried out by Miss Johnson and myself in 1921, and which were reported in the *Journal S.P.R.* for June 1922 under the title, *The Case of Miss Nancy Sinclair*. This title was scarcely pseudonymous, for "Sinclair" is one of Miss Johnson's real names, and "Nancy" is the name by which she is familiarly known to her friends. The suppression of the surname was merely her fancy at the time.

I would direct the reader's attention especially to the card-experiments on pp. 318-327, since these acquire a retrospective interest in the light of those we are now publishing. It will be seen that in most cases these card-experiments reach high anti-chance probabilities, and that some of them were apparently precognitive. I feel an absolute personal conviction that they were perfectly genuine cases of paranormal faculty. The cards, which I had just shuffled and cut on another table, were in all cases lying before me in a pack face downwards on the table in full light, and I watched them as closely as a cat watches a mouse. Miss Johnson sat beside me in

a state of strained concentration, often with her face buried in her hands. Could even an expert conjurer, under these conditions, have placed extra cards on the pack before me right under my nose without my noticing it? If so, it would have been a wonderful feat: but I am certain that Miss Johnson, who had no skill whatever in the manipulation of cards, could not possibly have done so.

Unfortunately, these results were obtained under conditions of very great nervous strain, and it would have been disastrous to continue under these circumstances. For years afterwards there was no opportunity of resuming work of this kind. At last, however, a fresh opportunity has presented itself, and we have started again with improved methods.

Before describing the recent experiments, I must say a few words about the percipient. I and my family have known Miss Johnson intimately since 1920, and there is no one in whose complete candour and integrity I have greater faith. Not only is she a thoroughly reliable and level-headed person in the affairs of ordinary life, but she has developed the rare faculty of self-discipline and self-analysis with regard to her paranormal faculty. She can distinguish to a considerable extent the alloy of the mind's contribution from the paranormal gold; and she is moreover very critical with regard to psychic phenomena in general. She combines the outlook of the investigator with the faculty of the sensitive. Her paranormal or parapsychic faculty has long ago, for me, proved its existence by passing the pragmatic test of being useful in daily life. To doubt the existence of what one habitually makes use of is, I think, to place oneself in rather a curious logical position. But the type of proof which is amenable to mathematical analysis is, of course, needed as well: and this, it is hoped, the present experiments supply.

I do not think that either of the terms "Supernormal Perception" or "Extra-Sensory Perception" (Dr Rhine's term) are quite the right ones to apply to a faculty like Miss Johnson's. Her power strikes one as being very wide in its scope and as being much more like what a French writer describes as an "*assimilation globale des événements*" than like what we usually call "perception". In the terrible terminology which people invent for psychical research, perhaps "parapsychic cognition" would describe this diffused faculty of getting to know things in general: but I prefer the simpler term "Extra-Sensory Perception", even if it is not the most exact, partly because it reduces easily to the three euphonious initials, E.S.P.

Our present method of research began by my hiding a small object while Miss Johnson was out of the room and by her finding it when

she came back ; and this, although a seemingly childish game, had more in it than appears at first sight. For one thing, the act of finding the object awakened in the percipient a feeling of inward satisfaction which was very marked, and this I took to be an important lead. It induced the right state of psychological *interest*. No doubt this pleasure in finding things is a psychological idiosyncrasy of the percipient's, but I have always believed that the fundamental problem in psychical research is a psychological one—that is to say that it consists largely in helping the material to emerge and to overcome those barriers which evidently interpose themselves in the way of its externalisation. There are no well defined routes ready prepared for the emergence of E.S.P. material as there are for the results of normal perception, or for motor and *normal* sensory complexes : it must borrow the sensory routes and use them as an interloper. Consequently, the first thing to do is to make its route as easy for it as possible ; and to do this one must induce in the percipient the most favourable psychological state.

It is of little use to devise evidentially water-tight conditions unless these include the conditions under which the phenomenon one wants to investigate can take place : therefore it is wise to adopt a form of experiment which creates interest in the percipient.

Another factor was this—I knew that Miss Johnson had always had great difficulty in expressing any paranormal feeling in words. She could not, for example, tell me where a hidden object was : but she could go and find it. It is fairly obvious that the act of putting any thought into words calls for a certain amount of conscious attention : and this is not compatible with the sort of emotional-impulse-factor which seems always to form part of a paranormal impression. One can always see in Miss Johnson the little impulsive, emotional accompaniment to any genuinely paranormal feeling. And to ask her to put the cognitive element of this into words is a little like handing a person a book of rules in a sudden emergency and asking him to look up the best thing to do. One cannot fit one's mind to pages and phrases when an emotional urge to do something is stirring in one. It is much easier to do something *active*. And I think in a minor way, the same sort of thing applies to the emergence of paranormal knowledge. *Action* is an easier mode of externalisation than *speech*. Therefore, I thought, would it not be a good thing if I could devise some sort of experiment in which the percipient had to *find* something ?

A further psychological consideration was that inhibitory associations are much more likely to be connected with words than with simple actions.

So I set myself the task of trying to discover a way in which this game of hide-and-seek might be turned into a quantitative experiment. I first took five ¹ empty match-boxes, putting a little cotton-wool into the bottom of each to deaden sounds, and told Miss Johnson to go out of the room while I hid a small object—actually a die—in one of the boxes. Then, shutting and moving all the boxes, I asked her to come in and point to the box in which she thought the die was hidden. In this way she began to score rather above chance expectation, but the method was slow and laborious and the match-boxes began to come to pieces, so I decided to make a better arrangement. I constructed five small, wooden boxes placed in a row with sloping lids overlapping the sides and facing the percipient. Behind them was a large, wooden screen on the other side of which the agent, or operator was seated, completely screened from the percipient. The backs of the boxes were arranged so as to be open towards the operator, who was provided with a long, flat pointer. The “hiding” of the object could now be done quickly by the operator thrusting his pointer into one of the boxes; the “finding” was done by the percipient raising one of the box-lids and looking in to see if the pointer was there. This was simple, and satisfied the demand for psychological interest. Two difficulties, however, remained to be overcome. One was that the percipient must not be able to *hear* the pointer being thrust into the box. The other was that the operator, seated behind the screen, must be able to check what the percipient was doing.

Neither of these difficulties proved to be great in practice. The boxes were lined with spongy rubber covered with soft flannelette, and the pointer was made from a thin strip of flat wood, rounded at the end and highly polished. Under these circumstances, the sound of the pointer entering a box was so reduced as to be quite inaudible on the other side of the screen. The check on the percipient was easily secured by having a strong light on the percipient's side of the screen. Each time a box was opened the light coming through it was visible to the operator, who could see which box was opened, and that only one box was opened at a time. All the operator had to do was to record each trial as a success or a failure.

This apparatus proved to be very simple and convenient. Not only did it fulfil the object in view by allowing the percipient to express her paranormal knowledge by means of a simple *action* and without the need for *speech*; but it also allowed the trials to be performed very quickly. We found in fact that a hundred trials could be made in from $3\frac{1}{2}$ to 4 minutes. This has two important advan-

¹ I owe the number “five” to Dr Rhine's work.

tages. (1) It allows the *impulse*-factor in the paranormal cognition to act without hindrance: there is not much time for the *feeling* which, *e.g.* indicates No. 4 box to be altered by association of ideas to some other number. The hand has merely to flick up the lid of the box, which falls back by its own weight. (2) It forms a very quick way of testing any subject for significance; for one can soon reach such a large number of trials that even a small paranormal factor will show itself.

The Experiments. These were quite simple. The apparatus was placed with a strong light on the percipient's side. The operator thrust the pointer into one hole at a time at random, saying the word "In" each time. On hearing the word "In", the percipient opened one of the box-lids. The operator, noticing which box the light came through, scored the result on squared paper, making a tick for a success and a dot for a failure. If more than one box-lid was ever opened at a time the trial was scored a failure. It was not considered necessary to note how many times each number was selected, either by the operator or by the percipient. The question of the operator's possible number-habits will be discussed later: and the percipient's number-habits (if any) could not be effective in raising the score.

It became evident after a little working with this apparatus that the paramount necessity was *speed*. When genuine E.S.P. is at work, each "guess" is accompanied by a powerful urge towards immediate externalisation. Anything which delays or thwarts this urge may lead to its being replaced by a semi-conscious guess.

For convenience, the experiments were broken up into blocks of 100 trials each. We usually did about six blocks at a sitting.

The experiments were begun on 10 October 1934, and the results here given extend to 20 February 1935, which brings us to a round total of 30,000 trials. Of these 9,364 were successes; that is to say the successes are 31.21 per cent. of the total number of trials. Since the probability of success at each trial is $\frac{1}{5}$, the chance expectation of successes in 30,000 trials is 6,000, or 20 per cent. This has been very greatly exceeded.

The best way to show the results and general course of events at a glance is to plot them in the form of a graph, and one is given on pages 60-61. Each unit of the abscissae represents 100 trials, while the ordinates represent percentage successes. Each point of the graph therefore shows the number of successes which were scored in that particular block of 100 trials. By plotting the results in this way we can see how the average success varied with changing conditions. For instance, vertical, dotted lines represent the presence of visitors. Chain-dotted lines represent trials in which the numbers were not

chosen by myself. One section shows where lamps were used in the boxes instead of the pointer : (Full lines.) A period of illness on the part of the percipient is shown, and the important "light feelings" : (Short dotted lines.) The horizontal line drawn at the 20-mark shows the average chance expectation of success. It will be seen from the graph how very variable the results are. Even at the same sitting, the scoring may oscillate between 26 and 41 : and both I as operator and Miss J. as percipient have the same feeling that there is something which keeps catching on and letting go again. The successes tend to go in "runs" or batches with more or less blank spaces between. On the whole, the tendency is for the scoring to improve during a sitting, but this is not always so.

With this great variability in the results, it is difficult to deduce from the graph what factors have affected the scoring. I only feel reasonably confident about the following points. High scoring is promoted by (1) Good health, and absence of worries and petty annoyances and by general cheerfulness ; (2) by the "light feeling", (L on the graph) which in its turn is favoured by (1) ; (3) by that tendency towards dissociation, which Miss J. refers to as "losing herself" in the experiments. In addition to this, any stimulus to interest, such as particular success, tends to raise scoring. A necessary condition for good results is also that the percipient must feel completely at ease. Even a slight circumstance which clouds the serenity of the mind, whether it be a slight jarring with the personality of someone present, or an irritating sound, or a feeling that someone is in a hurry, or even a new feature in the material conditions of the experiment, will upset this condition. In ordinary life we overcome these things by a slight effort of will ; but the investigator has to remember that they will have their effect on the material of E.S.P. before the will can act on them. That is why the demands of a sensitive must be listened to. There is no reason why they should not be reconciled with leak-proof evidential conditions.

The condition referred to as the "light feeling" is a very difficult one to define : yet it is important, for it is a sure indication that the paranormal faculty is in the ascendant. From the subjective standpoint, its ingredients probably include happiness, a feeling of being a little above and aloof from the material organism, and a quickening of all the mental faculties. If one can imagine such a thing, I should say that what I referred to above as the "emotional impulse" spreads itself over the period during which the "light feeling" lasts. Miss Johnson has told me at such times that other people's logical ways of arriving at conclusions seem to her to be irritatingly slow. She has got there in a flash before them.

For the rest, the highest score reached in any block of 100 trials was 51. The lowest was 14. And it will be seen that only on 12 occasions out of 300 did the score go below 20.

Possible Explanations of the Results

(1) *Chance.* A glance at the graph is really sufficient to show that the results are not due to chance. But this must be demonstrated more formally. If pure chance were the only determining factor, we should expect the successes to average $\frac{1}{5}$, or 20 per cent., of the trials. The points on the graph would be distributed equally above and below the 20-line. With a small number of trials the successes might deviate considerably from $\frac{1}{5}$ of the trials, and yet be all due to chance: but as the number of trials is increased, any given deviation from the $\frac{1}{5}$ -value becomes more and more improbable.

We want some measure of this; that is to say we want to know what is the probability that, with a given number of trials, our successes will deviate to any given extent from the $\frac{1}{5}$ -value. This measure is given to us by the mathematics of Probability. The deviation may be positive or negative. If there is some factor at work tending to raise the results *above* chance value, we shall find a *positive* deviation greater than chance can reasonably be held responsible for. If, on the other hand, there is a factor at work tending to depress the results *below* the chance value, we shall find a similar *negative* deviation. In the present case there is no evidence of a *depressive* factor. The few below-chance scores which show themselves may be regarded as due to chance acting alone, the elevating factor being in abeyance.

If d is the deviation from chance-expectation,
and σ is what is called the Standard Error.

then $\frac{d}{\sigma}$ is a quantity called X .

The Probability, P , which we require is the integral of a function of X , called the Probability Integral. These Probability Integrals have been worked out and are given in tables in books dealing with the subject of probability. I am here taking my figures from the tables given on p. 74 of Dr R. A. Fisher's *Statistical Methods for Research Workers*.

The Standard Deviation or Standard Error $= \sigma = \sqrt{npq}$

where n = the number of trials,

p = the probability of success at each trial $= \frac{1}{5}$,

q = the probability of failure $= \frac{4}{5}$,

pq then $= 0.16$, so that $\sigma = \sqrt{0.16 n}$,

and therefore $X = \frac{d}{\sqrt{0.16 n}}$

We can find from the tables the value of P corresponding to any value of X .

There is, however, something else to be decided. What value of P shall we say is sufficiently low to rule out chance as a reasonable explanation? In other words, when shall we say, "There is some other factor here besides chance", and call our results "significant"? It is customary to do this when P falls below 0.05 or when X rises above 2, which is nearly the same thing. This point is of course arbitrary, but it is necessary to fix on some definite point for significance. To quote Dr Fisher, "Deviations exceeding twice the standard deviation are thus formally regarded as significant." (Sometimes "Probable Error" is used instead of "Standard Error", and then the values of X are 1.5 times greater.)

We have now a way of deciding when a result becomes significant, and of calculating the odds against any given result being due to pure chance. But there is a further point to be taken into account. X may be either positive or negative, and the tables of the Probability Integral (P) have been calculated for both positive and negative values. But, since the factor which is here superimposing itself on Chance is acting so as to produce positive deviations only, we are entitled to halve the values of P which are given in the tables.

Let us take the first 1,000 trials which Miss Johnson did and calculate what are the odds against her result being due to chance. The successes she scored in these 1,000 trials amounted to 349. If pure chance had been acting alone, she would have been expected to score $\frac{1}{2}$ of the number of trials, that is to say 200 successes. Her positive deviation was therefore $149 = d$.

$$\sigma = \sqrt{npq} = \sqrt{0.16 \times 1,000} = \sqrt{160} = 12.65,$$

$$X = \frac{d}{\sigma} = \frac{149}{12.65} = 11.7.$$

From the Probability Integral tables, P corresponding to this value of X is of the order $\frac{1}{2} \times 10^{-29}$. Or, put into words, the odds against this result being due to chance are of the order of a hundred thousand billion billion to one!

Fantastic as these figures are, as we proceed the odds against chance become far greater still. For the whole 30,000 trials the number of successes is 9,364. This is only an average of 31.2 per cent. of the number of trials, but, because the number of trials is now so large, the value of X has mounted to about 480, and P is of the order of $\frac{1}{2} \times 10^{-460}$.

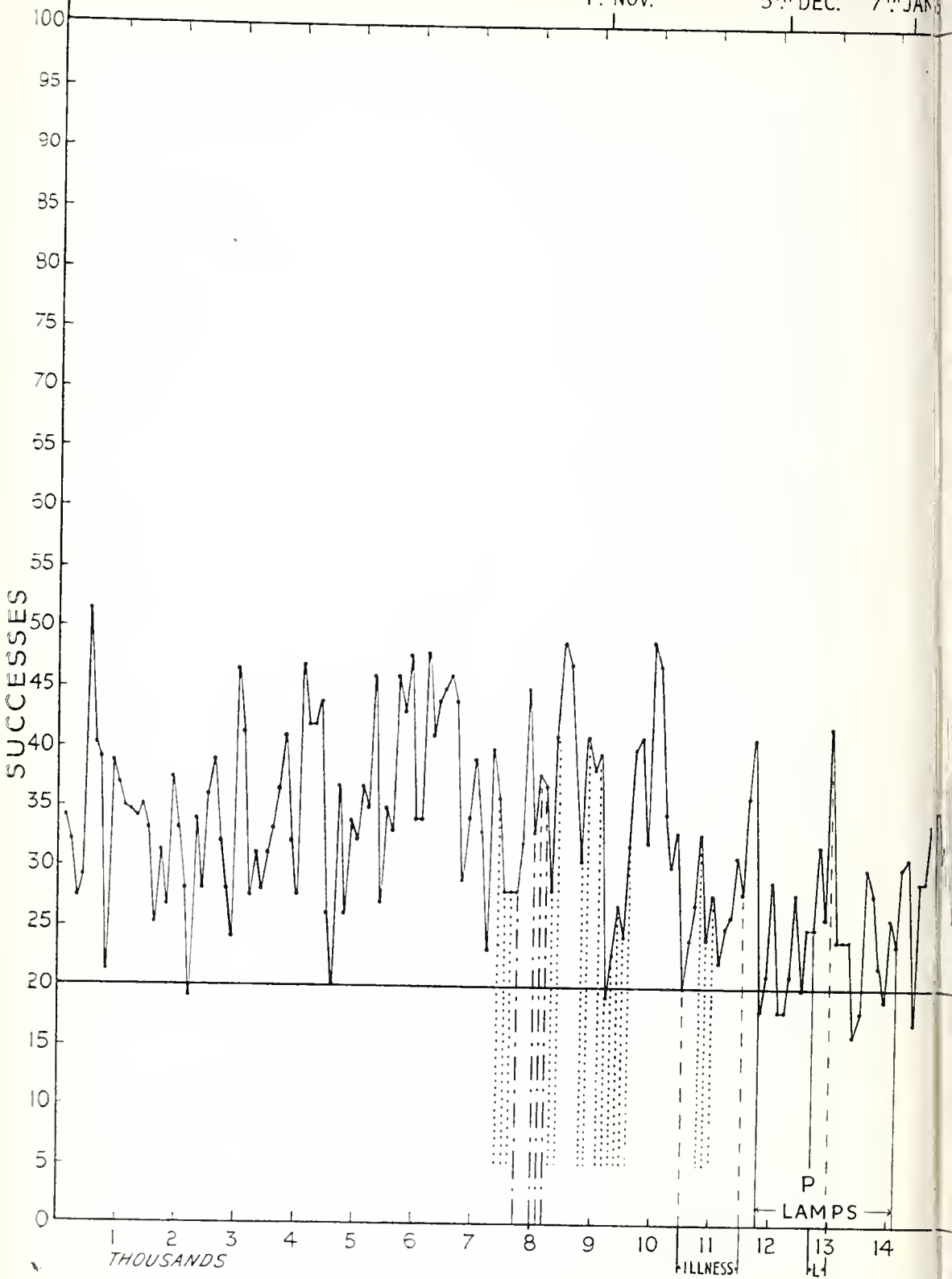
Such unthinkable figures mean nothing. No one regards chance as being a possible explanation of any phenomenon if the probability

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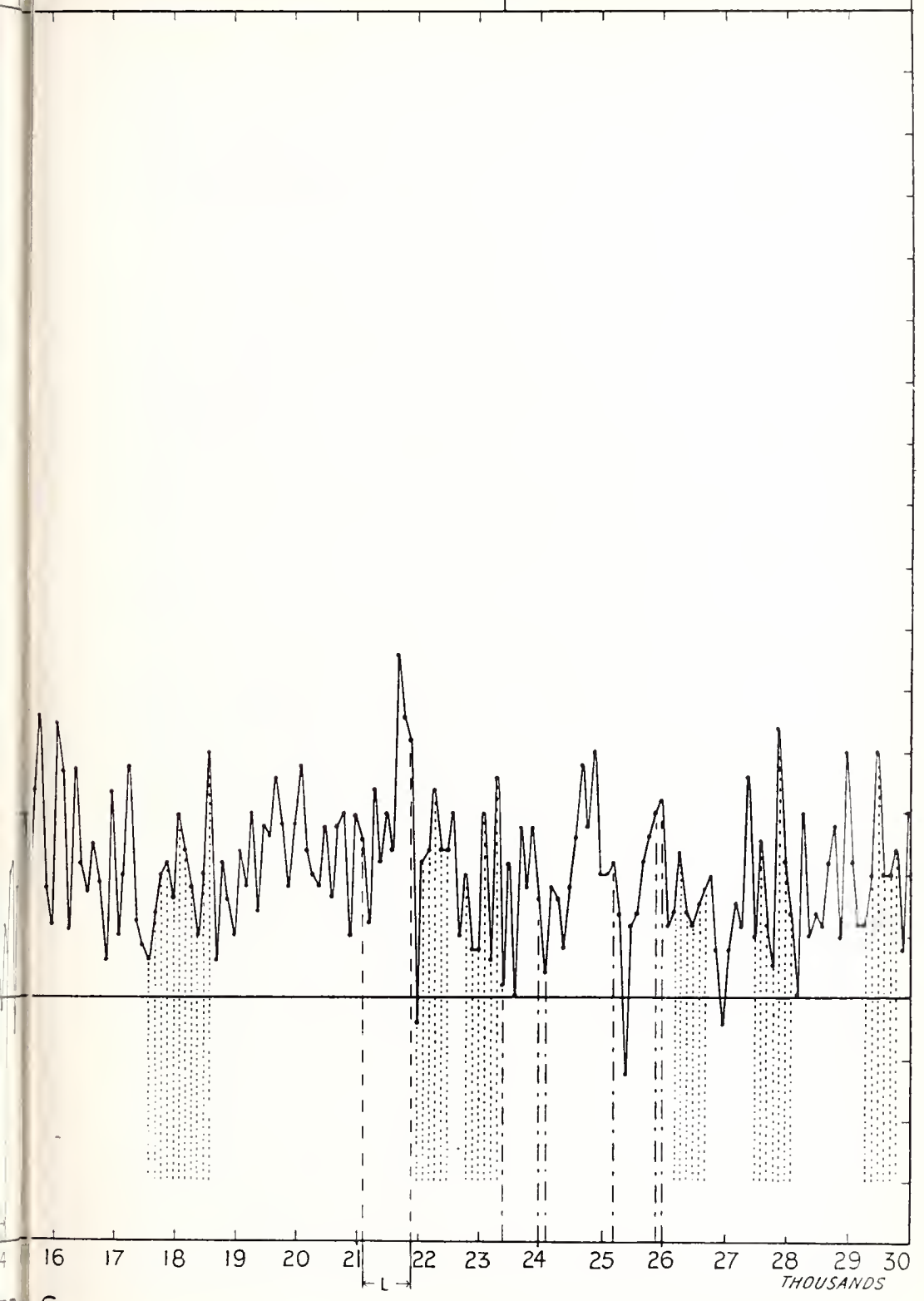


THOUSANDS

ILLNESS

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TRIALS



falls to 10^{-6} or anywhere near it : or, in other words, if the odds against chance are anything like a million to one. And here I had better say (although it should be obvious to any critic) that I have not piled up the odds against chance to these fantastic figures in order to make a chance explanation of my results less tenable. The figures of course accumulate automatically while the experiments are being carried on for other purposes.

The question of chance can then be dismissed, and we can pass on to consider other possible explanations.

Independence of Trials. The above calculations are based on the assumption that all the trials are independent of one another, so that the probability of success in every trial is $\frac{1}{5}$. Is this assumption justified? Is the pointer equally likely to enter any one of the five boxes on every occasion? Suppose that the operator, having put the pointer into a particular box on one occasion, always avoided putting it into the same box again on the next occasion, then the probability of success on the second occasion would be raised to $\frac{1}{4}$ instead of $\frac{1}{5}$. Or, suppose the operator habitually went to, say, No. 4 box after No. 2, or had some favourite sequence of "runs"; then the assumption we have made that the trials are independent of one another would not be justified.

Now, I think that this is the most likely criticism to be levelled against these experiments, and I wish to give it full weight. In the great majority of the trials I was myself the operator, selecting the numbers as I went along. What is more likely than that the percipient should have become used to my "runs", that is to my habits of selecting boxes in a certain order, and have succeeded in following me?

Plausible as it sounds, I do not believe that this factor, if it exists at all, has had any appreciable effect on the results. The experiments are admittedly a little like a game of hide-and-seek between operator and percipient; and if the operator *allowed* himself to form number-habits *and allowed* the percipient to learn them, then she might score above chance in this way. But one must not forget that the operator can *see* what the percipient is doing, and so can avoid doing what he thinks she expects him to. For example, suppose the percipient has scored two consecutive successes with boxes 1 and 2. She may be expected to try 3 next time, so the operator dodges back to 1 or repeats 2 or skips to 5. The operator's "runs" are not entirely his own; they are influenced by what the percipient does.

Two facts show pretty clearly that Miss Johnson has not, in fact, been scoring by following my number-habits. (1) 1,000 trials, consisting of 10 sets of 100 trials each, were done with numbers which

were not selected by myself at all. Some of these numbers were selected by a mechanical device and the rest by three other people. The 1000 trials contained 309 successes, or 30.9 per cent. Comparing this with the percentage of successes scored in the whole 30,000 trials, which was 31.2, we see that the correspondence is very close indeed. Can it be maintained in the face of these figures that it made any difference whether I was selecting the numbers or not? (2) If the percipient was, in fact, scoring by getting used to my number-habits, she should have become fairly expert in the course of 30,000 trials and her scoring should have gone steadily up. But it did not. It went down. During the first 10,000 trials she scored 3,449 successes, an average of 34.5 per cent. But the percentage success on the whole was only 31.2. She must have lost skill instead of gaining it, which is scarcely plausible. All these facts are shown on the graph.

It may be asked why we did not always use numbers selected by some chance-device, such as drawing numbers out of a bag. The reason is that it was found very difficult to keep looking at a card of numbers pinned up on the screen in front of one, and to operate the pointer and to record the results all at the required speed. Even a separate recorder did not help matters much. The operator, having constantly to look up and down, kept losing his place on the card of numbers, and causing delays which upset the percipient.

Hyperaesthesia. The next most likely criticism to be brought against the results is that they are due to hyperaesthesia. Hyperaesthesia of sight seems to be generally dismissed by those who have seen the apparatus. The lids of the boxes are of oak and overlap the sides. The screen is 26 inches square of $\frac{1}{8}$ inch three-ply wood, fitting into grooves in the sides of the box-unit. There are no cracks which could give the least help, and the point of the three-ply wood is that there cannot be a small crack going right through because of the crossing of the grain. I scarcely think that anyone would maintain that the most hyperaesthetic percipient could see *through* a wooden screen.

But auditory hyperaesthesia sounds *a priori* a more probable hypothesis. I think only a personal trial with the apparatus can enable anyone to form a fair judgement as to this. No one by careful listening on the percipient's side of the screen has been able to detect whether the pointer was being thrust in and out of the boxes or not. But no doubt there *is* some slight sound, and a critic, postulating extreme auditory hyperaesthesia on the part of the percipient, may say that she could be guided by it. But it must be remembered that *qualitative* hyperaesthesia alone would not be sufficient. The sound

must be *located*; not merely *detected*. And, as the total length occupied by the boxes is only six inches, the hyperaesthetic sense of direction must be extraordinarily accurate. All I can say is that those who have examined and used the apparatus do not regard the hyperaesthetic explanation as at all probable.

And, further, 2,300 trials were made without using the pointer at all. In these, five small electric lamps were placed in the boxes connected each to its own key, so that by pressing any key, the operator could light any one of the lamps at will. The keys rested on spongy rubber and when depressed, made contact with globules of mercury. They were absolutely silent. In these tests the explanation of auditory hyperaesthesia was entirely removed. They showed a marked drop in scoring, only 551 successes being secured, the chance expectation being 460. $d=91$, and the corresponding value of P is approximately $\frac{1}{2} \times 10^{-6}$, that is to say the odds against their being due to chance is about two million to one.

The graph is interesting here. A hasty critic might conclude the percipient's score dropped because the help due to auditory hyperaesthesia had been removed; but the graph shows that the score had already dropped beforehand, and that this drop coincided with the percipient's illness between 11 and 16 of November. Moreover, the line marked P shows where, for one block of 100 trials, the pointer was reintroduced in the middle of the lamp-trials. The percipient had just scored 25 with the lamps: she now scored 25 again with the pointer. It made not the slightest difference. We may notice another curious thing. In the middle of these lamp-trials, during which the percipient was not feeling very well, there suddenly occurred a flash of the "light feeling", when she recovered and scored 32 and then 42, and then dropped again to the previous level. We may, I think, attribute the low scoring-average with the lamps partly to bad health and partly to the change in the conditions of the experiment.

But if hyperaesthesia of sound has been eliminated in these lamp-experiments, what about hyperaesthesia of sight? I admit frankly that this theory is possible here. The box-lids, though overlapping the edges of the boxes and fitting pretty well, were not designed to be absolutely light-tight, and *in the dark*, faint streaks of light could be seen in the cracks between the lids by getting the head into the right position. But with the strong light shining down on the boxes, which was the working condition, no light in the cracks could be seen. (The bulbs in the boxes were worked dull and not at full brightness.) But by postulating hyperaesthesia of sight, it might be said that this was the cause of the successes with the lamps.

With the present apparatus this cannot be definitely disproved.

But one wants to know, (a) why, if the percipient was being guided by the lights, she did not score better than she did, and (b) if she *could* score as high as 42, why she did it only once when the sensory guide must have been constant all the time. In fact, I think one may say in general that if hyperaesthesia were the true explanation of these results, one would expect them to be much more constant than they are.

Normal Knowledge and Fraud. Is there any way in which the percipient could have obtained her results by normal sense-perception, deceiving both the operator and the witnesses? Everyone who has taken the percipient's place at the apparatus has so far come to the conclusion that it is impossible to see or to hear anything normally which could be of the least help towards scoring. There are no cracks which the percipient can see through. She cannot look over or round the screen or under the table without standing up or bending in such a way as to be perfectly obvious and without stopping the course of the experiment. She cannot make trial shots by raising the lids of one or more boxes a little way and peeping in, because the lids have been tested for this, and if one of them is opened the operator sees the light coming through it *before* the percipient can see whether there is anything in the box or not. If the slightest gleam of light shows through more than one box at a time, the operator marks the trial as a failure. Besides, the percipient has repeatedly scored above chance while being closely watched by a witness.

Collusion. Could there be fraudulent collusion between myself and the percipient? There is certainly some scope for this. The table is only 18 inches wide, and it would be quite possible for me to nudge the percipient with my foot under it. Dame Edith Lyttelton pointed this out, and at her suggestion a large drawing board was put against the side of the table so as to screen completely the legs of the operator from those of the percipient. Under these conditions, 400 trials were made, with 132 successes, that is 33 per cent., which is rather above the general average of scoring.

Another thing which might be construed as a possible source of leakage is the fact that the operator says the word "In" each time he thrusts the pointer into one of the boxes. Does the inflection of his voice indicate the number of the box? Do I do this? Or have I arranged with Miss Johnson some system of sighs and groans by which she knows the number of the box I am putting the pointer into? There are two points about this. One is that if I did these things, witnesses would probably have noticed it. And secondly, it would be something of a feat to do it at the rate of 27 times a minute. But what is more conclusive is the fact that the percipient has scored

well above chance with two other operators. With Mr H. F. Saltmarsh operating, 400 trials were made, with 121 successes, an average of 30.2 per cent. X works out at 5.12, and P is of the order of $\frac{1}{2} \times 10^{-7}$. With Mr Gerald Heard operating, 600 trials were made with 169 successes, averaging 28.1 per cent., and again P is of the order $\frac{1}{2} \times 10^{-7}$. So that if there is any question of voice-signalling, they must both have learnt the code.

We tried a lecturer's castanet instead of saying "In", but it worried the percipient and we gave it up. There seemed also to be an advantage in the voice-method, for the sound of it must effectually drown any microscopic sound the pointer makes, and so make the theory of auditory hyperaesthesia untenable.

A Conspiracy! Are Miss Johnson, myself and all our witnesses banded together in a fraudulent conspiracy? I confess that I do not feel equal to dealing with this suggestion. All I can say is that I hope more members will join the conspiracy!

Witnesses. The following is the list of investigators who have so far witnessed these experiments. I give their names in the chronological order of their visits. Mr H. F. Saltmarsh, Professor C. D. Broad, Mr E. H. Mallet, Miss I. Newton, Mr W. H. Salter, Hon. Mrs Alfred Lyttelton, Mr Gerald Heard, Mr and Mrs Kenneth Richmond, Mr Whately Carington, Miss Ina Jephson, Mrs W. H. Salter. In all, 5,700 trials were made in the presence of these visitors, who were usually though not always present two at a time. 1,700 of these trials were successes, giving an average of 29.8 per cent., which is again so close to the total average of 31.2 per cent. as to show that whatever was producing the scoring in the presence of the visitors was also producing it in their absence.

The presence of visitors might have been expected to lower the scoring, and the graph shows that there was some lowering at the beginning of each visit. But the recovery was quick as soon as the percipient began to get used to their presence.

Dame Edith Lyttelton and Mr Heard paid six visits in order to make a thorough examination of the whole process which they watched from both sides simultaneously. Mr Heard checked my scoring by watching the openings of the boxes and checked my addition of the number of successes afterwards. On some occasions he acted as operator with the result given above (169 successes in 600 trials). Mr Saltmarsh was also present several times, and also acted as operator, and these three witnesses have kindly appended their independent testimony.

Other Percipients. Although various people have tried their hands at taking the percipient's place and scoring with the apparatus, no

one except myself has done enough trials to enable any conclusion to be drawn. I have, however, myself acted as percipient for 1,100 trials, Mrs Tyrrell being the operator throughout. I scored 244 successes, an average of 22·18 per cent. d is here 24, and X works out at 1·8, giving the half value of P as 0·035. This is *just* below the significant value of 0·05, so that there is a trace of some factor besides chance. It may be that I have occasional feeble flashes of paranormal faculty, or it may be that there is a very slight amount of truth in the theory that the percipient can score by following the operator's runs. But at any rate the tremendous discrepancy between Miss Johnson's performance and my own tells heavily against any normal explanation of her results. Speaking in round numbers, she has scored successes at the rate of 32 per cent. of the number of trials, whereas I have only scored successes at the rate of 22 per cent. Or, put in another form, she has scored 12 per cent. above the chance expectation of successes, whereas I have only scored 2 per cent. above them. If she can do this by following the operator's runs, or by using her eyes and ears, why cannot I do it ?

Conclusion. We have here results which are colossally above chance. Either Miss Johnson has amply shown the existence of undifferentiated E.S.P.—that is to say of telepathy and/or clairvoyance—an extension of human faculty which carries with it the most far-reaching significance, or some possible means of reaching the results by normal or semi-normal (hyperaesthetic) means have not been sufficiently ruled out. Vague criticism of results which have reached this stage is not enough : it must be shown *exactly* how the results are obtained. The apparatus will be shortly removed to 31, Tavistock Square, and any critic who believes that an above-chance score can be obtained with it by normal means will have an opportunity of demonstrating how this can be done.

Meanwhile it is recognised that the present apparatus is imperfect and too limited in its scope. It was in the nature of an experiment, and experience shows that it can be greatly improved upon. A new apparatus is now under construction which, it is hoped, will overcome the deficiencies of the present one. It will allow pure clairvoyance and pure precognition to be tested independently. It will be electrically operated, self-recording and automatically fraud-proof, and will embody a mechanical selector which will select box-numbers of which the operator is ignorant. Thus, unconscious whispering and number-habits will be automatically excluded ; and operator and percipient will work at separate tables at a distance from one another and if desired in separate rooms. The question of a physical basis for visual hyperaesthesia (the only kind which will be relevant)

will, it is hoped, be able to be subjected to an objective, photographic test : and since a high speed of working will be possible, the rate of progress should be rapid.

Finally, I should like to express my thanks to Dr R. A. Fisher for kindly assisting me with the mathematics of the subject : also for the kind help given me by the President and by all those who have taken part in the experiments and have helped me by their welcome criticisms and suggestions.

NOTE BY DAME EDITH LYTTTELTON AND MR GERALD HEARD.

As stated in Mr Tyrrell's report, we attended six sittings at his house in order to watch his method of carrying out the experiments in extra-sensory perception. These dates were Jan. 17th, 18th and 24th and Feb. 6th, 8th and 13th. From our notes taken at the time the following particulars are quoted. On the first sitting, which began at 8.48, we watched six series of 100 calls each. Each of these runs of a 100 took between 3 to 4 minutes. This was the limit of Mr Tyrrell's working speed—how fast he could place the pointer in a hole, see whether Miss Johnson had guessed right and record failure or success. The nearer he could approach to the three-minute pace for the 100 run the better the guessing became. Miss Johnson often feels the check on her power of guessing owing to the inevitable slowness of this method. This will be overcome by use of the new machine. The six runs showed the following hits out of the hundred guesses : 23, 27, 30, 31, 23, 35. This showed the improvement which is generally noted as Miss Johnson becomes used to the presence of strangers. The drop to 23 in the last but one Miss Johnson felt was due to "slowness" on part of operator. On the 18th another six runs were made, showing 32, 29, 25, 30, 40. For the sixth Gerald Heard was tried as percipient—so as to study the method better. This run gave 31. On the 24th Mrs Lyttelton also made a short experimental run to test the method. This run included only 32 guesses and showed 7 correct. Miss Johnson's six runs of a hundred on this evening showed, 18, 31, 32, 37, 32, 32, successes. At the final sitting on Feb. 13th her runs showed 26, 26, (this run G. H. acted as operator) 30 (it was after this that on Mrs Lyttelton's recommendation a board was put so as to prevent possible foot contact between operator and percipient) 40 hits were then made out of the possible hundred. The next two runs were operated by G. H. again and gave 30 and 30 and a seventh was operated by Mr Tyrrell, giving 32.

At all these sittings no one was present except the two observers,

the operator and Miss Johnson. They were held in a Studio distant from the house. They all took place between 8.45 and 9.45 p.m.

In our opinion they gave evidence of the existence of extra-sensory perception on the part of Miss Johnson. We also think that when the new machine for carrying out tests of this faculty is used it will be possible greatly to increase the number of experiments and the data available. We recommend any member of the Society who wishes to go further into the matter to communicate with Mr Tyrrell who is anxious that as many members as possible should view the experiments for themselves.

(Signed) EDITH LYTTETON,
GERALD HEARD.

NOTE BY MR H. F. SALTMARSH.

I HAVE been asked to write a short comment on the experiment in E.S.P. which Mr Tyrrell and Miss Johnson have been carrying out in so far as I have personally witnessed it.

I have been present on five separate occasions on which trials have been made and the following table gives the details.

<i>Date.</i>	<i>Present besides G. N. M. T. and Miss J.</i>	<i>No. of Trials.</i>	<i>Successes.</i>
25 Oct. 34	Myself - - - - -	300	92
27 Oct. 34	Myself - - - - -	200	69
31 Oct. 34	Myself - - - - -	300	109
1 Nov. 34	Prof. Broad and myself - -	500	132
28 Feb. 35	Miss Jephson and myself - -	200	53

I have acted as operator in 400 trials with a total of 121 successes = 30.2 per cent.

I will not enter into any lengthy discussion of these figures except to say that it is clear that my presence alone did not act as a disturbing factor to so great an extent as did that of Prof. Broad and Miss Jephson. When I was the only visitor the percentage of success was 33.7, when another visitor was present it fell to 26.4. This can reasonably be explained by the fact that I have known Miss Johnson for a considerable time and that she is comparatively accustomed to my presence, whereas both Prof. Broad and Miss Jephson are relatively strangers.

This is confirmed by the fact that the first one or two runs of 100 trials which I witnessed gave a score which was considerably below the average for the whole.

Mr Tyrrell has described the apparatus and the *modus operandi* of the experiment, so I will confine my remarks to a few points on which I consider that criticism might be directed.

As regards *bona fides*, I, being well acquainted with those concerned, have no doubt whatsoever, but I recognise that my convictions are not evidence.

However, as the percentage of success for the four runs in which I acted as operator was nearly equal to that of the whole, it is clear that Mr Tyrrell is absolved from any complicity, unless, of course, I was myself a confederate. If this latter view be taken I have no more to say, seeing that no words of mine would then carry any weight.

The operator, himself, was the only person who was in a position to fake the record; it follows that lack of *bona fides* on Miss Johnson's part could only be in the form of some normal perception of the movement of the pointer. I examined the apparatus very carefully and watched the experiments being done with the utmost attention, and I feel confident that no normal perception was possible. I tried to act as subject myself and was able to obtain only chance score.

I suggested that the operator might have some habitual sequences in selecting where to insert the pointer and that Miss Johnson might unconsciously follow these, but the fact that the average score was reduced to so small an extent by my acting as operator meets this criticism. It is highly improbable that Mr Tyrrell and myself should have identical sets of habits in this respect, and Miss Johnson had had no opportunity of learning any habits which I may have.

The most formidable criticism is that of hyperaesthesia of hearing. Hyperaesthesia of sight I regard as being excluded, the operator being so completely screened from the subject. But as regards sound, while I am satisfied that no such explanation is possible, I admit that it cannot be completely eliminated with the present apparatus. Every precaution was taken to render the operation soundless, and, so far as normal hearing goes, I think successfully. But until a limit can be set to the possibility of hyperaesthesia, a definite pronouncement cannot be made. With the improved electrically operated apparatus now under construction this risk will be entirely avoided.

However, though theoretically the possibility of hyperaesthesia of hearing cannot be completely excluded from this experiment, I regard it, in view of all the circumstances, as almost negligibly small. Moreover, hyperaesthesia so marked and so consistent over so long a period would itself be a phenomenon quite as, if not more, super-normal than mere undifferentiated E.S.P.

There is only one further point, viz. the suggestion of unconscious whispering on the part of the operator. When witnessing the trials I was on the lookout for this; when operating myself I had it in mind and I was never able to detect the slightest signs. This is, I admit, not conclusive, for the whispering might be normally inaudible.

This risk again will be provided against in the electrical apparatus.

In conclusion I have no hesitation in stating that I am convinced, subject to an almost negligibly small reservation as regards hyperaesthesia and unconscious whispering, that true undifferentiated E.S.P. had been demonstrated in this experiment.

H. F. SALTMARSH.

OBITUARY NOTICE

MRS PHILIP CHAMPION DE CRESPIGNY

MRS PHILIP CHAMPION DE CRESPIGNY will be greatly missed by a large circle of friends with whom she has been associated in Art, Literature and Psychical Research. It was the latter which claimed her almost undivided interest since her attention was drawn to the subject by the death of her husband and a meeting with Mrs Etta Wreidt, the direct voice medium. Having satisfied herself that her husband had given convincing evidence of his survival Mrs de Crespigny made a careful and conscientious study of psychic phenomena in general. This accomplished it became her ambition to place the knowledge within reach of others. While Vice-President of the Ladies Lyceum Club she was instrumental in organising its Psychic Study Circle, and for the last five years has been President of the British College of Psychic Science. She was also Vice-President of the Marylebone Spiritualist Association. As a lecturer she was in much demand and, being well known in the realms of art and literature, attracted many who would otherwise have been but little interested in this subject.

It is an advantage that, in the home of her father, Admiral, the Right Honourable Sir Cooper-Key, G.C.B., F.R.S., she met many leading scientists. Throughout her life she endeavoured to study the laws underlying phenomena, and in Psychical Research it was her ambition to combine accurate observation with experiment and careful inference. Nor was her mind closed to the religious implication of the facts established, and many will be grateful to her for having brought consolation and confidence in their hour of bereavement or mental perplexity.

Her most recent book *This World and Beyond* (Cassell) is to some extent autobiographical and includes some account of experiences which brought the assurance that her deceased husband was in communication with her. It also recounts how on one occasion she was present at a demonstration of the psychic control of fire, and had the courage to hold in her hands a burning log which, under ordinary circumstances, would have inflicted severe burns, but which left her skin unhurt and gave only a feeling of pleasant warmth. To those who did not know her personally this book will reveal something of her genial mind. Her versatility was considerable and she moved with ease both on the surface and amid life's deeper problems. Her quick sense of humour was used effectively both in committee and in public speaking. Psychical Research and Spiritualism are losers by her departure.

C. D. T.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION.

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 29th May, 1935, at 5.30 p.m.

WHEN

MR KENNETH RICHMOND

WILL SPEAK ON

AN EXAMPLE OF THE EVIDENCE FOR
INTENTION IN BOOK-TEST MATERIAL

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

EXTRACT FROM NOTES OF A SITTING WITH
MISS FRANCES CAMPBELL

PRELIMINARY NOTE BY THE HON. EDITOR

The names involved in the following record must not be disclosed, but all are known to me, as is the date of the sitting, a recent one.

Before giving the record a few facts must be stated so that readers may estimate the results for themselves.

(1) The sitter had not sat with this medium before, nor had he seen her; nor had the purporting communicator sat with her or known of her.

(2) The sitting was arranged for in the following way: About two months or more before the sitting, The Rev. C. Drayton Thomas had given to this sitter Miss Frances Campbell's address and telephone number along with those of other mediums. At that time, the sitter had wanted these for a friend, not for himself; and, as a matter of fact, Mr Drayton Thomas was away on holiday in Cornwall when, one morning, the sitter rang up Miss Campbell's number, spoke to a third person and said,—‘ A sitter known to Mr Drayton Thomas wishes an appointment ’. An hour was named a couple of days ahead and agreed to, Mr Thomas knowing nothing about it.

(3) This sitter does not talk at sittings, beyond asking for more information (as below), or saying ‘ good ’ and ‘ thank you ’, or ‘ no ’ when a question can be denied; and, in the portion of the sitting quoted, he took no part, as it was given too quickly to allow of remarks whilst taking longhand notes.

(4) In regard to the main communicator, the personality depicted strongly suggested to the sitter a certain man, Y, who had died fifteen days before the sitting; the second communicator indicated equally strongly suggested Y's closest friend, who had predeceased him and who was also a close friend of the sitter.

(5) A second sitting, arranged for in the same way, the medium not seeming to recognise the sitter, was held about three weeks later. In this sitting there was a good deal of confusion which the sitter thinks he can explain. But, he reports that,—‘ While the same communicator, Y, purported to be communicating, reference was made to a picture, by way of indicating his profession of artist, and he was said to be pointing to the frame which the medium described as elaborate, saying that the communicator remarked, pointing to the frame, “ that's swagger ”. Now this was a peculiar term of praise for anything fine, used by the communicator when living. It was so distinctive a phrase that I [sitter] once said to him,—‘ If you

ever come back and say at a sitting "that's swagger", I shall know it is you."

We may now take the annotated record of the first sitting.

Copy of the Record of part of the Sitting, with annotations supplied by the sitter.

Text.

Annotations.¹

(After half an hour's sitting largely occupied by relations,—)

Sitter : Is there anyone more ?

Medium. A man in the spirit who wishes you to carry a message to a woman.

Was his head hurt ?

He got dazed. His heart failed.

He keeps on saying he was wearing his clothes.

He has a gesture of his own. He pushes back the sleeve from his wrist.

' Tell her, I heard her, I heard her. Thank God she talks to me as though I was still with her. Tell her I felt her patting my hand just before. I heard her saying and knew it was her :— " It is all right, it is all right."

He fell and broke his pelvis.

He died of heart-failure owing to pressure on the heart.

Yes. He had come in, got up from chair, having taken off walking shoes, crossed floor of sitting-room in his socks to get slippers, slipped and fell.

He did not wear a wrist-watch, and, though I do not remember this gesture with any conviction, on enquiring after the sitting, from some one who had lived for years with him, I was told he did this frequently. He was a highly nervous man, with such quick tensions.

A close friend, a woman, was with him when he died and told me, *after* the sitting, that she went on speaking to him when the hospital sister was telling her it was all over.

This was confirmed *after* the sitting. Unknown to me at time of sitting or to anyone except the woman and hospital sister.

¹ Known to the sitter at the time of the sitting, unless otherwise stated.

Not lonely be sure of that. I'm glad I didn't go on, I would have been a burden, far more than you know.'

Another person [says],—'He hoped that it would be all right but he didn't know, he didn't know.'

He's left earth only a little while. He is still weak.

He's nervous still. He makes some of us a bit embarrassed, he is so grateful.'

He says, "Something, some small thing was put in the coffin."

Then he says with clearness,—I was a clear-thinking man a couple of years ago, but I have not been quite so clear as I should have liked during these last two years.

I am back again to my old clear thinking.

He says, "I'm sorry I was impatient. I was impatient but that was because I used to get frightened".

He knows there was a query about his burial.

Autopsy showed this true, but known to me prior to sitting.

He was pretty sure that there was no survival; but shortly before his death had become a little less certain because of new work in science.

Died 15 days before sitting.

As I felt this was not 'in character', I made a point of asking someone who had known the putative person very intimately if this were possibly true. I was told when nervous he was apt to embarrass through a certain "un-English" courtesy.

Not confirmed.

Confirmed. Not known consciously to me prior to sitting.

When he was moved to hospital, he had been very upset. They took away his sedatives and would not permit him constant visits. He protested very strongly.

Only after sitting did I learn there had been so much trouble, but I knew there had been some.

It was thought he had left an order that his body was to be

Wednesday was his important day, the day that mattered to him, and three o'clock the hour that mattered. No, no, the hour and the day are different. Day and hour not the same day.

“Thank you for your special prayers — No! Meditative thoughts.” He laughs when he says that.

He was frightened because [he had] no clear idea of what would happen.

One woman, for there are two who care very much, would be frightened if he appeared. It would be no use to tell her.

He is very proud of a picture. It is to be put on the wall of a house, a place where people go to look at things.

He takes—or makes me see him doing it—his watch from his pocket and looks at it. He says that was a common gesture of his. He would take it out, look at it when he was talking and put it back without having noticed the time at all.

given to a hospital. Search was made for order, but not being found, cremation ordered by executors. Known to me before sitting.

The day he was taken to hospital.

I repeated as I wrote “Wednesday 3 p.m.” This provoked the protest “hour and day are different”. I presume that Wednesday was his worst day of distress. I did not know this at sitting, but was told after that that was the day he was most unhappy and frightened. Three p.m. was the hour he died on the Sunday following.

I practise certain routine methods of “recollection” and “meditation” regularly.

This might refer to his fear of death which he certainly had.

Two women closely associated: one, a materialist, is horrified at the thought of spirit communication.

He was an artist and collector of pictures.

A picture is to be “left to the nation”. It is a well-known masterpiece.

I am told this is true. I was consciously not aware of it.

There is a room he shows with books round in which he used to sit, and somewhere there is a clock he used to wind. There is a chair in the corner where he used to sit, a favourite place for him to sit.

His daughter (when she thinks of him) sees him sitting in that chair.

He says suddenly,—“ I'd rather have had that outing in the car and things have gone as they have, after all it is I that paid for it.”

He meets a dog called Jack.

And a man he used to argue with always. They always argued. Now they argue but they are friends. He says,—“ It is a great treat to stand straight ”, and puts his hand on his hip. He says he now feels quite free.

Suddenly he says,—“ D is there. Don ”.

Donald ?

“ No, no, DON.”

He says it would have made a great difference, such a difference, if he had known that he would see D.

D. knows you (sitter).

He is embarrassed at saying

I am told he used to wind a clock but don't remember it.

Yes, but not very evidential.

There is one daughter. He and she were not very intimate. She lived abroad much.

After undergoing a cure in France in July and early August, he drove himself back in his car making a tour. The autopsy showed that, as he was in such bad condition, he might have died on the way, and the strain may have hastened his end. The autopsy found arteries very hardened and a clot in one.

Not placed.

Not placed.

He was becoming lame from arthritis.

The medium put this query, not to the sitter, but to the wall at which she was looking, claiming to be talking to this person.

This led to the impatient reply, “ No, no, *Don* ”, with great emphasis.

D. is the initial of surname of a

the word but he says it, he says he wants to send you his love. D. is trying to be calm but really very moved that you wanted to talk to him.

He shows peculiar clothes, uniform? Clergyman's clothes? No, a cap and gown, and he shows in the background buildings like a school.

He is being hurried. He hesitates in speaking. He wanted to speak the other day, in a conversation the other day with another person known to you (Sitter), but they talked all about themselves and were interested in themselves.

He talks of the piano, connected with that other person somehow, and of old days of music. Handel's Largo is mentioned.

He says,—“Remember I liked my food. Wish you liked yours more.”

R. over here missed much not getting contact earlier.

Don who was a close friend of first communicator and of sitter.

This seems to refer to a Don at — —, —; a very close friend of the first communicator, Y, and of me (Sitter); he had died a couple of years earlier.

“D” was never a fluent speaker like Y. Very diffident.

A friend of mine and of “D” had had a sitting a week before. Sitting was arranged with exactly the same telephone anonymity as mine. On returning he gave me a description of the sitting and showed me brief notes. He obtained references nearly exclusively to himself, but thought there were one or two references to the “Don”. He certainly said nothing about me or any other of our friends. He is a very silent and cautious person.

The Don used to come to my home to hear the other communicator play music.

R. initial of the first communicator, Y.

CORRESPONDENCE

DR RHINE'S EXPERIMENTS

To the Editor of THE JOURNAL

MADAM,—In his Review of Dr J. B. Rhine's *Extra-Sensory Perception* in *Proc.*, xliii. 24, Dr Thouless appears to be criticising Rhine's work, not as being invalid in establishing the *fact* of E. S. P., but only as being invalid in establishing his high estimate of the frequency of its distribution, which he puts at about one in four. Dr Thouless says, "Indeed, there seems no reasonable doubt that if the D.T. experiments were carried out exactly as described, with adequately shuffled cards no other explanation than that of clairvoyance is possible. We have, however, plenty of other evidence in favour of the view that extra-sensory perception is to be found as a very exceptional mental power. The novelty of Dr Rhine's results lies in his apparent demonstration that this power is not uncommon and it is here unfortunately that his evidence is quite inadequately stated" (p. 36). And as Dr Thouless does not call in question the accuracy with which these results are stated but only criticises the inadequacy with which other results (not the D.T.) are stated, the reader is left with the impression that he accepts Dr Rhine's evidence as valid for the *existence* of E. S. P. (which is the major issue) but doubts the minor issue of its common occurrence; and that the latter only is the point which he is discussing. This point, he says, ought to be cleared up by repeating Rhine's experiments, for "his methods are so simple and his results so clear that his experiments can easily be repeated."

With regard to such repetitions, may I point out one thing, namely that there is a very important factor which some investigators seem inclined to overlook, and which certainly is not easy to reproduce, but which seems to be capable of exercising a decisive influence on the results? I refer to the factor of the mental or psychological atmosphere. The efficacy of this is apparently due to the fact that there exists a subconscious, controlling influence of some sort which has the power of enhancing, of inhibiting or even of reversing the results, and which is strikingly exemplified by Rhine's experiment with Linzmayer, reported on pp. 62-63 of his book, in which he urged the latter to go on with the experiments when he was feeling reluctant and uninterested, with the result that his scoring fell, first to chance value and then below it: and this against his conscious wish. And with Pearce we read, "Another factor that upsets Pearce's scoring, as a rule, is any change that he does not easily and spontaneously accept as likely to work"

(p. 77), which reveals the same influence operating again. I have also frequently noticed with Miss Johnson that the difference between a high score and a low one depends upon a most delicately poised psychological condition: for example, the fact of turning the conversation on a slightly unpleasant subject just before an experiment I have known to send the score down to chance value. Some subconscious entity evidently can act as a censor and it has complete power over the course of the experiments; and the conditions to which this censor is responsive are delicate and elusive in the extreme. Psychological "atmosphere" is, I am sure, one of the important ones.

Now, Dr Rhine's subjects were all working in a rather unusual psychological atmosphere. As students of psychology they were, presumably, familiar with the idea of psychological experiments: they were amongst people they knew in accustomed surroundings and the psychic experiments were merely adding a fresh zest to the ordinary routine of life without in any way upsetting it. And this was probably encouraging to the censor. These were conditions in marked contrast with those in which a person at random with unknown preconceptions about the whole subject (see note about Pearce above) is asked to sit occasionally for an hour at a time with strangers.

Dr Thouless seems to regard it as an easy matter for anyone to repeat Dr Rhine's experiments; but what I wish to stress is that anyone who "repeats" them without reproducing the psychological atmosphere has not really repeated them at all. The psychological atmosphere is elusive, but it is clearly of the utmost importance; and if the investigator disregards it, nature will not.

G. N. M. TYRRELL.

REVIEWS

Must Philosophers Disagree? By F. C. S. SCHILLER. London: Macmillan & Co. 1934. Pp. xi-359. Price 12s. 6d. net.

In this volume of twenty-four collected lectures and essays, most of which have been previously published, Dr Schiller covers a wide range of subjects which extend from the psychology of examinations to the problems of psychical research. He distributes these under the four headings of Educational, Historical and Speculative, with a concluding Philosophical Survey, and links them by a deliberate avoidance of technicality of style, which so often reduces, as he observes, the writings of philosophers to "verbalism and not literature". These readable essays, written in Dr Schiller's delightful style, are certainly the latter.

The initial question, "Must philosophers disagree?" is answered in the affirmative. They must, the author says, because Philosophy is not a single, coherent structure to which every philosopher adds his quota. Each philosophy is rather the product of an individual personality, stamped by that personality's history and character, and intelligible in the light of it alone. In fact, "Philosophy is best described as a sort of poetry, and often lyrical poetry at that." But, on the other hand, there need not be so much disagreement as there is. It arises because "brooding philosophers do not understand one another." They "keep their best trumps up their sleeves"; and etiquette taboos the asking of questions while a philosopher is alive; while, after he is dead, there is no way of ending the discussion as to what he really meant.

Some of the essays which follow contain a destructive criticism of syllogistic logic: and these are followed by an appreciative biographical sketch of William James, and an account of the Making of Pragmatism. There is also a particularly interesting essay on Nietzsche, whose ideas, the author thinks, may prove very infectious in the present state of the world. For, if present trends continue, "the gospel of the Superman may well seem preferable to that of the Social Insect." Nietzsche's 'immoralism' is presented rather as a transvaluation of values than as a negation of morality: but his unbalanced mind negates his own eulogy of the 'strong' by admitting that they are, after all, "weaker, less wilful, more absurd than the average weak ones. They are the *squandering* races." So that it is possible, after all, that the meek may inherit the earth by proving themselves to be more viable than their predatory neighbours.

Nietzsche, providing as he does an extreme example of the tendency amongst philosophers towards inward brooding and self-absorption, draws from Dr Schiller this rather delightful sentence. "In a queer philosophy", he says, "written by a crank, I once came across the frank admission that he had written his book in the hope of rendering his ideas intelligible to himself; ever since I cannot but think that this must be the real secret of many renowned philosophers."

The same article affords a ground for an interesting discussion of the Humanist theory of knowledge, which Nietzsche himself came half way towards accepting. As also with Vaihinger and others, he agrees with the doctrine that "'Knowledge', 'truth', 'logic', nay, even 'perception', are not in fact reproductions of the given, but manipulations of it and operations on it, which variously and wondrously transform it." But he drew the conclusion that the world which concerns us is subjective and therefore false. "Granting that all knowledge involves human manipulation, that truth is essentially

a valuation, that sensations do not occur, and that perceptions are already impregnated with valuations. because the original data were a chaos . . . ” yet, why should the inference be drawn, the author asks, that “ the world which concerns us at all is false ? ” Why the humble phrasing, “ it is all only subjective ? ” “ It is all *our* work ! ” rejoins Dr Schiller. “ Let us be proud of it.”

But the reader is left wondering why the protest should not be carried further. Why should the original data be admitted to be a chaos ? Granting the existence of an effective human factor in all these things, is there no theory of knowledge which will combine it with recognition of an ultimately ordered reality ? Is there no bridge which will span the gulf between Plato and Protagoras ?

The later section of the book contains essays on the subjects of Evolution, Creation, Novelty and the Metaphysics of Change. Here the author deals with the frequent confusion between the terms ‘ Evolution ’ and ‘ Epigenesis ’. But the crux of the whole matter resides in our conception of the meaning of Novelty. “ It is true that nothing ever arises out of absolutely nothing. There is always something out of which it grows. But that does not explain it wholly. It does not account for the *new* in it. It is only in so far as it is still the old, or the old over again, that it is accounted for by what it grew out of. In so far as it is new, it remains unaccountable, unpredictable, uncontrolled, undetermined, free. *That* factor in it, therefore, *has* arisen out of nothing, and Novelty as such *means*, Creation out of nothing ! ” And so, “ There is no basis in experience therefore, for the philosophic fancy of any unchanging substratum of change. . . ”

At any rate, this view engenders a hopeful and helpful attitude towards Psychical Research, the article on which is a reprint of Dr Schiller’s Presidential Address delivered to this Society in 1914. The question of proof by an accumulation of probabilities, which Dr Schiller supports in opposition to the demand for a single, irrefragable case, may well be revised in the light of scientific advance since that time. Physical law seems now to rest ultimately on probabilities. “ As we probe deeper,” says the author in a later written essay, “ all the objects of scientific interest are turning out to be immensely more complicated, individual, nay unique, than anyone suspected.” We are not, in fact, in science dealing with the essence of these individual and unique objects, but only with probabilities concerning their external relationships. Should we not give probability a logical value in psychical research ?

The central importance which Humanism attaches to the individual opens a wide vista of possibilities. “ We have no means of proving,” says Dr Schiller in his concluding pages, “ that our whole

life, with all the 'truths' it supports and all the 'realities' it attests, is not itself a 'dream': consequently death itself, when we come to it, may appear like awakening from a nightmare. This suggestion is an old one and there is much to be said in its favour . . .": and I think one may confidently assert that this generosity in the *a priori* attitude contrasts favourably with the crabbed and limited outlook which is dictated by the ingrained suggestions of common sense and an unphilosophical science. It throws open the door to infinite possibilities and creates an atmosphere in which human life may be lived in a spirit of high adventure.

G. N. M. T.

F. W. H. Myers' *Human Personality and its Survival of Bodily Death* is of central importance to every student of Psychological Research. The original edition in two volumes is not always easy to come by. We therefore welcome the appearance of a new abridged edition, published by Messrs. Longmans in the Swain Library (price 3s. 6d. net). The book has been edited and abridged by Myers' daughter and elder son, and runs to 300 pages, in addition to a useful biographical sketch.

In Psychological Research the old books are still the best: neither *Human Personality* nor *Phantasms of the Living* are nowadays as much read as they should be. This inexpensive and handy edition ought to encourage fresh study of one of the classics of our literature.

W. H. S.

THE JOURNAL IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL
OF THE
Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 26th June, 1935, at 5.30 p.m.

WHEN A PAPER ON

USEFUL GHOSTS

WILL BE READ BY

MISS INA JEPHSON

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

THE RESEARCH DEPARTMENT

THE Council have decided that in the present circumstances of the Society it would be desirable to appoint a young graduate of a British University as Research Student, to be trained in the Society's methods, with a view to possibly becoming Research Officer later.

PRELIMINARY EXPERIMENTS IN PRECOGNITIVE GUESSING

BY WHATELY CARINGTON, M.A., M.Sc.

“ *Why think? Why not try the experiment?* ”

Hunter, in a letter to Jenner.

1. Readers of the *Journal* will recollect that I appealed, a few months ago, for collaborators in some experiments in precognitive guessing which I was then planning. These were prompted by Mr Saltmarsh's "Report on Cases of Apparent Precognition" (*Proc. S.P.R.*, Part 136), and their object was to see whether any *prima facie* evidence of precognitive ability could be obtained experimentally.

I am deeply indebted to thirty-nine members of the Society and their friends not only for the trouble they have taken in providing me with material, but for the very high standard of care and conscientiousness they have maintained. In particular, I have to thank Mr and Mrs Saltmarsh, and Mrs B. V. Tennant, for notable contributions; while Mr J. Fraser Nicol, of Edinburgh, has earned my very special gratitude for the enthusiasm and thoroughness with which he organised a whole group of collaborators in that city. I am also again greatly obliged to Professor Fisher for valuable advice and help.

2. The procedure adopted was that of throwing an ordinary six-sided die from a "shaker" in the usual way, and subjects were required to write down, *before* throwing, what they thought or felt the number about to be thrown would be.

These guesses and throws were recorded on specially printed forms arranged in four columns of thirty guesses each, giving 120 guesses to the sheet. A full "set" of guesses consisted of ten such sheets, normally done on different days, making 1,200 guesses in all. The words "column", "sheet" and "set" will be used in the above-indicated senses of 30, 120 and 1,200 guesses respectively throughout this report. The word "group" will be used generically.

3. One set from one subject was so spectacularly successful as to be quite clearly *hors concours* and nothing but confusion would result from including it in the general analysis of the data. It is accordingly withdrawn and reserved for separate discussion in Section 13 below. Another set from the same subject was, however, obtained under different conditions, and this is included.

The whole of what follows, prior to Section 13, is accordingly subject to the reservation "excluding Mr X's freak".

4. TABLE I shows the numbers of guesses for the various subjects, their successes, and the probabilities that the results obtained were due to chance alone.

The first column gives the reference number of the subject; the second shows the total number of guesses made; the third gives the expected number of successes on the assumption that chance alone is at work; the fourth gives the number of successes actually scored; in the fifth is the value of χ^2 computed in the ordinary way from the difference between the numbers of successes observed and expected, and the number of guesses involved; in the last is the probability of this difference being due to chance. A minus sign is prefixed, purely as an indicator, to those values of χ^2 corresponding to successes *below* expectation.

TABLE II gives details, by sheets of 120, of 38 complete sets of 1,200. The first set is an exception, because I unwisely did this set (my own first attempt) in sheets of 100. The first 24 sets are first attempts, hereinafter described as "fresh" sets. Numbers 25 and 26 were actually preceded by 270 guesses each scored in sheets of 90 before the technique had been standardized; they are, however, counted as "fresh" for the purposes of subsequent calculations. The remainder are all second, third, etc., attempts. An asterisk indicates the sets which were controlled and certified by an experimenter and/or witnesses; these will be referred to as "experimenter" sets.

It is interesting to note in passing that these 23 experimenter sets score 4,657 successes—an excess of 57 over expectation—as compared with 2,948—a deficiency of 52—by the 15 "solo" sets. For what little it is worth it indicates that the soloists took no advantage of their freedom from supervision.

It is clear that these 40 subjects, *taken as a group*, afford no justification for supposing that any precognitive faculty is at work, for the total number of successes is exactly equal to expectation.

On the other hand, if we consider individuals, we find two subjects giving odds of better than a hundred to one (in opposite senses), two better than thirty to one (also in opposite senses) and twelve

TABLE I

SN	G	E	S	χ^2	P
1	6,000	1,000	978	- .581	.22
2	4,110	685	675	- .175	.34
3	3,600	600	625	1.250	.13
4	2,790	465	436	-2.168	.07
5	2,400	400	403	.027	.43
6	2,400	400	400	.000	.50
7	2,370	395	446	7.902	<.01
8	1,320	220	187	-5.940	<.01
9	1,200	200	192	- .384	.27
10	1,200	200	195	- .150	.35
11	1,200	200	215	1.350	.12
12	1,200	200	220	2.400	.06
13	1,200	200	218	1.944	.08
14	1,200	200	183	-1.734	.09
15	1,200	200	193	- .294	.29
16	1,200	200	224	3.456	.03
17	1,200	200	209	.486	.24
18	1,200	200	204	.096	.38
19	1,200	200	195	- .150	.35
20	1,200	200	201	.006	.47
21	1,200	200	184	-1.536	.11
22	1,200	200	183	-1.734	.09
23	1,200	200	222	2.904	.04
24	1,200	200	176	-3.456	.03
25	1,200	200	201	.006	.47
26	1,200	200	184	-1.536	.11
27	1,080	180	196	1.707	.10
28	840	140	142	.034	.43
29	660	110	110	.000	.50
30	360	60	65	.500	.24
31	240	40	47	1.470	.11
32	240	40	46	1.080	.15
33	240	40	36	- .480	.24
34	240	40	37	- .270	.30
35	120	20	15	-1.500	.11
36	120	20	20	.000	.50
37	120	20	15	-1.500	.11
38	120	20	23	.540	.23
39	120	20	15	-1.500	.11
40	120	20	19	- .060	.40
Totals	51,240	8,535	8,535	.000	.50 . . . 1

(*N.B.*—The number of participants is raised to forty by the inclusion of my own 6,000 guesses.)

TABLE II

SUCCESSSES FOR SETS OF 1,200 GUESSES BY SHEETS OF 120

Set	SN	Sheets										S	χ^2	P
		1	2	3	4	5	6	7	8	9	10			
1	1	Worked in sheets of 100; see text										(181)	-2.166	.07
2*	9	23	19	13	19	17	19	20	17	19	26	192	- .384	.27
3*	10	13	16	21	26	16	19	21	20	23	20	195	- .150	.35
4*	11	18	19	32	22	23	16	25	22	19	19	215	1.350	.12
5*	12	18	26	18	25	27	19	17	23	27	20	220	2.400	.06
6*	13	18	28	19	21	22	30	20	24	22	14	218	1.944	.08
7*	14	22	18	9	21	21	20	21	15	14	22	183	-1.734	.09
8*	15	14	20	22	21	28	20	14	19	15	20	193	- .294	.29
9*	16	23	27	25	27	23	18	18	23	17	23	224	3.456	.03
10*	17	24	22	21	19	26	18	23	18	16	22	209	.486	.24
11*	5	18	24	20	19	18	23	23	20	23	18	206	.216	.32
12	8	22	21	17	19	13	18	14	13	14	15	166	-6.939	.005
13*	3	14	26	28	24	12	25	18	18	25	16	206	.216	.32
14	6	18	15	19	17	24	32	19	17	10	20	191	.486	.24
15*	18	17	16	18	20	17	25	19	20	25	27	204	.096	.38
16*	19	17	18	18	28	12	22	25	19	19	17	195	- .150	.35
17*	20	17	20	18	22	17	24	23	21	23	16	201	.006	.47
18	21	21	17	20	20	15	23	14	23	16	15	184	-1.536	.11
19*	22	17	19	13	16	16	24	20	14	26	18	183	-1.734	.09
20	23	20	31	17	18	13	25	19	19	32	28	222	2.904	.04
21	24	11	14	18	12	18	17	17	26	23	20	176	-3.456	.03
22*	25	19	19	22	19	21	20	23	17	18	23	201	.006	.47
23*	26	18	16	23	18	16	22	12	14	23	22	184	-1.536	.11
24*	7	24	19†	21	19	23	28	23	26	25	20	228	4.704	.015
25*	4	18	18	21	21	26	16	13	18	18	18	187	1.014	.16
26	2	25	30	17	13	18	20	23	17	22	17	202	.024	.44
27*	5	14	25	21	17	15	24	18	20	28	15	197	- .054	.41
28	1	17	20	15	19	20	19	21	23	23	15	192	- .384	.27
29	1	20	15	21	17	24	25	20	16	23	28	209	.486	.24
30	1	23	21	26	13	23	22	18	19	14	13	192	- .384	.27
31	1	21	23	14	19	15	24	19	26	21	22	204	.096	.38
32*	3	15	17	17	18	24	28	21	20	21	23	204	.096	.38
33	3	29	19	20	22	23	19	16	22	24	21	215	1.350	.12
34*	7	17	25	24	23	17	27	22	19	26	18	218	1.944	.08
35	6	22	16	14	24	21	26	27	20	22	17	209	.486	.24
36*	4	18	21	18	20	23	18	19	17	23	17	194	- .096	.38
37	2	27	17	16	21	26	21	14	20	19	18	199	.006	.47
38	2	22	16	20	19	22	21	19	23	23	21	206	.216	.32
Totals		714	753	716	738	735	817	718	728	781	724	7,424		
												(7,605)		

Note.—SN stands for Subject Number; S for the total number of successes in the set; P for the chance of this score being obtained by accident.

between twenty-four to one and eight to one. We should expect a certain proportion of such relatively long odds to occur in a sample of forty subjects, even if chance alone were at work; in fact we should expect a hundred to one chance to come off about once in a hundred times, so that we need not feel particularly surprised at finding two of them in forty, especially as they are in opposite senses. Inspection suggests, however, that there are rather more of these specially successful (or unsuccessful) cases than chance alone would give, and we must accordingly consider the possibility of there being real tendencies to score above and below expectation operating with about equal frequency among our subjects. Either tendency, if significant, would imply an element of genuine precognition; but in the one case it would lead to the throw being forecast successfully, while in the other we should be obliged in addition to postulate some kind of reversal of effect leading to a below chance score. This hypothesis may seem a trifle far fetched, but it must not be dismissed *a priori* on that account.

5. The effect of such opposite tendencies would be to increase the dispersion, or "scatter", of the success totals (and their corresponding χ^2 s), which would be normally distributed if chance alone were at work.

There are various methods of testing for this kind of thing and, as I shall use them extensively below, I must devote a few paragraphs to them here.

A. χ^2 is itself distributed in the χ^2 distribution; thus, if chance alone were at work, we should expect to find 10% of the values given in the Tables (i.e., 4.0 in the case of TABLE I and 3.8 in the case of TABLE II) lying between the limits 0.0000 and 0.0158, 10% between 0.0158 and 0.0642, and so on up to the last 10% greater than 2.706. (Cf. Fisher, *Statistical Methods for Research Workers*, sections 15 and 20, pp. 60 and 83 in 4th Edn.).

We compute χ'^2 from the observed and expected frequencies of the original χ^2 s in these classes and find the chance of the distribution being fortuitous from the tables of χ^2 in the usual way. The number of degrees of freedom (hereinafter abbreviated to DF) is one less than the number of classes used.*

The 40 values of TABLE I give (with 7 DF)

$$\chi'^2 = 2.20; P = .47 \dots \dots \dots 2$$

which is negligible.

* There is some divergence of opinion as to the minimum size of class (as defined by the frequency expected) which it is legitimate to use. A minimum expectation of 5 is on the safe side, but we shall not come to much harm if we relax this to 4.0 (or 3.8, say, for Table II) where the figures make it convenient.

B. The sum of a number of χ^2 s is distributed in the χ^2 distribution, with DF equal to the sum of the DF associated with the constituent χ^2 s. Where this last sum is greater than 30, so that the corresponding values of P are not given in the tables, we use the fact that $\sqrt{2\chi^2} - \sqrt{2n-1}$ is distributed with unit standard error, where n is the number of DF concerned.

Applying this to TABLE I we obtain

$$S(\chi^2) = 52.306 ; P = .09 \dots\dots\dots 3$$

which is far from significant, though "signs of life", as one might say, are beginning to appear.

This test is more sensitive than A, where precautions of the kind indicated in the note are apt to have a somewhat repressive effect. On the other hand, it is perhaps unduly affected by the smaller fragments as compared with the larger groups.

C. In cases where the groups considered contain equal number of guesses, e.g., the sets shown in TABLE II, we may compare those observed with the expected variance of the success scores. The former is given by

$$S(x-x)^2 / (m-1)$$

where m is the number of groups, and the latter by npq , where n is the number of guesses in a group, p the chance of a guess being right, and q the chance of its being wrong.

The expected ratio of the two quantities is unity with standard error $\sqrt{2/(m-1) + 1/mnq}$.

Since we are here concerned only with dispersion, I use p' and q' , the *a posteriori* chances instead of the *a priori* p and q . These of course are S/N and $(N-S)/N$ respectively, where S is the grand total of successes for all groups and N the grand total of guesses.

Applying this to the 38 set scores of TABLE II, and writing OV for the observed and EV for the expected variance, we get

$$OV/EV = 1.2125 ; P = .18 \dots\dots\dots 4$$

where P is the chance of the excess of observed over expected variance being accidental.

We may apply the same process to the 370 sheet scores of TABLE II, or to the 1,480 column totals (not reproduced) and obtain

$$\text{For sheets } OV/EV = .9886 ; P = .44 \dots\dots\dots 5$$

$$\text{For columns } OV/EV = 1.0021 ; P = .48 \dots\dots\dots 6$$

It will be noted that the observed variance for sheets is actually smaller than the expected, though negligibly so.

D. In cases where, in addition to the groups containing equal numbers of guesses, they can be divided into sub-groups of equal size, e.g., the sheets of TABLE II, we may apply the full ritual of the Analysis of Variance, in precisely the same way as for the study of the quantity termed "Individuality" in my analysis of reaction times, etc., in the

case of trance personalities. The details are too elaborate to describe here.

This method is potentially important because it takes account of the consistency or otherwise of the subjects in addition to their gross scores. The latter might be nothing startling, yet might become significant if the internal consistency were high. For example, a subject scoring 21 in all ten sets would yield a value of P, by the method using only the gross score of .22; yet the chance of his scoring above expectation ten times running is a good deal less than one in a thousand. *Mutatis mutandis*, the same applies to the problem we are here considering.

6. We will now apply these methods, as appropriate, to various selections of data and tabulate the results. The figures in brackets are result numbers for purposes of future reference, if need be.

N.B.—A tiresome complication arises here, however, because in the case of an important subject (No. 7) a whole column (30 guesses) was omitted from the second sheet of the first set. This makes no difference to the χ^2 figures, which are computed from the guesses actually made; but it upsets methods C and D, for which homogeneous material is necessary. In working these figures I have accordingly credited the subject with the expected score, namely five successes, for this column (judging by the rest of her performance she is more likely to have scored six), which seems the least misleading procedure and likely to give the most reliable results. The purely illustrative examples in the text have been worked from the figures as they stand.

Note also: (i) My own first set is included only in "38 sets". (ii) The values of P found in the appropriate tables have been halved for methods C and D, but not for A and B. For these, the value shown should be understood as meaning "a distribution of the kind observed, or more so, is to be expected in $100 \times P\%$ of cases, if chance alone is at work; while for C and D read "there is a probability P that chance alone will produce effects of the observed magnitude *and in the observed sense*", or words to these effects in the two cases. (iii) An asterisk (*) indicates that the effect is in the direction of unduly *low* dispersion.

TABLE III

	Method A	Method B	Method C	Method D
	P	P	P	P
All Subjects - -	.94 (8)	.09 (9)
38 Sets - - -	.80 (10)	.40 (11)	.12 (12)	...
37 Sets - - -	.88 (13)	.48 (14)	.18 (15)	.15 (16)
25 Fresh Sets - -	.12 (17)	.05 (18)	.015 (19)	.06 (20)
12 Other Sets - -	.53 (21)	.93 (22)	.005* (23)	.09* (24)
23 Experimenter Sets	.30 (25)	.40 (26)	.34 (27)	.12 (28)
14 Other Sets - -	.75 (29)	.22 (30)	.35 (31)	.26 (32)
370 Sheets - - -	.93 (33)	.42 (34)	.45* (35)	...
1,480 Columns - -48* (36)	...

The table is somewhat redundant and not very interesting; in fact I suspect that the data throw more light on the methods than the methods on the data! For the most part, there is little reason to suspect the operation of anything but chance, although—if the ordinary affairs of life alone were concerned—we should scarcely hesitate to accept results 19 and 23 as significant. The latter is very peculiar, and I do not profess to understand it, but the former strongly suggests that, in “*fresh*” (first attempt) sets, there is a tendency for subjects to score unnaturally high or unnaturally low.

Personally, I should hesitate about hanging the proverbial dog on this evidence alone, and I certainly should not care to rest a case for precognition on it. On the other hand, if we find—as I think we shall—that there is good reason, on other grounds, for concluding that something of a precognitive character is going on, then we may legitimately interpret these results as indicating that the “*freshness*” of a set is important to its operation, and that it is liable to operate in either direction—that is to say, either towards success, or, by reversal, towards failure.

I do not think that the study of dispersion can usefully be carried further, but before I leave the topic there is one point of considerable methodological importance to be noted. The tendency to high dispersion may be genuine—I think it is—but, even so, it is clear that it could not be deduced with assurance from a mere inspection of the figures—that is to say, it is not manifestly and without need of testing beyond the reasonable range of chance. Consequently, if we had been testing these subjects as a matter of routine, it would have been dangerous to pick out the best (and/or worst) performers for intensive study, since we should expect to get very much this range of success and failure, by chance alone, in a sample of this size. We might then find ourselves in the position of trying to account for an apparent lapse of paranormal faculty by searching for change of conditions, etc., whereas the original high (or low) score, though intrinsically “*significant*”, would have been only one of the abnormal scores to be expected in a large sample. The full epistemology here is evidently not easy, but it seems clearly indicated that it is likely to be better to work with a considerable group of subjects (if only to provide a night sky against which the stars may shine) until, if ever, some establish their regular paranormality by the consistency of their performances.

7. Reference to TABLE II will show that the totals for the different sheets (occasions)—i.e. the numbers 714, 753 ... etc., at the foot of the page—vary in an apparently very remarkable manner.

TABLE IV shows these totals split up for fresh, experimenter,

etc., sets, while the line of Ps shows the probabilities, obtained by method D, that the inter-total variations are a matter of chance alone.

N.B.—It is necessary to adopt some such procedure for the kind of reason I have just discussed. The chance of obtaining 817 successes in $37 \times 120 = 4,440$ throws, as for the sixth sheets, is small; the expectation is 740 and the observed excess over it is 77; whence we have

$$\chi^2 = 4.807; P = .03 \text{ v.n.} \dots \dots \dots 37$$

But we have to consider this result, not as an isolated event, but in the observed context of nine others, and to ask how often variations of the kind shown by these totals are likely to occur by chance in a sample of this size.

TABLE IV

Sheet	Fresh and Other.		Experimenter and Other.		All Sets.
	F	O	E	O	
1	469	245	422	292	714
2	523	235	489	269	758*
3	490	226	459	257	716
4	506	232	486	252	738
5	482	253	451	284	735
6	543	274	501	316	817
7	484	234	456	262	718
8	483	245	450	278	728
9	514	267	495	286	781
10	496	228	453	271	724
P	.06	.09	.12	.26	.15
RN	(38)	(39)	(40)	(41)	(42)

It will be seen that, in their context, the fluctuations are not very surprising, though it is interesting to note that the sixth sheets give the highest total for all groupings of the data, and that there is always a peak at the ninth total also. Again, the results are far from justifying the conclusion that anything paranormal is occurring; but, again also, if we accept this on other grounds, we shall find them interesting. The suggestion is that some psychological effect favourable to success is produced at the sixth "sitting"—e.g. the feeling that half the rather tedious business is over and the back of the work broken; while the same sort of thing occurs at the ninth—perhaps that "the end is in sight". The last sheets we must suppose to be done casually and merely for the sake of getting the job finished. The possibility of fitting some such psychological explanation to the facts seems to me to add greatly to the likelihood that they are not, in fact, fortuitous—it would have been different if, for example, the peaks had come at the fourth and seventh sheets; and, incidentally, we have here again (supposing the indications to be veridical) good indirect testimony to the *bona*

fides of the subjects. It seems very unlikely that they would extensively decide to throw in a few spurious successes in the sixth sheet, and conscientiously remove them from other sheets so as to make the grand total come to the expected value!

But I am running far ahead of my data here and we must now turn to a quite different form of analysis, which does seem to show conclusively that some factor other than chance is at work.

8. So far we have concerned ourselves only with the *numbers* of successes; we will now discuss their *grouping*.

Since the chance of any given success being followed by another is, in general, one sixth, it is easy to see that a certain number of "runs" of two, three, four or more successes will occur, even if chance alone is at work. Theoretically, one sixth of the guesses should be successful, and twenty-five thirty-sixths of these successes should be singletons while the others are followed by one or more further successes.

But in practice it is undesirable to deal with the pairs, threes, etc., partly because it is unduly laborious and partly because the expectations soon become too small to be useful. But we can estimate the normality of the grouping, or "packing" as I may term it, just as well by comparing the observed with the expected number of "singletons"; for there will be a deficiency of these if the packing is "close" (i.e., if there is an undue tendency to form pairs, threes, etc.) and an excess if it is "loose".

This is equivalent to enquiring whether the fact of guessing successfully makes the subject more or less likely to guess successfully next time. Either would involve a paranormal factor of essentially precognitive character, but again we have the possibility of this working in either of two opposite directions.

It is important to note that, in calculating the expected numbers of singletons we must not use the *a priori* values, one sixth and five sixths, for the chances of success and failure, but *a posteriori* values calculated from the known number of successes in the group. Our problem is not "how many singletons do we expect to find in a group of n guesses?" (to which the answer is $25n/216$) but "how many singletons in a group of n guesses of which a total of t have been successful?" We want to know, in fact, whether the packing is abnormally close or loose *given* the number of successes actually observed in the group.

The singletons have, naturally, been scored by sheets, because each sheet was (in general) the product of a separate session, and I have allowed myself the slight latitude of treating each sheet as a "ring"—that is to say, of pretending that the first guess follows

on the last, so that if both were successful a pair would be counted instead of two singletons. This avoids trouble with end effects and is quite harmless in view of the fact that, as will appear below, I have developed a purely differential method for testing the results obtained.

In these circumstances, the expected number of singletons is

$$E = t(n-1)(n-t-1)/(n-1)(n-2)$$

where n is the number of guesses in the group and t the total number of successes.

Then if s is the number of singletons actually observed in the group we have

Total successes observed	-	-	-	t
Singleton „ expected	-	-	-	E
„ „ observed	-	-	-	s
Difference	-	-	-	$E - s$
Grouped „ expected	-	-	-	$t - E$
„ „ observed	-	-	-	$t - s$
Difference	-	-	-	$s - E$

Whence χ^2 equals $(E - s)^2/E + (s - E)^2/(t - E)$, which reduces to $t(s - E)^2/E(t - E)$.

9. We are thus in a position to test any group or, by methods A or B above, any group of groups—for normality of packing, and as soon as I applied the test, very remarkable results began to emerge.

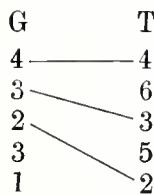
It so happens that there are 57 groups of data in my files, namely 38 complete sets of 1,200 and 19 assorted fragments of varying sizes. Each of these has some kind of claim to autonomy (that is why they are separated) and it therefore seemed appropriate to use these 57 groups as units, so to speak, for the test and to determine the value of χ^2 as above for each.

I accordingly calculated the expected number of singletons for each *sheet*, summed these expectations for the *group*, and computed χ^2 from the difference between this sum and that for the singletons actually observed in the group. High values of χ^2 were comparatively common and $S(\chi^2)$ for the 57 groups came to 129.054; $\sqrt{2}\chi^2$ is 16.066 and $\sqrt{2 \times 57 - 1}$ is 10.630; the difference between these is 5.436 and since this difference is distributed with unit standard error (cf. section 5 B above) the probability of the observed effect being due to chance is something less than 10^{-7} , or one in ten million, (RN 43).

10. This result constitutes coercive evidence of “anomalous packing” and, if it is veridical, is clearly of the very utmost importance, for there can be no reasonable doubt that some potent factor other than chance in the usual sense of the word must be involved.

On the other hand, it might be contended that it is at least not inconceivable that the effect is due to some peculiarity of the "distribution"—in the statistical sense—of the guesses or throws, or both ; as, for example, by the subjects calling numbers in far from equal proportions and/or the dice being biassed or improperly thrown. A suitable combination of the two abnormalities could, of course, account for high or low *scoring*, but I cannot see at present how it could affect the *packing* here studied. But this may be due merely to my own limitations and, in any event, it is always better to "try the experiment".

I accordingly went through all the material again, comparing each guess, not with the throw for which it was intended, but with the *next*. I then repeated the process scoring each guess against the throw *next but one*. I shall refer to these as "plus one" and "plus two" comparisons (or guesses, successes, etc.) respectively. The following figures should serve to make all clear :



The two fours constitute a normal success ; the two threes a "plus one" success ; and the two twos a "plus two" success.

It will be understood that we have to discard the first throw and the last guess when scoring for plus one successes, and the first two throws and last two guesses when scoring for plus two successes. We are accordingly left with 99.17% and 98.33% of the original material arranged in the original order ; the only significant difference is that for the normal scoring the guesses were, so to say, "aimed" at the throws with which they are compared, while in the other two cases they were not. This puts the whole enquiry on a pleasantly differential basis and effectively short-circuits, I believe, any possibility of artifacts arising from abnormalities of distribution, etc., of the kind just mentioned.

I also repeated, of course, the whole procedure of calculating χ^2 s for singletons as already described. Repeating the result for normal successes we have :

	S(χ^2)	P	
Normal singletons	129.054	10 ⁻⁷43
Plus one ,,	80.017	.04544
Plus two ,,	72.258	.1745

The difference between the normal and plus one figures is obvious and can also be directly tested for significance as follows: $\sqrt{2}\chi^2 - \sqrt{2n-1}$ is distributed with unit standard error (cf. Fisher, *loc. cit.*, 4th Edn., p. 62) whence, by the ordinary rules, $\sqrt{2}\chi_1^2 - \sqrt{2}\chi_2^2$ is distributed with standard error $\sqrt{2}$. (N.B.—This, as Professor Fisher points out, “comes very neatly to the same as saying that $\chi_1 - \chi_2$ has a standard error unity” and is “a new and pleasing peculiarity”.)

Applying this we have

$$(\sqrt{2\chi_{+0}^2} - \sqrt{2\chi_{+1}^2})/\sqrt{2} = 2.415 \text{ and } P = .01 \dots \dots \dots 46$$

for the chance that the value of $S(\chi^2)$ for the normal singletons exceeds that for the plus one singletons as a result of chance alone.

The difference between the values for plus one and plus two singletons is not significant, and it is for consideration whether the value for P of .045 for the former is accidental or genuine.

The sum of χ^2 s for plus one and plus two singletons combined is 152.275 with 114 DF and this gives a value of $P = .02$ (RN 47).

On the whole I think there is little doubt that the effect of anomalous packing which is so very marked for normal successes extends also in lesser degree to those of the plus one and plus two variety.

If my strength were as the strength of ten and I could command a forty-eight hour working day, I would pursue the point a few stages further, that is to say, for plus three, plus four, etc., successes. As it is, I fear that refinements of this kind will have to wait until we have a machine which will score guesses and throws (or other events) mechanically and will also enable us to count normal and “plus- n ” successes by singletons, pairs, threes, etc., automatically.

I should like to take this opportunity of saying that, unless the foregoing results be wholly rejected, the provision of such a machine (which should not prove prohibitively difficult to design or expensive to construct) appears to me to be almost indispensable for the proper prosecution of this class of work and of paragnostie studies generally.

The foregoing shows clearly how material which is of no apparent interest when taken in bulk may become highly informative when methods of sufficient resolving power are applied to its fine structure. And I speak from experience when I say that—quite apart from considerations of accuracy and error—the application of such methods of deep analysis by human labour alone is quite impracticable on any extensive scale or as a matter of routine.

11. To revert : TABLE V shows the values of $S(\chi^2)$ for singletons, calculated as above for normal, plus one and plus two successes and various selections of data.

TABLE V

		NORMAL		PLUS ONE		PLUS TWO	
		$S(\chi^2)$	P	$S(\chi^2)$	P	$S(\chi^2)$	P
57 Groups	- -	129.054	10^{-7}	80.017	.045	72.258	.17
40 Subjects	- -	91.776	10^{-5}	52.138	.15	49.249	.30
38 Sets	- -	74.545	.001	54.586	.07	51.804	.13
19 Fragments	- -	54.509	.01	25.431	.16	20.454	.4
26 Fresh Sets	- -	53.430	.001 v.n.	37.045	.075	33.047	.15
12 Other Sets	- -	21.115	.05	42.972	.01*	39.211	.01*
23 Experimenter Sets	- -	58.017	.01	30.813	.15	16.677	.85
15 Other Sets	- -	16.528	.4	49.204	.01*	55.581	.01*

Note.—In this case, fresh and experimenter sets have a higher mean value of χ^2 than non-fresh and non-experimenter. There are 26 fresh and 15 non-experimenter sets, because my own first set can be used here. Values of P marked * are actually much less than .01, but the tables available do not go beyond this point.

These results may be taken as numbered for reference from 47 to 67 inclusive, starting with the first of the second line and reading successive lines from left to right in the obvious way.

The change over from the “superiority” of fresh and experimenter sets for normal successes to the reverse for plus ones and plus twos is very remarkable and unquestionably significant ; but I cannot at the moment account for it.

Among the 57 groups just discussed there are 24 “close packed” and 33 “loose packed” ; the expectation is, of course, 28.5 on the hypothesis of chance alone, and this leads to

$$\chi^2 = 1.421 ; P = .12 \text{ v.n.} \dots \dots \dots 68$$

which is no more than suggestive. The indication is that, generally speaking, a successful guess tends to reduce rather than increase the chance of another success immediately following ; the effect of success, in fact, appears to be inhibitory, but more work is needed to settle the point fully.

12. We may conclude with complete assurance that there is a non-chance factor involved in the “packing” of normal successes, and that this probably (though much less certainly) extends to plus one successes and possibly, in smaller degree, to plus two successes also. The differential method employed has made it clear that this effect cannot be due to any peculiarities of the data themselves (or, if the tendency to abnormal packing among plus one and plus two successes be ascribed to this, then there must be

significantly a further factor operating in the case of normal successes). There remains, however, as so often in the present state of psychological research, the odious question of possible falsification or pathological mis-scoring by the subjects and/or experimenters.

I need hardly say that I am, on principle, only too anxious to think the worst of my subjects, and if there had been any marked tendency towards positive (successful) results in the bulk data, I should naturally have pressed the successful subjects for a repetition of the experiment under strict conditions; indeed, I intimated my intention of doing so in the memorandum which I circulated at the beginning of the work. Actually, as we have seen, any score above expectation is just about offset by a corresponding score below expectation; so that, even if the observed dispersion were significantly greater than that attributable to chance, we should have to postulate substantially equal and opposite inexactitudes among those concerned.

Generally speaking, of course, the opportunities for what I have broadly termed "leakage" are much smaller in the case of pre-cognitive material than where we are concerned with paragnosis of other kinds. No amount of peeking and peering—whether deliberate or otherwise—will enable a subject to see a non-yet-happened event as it might enable him to see a contemporaneously existent card, for example. False results due to such causes can only be ascribed to deliberate distortion or almost criminal carelessness—indeed, even the latter must be systematic, for it must never be forgotten that *random* mis-scoring can never lead to statistically significant results.

I am happy to say that I can find no trace of either factor being operative; on the contrary the internal evidence in favour of perfect probity and great care throughout the subjects and experimenters is so strong as to be, humanly speaking, conclusive.

In the first place, in order to attribute the anomalous packing to causes of this kind, we should have to suppose that rather more than half of those concerned adopted, in varying degrees, the strange trick of neither inventing nor suppressing successes but merely putting them in the wrong place, carefully separated from their neighbours; while there was a tendency in the remainder to do the same kind of displacement in the opposite sense; in the second, these peculiar operations must be carried on (with a remarkable degree of unanimity) in such a way as to ensure the emergence of the significant results 60 and 61, 66 and 67 (plus one and plus two singletons for non-fresh and non-experimenter sets). Frankly, I find the suggestion quite fantastic and incredible.

Apart from this, however, it must be remembered that more would be involved than the mere scoring of a success where none had taken place. Inasmuch as every guess and throw was entered on the sheets, it would be necessary to falsify one entry or the other, in order to make a success appear. Assuming that chance alone were operative over the remaining entries, this would necessarily involve, I think, the introduction of a tendency towards positive correlation of the guesses and throws as regards the frequencies of the ones, twos, threes, etc., called and thrown. Certainly it would do so in the extreme limit of writing down as a throw (regardless of facts) the same number, in every case, that had already been recorded as guessed; presumably, therefore, though the argument is a trifle tricky, the same would apply *pro rata* on a small scale.

If chance alone were at work, we should expect positive correlations to be just as common as negative, but no commoner; if falsification were operative, we should expect to find them commoner. What we actually do find is that they are a good deal rarer. Admittedly, of the 57 groups mentioned, 23 show an excess of positive correlations and 34 of negative; while of the 440 sheets composing them 204 are positive to 230 negative and 6 zero. Neither of these yield significant results, but for the 40 subjects, I find no fewer than 28 showing a preponderance of negatively correlated sheets to 12 with a preponderance of positively correlated. This is significant, for we have

$$\chi^2 = 6.4; P = .01 \dots \dots \dots 69$$

Even if the argument just adduced is imperfect, we must additionally credit our performers with the perverted ingenuity necessary to bring about this very curious result, in addition to the complications already mentioned.

Alternatively we shall regard it as another example of the very queer tendency towards the opposite of what one would expect, which is constantly cropping up in this work.

It seems also just worth noting that the only discernible connection between packing and scoring is that, of the 38 sets considered, the 20 which score more than 200 (expectation) have mean values for the corresponding singleton χ^2 (normal successes):

For loose packing, 29.285; for close packing, 14.889.....70
 while the 18 which score less than 200 have mean values

For loose packing, 11.587; for close packing, 18.784.....71
 These figures look significant, though I do not know how to test

them. They imply that success in bulk scoring is associated with that property which tends to prevent one individual success being immediately followed by another.

To sum up: Unless some method of systematic mis-scoring can be suggested (not too preposterous intrinsically) which will account for all the significant results observed, it will be unreasonable to invoke such causes in explanation of the effects. Humanly speaking, I am confident that this is impossible, and I accordingly have little hesitation in saying that I regard the operation of *some* paranormal factor of precognitive character as established beyond any reasonable doubt.

13. In these circumstances we can better appreciate the remarkable performance of Mr X, to which I referred in section 3 above.

This gentleman's records showed no fewer than 407 successes in 1,200 trials. This gives a value for χ^2 of 257.094 (RN 72) and a chance of accidental occurrence of one in more millions than are worth working out: I decline to pander to a depraved taste for astronomical figures. In other words, we may dismiss the chance hypothesis without further ado.

In this set the scoring was done by Mrs X, and I understand that although the subject himself shook the die, he threw it in such a manner that he could not see the result. No third party was present as witness at any session, and it will readily be understood that the receipt of these forms placed me in something of a quandary. The great truth that one swallow does not make a summer, no matter how handsome a bird it may be, is fundamental to critical statistics. Moreover, if, in the light of our general knowledge of nature and of mankind, we ask the straight question: Is it more likely that a man can "foretell future events" to this order of success, or that a lady—of whatsoever probity and intelligence—might in such circumstances inadvertently render unto Caesar more than Caesar's due? even St Paul would forgive us, I think, for halting between two opinions. In particular it seemed to me possible that the monotonous occupation of watching throws and entering figures might, in a sensitive subject, induce some kind of dissociated state in which the natural desire for Mr X's success would over-ride the very artificial conventions defining accuracy in such a context. (*N.B.*—When a die is thrown, three faces are usually visible without difficulty; it is only a convention, with which the subconscious may well be unconcerned, which decrees that only one of them is to be used for scoring successes.)

The hypothesis is admittedly far-fetched; but so is that of paranormally bringing off 407 successes in 1,200 trials.

Such invidious positions are constantly arising in psychical research at its present stage of development, and much futile controversy and heartburn is occasioned by the failure to realize the simple question of greater or less probability which is always involved.

However, the whole question takes on a different aspect now that we have decided, on other grounds, that *something* of a paranormal and precognitive character is taking place in other cases; for there is nothing, in principle, more improbable about scoring 407 successes (or any other number) precognitively than about having any sort of cognitive relationship with a future event; the difference is one of degree rather than of kind, and prudence is not served by straining at the conspicuous gnat while quietly swallowing the less evident camel.

In the context, therefore, of the significant and paranormal, albeit recondite, results obtained from other subjects, I have negligible hesitation in accepting Mr X's achievement at its face value, and our next step should be to enquire what light Mr X's remarkable performance throws on the nature of the general process we have been examining.

14. Result No. 43, supported by various indications, tells us that the relationship between guess and throw is not a matter of chance alone; the application of the differential method by means of "echelon scoring", leading to RN 46, assures us that this is not due to peculiarities of the guesses or throws as such, but to their relation to each other; RN 44 and 47 strongly suggests that the influence of the non-chance factor extends, in reduced degree, to throws later than that to which the guess is intended to refer.

I conclude that something in the nature of precognition occurs, but that in order to make a successful precognitive guess, three factors are necessary:

1. What I may term a *bare prescience*, or awareness, of the (most probable form of the) future event:
2. The correct *location* of this prescience with respect to two or more events to which it might refer:
3. The correct utilization of this located prescience by reacting in the right *direction*; that is to say, by calling what the prescience indicates and not what it excludes.

The frequent occurrence of contrary-to-expectation results strongly suggests that the subjects are just about as likely to call paranormally wrong as paranormally right. Compare, here, Dr

Rhine's results on the effects of fatigue and disinclination which suggest, as Mr Tyrrell says, that "there exists a subconscious, controlling influence of some sort which has the power of enhancing, inhibiting or even of reversing the results."

We are thus led to the interesting view that precognitive ability consists not so much in a "foreknowledge of the future"—whatever that may mean—as in a successful use of the foreknowledge available. The difference between a good precognizer and a bad one would seem to be that the former can correctly apply his prescience or "contact with the event", while with the latter it is either muddled or actively perverted. I have no doubt that the faculty of bare prescience varies considerably between different individuals; but I certainly have the impression that its correct utilization is the crux of the matter, and this is likely to depend on delicate psychological factors the importance of which is emphasized by both Tyrrell and Rhine.

15. In this connection, the method adopted by Mr X in the course of his record-making set is of great interest. He writes:

"... the method I used ... consisted of variations of the following: I would note the objects in my view and then simply select groups of these, giving the number arrived at by taking, say, four chimneys on a roof, three legs visible of a chair at my side, six folds in the curtains, and so on. Then, as a change, I would switch to the numerals of my own birthdate—1151632—that is, 11th May 1902. Then in the same vein I would take the birthdays of my family. Another run of figures would come from taking the numbers of the various houses we have resided in. . . . Naturally a certain amount of selectivity entered into these methods, and I simply followed the whim of the moment."

This suggests that the trick is to occupy the foreconscious loosely, so to speak, with possible numbers and leave it to the subconscious to say "snap", as it were, when the right number occurs, rather than to strain after results by attempts at visualization, etc. There is also a strong impression abroad (not yet studied) to the effect that high speed of guessing is conducive to success; this would fit in well with what has just been said.

These last speculations apart, however, it is clear that the investigation not only establishes the fact of precognitive paragnosis to a high order of probability (in my strong opinion, at any rate) but also tells us a fair amount about the kind of thing that is going on.

JOURNAL

OF THE

Society for Psychical Research

The Rooms of the Society will be closed after Wednesday, 31 July, until Monday, 9 September. Correspondence will be forwarded to the staff during this time. The next number of the "Journal" will be issued in October.

MEMBERS are asked to return, renew or exchange Library books before 31 July. Each Member may borrow as many as six volumes for the vacation before the Rooms close.

APPOINTMENT OF RESEARCH STUDENT

THE Council have appointed Mr C. V. C. Herbert to be Research Student for one year from September next. Mr Herbert read Natural Science and Law at Cambridge, and is a Fellow of the Royal Astronomical Society. He has worked for several years as a research student in astronomy at the University of London Observatory, chiefly at photo-electric photometry.

Mr Herbert is a member of the Society, and has sat with several mediums of different types. He was in charge of the apparatus used at the Rudi Schneider Sittings in the autumn of 1932, and contributed to the report published in *Proceedings*, vol. xli.

NEW MEMBERS

(Elected on 1 May 1935)

Collins, B. Abdy, C.I.E., Deccan House, Aldeburgh, Suffolk.

Worcester, Dr Elwood, 186 Marlborough Street, Boston, Mass. U.S.A.

Student-Associate

Coombe-Tennant, A. H. S., 73 Portland Place, London, W. 1.

(Elected on 29 May 1935)

Kennedy, Miss H. E., 56 Westwood Avenue, South Harrow, Middx.

Mackay, A. G., M.B., 10 Spence Street, Newington, Edinburgh.

Matthews, The Very Rev. W. R., Dean of St. Paul's, The Deanery,
London, E.C. 4.

Murray, Lady, 19 St. James's Square, London, S.W. 1.

MEETINGS OF THE COUNCIL

THE 330th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 1 May 1935, at 4 p.m., THE PRESIDENT in the Chair. There were also present: The Earl of Balfour, Mr Gerald Heard, Lord Charles Hope, Miss Jephson, Sir Lawrence Jones, Bart., Professor William McDougall, Dr T. W. Mitchell, Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt and Miss Nea Walker; also Miss Isabel Newton, Secretary.

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

Two new Members and one Student-Associate were elected. Their names and addresses are given above.

Professor R. H. Thouless was elected an Honorary Associate of the Society for the year 1935-1936.

The Council expressed their thanks to Lord Charles Hope and his friends for their gift to the Society of the special apparatus which they acquired and lent to the Society during the joint investigation of Rudi Schneider in 1933.

THE 331st Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 29 May 1935, at 4 p.m., THE PRESIDENT in the Chair. There were also present: Professor E. R. Dodds, Mr Gerald Heard, Lord Charles Hope, Sir Lawrence Jones, Bart., The Hon. Mrs Alfred Lyttelton, G.B.E., Mrs W. H. Salter, Admiral the Hon. A. C. Strutt, R.N., and Miss Nea Walker; also Miss Isabel Newton, Secretary.

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

Four new Members were elected. Their names and addresses are given above.

Mr C. V. C. Herbert was appointed as Research Student for one year, the appointment to date from September.

Permission was given to the Editor to relax the rule of privacy in the case of the *Journal* for June 1935.

GENERAL MEETING

THE 184th General Meeting of the Society was held at the Conway Hall, London, W.C. 1, on Wednesday, 1 May 1935, at 5.30 p.m., when PROFESSOR C. D. BROAD gave his Presidential Address, which will be published later in *Proceedings*.

CORRESPONDENCE

MR TYRRELL'S ENTITIES

To the Editor of THE JOURNAL

MADAM,—Mr Tyrrell's letter on Dr Rhine's experiments in the May issue of the *Journal* raises a point of some interest and importance to all those who are trying to repeat them and indeed to all investigators of alleged psychic phenomena.

Mr Tyrrell maintains that "subconscious" entities are at work. Psychological atmosphere is also important if good results are to be obtained; and if this mysterious atmosphere is not reproduced then the experiments are not really repeated at all. We must institute a Department of Psychic Aerology without delay. Every fraudulent medium in the world will apply for membership.

Now all this has a familiar ring to anyone who knows anything of the history of spiritualism. The same argument has been used to defend every clumsy experiment or blatant imposture. Just as Mr Tyrrell's entity "has complete power over the course of the experiments" so is it claimed that the critical mind can cause as much havoc in the séance room as an earthquake. Bad spirits may be attracted to the evil thinker and either there will be no results at all or wrecking tactics may become apparent.

Although Mr Tyrrell states that the conditions to which the entity is responsive are delicate and elusive yet some other equally mysterious entity seems to inform him of what is occurring, just as another entity informed Dr Rhine (judging from his correspondence with me) that were I to visit Carolina in order to see his marvels my time would be a total loss. How do these gentlemen know these things? Mr Tyrrell knows that Miss Johnson's scoring will go down if the conversation is turned to a slightly unpleasant subject just before an experiment. How does he know that that was the reason? How does he know that it was not because Miss Johnson's tea was not the right temperature, or because his bacon was not of the right brand or because the moon was not in the right quarter? Seriously the situation is becoming a little farcical, and I strongly suspect that the

“entity” that prompted Mr Tyrrell’s letter was the knowledge that recent alleged reproductions of Dr. Rhine’s experiments are not illustrating the occurrence of E. S. P. For the new gospel is this. Conditions are “right” when E. S. P. can be demonstrated. If there is no E. S. P. then conditions are “wrong.” (Are these to be counted?) What we want is our E. S. P. and we mean to get it. Well, they will, and others at least will have the opportunity of enjoying a good laugh.

E. J. DINGWALL.

[Mr Dingwall is surely confusing a sympathetic atmosphere, that is, an atmosphere which seeks to avoid conditions likely to have an inhibitory effect upon the percipient, with an atmosphere lacking in scientific precision or favourable to fraud. It is quite possible to be psychologically sympathetic on the one hand, and both accurate and observant on the other. Any one who finds a difficulty in combining these two conditions had better leave psychical research severely alone.—HON. ED.]

To the Editor of THE JOURNAL

MADAM,—In his letter to the *Journal*, Mr Tyrrell draws attention to a necessity in the repetition of Dr Rhine’s experiments which I probably did not sufficiently emphasise in my review. Similar results cannot be expected unless essential psychological conditions are reproduced. A condition emphasised by Rhine is absence of effort on the part of the subject; a general attitude of optimism as to results is probably another. The possibility of psychological conditions not having been favourable should make us cautious in drawing definite conclusions from any single experimenter’s failure to get the same results. There does not, however, seem to be any sufficient reason for supposing the psychological conditions favourable to extra-sensory perception are so exceptionally difficult to obtain that competent experimenters used to paying attention to such factors in their work cannot be expected to be able to obtain them. The positive results reported by Rhine are also stated by him to have been obtained by a good many other people working at Duke University, therefore, one must suppose, with a good deal of variation of psychological conditions. Those who wish to repeat these experiments must take all the trouble they can to get favourable conditions. If, however, they were all to prove unable to get the same results as Rhine reports, the supposition that we in this country are unable to obtain favourable psychological conditions so easily obtained in America would be a somewhat improbable explanation.

As to what is the major issue in Dr Rhine's work, I simply disagree with Mr Tyrrell. If, as Rhine believes, the existence of E. S. P. has been sufficiently proved by earlier investigations, it is of no interest to prove it all over again. The next step is to find out something about it. It is true that no experiment or observation which cannot be repeated at will can give certainty, since the probability of it being correct depends on the credibility of the experimenter which, however great it may be, is not infinite. But this margin of uncertainty is not much reduced by any single new set of experiments. There are, I think, two major issues. First, has Dr Rhine discovered that the capacity for E. S. P. is much more frequent than was previously supposed? If so, this is a new and important finding. Secondly, has he discovered a method of experimenting in telepathy and clairvoyance which is easily repeatable by any competent experimenter? If so, we need certainly worry no more about the problem of the existence of E. S. P., since every sceptic can find out for himself, and we can get on with the next step of finding out more about it.

Unhappily, as I said in my review, the evidence on the first of these two points is inadequately stated in Rhine's book. Evidence on the second can only be obtained by widespread attempts at repetition. I hope these attempts will be made, with the precautions which Mr Tyrrell so justly points out.

Yours faithfully,

ROBERT H. THOULESS.

REVIEWS

"Mr Dunne's Theory of Time in *An Experiment with Time*,"
by Professor C. D. BROAD. *Philosophy*. Vol. X, No, 38,
April 1935. Macmillan & Co. Ltd. 3s. 6d.

In the March issue of the *Journal* I gave a short notice of Mr Dunne's recent book, *A Serial Universe*, wherein he expounds further his theory of Time. In that notice I refrained from entering into any criticism of the theory, which was fortunate, as it has now been done by our President, needless to say, far more competently than I could ever have hoped to do, in the April number of *Philosophy*. Professor Broad follows his usual method of stating the theory in his own terms and, as his statements are always characterised by that lucidity and exactness for which he is distinguished, the method is an excellent one. Were all our philosophers able and willing to express their meaning as clearly and unambiguously

as he does there would be far less confusion and misunderstanding and, I am tempted to say, considerably less "philosophy".

The theory is not an easy one to formulate, and Professor Broad's statement requires careful reading and some clear thinking, but, given these, I think that there should be no difficulty in understanding it. His primary purpose is to work out how Mr Dunne's theory affords an explanation of precognition, and he starts with an account of Hinton's suggestions, given in his book on *The Fourth Dimension*. Mr Dunne's theory, he says, is clearly based on Hinton's work. He then gives a clear account of that theory, illustrated with diagrams, and goes on to show how precognition might be explained. He also deals with the point raised by those "warning" cases of precognition where the precognitive experience causes action to be taken which prevents its complete fulfilment. His explanation is based on the conception of a future which though determined is yet plastic.

Professor Broad then discusses Mr Dunne's concrete interpretation of his theory and rejects the idea that the "Substratum" is to be identified with the Observer's brain. He states four assumptions which might be sufficient for a concrete interpretation of the "Reagent" but confesses that he is unable to discover any empirical object with which he can identify it.

Finally he deals with Mr Dunne's theory of "Serialism", which, so its author claims, is necessary apart from any evidence for precognition.

Professor Broad rejects, not only this view, but also the theory of Serialism itself, holding that, even if it were true that the facts about Time require us to postulate an infinite regress, the conclusion drawn by McTaggart, viz. that of the non-reality of Time, is to be preferred: he does not, however, admit that any such regress is necessary.

For those who found Mr Dunne's two books rather heavy going I would recommend a careful study of this article, while those who were able to assimilate Mr Dunne's theory from his own books will find much further enlightenment as well as reasoned and authoritative criticism in what Professor Broad has to say.

H. F. S.

J. ARTHUR HILL, *Towards Cheerfulness*. Rider. 5s.

Mr Hill's books on psychical research are well known. In *Towards Cheerfulness* he gives us his personal reflections on psychical research and much else besides, on Shakespear's politics, on war and peace, on cure by suggestion, on the Yellow Peril. . . . He touches, in fact, on

many subjects without perhaps going very deeply into any one of them, writes with charm, great commonsense and quiet optimism, and is always easy to read.

He is, however, at his best on psychical research and spiritualism, and though he does not in this little book, present any of the evidence on which he bases his belief in survival (for this, the earlier books must be read), his chapters on these subjects might well form a model introduction for those embarking on them for the first time.

Perhaps his philosophy can best be illustrated by a quotation from page 179. "Besides, from our own point of view, it is best to look at the bright side of all possibilities. If the bright side turns out to be the true one, we have done the right thing, and even if it does not—well, we have had the pleasure of the cheerfulness, and have been saved from exercising a depressing effect on those around us. Therefore is it not legitimate to accept as much as we can of all optimistic ideas?"

S. R. W. WILSON.

On Dreams. WILLIAM ARCHER, edited by Th. Besterman, with an Introduction by Prof. Gilbert Murray. Methuen. 7s. 6d. net.

William Archer, the dramatist and dramatic critic, was interested in dreams, as Mr Besterman tells us, "for over a quarter of a century, for ten years of which he kept a careful record of his own dreams". Prof. Murray, a friend of long standing, describes him as "one of the extremely few people who could be trusted to give an exact account of any experience he met with—even of his dreams", and as "a most critical and disinterested observer". Archer turned the light of his own experience on to current theories as to the causes and meaning of dreams, and began a book, which he unfortunately never lived to complete, illustrating by examples chosen from his collection points in these theories which seemed to him to call for comment and criticism. The task of bringing the book out in the form the author apparently intended fell to Mr Besterman's experienced hands.

Much of the book is devoted to a criticism of Freud's theories, which Archer thought "not indeed baseless—but stated with far too great generality". Some of the dreams quoted suggest that the Freudian element was larger than the dreamer perhaps realised.

Of the "Supernormal" there is little trace. One dream (pp. 193-195) may have been precognitive. Archer writes "The thing is probably a mere co-incidence, but it is certainly odd": and that fairly states the case.

The following passage is interesting:—"If I may trench just for

a moment on a dangerous domain, I would suggest that many of the communications which purport to come from disembodied spirits proceed in fact from hints supplied by the sitter's subconsciousness, transferred to the brain of the medium, and there dramatized by much the same mechanism, and much the same processes, as we find at work in dreams. I do not say, and do not think, that all alleged spirit-communications can be thus accounted for. . . . This implies of course that thought-transference lies within the sphere of normal psychology. I confess I cannot understand the state of mind of anyone who can ignore the mountains of evidence for its reality".

W. H. S.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

ERRATUM

Preliminary Experiments in Precognitive Guessing: p. 96, l. 6.

For $E = t(n-1)(n-t-1)/(n-1)(n-2)$

read $E = t(n-t)(n-t-1)/(n-1)(n-2)$.

W.W.C.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

The Fourth
Frederic W. H. Myers Memorial Lecture

WILL BE DELIVERED BY

MR WHATELY CARINGTON

AT

MANSON HOUSE

(26 PORTLAND PLACE, W. 1)

ON

WEDNESDAY, 30 October, 1935, at 5.30 p.m.

N.B.—*Admission will be by Ticket only. Full particulars are given on the following page.*

FORTHCOMING MEETING

November 27. "On the Mediumship of Carlos Mirabelli". By Mr Theodore Besterman.

ILLNESS OF MISS NEWTON

OUR members will all greatly regret to learn that Miss Newton has had a recurrence of the ill-health from which she suffered about two years ago, and will wish her a speedy recovery. For a time, however, she will be unable to attend at the office. It is hoped that during her absence members will do everything in their power to lessen the burden of work falling on the office.

THE RESEARCH STUDENT

MR C. V. C. HERBERT, who has recently begun his work as Research Student, will be glad to see members of the Society, or others who may wish to discuss matters relating to the Society's work, on any Tuesday or Thursday from 11.0-1.0 and 2.30-4.0, except, of course, when he is unavoidably absent on the Society's business. Should these hours be impossible in any individual case, Mr Herbert would endeavour to arrange to make a special appointment.

THE FOURTH MYERS MEMORIAL LECTURE

THE Fourth Frederic W. H. Myers Memorial Lecture, entitled *The Meaning of "Survival"*, will be delivered by Mr W. Whately Carington at Manson House, 26 Portland Place, W. 1., on Wednesday, 30 October 1935, at 5.30 p.m. Admission will be by ticket only. Tickets can be had on application to the Secretary, S.P.R., 31 Tavistock Square, London, W.C. 1.

Manson House is conveniently situated within a few doors of the B.B.C. and Queen's Hall, and about five minutes' walk from Oxford Circus or Regent's Park Tube Stations.

THE SOCIETY'S TENANCY OF NO. 31 TAVISTOCK SQUARE

THE Council are glad to report that they have obtained an extension of the Society's lease of No. 31 Tavistock Square until midsummer 1956. The old lease was due to expire in 1937, and although the new lease, which has been substituted for it, is at a higher rent, the

Council consider the Society fortunate in being able to remain in quarters so well adapted to its needs, and in avoiding the heavy expenses of a move.

NEW MEMBERS

- Anrep, Mrs.**, 86 Charlotte Street, London, W. 1.
Cort van der Linden, P. W. J. H., Zuiderpark 2, Groningen, Holland.
Davis, F. M., Etive, London Road South, Lowestoft, Suffolk.
Fodor, Dr Nandor, 47 Beverley Court, Chiswick, London, W. 4.
Guénault, P. H., 31 Lyddon Terrace, Leeds.
Hanson, Mrs., 5 De Walden Court, Meads Road, Eastbourne.
Heard, Rev. A. St J., The Rectory, Caterham, Surrey.
Magnus, Mrs Laurie, 34 Cambridge Square, London, W. 2.
Nisbet, B. C., 42 Iverna Court, London, W. 8.
Thompson, Mrs E. Roland, Oak Hayes, Crewkerne, Somerset.
Thomson, Lady, Bell Cottage, Kynance Mews, London, S.W. 7.

STUDENT ASSOCIATE

- Thomson, A. F. H., Bell Cottage, Kynance Mews, London, S.W. 7.

MEETINGS OF THE COUNCIL

THE 332nd Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 26 June 1935, at 4.15 p.m., THE PRESIDENT in the Chair. There were also present: Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Admiral the Hon. A. C. Strutt, and the Rev. C. Drayton Thomas; also Miss Isabel Newton, Secretary.

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

A letter was read from Mr W. R. Bousfield resigning his seat on the Council on account of failing health. The Council accepted the resignation with much regret, and unanimously passed a resolution of thanks to Mr Bousfield for the work he has done as a Member of Council and for the valuable advice and help he has given on many occasions, particularly in connection with the revision of the Articles of Association.

The Council filled the vacant place among their elected members, caused by Mr Bousfield's retirement, by appointing to it Professor C. D. Broad, hitherto a co-opted member.

THE 333rd Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 25 September 1935, at 4 p.m., SIR ERNEST BENNETT in the Chair. There were also present: Professor E. R. Dodds, Mr Gerald Heard, Miss Ina Jephson, the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter, Mr S. G. Soal and the Rev. C. Drayton Thomas; also Miss E. M. Horsell, Assistant-Secretary.

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

Eleven new Members and one Student Associate were elected. Their names and addresses are given above.

Reports were received from Mr Whately Carington and Mr Kenneth Richmond with regard to the recent International Congress at Oslo.

The Council received with the very greatest regret a letter from Professor F. C. S. Schiller, F.B.A., stating that owing to ill-health he feared he would not in future be able to attend meetings of the Council, and tendering his resignation as a member of Council for that reason. The Council wish to place on record their warm appreciation of the distinguished services that Professor Schiller has rendered the Society during the period of upwards of thirty years that he has been their colleague, and they are glad to think that as Vice-President of the Society he will remain in close contact with them and the Society.

OBITUARY

WE regret to record the death in July last of Major W. Rampling Rose, for more than twenty years a member of the Society.

Major Rose, who was the founder and head of a large photographic business, and a recognised expert in matters relating to photography, on many occasions placed his great knowledge of the subject at the Society's service. In particular he was a keen student of "psychic photography", and in collaboration with Mr F. Barlow undertook a patient and thorough investigation of "the Crewe Circle" and Mrs Deane, of which a record appears in *Proceedings*, Vol. XLI (pp. 121-138). He was also a generous contributor to the Society's funds.

WE also regret the loss sustained by the Society through the death of Mr. P. W. A. Cort van der Linden, Councillor of State, and formerly Professor of the University of Groningen, Holland. Mr. Cort van der Linden was one of our oldest Associates, having been elected in 1890, and until his last illness followed the Society's work with the keenest interest.

PRIVATE MEETINGS

THE 137th Private Meeting of the Society was held on Wednesday, 26 June 1935, at 5.30 p.m., THE PRESIDENT in the Chair.

MISS INA JEPHSON read a paper entitled "Useful Ghosts". An interesting discussion followed, in which the President, Mr G. W. Lambert, the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter and the Rev. C. Drayton Thomas took part.

The 138th Private Meeting of the Society was held in the Society's Library on Wednesday, the 25 September 1935, PROFESSOR E. R. DODDS being in the Chair, when MISS EVELYN M. PENROSE read a paper on "Divining for Water, Mineral and Oil". Miss Penrose, who was for some time employed as diviner by the Government of British Columbia, said that in Cornwall, her native county, divining for metals had been employed long before divining for water, the first diviners having been brought over from Germany in the sixteenth century. She gave a demonstration of the methods and implements used by herself and other diviners. She referred in particular to recent developments whereby she and others divine from a map without knowledge of the terrain concerned. Miss Penrose, who disclaimed any psychic gifts, admitted she had no idea how this was done, but said she found that in practice she could do it. She gave a very interesting account of her own sensations when dowsing.

In the discussion after the paper the following members took part, Lady Barrett, Mrs Goldney, Mr K. Richmond, Col. A. H. Bell (President of the British Society of Dowzers), Prof. Stratton, Mr Harold Myers, Mr Hettinger, Mr Herbert and the Rev. C. Drayton Thomas.

NOTE ON PRECOGNITIVE GUESSES

BY W. WHATELY CARINGTON

IT has been pointed out to me by Professor Fisher that unless we know the distribution of singletons about their expected value (or at least the Variance of this value) it is impossible to determine whether any deviation from this is significant. It follows that the conclusions reached in sections 8 to 12 of my "Preliminary Experiments in Precognitive Guessing" (*Journal*, June 1935, pp. 95-100) are not necessarily valid; though it by no means follows, of course, that they are necessarily *invalid*. They must in fact be put in cold storage pending investigation of the point mentioned. This is now in hand.

It is worth noting, however, that study of grouping generally, and its association with success, and of the effect of preference with

respect to particular numbers on the successes obtained with them reveals further anomalies difficult to ascribe to normal causes. The results obtained will, I hope, be published shortly.

CORRESPONDENCE

LORD BALFOUR'S PAPER ON MRS WILLETT'S SCRIPTS.

To the Editor of THE JOURNAL

DEAR Madam,—Having read Lord Balfour's article on Mrs Willett's Scripts with great interest, and noticing the difficulty he finds in conceiving how the different layers of consciousness can show themselves as separate entities in cases of multiple personality and yet not be separate selves, it has occurred to me that the analogy of what exists in the physical realm may be useful.

In the lowest unicellular animal we have an elementary individual sufficiently conscious of its environment to recognise its food and move to it, and perform other functions.

A little higher we have an animal consisting of a score or two of such cells. Each has the same essential structure as before, though one is now more specially devoted to one function and another to another. But in addition we find a higher self emerging, which co-ordinates the information received from the individual cells and can direct and co-ordinate their movements. By their specialisation it has a better knowledge of its environment, *i.e.* a higher class of consciousness. For what is consciousness but a perception of the environment?

Also there is no reason to think the individual cells have lost their selfhood, any more than individuals in an army because they obey their officers, though this may cause some of their activities to be in abeyance for the time.

In higher animals the same process has been extended. Groups of cells form various organs, each of which has still its own individuality—the heart cut out of an animal just killed will go on beating for a time.

But, again, here we have a higher self co-ordinating all these organs.

In health we are inclined to deny that our organs, let alone the individual cells, are separate entities in the sense that an amœba is one. But it is otherwise in illness; for instance, in the case of cancer an individual cell starts multiplying regardless of its neighbours, and behaving just as the unicellular animal would do. Each organ, too, has its own selfhood, and if suitably fed can go on living and functioning for a long time when removed from the body.

In short, our bodies are composed of successive grades of "selves," which in health show no sign of their selfhood, but which in illness become apparent.

May there not be something of a similar character in our mental nature? The supreme "I" co-ordinating lower grades of consciousness and thought, from which it draws knowledge of its environment and whose thoughts it utilises, without suspecting that they are subordinate selves, which in disease or other disturbance of the usual order may manifest their separate individualities.

The "supraliminal consciousness" on this view would therefore consist of the supreme ego co-ordinating such of the subsidiary selves as are of use to it in its ordinary life (in the language of the scripts "which pay"), while the subliminal are those which it can leave to function by themselves (the ordinary bodily ones), and those which are not immediately useful to it in this life.

While therefore this difficulty does not seem very serious to me, the scripts raise another in my mind, a difficulty which has continually met me in many automatic messages, but which is specially evident in these.

The scripts give a clear account of the elaborate series of interactions between communicators and the subliminal of the autonomist necessary to enable some simple idea, *e.g.* fire, to emerge. And yet they can pour forth pages of abstruse explanations with hardly a check.

It is this more than anything else which casts doubt on the personality of the communicators. If they are merely subliminals they can pour out their unverifiable ideas easily, but when they come to veridical matters, of which they gain fragmentary knowledge by telepathy, then they only can stumble along. If this were so it could be understood, but if they are what they claim to be it is very hard to understand, and I should be most interested to know if there is any reasonable explanation of it.

Yours, etc.,

J. H. HUME-ROTHERY.

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES.

To the Editor of THE JOURNAL

MADAM,—May I congratulate Mr Whately Carington on the exemplary scientific integrity he displays in his paper in Part 141 (Vol. XLIII) of *Proceedings*? I wish the spirit shown in section 10 of that paper were more often met with in psychical research.

Mr Carington having so readily acknowledged the error of his

former ways, and having pretty plainly hinted that his present conclusions are not yet to be taken as the true gospel, I hope it will not seem ungenerous if I question whether even now he goes far enough in this direction. Mr Carington, of course, knows and says that facts are holy things, but I think he is still too ready to regard as "facts" the data handed to him by myself and others. I most cordially agree with sections (2) and (4) of Mr Maby's "Note". I have repeatedly urged similar considerations. From my own experience in recording these data and in watching others record them, I have no doubt whatever that the personality of the operator plays a very much greater part than Mr Carington seems ready to admit. This influence is naturally most potent in a case like that of Rudi Schneider. I should be prepared to lay pretty substantial odds that appreciably different results would have been obtained with Rudi and Olga if these personalities, instead of being confronted by myself, then the Society's Investigation Officer, before whom he stood in some awe, had been tested by someone with whom he felt more at ease. Even my work with Mr Gatty, in which boredom was the predominant element with both subject and operator, notwithstanding our profound interest in what we were doing, I was conscious of a subtle difference in my attitudes to his two "personalities".

These considerations may seem somewhat fine-drawn, but I think that they are very important, and I do not see that they can be segregated even by the elegant analysis of variance. There are other considerations, such as the varying technique used by different operators, a fact of importance for comparative purposes. Again, Mr Carington has made allowance for fatigue on the part of the subject: but what about the operator's very real fatigue as he approaches the end of a double 100-word session?

I hope, therefore, that Mr Carington will forgive me if I say that he appears to have missed the point when he writes (p. 367): "Is it suggested . . . that if I had been present I would have recorded times, words or reproductions other than those actually given . . . ?" The answer is obviously in the negative, but this is not surprising since this Aunt Sally has been erected by Mr Carington's unaided efforts. The question should read: "Is it suggested that if I had been the operator, times, words and reproductions would have been given to me to some extent different from those actually given to operator X, Y or Z?" To this question I have no hesitation in returning an affirmative answer; and if I am right in doing so, to what extent are Mr Carington's results affected? It is a matter for experimental inquiry.

Yours, etc.,

THEODORE BESTERMAN.

To the Editor of THE JOURNAL

MADAM,—While more than impressed by the industry, skill and intellectual power displayed by Mr Whately Carington in his work published in the last *Proceedings*, I venture to voice my suspicions that there are contingent aspects of the subject which evade the grasp of his all-but-intelligent stupendous mathematical machine, and hence, also, my regrets that he gave Mr Maby's criticism such short shrift.

For example, regarding the question of the possible effect of telepathy in vitiating the results. In reply to Mr Maby's remark in this connection, Mr Carington asks, "Who is supposed to have telepathed what to whom?"

It seems clearly possible that Mr Drayton Thomas, with his perfect (and well-founded) faith in the genuineness of "J" and "E", and knowledge of their lives, dispositions and characters, may have, so to put it, an intensifying effect upon their manifestations through Mrs Leonard, and possibly more (say) in the case of "E" than of "J". Surely a series of word reactions should be obtained from "J" and "E" when Mr Thomas is not present before the suggestion of telepathy can be so confidently rejected. The same applies to Mr Irving and "D".

At the risk of being riddled by the volleys of intelligent critics (p. 340), I venture, fortified by birth and long residence in, and knowledge and love of the East, to support Mr Thomas' contention that Feda's replies have "a strong Oriental flavour". Putting myself (*more* Mr Gatty) in an Oriental "atmosphere", I considered Feda's reactions to the 74 words given (p. 374). I rejected 9 only, comprising 15 different reactions. I accepted 96 reactions (80 different) to the remaining 65 words, these (96) comprising different reactions to the same word and the same reaction to different words (*e.g.* *wild*—elephants; *dervish*, *jungle*; *shoot*—elephants; *cook*—curry; *make*—curry). *Dress*—sari; *yellow*—robe [Buddhist priest]; *insult*—idol; *sleep*—mosquito; *bald*—priest [Buddhist again]; *cook*—curry; boy [servant]; *hunger*—dog;—struck me as reactions which mere book-knowledge of India would not stimulate, and there are many others. It would be interesting to know the views of other Anglo-Indians, and instructive if they vary in opinion! At any rate this seems to be an issue which the mathematical machine cannot handle, and before we dogmatise on the subject, we should ascertain whether Mrs Leonard has a repressed Oriental complex.

Yours, etc.

ERNEST S. THOMAS.

EXTRA-SENSORY PERCEPTION.

To the Editor of THE JOURNAL

DEAR MADAM,—I was much interested in a remark by Mr G. N. M. Tyrrell, in the April, 1935, *Journal*, to the effect that . . . “Miss Johnson had always had great difficulty in expressing any paranormal feeling in words,” because I myself have had this same experience, in a minor degree, on many occasions. For example, I have often had what seemed to be momentary premonitory flashes of something about to be done or said, and could have used the words about to be uttered myself, but *could* not. Some faint but very definite power seemed always to prevent my speaking the words until too late; then I realised that if I *had* uttered them, they would have been correct! Let me state emphatically that these experiences did *not* represent so-called “pseudo-presentiments”, but genuine experiences, which were carefully noted at the time. Is it possible that there is some subtle influence at work here, connected with experiences relating to the future? I merely propose the question without attempting to answer it.

Yours, etc.,

HEREWARD CARRINGTON.

To the Editor of THE JOURNAL

MADAM,—With regard to the two major issues raised by Dr Thouless in his letter to the *Journal* for July last, may it not be that the frequency of the capacity for E.S.P. does not resolve itself into two rigidly distinct classes—those who have it and those who have not—but rather that its manifestation is a function of the method employed to detect it? If the investigator is prepared to try long enough and carefully enough, he is surely more likely to get a result, given a moderately promising subject, than if he is content with a casual test. But it is still true that there is a rough division between promising and unpromising subjects, and the proportions between the two classes we do not know.

With regard to the ease of repetition of Dr Rhine's experiments, I cannot help feeling that Dr Thouless is drawing the rather natural, but yet erroneous conclusion, that to obtain results in E.S.P. is a good deal easier than it really is. At any rate, as far as my own experience may be allowed to count, the outstanding feature is the elusive and fluky nature of the faculty. The idea underlying the

method of collective experiment, that the matter can be settled by issuing something in the nature of an examination paper to all and sundry, seems to me to be based on too simple a view of the matter. One needs to maintain personal contact with the subject and to stand by, ready to seize one's opportunity, very much as if taking a photograph of a solar eclipse: and I fear that "every sceptic" who essays to verify the matter for himself will find that he has embarked on a considerable task.

Dr Dingwall's attitude is an interesting variation on the established method of experimental science. Instead of trying to find out from nature the conditions under which he is likely to get positive results, he proposes to dictate to nature the conditions under which he will accept them. "Atmosphere", and the many factors affecting the internal balance of forces within the subject, are not to be allowed to have anything to do with the results, whatever nature's laws on the subject may be. Only external conditions are to be allowed to count. Dr Dingwall is going to decide the circumstances under which E.S.P. may occur—an extension, I suppose, of the vogue for dictatorship into experimental science. But he is not original. King Canute tried the method a long time ago, with the only result that he got his feet wet. And now we have the modern "Canute Complex" complacently laying down the law to nature: and we know what is going to happen. After stultifying its own experiments, it will cry loudly that the object of them does not exist!

Yours, etc.,

G. N. M. TYRRELL.

REVIEWS

A Bibliography of Sir Oliver Lodge, F.R.S. Compiled by THEODORE BESTERMAN. Foreword by SIR OLIVER LODGE. London: Oxford University Press, 1935. Pp. xiv-219. Price 21s. net.

We welcome the appearance of this book which illustrates in a convenient form Sir Oliver Lodge's activities during his long and distinguished life in many spheres, including of course that of psychical research.

The number of Sir Oliver Lodge's publications described in this work, including books, pamphlets, and contributions to scientific and other periodicals, and also translations into foreign languages, amounts to 1,154. Of particular interest is the "Rough Classification of Sir Oliver Lodge's Writings," given by Mr Besterman in his

preface. The book celebrates the sixtieth anniversary of the publication of Sir Oliver Lodge's first work, and we may hope a supplementary volume of further publications will in due course become necessary.

WE welcome the appearance of *Bulletin I* of the University of London Council for Psychical Investigation. The Bulletin is a Supplement to the Short-title Catalogue of the Research Library; it is compiled by Mr Harry Price, and has nine illustrations.

W. H. S.

THE *JOURNAL* IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

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ON

WEDNESDAY, 27 November, 1935, at 5.30 p.m.

WHEN A PAPER ON

“The Mediumship of Carlos Mirabelli”

WILL BE READ BY

MR THEODORE BESTERMAN

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

NEW MEMBERS

Butter, Colonel C. A. J., Cluniemore, Pitlochry, Perthshire.

Chesters, Denis, 126 Widmore Road, Bromley, Kent.

Falk, Mrs., 7 Sion Hill, Clifton, Bristol 8.

Gregory, C. C. L., 86 Friern Park, London, N.12.

Librarian, Adyar Library, Adyar, Madras, S. India.

MEETING OF THE COUNCIL

THE 334th Meeting of the Council was held at 31 Tavistock Square, London, W.C.1, on Wednesday, 30 October 1935, at 3.45 p.m., THE PRESIDENT in the Chair. There were also present: Mr Whately Carington, Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, the Rev. C. Drayton Thomas, and Miss Nea Walker.

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

Five new Members were elected. Their names and addresses are given above.

The Council filled the vacant place among their elected members, caused by Professor F. C. S. Schiller's retirement, by appointing to it Mr W. Whately Carington, hitherto a co-opted member.

CORRESPONDENCE

PRELIMINARY EXPERIMENTS IN PRECOGNITIVE GUESSING

To the Editor of THE JOURNAL

MADAM,—May I venture to make a few comments on Mr Carington's most excellent paper on the precognition experiment? One cannot fail to admire the delicacy of this instrument of analysis when wielded by hands so skilful as his, and one stands amazed at his magnificent industry and patience.

It is of the first importance that he should have found unmistakable indications of an element of precognition in the "anomalous packing", but it seems to me that several very obscure questions arise.

If there be an element of precognition it is quite clear that its effect is somehow or other masked or concealed in the totals, for these come out exactly at chance expectation.

Mr Carington suggests that this masking of the effect in the totals

is due to the operation of some sort of negative faculty. The existence of such a faculty, *i.e.* one which produces results below chance, has already been suggested by Rhine and Tyrrell. I find it, however, very difficult to conceive.

The fundamental datum line, the "tabula rasa" on which everything is written, is a score of $N.p$ successes and $N(1-p)$ failures. This represents zero faculty—even a robot would get this result. In Mr Carington's experiment this is, of course, 20 successes in 120 trials; in both Dr Rhine's and Mr Tyrrell's 20 successes in 100 trials. For the purposes of this argument I will take the latter figures. If there be such a thing as paranormal faculty, whether of E.S.P. or precognition, it is obviously one which does not give 100% success. Each subject seems to have a limit beyond which he cannot go. One obvious explanation of this is that the faculty comes in waves or gushes and is then somehow inhibited.

Whether this be the true explanation or not does not affect my argument; the point is that, so far as has been experienced, no subject has been able to obtain 100% success.

We may therefore speak of a faculty as having say 20% efficiency. Now a 20% faculty acting on 100 trials in an experiment where $p = \frac{1}{5}$ would give, on the average, 36 successes, for, whether it is a matter of waves or gushes followed by periods of inhibition or whether the failure to secure 100% successes is due to some other cause, we may assume that, over a long period, the faculty is spread out evenly.

As I have said, the datum line is 20 successes in every 100 trials, so that in one-fifth of the trials the faculty will come across cases which would be successes anyhow. Now the only possible function of a positive faculty is to turn what would have been a failure into a success; it cannot affect what would have been a success whether it operated or not.

It has therefore left for its effective operation the 80 trials where chance would give failure: 20% of 80 = 16; thus the 16 successes due to the faulty plus the 20 due to chance make up 36. Now take the case of a negative faculty, that is to say a faculty which produces results significantly below chance.

The only possible function for such a faculty is to turn success into failure; if it operate on that which would anyhow be a failure its effect is nil. It therefore has only 20 trials whereon it can act effectively; the remainder will be failures whether it operate or not. 20% of 20 = 4, so a negative faculty of 20% efficiency would give a score of 16 successes.

It is clear that such a result would be of no avail to mask the

effect in the total of a positive faculty ; in order completely to do so the negative faculty must produce 16 failures—that is to say, it must have 80% efficiency.

It is further clear that no negative faculty whatsoever could completely mask the effect in the totals of any positive faculty, the efficiency of which exceeds 25%

Now I find it very difficult to believe that the negative faculty should have an efficiency four times as great as that of the positive ; if it comes in waves, it is a very extraordinary wave which has a trough four times as deep as the crest is high.

The only other alternative which I can think of seems to me even less acceptable. We should have to assume that the subject is able somehow to concentrate his faculty on those instances which by chance would be successes ; this involves, among other things, a knowledge of what is going to turn up, a precognitive knowledge, moreover, which must have 100% efficiency. Though I am inclined to accept the actuality of precognition, I cannot swallow this.

It is indisputable that scores significantly below chance do occur, but it seems to me that the hypothesis of a negative faculty is untenable.

At the present moment I am unable to suggest any alternative.

Yours etc.,

H. F. SALTMARSH.

To the Editor of THE JOURNAL

DEAR MADAM,—Those of us who know the work of Mr Whately Carington know him to be a careful and sceptical investigator, so that he will sympathise with those who find his preliminary results on precognition (reported in this Journal, June 1935) very improbable and who prefer to explore other possible explanations than his subjects' precognitive powers. It is certainly true that any significant failure of results to conform to chance expectation would be sound evidence for genuine precognition if all other possibilities were excluded. Nevertheless a precognitive power whose only effect is to reduce slightly the subjects' likelihood of guessing twice in succession is certainly working in a mysterious way which sharpens our suspicion that something is wrong.

I propose to consider only the main body of the results and to exclude those of Mr X who showed a remarkable excess of right guesses over mean chance expectation. These experiments were not properly witnessed and, therefore, cannot be allowed as evidence. The author only admits them because he thinks the existence of paranormal precognitive power is established by the other cases.

If it is not, it obviously cannot be established by Mr X's case until this subject shows the same results in properly witnessed experiments.

It does not seem to me, however, that the other evidence put forward by Mr Whately Carington is sufficient to establish any paranormal precognitive power on the part of his subjects. It must be admitted that if either the series of dice-numbers or the series of guesses is truly random, the effect could not be explained except by precognition. If, on the other hand, both depart from randomness, special kinds of such departure could produce the result obtained.

I do not think it will be doubted that the guessed series is unlikely to be random,—indeed that it would be impossible for anyone so to overcome associative processes in his mind that he could call out a series of numbers which was even approximately a random series. It seems to me to be equally unlikely that he could produce one by tossing dice. The acts of picking up the die, shaking it, and turning it out are a series of muscular movements which will form a habit-system likely to be repeated every time with little modification. A perfunctory tossing will produce a series of numbers far from random: but the most careful shaking (such as I suppose was used in these experiments) will produce a non-random series whose non-randomness may be perceptible only to careful statistical examination. Such non-randomness may be shown by a greater or lesser tendency for a number to be followed by itself than would be calculated from mean chance expectation. One cannot say beforehand which would happen, perhaps either with different subjects, depending on the particular habit system adopted in shaking the dice. Nor can we be sure how associative processes will make the guessed series depart from randomness.

Let us suppose that a particular subject who has guessed correctly will tend not to repeat that guess next time, or that, whether successful or not, he tends not to repeat the same guess twice in succession. Let us suppose also that he will be slightly more likely than m.c.e. to toss a second time the number previously obtained. Then a correct guess will make somewhat less likely a succeeding correct guess, *i.e.* the correct guesses will be "loose-packed." This is what Whately Carington observed and what he has regarded as evidence for precognition. The above is, however, an alternative explanation. Whether or not it is the true one could only be discovered by an examination of Whately Carington's raw data. The same result would, of course, follow from the subject tending to repeat a successful guess while the chances of the toss are somewhat against the same number turning up twice consecutively.

I have myself tried making a succession of dice tosses to discover whether I obtained a random series. The indication was that it was not random, but I did not make a very extensive series, because it was obvious that the important point to determine was not whether I produced a random series of tosses but whether Mr Whately Carington's subjects did.

In order to test out the above suggestion, it would be necessary to know: (a) whether the series of tosses shows pairs of similar numbers in succession significantly either more or less often than mean chance expectation: (b) whether a successful guess was more or less often followed by the same guess than mean chance expectation in a random series, and (c) whether the above two departures from randomness, if they occurred, would produce the amount of loose-packing actually found in the results.

On the supposition that it is previous success which influences the subject's subsequent guess, the reduction of loose-packing when the guesses are compared with the tosses removed one and two places from them is exactly what would be predicted. This, however, could only happen if the subject knew when he had made a successful guess. This I presume to have been the case. On the other supposition, that there is merely a tendency for the subject to avoid or to prefer two consecutive guesses of the same number, the looseness of packing would, so far as I can see, remain the same however the guessed and tossed series may be dislocated with respect to one another.

I wish to make it clear that I am not suggesting that the subjects may have tossed the dice in a perfunctory manner. I am suggesting that, however careful was the shaking, complete randomness would only be obtained if the die were shaken for an infinite time or by a being with an infinitely pliable nervous system who formed no habits. With actual human subjects, shaking carefully and conscientiously, nothing but the records themselves can tell how big is the departure from randomness and whether it is enough to account for the loose-packing of correct guesses. Until this point has been determined, Mr Carington's results cannot be regarded as evidence for precognition since a significant departure from mean chance expectation in closeness of packing might be due to another cause.

Yours faithfully,

ROBERT H. THOULESS.

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES.

To the Editor of THE JOURNAL

MADAM,—Mr E. S. Thomas's letter in the October *Journal* (p. 121) reminded me that I too had been inspired by the "strong oriental flavour" of Feda's word-reactions to try a little private experiment. His letter also persuaded me that it might not after all be entirely superfluous to report the result of that experiment.

About a week after I had read Mr Drayton Thomas's article in *Proceedings*, Part 141, I asked my wife to read me the list of 74 words printed on p. 374 (which I had not looked at in the interval) and to record my replies and reaction times. Half an hour later we had a "reproduction test"; and about two months later, when I had seen Mr E. S. Thomas's letter, we went through the whole list once more. Mean reaction time, first experiment 3.36 seconds, second experiment 3.73 seconds (Feda's mean reaction time is stated to be about 4.20 seconds). Here are some of my replies (stimulus words printed first, words repeated in reproduction test printed in italics):

Mountain : <i>Washington</i> . Apa-	Noise : New York.
lachian.	Hill : <i>Bunker's</i> . Bunkers.
Make : <i>Whoopee</i> . Whoopee.	Eat : <i>Corn-pone</i> . Waffles.
Sick : Wood spirit.	Door : Dooryard.
Head : <i>Prsident</i> . Presidency.	Red : <i>Indian</i> . Sinclair.
Cook : <i>Sweet Corn</i> . Corn-pone.	Sing : Sing-Sing.
Wild : <i>West</i> . West.	Village : Greenwich.
Wicked : Democrats. Roosevelt.	Angry : <i>Democrats</i> . Roosevelt.
Tree : Gum-tree. Maple.	Dead : <i>Lincoln</i> . John Brown.
Street : <i>Wall</i> . Wall.	Pay : <i>Wall Street</i> . Wall Street.
Paint : <i>Town red</i> . Town red.	Walk : Cake.
Carry : <i>Carolina</i> .	Bread : <i>Maize</i> . Maize.
Bury : <i>John Brown</i> . John	Pity : <i>New York City</i> . New
Brown.	York City.
Beat : <i>Palm Beach</i> . G-man.	Justice : <i>Negroes</i> . Roosevelt.
Brown : <i>Redskin</i> . John.	Book : Abraham Lincoln. Uncle
Insult : <i>Coughlan</i> . Republicans.	Tom's Cabin.
Horse : <i>Wild West</i> . Wild West.	Rich : Long Island. Wall Street.
Bet : <i>Bottom Dollar</i> . Bottom	Travel : American Express.
Dollar.	Old : <i>New Orleans</i> . Folks at
Black : <i>Negroes</i> . Peril.	home.
Mad : Father Coughlan.	Home : <i>Kentucky</i> . Kentucky.
Town : <i>St. Louis</i> . Louisville.	Purpose : <i>Recovery</i> . Presidency.

Fear : Prohibition. Gunman.	Pen : Corral.
Drive : Go-Getter.	Shoot : <i>Gunman</i> . G-man.
Young : <i>Owen D.</i> Owen D.	Girl : from Louisiana.
Read : <i>Chicago Tribune</i> . Sena- tor.	Dance : Tango. Build : <i>New York</i> . Capitol.

These replies have, I submit, a strong American flavour. Yet I am not an American; have never visited America; have no intimate friends in that country; and am quite discreditably ignorant of its history, geography and customs. I approached the experiment, however, with the firm intention of establishing my pseudo-identity as an American citizen and a member of the Republican party. Considering that I lacked Feda's many years of practice in being a foreigner, I perhaps did not do so badly: at any rate I beat her on the speed-test. Comparing the two sets of replies, the reader will notice (1) That, like Feda, I keep certain responses handy and use them whenever possible, *e.g.* Angry-Roosevelt, Wicked-Roosevelt, Justice-Roosevelt: cf. Feda's Make-Curry, Cook-Curry, Hot-Curry, Eat-Curry (as though through those long years in the Summer Land the poor child had been obsessed and perhaps tormented by the image of her favourite dish);

(2) That, like Feda, I make in my haste a few mistakes: *e.g.* "The Old Folks at Home" is not an American song (false association *via* "My Old Kentucky Home" ?), any more than "Durbar" is an Indian coin (false association *via* "dollar" ?);

(3) That I lug in my trite American references by the ears, just as Feda lugs in her trite Indian ones, "John" his biblical allusions, "Etta" her painting. (Contrast Mrs Leonard, whose responses appeared, to the naïve surprise of Mr Drayton Thomas, "commonplace and non-identifying". Even so would mine have appeared if, like her, I had been playing the game according to the rules instead of trying to establish a false identity and grossly over-doing it.)

I conclude (a) that responses whose reaction times average more than about three seconds,¹ however "reliable" in Mr Carington's statistical or Piekwickian sense, are, as Mr Maby has already hinted, open to the suspicion of purposive and systematic faking; (b) that the results obtained with Feda are not distinguishable in quality from faked results.

Yours, etc., E. R. DODDS.

¹ This applies to all the Leonard personalities in the first Thomas experiment, and to "John" and "Etta" in the second; see *Proc.*, XLII, pp. 236, 238.

REVIEWS

Walter Franklin Prince. A Tribute to his Memory. By FRIENDS AND COLLEAGUES IN PSYCHICAL RESEARCH. Boston Society for Psychic Research, 1935.

Every serious psychical researcher will welcome the issue of this volume as a tribute to the memory of one of the outstanding psychical researchers of our time, a man widely honoured both for his penetrating intellectual gifts and for the nobility of his character. It is appropriate that the Boston Society, whose destiny he in large measure controlled from its foundation and which owes pre-eminently to him the high position it holds among psychical research Societies, should have undertaken the issue of this memorial volume.

Among his many gifts Prince, more than any psychical researcher of our time, succeeded in establishing happy personal relations with students of the same subject in his own country, in England, and on the Continent of Europe. For many years he kept up an intimate correspondence with them, and in this small book tributes to his memory are paid by many who had highly appreciated this interchange of letters, or who had in other ways met him as a colleague in their researches.

In addition to these personal appreciations Professor Gardner Murphy contributes a review of the Doris Fischer case, probably the work for which Dr Prince will be longest remembered, and Dr Worcester a note on recent developments in the same case. The volume concludes with a useful summary of the leading events in Prince's life, and a list of his published works compiled by Mrs Allison.

The highest praise one can give the volume is to describe it as not unworthy of the man to whose memory it is dedicated.

The Mystical Qabalah. DION FORTUNE. London, Williams and Norgate. 1935. Pp. viii, 306 and 3 plates. Price 10s. 6d. net.

It is quite impossible in a short review to do justice to this arresting treatise on a subject of a class which does not usually allure the British mind. Attention is soon beguiled by the attractive presentation which dispels the misgivings that may be at first aroused. The following is a conspectus of some of its main principles.

The Qabalah is a Western development of the cosmic philosophy in which the great ancient faiths of the world were or are rooted.

Man, it is proved by occult subconscious experience, is a pattern of the Universe *in petto*: a microcosm. The study of his nature, powers, energies, and qualities therefore throws light on the Macro-

cosm. The Causative Force is more akin to Mind than to Matter : a conclusion to which Science is to-day gradually coming. The initiate thinks of the Natural Forces as Persons, and this personalisation has resulted in the building up of Thought Forms on the Astral Plane to represent them. Angelic beings for the Qabalist are Cosmic Forces. Thus the deities of the ancient faiths became Realities for their worshippers (and still 'are')—being qualities, emotions, and forces similarly 'personified'.

Through the 'cosmogenetic' glyph called the Tree of Life the aspirant can, by meditation, get (subconsciously) into touch with the fundamental streams of cosmic energies ; the effect being an access of energy to the soul and increased well-being. The adept occultist can canalise and use the powers (both good and bad) for the benefit or hurt of humanity.

The Tree constitutes a Method of Thinking—by feeding the Subconscious with symbols " which have a peculiar logical basis deep in the Subconscious mind ". These symbols are circles representing the 10 Holy Sephiroth (spheres) of cosmic forces and energies personified—arranged on, and down, 3 pillars in a zigzag order of their successive emanation (each from the one preceding) from the *Primum Mobile* to Malkuth (Earth). They are connected by Paths symbolising their interactions. Vertically on the 3 pillars the descent from 'primal essence' into matter is typified : on them the Sephiroth have qualities and energies roughly typified by Force, Form, and Consciousness (the Central pillar) in descending grades : each with many collateral 'functions'. The Tree, being not a graph or map but a method of thought, is dynamic in its effects upon the subconscious, which 'throws' the symbolic concepts like shuttles, and the resultant web of ideas is presented to the conscious mind, which assimilates what it can, progressively, from this contact of the Subconscious with that revealed Reality (the World Soul). 'Magic' has a rational basis in the realm of initiated subconscious operation.

Such is a bare outline of the central scheme. The whole system, *pace* its Hebrew setting, is rational in its constitution, and sensible and sane in its application to life.

The book is eminently readable ; the arrangement and handling of the material masterly, the style simple, straightforward and restrained. In spite of some deep sayings it is never ponderous or obscurantist. Analogies from psychology and physical science are employed with telling effect, and, surprising in a work of this kind, the pages are further enlivened with flashes of vigorous humour. The 'crede experto' of a writer of this calibre can hardly be lightly dismissed.

ERNEST S. THOMAS.

Okkultismus. Täuschungun und Tatsachen. By VON F. MOSER.
Orell Füssli Verlag, Zurich.

Frau Moser's two enormous volumes undoubtedly constitute a very important and valuable contribution to the literature, already so abundant, bearing on our researches. There are 479 pages in the first volume and 483 in the second (index not included); both are profusely illustrated, chiefly though not exclusively with portraits of the leaders of the movement, and the erudition displayed by the author is positively bewildering. Frau Moser (a member of the S.P.R.) appears to have read almost everything dealing with Psychological Research and "Metapsychics," at least in the languages accessible to her: and surely that is a good deal.

The very title of her book partly reveals its tendency: if there are in "occultism" delusions, there are also facts; and Frau Moser indefatigably—and on the whole very objectively, as it seems to me—proceeds to separate the tares from the wheat.

To analyse adequately such a book in two pages of the *Journal* is manifestly impossible. Frau Moser will therefore, I hope, pardon me if I limit myself to a few remarks.

In Volume I. I find a chapter dealing with some of the classical investigations in the domain of Physical Spiritism: the London Dialectical Society, Sir William Crookes' séances with D. D. Home and Florence Cook, Professor Zöllner's experiments with Slade (with striking reproductions of photographs from the Leipzig savant's *Wissenschaftliche Abhandlungen*), Eusapia Paladino; also Prof. Lombroso and A. R. Wallace's investigations (these, I think, might have been omitted). These old episodes are believed by the author to be genuine, not excluding the Katie King "materialisations." Whatever may be thought of such a standpoint, the position of Frau Moser towards more recent researches in Physical Phenomena ("Eva Carrière," Linda Gazzera, Stanisława Tomczyk, Mirabelli, Jan Guzik, "Margery," etc., etc.) seems rather illogical, for she is inclined to reject the genuineness of these latter "phenomena" *en bloc*, only making an apparent exception in favour of Willi and Rudi Schneider. That most of the investigations in question present most suspicious features no sane critic will deny. But it will be more logical to suppose, so at least it seems to me, that some of the older experiments—if not all—were vitiated by sources of error then unsuspected, than to postulate such a difference "in kind." In particular, the distinction drawn by the author between the *Lichtmedien* of the remote past and the more recent *Dunkelmedien* appears to me rather artificial—with the possible exception of the Slade-Zöllner experiments, where, supposing these to have been

non-authentic, very exceptional circumstances favourable to fraud *may* have operated (I do not say, *did* operate).

Frau Moser's attitude towards the spirit hypothesis is on the whole unfavourable, though one has the impression that she avoids committing herself, and though she might have drawn up against that hypothesis a much more powerful indictment so far as experimental Spiritism is concerned. She admits, however, incidentally that numerous researchers among the best have arrived at the conclusion that in some cases the spirit hypothesis is the only possible one. She adds that "it is in any case the simplest" (p. 640). (Frau Moser does not add that from the simplicity of a hypothesis it does not always follow that it must be the true one.) On the other hand, I miss in *Okkultismus* a chapter on Phantasms of the Dead and Haunted Houses, which might have furnished the author with spontaneous evidence for something like survival much more impressive—to me at least—than acres of records of mediumistic trance-utterances.

The chapters on telepathy and thought-transference, teleesthesia (clairvoyance), precognition, crystal-gazing, animal magnetism, hypnotism, "hylomancy" (psychometry), xenoglossy, *Kriminalmedien*, etc., are as full of incidents, observations and experiments gleaned in the literature of the subject as the chapters on Physical Phenomena. Frau Moser's conclusions are on the whole positive almost throughout. I note that in dealing with the *Kriminalmedien* she recommends extreme circumspection, whilst also admitting the genuine character of some of the alleged facts. This circumspection is, I think, much to be commended, especially if we recollect the recent fiasco of various rhabdomancers (posing then as "radiæsthesists") in the case of Nicole Marescot, a little girl apparently kidnapped a few months ago in France. I will not deny, however, that some of the evidence brought forward in the book under this heading is impressive, especially the verdict of the Leitmeritz (Czechoslovakia) tribunal in the Hanussen case (p. 618, etc.).

A few more brief remarks before I have finished.

Frau Moser will permit me to say that her book might have been considerably abridged without any damage to its contents. To give no other instance: the trio Home—Slade—Eusapia are marshalled before us twice (pp. 82-105 and 114-134, and 675-706). Once would have been quite enough, one would think.

Then, as a general rule at least, I would have omitted *dreams* as evidence of supernormal impressions. They almost always leave behind them far too vague a recollection, and are besides so numer-

ous that purely fortuitous coincidences become more unavoidable than anywhere else.

By the way, I am inclined to attribute to Chance pure and simple more importance than the author does, and am convinced that it can afford us an adequate explanation of not a few exact statements made by mediums or mesmerised subjects. I am convinced that much caution is here necessary, as several striking and obviously purely accidental coincidences in my own experience abundantly testify.

Frau Moser does not think much of the S. J. Davey experiments: they had, she says, "little in common with the best slate-writing performances, and "were not in the least like" the best Slade séances as described by Barrett and Wedgwood (pp. 914, 915). I am not sure Frau Moser does full justice to these "epoch-making" Davey sittings, whose importance is fully proved, *e.g.* by some of A. R. Wallace's pronouncements, both public and private. Writing to me in answer to a letter of mine (in 1891, I think), he said, after enumerating some of Davey's particularly striking feats:

"I shall not believe this to have been conjuring by any vague general statements, but shall require a precise description of how it was done."

(I am citing from memory, but remember this sentence with peculiar distinctness.)

On the other hand, I have never been inclined to press too far the theory of mal-observation, seeing that in the last analysis almost all evidence depends on human testimony, and also because experiments devised so as to exclude mal-observation to-day may prove unsatisfactory from that point of view at some future time.

On page 227 (Volume I.) the author gives—at second-hand—an account of Mgr. Valousek, a Czechoslovakian prelate and senator, which once more seems to go a long way towards proving that collective hallucinations (induced) are a real fact after all. I think this account one of the most interesting and suggestive pieces of evidence Frau Moser's *magnum opus* contains, and I can only regret that the testimony is not at first-hand.

Magnum opus indeed! The author is not infallible, but the book represents a splendid and to a considerable extent successful achievement worthy in many respects of admiration.

We heartily hope it may soon reach a second edition or be translated into English or French in a somewhat abridged form. In that case, we shall expect a few inaccuracies and misspellings of no great importance to disappear.

PEROVSKY-PETROVO-SOLOVOVO.

La Verità sullo Spiritismo. C. E. BECHHOFFER ROBERTS. Translated by Professor Maurizio Korach. Svo. pp. 292. Milan : A. Mondadori, 1935.

Hitherto there has been a lack of translations into Italian of English books on psychical research, with a few notable exceptions. We therefore welcome Professor Korach's translation of Mr. Bechhofer Roberts's book *The Truth about Spiritualism*, reviewed in our *Journal*, Vol. XXVII, p. 329. The Italian edition has a preface by the translator and several illustrations.

W. H. S.

Psychics and Mediums, A Manual and Bibliography for Students. GERTRUDE OGDEN TUBBY. Marshall Jones (Boston, Mass.). \$2.00.

Miss Tubby, who was Secretary of the American S.P.R. in its less controversial days, endeavours to help persons beginning the study of Psychical Research, with advice, based on her own long experience, both as to how they may best exercise any psychic faculties they possess, and how they may best derive advantage from sittings with persons whose powers are more fully developed.

One crux confronts every author who writes on Psychical Research for beginners, viz. : how is the question of fraud to be dealt with ? One way is to confine the book to such phenomena (and there are plenty) as are practically free from fraud : if this course is followed the question of fraud can be dealt with briefly, the main sources of error, such as faulty recording, being non-fraudulent. This means leaving out a very large part of what is generally meant by Psychical Research.

Another way is to state boldly that as regards many phenomena fraud is the dominant issue, and then to discuss in some detail what types of fraud are connected with what types of phenomena ; what conditions favour fraud, and what assist in prevention or detection. The second way is of course far preferable, if the size of the book permits. In a small book the first is the only practicable one.

Miss Tubby is not oblivious of the issue of fraud (see pp. 79-80), but her treatment of it is much too summary in view of the space she devotes to several types of phenomena in which fraud has been notoriously rampant. This defect is all the more curious as the book is dedicated to Richard Hodgson " a great pioneer and discoverer in the larger world of the psychic ", and particularly a great pioneer in the prevention and detection of trickery.

But if there are passages that would have made Hodgson stare and gasp, there are others which he would have warmly commended. If all sitters with mediums giving "communications" were to observe the rules stated and discussed in Chap. VIII it would be a great gain.

W. H. S.

The Recollections of a Geographer. E. A. REEVES, Foreword by Lieut.-Colonel Sir Francis Younghusband. Pp. 224. London: Seeley, Service & Co. Limited. 1935. 8s. 6d. net.

Mr Reeves, who is a member of our Society, was for fifty-five years in the service of the Royal Geographical Society, entering that service in 1878 and resigning in 1933. His long and distinguished career thus coincided approximately with the life-time of our Society. Alike in geography and psychical research, the period witnessed a great extension of knowledge and improvement of technique, and if at the close of it the psychical researcher must regretfully admit that in his subject there are large territories still incompletely mapped, he may derive some comfort from Mr Reeves's statement that, notwithstanding the far greater antiquity of geographical study, the mapping of the earth's surface is not by any means complete.

The greater part of the book consists of an account of the progress of geography during the period in question, with reminiscences of the famous men and women with whom Mr Reeves was brought in touch through his work. The last chapter, however, entitled "Psychic Experiences", will be of especial interest to our members. Throughout his life Mr Reeves has had psychic experiences of various kinds, including several instances when in a normal waking condition he has had visions of great vividness. In one case while living in the country, on the afternoon of Boat Race day, after the Race had been decided, but before he had any normal knowledge of the result, he was able to describe correctly several incidents in the Race, including the winning of the toss. Moreover, in his own words, "I suddenly saw, projected on the wall of the room, a clear picture of the race. There was Hammersmith Bridge crowded with people, and just before the two boats reached the bridge I could see them with their crews pulling for dear life. Cambridge was half a length ahead, and I felt sure they would continue to gain to the end. On the Monday morning following, on the front page of one of the picture papers, was the view that I had seen." Mr Reeves, who has never been interested in boat races, attributes his experience to

“some sort of delayed telepathy”. In the other cases of visualisation quoted by him there was no verifiable correspondence with external events.

Mr Reeves also gives accounts of sittings with professional mediums, in consequence of which he has become a firm believer in spirit communication.

W. H. S.

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued “For private circulation among Members and Associates only.” The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NO MEETING

THERE will be no Meeting of the Society in December, as the usual day for the Meeting will be Christmas Day. The next Meeting will be held on Wednesday, 29 January 1935, when Miss G. M. Johnson will read a Paper on "Psychical Research from a Sensitive's Point of View."

OBITUARY

As we go to press we deeply regret to record the death at the age of 85 of Professor Charles Richet, one of the oldest and most distinguished members of the Society. Professor Richet was President in 1905, and was for more than 40 years a corresponding member.

A fuller Memorial Notice will be printed later.

PRIVATE MEETING

THE 138th Private Meeting of the Society was held on Wednesday, 27 November 1935, at 5.30 p.m., Admiral The Hon. A. C. Strutt in the Chair.

Mr Theodore Besterman read a Paper on "The Mediumship of Carlos Mirabelli," which is printed below.

I

Mirabelli is a short, thick-set man said to have been born in 1889, but looking older. Genial, temperamental, dynamic, unreliable, he is the embodiment of what English people understand by the Latin, and especially the South American type. His father, Luigi (Luiz), and his mother were both Italians, but the medium was born in Brazil. Rumour, even printed evidence, about his early history is as plentiful as it is unreliable; nor have I thought it worth while to

investigate it at all closely. It will be enough for my present purpose to say that after the war Mirabelli was employed as an assistant in a shoe shop in São Paulo. Mysterious happenings of a poltergeist kind took place and were thought to be due to his presence.

Since that time Mirabelli has been more or less exclusively occupied as a medium. Though he is anxious not to be considered as a professional, in fact he is. Directly or indirectly Mirabelli demands and obtains (as I know only too well) substantial fees, far more substantial, indeed, than any ever asked of me before when attending sittings on a medium's own premises. The procedure is this: Mirabelli founds or causes to be founded an institute, for which he works, on the premises of which he lives, and to which sitters make payment. It was in this way that the Academia de Estudos Psychicos "Cesar Lombroso" was founded at São Paulo in September 1919; and Mirabelli's move to Rio de Janeiro led to the foundation there, in November 1933, of the Instituto Psiquico Brasileiro. It was in the house of this latter organisation that took place the five formal and the various informal sittings I was privileged to attend with Mirabelli in August 1934.¹

The attention of European students of psychical research was first drawn to the mediumship of Mirabelli (apart from vague rumours) in 1926, by the publication of a pamphlet in Portuguese entitled *O Medium Mirabelli*. This little book (a copy is in the Society's Library) is described on the titlepage as the result of an inquiry conducted by the Academia de Estudos Psychicos "Cesar Lombroso"; and this statement naturally carried a certain amount of weight. We can now see, however, that even the limited importance attached to this phrase was a mistake, for it would be very little exaggeration to say that the Academia and Mirabelli are, or were, to all intents and purposes, one and indivisible. An interesting illustration of this was the presentation to me, on the conclusion of the sittings, by *Mirabelli*, of the São Paulo Academia's diploma of merit.

The chief reason, however, why this pamphlet made a considerable stir² was the very long list of reputable names printed in it as of persons who vouched for the phenomena recorded. From the first, however, it proved difficult to obtain information from Brazil as to the precise status of this list. Such information as was received,

¹ This was made possible very largely by a substantial grant from a recent anonymous donation made to the S.P.R., and by a generous donation from Miss May Walker.

² Sometimes a very exaggerated one, as when Mr E. J. Dingwall described the Mirabelli case as of "enormous importance" (*Journ. A.S.P.R.* [1930], xxiv. 302).

by Dr Osty among others, gave rise to misgivings. And now, after my visit to Brazil, I have no hesitation in saying that the list is almost valueless. Far from being a list of investigators testifying to the reality of the phenomena they had observed, it really partakes far more of the nature of a visitors' list. Most of them had no idea of the use that was being made of their names.

The names of a number of persons who really did accept the phenomena undoubtedly remain : but I hope it will not seem discourteous if I say that their testimony is of relatively little value. These gentlemen have in most cases had no experience with mediums other than Mirabelli, and they have no notion of the conditions under which psychical research should be conducted. Their testimony, in short, has such value as can be given to evidence put forward by inexperienced and more or less casual visitors, concerning events over which they have no control.¹

These considerations certainly deprive the pamphlet *O Medium Mirabelli* of most of its interest, for the actual descriptions contained in it of the phenomena observed are almost as inadequate as the phenomena themselves are startling : while the illustrations are highly unconvincing set pieces. Nevertheless, the claims made for Mirabelli were so extensive and the testimony, at first sight, so overwhelming, that, as I have said, considerable interest was aroused. Efforts were made from the first to induce Mirabelli to come to Europe, efforts which were renewed time and again, but always without success. This failure was due to the impossible terms and conditions laid down by the medium, which, had they been accepted, would have made the resulting sittings the merest formalities.

Reports, however, continued to reach Europe from time to time, and a few years ago Professor Driesch had the opportunity of attending some demonstrations given by Mirabelli. Professor Driesch was not unimpressed by what he saw, but he made it clear that the conditions were such as to make a reasoned judgment impossible. (The name of Professor Driesch promptly appeared, in *O Medium Mirabelli Mystifera ?*, among those testifying to the reality of Mirabelli's phenomena.) More recently Miss May Walker had some sittings with Mirabelli, which she kindly reported to me at length. Her conclusion was, in brief, that some of Mirabelli's phenomena were

¹ In my notes I find the following first impression :

“ [Mirabelli is] so accustomed to a reverential audience, that his procedure is crude in the extreme. If his phenomena are fraudulent it is inconceivable that expert observation over a few sittings should fail to reveal the technique.”

It must not be supposed, however, that no suspicion is felt of Mirabelli in Brazil. He has been in trouble with the police on account of his activities, and several sitters, to my own knowledge, have felt, and feel, considerable doubts.

suspect, but that others seemed to be genuine. All these circumstances made me anxious to have sittings with this medium, and in 1934, as already mentioned, this was made possible, as part of an intended more general tour of America, which I was unable to complete owing to ill-health.

II

The sittings in which I took part were held in the evening, and they usually extended into the night. I made brief notes, as opportunity offered, during the sittings, and very full notes immediately on returning to my hotel or early the following (or rather, the same) morning. On the conclusion of each sitting an official record of the proceedings was made by those present, the essential parts being dictated by Mirabelli himself. Copies of these records, in the original Portuguese, are preserved in the Rooms of the Society; and I venture to suggest that a comparison of these records with my own notes will throw some light on the contents of such Mirabellian products as *O Medium Mirabelli*, *Mantendo a Verdade*, *O Medium Mirabelli Mystifica?*, Miguel Karl's *Martyrios e Acrisolamento do Medium Mirabelli*, Dr Carlos Pereiro da Castro's *O Espiritismo scientifico*, Dr Thadeu Medeiros's *As Faculdades Metergicas do Professor Mirabelli*, which are all in the Society's Library.

I do not propose to give a minute account of the whole of the transactions at the sittings I attended; it will be seen from what follows that this would be wearisome and uninteresting. All I propose to do is to give a general idea of what occurred, enlarging on only one or two of the phenomena.

First of all, a word about the conditions under which the sittings were held. These varied considerably. The illumination ranged from darkness to the brilliant light of seven or eight small naked electric lamps at the ceiling; the degree of light is sufficiently indicated during the description of each phenomenon. Those present always included officials of the institute, and usually Mme Mirabelli; the rest were invariably adherents of or sympathisers with the medium; the sitters ranged in number from one or two present at specific phenomena, to a group of some twenty, seated like an audience. The conduct of the proceedings was exclusively in Mirabelli's hands; and it was made clear to me from the beginning that I was to be merely a spectator. No sort of control was at any time exercised, suggested or asked for by any sitter other than myself, and then without success.

In general, the phenomena were of the following types:

(1) The largest group consisted of the supposedly paranormal

appearance of objects, usually small. These were undoubtedly all faked. Mirabelli merely distracted the attention of the sitters, usually by announcing that he saw an apparition floating near the ceiling, after which he took things out of his pocket and threw them about the room. It was very quickly possible to judge from the increasing excitement of Mirabelli's manner (always considerable) and from the increasingly persistent way in which he compelled the sitters to look in the direction in which he pointed, that an apport was in preparation. And on several such occasions, by discreetly glancing in the wrong direction (for this takes place in full light), I had the privilege of seeing that rare phenomenon, the *beginning* of the flight of the object—though it is true that on these occasions the propulsive force was provided by Mirabelli's hand.

To explain the ease with which such crude fraud is perpetrated it is as well to repeat the fact that there is never anything remotely approaching to a control of the medium. It is also well to realise that the sittings are held literally in the rooms of the Academy. Those present are only metaphorically sitters, for most of the time the medium is moving, even running, through a series of rooms opening into each other, with the sitters after him like the tail of a comet.

The following quotation from my notes illustrates an even cruder type of apport. "All through the evening Mirabelli had seen [an apparition] floating about carrying a bunch of flowers. Repeated attempts had failed to materialise these, until [an] Italian oration [which Mirabelli was to deliver]. As he was about to start on this Mme Mirabelli vanished from the room and all the doors were carefully closed. Mirabelli spoke throughout in a very loud voice, in fact he shouted. When he had finished he took two or three of us to a distant room at the end of a passage, and there, in darkness, there fell three separate flowers; finally Mirabelli turned round in ecstasy and saw [the apparition] putting the flowers on the chandelier, which was of course invisible in the darkness. Thereupon he rushed to the switch, and lo and behold the chandelier was wreathed with flowers. The amazement and delight of the sitters are indescribable; but surely it is simpler to assume that Mme Mirabelli had arranged the flowers there during the oration? Certainly nothing could have been easier and safer, though the evidence is obviously lacking." This note does not make it clear that we *entered* the room in darkness.

(2) A not very interesting phenomenon, several times repeated, was the following: several bottles of "magnetised" water were placed on a table; at a little distance away, ranging from a couple of feet to about five yards, the medium and a few sitters were seated in a semi-circle of chairs; the lights were turned off as soon as we

entered the room ; and, after we had taken our places, one bottle could be heard clicking against another, the tempo being varied to order. I cannot say definitely that I *know* how this was done, but I strongly suspect a black thread. I do not think that Mirabelli *attaches* a thread to the bottle which moves, but that a double thread is looped round it. If this is done it is only necessary to let go of one end of the thread, which can then be recovered and secreted. Moreover, the proceedings usually terminated by Mirabelli praying (literally, for the sittings are conducted in a very pietistic atmosphere) for a specially loud tinkle. This prayer is usually granted and the bottle falls over ; the reader will draw his own conclusion.

(3) A kindred phenomenon was the paranormal tearing of paper. This is described as follows in my notes. "With me alone [Mirabelli] went into yet another room, where nothing happened. He took up a sheet of thin paper, packing-paper size, and returned with it to the room in which the small group was assembled. This room was light enough for features to be distinguished at five or six yards. He then tore the sheet about two or three inches down, made me hold it up at arm's length, and ask [the apparition] to tear it in two. After several minutes, during which Mirabelli repeatedly touched the paper, I felt a slight tug, the paper moved, but the pull then relaxed with a sort of jerk. After a further interval, there was another and stronger tug, and the paper was torn across at an angle. I was then made to hold a ruler across the palm of my hand, and this was eventually pulled out of it. Later a pair of spectacles was made to move on a table. This was practically all that happened, and I feel pretty sure that these phenomena were done by means of threads, though, notwithstanding every effort, I was quite unable to catch sight of one. However, the following points are to be borne in mind : (1) the movements are always produced *towards* Mirabelli ; (2) they always occur at a moment when Mirabelli is in movement, though once or twice only his head was moving (I wonder what an examination of his beard would reveal ?) ; (3) immediately after each phenomenon of this kind, Mirabelli makes violent gestures of joy and throws himself on and embraces the object concerned. Also I took possession of the torn paper and find on it marks which, without being in any sense conclusive, support the hypothesis of a thread pinned to the paper. The ruler I was unable to examine closely ; but I did find similar punctures all at one end, though nothing that would not be found on most rulers, I suppose."

(4) There only remain, in the department of the parapsychical phenomena, two types, which I will now proceed to describe somewhat more fully. The first is thus reported in my notes : "The

audience was seated with Mirabelli facing them. He then delivered a little lecture (a thing he is liable to do, with considerable oratorical force, on the slightest provocation) on the difference between conjuring and genuine dematerialisation, etc. He proceeded to illustrate this by showing a few elementary coin-vanishing tricks, using a special coin of an obsolete kind, dated 1869, to which he drew our attention. Then he showed [what he claimed to be] some genuinely mediumistic phenomena. He went into another room accompanied by [one of the sitters], there, we were told, held the coin in his open palm, with the sitter's open palm over it. The coin then vanished, Mirabelli returned to the room in which we were sitting, and asked me where I wanted the coin re-materialised. I elected for my own pocket and in a moment or two Mirabelli announced that the coin had been precipitated into my breast-pocket; there I duly found it. This performance was repeated with each of the male sitters present, with success, except that on one occasion I ventured correctly to forecast to my neighbour where the coin would be found. It must be noted that at no time during the progress of this phenomenon did Mirabelli approach within three yards of the main body of sitters.

The way this trick was done was simple in the extreme. At a given moment, before the lecture, Mirabelli asked the male sitters one by one into an adjoining room, where he examined them "magnetically", making passes over them, etc. While doing so he slipped a coin into the pocket of each "patient". The vanishing of the coin is of course elementary palming, and the rest is obvious. All that is required is unlimited impudence and a sufficient number of similar coins. What first aroused my suspicion was this: when asked to examine the 1869 coin I *did* examine it and made a mental note of its characteristics. When I found the coin in my breast-pocket I immediately saw, from minute characteristic marks, that it was not the same one, and the rest was then obvious. Again, every coin was found in an outside breast-pocket except X's, who had his materialised into his hip pocket, and X had been the only "patient" who had been asked to take his jacket off, as I happened by chance to notice. What surprises me in this trick is the odd mixture of very soundly thought-out impudence (the medium admitting to and exhibiting conjuring skill), ingenious technique (I forgot to mention that when the "patients" returned to the circle they had to join the chain, thus preventing premature accidental discovery of the coins), and crude performance (hiding a coin on *every* "patient"). There is no doubt that with a little more care this trick could be developed into a really startling performance; no doubt in more sophisticated circles Mirabelli's genius would receive the necessary impulse."

(5) I have left till last the second of the two phenomena referred to above, and the only really impressive part of the Mirabelli sittings. To avoid any unconscious misrepresentation, I again quote from my contemporary notes: "To demonstrate the existence of human magnetic currents and the direction of their flow, Mirabelli produced a bottle with a perfectly bona-fide screw cap, on which he balanced an absolutely straightforward blackboard measuring roughly about 2 ft. \times 3 ft. (exact measurements later). The blackboard was slightly distorted, as if from heat. Several sitters then stood round holding their hands over the edges of the blackboard; Mirabelli did likewise, and in a minute or two the board began to revolve, performing about one and a half revolutions before it heeled over and fell. The same thing happened with a small swagger cane, weighted at each end with a metal ring. I can guarantee that no threads were used in either case. It remains to be seen by trial whether this can be done by slight blowing or the like." All this took place in brilliant light. I may add that, under the same conditions, I found that blowing cannot do the trick. The measurements of the blackboard I afterwards found to be: 69.9 cm. \times 80.3 cm. \times 1 cm., weight 3.65 kgm. The round cane is 60.2 cm. long, tapering from diameter 16.5 mm. at one end to diameter 11 mm. at the other; it weighs just on 70 gr.

Of a later sitting I wrote: "Then followed a repetition of the blackboard rotation phenomenon, which was done exactly as before, except that this time the board was supported on the bare mouth of an empty bottle. The actual performance was more striking, as the board performed, I think, three complete rotations before falling off. The light, though not so brilliant as last time, was very good, but not quite good enough for the film I took to come out, I suspect. I am still puzzled by this phenomenon; taking into account the good light, the fact that Mirabelli performs the phenomenon completely surrounded by standing "sitters", who seem to have complete liberty of movement, and the fact that he expressed no objection whatever to the filming, although I strongly emphasised the fact that the camera and the film were very special ones and would show every detail, the fact that Mirabelli allowed me on each occasion to arrange the *mise en scène* and did not precipitate himself on the board as it fell, the fact that the room, the table, and the bottle were all different, though the board was the same, all these circumstances make the hypothesis of threads practically impossible, while any other fraudulent method is difficult to conceive."

Major Rampling Rose had very kindly loaned me an excellent Siemens cinematographic camera, with which I had hoped to obtain

a film of the medium at work. Unfortunately the light, visually brilliant as it was during the blackboard phenomenon, was insufficient for cinematographic work, and the film shows no more than a vague blur of movement. I have described this phenomenon as candidly as may be, and I can only add that it still puzzles me. It is the only phenomenon of the many I saw during the Mirabelli sittings which presents a *prima facie* case, in so far as one is entitled to say this of a single phenomenon that appears inexplicable in a mediumship the other manifestations of which seem clearly fraudulent. Waiving such philosophical points, I could certainly wish to have the performance repeated under conditions making cinematography possible.

III

Mirabelli's mediumship is not, however, by any means restricted to parapsychical phenomena. Indeed, far more time is taken up at his sittings by the writings produced by his hand in numerous languages and on many subjects, writings which purport to emanate from the spirits of deceased persons, most of them eminent. A collection of these writings was published in São Paulo in 1929, under the title of *Mensagens do Além obtidas e controladas pela Academia de Estudos Psicicos "Cesar Lombroso" através do celebre Medium Mirabelli*. This book enables us to estimate the value of these polyglot productions. I also had the privilege, during my sittings, of witnessing the production of several of such spirit communications.

We may consider first the languages in which these communications are written. The text of the book under examination contains scripts written in English, Hebrew, Greek, French, and German, in addition to Mirabelli's native Portuguese and his parents' native Italian. This is merely the order in which the languages mentioned first appear in the book. The amount of material in each language varies considerably. The bulk of the scripts is in Portuguese; apart from this only in French are there any long passages, totalling about thirty-five pages. There is a total of about one and a half pages in German, including one fairly long script. The other languages occur in small fragments only, totalling a page and a half of English, two-thirds of a page of Italian, a dozen lines of what may pass for Hebrew, and five words of Greek. Except in fragmentary facsimiles of Oriental scripts, we are given no examples of any of the other numerous languages in which Mirabelli is stated to have written (pp. 9-10), such as Dutch, Czech, Spanish, Polish, Latin, etc.

Indeed, *Mensagens do Além* and my own experience suggest that

Mirabelli writes freely only in Portuguese and in stilted French. Here is a specimen of the latter taken from a script executed in my presence on 14 August 1934. It purports to emanate from a Professor Pozzi, though it is not clear why he communicated in French, and is reproduced *verbatim et literatim* from the original holograph now in the possession of the Society. This document begins as follows :

“ Bonsoir.—Il y a deja bien de temps que je ne retourne plus, pour vous dire combien je vous chéris, et combien j'apprecie le developpement que vous donnez constamment à la très sainte science de Kardec, la revelation du salut du monde.

“ Le Christ en nous annonçant la future venue de l'Esprit de la verité, savait bien ce qu'il disait, et connaissait, bien la signification que ses paroles avaient : son regard, en voyant dans l'avenir, était plus profond que celui des astronomes qui veulent deceler les mystères du ciel avec leur lorgnettes de fer et de verre ! ”

This is a decidedly favourable specimen of the French in which the scripts are written. Sometimes, however, the supposed communicating spirit rounds off a message with a few words in his own or in some other language. Thus, Cromwell, after three pages of Portuguese, adds a dozen lines in English, beginning : “ Till now I did write in Portuguese, that's for me a foreign language, but presently I must show you I have not forgotten my dear mother country and my loved english tongue, that I spook [how apt ! *Th. B*] in my youth ” (p. 102). Newton signs himself : “ Newton The man of the apple He who has ears . . . he who has eyes . . . as you know from the holy books ” (p. 88). And Lombroso concludes a message in French with a few lines in English, German, Italian, and Portuguese, in which he challenges the “ Herren Doktors ” (note the form of the plural) to explain the medium's command of languages. The English lines read : “ I hope this proof will be more than to make you believing, oh ! men ! you that always be in doubt. Jesus clear and help you ever and for ever. Good by *the same* [that is, Lombroso] ” (p. 159). The scraps in foreign languages are often quotations, though not acknowledged as such. An example is Newton's use of Ben Jonson's “ It is not growing like a tree in bulk ; doth make Man better be ! ” (p. 86). It is noticeable that the language of such quotations, as in the instance just given, is much more accurate than that of “ original ” material.

It would be idle to discuss, for it would be begging the question, why foreign spirits should communicate in good Portuguese when they “ get across ” their own languages so badly. “ William Crooks ”, at the end of a long communication in Portuguese, explains the matter in the following words : “ Anybody, amongst you,

schall certainly be surprised at hearing me in a tongue that during all my life were foreign to me, but you must undoubtedly know, thal [*sic*] we, abiding in the kingdom of the spirit, must have faculties that, till now, have been unknown by all the men on earth : if I were in Prussia, I should speak german. . . . I pray now, that the best and purest ghosts may defend you and learn you to partake of their elsarness [*sic*]. Good bye ” (pp. 39-40). This is no doubt plausible : but would Crookes forget how to spell his own name ?

Without entering more closely into such theoretical points, it is obvious enough that this kind of thing hardly justifies a claim that the medium “ writes ” in many languages, and it is equally obvious that, linguistically, such scripts as these do not demonstrate anything paranormal, even if we could rely on the texts printed being uncorrected, which we cannot, since no facsimiles are given of any of the scripts reproduced in the book.

This need not, however, be an insuperable difficulty for the spirit-hypothesis if the content of the scripts prove to be significant. The Brazilian supporters of Mirabelli consider these communications to be of great value, and receive them with a good deal of awe. The justice of this view must necessarily remain a matter of opinion. I can only give it as my considered judgment that the scripts are very largely quite uncharacteristic, in an evidential sense, of their supposed authors, that they contain no evidence of paranormal knowledge, and that the moral instruction which forms the bulk of them consists of the tritest and most stereotyped sentiments, inspired largely by *Allan Kardec* and by popular notions of Indian philosophy. There is certainly nothing in them beyond the reach of a man of Mirabelli's linguistic background and comparatively good education.

A few points remain to be considered in regard to these alleged spirit communications. The first is Mirabelli's alleged ignorance of all languages but Portuguese. I am satisfied that this claim, often repeated to me by Mirabelli and his friends, is untrue. In regard to Italian it is naturally suspect, in view of the medium's Italian parentage. Moreover, the medium several times delivered in my hearing long harangues partly in Portuguese and partly in Italian. The Portuguese passages were supposed to emanate from the medium, while those in Italian were alleged to be uttered under the inspiration of his father. As, however, the transitions from one language to the other were frequent, as the sentiments expressed were uniform and continuous, and as the medium's behaviour did not change in the slightest visible degree, it is not easy to accept this proposition.

In regard to French and English, I have grave doubts as to Mirabelli's complete ignorance. My knowledge of Portuguese, as a spoken language, being scanty, my numerous conversations with Mirabelli were conducted, on my side, in French, a little German and a little English. I could not help noticing that the medium usually knew what I was talking about, especially during the course of the somewhat delicate financial negotiations. Further, in regard to English, at least, there is the additional evidence of a farewell message very kindly written, and presented to me, by Mirabelli. The language of this is, if anything, superior to that of the English scripts. Here is a specimen passage, which I reproduce with all modesty :

“ We want you to be messenger of the wonders you have gathered from this faculty without rival. Tell our brother of London the Professor [Mirabelli] greatly desire to go to London to demonstrate experimentally and scientifically, the admirable phenomens wich you have observed during your visit here.

We are proud of having had the opportunity of being with a Spiritualist and progressive character such as our adoptive brother. For that very reason the Academy Cesar Lombroso of São Paulo confers upon you the diploma of which you are so deserving.”

It cannot be pretended that these sentences, even allowing for the obviously borrowed phrases, testify to a complete ignorance of English.

Another point is this : it is claimed on behalf of the medium that the rapidity with which these communications are written is in itself evidence of their paranormality. It is also claimed that the medium, while writing in one language, will carry on a conversation in another. Both these claims were made, with what I judged was customary enthusiasm, on behalf of the principal script produced for my benefit, Pozzi's French communication from which I have quoted. This document extends to under 1800 words, and was written, by my watch, in 53 minutes, that is, at the rate of 34 words a minute. Even Edgar Wallace or Alexandre Dumas *père* would undoubtedly have found it difficult *regularly* to compose *original* matter at this rate in a language not his own. The words in italics are, however, all-important. What cannot be done regularly can often be done in an occasional spurt. And there is a vast difference between original matter and an incandite concatenation of sentiments of the most hackneyed description, easily paralleled in scores of spiritualistic writings of the Continental school.

Apart from these sufficient considerations, it must be remembered that Mirabelli has written such messages by the hundred. He had

complete freedom, for the theme was not given to him. Moreover he may have written this identical message on half a dozen previous occasions, since his audience is always changing and a very small proportion of his writings finds its way into print. In short, I should hesitate to say that this performance was even difficult; and to say that it *requires* a paranormal hypothesis is preposterous.

As for the medium's simultaneous conversation, this was very reminiscent of the "talking in foreign languages" found with mediums of the Valiantine type. The talking consisted, in this case, of occasional exclamations and murmurs, no more.

Such, then, is the far-famed mediumship of Carlos Mirabelli (whose real Christian name, by the way, is Carmene). There are two possibilities: he is either a fraud pure and simple, or else he possesses a certain narrowly-defined paranormal faculty, round which he has erected, for commercial purposes, an elaborate structure of fraud. The sittings I attended were held under conditions over which I was not allowed any control, so that I cannot pretend to decide for one or other of these alternatives. I can only repeat, with the reservations already made, that there appears to be a *prima facie* case for the second possibility.

CORRESPONDENCE

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—No one will dispute Professor Dodds' claim, in his striking experiment, that he has impregnated his replies with a strong American flavour, or deprecate his admission that he has overdone it: even a film fan would admit that!

Professor Dodds has only produced another Gatty experiment, but in an "atmosphere" which has not been *vécu*. When he assumes that Fedá is at the same game, and confidently instances "curry" to bear him out, his choice of example is hardly apt! This reaction for the words he quotes is as natural to an Indian as "bread" for "make", "break", "eat", "earn", to a Briton.

The length of Fedá's reaction-times is certainly open to suspicion, but as this, we learn, is characteristic of all the Leonard personalities tested it may be due to some physical or psychophysical factor analogous to deafness, slowness of perception, or other defect in the "entranced" apparatus. Expert opinion (*i.e.* from Indians and Anglo-Indians) on Fedá's credentials will be interesting if not valuable.

Yours, etc.,

ERNEST S. THOMAS.

To the Hon. Editor of THE JOURNAL

MADAM,—According to his letter in the November *Journal* (p. 131) Professor Dodds concludes that “the results obtained with Feda are not distinguishable in quality from faked results,” and his argument is that he has produced results of equal quality, and in shorter average time, by impersonating a foreigner.

I grant the possibility of producing tolerably passable results by impersonating various types of people. While conducting the Leonard word reaction experiments I tested this for myself and achieved fair success; but it tends to lengthen reaction time. The Professor’s argument is based on this question of time; he thinks that Feda’s mean reaction time of 4·20 seconds is suspiciously long when compared with his own achievement of 3·36 seconds. Is he not failing to allow for the asserted *modus operandi* of trance control? Let him go through the list of stimulus words again, whispering his replies to *another person*, who shall then *voice them for him*. The mean reaction time, as taken from the replies voiced by that second person, will be considerably longer than when the Professor spoke the reactions himself.

Feda’s explanation of her speaking has always been that her thoughts have to be passed to the medium and spoken by means of the latter’s organism. The mean time for Feda’s word reactions is consistent with this statement. It would indeed have been surprising had Feda been as quick in completing this double process as was Professor Dodds with the single process of voicing his own replies.

C. DRAYTON THOMAS.

To the Hon. Editor of THE JOURNAL

MADAM,—It is always a pleasure to be criticised by so courtly an exponent of the art of controversy as Mr Besterman, and few men are better qualified to speak on the points he raises. It is accordingly with a considerable fear of appearing merely intransigent that I venture to express the view that he has done little more than point out that “If things had been otherwise, they would almost certainly have been different”—a proposition which I should be the last to dispute. In particular, I agree that a less formidable operator than Mr Besterman in his official capacity might well have caused less disturbance on the “suspicion words” in the case of Rudi and Olga than that actually observed; but I would certainly not agree that any change of operator would be likely to alter their strong similarity.

If ever opportunity permits, the study of the influence of the operator should certainly be undertaken, but in general I think that

the effects of his fatigue, etc., to which Mr Besterman alludes, are either removed by correction or subsumed under error in the manner indicated in *Q.S.T.P.*, ii, 2*b*, and my reply to Mr Maby's Note, section 4.

But at present I see no reason for supposing that the main outcome of the work to date would be in any appreciable degree affected by the factors in question.

Mr E. S. Thomas still leaves me in doubt as to who is supposed to have telepathed what to whom ; but I cordially agree that a set of " J " and " E " reactions taken by someone other than Mr Drayton Thomas would be of the utmost interest—if it were practicable to obtain them, which I doubt. On the other hand, I fear I remain unimpressed by Fedá's oriental reactions and attach a good deal more weight to Professor Dodds' pleasant little experiment reported in the *Journal* for November.

Yours, etc.,

WHATELY CARINGTON.

PRELIMINARY EXPERIMENTS IN PRECOGNITIVE GUESSING

To the Hon. Editor of THE JOURNAL

MADAME,—J'ai lu avec grand intérêt l'article de M. Carington sur le précognition (néologisme excellent—préférable au mot de prémonition). J'ai vu aussi les observations de M. Saltmarsh et de M. Thouless (Nov. 1935).

Puis-je rappeler à vos lecteurs qu'avant M. Carington, dans mon livre (*l'Avenir et la prémonition*) j'ai présenté des expériences que je considère comme très importantes (p. 165). Expériences que j'ai dirigées et qui ont été faites par Armand Durex et sa sœur Brigitte.

C'est la première fois, je crois, qu'on étudiait mathématiquement la connaissance de l'Avenir donnée par un médium. 37 petits papiers minuscules sont repliés sur eux-mêmes, de manière à former une petite boulette. Brigitte devait prendre un de ces papiers minuscules et, avant qu'elle ait tiré, Armand indiquait le numéro qu'elle allait tirer—de sorte qu'il ne peut pas être question de télépathie. Sur 17 expériences Armand a dit le chiffre exact 6 fois. La probabilité étant de $1/37$ —nous sommes bien au-dessus du chiffre probable.

Dans une seconde série, sur 17 expériences, il y a eu 2 succès. Ce qui donne, finalement, avec une P. de $1/37=8$, succès sur 32 expériences. Assurément, ceci représente une probabilité composée de : $1/300,000$ environ. Le hasard peut donner cela—mais cela est peu vraisemblable.

On ne faisait qu'une expérience par jour.

Inutile d'ajouter que ce n'étaient ni Armand, ni Brigitte qui écrivaient les papiers. C'était une tierce personne, tantôt moi—tantôt Robert, le mari de Brigitte.

J'aurais bien voulu qu'Armand pût continuer les recherches ; mais (chose bien curieuse !) il ne croit pas à sa lucidité—et ces expériences l'ennuient beaucoup.

Je n'en ai, du reste, publié qu'une partie et je pense, prochainement, les publier, avec plus de détails, dans la *Revue Métapsychique*.

La seule objection qu'on puisse faire à ces expériences, c'est que c'est la lucidité de Brigitte, et non celle d'Armand, qui est en jeu. Alors, quand Armand a dit un numéro, Brigitte, par sa lucidité, devine, dans la corbeille où il y a 37 papiers, quel est le papier correspondant au numéro dit par Armand. De sorte qu'il ne s'agirait pas de la précognition d'Armand, mais de la lucidité de Brigitte.

CHARLES RICHTET.¹

MEDIUMISTIC INTELLIGENCES

To the Hon. Editor of THE JOURNAL

MADAM,—In the *Journal* for October Mr Hume-Rothery points to one of several reasons which make it difficult for many of us to regard mediumistic intelligences—Mrs Willett's and other—as independent personalities : the contrast between the free flow of unverifiable matter, however abstruse and technical, and the strain and stumbling when it is evidential. There is also, in Mrs Willett's case, as it seems to me, a special difficulty : the exceptionally high proportion of private matter in her scripts, although the communicators represent themselves as men who were either officers of the S.P.R. or closely in touch with its work. It can hardly be supposed, I think, that Myers and Gurney, at least, would not know that veridical detail, to be of value, should be of a kind that can be published. Indeed, they were probably tantalized, in their time, by just such disabilities in this respect as now handicap Lord Balfour.

Yours, etc.,

HUBERT WALES.

ERRATUM.—Page 135, line 1, for “By von F. Moser.” read “By F. Moser.”

¹Professor Richet's letter arrived shortly before the news of his death.—HON. ED.

JOURNAL
OF THE
Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 29 January, 1936, at 5.30 p.m.

WHEN A PAPER ON

“PSYCHICAL RESEARCH FROM A SENSITIVE'S
POINT OF VIEW”

WILL BE READ BY

MISS G. M. JOHNSON

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

NEW MEMBER

Mellor, Miss J. V., 41 Kensington Park Gardens, London, W. 11.

MEETING OF THE COUNCIL

THE 335th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 27 November 1935, at 4 p.m., THE PRESIDENT in the Chair. There were also present : Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter and Admiral the Hon. A. C. Strutt : also Miss E. M. Horsell, Assistant-Secretary.

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

One new Member was elected. Her name and address are given above.

PRECOGNITIVE GUESSING, II :
REVISED AND EXTENDED ANALYSIS

BY WHATELY CARINGTON, M.A., M.Sc.

FURTHER work on the data discussed in the *Journal* for June, 1935, has led to the following revised and additional results :

1. *Dispersion.* (a) I may have been unwise to treat Set No. 9 (TABLE II) as "fresh". Actually, it was "Mr X's" second (witnessed) set, but it was done at a considerable interval after the first and under different conditions. These facts seemed to justify its being considered "fresh" for the purpose concerned, but as more conservative results are obtained by transferring it to the other category, it will be prudent to do so.

(b) On the other hand it is evidently more reasonable to credit subject No. 24 with 6 successes in the accidentally omitted column than with 5. (Cf. p. 92, § 6.)

(c) The perplexingly *low* dispersion shown by RN 23 was mainly due to a misunderstanding by myself of how these cases should be computed.

(d) The only reason for excluding my own first set from the Fresh and Experimenter set results under A, B, and C in TABLE III was for the sake of comparison with those under D, which leads to nothing of interest.

Amending accordingly we obtain the following values as the best available estimates of the dispersion in the cases given, using Method C throughout. The other cases are of no great importance and have not been re-computed.

	P	RN
All Sets - - -	.12	12 as before
25 Fresh Sets - -	.023	19.1
13 Other Sets - -	.33*	23.1
23 Experimenter Sets -	.32	27.1
15 Other Sets - -	.12	31.1

* In inverse sense ; *i.e.* low dispersion.

We conclude that there is distinct evidence (odds of 40 to 1) that the dispersion is unduly high for fresh sets.

2. *Singletons.* (a) Professor Fisher has kindly pointed out that my treatment of these is invalid, because, although the formula for the expected number appears to be correct (subject to *erratum*, p. 112, July), we do not know the distribution about this expected value and consequently cannot determine the significance of any deviation from it. The important results 43, 44, and 45, and others dependent on the treatment concerned, must accordingly be set aside until we know what this distribution is.

(b) It is, however, possible to apply a much less sensitive test, not depending on a knowledge of the distribution, to the point at issue.

Sacrificing most of the quantitative element, we may confine ourselves to enquiring whether D_0 , the difference between the expected and observed numbers of singletons, is *greater*—regardless of sign—for normal than for plus-one or plus-two successes. The expectation, of course, is that it will be greater no oftener than it is less.

Writing D_0 , D_1 , D_2 , for normal, plus-one and plus-two respectively, we find for the 57 groups :

$D_0 > D_1$,	34 times in 57 ;	χ^2 2.123 ;	P .07373.1
$D_0 > D_2$,	35 ,, ,, 57 ;	χ^2 2.965 ;	P .04373.2

Corresponding figures for other groupings are :

For 38 Sets :

$D_0 > D_1$,	20 times in 38 ;	χ^2 .053 ;	P .4174.1
$D_0 > D_2$,	22 ,, ,, 38 ;	χ^2 .947 ;	P .16574.2

For 25 Fresh Sets :

$D_0 > D_1$,	16 times in 25 ;	χ^2 1.960 ;	P .0875.1
$D_0 > D_2$,	15 ,, ,, 25 ;	χ^2 1.000 ;	P .1675.2

For 13 Other Sets :

$D_0 > D_1$,	4 times in 13 ;	χ^2 1.923 ;	P .9276.1
$D_0 > D_2$,	7 ,, ,, 13 ;	χ^2 .077 ;	P .3976.2

For 23 Experimenter Sets :

$D_0 > D_1$,	15 times in 23 ;	χ^2 2.130 ;	P .07377.1
$D_0 > D_2$,	18 ,, ,, 23 ;	χ^2 7.348 ;	P .00377.2

For 15 Other Sets :

$D_0 > D_1$,	5 times in 15;	χ^2 1.667;	P .1078.1
$D_0 > D_2$,	4 ,, ,, 15;	χ^2 3.267;	P .03578.2

Most of this is inconclusive, but there is certainly a suggestion that singletons for " aimed " (normal) guesses deviate from their expected values more than do those for " unaimed " guesses as represented by the plus-one and plus-two material: in particular RN 77.2 is significant as it stands.

The tendency is most strongly marked in experimenter as compared with non-experimenter sets, in which, of course, if chance alone were at work, we should expect $D_0 > D_1$ and $D_0 < D_1$, etc., to appear with frequencies proportionate to the number of sets. Actually we have :

	$D_0 > D_1$	$D_0 < D_1$	Total	$D_0 > D_2$	$D_0 < D_2$	Total
Experimenter Sets	15	8	23	18	5	23
Non-Experimenter Sets	5	10	15	4	11	15
Total	20	18	38	22	16	38
χ^2	-	-	3.702	-	-	9.914
P	-	-	.026 (79.1)	-	-	.0015 (79.2)

It is difficult to avoid the conclusion that there is something distinctly aberrant about the singletons for normal guesses, especially in experimenter sets, and that plus-one guesses share this to some extent.

(c) This view is strongly confirmed by the marked association of " loose packed " sets (excess of singletons) with specially high OR low scoring (successes) in the case of normal guesses, which does not obtain for plus-ones or plus-twos.

TABLE VI is arranged to show the highest and lowest scoring sets in contiguity, and whether they are Loose or Close packed. The letters under " Set " are my file references.

TABLE VI

HIGH SCORES					LOW SCORES					
Set	Score	Normal L or C	Plus 1 L or C	Plus 2 L or C	Set	Score	Normal L or C	Plus 1 L or C	Plus 2 L or C	
AN	228	L	L	L	L	166	L	C	C	
I	224	L	L	L	W	176	L	L	C	
V	222	L	L	L	X_1	181	L	L	C	
E	220	L	L	L	G	183	C	C	L	
F	218	L	C	L	U	183	L	L	L	
AO	218	L	C	C	T	184	L	L	L	
D	215	L	C	L	AF	184	L	L	L	
AL	215	C	L	C	Q	187	L	C	L	
Totals		7	1	5	3		7	1	5	3

J	209	C	L	L	N	191	L	C	L				
X ₃	209	C	C	L	A	192	C	C	L				
AP	209	C	L	L	X ₂	192	C	C	C				
K	206	C	C	L	X ₄	192	L	L	L				
M	206	L	L	L	H	193	C	C	C				
AY	206	L	L	L	AV	194	L	C	L				
O	204	L	C	C	B	195	C	C	L				
X ₅	204	L	C	L	P	195	L	C	C				
AK	204	C	C	L	C	197	C	L	C				
R	202	L	C	L	AX	199	C	L	C				
S	201	C	L	C									
AE	201	C	L	L									
Totals		5	7	6	6	10	2	4	6	3	7	5	5

I have taken the eight highest sets (AN to AL) and the eight lowest (L to Q) as representing “ specially ” high and low scoring, mainly because there seems to be a natural break in the success scores at this point. The true quartile points for 38 variates are, of course, at 9·5 places from the extremes ; but it is perfectly legitimate to divide at any point provided (as is the case here) that the division is made at the same place in all cases.

Referring to these 16 sets as “ extra-quartiles ” (EQ) for convenience, and to the others as “ intra-quartiles ” (IQ) we now have :

	NORMAL			PLUS 1			PLUS 2		
	L	C	T	L	C	T	L	C	T
EQ -	14	2	16	10	6	16	10	6	16
IQ -	9	13	22	9	13	22	15	7	22
Totals -	23	15	38	19	19	38	25	13	38
χ^2 -	8·416			1·727			·133*		
P -	·002 (80·1)			·095 (80·2)			·36 (80·3)		

In the foregoing, plus-one and plus-two entries are marked L or C according to whether their own (plus-one or plus-two) singletons are more or less than expectation. If we retain this feature, but rearrange the sets on the same lines as TABLE VI but in accordance with their plus-one or plus-two success scores, instead of normal scores, we obtain on working the same calculations for the new EQ versus IQ :

For plus-ones $\chi^2 \cdot 041$, P ·42 (81·2) ; for plus-twos $\chi^2 \cdot 225^*$, P ·32 (81·3) ; the values for normals necessarily remaining the same.

Retaining this last arrangement, but marking L or C in accordance with excess or deficit of normal singletons, we find :

For plus-ones $\chi^2 \cdot 675$, P ·21 (82·2) ; for plus-twos $\chi^2 \cdot 765^*$, P ·19 (82·3).

* In inverse sense : association is reversed.

RN 80.1 is highly significant, while none of the others are anywhere near it: but in all cases plus-ones are intermediate between normal and plus-twos. (*N.B.*—All three ways of working the plus-two data show an inverse association; but $S(\chi^2)$ is only 1.123 with three degrees of freedom, which is quite insignificant, P being about .4).

There is, however, a possibility, not yet fully explored, that the effect observed may be due to the skewness of the singleton distribution, this skewness itself depending on the score. RN 80.1 must accordingly be accepted with some reserve.

3. *Grouping Generally.* I am deeply indebted to Mr W. L. Stevens, of the Galton Laboratory, for giving me an exact method of treating groups. He has shown that in an aggregate of guesses containing a successes and b failures the expected number of groups of successes (of all sizes, not singletons only) is given by

$$\frac{a(b+1)}{a+b} \text{ with variance } \frac{a(a-1)(b+1)b}{(a+b)^2(a+b-1)}$$

Applying these formulæ to the 431 sheets which make up my 38 sets and 19 fragments, *seriatim*, we find

For 431 sheets from all sources	$S(\chi^2)$ 445.716 ; P .2483
For 382 ,, ,, 38 sets	$S(\chi^2)$ 389.093 ; P .3084
For 49 ,, ,, 19 fragments	$S(\chi^2)$ 56.623 ; P .1885

The analogue of my RN 43 is obtained by finding χ^2 for each set or fragment from the difference between its expected and observed group-totals, and the total variance, and summing these. We obtain :

$$S(\chi^2) 83.620 ; P .02 \text{ v.n.} \dots\dots\dots 86$$

Most of the effect, however, is due to the fragments, which give

$$S(\chi^2) 33.481 ; P .01 \dots\dots\dots 87$$

while for the 38 sets we have only

$$S(\chi^2) 45.139 ; P .40 \dots\dots\dots 88$$

It is clear that either singletons are very much more erratic than groups generally, or that the method I first used grossly exaggerated the effect; or both. There is some evidence in favour of the first supposition, and the second is not improbable, though we cannot tell until the variance for singletons has been found. On the present evidence, we can only say that there is a very strong suggestion to the effect that the grouping is actually anomalous in the

same sense as previously reported. In most other contexts we should regard it as clearly demonstrated.

4. *Grouping and Scoring.* More significant and interesting results begin to emerge as soon as we consider the connection between grouping and scoring.

(a) To start with, the deviations from the expected numbers of groups for the 200 sheets of the high-scoring sets (Cf. TABLE VI) are significantly less than those for low-scoring sets.

and
$$S(\chi^2)_{\text{HIGH}} \text{ is } 163.847 \text{ with } 200 \text{ DF}$$

$$S(\chi^2)_{\text{LOW}} \text{ is } 225.246 \text{ ,, } 182 \text{ DF.}$$

Proceeding as on p. 98 of my first paper, we obtain

$$\sqrt{2\chi_1^2 - \sqrt{2\chi_2^2}} / \sqrt{2} = 2.8605 ; P \cdot 005 \dots\dots\dots 89$$

(b) We may also consider the distribution of Loose and Close packed sets, or sheets, among EQ and IQ scorers, on the basis of groups, just as we did in 2 (c) above for singletons. We again find a significant association between loose packing and EQ scoring, in the case of normal guesses, which does not occur for plus-one or plus-twos ; and plus-one results are intermediate between normal and plus-two as before. The figures are :

	BY SETS			BY SHEETS		
	χ^2	P	RN	χ^2	P	RN
Normal	- 6.128	.017	90.1	4.304	.04	91.1
Plus 1	- 1.080	.30	90.2	3.113	.08	91.2
Plus 2	- .915	.34	90.3	.045	.83	91.3

It should be noted that while these results agree with those obtained from the singletons, the latter are distinctly stronger ; this tends to support the suggestion made in § 3 above that singletons are more aberrant than groups, *i.e.* that there is some "virtue" about a singleton success as such.

(*N.B.*—The same caveat as to the possible effect of the skewness of the singleton distribution should be entered here as in 2 (e) above. In neither case, however, would this appear competent to account for the order in which normal, plus-one and plus-two results arrange themselves.)

I find it very difficult to suppose that these significant and concordant results are all due to chance alone ; and I cannot think of, nor has any critic suggested, any systematic abnormality of calling or throwing that could give rise to them.

5. *Effect of Preference.* On general grounds we should expect that, if there is any kind of psychological connection between the

guess and the throw at which it is "aimed", the degree of preference shown for the different numbers 1 to 6 (or "values" as I shall call them here) would have some influence on the number of successes obtained with each. That is to say, one would expect preferred values to score, on the whole, more (or possibly less) than their fair share of successes. It is, of course, obvious that the more often a value is called (or thrown) the higher will be its score, if other factors are equal: but this is allowed for in the following treatment:

(i) In a sheet of 120 guesses, the normal number of calls for any value will be 20; so that all values called more often than this are preferred values.

(ii) It is easy to show that any given value may be expected to score $GTM/S(GT)$ successes in a sheet, where G is the number of times it is called, T the number of times it is thrown, and M the total number of successes for the sheet. This enables us to calculate the expected number, E , of successes for each value.

(iii) For each sheet, I correlate $G - 20$ with $O - E$, where O is the observed number of successes for the value concerned, up to the point (A) of determining whether the preferred values between them score more than their total expectation or less; or (B) of determining the *sign* of the algebraic sum of the products $(G - 20)(O - E)$, *i.e.* whether the correlation is positive or negative. I have not concerned myself with the actual value of the correlation coefficient, and the tests used are accordingly of a somewhat "diluted" nature and likely to yield feebler indications than a more fully quantitative treatment might do.

Plan A may be said to take account only of the fact of preference, while plan B is concerned also with its degree.

Example (Sheet V, 2):

Value	G	T	$G \times T$	O	E	$O - E$	$G - 20$	Products
1	6	13	78	1	.93	.07	-14	-.98
2	13	21	273	4	3.25	.75	-7	-5.25
3	18	22	396	5	4.71	.29	-2	-.58
4	42	25	1050	11	12.50	-1.50	22	-33.00
5	23	21	483	7	5.75	1.25	3	3.75
6	18	18	324	3	3.86	-.86	-2	1.72
Total	120	120	2604	31	31.00	0.00	0	-34.34

It will be seen (A) that the preferred values 4 and 5 score between them 18 successes as compared with 18.25 expected and that (B) the algebraic sum of the products is -34.34 ; so that the sheet is reckoned as negative on either plan.

Performing this calculation for each of the 382 sheets of the 38 sets, we obtain :

	METHOD A	METHOD B
Positive Sheets -	199	210
Negative Sheets -	183	172
χ^2 - - -	.670	3.780
P - - - -	.21	.025 v.u.....92. 1 and 2

There is clearly a fairly strong general tendency for preferred values to obtain more than their fair share of successes, and this is more marked, as we would expect, when the degree as well as the bare fact of preference is taken into account.

If we now tabulate our results in the manner of TABLE VI, that is to say by sets and according to the set scores, and count the positive and negative sheets occurring in the four "quadrants" we find a very curious state of affairs. We have

	METHOD A		METHOD B	
	+ ve	- ve	+ ve	- ve
For E - Q High Scorers -	31.5	48.5	32	48
„ I - Q „ „ -	66	54	69	51
„ E - Q Low „ „ -	47	35	49	33
„ I - Q „ „ -	54.5	45.5	60	40
	<u>199</u>	<u>183</u>	<u>210</u>	<u>172</u>

It is clear that specially high scorers form a strong exception to the general rule just mentioned. Comparing E.Q.H.S. with the others we have, for Method A :

	Positive	Negative	Total
E.Q.H.S. -	31.5	48.5	80
Others - -	167.5	134.5	302
Total -	199	183	382
Whence χ^2 - -	6.560 ; P .0193		

and for Method B :

	Positive	Negative	Total
E.Q.H.S. -	32	48	80
Others - -	178	124	302
Total -	210	172	382
Whence χ^2 - -	9.166 ; P .001394		

I should have expected success of preference to have been associated with scoring in the opposite sense, but the figures seem coercive.

6. *Discussion.* Apart from criticism of real value, such as that of Professor Fisher mentioned above, there appears to be (judging from my correspondence) a certain tendency to cavil at the fact that many of my results indicate the operation of non-chance factors. This takes one of two lines :

A. The data do not show the existence of a strong and/or wide-spread faculty of precognition or ' foretelling the future ' ; it would be contrary to common experience, and quite incredible, if they did ; therefore my results are mere artifactual wish-spawn.

B. There " must " be " some sort of correlation between guesses and throws " : otherwise nothing but chance results could be obtained.

I do not know which of these I like best, for each reminds me that the circle is the perfect figure—in Logic as elsewhere ; but the following remarks seem relevant.

General considerations of the kind commonly known as philosophical make it appear not wholly improbable, to my mind at least, that consciousness may have some kind of a forward extension in time, and thus some kind of " contact with futurity ". On the other hand, the continued prosperity of Casinos and thimble-riggers makes it clear that " straight " precognition of the type of event here considered is at best extremely rare. Further, a life in which straight precognition were the rule would, I think, be quite impracticable—not to say insupportable.

Consequently, if anything of the nature of " preawareness " (to coin a word) exists at all, it seems much more reasonable to suppose that it would be found in the highly garbled and perverted guise suggested by my results than in the straightforward, but incredible, form which critics of type A simultaneously demand and repudiate. If I had obtained strong evidence of straight precognitive faculty, I should have found it very difficult to accept ; but the curious indications I have actually obtained seem to fit very well with the supposition that future events are not so completely insulated from contemporary consciousness as classical theory would maintain, but that something in the nature of an intricate protective mechanism, or censorship, is interposed to prevent our minds being over-run by impressions which would only confuse us.

As regards " criticism " of the second type, I fear I must insist that nothing of the kind is even worth discussing unless it is reasonably specific. By this I mean that it is up to the critic to *show how* some prescribed tendency in guessing or throwing, or both, could actually produce some at least of the effects observed. As soon as

this is done we can examine the data to see whether such a tendency actually exists and whether it does in fact produce the effects concerned. But vague contentions based on "must" are futile.

On the other hand, it is clear that the mathematics of the subject are much more intricate than an amateur like myself might suppose, so that it is especially necessary to be on guard against inadvertently generating artifacts by inappropriate technique.

CORRESPONDENCE

PRELIMINARY EXPERIMENTS IN PRECOGNITIVE GUESSING

To the Hon. Editor of THE JOURNAL

MADAM,—In the *Journal* for November Mr Saltmarsh flatters me unduly, for it seems very possible, as pointed out in my Note in the October issue, that the effects he discusses may have been due to an insufficiently skilful handling by myself of the technique employed.

It would accordingly be premature to discuss in detail the highly ingenious argument he puts forward: but, speaking for myself, I think I should prefer his second alternative to his first—supposing that further work shows that one or the other must be invoked. If we are going to swallow the camel of precognition at all, it seems to me likely to prove a mere straining at gnats to object to the view that some stratum of the subconscious is 100 per cent. precognitive—or substantially so with respect to the events considered—and that the effects observed may be produced by the action of a somewhat erratic censor between this layer and the conscious. But I do not feel called upon to cross this bridge just yet.

Similar reservations perforce apply to any attempt to answer Professor Thouless' welcome comments: but here I am on firmer ground in one respect at least, in that—with the utmost deference—I do not believe one little word of his muscular habit-system story! This seems to me just the kind of thing which, translated into terms of roulette croupiers at Monte Carlo, has led to innumerable suicides, forgeries, embezzlements, bankruptcies, elopements and kindred disasters—for it is just sufficiently plausible to be dangerous. Unquestionably one forms habit-systems of shaking and throwing so far as the major movements are concerned, but their minor variations are legion and the combinations of these innumerable; and it is here, I think, that the basis of randomness is to be found.

I should be sorry to attempt to define true randomness or "pure chance" with precision; but I do not think we shall be far out if

we take it as that state of affairs in which the possible number of combinations of independent result-affecting variations of the causes concerned is large compared with the number of possible results—or something very like this; and, if some such criterion be adopted, I think it will be clear that it is satisfied by any normal process of shaking and throwing a die, however stereotyped its main outlines may be. Variations of the precise angle at which the die is picked up, of the amount of spin given in dropping it into the shaker, of the exact force and direction of the shake, of the angle at which it is ejected, of the height of the shaker at this moment, and of a host of other factors, all contribute to produce substantially perfect randomness. This, indeed, is presumably the reason for dice having been used for so many centuries as a basis of games of chance, while I have yet to hear of anyone making a living by cultivating a habit-system of throwing.

However, the matter is best settled by an appeal to facts, and so soon as I am free from the pressure of further work, I shall be interested to investigate at least an extensive sample of my raw material to determine whether there are any signs of Professor Thouless' hypothesis being true. For the moment I can only thank him for putting forward a constructive suggestion susceptible of test in welcome contrast to objections based on "must" and lacking any account of how they are supposed to work.

Yours, etc.,

WHATELY CARINGTON.

To the Hon. Editor of THE JOURNAL

MADAM,—As Mr Saltmarsh suggests, there is no evidence whatever that precognition has 100% efficiency: it is, however, a tenable hypothesis that some region of the subliminal has a complete relationship with future event, though this relationship is not brought to what we should call a cognitive level. It would be at the obscure level which Professor Broad's presidential address has done a good deal to define, if not to illuminate. And I should limit this hypothesis of an obscure but complete relationship with future event to "preawareness" of maximum probabilities, since the idea of a rigid future contains a contradiction of known psychological data—I say nothing of the metaphysical difficulties.

Mr Carington's experiments, however, deal only with events of maximum probability; and I suggest that in guesses at these events it may not be at all a question of some "preawarenesses" being

“right” and others “wrong.” This is what we can say of the guesses when they emerge as items of cognition, true or false, chance-determined or otherwise. My own impression is that correct “pre-awareness” exists, at its own subcognitive level, over a very large field: and that its results tend to emerge in highly distorted forms because our minds contain strongly organised protective resistances against seeing the future, in our present stage of evolution. (We are not yet very good at seeing the present.)

On this basis, I think, 100% precognition—of a kind—can be accepted as a working hypothesis, so as to remove Mr Saltmarsh’s difficulty, and leave some explanation possible, on Mr Carington’s lines, for his undoubtedly significant discovery of “anomalous packing” with its implication that *some* touch exists between the mind and future event.

Yours, etc.,

KENNETH RICHMOND.

THE MEDIUMSHIP OF MIRABELLI

To the Hon. Editor of THE JOURNAL

MADAM,—The report of Mr Besterman on Mirabelli printed in the *Journal* for Dec. 1935 is so remarkable a document that I should like to make a few comments upon it.

Five years ago I drew the attention of English-speaking inquirers to this case and emphasized its enormous importance to psychical researchers. Since then a number of persons from Europe have visited South America and, as far as we can judge from printed reports, have done little or nothing to clear up the puzzle. Indeed, from Mr Besterman’s paper it would seem that he does not understand what the problem is. Had he been given definite and precise instructions what to find out in Brazil he might have had something of value instead of bringing back stories of silly tricks. In spite of these, however, Mr Besterman has come to a surprising conclusion. He thinks that there is a *prima facie* case that Mirabelli may possess some “paranormal” faculty, and this is based on the fact he was unable to detect the *modus operandi* of a revolving blackboard effect. Apart from the fact that there was no reason why he should have been able to understand it, are we expected to believe with the late Alfred Russel Wallace and Hewat McKenzie and now Mr Besterman that because these gentlemen could not and cannot discover how certain conjuring tricks are done there is a *prima facie* case for the

successful performers possessing "paranormal" faculties? It is this that makes psychical research ridiculous, and rightly so.

In my account of Mirabelli, which was printed in 1930 by the A.S.P.R., I described certain phenomena and named the parties who were said to have been present. Among these were Dr G. de Souza, Mr Soares, Dr Cavaleanti, Dr Moura and Dr Mendonça. Did Mr Besterman interview any one of these persons? Did he talk to any of the sitters who are recorded as being present at the alleged materializations of Bishop Barros, Prof. Ferreira, or Dr de Souza's daughter? To say that their testimony "is of relatively little value" is beside the point. It is as valuable as that of Mr Besterman, since what they record is quite as striking as anything with D. D. Home. Do these witnesses exist? Were they present at these sittings? Were they lying or are they made to record phenomena which never took place at all? Or must we admit that certain "events took place which were described by those who witnessed them in the terms we have read"? What were those events? I wrote these words in 1930. No answer has been attempted. Yet in 1934, at heavy cost to the S.P.R., Mr Besterman goes to South America ostensibly to inquire into what he terms Mirabelli's "astounding feats" and comes back with tales of revolving objects which puzzled him, just as Professor Driesch and Miss May Walker came back with stories of a door which closed by itself and a camera which jumped on a table.

The problem of Mirabelli is the same as that of Home. In the latter case the witnesses are dead and cannot now be interviewed: in the former case they are living and can be seen and cross-examined. Signed statements by Dr G. de Souza, Dr Moura or Dr Mendonça describing in their own words what they saw on certain occasions as recorded in *O Medium Mirabelli* would be worth far more than stories of revolving blackboards and jumping cameras which puzzled observers who would be equally puzzled by 90% of conjuring tricks performed by even moderately skilled artistes. As it was it seems that the medium found Mr Besterman fairly "easy," since he not only put a coin into Mr Besterman's pocket without the latter knowing anything about it, but then suggested to him to name the very pocket into which the coin had been put when, according to Mr Besterman's account, he had been given free choice to name the place where he wished the coin to re-materialize. If Mr Besterman had said, "under my heel inside my shoe," what would have happened? Evidently Mirabelli knew his sitter.

Yours, etc.,

E. J. DINGWALL.

PROXY SITTINGS

The Editor, JOURNAL OF THE S.P.R.

MADAM,—The importance and interest of the proxy sittings described in “*Through a Stranger’s Hands*”¹ is so great that I venture to suggest the extension of the method a little further. For while it is clear that the Society must continue its excellent work of testing the evidence for survival by every ingenious method that can be thought of, there seems no reason why other experiments should not be carried on concurrently, with the hypothesis of survival provisionally adopted as a working basis.

There are now many people who possess extra-normal power and who seem to themselves to be in touch with some definite group of discarnate personalities, acting together and more or less responsible for the communications made by automatic writing or clairaudient dictation. The “free flow of unverifiable matter” coming through these channels is often interesting, but there is no means of separating out what comes from the medium’s subconsciousness and what comes from other sources. Might it not be worth while to try some proxy sittings *not* for evidence of the survival of the recently dead, but for evidence of the independent intelligence of these “groups”. If, for instance, a group A produced through their usual channel (an amateur sensitive probably) a definite body of teaching, might it not be possible to invite the A group to communicate something relevant to that set of ideas through a Leonard sitting, the sitter being in complete ignorance of the kind of thing expected?

Yours very truly,

THEODORA BOSANQUET.

SCARCITY OF ACCOUNTS OF SPONTANEOUS CASES IN
The Journal.

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

June 29, 1935.

MADAM,—As a member of the S.P.R. of more than forty years’ standing, may I express the regret that accounts of spontaneous cases on the pages of our publications appear to grow scarcer and scarcer?

I wonder what the reason of that scarcity can be. Three hypotheses present themselves to me:

(1) Such cases are of less frequent occurrence than before.

¹*Through a Stranger’s Hands, New Evidence for Survival*; compiled by Nea Walker. (Hutchinson, 16s.).

(2) Accounts of them are being sent to the S.P.R. far more seldom than before.

(3) The S.P.R. attaches much less importance to them than formerly.

Hypothesis 1, if true, would be both extraordinary and interesting. Hypotheses 2 and 3 I would consider as most undesirable.

For it has long seemed to me that the collection and study of spontaneous cases are likely to be one of the most fruitful fields of "psychical" investigations. And judging by my own experience—and why should I be here a particularly privileged person?—incidents affording *prima facie* evidence of supernormal, nay even *post mortem* action are relatively not very infrequent.

We ought, in my opinion, to collect and register as many as we can. I do not say our Society does not; very likely it does. But I note that our *Journal* contains now but very few such incidents indeed.

And yet does not such a work as *Phantasms of the Living* constitute even now, some fifty years after its publication, one of the foundation stones on which the S.P.R. has been raised?

If there is some hope for Psychological Research of getting tangible and positive results it is, I think, chiefly in the study of *spontaneous* phenomena. Could not a decisive effort be renewed in *that* direction?

I am,

Yours, etc.,

PEROVSKY-PETROVO-SOLOVOVO.

[We judge from the response in 1934 to appeals made during the B.B.C. Talks, that spontaneous phenomena are occurring very much as they always did. But it is difficult to persuade people to take the trouble involved in sending accounts promptly: thus, only four per cent. of the cases of telepathy which reached Mrs Salter after her talk were up to the standard required for publication in the *Journal*. It would seem that the only way of getting a steady flow of spontaneous cases is to have a large number of active and persistent *collectors*. And, if the Count's letter stimulates any of our members to take up that work, it will have been of great service. Some of our most energetic members happen to be working on other lines. But we should indeed welcome a response to the Count's appeal. Perhaps new members may feel inclined to respond and thus to gain an insight into the work of the Society which they will not otherwise get?

HON. ED.]

JOURNAL

OF THE

Society for Psychical Research

NOTICES OF MEETINGS

The Annual General Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

On WEDNESDAY, 26th February 1936, at 3.30 p.m.

*To transact the business set out on the formal notice dated
the 16th January 1936, and already circulated*

AFTER AN INTERVAL FOR TEA

MR C. V. C. HERBERT

WILL GIVE AN ACCOUNT OF HIS

“ EXPERIENCES WITH CONTINENTAL MEDIUMS
LAST AUTUMN ”

*N.B.—Members alone have the right to take part in the business
of the Annual General Meeting, but Associates may be present both
during the transaction of business, and when Mr Herbert is giving his
account.*

MRS HENRY SIDGWICK

It is with the deepest sorrow that we record the irreparable loss sustained by the Society through the death of Mrs Henry Sidgwick on the 10th February. Ever since the Society's foundation, fifty-four years ago, Mrs Sidgwick played a leading part in our work, and she has left an enduring mark both on psychological research as a study, and on the S.P.R. as an organisation for pursuing it. The Annual Report printed in this issue mentions her attaining the age of 90 during last year.

HON. EVERARD FEILDING

WE also regret to record the death on the 8th February of the Hon. Everard Feilding, for many years Member of Council and Hon. Secretary of the Society, and an important contributor to our *Proceedings* and *Journal*.

NEW MEMBERS

Cuddon, Eric, M.A., B.C.L., 6 Pump Court, Temple, London, E.C. 4.

Doulton, Peter D., 37 De Vere Gardens, London, W. 8.

Hamilton, Mrs T. Glen, 185 Kelvin Street, Winnipeg, Canada.

Ketner, Dr C. D., Den Helder, Holland.

Ross, Robert, H.M. Consul, British Consulate General, 360 N. Michigan Avenue, Chicago, Ill., U.S.A.

Verner, L. H., 2 Molesworth Avenue, Stoke, Coventry.

Student Associate

Leggett, D. M. A., Trinity College, Cambridge.

MEETING OF THE COUNCIL

THE 336th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 15 January 1936, at 3.45 p.m., THE PRESIDENT in the Chair. There were also present: Mr Oliver Gatty, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Dr T. W. Mitchell, Mr W. H. Salter, Mrs W. H. Salter, and Admiral the Hon. A. C. Strutt; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary.

Six new Members and one Student Associate were elected. Their names and addresses are given above.

The Report of the Council for 1935 was considered, and adopted as amended.

THE MYERS MEMORIAL LECTURE, 1935

THE attention of our Members is called to the publication of this lecture, the fourth of the Series, which was delivered last autumn by Mr Whately Carington. It is entitled *The Meaning of "Survival"*, and can be obtained from the Secretary (price 1s.).

ANNUAL REPORT OF THE COUNCIL FOR 1935

(1) *Organisation of Research.* As announced in the Annual Report for 1934 the Council appointed a Research Committee, to advise as to the future organisation of research and to initiate and carry out such research work as was practicable in the immediate future. After long consideration of various possibilities, the Council adopted two recommendations of the Research Committee: (1) That in the present circumstances of the Society it would be desirable to appoint a young graduate of a British University as Research student, to be trained in the Society's methods, with a view to possibly becoming Research Officer later; and (2) That Mr C. V. C. Herbert should be appointed Research Student for one year from September 1935. Mr Herbert is a graduate of Cambridge University, a member of the Society, and has had experience in psychical research and various branches of scientific work.

The Research Committee continues to meet regularly.

(2) *Experiments conducted by Mr G. N. M. Tyrrell.* Mr Tyrrell has during the year actively pursued his experiments in Extra-Sensory Perception, both with the apparatus designed by him and described in the *Journal* for April 1935, and with a new electrical apparatus he has designed, the object of which is both to eliminate sources of error in this class of investigation, and, where positive results occur, to differentiate between different types of extra-sensory perception, such as telepathy, clairvoyance and precognition. Additional experiments have also been done with cards. Mr Tyrrell has had interesting results, and it is hoped that these will be embodied in a report which will shortly be given to the Society. The Council appealed to members of the Society to collaborate with Mr Tyrrell in his experiments, and they wish to express their thanks

to all those who responded to this appeal. The cost of constructing the electrical apparatus already mentioned was borne partly by a generous donation from the President, Professor C. D. Broad, and partly by the funds of the Society. Additions to the apparatus were made by Mr Tyrrell with the generous co-operation and assistance of Mr Saltmarsh.

(3) *Quantitative Study of Trance Personalities.* Mr Whately Carington has continued his important studies in this field. A further report by him was read to the Society on the 27th March and has since been published in Part 141 of *Proceedings*. Mr Carington has also been conducting experiments in precognition.

(4) *Records of Sitzings with Mrs Leonard.* For some time past various members of the Society have been so good as to place at the Society's disposal complete annotated records of their sittings with Mrs Leonard with a view to providing matter for a thorough psychological investigation. Mr Kenneth Richmond, a trained psychologist, has kindly consented at the Council's request to undertake the laborious task of making such an examination, and on this task he has now been engaged for the greater part of the year. During this period a further opportunity occurred, which he gladly took, of studying Leonard material of a slightly different type, namely records of "proxy" communications obtained by Miss Nea Walker from "communicators" concerning whom she had very small information, and Mrs Leonard none at all. As a result of this study several of these records have been published in a book (*Through a Stranger's Hands: New Evidence for Survival*, compiled by Nea Walker) edited by Mr Richmond with a Preface by Sir Oliver Lodge. Mr Richmond's main task is likely to occupy him for some time to come, but he hopes very shortly to present a first report on it.

(5) *Finance of Research.* In respect of all the above-mentioned pieces of research work the Council has made grants which, while they cannot be considered as commensurate with the value of the work done, have nevertheless facilitated the doing of it. The grants have been made from the anonymous donation referred to in the Annual Report for 1934, which has enabled the Council to encourage research in several directions in a way that would otherwise hardly have been possible.

(6) *Mr Herbert's Tour.* Mr Herbert took up his duties as research student after the summer holidays. Owing to the generosity of a member of the Society, who offered to pay all expenses, he was able in company with some other members of the Society to undertake a visit to Austria, Poland and Czecho-Slovakia, in the course of

which he had sittings with various mediums, including Rudi Schneider, Kluski and Ossowiccki. In particular he had an opportunity of observing in operation an apparatus designed by Professor Schwaiger, which may prove important in parapsychical investigation.

(7) *Mrs Warren Elliott.* Mrs Elliott, who in the year 1928, gave a long series of sittings for the Society, a report on which was published in Part 112 of *Proceedings*, has very kindly offered to permit a further investigation of her phenomena to be made by the Society. The Council has very gratefully accepted this generous offer, but Mrs Elliott's regrettable ill-health has so far prevented any experiments being carried out.

(8) *Other Research Works.* The Council regrets that the number of spontaneous cases reported to the Society does not show any appreciable increase. This branch of the Society's work is important, and the Council once again expresses the hope that members will do all they can to bring well-attested spontaneous cases to the notice of the Society. It is intended that such cases shall in future be dealt with by Mr Herbert, in consultation with the Secretary and Hon. Secretary.

(9) *Myers Memorial Lecture.* The Council appointed Mr Whately Carington to deliver the Myers Memorial Lecture for 1935. Mr Carington gave his lecture on the 30th October, and it has since been published by the Trustees of the Fund under the title *The Meaning of "Survival"*. Copies (price 1s.) can be obtained from the Secretary.

(10) *The Presidency.* The Council elected Professor C. D. Broad, Litt.D., Knightbridge Professor of Moral Philosophy in the University of Cambridge, President for the year. Professor Broad delivered his Presidential Address on the 1st May: it has been published as Part 142 of *Proceedings*.

(11) *Changes in the Council.* The Council greatly regrets to record the loss from their numbers of three Council Members, who found themselves, owing to illness, unable to continue to attend Council Meetings, namely, Mr. J. Arthur Hill, Mr W. R. Bousfield, K.C., F.R.S., and Professor F. C. S. Schiller (a Vice-President and former President of the Society). All three had for many years been members of the Council, and have rendered great service to the Society and to psychical research. The Council appointed Professor Broad and Mr Whately Carington (hitherto co-opted members) to fill the vacancies among the elected members of Council caused by the resignations of Professor Schiller and Mr Bousfield.

(12) *Mrs Sidgwick's Ninetieth Birthday.* On the 11th March Mrs Sidgwick, who has been very closely connected with the Society's

work ever since its foundation, and to whose services in research, in administration, and in counsel, the success, and even the continuance of the Society has been largely due, completed her ninetieth year. The Council at their March meeting passed an unanimous vote of congratulation to her, which they are sure will have the hearty approval of the whole Society.

(13) *Changes among the Officers and Honorary Officers.* In March Admiral the Hon. A. C. Strutt took over the duties of Hon. Treasurer; the Council wishes to thank him for undertaking this work.

Mr Besterman's appointment as Investigation Officer and Librarian terminated in January, and the Council then took occasion to place on record their appreciation of the many services rendered by him to the Society.

Miss Kathleen Watkins, F.L.A., has very kindly agreed to act as Hon. Librarian. The Council consider themselves very fortunate in obtaining the services of a trained professional librarian for this position.

They also wish to thank Miss H. E. Kennedy, who has offered to translate for the Council any letters, etc., in the various foreign languages with which she is familiar.

(14) *New Honorary Member, Corresponding Member, and Honorary Associates.* The Council has made an addition to the very small numbers of Honorary Members of the Society by electing Professor R. A. Fisher, of London University, who has on several occasions assisted the Society's investigations by placing at the disposal of our members his unrivalled statistical knowledge. In electing Dr Elwood Worcester as a Corresponding Member they have renewed the close personal ties between our Society and the Boston Society for Psychological Research. Dr R. H. Thouless of Glasgow University, and Professor Hoernlé of Witwatersrand University, South Africa, both of whom have taken an active part in the Society's work, have been elected Honorary Associates.

(15) *The Tenancy of the Society's Premises.* The Society's original Lease of No. 31 Tavistock Square was due to expire in June 1937, and in view of the dislocation of the Society's work which would occur if the Society were compelled at the expiration of this Lease to move to other premises, the Council approached the Landlord as to the possibility of a renewal of the Lease. They consider themselves very fortunate in having secured from the Landlord an agreement for a new Lease of 21 years to run from June 1935 (the last two years of the old Lease being surrendered), even though the rent under the new Lease will be somewhat higher than under the old. As on many previous occasions the Council wishes most

cordially to thank Mr Sidney Scott for conducting the legal business involved.

(16) *The Oslo Congress.* The Fifth International Congress for Psychical Research was held at Oslo in the latter part of August on the invitation of the Norwegian S.P.R. Several members of the Society attended, and Mr. Whately Carington, Dr E. J. Dingwall and Mr Kenneth Richmond contributed papers. It is expected that the next Congress will be held in Budapest.

(17) *Library.* In addition to Members who consulted books in the Library, 71 Members borrowed books, the total number of books lent being 369 ; 83 books were borrowed by members of the public through the National Central Library, many more than last year ; and several Members availed themselves of the privilege of borrowing books on various subjects from other Libraries through the Society and the National Central Library.

(18) *Obituary.* During the past year the Society lost in Professor Charles Richet not only a Corresponding Member who had held the office of President, but one of the most distinguished scientists who has ever been actively engaged in Psychical Research. Among other losses to the Society by death we may mention Mr Giles T. Pilcher, Mr J. G. Bower, Mr P. W. A. Cort van der Linden, Major Clive Behrens, Mr Arthur H. Leaf, Mrs Champion de Crespigny and Major W. Rampling Rose.

(19) *Membership of the Society.* During the year 36 new Members and two Student Associates were elected ; one Associate became a Member, and one previous Associate who had resigned during the financial depression was re-elected. The total loss in numbers from deaths, resignations and other causes were 35 Members and 14 Associates, leaving a net increase of two Members and a decrease of 11 Associates. The total membership of the Society now stands at 734.

(20) *Publications.* Five Parts of *Proceedings* were published during the year : Part 139 in April, Part 140 in May, Part 141 in July, Part 142 in October, and Part 143 in December.

The Secretary's sales to the general public amounted to £96 17s. 3d. and to Members of the Society £72 0s. 4d., a considerable increase in both departments on the sales in the past few years. The returns from the Society's agent in the United States amounted, after deduction of commission, to £11 0s. 2d.

(21) *Meetings.* The following meetings have been held during the year :

31 Jan. " Dr Rhine's Recent Experiments in Extra-sensory Perception ", by Professor R. H. Thouless.

“ *Balance in hand, 31 December 1935 :*

On Current Account at Westminster Bank	£8 1 1
In Secretary's Hands	7 8 8
Due from Research Fund (1934)	79 4 6
	<hr/>
	94 14 3
	<hr/>
	£2,236 14 3

£2,236 14 3

RESEARCH FUND.

To Balance, 31 December 1934 :

On Deposit Account at Westminster Bank	£745 0 0
On Current Account at Westminster Bank	5 8 1
Due by General Fund to Deposit Account	9 0 0
	<hr/>
	£759 8 1
	<hr/>
Less Due to General Fund	79 4 6
	<hr/>
	£680 3 7

“ *Interest on Deposit Account*

3 6 8

“ *Sale of Gramophone Records*

1 14 0

By General Expenses

“ <i>Grant to Mr W. Whately Carrington in connexion with his Quantitative Study of Trance Personalities</i>	- - - -	£4 11 7
“ <i>Grant to Mr G. N. Tyrrell for research in Extra-sensory Perception</i>	- - - -	100 0 0
“ <i>Grant to Mr Kenneth Richmond for the Psychological Study of Mrs Leonard's Mediumship</i>	- - - -	50 0 0
	<hr/>	44 0 0
	<hr/>	£198 11 7

“ *Balance in Hand, 31 December 1935 :*

On Deposit Account at Westminster Bank	£554 0 0
On Current Account at Westminster Bank	11 8 9
In Secretary's Hands	0 8 5
	<hr/>
	£565 17 2
	<hr/>
Less Due to General Fund (1934)	79 4 6
	<hr/>
	486 12 8
	<hr/>
	£685 4 3

£685 4 3

ACCOUNTS OF RECEIPTS AND PAYMENTS FOR THE YEAR ENDED 31 DECEMBER 1935.

FREDERIC W. H. MYERS MEMORIAL FUND.

<i>To Balance, 31 December 1934 :</i>	
On Current Account at Westminster Bank Limited	£23 12 5
„ <i>Royalties</i>	0 4 10
„ <i>Interest on Investments</i>	35 16 11
	<u>£59 14 2</u>
<i>By Fourth Memorial Lecture :</i>	
Lecturer's Fee, Expenses of Meeting, etc.	£31 7 9
„ <i>Balance 31 December 1935 :</i>	
On Current Account at Westminster Bank Limited	28 6 5
	<u>£59 14 2</u>

MEMORANDUM OF ASSETS.

GENERAL FUND.		FREDERIC W. H. MYERS MEMORIAL FUND.	
£58 11	2 2½ % Annuities.	£750 0 0	5 % Conversion Stock 1944-64.
£92 19 0	2½ % Consolidated Stock.	£250 0 0	3½ % Conversion Stock 1961.
£219 8 7	3½ % Conversion Stock 1961.		
£588 17 8	3½ % War Stock 1952.		
£800 0 0	York Corporation 3% Redeemable Stock 1916-41.		
£250 0 0	New South Wales 5 % Stock 1935-55.		
£1,200 0 0	Nigeria Government 3% Inscribed Stock 1955.		
£1,161 0 0	London Midland and Scottish Railway Company 4% Debenture Stock.		
£562 0 0	London Midland and Scottish Railway Company 4% Preference Stock.		
£1,540 0 0	East Indian Railway Irredeemable Debenture Stock.		
£520 0 0	East Indian Railway Deferred Annuity Class " D ".		
300	South Staffordshire Tramways (Lessee) Co. Ltd. (in voluntary liquidation) Deferred Shares of 5s. each.		

We have examined the above Accounts and compared them with the Society's Cash Books, Receipt Books and Vouchers, and certify that they are in accordance therewith. We have also verified the investments of the General and Myers Memorial Funds as set forth in the above Statements.

70A Basinghall Street, London, E.C. 2, 23 January 1936.

HARTLEYS, WILKINS, AVERY & FLEW, Chartered Accountants.

ENDOWMENT FUND.

Owing to the illness of the Hon. Treasurer of the Endowment Fund, Mr J. G. Piddington, the Accounts of the Endowment Fund have not yet been audited. It is hoped to include them in the next number of the *Journal*.

- 27 Mar. "The Quantitative Study of Trance Personalities" (Continued), by Mr W. Whately Carington.
- * 1 May. Presidential Address by Professor C. D. Broad, Litt.D.
- 29 May. "An Example of the Evidence for Intention in Book-test Material", by Mr Kenneth Richmond.
- 26⁷ June. "Useful Ghosts", by Miss Ina Jephson.
- 25 Sept. "Divining for Water, Mineral and Oil", by Miss Evelyn M. Penrose.
- *30 Oct. Fourth F. W. H. Myers Memorial Lecture. "The Meaning of 'Survival'", by Mr. W. Whately Carington.
- 27 Nov. "The Mediumship of Carlos Mirabelli", by Mr Theodore Besterman.

CORRESPONDENCE

THE MEDIUMSHIP OF CARLOS MIRABELLI

To the Hon. Editor of THE JOURNAL

MADAM,—In Mr Theodore Besterman's Paper published in the *Journal* of December 1935 he states, in connection with "the blackboard rotation phenomenon", that "... the fact that Mirabelli allowed me on each occasion to arrange the *mise en scène* and *did not precipitate* himself on the board as it fell . . ."

I was permitted to see the film in question last summer, and have again more recently had it projected. It seems to me that Mr Besterman is mistaken. The second occasion of the blackboard rotation phenomenon is fairly clear in the film. It shows Mirabelli throwing himself upon the board just before the camera was stopped. No doubt Mr Besterman was unable to see this, as he was manipulating the camera at the time. The first occasion of the same phenomenon is very obscure on the film, but as far as I could judge Mirabelli is not then seen to touch the blackboard.

Yours, etc.,

CHARLES M. HOPE.

* General Meetings.

THE SCARCITY OF SPONTANEOUS CASES : AN APPEAL TO MEMBERS
FOR COLLABORATION

To the Hon. Editor of THE JOURNAL

MADAM,—In the January *Journal* Count Solovovo raises a most interesting question : as to why accounts of spontaneous cases get scarcer and scarcer in our pages.

There is no reason to suppose that such cases occur less frequently. As you point out in your editorial note, the response to the appeals made two years ago in the B.B.C. talks disproves that. Nor does our Society attach any less importance to them than formerly.

Count Solovovo's hypothesis No. 2 is the true one, namely " that accounts of them are being sent to the S.P.R. far more seldom than before". Having during the last ten years or so seen practically all the spontaneous cases, good, bad and indifferent, reported to the S.P.R., as and when they were reported, I can say most positively that during the last four or five years the decline in reports received has been strongly marked : it has more than once been the subject of comment in the Council's reports.

Two causes are, I think, mainly responsible for the decline. First, the fact that during these years the daily press has printed more than one collection of alleged spontaneous cases, unverified, uncorroborated, and, judged by S.P.R. standards, quite worthless. Not only have a number of possibly good cases been lost to the Society in this way, but among more intelligent newspaper readers the whole subject has been cheapened and reduced to the level of a " stunt". Secondly, public attention, so far as it has any concern with psychical research, has been diverted to other and more sensational matters.

All this is most deplorable. The remedy is, as you, Madam, suggest, more active collaboration by our members in collecting cases and reporting them to Tavistock Square. It must be emphasised that *the greatest possible promptness in reporting is essential*. Mr Herbert and the other officers of the Society will lose no time in following up a case as soon as it is reported to them. It would be particularly valuable if members living in the provinces would *keep a sharp eye on the local press*, which often prints accounts of apparitions, premonitions, hauntings, etc., and would at once cut out the accounts and post them to Mr Herbert, who will be glad to receive them and to make all necessary enquiries.

Yours, etc.,

W. H. SALTER (*Hon. Sec.*)

To the Hon Editor of THE JOURNAL

MADAM,—May I as one of the oldest Members of the Society for Psychical Research strongly endorse the remarks of Count Solovovo in his letter in the last *Journal*. Anyone referring to the earlier numbers of the *Journal* and comparing them with those of the last few years will at once appreciate how far the Society has drifted away from its original conception. What we require are facts and observations throwing light on a future life, and the connection of those who have passed away with those at present alive. Instead of this we get long letters of vague theory and futile mathematical calculations about the proportions of guess-work. These may possibly be suitable for mathematical or statistical societies, but have no bearing on the objects we founded the Society for half a century ago. I for one (and I believe there are many others) would be very glad to see the Society return to its own functions of investigation of Psychic Phenomena.

Yours truly,

HENRY N. RIDLEY, F.R.S.

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—The point made by Mr Drayton Thomas is, it seems to me, an important one, viz. that the possible *modus operandi* of the trance, and what may conceivably happen therein, might appreciably lengthen the reaction times. He continues: "Let him (the critic) go through the list of stimulus words again, whispering his replies to another person, who shall then voice them for him. The mean reaction time, as taken from the replies voiced by that second person, will be considerably longer than when the Professor spoke the reactions himself".

This agrees with the remark made in our Report:

"... It must be emphasized that, on theory, the reactions are not obtained under normal conditions, and a longer reaction time than usual must be allowed in consequence. On the spiritistic theory, a certain amount of time for 'transmission' must certainly be allowed" (*Bulletin A.P.I.*, pp. 41-42).

It seems to me that this point has been unduly slighted by those who have dealt with the reaction times, in the trance state, as compared with normal reactions. Yet, if the spiritistic theory is to be considered at all, surely this must be taken into account?

Yours, etc.,

HEREWARD CARRINGTON.

REVIEW

GERALDINE CUMMINS, *Beyond Human Personality*. Svo. Pp. 223.
(London : Ivor Nicholson & Watson, Ltd.) 1935. Price 6s. net.

This volume contains a further series of automatic scripts purporting to be communicated by the late F. W. H. Myers. The first series was published in 1932 under the title *The Road to Immortality*, a review of which appeared in the *Journal* for May 1933 (pp. 75-76). The reader is referred to that review for a general account of the scripts written by Miss Cummins, and purporting to come from Myers. They started in 1924, and have continued to the present time, the essays in the volume under review having been mostly written in 1933 and 1934.

For the student of psychical research the chief point of interest in the scripts is the claim of authorship. In the first place it must be noticed that they do not purport to be Myers' actual words. The medium is said to be an "interpreter" (see Introduction, p. 17), and her general stock of ideas and level of culture limit and control the mode of expression. Any comparison of style and vocabulary with those of Myers' published works would therefore be *ex hypothesi* valueless. The most one can look for is some parallelism of thought, and in assessing this opinions will differ. The earlier scripts published in *The Road to Immortality* contained several passages to which fairly close parallels (in thought, not style) can be found in *Human Personality*, and certain of Myers' poems. As Miss Cummins had not read any of Myers' works at the time the scripts were written, this was interesting, though the evidence fell far short of proof of the identity of the "communicator". Since the publication of the earlier volume, Miss Cummins has no doubt increased her normally acquired stock of information about Myers, but the "communicator" has not made use of this in the scripts under review. His lines of thought have been projected so far into the realms of speculation that they have lost almost every vestige of "clue-forming" material.

The authors referred to by name in these scripts are Job, St. Paul, Plato, Xenophon, Plotinus and "Madam Blavatsky". The last named is criticised for sponsoring an erroneous hypothesis (p. 136), but the reference to her is hardly convincing, considering that Myers was a member of the Committee of the S.P.R. which investigated the impostures of that lady, who was responsible for a good deal more than erroneous hypotheses.

In general it may be said that the script, while lacking in individual character, has gained in family likeness to many other scripts

purporting to describe the conditions of life after death. The usual topics recur, such as clothes in the next world (p. 37), the fourth dimension (p. 39), sex relationships (p. 41), the man with two wives (p. 42), reincarnation (p. 76), animals (p. 197). Close parallels in matter and style can be found in many other scripts purporting to come from different individuals, in which all the above subjects are dealt with in the same rather colourless fashion. (For instance, all the subjects mentioned are to be found in *Letters from a Living Dead Man*, Elsa Barker : Rider, 1915.)

In chapters entitled "Beyond Human Personality" and "Solar Man" there are astronomical speculations regarding life on the planets, the sun, and the stars, of a kind with which Myers was no doubt familiar in his lifetime. See, for instance, his poem "A Cosmic History" (*Collected Poems*, p. 384), and footnote thereto. But the treatment in the script is more akin to work in that field by other "script authors" than to anything Myers wrote himself. The same may be said of the chapter on "Prayer".

In editing a work of this kind, it is important to distinguish clearly between actual and "script" personalities and their respective statements. Miss E. B. Gibbes, who writes the Introduction, is careful in this respect in the sub-title, which speaks of a "Detailed Description of the Future Life purporting to be communicated by the late F. W. H. Myers", and in the closing paragraphs of the Introduction, where she speaks of the "alleged F. W. H. M.". But elsewhere she abandons caution and says, for instance, "Readers of *The Road to Immortality* will remember the account given by Frederic Myers of the World of Illusion" (p. 15). This is hardly fair to the memory of that great pioneer of psychical research, whose part in these scripts, even on the spiritist hypothesis, falls far short of authorship, in the ordinary sense of that term. G. W. L.

NOTES ON PERIODICALS

Revue Métapsychique.

July-August 1935.—M. Mangin-Balthazard contributes a paper on palmistry. M. Sainte Lague discusses the birth and death of the universe from an astronomical point of view.

September-October 1935.—Dr Osty, in an interesting paper, gives an account of a young girl, whom he calls "Mademoiselle Osaka", who has a remarkable faculty for remembering numbers. Beginning as a very backward child, she was inspired by a professional "lightning calculator" to develop her powers of figure memory to

a very high degree. The paper also discusses several other figure memorisers and phenomenal calculators.

November-December 1935.—A brief obituary notice and photograph of Professor Richet. A full-length notice will appear in the next number.

Professor Cazzamalli contributes the text of his paper read before the Oslo congress on his cerebral radiation experiments.

Mme de la Motte Carrel gives an account of dowsing experiments with maps and plans. She uses the pendulum method.

The book reviews include an appreciation of Lord Balfour's paper on the Willett scripts in *Proceedings*.

In the notes mention is made of Professor Broad's presidency of the S.P.R., and references made to his presidential address. A case of fire-walking is described; and several cases of premonition are related in the Correspondence.

Tijdschrift voor Parapsychologie.

September 1935.—J. M. J. Kooy contributes a long and interesting paper on prophetic dreams. One of the best of these is a dream of a railway accident, which was foreseen with accurate details of injuries, etc.

November 1935.—An account is given of the parapsychological congress at Oslo.

THE *JOURNAL* IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychological Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 25th March 1936, at 5.30 p.m.

WHEN A PAPER ON

The Structure of Communicator-Personality in
Leonard Sitzings

WILL BE READ BY

MR KENNETH RICHMOND

N.B.—*No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.*

FORTHCOMING MEETINGS

- April 29 "Further Research in Extra-Sensory Perception." By Mr G. N. M. Tyrrell.
- May 27 "The Flower Medium: Miss Hylda Lewis." By Mr J. Cecil Maby.

NOTICE

IN congratulating our distinguished Vice-President and former President, Prof. Gilbert Murray, on attaining the age of seventy years, we wish to call our members' attention to the fact that in each of the two volumes of essays compiled in honour of the occasion there appears an article on psychical research. To *Greek Poetry and Life* Prof. E. R. Dodds contributes an essay on "Telepathy and Clairvoyance in Classical Antiquity," and in *Essays in Honour of Gilbert Murray* is an article by Mrs Salter on "The Evidence for Telepathy."

Attention should also be called to the series of articles on psychical research which appeared in the *Spectator* (December 6th, 1935, to January 10th, 1936) inclusive. The contributors were the President, Dame Edith Lyttelton, Mr Whately Carington, Mr Richmond, Mr Saltmarsh and Mr Tyrrell. These articles have now been added to the Library.

NEW MEMBERS

Dart, Rev John L. C., Presbytère St Georges, 7 rue Auguste Vacquerie, Paris XVI.

Ripon, The Right Rev the Lord Bishop of, The Palace, Ripon.

MEETING OF THE COUNCIL

THE 337th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 29 January 1936, at 3.45 p.m., THE PRESIDENT in the Chair. There were also present: Miss Ina Jephson, Sir Lawrence Jones, Bart., The Hon Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Admiral the Hon A. C. Strutt, R.N., Rev C. Drayton Thomas and Miss Nea Walker; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary.

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

Two new Members were elected. Their names and addresses are given above.

Mr Kenneth Richmond and Dr H. W. C. Tenhaeff were elected Hon Associates of the Society for the year 1936.

PRIVATE MEETING

THE 139th Private Meeting of the Society was held on Wednesday, 29 January 1936, at 5.30 p.m., SIR LAWRENCE JONES, Bart., in the Chair.

MISS G. M. JOHNSON read a paper on "Psychical Research from a Sensitive's Point of View," which gave rise to an interesting discussion.

THOUGHTS ON THE REACTIONS OF "DORA" TO
THE WORD-TESTS

BY THE REV. W. S. IRVING

IT well may be that the calling of words, and the noting the reactions to those words of various Communicators, will prove to have opened out a new and fruitful field of investigation for students of Trance phenomena, quite *apart* from the reaction-timing and the taking down of the reproductions. On 13 September, 1934, at the conclusion of her first set of these tests, "Dora" remarked to me :

"Do you know, while I'm giving these I could give you quite a good sitting? They bring up so many memories. It's because I'm not trying to remember—I'm myself, they come to me. These provoke so much thought and memory, too good to be missed. This is so natural. I'm not trying to remember as a test—naturally. I think you'll find out, after, I've said quite a lot of personal things."

After her fourth series of word-tests, she added :

"It's been very interesting because it's awakened so many memories—so many memories."

The study of her five series of reaction words, taken as a whole, which I have since made, fully justifies, I think, her remarks ; and, further, the kind of reminiscences evoked by these more or less commonplace words differs from that of the reminiscences usually given.

In ordinary Trance sittings I have found that the material, through Mrs Leonard, has consisted mainly of reminiscences of the things that we did and thought important in bygone days, together with present-day tests, and knowledge shown of present, past, and occasionally even future happenings. The stimulating effect of these reaction words would seem to be to strike a quite different chord—to restore the mental attitude of the Communicator much as it was in past surroundings, as well as to bring back memory of trivial yet evidential details in those surroundings. I

<i>Number of Sitting</i>	<i>Test-word</i>	<i>Reaction</i>	<i>Additional comments by "Dora"</i>
1, 2, 3, 4, & 5	sail	work	Oh dear me! How I remember the wretched things! [Jumble-sales? W. S. I.]
1	life	quiet	
2	"	happy	
3 & 5	"	routine	
4	"	Parish	
1, 2, & 5	room	School	
3 & 4	"	sewing	
2, 3, 4	demand	attention	
1	do	Parish	Constant doings.
2	"	sewing	
3	"	work	
4	"	rounds	Do the rounds.
1, 2, 3, 4, 5	call	visits	I hated them.
1, 3	poor	Parish	
2	"	treatment	I won't say, "collection"
1	light	lamp	
1	annoy	Saturday	We wanted to be quiet on Saturdays. Didn't want people on Saturdays.
2	"	you	
3, 4	"	us	I didn't give, "callers".
5	"	Parish	
1	select	prizes	
3, 4, 5	"	Committee	I didn't like them.
1, 2, 4, 5	garden	tidy	Always trying to do it in old days.
1	dirty	homes	I used to say that!
2	draw	bazaar	The " <i>draw</i> " in raffles.
3	eat	peace	What we used to like to do!
2	good	evening	We loved the evenings you didn't have to go out.
3	short	stipend	
4	"	holiday	
3	small	stipend	
5	read	notices	
5	judge	competitions	
1	chance	show	Of a show.
2	"	London	
3	"	opportunity	
4	"	away	To get away.
1, 2, 3, 4, 5	roof	mend	

can, perhaps, best illustrate these points by picking out examples from the reactions made by "Dora", which seem to have a bearing on a particular subject, and to show a connected line of thought. For instance, the following reactions dealing with the subject of *Parochial life* seem to be singularly appropriate to what must have been her point of view when in a pessimistic mood, needing a change.

To anyone acquainted with the life of the Vicar's wife in an out-of-the-way and very poor country Parish where the Vicar's stipend is small, the Vicarage and garden have to be kept in repair, and there are only oil lamps for light at the Vicarage, the above speaks for itself. In this particular case, the Vicar's wife, Dora, was accustomed before marriage to a life of considerable gaiety. There has been nothing even approaching this line of thought at any of my previous sittings. Next, let us take the subject of *holidays* which was introduced above by the words "short" and "chance":

1, 2, 3, 4, 5	lake	Windermere	You ought to have a memory about it
1, 2, 4, 5	town	Scarborough	
1, 2, 3, 4	valley	near	
5	"	holiday	
3	fetch	luggage	That reminds me of "valley".
1	rage	torrent	
2	"	waterfall	Couldn't think of the word!
3	load	station	We often had that! I'm sure you must remember! Makes me laugh, that does!
1, 2, 3, 4, 5	abroad	holiday	Talked about it. My word!
1	voyage	channel	
2, 4, 5	"	island	
3	"	continent	
2	fish	pond	
1, 2, 3, 4, 5	ride	tram	Plenty of that.
2	drive	station	
5	"	home	
3	dirty	carriage	I used to say that.

At the end of the 4th Series, Dora added, “. . . Do you remember a holiday we had where was a beautiful valley? You could look right down the side of it—lovely place. Do you remember we nearly got somewhere else instead of going there?” (Do you mean Scarborough?) “No. I’m thinking of somewhere else. We’d very nearly gone to another place again. Not in that part. There was a ‘C’ connected with it. . . .”

It is not, I think, difficult to trace a clearly connected line of thought in some of the above reactions: Scarborough is a town that has pleasant memories for my wife and myself. She lived there for a time as a child. We spent part of at least a dozen summer holidays there, and for a short time I was Assistant Curate at a Church in that town. In itself, however, the mention of Scarborough must be said to have a diminished value here, in that it has already been mentioned by Dora at past Sittings on several occasions; but the interest, here, is not in the mention of Scarborough, but in the line of thought that would seem to be started thereby. Scarborough is divided into two parts by the *valley*, and, over the valley runs a toll bridge leading from north to south of the town. Arriving at Scarborough, on holiday, we regularly crossed the valley bridge with our luggage to get to the House of Rest where we generally stayed. The same occurred during the time we lived at Scarborough, as we lived on the south cliff near the valley. My wife, also, when a child, lived in a house close to the valley. But this is not all: remembrance of that valley seems to have recalled to Dora’s mind a holiday near another and somewhat similar valley, where there is a far deeper view to look down upon—the valley beneath Clifton Suspension Bridge which crosses the Avon gorge. In, I think, 1917 we had the chance of slipping away for a few days’ holiday. We did not decide where to go till we got as far as Gloucester and could learn from the newspapers there whether Birmingham or Bristol had the best “Shows” on at the theatres. We chose Bristol, staying at Clifton, and the reference to our nearly having gone to another place seems pretty clear; though, “again” is not appropriate. Bristol and Clifton were quite well known to my wife, however, apart from that particular holiday, as we stayed several times at Clifton. It is not improbable that the reaction “pond” to the word “fish” may be a further reference to the town, as *Fishponds*, which is a part of Bristol, was a familiar name to my wife, and the reaction would, I think, be a natural one.

Not much need be said about the other “holiday” words, but “Windermere” is a good reaction to “lake”, as in 1901 and 1902

we stayed for a day or two at an hotel at the head of Windermere : some of the other lakes would not have been applicable. "Holiday" fits in all right with "abroad" when it is coupled with the remark "Talked about it!" Although we were never abroad together, we had talked about a holiday in Paris the following year—a sister of my wife's was then living there. "Voyage"—"channel" is good—we crossed the Bristol Channel from Weston to Cardiff about a month before my wife's last illness. And "voyage"—"island" is reminiscent of trips we made from Southsea to the Isle of Wight.

Turning to the subject of the giving of the reaction words : Mrs Leonard (prepared)¹ seemed to be but mildly interested. Feda and "Dora", however, were both bubbling over with reminiscences, and had to be continually checked lest the power should give out before the end. This was a very great pity as their sayings seemed to be of value. In reproductions, Feda seemed to have no special interest ; but "Dora" was intensely anxious to remember, and kept saying, "Don't tell me ! Don't tell me !"

CORRESPONDENCE

THE STUDY OF THE PSYCHOLOGICAL ASPECTS OF MRS WILLETT'S MEDIUMSHIP

To the Hon. Editor of THE JOURNAL

MADAM,—I think that everyone will agree that one of the outstanding events in the recent history of psychical research has been the publication of Lord Balfour's "Study of the Psychological Aspects of Mrs Willett's Mediumship", *Proc.* XLIII.

A prolonged and careful study of this report has led me to see that there are involved therein matters of quite fundamental importance not only for psychical research, but also for psychology and metaphysics. To refuse to accept the views put forward with such cogency of argument and supported by such weight of evidence is difficult, yet acceptance entails for many of us a radical readjustment of our basic ideas.

In these circumstances I am emboldened to put forward a plea for further enlightenment on several points which remain obscure to me. May I say at once that the questions which I ask are framed in no spirit of criticism, but solely with a sincere desire for information.

¹ It will be remembered that, for the purpose of these word-reaction tests Mr Whately Carington distinguishes between words put to Mrs Leonard in a 'prepared' state, *i.e.* just before giving a sitting ; and words put to her apart from sittings altogether, *i.e.* in a 'normal' state.—HON. ED.

The matter of paramount importance is the monadic theory of personality. In this connection I have read Lord Balfour's Presidential Address, *Proc.* XIX, and his article in the *Hibbert Journal*, April 1913. I have also refreshed my memory of Prof. McDougall's Presidential Address, *Proc.* XXXI, and I am familiar with the relevant passages in his *Outline of Abnormal Psychology*.

In spite of a careful study of all these sources of information, I still find myself rather puzzled over many points of the theory. As I understand it the human personality is regarded as an association of selves, composed of a dominant monad at the head of an hierarchy of subordinate monads, interacting with each other and intercommunicating by means of telepathy. Prof. McDougall employs the analogy of an army; the dominant monad is represented by the Commander-in-Chief, receiving reports from and issuing instructions to his subordinate officers.

I was tempted at first to think of the personality as being analogous to the entire army considered as an unity in much the same way as a colonial organism is an unity, but we are told that the real self is the dominant monad. Prof. McDougall says: "I who consciously address you am only one among several selves or egos which my organism, my person, comprises. I am only the dominant member of a society, an association of similar members" (*Proc.* XXXI, page 111). Lord Balfour also says: "My own instinctive conviction is that my true self is the 'me as I know myself', and that it will be as this *me*, with its formed character and stored memories, that I shall survive, if survival there be. Other selves co-conscious with 'me as I know myself' may also survive" (*Proc.* XLIII, page 275). The "me as I know myself" is then the dominant monad. Who, then, are the subordinate monads, the other conscious selves? Are they also *me*, but not "me as I know myself"? As they are selves, presumably they are self-conscious, and therefore know themselves as someone. In cases of dual personality one of these selves apparently assumes a personal identity different from the "me as I know myself", and in some instances, such as Sally in the Beauchamp case, claims to have had a separate personal existence long before the date of emergence.

Further, does the theory assume that these subordinate selves are capable of existing, in any circumstances, apart from the association? Are the subordinate monads permanent members of the association, or is there a process of change therein analogous to that of the cells composing a living organism in virtue of which, though the organism maintains its identity, individual cells may decay and vanish, being replaced by other similar cells which carry

on their functions? If so, where do they go and whence do the new ones come? Prof. McDougall speaks of a "budding" in his Presidential Address: are we to suppose that the subordinate monads are budded off the dominant monad, either by a process analogous to propagation by simple fission or like suckers thrown off by a plant, and that this process goes on throughout the whole of life, or that it is confined to infancy or some period in the earlier history of the individual? The dominant monad, being a complete self, possesses cognitive and affective-volitional faculties, it has its own proper train of memory. Does it perceive the external world directly? Can it employ the entire mechanism of the physical body, including the sense organs, or does it make contact with its external environment only by deputy through the subordinate monads?

These latter, being also selves, presumably enjoy possession of the full complement of mental faculties: do they also have full run of the physical organism or are they specialised as to function? I rather gather from Prof. McDougall's account that the latter view is correct, so that the subordinate monads should, strictly speaking, be considered as less than complete selves, ranging from those in the hierarchy nearest to the dominant, which would be nearly complete, down to the lowest orders whose specialised function is, say, some simple reflex.

There are hosts of other questions which I should like to ask, but space forbids.

Before leaving the matter, however, there is one point which I must raise. Lord Balfour rightly insists that inter-monadic telepathy is essential to the theory, and that without it it cannot stand. In consequence we are bound to give up the view, held by Myers, that telepathy is a function of the subliminal alone and is not shared by the supra-liminal personality. Presumably the same argument applies to telaesthesia.

Now it seems to me that the weight of the evidence points to there being a certain amount of dissociation in the large majority of cases of supernormal mental phenomena, so that although the dominant monad may possess the faculties to some degree, they are in the main exercised by selves other than the "me as I know myself". Telepathy and telaesthesia, in their more advanced forms, are a heightening of mental activity, and as such it would surely be strange to find them in the possession of the subordinate monads to a greater degree than in the dominant.

I, very diffidently, venture to suggest the following view. The dominant monad, the "me as I know myself", occupies a position

not at the head of the hierarchy, but somewhere about the middle ; ranging below it are the subordinate monads, each specialising in its proper function and each endowed with powers of telepathy and possibly telaesthesia in appropriate degree ; ranging above it are other selves as superior to the dominant as the dominant is to the subordinates : these would possess telepathic and telaesthetic powers in a more perfect and complete degree than does the dominant ; finally we reach the head monad of all, whom we might call, with Myers, the transcendental self.

To borrow Prof. McDougall's analogy, while the dominant monad is represented by the Commander-in-Chief located at G.H.Q., these superior monads would be represented by the Supreme Army Council, the Cabinet, and finally by the Supreme Head of the State : they would be located away from the seat of war, and would function mainly in what Myers calls the metetherial environment.

On such a view evolution would consist in the gradual shifting of the "me as I know myself", higher up the scale, and, if there be survival, this would be simply a definite step up, possibly accompanied by a shedding of some of the lowest of the subordinate monads as being no longer required.

Lord Balfour warns us that difficulties arise in connection with the problem of "the one and the many", and these apply with equal force to the monadic as to the stratum theory. It seems to me that the scripts themselves afford a hint of a possible solution.

Mrs Willett is aware of presences and of their characters, she can distinguish between different communicators apart from any visual or auditory signs.

Gurney says : " I want you to see the passage of thought, not ocular or aural " (page 93).

I suggest that the knowledge acquired by telepathy and telaesthesia is sometimes of the nature of pure undifferentiated cognition, that is to say, it is neither visual, auditory, tactual, etc., but simply direct knowledge of reality, which has to be translated into the language of sense perception before it can be made intelligible to the supraliminal.

It may be that this is what Gurney refers to when he speaks of the "weaving" process and of crystallization, and says : " It is a dangerous weapon, yet we can't do without it " (page 234).

I gather that Lord Balfour himself takes this view, for he says on page 93 : " I incline to the view that all visualisations of communicators, and *a fortiori* all fully developed hallucinatory phantasms are to be regarded as subjective constructions—symbolic, it may be, of some objective reality, but still subjective constructions

—except in so far as there is evidence for attributing to a communicator a definite intention to transmit a pictorial image of himself.”

If this be so it appears that the cognitive faculty of some at least of the monads transcends the limitations of sensory perception, and is emancipated from the conditions of space and time as they apply to our supraliminal minds.

On page 182 occurs the following: “And there isn’t any time or place. . . . It’s like every single thing and time and thought and everything brought down to a single point”. In another place, page 72: “How nothing time is”.

Now if it be possible that the categories of time and place be transcended, it is only one step further to the transcendence of the final fundamental category of all, viz., that of number and sameness and difference.

Myers speaks of the “inconceivable oneness of souls”, and long before him the Vedantic philosophers taught the doctrine of Maya and how the self on attaining enlightenment learns at last the truth of the mystic phrase “Tat tvam asi”—Thou art That—and the Atman realises its identity with Brahman (neuter).

Thus the difficulties concerning the one and the many may be solved by transcending those categories. The extracts from the scripts given on pages 215-218 commencing with the words, “The one remains, the many fall and pass” and ending “When I am everything, you know, and everything is me”, contain abundant illustrations of this idea.

There is one further psychological point which I must mention. In the account of the process of mutual selection given in the scripts it would seem on the face of it that the communicator holds the somewhat naïve “storehouse” theory of memory. I do not suppose for a moment that the living Gurney held this view, nor, if it be really his surviving spirit who communicates, that the words used are anything more than a colloquial *façon de parler*, but the difficult question as to where and what are memories when they are not actually being remembered is raised in acute form.

This matter was discussed by Prof. Broad in his Presidential Address, but, with all respect, I find myself unable to accept his proposed solution. It seems to me that any trace theory of memory would require so complicated a set of psychological mechanisms to make it workable, mechanisms, moreover, which have to be postulated for that sole purpose and for the existence of which there is no independent evidence, that I feel very chary of acceptance. Mr Russell’s theory of mnemonic causation commends itself to me even less: it can hardly be considered a satisfactory solution of a diffi-

culty to invent *ad hoc* an entirely new mode of causation which, after all, amounts to little more than coining a fresh name for that which it is sought to explain.

I should be inclined rather to seek for a solution on the lines of a transcendence of the time condition and apply this conception to the realist theory of memory. The scripts themselves suggest this, as in the passage quoted above, "How nothing time is".

We should then have to imagine some part of the mind, or perhaps I ought to say some superior monad, who could grasp the entire past history of the personality in a single conspectus much in the same way as we grasp the whole of a single specious present.

I fear that this letter has attained to an unseasonable length: my only excuse must be my intense desire for further light on these most important topics, and the feeling that were I to raise all the questions which I should like to ask it would be at least twice as long.

Yours faithfully,

H. F. SALTMARSH

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—Remarkable as the proxy ease extending over eleven sittings with Mrs Osborne Leonard certainly is (*Proc. Dec.*, 1935), I hesitate nevertheless to share the Reverend C. Drayton Thomas' conviction that Bobbie Newlove and his friends in the Beyond gave the messages. The author seems to undervalue the possibilities of mind-reading (*Gedankenlesen*), especially the faculty of certain mediums to avail themselves of the unconscious of the sitter as bridge to the thoughts of distant and unknown persons (*télépathie à trois*). I miss therefore the due consideration of Jaek's, Bobbie's friend's, person as source of telepathic information on the subject, and do not quite understand why Jaek should be "certainly a most unpromising and unlikely source, etc." (p. 502), if I were not obliged to assume a preconceived opinion of Mr Drayton Thomas. It may also be imagined, *perhaps even ascertained*, that the boy spoke with other persons about the death of his friend Bobbie and mentioned on this occasion that he and Bobbie had played with the water on the Heights. Is it really out of the question that someone (for instance, a medical man) surmised thereupon, perhaps in the presence of the boy, a connexion between the two facts, although the author asserts so apodictically that no one on earth had the least suspicion that the throat affection was traceable to the contaminated water

(pp. 501-2 and 508) ? Likewise, I should not be so rash in affirming that members of the "gang" would have no idea that Bobbie hurt himself by playing with the water (p. 502). One could even conjecture that the suggestion, "which had never occurred to anyone" (p. 507) and which related to the probable cause of the child's death, emerged only in the Leonard-Feda mind.

Wishing to strain the telepathic hypothesis (including "absence-telepathy" and mind-reading) to the utmost, I should assume that Bobbie's people constituted the link between medium and sitter on the one side and the persons knowing about the existence of the pipes on the other side. As most of the psychical researchers believe that the medial utterances are based upon tapping the actual or latent thoughts of present or absent persons, I am rather astonished to read on p. 502 : "We have no record of long and detailed messages being conveyed from one person to another by telepathy. Whether spontaneous or experimental, telepathy is always fragmentary." Or does the author strictly distinguish between telepathy in the narrower sense of the word and mind-reading ? Then he may be right. A further remark, relating to the third sitting (p. 455) : "Sw . . . L ; these letters present an unsolved puzzle." Could not they mean : *Swamp-land* ?

I surely do not deny that the spiritualistic interpretation of this highly interesting and striking case seems the most obvious and simple, but the most simple explanation is not always the right one ; truth is very often not simple.—I beg to be excused for my bad English : I hope you will find, however, what I intended to say.

Believe me,

Yours, etc.,

BARON ALFRED DE WINTERSTEIN

To the Editor of THE JOURNAL

MADAM.—Dr Baron Alfred von Winterstein rightly points out that I place little value on the hypothesis of mind-reading, especially as applied to cases where the persons, whose minds or memories are supposed to be tapped, were distant and unknown to the medium. One does not deny the possibility, but I am unable to recall creditably recorded instances where the information produced was demonstrably by that method and by no other method.

Is there on record any case where a medium's exursive activity can be shown to have obtained information comparable in accuracy and amount to that found in the Newlove case ?

Should it be shown that some mediums can tap a sitter's memories,

it would yet remain to be proved that the same thing could be done with the memory of a distant person who was unknown to the medium.

I am the more inclined to disregard the hypothetical action of distant telepathy in the Newlove case because I have repeatedly observed instances where information was given by a medium under conditions precluding telepathy from any minds on earth. One cannot enlarge on this in a letter, but I would refer to my publications, *Some New Evidence for Human Survival* (especially Part II) and *Life Beyond Death with Evidence* (chapters xi and xiii).

Yours, etc.,

C. DRAYTON THOMAS

[Regarding Mr Drayton Thomas's remark, "Should it be shown that some mediums can tap a sitter's memories," there can be little doubt that all mediums do this, and to a considerable extent.—HON. ED.]

THE MEDIUMSHIP OF MIRABELLI

(*Translation*)

To the HON. EDITOR

MADAM,—The rather mild judgment of Mr Besterman (who is such an experienced expert) concerning Mirabelli's metapsychic performances, may very well be based upon tactical considerations. Mr Besterman knows that demonstration sittings cannot prove much, since he can have no influence upon the method followed. Perhaps he wants, by means of a very little praise, to pave the way for later, laboratory, sittings, which up to the present have not been obtainable.

On the other hand Mr Besterman's complete refusal of Mirabelli's "scripts" is to be wholly approved. "William Crookes" can't spell his own name correctly! It is just as bad with "Cesar Lombroso," whose name the "Academia de Estudios Psicicos" has even added to its list, and who has authorized the publication of the Mirabelli scripts. Now, Lombroso was a Jew. But he makes Mirabelli write automatically: "Jesus hear and help you," etc. It would appear that on the other side not only can one have lessons in various languages, but also one can change one's beliefs.

A similar case of *post mortem* change of belief I wrote about in the *Zeitschrift für Parapsychologie* of Nov. 1931 ("Sensitive or Medium"). The American spiritist M. Arthur Ford, had given, in a private sitting in Berlin (April 27, 1928), a communication to a sitter from his mother, and she expressly revealed herself as a very pious

Christian and concluded each sitting with a spiritist propaganda speech. But she was, as can be proved, a Jewess.

Such spiritistic absurdities as this have apparently been frequently produced, but they can't always have been so well established as in those two analogous cases—that of Mirabelli and that of Ford.

Yours very truly,

(DR) CARL BRUCK

REVIEW.

PIERRE-EMILE CORNILLIER, *The Prediction of the Future*. Translated by L. E. Eeman. (Author-Partner Press, Ltd.) 5s. nett.

This is a translation of M. Cornillier's *La Prédiction de l'Avenir* (Nouvelle Théorie expérimentale). The author, who is a firm believer in the Spirit Hypothesis, gives numerous instances of prophetic utterances by "mediums". One prophecy, relating to the President of the United States, has not been made public, but has been deposited in the Westminster Bank. If it is realized, the deposited document is to be opened in the presence of witnesses, some of whom the Society is to be asked to nominate. The translator has done his work well, and the book is excellently printed. It would, however, be improved by an index.

C. V. C. H.

NOTES ON PERIODICALS

International Institute for Psychological Research, BULLETIN I.

This is the first number of a series of bulletins to be published in collaboration with the American Psychical Institute of New York. It contains a useful list of references to published accounts of queer happenings which have been attributed to Poltergeist manifestations. The list, which extends from A.D. 530 to the present day, has been compiled by Dr Hereward Carrington.

In an introduction, Dr Carrington expresses surprise at the cautious attitude towards accepting these phenomena which is adopted by some students, and states emphatically his personal opinion that they are often genuine. He goes on to describe the adventures which befell the Rev. Dr Eliakim Phelps, of Stratford, Connecticut, in 1850 and 1851. His account of this famous case is an interesting one; but it seems a pity to introduce into a quasi-scientific publication the footnote which appears on page 8. In this, the author adduces Dr Phelps' family motto—*Veritas sine Timors*—as evidence of his credibility.

The rest of the bulletin contains a note by Dr Nandor Fodor on a supposed psychic manifestation in Spain which occurred in 1934.

The bulletin is excellently printed, but it would be an advantage, for purposes of reference, if the date of publication were added to subsequent numbers.

International Institute for Psychological Research, BULLETIN II.

This contains an account, written by Dr Fodor, of a series of sittings with the medium Lajos Pap held at the Institute. It is illustrated with diagrams and photographs. A large number of "phenomena" were observed, mostly "apports". The author concludes that "none of the phenomena . . . can be considered supernormal; some of them were definitely fraudulent, others highly suspicious". Dr Fodor and his assistants at the Institute are to be congratulated on a highly efficient piece of research.

Zeitschrift für Metapsychische Forschung.

August 1935.—Professor M. Verweyen describes an investigation of two dogs which, it is alleged, are able to convey information by signals. They seem to be able to answer questions, and to calculate. The author discusses various possible explanations, but arrives at no conclusion.

Dr von Chengery gives an account of a sitting with the medium Lajos Pap, at which documents were received as apports. The sitting was held in the author's laboratory under, it is stated, the strictest conditions.

Dr Schröder discusses the Indian Mango Trick, which he states that he has seen performed.

September 1935.—A further account of the sittings with Pap in Buda Pest. Pap brought an action against Dr Chengery for the return of the apported documents. This was decided in Pap's favour.

Ernest Bozzano describes some violent poltergeist phenomena, and gives an account of a sitting with Kluski.

December 1935.—Professor Kasnacich contributes a paper on Mysticism and Occultism in German Literature.

Dr Baron Alfred von Winterstein reviews in "The Visions of a French Clairvoyante" [Mme Jeanne Peyroulet] Dr Osty's book *La Connaissance Supranormale*.

Joseph Peter contributes a paper on the mediumship of Kate Fox.

ERRATUM.— p. 160, Table VI, HIGH SCORES.

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JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 29th April 1936, at 5.30 p.m.

WHEN A PAPER ON

Further Research in Extra-Sensory Perception

WILL BE READ BY

MR G. N. M. TYRRELL

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

FORTHCOMING MEETING

May 27 "The Flower Medium : Miss Hylda Lewis." By Mr J. Cecil Maby.

NEW MEMBERS

Gilbert, A. Stuart, I.C.S. (retired), 7 Rue Jean du Bellay, Paris, IV. Librarian, Commonwealth Parliamentary Library, Canberra, F.C.I., Australia.

Strutt, Hon. John A., 18 Hyde Park Square, London, W. 2.

Student Associate

Dryer, I. E., King's College, Cambridge.

MEETINGS OF THE COUNCIL

THE 338th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 26 February 1936, at 2.45 p.m., THE PRESIDENT in the Chair. There were also present : Sir Ernest Bennett, M.P., Mr Gerald Heard, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, R.N., the Rev. C. Drayton Thomas and Miss Nea Walker ; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary.

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

Three new Members and one Student Associate were elected. Their names and addresses are given above.

THE 339th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, immediately after the Annual General Meeting, SIR LAWRENCE JONES, Bart., and later the PRESIDENT, in the Chair. There were also present : The Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, R.N., the Rev. C. Drayton Thomas and Miss Nea Walker ; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary.

Professor C. D. Broad was re-elected President for the year 1936.

Mr W. H. Salter was re-elected Hon. Secretary, Admiral the Hon. A. C. Strutt, Hon. Treasurer, Mrs W. H. Salter, Hon. Editor of the *Proceedings*, Miss Nea Walker, Hon. Editor of the *Journal*, and Miss Kathleen Watkins, Hon. Librarian.

Committees were elected as follows :

Committee of Reference and Publication : The Earl of Balfour, Professor C. D. Broad, Mr Whately Carington, Sir Oliver Lodge,

Dr T. W. Mitchell, Mr J. G. Piddington, Mr W. H. Salter, and Mr H. F. Saltmarsh.

House and Finance Committee : Miss Ina Jephson, Mr G. W. Lambert, Mr W. H. Salter and Admiral the Hon. A. C. Strutt.

Research Committee : Mr Whately Carington, Mr Oliver Gatty, Mr Gerald Heard, the Hon. Mrs Alfred Lyttelton, G.B.E., Dr T. W. Mitchell, and Mrs W. H. Salter.

Library Committee : Professor E. R. Dodds, Sir Lawrence Jones, Bart., Admiral the Hon. A. C. Strutt and the Rev. C. Drayton Thomas ; also the Secretary, *ex officio*.

Corresponding Members and Hon. Associates were elected for the year 1936 as follows :

Corresponding Members : Professor Henri Bergson, President Nicholas M. Butler, Dr Max Dessoir, Professor Dr S. Freud, Professor Pierre Janet, Dr C. G. Jung, Count Carl von Klinckowstroem, M. Maurice Maeterlinck, Professor T. K. Oesterreich, Dr Eugène Osty, Dr Rudolph Tischner, Carl Vett and Dr Elwood Worcester.

Honorary Associates : Miss H. A. Dallas, Rev. A. T. Fryer, David Gow, J. Arthur Hill, Professor R. F. A. Hoernlé, Rev. W. S. Irving, Professor J. H. Muirhead, Mr Kenneth Richmond, Professor Charles Sage, Miss F. Melian Stawell, Dr A. Tanagras, Dr H. W. C. Tenhaeff, Professor R. H. Thouless and Dr Th. Wereide.

ANNUAL GENERAL MEETING

THE Annual General Meeting of Members of the Society was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 26 February 1936, at 3.30 p.m., THE PRESIDENT in the Chair.

The following members were present : Mrs J. H. Ball, Mr F. Bligh Bond, Miss Carruthers, Miss M. C. Crosfield, Lady Dewar, Dr E. J. Dingwall, Mr G. W. Fisk, Mr Stuart Gilbert, Mrs E. W. Goossens, Lt.-Colonel H. F. Grant-Suttie, Mr C. V. C. Herbert, Mr Richard A. Howden, Miss G. M. Johnson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mrs Laurie Magnus, Mr W. H. Salter, Mrs W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, R.N., the Rev. C. Drayton Thomas, Mr Gilbert M. Thomas, Mr G. N. M. Tyrrell, Mrs Vatcher, Mrs Wagstaff, Miss Nea Walker ; and Lady Clwyd, Miss Dickinson, Mr Kenneth Richmond, Mrs Wilkins and Mr C. F. Wilkins (Associates).

The Secretary having read the Notice convening the Meeting, the President called upon the Hon. Treasurer, Admiral the Hon. A. C. Strutt, to make his financial statement.

Admiral Strutt, in presenting the accounts for the year, said that at first sight the Society appeared to have paid its way, seeing that it started the year with a nominal balance of £39 15s. 4d., and ended it with one of £94 14s. 3d. But this was not the case, because it had received £350 on the sale of certain holdings of stock, and had re-invested only £250. The balance of £100 had not been re-invested in view of the dilapidations with which the Society was faced on the surrender of the lease; these however had not yet been carried out or paid for. The rent, hitherto £175, had been raised to £240, and with this increase of expenditure there was a continual tendency towards a decrease of income, caused by the conversions of the Society's holdings to a lower rate of interest. The acquisition of new members was the most obvious method of increasing income, and he hoped that existing members would do all they could to persuade new members to join and so help the Society.

The President then invited questions on the Financial Statement and Annual Report. Dr Dingwall congratulated the Council on giving grants for particular pieces of work done by members, which he said was a much better policy than a heavy expenditure on physical mediums. He hoped they would continue on these lines.

The Hon. Secretary, Mr W. H. Salter, thanked Dr Dingwall for his remarks, with which he was in general agreement. The Council could not however give an absolute pledge not to pursue any particular line of research. As regards dilapidations, Mr Salter mentioned that several years ago the Society had put by £150 out of income towards the liability for dilapidations.

The President said that, before putting the Report to the vote, he would like to inform the Society that the Council was preparing shortly to make an appeal for funds to the public. He hoped that by that means the Society might get money, not only for income but possibly endowment, and the appeal might also result in an increase of members. He referred to the public interest aroused by the broadcast talks in 1934 and to the more recent articles in *The Spectator* by various members of the Society. He had heard from various booksellers that they had enquiries asking for the *Proceedings*, and he noticed that there had been a considerable increase in the Society's sale of publications last year.

The President then moved that the Report and Financial Statement be adopted, and the resolution was carried unanimously.

The President announced that there were no candidates for election to membership of the Council other than the six members who retired by rotation, and he accordingly declared the following six members re-elected: The Rt. Hon. The Earl of Balfour, Sir

Ernest Bennett, M.P., Sir Lawrence Jones, Bart., Sir Oliver Lodge, F.R.S., Professor W. McDougall, F.R.S., and Mr S. G. Soal.

Messrs. Hartleys, Wilkins, Avery & Flew were re-elected Auditors for the forthcoming year.

Before closing the meeting, the President spoke of the very serious loss that the Society had sustained through the deaths of Mrs Sidgwick and the Hon. Everard Feilding. It was hardly necessary, he thought, to say much about Mrs Sidgwick, one of the oldest members and one of the founders of the Society. Any one who had read the *Proceedings* from the beginning would know the admirable and first-rate work she did. She had a most extraordinary combination of qualities: a balance of judgment, neither going to an excess of scepticism on the one hand, nor superstition on the other, invaluable qualities which were extremely necessary in psychical research. While it was unnecessary to praise the various reports she had written in many volumes of *Proceedings*, he might mention that he had been particularly struck in re-reading, shortly before her death, her examination of cases of telepathy: "Phantasms of the Living", in *Proceedings*, Vol. XXXIII.

The President spoke also of the valuable work done for the Society by Mr Feilding. He asked the members present to stand, to show their deep sympathy with Lord Balfour and the other members of Mrs Sidgwick's family, and with Mrs Feilding.

After the Annual General Meeting, MR C. V. C. HERBERT gave an account of his "Experiences with Continental Mediums last Autumn". The President was in the Chair.

This will be printed later.

CASE

L. 1307 INTIMATION OF FATHER'S ILLNESS

The following letter from Mrs Shortreed, addressed to the Secretary of the Society, was handed to Mr Herbert on 17th December, 1935:

"I had occasion to look for a book on telepathy at our public library here and chose this one: *Evidence for Telepathy*, by Mrs W. H. Salter. My story is simply this:

"My father has had a weak heart for some time back, but has been going about as usual, with no immediate danger, as he was very careful. I sleep alone usually, but on Tuesday night, the 3rd of December, my daughter was in bed beside me, and at 12 o'clock¹ I

¹ In answer to an inquiry, Mrs Shortreed writes: "I awoke with my clock chiming twelve strokes, and thought for the moment the clock had awakened me; and again the half hour and 1 o'clock struck ere I settled down again."

ENDOWMENT FUND FOR PSYCHICAL RESEARCH I

For the Year ended 31st December, 1935.

<i>To Balance in Hand 31 December 1934 :</i>				
At Lloyds Bank Limited on Current Account - - - - -	£223	3	4	
<i>Interest on Investments - - - - -</i>	392	6	1	
	£615	9	5	
				£30 4 2
				585 5 3
				£615 9 5

By Investigation Officer's Salary, 25 December 1934 to 22 January 1935 - - - - -

Balance in Hand 31 December 1935 at Lloyds Bank Limited on Current Account - - - - -

MEMORANDUM OF ASSETS.

£1,400	0	0	4	% Consolidated Stock.
£908	0	11	India 3½% Stock 1931.	
£550	0	0	India 4½% Stock 1958-68.	
£161	11	6	New South Wales 5% Stock 1935-55.	
£1,797	0	0	London and North Eastern Railway Co. 4% Debenture Stock.	
£1,055	0	0	Great Western Railway Co. 5% Rent Charge Stock.	
£800	0	0	Great Western Railway Co. 5% Guaranteed Stock.	
£2,258	0	0	London, Midland and Scottish Railway Co. 4% Preference Stock.	
£514	0	0	London Passenger Transport Board 4½% "T.P.A." Stock.	
£260	0	0	East India Railway 3½% Debenture Stock.	
£1,260	0	0	East India Railway 4½% Irredeemable Debenture Stock.	
£700	0	0	Great Indian Peninsular Railway 4% Debenture Stock.	

We have examined the above Account and compared it with the Endowment Fund Pass Book and certify that it is in accordance therewith. We have also verified the Investments of the Endowment Fund as set forth in the statement opposite.

HARTELEYS, WILKINS, AVERY & FLEW, *Chartered Accountants.*
70A Easinghall Street, London, E.C. 2, 11 *March 1936.*

NOTE by the Hon. Treasurer:—Since the end of the financial year the Trustees of the Endowment Fund have purchased £430 3½% War Stock at a cost of £462 16s. 9d.

¹See *Journal* for February, 1936, p. 182.—HON. ED.

sat bolt upright, feeling a keen sense that my parents needed me. I sat tense, waiting on the telephone to ring. Until 1 o'clock, I turned over in my mind over and over again, sitting up in bed, then rising, almost 'phoning for a taxi; but at last I decided against this, as I might disturb my parents needlessly, if they were all right. I told my sleeping daughter, who mumbled incoherently that I should go to sleep. Next morning at breakfast I told the family of my premonition (for such it proved). My husband said I was worrying needlessly: however, I determined to send my daughter down to investigate. We had hardly finished breakfast when the telephone did ring with a brief message: 'Would Nan come down at once?' I knew it. I dressed in two seconds, meanwhile 'phoning for a taxi, and was with my father almost continuously until he died on Thursday, December 5th.

"When I told Mother, this was her story: On Tuesday night at 12 o'clock, Dad suddenly took a turn for the worse, and got out of bed, gasping for breath. Mother kept saying until 1 o'clock: 'Will I send for Willie?' (My husband, in whom Mother has great faith, and of whom she is very fond.)

"Was my Dad, who adores me, asking in his own mind for me, or probably concentrating on me? When I told him that we would sleep at their house alternately, Dad only weakly said: 'I'd rather have you, Nan.' But poor Daddy did not require me any more.

(Signed) NAN SHORTREED."

Mrs Shortreed's mother, Mrs Legge, writes as follows:

"My husband, who was very ill on Tuesday the 3rd of December with heart trouble, took a bad turn during the night, and was unable to sleep or lie in bed. I, being alone in the house, got very alarmed, and wondered if I ought to send for my daughter. As a matter of fact I was just on the verge of going out to 'phone for her, and yet was afraid of leaving my husband alone; so just carried on till the forenoon, when I got a friend to 'phone, and my daughter arrived almost at once in a taxi. I wondered how she managed so quickly. When she told me of her experience during the night and was sure she was needed, which had she not told her own family, I could hardly credit it.

"I omitted to say that the time I was anxious about my husband was between 12 and 1 o'clock, between Tuesday and Wednesday.

(Signed) ANNIE LEGGE."

The following is a copy of a letter received from Mrs Shortreed's daughter, aged 15:

"I have read your letter to Mrs Shortreed, and am now answering my part of it.

“On the night of December 3rd, I was awakened by Mother (we were sleeping together that night), who said: ‘My mother wants me, Anne!’ I knew that lately Mother had been worrying about her father’s illness, and, as I was only half awake, I said: ‘Oh! lie down and don’t worry, Mummy!’ After which I knew no more.

“Next morning at breakfast table, Mummy explained to the family her ‘midnight experience.’ ‘At about 12 o’clock I awoke, with the feeling that my mother was calling me, I determined I would get up and ’phone for a taxi,’ she said. ‘I sat up on the edge of my bed, and thought: now, if I ’phone, they will think I am worrying, which will naturally worry them all the more. No! I’ll lie down; they might ’phone! At about 1 o’clock,’ Mother said, ‘the feeling left me.’

“Mother then said: ‘Now I’m waiting for a ’phone-call, I’m sure it’ll come.’ Sure enough! at about 10 o’clock the telephone rang, and Mother was called down to Leith; her father having had a very bad ‘turn’ between 12 o’clock and 1 o’clock.

“(Signed) ANNE SHORTREED.”

Mr William Shortreed, P.A.S.I., has sent a statement, as follows:

“23 Duke Street,
Edinburgh, 17th January, 1936.

“I beg to confirm the statement made by my wife. At 8 a.m. on Wednesday morning, 4th December, 1935, while we were at breakfast, my wife mentioned that she had been very much disturbed during the night and early morning by a presentiment that all was not well with her father. She stated that she had a feeling that her parents wanted her during the night, 12 p.m. to 1 o’clock, and had been on the point of arousing me to call a taxi for her to go to them.

“I left town for a few hours on business, and when I returned about 1 p.m., my daughter informed me her mother had received a telephone call at 10 a.m. to go to her parents, and that she was still there. Her father died the following day.

“(Signed) WM. SHORTREED.”

In a letter to Mrs Shortreed, Mr Herbert asked her if she had ever had similar experiences. She answers: “. . . I cannot say I have had another such experience, except on one or two occasions I have felt uneasy about something, or have had the common vivid dream, which I have not been able to connect up, as in my father’s illness. My daughter and I often say things, or sing a song spontaneously, using the same words, tone, etc.”

The weak point of the case, of course, is that Mrs Shortreed knew that her father was seriously ill, though she had no reason to be

acutely anxious about him at the time of her premonition. The coincidence of the times, i.e. that Mrs Shortreed woke precisely at midnight, and that her uneasiness remained with her until 1 o'clock, whereas Mrs Legge tells us that her anxiety was also between twelve and one, is certainly very striking.

The case is strengthened by the fact that the percipient has had no other experience of a similar nature.

CASE.

L. 1308 RETROCOGNITIVE CLAIRVOYANCE OR DEFERRED TELEPATHIC IMPRESSION IN DREAM.

This case was sent to Mr Saltmarsh as a result of his letter in *The Spectator* of 17 January, 1936.

Mrs Samson writes as follows :

“ Ayres End, Kersey, Hadleigh, Suffolk, 17 January, 1936.

“ I was staying in Torquay in May, 1917, when a telegram came from the War Office stating that my step-son, Capt. H. W. Samson, Royal Scots, was missing. He had been in the attack on 3rd of May, and out of his company all the officers, and nearly all the men, were gone. The next day after receiving the telegram (13th May) I dreamt that I was standing in a rather small room in a strange country. I saw ‘ H ’ lying on a couch, his face and uniform covered with mud, face drained of every drop of blood, the neck of the tunic open and the chest exposed. Over him were bending two nuns ; one was dressed in black—a stout elderly woman—the other was young and in a white habit. The older woman lifted his head. He was quite unconscious, but I knew he was alive. I stood about two yards away from him, but I could not move a finger to help him, and I felt so grateful that the two nuns were kind and even solicitous about him.

“ He was missing for eight weeks, and then we received a letter from him from a German hospital. He had been wounded and taken prisoner. I wrote and told him about my dream and that it had comforted us and given us hope when every one in Command had reported him as ‘ killed ’. But I said ‘ How silly to think of nuns nursing soldiers in Germany ; it would be much more likely to be a male attendant smoking a cigarette ! ’ I did not get his reply until October.”

Extract from letter from Captain H. W. Samson, Royal Scots, to Mr and Mrs Samson. Dated 1917, September 20 :

“ That was an extraordinary dream you had, Jessie : for, about that time I was in hospital at Aix la Chapelle in a lazaretto, which

was looked after by nuns. One of them, Sister Benedieta was the head nun and was dressed in black, while there were a few novices dressed in white. There was a couch in the room, but I hadn't any uniform on then and probably I was rather muddy and covered with blood as it was only there that I began to get attention. I have promised to go back and see them after the war is finished."

Mrs Samson adds :

"The curious part is that he received this first attention on the 6th May, yet I did not dream the dream until the 13th May. Also I *knew* I was in a foreign country, so that the usual explanation of the unconscious person sending a message which is received by someone *en rapport* does not fit this case. He is not imaginative and I never had any supernormal experience before or since."

The original letter from Captain Samson was sent to the S.P.R.

It may be objected that the case is very much weakened by there being no contemporary record of Mrs Samson's dream. After nearly nineteen years, her memory of the dream may have altered very considerably in the direction of increased coincidence between the dream and the reality. But against this it may be evidenced that the letter from Captain Samson, which is a contemporary document, is itself strong corroboration of the truth of the account of the dream. Unless the dream were substantially as stated, Captain Samson's remarks are unintelligible. The attention of readers is directed towards this point, and they are left to form their own estimate of the evidential value of the case.

CORRESPONDENCE

THE SCARCITY OF SPONTANEOUS CASES

To the Hon. Editor of THE JOURNAL

MADAM,—As a somewhat close student of Psychic Research for some thirty years and as an Associate of the *S.P.R.* (all but a short interval) for not far off that time, may I be allowed to echo very heartily the letter of Count Perovsky-Petrovo-Solovovo, dated 29 June 1935, and appearing in the *Journal* for January of this year.

The late Professor Flammarion as evidenced by his more recent books, *Death and Its Mystery* (3 vols.), and also in his *Haunted Houses*, seemed to have had very little, if any, real use for mediumistic (induced or experimental) Phenomena, and I think the same could be said of the late Andrew Lang, another of our former Presidents.

Many years ago Professor F. S. C. Schiller referred at one of the *S.P.R.* meetings to the fact that logical, as opposed to mathematical,

proof is *cumulative* rather than inductive, *i.e.* like a rope with many strands of fibre instead of being like the chain of mathematical proof which is "no stronger than its weakest link".

It is this cumulative proof which Professor Flammarion appears to have relied upon and it has always seemed to me that if one has a mind logical enough to digest a very large number of well-attested facts, the result is more satisfactory than instances of mediumistic Phenomena which may be in perhaps the majority of cases explainable by a number of alternative hypotheses.

In my own life-time I have personally become aware of several cases at least equal if not superior as a matter of evidence to the elusive "Morton Case" set out in Myers' *Human Personality*, and elsewhere in literature and indeed it appears to me that the S.P.R. have never (unless at the time of its "Census" of some forty years ago), got hold of a tenth part of the really strong cases that have occurred.

Most people have seemed rather afraid of the S.P.R. and fail to send up cases. Moreover, there has been of late years a tendency almost to confine the work to very lengthy matters which are beyond the intellect or the patience of most ordinarily educated people.

Yours, etc.,

GUY HEATON (M.A. Oxon.)

THE PROXY SITTINGS

To the Hon. Editor of THE JOURNAL

MADAM,—May I venture to suggest that it might be wise to suspend judgment on the value and meaning of the proxy sittings until further investigations are carried out? From what I can gather it would seem that the majority of inquirers are of the opinion that these sittings show at least evidence of "telepathy"; and some of them (*e.g.* Mr Drayton Thomas) believe that they furnish good evidence of "survival".

I have recently made a very careful study of the Newlove Case and have just finished *Through a Stranger's Hands*. In both these publications I find little evidence of anything "supernormal" whatever. Verification varies with the ingenuity of the annotator; and it is of great interest to note with what efforts and with what success the Control makes use of the little information given by the sitters. In this respect the transformation of the drains to the pipes through the kind assistance of Mr Thomas will, I think, remain a classic instance of a principle which, I suspect, runs all through the mental phenomena, namely that it is the sitter who produces the results, not the medium. I am, etc.,

E. J. DINGWALL.

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—After reading Mr J. Cecil Maby's "Further Note", concerning Mr Whately Carington's "Quantitative Study of Trance Personalities", in the *Proceedings* of December, I too am tempted to raise a question as to Mr Carington's procedure of collecting the data for his experiments. My question concerns the laboratory conditions under which Mrs Eileen Garrett was tested by Mr Theodore Besterman in the S.P.R. for Mr Whately Carington's report.

I happened to be in London the summer of 1933 when these tests were being made. I heard at that time from Mrs Garrett, that Mr Besterman usually worked alone, with the exception of a few occasions when his wife was present. This meant that Mr Besterman, single-handed, undertook to be the recorder in these experiments of psychogalvanic reflexes, word association reactions, etc., while simultaneously manipulating a galvanometer and a stop watch.

While these experiments were being made, Mr Besterman told Mrs Garrett and Dr William Brown and myself, at different intervals of time, that the results of Mrs Garrett's tests with the psychogalvanic reflexes were largely negative. And Mr Carington, in his "Quantitative Study", Part I, p. 184, refers to these tests in no uncertain terms, saying, "Mrs Garrett proved to be by far the worst subject for the psychogalvanic reflex that I have ever worked with, while Uvani was little better if at all." As concerns this record material, Mrs Garrett states she did not work with Mr Whately Carington directly at any time.

I should like to report in relation to Mrs Garrett's reactions to the psychogalvanic tests with both Mr Hereward Carrington and Mr Besterman, that she distinctly remembers that in both experiments when the galvanometer failed, there was no observer or technician available to restore it to working order, and no new galvanometers were brought in to replace the imperfect machines in either experiment. And there was no attempt made, on either occasion, to ask her for more of her time, in order to make the absolute rechecking of these failures.

As far as we can gather, therefore, the material collected under such conditions in both experiments was used in the construction of Mr Whately Carington's and Mr Hereward Carrington's records. Was it, then, due to lack of a functioning galvanometer, or was it due to lack of Mrs Garrett's responses that negative psychogalvanic reflexes were produced? On this point she herself has always been in conflict.

No such difficulty in the recording of psychogalvanic reflexes has however appeared during her later work with trained medical men and technicians in London and New York. In Mrs Garrett's work with Dr William Brown in 1934, the recording of these reflexes went quite smoothly. In this research, Mr R. J. Bartlett, M.Sc., manipulated the psychogalvanic apparatus for Dr Brown. (See "Sleep, Hypnosis and Mediumistic Trance", by Dr William Brown, *Character and Personality*, Dec. 1934.)

Similarly, there has been no difficulty in the psychogalvanic recordings of Mrs Garrett in the second experiment, now being conducted in New York, under the direction of several prominent medical men, assisted by a trained technician and a recorder. In this case, the psychogalvanic reflexes not only of Mrs Garrett and her control, Uvani, but of a secondary control, Abdul Latif, as well, have been successfully recorded. This new research bids fair to be of great importance in the study of mediumship, as it includes a thorough physiological examination of not only Mrs Garrett in her normal waking state, but also of Uvani and Abdul in her trance state. All three of these personalities are being checked as to pulse, respiration, blood-pressure, blood counts, blood-chemistry, and typical drug responses. Perhaps most interesting of all the tests are the series of basal metabolisms and cardiograph records of Mrs Garrett and the two controls.

Would the results of these later researches not lead us to believe that the fault lies not so much in Mrs Garrett's negativity as in the lack of observation and training in those who conducted the earlier experiments? And how might this affect the conclusions of Mr Whately Carington's report?

Why, also, did Mr Carington eliminate from his own Word Test, in his experiments with the trance personalities, twenty of the original words in Jung's Word Association list? He says, "the reason for doing this (deleting the twenty words), in the first instance, was that I feared lest the inclusion of many words of great common interest—such as those bearing on erotic life and the like—might obscure individual differences in respect of other matters."

What individual differences could be of greater significance than those "concerning erotic life and the like"? The use, then, of these Word Association Tests, seems to me to become of doubtful value when all the key words that deal with the repressions of the unconscious are eliminated. Do we know yet to what extent the unconscious or subconscious of a medium and trance personality are similar? And what could be of more vital importance to study? If Mr Carington has some unpublished data on this subject, perhaps

at some later date he might share it with us and so permit us to know his reasons for excluding these twenty words from his own Word Association Tests.

These tests were developed originally by the psycho-analysts to uncover the buried complexes of which the individual tested was himself unaware. But in Mr Carington's experiments, with the constant repetition of these lists of words to the normal waking personality of the medium and to the trance personalities, the effect of the stimulus words must surely die down. This then raises the question of the ultimate value of such reaction responses, when repeated so often in close succession.

To clarify my point, I would note that Mrs Garrett told me that, subconsciously, the original words and their reaction responses, in these Word Association Tests, became to her, after numerous repetitions, quite mechanical and perhaps subconsciously memorised.

Very truly yours,

(MISS) MARGARET NAUMBURG.

To the Editor of THE JOURNAL

MADAM,—I venture to offer my opinion on the Oriental flavour of Feda's word-association responses; my qualifications for so doing are that I have lived in Delhi as a missionary for twenty-four years, have a good knowledge of Hindustani, and friendships and acquaintances among both Hindus and Musalmans of various social classes.

In the list given in *Proceedings* 141, p. 374, and in the notes thereon on the following pages, I note the following words:

(1) Quite impossible for an Indian living near Simla 130 years ago—*Samisen, amah, yashmak*. Note that in the case of *yashmak*, not only would the word be unknown, but the thing; Musalman women in North India never wear a veil of any sort, but a garment (the *burqa*) that covers the whole figure. The responses are as impossible for the character claimed as they would be, say, for the daughter of a Scotch laird of the same period.

(2) Very unlikely for the person and period:

(a) *Curry*.—To an Indian, curry is not a meal or a dish, but a relish added to the staple dish of wheat cakes or of boiled rice. The reaction to "make", "cook" or "eat" would therefore be some phrase such as "curry and rice", never "curry" alone, as it can be to an Anglo-Indian or one who has merely read about India.

(b) *Sari* for "make".—No Indian woman *makes* a sari, as she may make some other pieces of dress; it is simply a piece of woven cloth or silk which is bought.

(c) *Mission, missionary*.—In about 1800, when there were only one or two missionaries in the whole of North India (Carey in Bengal, 1000 miles from Simla), it is almost impossible that any Indian near Simla should have ever heard of such a person.

(d) *Elephants* (wild, shoot).—I do not think that there would be any wild elephants anywhere near Simla, nor would shooting elephants be a sport even of noble or royal Indians.

(e) *Tablets*.—I know of no evidence of the use of tablets for writing in India as late as 1800; paper had been regularly used for several centuries.

(f) *Burmah*.—Very unlikely to have been heard of by an Indian at the place and time claimed.

(g) *mango* (hot).—I find it difficult to conceive of any Indian having this association. Mango is indeed used as one among many constituents of chutnies, but, particularly in North India, where the fruit is so common, the natural, if not inevitable association with “mango” is coolness and refreshment.

(3) The likelihood or otherwise of certain reactions depends very much on whether the person concerned was a Hindu or a Musalman (or Musalmani). *Koran* is a highly unlikely reaction of a Hindu to “book” or “read”, and so is *harem* for “home”; on the other hand *pyre* for “dead” or “bury”, and *idol* for “insult” are improbable for a Musalman. Yours, etc.,

F. J. WESTERN,
Bishop of Tinnevely,
S. India

REVIEW

DR A. TANAGRAS. *Le Destin et la Chance*. Société des Recherches Psychiques Hellénique, Rue Aristotélous, 67, Athènes, Grèce. Price: 15 francs français, 100 drachmes.

This book, by the president of the Greek Psychical Research Society, is chiefly concerned with the author's “Théorie de la Psychobolie”. This theory postulates that energy, resulting from disturbances in the unconscious mind, is capable, in certain individuals, of exteriorisation; where it manifests itself by reacting with inanimate objects (telekinesis), with another human mind (telepathic suggestion), or with the tissues of living organisms (the “evil eye”, etc.).

After a preliminary chapter on causation, Dr Tanagras reviews the question of precognition, and its reconciliation with an undetermined future, supporting his arguments with several interesting cases. He next considers telekinetic phenomena, citing Osty's work with Rudi Schneider, and quoting various cases both from his own

country and elsewhere. He then deals with telepathic suggestion, and with the action of his psychobolic influence on plants and animals. The first part of the book ends with a chapter on the tendency for unconscious impulses to obtain realisation, this tendency being the driving force behind the psychobolic influence.

The second part of the book is chiefly concerned with chance and its relation with his psychobolic theory, the author touching, in the final chapters, on the moral and philosophical aspects of the subject. The book ends with a résumé of the theory, in German, taken from an article by Professor Schroeder in the *Zeitschrift für Metapsychische Forschung* for April 1934.

Although many people will not be convinced by Dr Tanagra's theory, few will deny the skill with which the author marshals his material in support of his views. In all, he cites 101 cases, many of which are of considerable interest. Dr Tanagra must be congratulated on a painstaking, if not completely successful, attempt to find a theory universally applicable to all psychic manifestations.

C. V. C. H.

NOTES ON PERIODICALS

Tijdschrift voor Parapsychologie.

January 1936. Dr Tenhaeff contributes a paper on Mediumship. He holds the view that paranormal faculty is very widespread, and cites the Society's work on E.S.P. He divides mediums into two classes, clairvoyant and clairaudient. He states, probably with reason, that all supernormal faculty is accompanied by dissociation, verging on trance. He discusses the symbolism of mediumship.

Dr Dietz attacks theosophy and occultism.

Herr Felix Ortt writes on the nature of "controls." He does not consider Mr Whately Carington's case for controls being secondary personalities of the medium to be by any means proved.

THE *JOURNAL* IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on a definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 27th May 1936, at 5.30 p.m.

WHEN A PAPER ON

The Flower Medium : Miss Hylda Lewis

WILL BE READ BY

MR J. CECIL MABY.

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

FORTHCOMING MEETING

THERE will be no Meeting at the end of June. A General Meeting has been arranged instead for *Wednesday, 8 July*, at 6.30 *p.m.*, which will be held at Manson House, 26 Portland Place, London, W. 1, when Sir Ernest Bennett, M.P., will read a Paper on "Some recently received Cases of Haunted Houses."

The Meeting will be announced in the *June Journal*. Visitors will be admitted by invitation cards (which will be circulated with the *June Journal*) signed by a Member or an Associate.

SOME RECENT SITTINGS WITH CONTINENTAL MEDIUMS¹

BY C. V. C. HERBERT

IN the autumn of last year, owing to the kindness of a member of the Society who prefers to remain anonymous, I was able to go to Austria and Poland to sit with well-known mediums. The sittings were informal, and I was present as a private individual, not as a representative of the Society.

I went first to Weyer, in Austria, where I joined three other members of the Society, Miss Reutiner, Lord Charles Hope, and Mr Evelyn. Weyer is a small town in the lower Alps, not far from Vienna, and for some time past has been the home of Rudi Schneider. I stayed in Weyer for nine days, and had no less than fourteen sittings with this medium.

Rudi Schneider had a flat in the house of a Herr and Frau Schickl, and was accompanied by his wife and mother. Herr Schickl is a timber merchant in Weyer, and both he and his wife are confirmed spiritualists. The sittings were held in a basement room in Herr Schickl's house. The room was unfurnished and made an excellent séanee room. For some time, Herr Schickl had been experimenting with an elaborate apparatus, said to have been constructed from directions given by Rudi Schneider's control personality, whom we call Rudi-Olga. The purpose of the apparatus was to obtain photographs of a paranormal nature, and we were assured that one had actually been produced bearing a portrait of the late Baron von Schrenk Notzing. Unfortunately Rudi-Olga had given orders that this photograph should be destroyed, so we were unable to examine it. Many other photographs were shown to me, bearing more or less distinct images of an indefinite character. One, the Schickls thought, was like a human face; but I was not able to make this

¹ Containing the substance of a talk by Mr Herbert at the Annual General Meeting of the Society on 26th February 1936.

out. They also showed me the apparatus with which the photographs were said to have been produced. It seemed to me to be a very fantastic affair, and I do not think it worth my time to describe it to you now.

I attended one sitting at which the apparatus was used, and through the kindness of Herr Schickl was allowed to fix up a camera alongside the camera that formed the central feature of the apparatus; so that anything appearing on their plate ought to have appeared also on mine. During the taking of the picture, which was done in complete darkness, Rudi Schneider was not in trance. He and the rest of the sitters grouped themselves round the apparatus, which reached to the ceiling, and led by Frau Schickl they all sang or rather shouted in chorus, producing the most incredible pandemonium that I have ever heard in a séance room. I placed myself on a window ledge, overlooking the apparatus, in such a position that I was satisfied that no attempt was made to affect the cameras by means of a normal light (an electric torch, etc.). I had invited fraud by fixing up my camera some time beforehand, and leaving it unattended with a film in position. But before the actual sitting began, I had removed the film, unknown to anybody, and replaced it by another. Thus, if the film had been tampered with, the "psychic" picture would have come out on the wrong negative. Both films were brought back to England and carefully developed by me. Both were absolutely blank. The plate from Herr Schickl's camera was developed directly after the sitting, and showed a very faint marking, attributed by the Schickls to supernatural forces. In my opinion the marking was due solely to unskilful development, and possibly also to the plate being stale. The same causes would also account for most of the images on other plates that they had got, always excepting the Baron's portrait, which, as I have said, had been regrettably destroyed. The Schickls are still continuing their experiments without Rudi Schneider, and are getting similar results; so there is little reason to believe that Schneider had much to do with it.

Of the remaining thirteen sittings, nine were for the usual telekinetic phenomena, and the rest for interrogating Rudi-Olga, collecting the "power", etc. At six of the nine telekinetic sittings movements of objects were recorded by one or more sitters, and at three of them I am convinced that actual movements did take place. These three were the evening sittings of October 6th, October 7th, and October 8th. At each of these, movements took place when I was controlling the medium myself.

I think that a few words on the actual method of control used

would not be out of place here. The controller sits opposite Schneider and places his two feet against the legs of Schneider's chair. He holds the medium's two wrists, one in each hand. The second controller sits on the controller's left, and holds Schneider's right arm with his right hand, the second controller's left hand being held by the first of the sitters, all of whom join hands. These positions are taken up before the lights go out. It has been suggested that Rudi Schneider is able to free a hand during a sitting without the knowledge of the controller. This seems to me to be absolutely impossible. Unless the controller were himself in trance, he would be bound to know the very instant that Schneider got free. I am prepared to say without the slightest hesitation that during the whole time that I was controlling neither of the medium's hands was ever free. As regards the medium's feet, the position of the controller's legs, pressed against the medium's chair, makes it impossible for him to free these without instantly being detected.

The medium's hands and feet are thus perfectly under control, nor do I consider that he could have produced the movements that we saw by any other normal means. I am quite satisfied that he had nothing in the nature of an extensible rod, held in his teeth; and that he could not have produced the movements by moving his body. In fact, during some of the best movements he was completely motionless, with his head down on my knees.

I have here the actual notes of the three sittings in question. They were written down by me directly after each session, and as they are quite short I will read them to you now.

1935, October 6th. Evening.

1st Section : hrs. 20.15 to hrs. 21.27.

Order : Rudi Schneider, C.V.C.H. (controller), Frau Schwaiger (2nd controller), Professor Schwaiger, Miss Reutiner, Lord Charles Hope, Herr Kustersitz.

Several curtain movements were reported, one of which I am quite certain was real, and which I am convinced could not have been done normally by Rudi Schneider. There was also a movement of the red lamp, reported by Lord Charles Hope, which was disclaimed by Rudi-Olga.

2nd Section : hrs. 21.45 to hrs. 22.33.

Same order.

No phenomenon was reported.

Rudi-Olga asked for a change of controller.

3rd Section : hrs. 22.47 to hrs. 23.38.

Order : Rudi Schneider, Professor Schwaiger (controller), Frau

Schwaiger (2nd controller), Lord Charles Hope, Miss Reutiner, C.V.C.H., Herr Kustersitz.

No phenomena were reported.

Note on this sitting.

During the interval after the first section, I examined the inside of the "cabinet" with an electric torch, and found nothing suspicious. At the time that the curtain movement, that I saw, took place, I had perfect control of Rudi Schneider. He was leaning forward with his head on my knees, and was quite motionless. The quick breathing had temporarily ceased. I had hold of both his wrists, and was gripping his legs with my knees. My feet were pressed against the legs of his chair. I am quite satisfied that he could not have moved the curtain by normal means. Both doors were locked throughout the sitting.

1935, October 7th. Evening sitting.

Before and after the sitting I examined the "cabinet" and found nothing suspicious. I also looked under the large table in the corner of the room.

During the sitting, the door of the room and the door of the vestibule were both locked, the keys remaining in the locks. Both doors were found to be still locked at the end of the sitting.

Order : Rudi Schneider, C.V.C.H. (controller), Miss Reutiner (2nd controller), Mr Evelyn, Frau Leeder, Lord Charles Hope, Herr Kustersitz.

We began at hrs. 20.32 and ended at hrs. 22.02.

There were two movements of the curtain, both predicted by Rudi-Olga. The first was seen by Miss Reutiner, Herr Kustersitz, and myself. I am quite certain that it was a real movement, and that the control of the medium was perfectly effective. Rudi-Olga had asked us to count to five, and then to "look out" at the curtains. The movement began about two seconds after we had said "five". Rudi Schneider was leaning forward with his head on my lap. I had a firm hold of his two wrists, and his legs were gripped between my knees. My two feet were pressed against the legs of his chair. The distance from the inner edges of the two curtains to the nearest point on the medium's chair was 120 centimetres. The distance from the right-hand edge of the right curtain to the chair was 40 centimetres. The reason that the movement was not seen by the other sitters was, I think, that it was an in-and-out movement, at right angles to the plane of the curtains, and was thus more apparent to the sitters on the ends of the semicircle of chairs than to those who were more opposite the centre of the curtains.

The second movement was only observed by Herr Kustersitz. By this time, the luminous strips on the curtains (by which the movements were made visible) had become very dim. Herr Kustersitz was certainly in the best position to observe curtain movements, and it is possible that this one (which he described as large) was real.

Lord Charles Hope held both Herr Kustersitz's hands throughout the sitting.

1935, October 8th. Evening sitting.

During each period, both doors were locked on the inside, the keys were left in the locks. The room was searched before the sitting by Lord Charles Hope and myself. Nothing suspicious was found.

1st Period : hrs. 20.10 to hrs. 21.04.

Order : Rudi Schneider, C.V.C.H. (controller), Miss Reutiner (2nd controller), Count Schönborn, Countess Schönborn, Mr Evelyn, Frau Leeder, Lord Charles Hope, Herr Kustersitz.

No phenomenon was reported.

2nd Period : hrs. 21.18 to hrs. 22.17.

During the first part of the 2nd period the order was the same as for the 1st period. Then, at the request of Rudi-Olga, I changed places with Count Schönborn, who took control.

While I was controlling, there was a small curtain movement, predicted by Rudi-Olga, which came after a count of five. It was seen by myself, Miss Reutiner, Mr Evelyn, and Herr Kustersitz. I have no doubt that it was a real movement, and that it could not have been produced by Rudi Schneider by normal means. When the movement happened, his head was down on my right thigh, I had a firm grip of his two wrists, and his legs were held between my knees. My two feet were pressed against the legs of his chair. The distance from the curtains to the medium's chair was the same as for the sitting of October 7th (*i.e.* to centre of curtains, 120 centimetres, to right edge of curtain, 40 centimetres).

3rd Period : hrs. 22.32 to hrs. 0.02.

Order : Rudi Schneider, Count Schönborn (controller), Miss Reutiner (2nd controller), C.V.C.H., Countess Schönborn, Mr Evelyn, Frau Leeder, Lord Charles Hope, Herr Kustersitz.

One good curtain movement, seen by everybody except Rudi Schneider and Countess Schönborn. One doubtful movement.

4th Period : hrs. 0.15 to hrs. 1.35.

Same order as for the 3rd period.

Several good curtain movements seen by all the sitters. A small stool, which stood under the red lamp, was overturned and fell

towards the sitters, coming to rest on my foot. It fell as if a force had been applied to it in a direct line from the centre of the curtains, and acting away from the curtains. The stool had four legs and although it was not heavy, it needed to be tipped through a large angle before it would overbalance. Distance from centre of stool to the centre of the curtains, 95 centimetres. Distance from the stool to the medium's chair, 93 centimetres. I have considerably longer arms than Rudi Schneider, and, when sitting in the medium's chair, I found that I could not reach the stool unless I leaned hard over to the right. Miss Reutiner is positive that throughout the sitting Schneider never leant over to the right. Count Schönborn has had considerable experience in controlling this medium, and Miss Reutiner, who was acting as 2nd controller, vouches that his control, so far as she was able to judge, was very firm and strict throughout the sitting. Lord Charles Hope reports that he was holding both Herr Kustersitz's hands in his left hand.

After the sitting was over, I searched the "cabinet" and the room and found nothing suspicious.

Let us consider the movements produced while I myself was controlling the medium. They were all movements of the curtains forming the front of the "cabinet", and I am absolutely certain that they were actual movements and not creations of the imagination caused by the bad lighting. I am also sure that they could not have been caused by accident—draughts, etc. To me it seems impossible that they could have been done by the medium by normal means. I am thus faced with three alternatives, viz. that they were produced by fraudulent manipulation on the part of one or more of the sitters; that they were done by fraud by a person or persons from outside the room; that they were produced by paranormal means.

In considering the first of these three alternatives, I should like to call your attention to the fact that at the sittings in question no member of the Schneider family was present as a sitter, nor were either Frau or Herr Schiekl in the room; whereas at other sittings, at which no phenomena were recorded, the two Schiekl and Schneider's wife and mother were all in attendance. The full list of sitters at these three sittings is as follows:

Lord Charles Hope	} present at all three sittings.
Miss Reutiner	
Herr Kustersitz	
C.V.C.H.	
Professor Schwaiger	} present only on October 6th.
Frau Schwaiger	

Mr Evelyn	}	present on October 7th and 8th.
Frau Leeder		
Count Schönborn	}	present only on October 8th.
Countess Schönborn		

Professor Schwaiger is the chief engineer of Radio Wien, the Vienna broadcasting station, and has done a great deal of research in the field of parapsychology. Herr Kustersitz is an engineer and is the professor's assistant. Frau Leeder is a venerable old Austrian lady, the widow of an official of the Court. The Schönborns are Czechoslovakians who have had much experience with sittings with this medium. The others are all members of this Society.

It will be seen that there were only four persons who were present at all three sittings, viz. myself, Lord Charles Hope, Miss Reutiner, and Herr Kustersitz. So, unless there were two or more swindlers, it must have been one of these who was cheating. This does not seem to me to be at all probable. Nor is it any easier to suspect Professor Schwaiger and his wife, Mr Evelyn, Frau Leeder, or Count and Countess Schönborn of nefarious practices. Except at the sitting of October 6th, when Herr Kustersitz's left hand was free, every hand in the room was being controlled by a person other than its owner; ¹ so that, if the movements were done by a sitter *by hand*, he must have received active or passive support from another person. The only persons who sat next to each other at all three sittings were Lord Charles Hope and Herr Kustersitz. So, if they can be acquitted of collusive fraud, it would have needed at least four persons working in collaboration for the movements to have been done by hand. But if the sitters' hands were controlled, their feet were free. Could anyone have kicked the curtains? Herr Kustersitz was near enough; but then there was a massive apparatus in the way. I do not think he could have done it without arousing Lord Charles Hope's suspicion. The other sitters, except possibly Lord Charles himself, were all too far away. How about some sort of extending rod, held in the teeth? Possible, but very unlikely.

Let us consider the possibility of fraud from outside. Could someone have been concealed in the room? The only possible places of concealment were searched before and after the sittings. No one was found. Could someone have got into the room during the sittings without any of the sitters noticing? The room was unfurnished except for the requisites for the sittings. The walls were plain whitewashed plaster. I am certain there were no trap doors

¹ Except my right hand, which was holding the medium's left hand. But for the purposes of this paper, I must claim to be above suspicion.

or secret entrances. The windows were securely shuttered. There was a single door, communicating with a vestibule, also unfurnished, from which a second door led into the garden. Both these doors were locked on the insides, and the keys left in the locks. It would mean that somebody had to open the outer door, by forcing round the key with pincers or a wire ; close the door after him ; repeat the process with the door of the séance room ; get through the door without making enough noise to be heard by Lord Charles or Herr Kotersitz, both of whom were sitting close to the door ; move the curtains ; get out of the room ; lock the door after him with his wire or pincers ; and then lock the door of the vestibule in the same way. There was very little noise going on during the sittings ; none of the shouting and song which is sometimes a feature of sittings with this medium. I think it is utterly impossible for such a feat of housebreaking to pass undetected. Moreover, I examined both keys carefully, and could find no trace of scratches, such as forcing them round from the outside would be expected to produce.

Could the curtains have been moved from outside the room ? I examined the " cabinet " and its fittings with great care in an excellent light. There were no holes in the wall, through which strings or wires might have passed into another room. Nor were any such strings seen when the " cabinet " was examined before the sittings. I could find no hole in the ceiling, through which a rod might have come down from the floor above. A magnet ? Certainly there *might* have been a powerful electro-magnet hidden in the next room, which would act on pieces of metal sewn into the curtains ; but I must admit that I do not consider it very likely.

A supernormal explanation, though intrinsically very unlikely, is rendered less so in this case, since there is a lot of evidence¹ that very queer things do happen at sittings with this medium.

We are thus faced with three improbabilities, and I will leave you to decide which one you accept. Or, possibly, one of you may be able to suggest a more likely explanation.

Before I leave the subject of Rudi Schneider, I should like to say a few words about an apparatus, designed by Professor Schwaiger, that was used at some of the sittings. It was an application of the principle of dark television, by means of which a visible image is obtained of the cross section of a beam of infra-red. One can thus " see " objects that enter the beam, although it is completely stripped

¹ See E. and M. Osty, *Les Pouvoirs inconnus de l'Esprit sur la Matière* (Paris 1932).

Lord Charles Hope and others. Report of a Series of Sitzings with Rudi Schneider. *Proc. S.P.R.*, 1933, June, XLI, 131.

of all visible light. Light from a projector lamp is refracted into a parallel beam by a lens, and the beam is analysed by a rotating "scanning drum", which will be familiar to those conversant with television apparatus. The resulting small beam traverses the whole field during each revolution of the scanning drum. The visible light is absorbed from the beam by means of a filter, the remaining infra-red being focused on to a photo-electric cell by means of a second lens. The amplified output of the cell is made to activate a neon glowlamp, the light from which is synthesised by a viewing drum, rotating in synchronisation with the scanning drum. Owing to the phenomenon of persistence of vision, the eye of the observer, applied to the scanning drum, sees, not a small spot of light rapidly traversing the field, but a uniform illumination of the whole area of the field. Any occulting object placed in the infra-red beam, will be seen as a dark silhouette against this bright area. This silhouette picture, which is smaller than the original object, is not quite as clear as could be wished; though, with a more elaborate apparatus, greater definition could no doubt be obtained. As it is, however, a very good idea can be had of the nature of any object entering the field when the apparatus is in use. For example, we tried the experiment of blowing cigarette smoke into the beam, and its presence was at once detected by the observer.

By recording the current through the neon lamp, either on a wax disk or on a photographic film, the moving picture seen by the observer could be reproduced at will. Professor Schwaiger has not done this yet; but the method is well known, and would present no special difficulties. Such reproduction would obviously be of enormous importance in psychical research, and I understand that a recording apparatus will be constructed as soon as any results are obtained with the apparatus.

The apparatus was used at the sittings of October 1st and October 8th; but nothing was observed with it.

From Weyer, we went on to Warsaw, where my chief objective was M. Ossowiecki. I got into touch with him soon after my arrival, but found unfortunately that he was not at all well. He told us that his blood-pressure was abnormally low, that he was very run down, and that his doctor had forbidden him to attempt any clairvoyant work for some time. In spite of this, he very kindly consented to try an experiment for me, and did in fact do his utmost to "read" some specially prepared letters that I had brought with me from England.

I had planned to try three main experiments with M. Ossowiecki,

which were as follows : the "reading" of an envelope the contents of which were unknown to any living person ; the "reading" of a similar envelope the contents of which were unknown to any person, alive or dead ; and the "reading" of an envelope containing a message in the form of a latent image on photographic paper. For the first, I took a manuscript book, written in French, tore out a page in the dark, sealed it in an envelope, and destroyed the rest of the book. Any question of telepathy was thus disposed of. For my second envelope, I built up a number of six digits, by taking a large number of similar pieces of paper, on each of which was written a single digit, selecting six of these in the dark, and pasting them in a line on to a sheet of paper, thus forming a six-digit number. Both of these envelopes contained, in addition to the message, small pieces of sensitive photographic paper ; so that any attempt to read them by means of X-rays, or by opening the envelopes, would have betrayed itself by the fogging of the paper. For my last envelope, I prepared a special "pen", consisting of a hollow tube, pointed at one end, and containing a minute electric lamp. With this I "wrote" on a sheet of photographic paper, which was then enclosed in a sealed envelope. After the "writing" had been done, the paper appeared quite unaltered, the writing only coming out if the paper was placed in a developing solution. This arrangement was absolutely proof against fraud by opening the envelope, X-ray examination, etc. In case this test should prove impossible, even if M. Ossowiecki's powers were perfectly genuine, I also wrote a word or two on the back of the paper in ordinary pencil.

As an additional precaution, and to guard against possible substitution of the envelopes, I made secret marks on each of the three, and examined these carefully before and after each experiment. M. Ossowiecki made several attempts to read the envelopes, devoting in all several hours to them. He was not successful with any. He continually complained of feeling ill, saying that as soon as he concentrated on an envelope, he began to feel giddy.

One afternoon, when he was having tea with us in the Europejski Hotel, he said that it was much easier to "read" something that had just been written. He suggested that I should make a drawing on a card and give it to him to try. I went to the other end of the room and drew at a writing table, first making sure that no one could overlook me. I drew a teapot, which happened to be the first thing that came into my head. I drew it in three parts : first the body and lid, then the handle, then the spout. I put the card into one of the hotel envelopes, and sealed it up. I then tried holding the envelope up to a strong light, and found that the drawing could

be seen through the paper. This was very unfortunate, but it was too late to do anything. I returned to M. Ossowiecki and gave him the envelope, watching him very carefully to see if he held it to the light. This I was certain that he did not do. It was not enough to hold the envelope to the window. A bright light was necessary. The only light near M. Ossowiecki was an electric wall bracket which was directly behind him. To make use of this, he would have to have turned round in his seat, and I am certain that he did not attempt to do it. M. Ossowiecki held the envelope in his hands, rubbed it between his fingers, and then placed it on the table with his hand over it. He never examined it closely, nor was he wearing his spectacles, which were very strong ones, and without which I doubt if he could have seen very much. He then spoke in French. A translation of what he said is as follows :

“ I can't do anything to-day. . . . There are three parts to this drawing. It is as if there is a pipe ; then as if . . . I don't know what. I begin to see something . . . a head ? It is not an ordinary drawing, it is something fantastic. I don't know what it is. . . . No. I cannot do it. . . .

“ I see . . . a head ? . . . A tail ! A tail ! Is it a cat ? I don't know what it is. You are not an artist, are you ? Not good at drawing. Give me a bit of paper.”

I handed M. Ossowiecki a pencil and a piece of paper, which he drew on. His drawing is not unlike my teapot, but is drawn on end, with the handle at the top.

“ No ! No ! Look ! Look ! ”

He drew a second picture. This drawing is done the same way up as the first, but is much more like my teapot. M. Ossowiecki did not seem to have any idea of what it was supposed to be.

As I have said, I am quite convinced that M. Ossowiecki did not hold the envelope up to the light ; but afterwards I found that by pressing the envelope down on to the table, one could just see the drawing through the paper. I am fairly certain that he did not do this, but there is a possibility that he did, as I was not on the look out for it while I was watching him. But I do not think that he could have seen the form of the drawing without his spectacles. This little experiment is thus of some interest, though I do not, of course, put it forward as any proof of supernormal faculty.

While I was in Warsaw, I also attended two sittings with the clairvoyant Sabira, who has a great reputation in Poland. Lord Charles Hope, Mr Evelyn, and Miss Reutiner also attended the sittings. We tried Madame Sabira's powers with various experi-

ments. I gave her a watch, and she attempted a description of its late owner. Letters were similarly treated, and at her special request, we prepared, for the second sitting, a number of identical boxes, each containing a different object. These she "psychometrised", and gave descriptions that she said applied to the objects. We were not, however, able to detect the faintest sign of any paranormal faculty. All the objects were wrongly described, the characters of the writers of the letters were unrecognisable, and the owner of my watch was chimerical.

I may add that a sitting with this medium was lately held in London at the Society's rooms, and an attempt was made by Madame Sabira to give medical diagnosis from photographs of the patients. The experiment was very carefully carried out, and again no evidence was obtained of paranormal faculty.

From Warsaw we went to Kracow, where we had heard rumours of a wonderful medium "better than Kluski". This remarkable individual was first said to be a judge of the High Court; but afterwards they told us he was really an engineer. But, whatever he was, he completely failed to materialise.

We came back via Prague, where we were introduced to various distinguished foreigners who were interested in psychical research. In Prague we again met Count and Countess Schönborn, who told us that, after we had left Weyer, a wonderful sitting with Rudi Schneider had taken place while the medium was staying with them at their shooting box in Austria. Herr Kotersitz had also been present. A materialised hand had come out from the "cabinet" and rung a gong. Another sitting with Herr Schickl's photographic apparatus had been held, at which three cameras were used: the usual one manipulated by Herr Schickl, one put up by Count Schönborn, and one belonging to Herr Kotersitz. Both Herr Schickl's camera and the Count's had produced an image; but the images were quite different in the two pictures. The Count's resembled a half moon, while Herr Schickl's showed a criss-cross arrangement of bright lines. Herr Kotersitz's camera produced a complete blank, as mine had done. Without wishing to appear unduly conceited, I cannot help suggesting that possibly when this apparatus is used, the amount appearing on the plates is inversely proportional to the skill of the photographer.

In conclusion, I should like to thank very heartily the anonymous Member who so kindly paid the expenses of my trip; and also to thank the Council for allowing me to be absent from London for over four weeks.

CORRESPONDENCE

PROXY SITTINGS

To the Editor of THE JOURNAL

MADAM,—Dr Dingwall's theory that the sitter makes the sitting would involve so immense a saving of thought and labour that its attractions are obvious: but the only grain of demonstration to support it, so far, is the not very difficult observation that, in the Newlove case, Mr Dayton Thomas's mention of diphtheria could have made Feda think of drains and speak of "pipes." The contribution of the sitter in transforming this elementary association of ideas into an elaborate chain of evidence leading to something not drains as a likely cause of something not diphtheria, remains a matter of assertion without evidence.

The normal but unnoticed reactions between sitter and medium deserve very careful tracing and analysis in all cases, and it is a disappointment that Dr. Dingwall continues to support his theory by declarations instead of turning his abilities to the finding of instances which might be of real value.

Yours etc.,

KENNETH RICHMOND

To the Hon. Editor of THE JOURNAL

MADAM,—Commenting on mental phenomena in the April issue of the *Journal*, Dr Dingwall makes the astonishing assertion "that it is the sitter who produces the results, not the medium." This being the case, can he explain the following experiences in connection with sittings I have had for automatic writing with Miss Geraldine Cummins?

(1) Some years ago she wrote the surname and christian name of a man who claimed that he had recently died. He gave the name of the street in which he lived in a town which neither she nor I had ever visited. The alleged communicator was a complete stranger to us, but I traced the widow at the address given and she corroborated further details unknown to us which had been written by Miss Cummins. Am I to assume that I am responsible for the production of the above information?

(2) The young daughter of a friend of mine died at the age of fifteen. I had not seen her since she was five. The mother asked if I would try to get into touch with her. After some confusion which was subsequently elucidated, Miss Cummins produced a personality which was recognised by the mother (who was not pre-

sent but to whom I forwarded the script), together with other details connected with the child which were unknown to me. One of these was a statement as to where the mother was and what she was doing at a certain Christmas. The mother's whereabouts were, at that time, entirely unknown to me and Miss Cummins was unacquainted with both mother and child.

(3) The case of T.M.: A man whom, though a friend of my brother-in-law, I had never met, purported to communicate through Miss Cummins giving many details entirely unknown to either of us which were subsequently verified. (This case is fully reported in *Psychic Science* for October 1935).

Regarding Nos. 2 and 3, Dr Dingwall may say that I was *en rapport* with the mother in the one instance and with my brother-in-law in the other and that thus I was able to produce these results. But this hypothesis does not apply to No. 1, unless he claims that I have the power of penetrating the minds and surroundings of people unknown to me who don't interest me in the least and imparting telepathically the details ascertained to the medium.

In view of the many records extant in which evidence has been given by a medium of knowledge unknown to anyone present at the sitting, I think Dr Dingwall's remark the most preposterous I have ever read.

I entirely agree with Mr Drayton Thomas's statement that it yet remains to be proved that a medium can tap memories of a distant person unknown to medium *and to sitter*.

Yours faithfully,

E. B. GIBBES.

THE MEDIUMSHIP OF CARLOS MIRABELLI

To the Editor of THE JOURNAL

MADAM,—Owing to absence abroad and other causes I am only now able to answer Lord Charles Hope's letter in the February *Journal* about the film I took during the Mirabelli sittings. On my arrival in New York from South America after the Mirabelli sittings I had this film processed and projected by the Eastman people. As projected by them the film showed nothing more than a vague blur, and I was assured that no better result was obtainable. I made the mistake of accepting this assurance, coming from the source it did. And it now appears, thanks to Lord Charles Hope, that I made the even worse blunder of supposing that it is possible to film a scene and at the same time correctly to observe its details. For, although I was right in saying that Mirabelli did not throw himself on the

blackboard, I overlooked what comes pretty well to the same thing and what the film clearly, though momentarily, shows, that he did in fact, on one occasion, grasp the edge of the blackboard as it fell.

At the same time, it is only fair to point out that the film, when projected at close range and as sharply defined as possible, brings out the points I made in my paper in regard to the brilliant light, the proximity of the "sitters," and the consequent unlikelihood of the use of threads passing unperceived. On the whole, I feel that the position remains about what it was before. Nevertheless, I am greatly obliged to Lord Charles Hope for the trouble he has taken.

Dr Dingwall's letter in the January *Journal* calls for little comment, and such as it requires has already been made by Dr Bruck in the March *Journal*. Dr Dingwall is one of your flash pugilists though, it must be pointed out, strictly in theory and in theory only. He is satisfied with nothing less than a knock-out blow in the first round, whether to terrorise his opponent, astonish his audience, or bolster up his own self-confidence, it is not easy to determine. I must confess that, and in practice as well as in theory, I am less ambitious. So long as I gain the advantage in the end, I am quite content to give away a few points in the early rounds, thus giving myself the opportunity of gaining some insight into the other man's methods. For I am interested not only in determining his merits, but also in promoting knowledge of the science in general.

Yours etc.,

THEODORE BESTERMAN.

MISPRINT—*Journal for April*

P. 215, line 12, for "elusive" read "classic."

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on the definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL
OF THE
Society for Psychical Research

NOTICE OF MEETING

A General Meeting of the Society

WILL BE HELD AT

MANSON HOUSE

(26 PORTLAND PLACE, W. 1)

ON

WEDNESDAY, 8 July, 1936, at 6.30 p.m.

WHEN A PAPER ON

“SOME RECENTLY RECEIVED CASES
OF HAUNTED HOUSES”

WILL BE READ BY

SIR ERNEST BENNETT, M.P.

N.B.—*Members and Associates will be admitted on signing their names at the door. Visitors will be admitted on production of an invitation card signed by a Member or Associate.*

NEW MEMBERS

Fitzgerald, Lieut.-Colonel, C. R. L., D.S.O., 5 Parkholme, Fairfield Road, Eastbourne.

Gorle, Mrs G., 64 Dry Hill Park Road, Tonbridge, Kent.

Spender, J. A., Well Hill House, Chelsfield, Farnborough, Kent.

Wisdom, John, 16 Clarendon Road, Cambridge.

MEETING OF THE COUNCIL

THE 341st Meeting of the Council was held at 31 Tavistock Square, London, W.C.1, on Wednesday, 29 April 1936, at 3 p.m., THE PRESIDENT in the Chair. There were also present: Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter and Admiral the Hon. A. C. Strutt, R.N.; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary.

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

Four new Members were elected. Their names and addresses are given above.

The Council expressed their thanks to Lord Charles Hope for the gift to the Society of the special apparatus which was used in the experiments with Rudi Schneider in 1932 (see *Proceedings*, vol. xli, Part 131).

PRIVATE MEETING

THE 141st Private Meeting of the Society was held on Wednesday, 29 April 1936, at 5.30 p.m., the Hon. MRS ALFRED LYTTELTON, G.B.E., in the Chair.

MR G. N. M. TYRRELL read a paper on "Further Research in Extra-Sensory Perception" which, it is hoped, will be published in full in *Proceedings*. A discussion followed in which Dr Dingwall, Mrs Goldney, Mr C. V. C. Herbert and Mr Kenneth Richmond took part.

CASE

A HAUNTED HOUSE

G. 298: VISUAL AND AUDITORY MANIFESTATIONS

The owner of the house, whom we may call "Mr Westfield" (his real name and the address of the house being known to the Society) very kindly allowed a full investigation to be carried out. The phenomena reported were as follows:

Several persons had seen a figure resembling an elderly or middle-aged woman, dressed in grey. The apparition was only seen for a few seconds, and had never been heard to speak. Particulars of the appearances were entered from time to time in a notebook, the dates of the entries not being given. Some of these accounts are as follows:

By "Mrs Westfield":

"I saw the ghost in February 1932 in the night nursery. I thought it was my daughter in a grey dressing gown out of bed, so I was very cross and asked her what on earth she was doing out of bed. She was standing by the fire, bending over it, and instead of answering me she merely vanished! My daughter was peacefully sleeping in the same room."

By "Mr Henry Westfield", aged 21, Mr Westfield's son:

"1930. I cannot remember the date, but it was on a Saturday night, about 11 p.m. I was sleeping in my father's room and was awakened on hearing a noise. I saw what I took to be my sister, but she did not reply when I spoke to her. I took my eyes off her to get my torch, and on looking again she had disappeared. There was a small light in the passage way which made her cast a shadow on my bed."

"1931. November. At about 9 p.m. I went up to my mother's room to get a torch. As I came out of the door I saw a small woman standing in front of the light. I knew at once it was the ghost as I had seen it before: she was old, with a blue grey dress on, which came down to her ankles. She had what looked to be a grey veil hanging from her head down her back, like a nurse's cap. She was standing in front of the lamp, and I could not see the light through her. She turned and walked into the darkness up the stairs."

"1932. April. At 1 a.m. I was awakened on hearing someone walking down stairs. I got up and looked out of my bedroom door, and saw the little woman in grey walking down the front stairs with a light in her hand."

By "Miss Westfield", aged 22 :

"June, 1932. At 1.30 a.m. I awoke with a start and to my surprise I saw standing by my bed the figure of a small woman. She carried no light and was dressed in a bluish grey dress with a tight waist and falling in thick folds over her hips. Her hair was long and brown, hanging over her shoulders. At first I thought it was Mother, and was very surprised that she should walk round the room with no light, for there was no other entrance to the room except through a locked door. I switched on the electric torch and found no one. A few minutes later, Mother came in and said a figure answering to the same description had been seen leaving my sister's bedroom."

By "Miss Rosemary Westfield", aged 16 :

"November 1st, 1933. It was just before lunch when I was entering the hall from the stairs that I saw the ghost. I did not see her distinctly, but she was standing by the round table in her usual clothes, and seemed to me to be very much interested in a bowl of flowers which stood on the table. As I approached her, she walked swiftly and noiselessly away to the other part of the hall, where she disappeared before I could get round the corner."

By "Miss Evelyn Westfield", aged 15 :

"November 1st, 1933. About a quarter of an hour after Rosemary had seen the ghost, I was walking down the stairs, and I saw a lady standing in the doorway of the drawing room. She then walked swiftly through the door and round the table, and after that I could see her no longer. She was wearing her usual blue grey clothes, and I could not see her face. Elizabeth (*i.e.* Miss Westfield) was putting wood on the fire, and did not see the ghost, which was behind her, because she made no noise as she walked."

Various other similar appearances are recorded by the family, some at night, others by daylight. Only on one occasion does the figure seem to have appeared to two persons simultaneously. This was during August, 1935. At about 8 o'clock, while the family and several guests were sitting at dinner in the hall, a young Dutchman present, who had never heard of the ghost, saw a stranger walk into the door, cross the hall, and go upstairs. "Mr Henry Westfield" also saw it, and recognised it as the ghost. A search was made, but nothing could be found.

The most alarming appearance was in 1934. "Mr Henry Westfield" was going upstairs to bed at about 11 p.m., when he suddenly remembered that he had left something behind. He turned round and saw the ghost a few feet away, following him up the stairs.

Apart from these appearances, there are several other phenomena. Heavy footsteps are sometimes heard.

“ Miss Westfield ” writes :

“ August 1933. At about 2 a.m. I was awakened by the sound of footsteps coming down a narrow passage which led to my room. I thought that somebody wanted something, so I sat up in bed and waited. The person appeared to pause at the door and I thought I heard a knock, but I was not sure. I called but there was no reply. The next morning I was told that other people in the house had heard the sound of footsteps walking up and down the passage. Several times since, although I have changed rooms, I have been awakened suddenly in the night with the impression that someone had entered my room.”

There are several reports of a howling or screaming noise being heard, generally by daylight. This noise does not seem to be in any way connected with the wind, being heard as often in calm weather as during a storm. Many people complain that they wake in the night with an unpleasant feeling that somebody is bending over their bed. Sometimes they feel touches on the bedclothes.

“ Mrs Westfield ” writes :

“ About 5 years ago or less I was really terrified. I was very tired and in a deep sleep when I was awakened by my bed shaking and vibrating as if someone was trying to move it backwards and forwards. I half woke and heard someone shuffling round the bed. The person bent over me, and it seemed that someone touched my feet and then began to grope their way up to my face. I was frozen with terror and seized a match box, and as soon as I got a light it stopped. I got out of bed and with a powerful torch searched the room and the whole house, but there was nothing to account for it. It was a dead calm moonlight night.

“ Exactly the same thing happened about three years later, when someone seemed to grope round my bed, and actually feel their way up to my face and press their face close to mine.”

At least two of the people who have reported this phenomenon had not previously been told anything about the ghost, so that suggestion, unless it were a telepathic suggestion, would not account for it.

The old family nurse, “ Miss Johnson ”, reported that she had several times heard heavy footsteps walking along the passages when there was no one there. On some of these occasions, a young nursery maid, who was in the room with her, had heard nothing at all. The nursery maid was not in the least deaf.

During January of 1936, Mr Herbert and Lord Charles Hope

spent two nights in the house. They took the opportunity of questioning very fully all the available witnesses, including the members of the family whose accounts are given above, and "Miss Johnson". They formed the opinion that all these persons were reliable witnesses, and that they were fully convinced they had heard and seen the various phenomena. The house was carefully examined. It is a very old foundation which has been considerably altered and partially rebuilt. The floors seemed to be unusually solid, and there was very little of the creaking of boards so often met with in old houses.

Neither of the investigators experienced anything unusual, but during the second night that they were in the house, "Mr Henry Westfield" reported that he had seen the figure of the old lady on the staircase. Both investigators interviewed him a few minutes afterwards, and were satisfied that his account was perfectly *bona fide*. He was clearly convinced that he had really seen the figure.

The investigators brought to the house the well known clairvoyant, Miss Frances Campbell. Miss Campbell had been told that the ghost was that of a woman, and that footsteps had been heard at night; but apart from this she had been given no material information about the phenomena. During a *séance* that was held, Miss Campbell said that there were several ghosts, one of which was the ghost of a dog. This is of interest, as some time previously "Miss Westfield" had received a "message" with a planchette board, which said that there was a ghost of a bloodhound in the house. Miss Campbell had not been told about this. Miss Campbell described the principal ghost as follows:

"The ghost is an elderly lady. She is quite nice but is rather apt to be frigid with strangers."

[Question: what is the old lady like?]

"It is difficult to say. She has a clear complexion. She is dressed in a dress that is not modern, but not very old. A long skirt."

[Question: hair?]

"It is hidden in a cap. She has small hands."

[Question: what colour dress?]

"Grey dress."

[Question: tall?]

"More than average but not tall. She carries herself with such dignity that she seems taller than in reality."

This description agrees very well with the reported apparition, except that all the people who have seen the figure describe it as short.

The case presents many interesting features, and is probably as well authenticated a ghost story as it would be possible to find. The phenomena are many and varied, and have been witnessed by several people, some of whom are quite independent (*i.e.* they did not know about the ghost) so that suggestion can be eliminated. It seems highly improbable that so many phenomena of such widely different types could have been the result of malobservation or pure imagination. But on the other hand there does not seem to be much evidence that the manifestations have any objective reality. On the contrary, there is evidence that they are subjective. All the members of the family who have witnessed the phenomena are of the type generally known as "psychic": many have had previous telepathic experiences. The only member of the family who did not seem to be at all "psychic", *i.e.* Mr Westfield, has never seen or heard anything unusual in the house. Again, "Miss Johnson" heard the footsteps when the nursery maid could hear nothing at all.

It is possible that the phenomena are all hallucinatory, and that the hallucinations have a cause in common which one may designate as "the ghost", without going further into considerations of what "the ghost" actually is. There is no suggestion that it is the departed spirit of any person.

CORRESPONDENCE

DONATIONS

To the Editor of THE JOURNAL

DEAR MADAM,—May I ask you to print in the *Journal* the sub-joined copy of a letter received from one of our members, who does not desire his identity to be disclosed.

8th May, 1936.

DEAR MADAM,—In lieu of my subscription, I propose vesting £100 in 2½% Consols in the Society—to be treated as Capital. If the Trustees agree to this, will you be so good as to inform me who should be registered as Owners and at what address? I prefer not to be mentioned by name as having done this—though it seems an easy way for members, especially for those getting on in years, to perpetuate their subscriptions.

Yours faithfully,
(Sgd.) —.

The Secy., S.P.R.

In reply to a letter of mine thanking the donor for his generous gift and asking permission to reproduce his letter, he writes: "It may serve to crystallize half-formed intentions to leave some small legacy which, owing to the trouble of a codicil and the reluctance to alter a will, do not eventually materialise. By my procedure all legal expenses, trouble to executors, and claims for death dues are avoided."

May I venture to commend the example thus set to other members of the Society who may wish to help the Society's work in the most permanent and practical way.

Yours faithfully,

W. H. SALTER,
(Hon. Secretary).

MEMORY AND TELEPATHY

To the Editor of THE JOURNAL

DEAR MADAM,—There is one aspect of the problem of telepathy which has never, to the best of my knowledge, been adequately dealt with. This relates to its connection with memory.

Let me first of all refer to one or two generally accepted facts. According to mechanistic physiology, all our memories consist in definite "traces" left upon the nervous system, and particularly upon the brain. These traces have been given various names—*engrams* and what not. These are supposed to be somehow "deposited" in the brain in somewhat the same manner that grooves are cut in a phonograph record: the actual *music* is not left upon the record—only its physical counterpart. And, just as the needle, re-travelling these grooves, can reproduce the music, so renewed activity of these cells, or the passage of nervous currents through them, will re-stimulate and revive these latent memories, causing them to be reproduced and recognized. The analogy is a nice one, giving us food for thought.

It is true that Bergson and others have protested against this mechanistic theory of memory; but it is safe to say that it is held by the great majority of physiologists to-day, in some form or another; and for ordinary daily life it might be held a conceivable possibility—once we overlook the ever-present problem of the connection of mind and brain!

But telepathy (and of course other psychic phenomena) assuredly raise grave doubts as to the feasibility of this currently-held explanation.

All our past experiences must reside within us as memories of some sort, being only recalled when they rise into consciousness. On the mechanistic theory, these memories must consist of "traces" left on the brain, *i.e.*, physiological changes representing "potential" memories only, and not actual mental states or entities—memories proper. These only become such in the act of their recall.

Now it is the commonest thing in the world for a medium to ransack the sitter's memory, and extract from it all sorts of hidden and forgotten events. Instances of this are assuredly numerous. In psychic experimentation, as Miss X expressed it years ago: "what is on the top of one's mind does not tumble out first."

Assuming unconscious telepathic transfer from the sitter, for the sake of argument, or (what seems to happen in such cases) actual "mind reading", what could be the conceivable *modus operandi* involved? Inasmuch as these latent memories of the sitter must represent (on the mechanistic theory) merely physiological brain traces, we should have to assume that the subconscious mind of the entranced medium could somehow reach into the brain of the sitter, find the right *engrams*, interpret them and perceive them to be certain specific "memories", such as a boating trip up the Thames, a picnic, a passage in Kant's *Critique*, and so on. The very formulation of such a conception renders it so preposterous as to rule it out from serious consideration.

Yet it is only these physiological traces which actually exist, according to the mechanistic theory commonly held. The memories themselves do not exist in a mental world *as* entities. If they did we should have a sort of mental reservoir—as Bergson postulated. If such a mental world exists, we should surely have something incapable of being explained by mechanistic physiology. But if it does not exist, then how are we to explain the telepathic transfer or "reading" of such memories? Do not such facts in themselves disprove the current theories of memory, held by practically all psychologists?

We seem to be upon the horns of a dilemma. Perhaps light may be thrown upon this problem by further discussion of it by some of our members? Assuredly it represents an interesting question, which is sadly in need of answering!

Yours etc.,

HEREWARD CARRINGTON.

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—I am afraid that I find myself in substantial agreement with Dr Dingwall in his implied estimate of the Newlove Case.

In the first place there is no satisfactory demonstration that the pipes in question had anything whatever to do with the boy's death. Moreover such pipes as Mr Thomas describes are exceedingly common in hilly districts and I feel confident that had Mr Thomas applied Fedá's vague descriptions to the case of another boy who had died from diphtheria in another district his enthusiasm would have led him inevitably to the discovery of suspect pipes and possibly contaminated surface water into which such pipes usually drip. Further, by giving slightly different interpretations to Fedá's vague and commonplace allusions to "animals", "barns with one side open" "cross roads leading to a dark bridge where trains go what you call 'expectorating'," etc., Mr Thomas would soon have discovered a route leading to the pipes which appeared to fit in with the descriptions at the sittings.

Indeed, almost the only question which I find intriguing in a study of these vague records is the problem of how Mrs Leonard obtained the names "Bentley" and the fair approximation to the name "Catlow." It would be premature however to jump to the conclusion that these names indicate supernormal knowledge on the part of the medium. For all we know to the contrary "Bentley" streets may be fairly common in northern or midland towns; it is at any-rate up to Mr Thomas to demonstrate that they are not. But another possibility suggests itself. I understand that Mr. Thomas had visited Nelson in some year previous to the sittings. May not the names Bentley Street and Catlow have been mentioned casually in his presence—for instance by two persons in conversation together within his hearing? Now if Mrs Leonard uttered names which sounded in some respects like Bentley and Catlow it seems not impossible that the dormant subconscious associations in Mr Thomas's mind might provoke an auditory illusion which would lead him to imagine that the words he heard were "Bentley" and "Catelnow". Such illusions are very common among people with normal hearing.

As to the influence on the general trend of ideas by Mr Thomas's verbal suggestions during the sitting, this may well be quite as serious as Dr Dingwall assumes. For is it humanly possible for even an experienced sitter like Mr Thomas to make quite accurate records of all his own remarks and intonations in addition to recording

accurately the very fluent conversation of Fedá ? My own considerable experience with Mrs Garrett leads me to doubt it. I once made an experiment of allowing *two* shorthand writers to make a record of one of her sittings. A comparison of the typescripts showed that *each* had made omissions of whole phrases and of individual words. In the case of Mrs Leonard a mechanical voice record would seem to be essential before the material is subjected to such a psychological examination as Mr Kenneth Richmond is proposing to undertake. In the absence of such records one must admire Mr Richmond's courage though not his scientific discretion.

Yours etc.,

S. G. SOAL.

To the Hon. Editor of THE JOURNAL

MADAM,—May I be permitted a few words in reply to the misstatements of Miss Margaret Naumberg in the April *Journal* ?

In our tests with Mrs Garrett, under the auspices of the A.P.I., at least seven persons were working over her at every sitting. They were : (1) the reader of the stimulus words, (2) the observer of the galvanometer, (3) the stop-watch time-keeper, (4), (5) and (6), three independent note-takers of the reaction words—to ensure accuracy and agreement—and (7) a note-taker of any physical reactions of the medium and of the general run of events during the séance. This can hardly be called a one-man show !

It was distinctly understood, before we began our sittings, that Mrs Garrett was to sail June 1st, so that her time was strictly limited, and séances had to be crowded in order to get all of them into the available time, as it was.

The galvanometer was under the supervision and in charge of the best technician procurable from the New York Medical Center ; in addition, the machine was carefully observed and inspected by two other experienced laboratory men, and also by the technician sent us from the Central Scientific Company, which manufactures the apparatus. All repeatedly checked and tested the galvanometer, but could find nothing wrong with it. During the next sitting, however, it acted as badly as ever ! The statement is quite erroneous, therefore, that “ there was no observer or technician available to restore it to working order ”.

Against the above concrete facts, Miss Naumberg quotes, by way of proving her statements, the “ memories ” of the medium—and

“ memories ” of events which transpired in her absence, of which she knew, and was supposed to know, nothing !

Very truly yours,

HEREWARD CARRINGTON

To the Hon. Editor of THE JOURNAL

MADAM,—May I contribute to the discussion on the oriental flavour of Feda's word association responses ? I have been engaged in medical work in India for many years and for some of these years in a hill state near Simla.

I disagree with Bishop Western in some points to which he refers in his letter in the April number of the *Journal*. For instance, he says that in North India the association with mango would be not “ hot ” but “ coolness and refreshment ”. This is the European idea, but it is one of the strongest and firmest beliefs of the Indian woman that the mango is “ heating ”. There is, during the mango season, a great outbreak of boils and skin troubles and the women lay the blame on the heating properties of the mango. Reference to “ mango boil ” is found in old-fashioned textbooks of tropical medicine such as Moore's *Family Medicine*.

As regards the remark about “ curry ”, my experience is that the Indian woman in her home usually speaks of “ káři ” (curry) taking the word “ bhát ” (rice) for granted. It is rather the Indian cook serving Europeans who talks about “ káři bhát ”.

It is true, as Bishop Western says, that sáris are generally bought, nowadays. But someone weaves them in the factory. In 1800 there were no factories and the sáris were no doubt woven at home on a hand loom. It is not correct to say that Musalman women “ never wear a veil of any sort ”. The “ burqa ” is only worn outside or when receiving men at home ; in the house, when in the family circle, the Musalman woman wears the loose trousers, shirt and “ chader ” or shawl over the head, which is drawn over the face when she feels shy.

It is true that there are words among the responses which appear unlikely for a woman living in 1800, such as missionary. But, if the assumption is correct, that Feda is the spirit of an Indian woman who passed over many years ago, is it not possible that she has mingled with spirits passing over later and has learned of the enormous expansion of missionary work in her old country and of the association the term certainly has with justice ?

Yours, etc.,

M. I. BALFOUR, M.B.

THE "VITA NUOVA" CASE

To the Hon. Editor of THE JOURNAL

MADAM,—It is refreshing to find that we bid fair to have a collator of scripts among us once more, and I do not decry the value of Mr Kenneth Richmond's masterly paper in saying that I do not think the "threes" episode very convincing.

In the first place this sort of thing is more characteristic of Myers. Then Mrs Salter's earlier script "The bridge and the river—Misericordiam Domini" is very strongly suggestive of Hood's "Bridge of Sighs", borne out by the later script perhaps: "An arch and a bridge over a river—" *not the bridge of sighs, but it is a real bridge*" (my italics) . . . "a triple arch". "A real bridge"—*i.e.* not the poem?

A real triple arched bridge associated with Dante (Florence) is surely *the* bridge over the Arno.

"Triple-arched" at first suggested to me a well-known verse in Keats' "Eve of St Agnes" (a casement high and triple arched there was).

In this poem there are certainly 3 principal characters: it is perhaps a "stretch" to say the same of the Bridge-builders!

So Keats' would have done equally well?

Yours, etc.,

ERNEST S. THOMAS.

THE SCARCITY OF SPONTANEOUS CASES

To the Editor of THE JOURNAL

MADAM.—I am glad to see that the *Journal* is now discussing the scarcity of cases spontaneously offered to the S.P.R. I venture to think that this scarcity may be largely due to the treatment which the bearers of tales about the supernatural too often receive from the officials of the Society. To get such tales sent in, it is necessary to show sympathy and a great deal of tact. In the early days of the Society, Gurney and Myers developed an admirable skill in collecting material. But in recent years the official attitude has changed, and hardly any evidence is now considered collectible. The result is that evidence is no longer offered spontaneously.

There is also a subtle logical error, which has long vitiated the methods of the Society, to which Mr Heaton draws attention in his letter in the April *Journal*. It is at bottom a mistake about the nature of scientific method. The old logic assumed that single cases could be obtained which would absolutely prove a scientific hypothesis. But

nowadays scientific logicians recognize that inductive "proof" is always *cumulative*, and only yields growing *probabilities*. Hence what we should look for is evidence that will gradually grow more probable. But if we insist on using a logical canon which rejects every piece of evidence as "inconclusive", as it arises, we can make no scientific progress.

If our present President could persuade our Secretariat that cases far from "cogent" may yet be worth collecting, and that some of these cases are in various ways interesting and worth publishing (if necessary with critical annotations), I feel sure that the scarcity of spontaneous cases could be diminished, without detracting from the scientific standing of the Society, and with great benefit to its popularity, and so to its numbers and resources.

I am,

Yours faithfully,

F. C. S. SCHILLER.

REVIEW

Towards a Method of Evaluating Mediumistic Material. By J. G. PRATT. Bulletin XXIII, Boston Society for Psychical Research, Inc.

To those of our members who have been feeling a little anxious at the prolonged silence of the Boston Society the publication of this Bulletin, with its foreword by Dr Gardner Murphy, will come as a relief. With the lamented death of Dr Walter Prince it was impossible entirely to repress a doubt whether the Society would be able to continue its work in which he took so great and able a part. Dr Murphy dispels this doubt and shows that although the character of the work may change it is still capable of performing much useful service. This is a matter for congratulation, for scientific psychical research can ill afford to lose any supporter, much less one of the calibre of the Boston Society. Mr Pratt's work is based upon two series of sittings with Mrs Eileen Garrett which took place under the auspices of the Parapsychological branch of the Department of Psychology, Duke University, being part of a programme of research to be carried out under the guidance of Dr J. B. Rhine.

The problem which Mr Pratt sets himself is to discover some method by which it can be determined whether the relevance for the subject of what is said by the medium in trance is greater than can be attributed to chance.

He commences by examining briefly the methods which I adopted

in my report on the Warren Elliott case and he, quite rightly, points to a weakness therein, viz., the method of allowing for the differences in the annotators in respect to the "will to believe", that is to say, to find veridicality in the record, and the "will to disbelieve".

As a matter of historical fact I did not entirely neglect this point, it is mentioned specifically in the preface to my report, but I am happy to confess that Mr Pratt's method of dealing with it seems to me to be a considerable improvement on my own. It is more objective, being based on a comparison of the subject's annotations of a number of records referring to other sitters with those of his own proper sitting; whereas my method was subjective inasmuch as it depended solely on my judgement of the allowance to be made for the peculiarity of each individual sitter in this respect.

In the second series dealt with by Mr Pratt a further step was taken in that the whole set of records was annotated by each sitter in ignorance of which particular sitting was his own. This seems to me to be a most valuable addition to the technique of evaluating mediumistic material and is one which should be adopted wherever possible.

There is not space in a review to deal adequately with the various points raised by Mr Pratt or to attempt to offer any detailed comment or constructive criticism, but there are two points which I feel that I must mention.

First. That the method of itemising the reports of the sittings in such a fashion that the annotations are reduced to "yes", "no" or "unknown" may possibly be considered to be an undue simplification of the material.

I must apologise for again mentioning my own work, but I cannot help thinking that if the sitter be allowed, in fact encouraged, to give full details of the verifying event or circumstances, as was done in the Warren Elliott case, a better judgement of veridicality can be formed. I was inclined to distrust a plain "yes" and always wanted details. Sometimes these details, when furnished, led me to reject the affirmative answer in the case of sitters whose "will to believe" appeared to me to be unduly strong.

The second point is that Mrs Garrett's trance utterances, judging from the samples quoted in the Bulletin, appear to be far more prolific in the matter of names than is the case with most mediums. Where a name or names, together with some five or six descriptive details, is correctly given the anti-chance value may be very high indeed, particularly if the name should be an unusual one. The plain "yes" or "no" scoring hardly allows for this. This point

brings in the question of "grouping" which has assumed such importance in E.S.P. work.

Mr Pratt's method of determining the probability of any particular statement being true for anyone in general is also objective and is based on the annotation of records by persons to whom they do not refer, in a manner somewhat similar to what I called my "chance experiment". Besides actual sitters whole sets of records were annotated by persons entirely outside the experiment. It has the advantage of giving a perfectly definite figure for probability but I am still rather inclined to like the idea of a "Committee". Experience has shown me that talking things over does lead to a clarification of ideas and, as a rule, to some agreed figure for probability. Perhaps the two methods could be combined. Of the actual results of the experiment I will not speak, its importance lies for the most part in the methods.

I think that Mr Pratt is to be congratulated for having made a notable contribution to this important subject, Dr Rhine for his share in the work and for his interesting analysis printed at the end of the Bulletin and the authorities of Duke University, again specially including Dr Rhine, for setting on foot and encouraging these researches. I trust that we may look forward to further communications from the same source.

H. F. S.

NOTES ON PERIODICALS

Bulletin II of the University of London Council for Psychical Investigation (price 5s. net) contains a report by Mr Price on Kuda Bux's fire-walk, with twenty illustrations and a bibliography. This fire-walk, as performed in England last year in the presence of Mr Price and others, differed considerably from the fire-walk as frequently reported from India. Explanations of immunity from burning applicable to the conditions of the Indian fire-walk do not seem to fit the English experiments. In these, for example, there was no top layer of cool ashes to act as a non-conductor; Kuda Bux's feet were not calloused, and so far from their having been moistened beforehand, they seem to have been particularly dry throughout. Mr Price concludes that the dryness of the feet, due to self-suggestion, was the principal cause of Kuda Bux's success in the particular conditions of the test. Perhaps his light weight was a contributing factor.

W. H. S.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 30 September 1936, at 5.30 p.m.

WHEN A PAPER ON

“STATISTICAL AND OTHER TECHNICALITIES
IN PSYCHICAL RESEARCH”

WILL BE READ BY

MR W. H. SALTER.

N.B.—*No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.*

The Rooms of the Society will be closed after Friday, 31 July, until Thursday, 10 September. Correspondence will be forwarded to the staff during this time. The next number of the "Journal" will be issued in October.

MEMBERS are asked to return, renew or exchange Library books before 31 July. Each Member may borrow as many as six volumes for the vacation before the Rooms close.

NOTICE

SUBJECTS FOR EXTRA-SENSORY PERCEPTION

It is desired to carry out some tests in quantitative Extra-Sensory Perception with an entranced subject whose "control" would act as percipient. The undersigned would be grateful to any member of the Society, or to the friend of any member, living in London and able to fulfil this condition, who would be good enough to undertake a series of simple tests.

Also it would be of interest if a child aged from about 6 to 10 could be tested as subject. It is not of course intended to hypnotise the child, for whom the experiment would merely be an amusing guessing game.

G. N. M. TYRRELL, 6 Carlton Hill, N.W. 8.

IN MEMORIAM

We have to record with much regret the death of one Member and two Associates of the Society, all of long standing.

MISS ALICE BALFOUR, a sister of Mrs Sidgwick and of Lord Balfour, was elected a Member in December 1882, the year of the Society's foundation, and came very near therefore to being an original Member. Like others of her distinguished family she combined an interest in psychical research with interest in natural science, and although she had not in recent years taken an active part in the Society's work, she was throughout a keen supporter of its policy and made some contributions to early numbers of the *Journal*.

MISS F. MELIAN STAWELL, who was elected an Associate in November 1908 and an Honorary Associate in 1921, was a classical scholar of much distinction and made valuable contributions in the

fields of literature and philosophy. She was deeply interested in the problems of psychical research, especially in the problem of survival, and was at all times ready to bring her eager and yet critical mind to bear upon any evidence that was presented to her. One of her earliest contributions to our publications was a critical discussion in the *Journal*, Vol. xiv, p. 231, on "The Naples Report on Eusapia Palladino," and she contributed to *Proceedings*, Vol. xxix, p. 260, a criticism of the evidence for survival set forth in Lord Balfour's paper on "The Ear of Dionysius," to which Lord Balfour replied at length, describing her criticism as "of that serious and thoughtful kind that helps to throw light upon a problem even when one cannot agree with it."

I remember Miss Stawell first when she was a classical student at Newnham College and I was still a child. She stands in my memory as an almost legendary figure of prowess in scholarship, having been one of the small band of women to be placed in Class I, Division I of the Classical Tripos. One of my last and clearest recollections of her was at the Jubilee Meeting of the Society in July 1932, when she told me how deeply impressed she had been by Mrs Sidgwick's declaration (made on her behalf by Lord Balfour) of belief in survival and the possibility of communciation with the dead, and how greatly she regretted that her old friend Lowes Dickinson had not been able to hear it made. Lowes Dickinson died in August 1932 and Miss Stawell contributed an obituary notice of him to the *Journal*, Vol. xxvii, p. 322.

MR E. S. THOMAS, Curator during the last years of his life of the Pitt Rivers Museum, Oxford, was elected an Associate in December 1907. He made frequent and valuable contributions both to *Journal* and *Proceedings*: among these may be mentioned "A Dream Memory Recovered after many Years," *Jour.*, Vol. xxi, p. 296, "Notes on a Sitting with Mrs Brittain," *Jour.*, Vol. xxvi, p. 135, "A Series of Mediumistic Statements made to four Sitters," *Jour.*, Vol. xxvii, p. 74, "The Fire Walk," *Proc.*, Vol. xlii, p. 292. He also contributed numerous reviews and letters, mainly concerned with the question of survival in relation to telepathy. Officers of the Society can testify to the kindly and willing help he gave them whenever occasion arose.

He was at the time of his death President of the Oxford Society for Psychical Research and took part in the recent investigation at Oxford into the phenomena of "The Flower Medium."

H. de G. S.

CORRESPONDENCE

PROXY SITTINGS AND A QUERY REGARDING D. D. HOME

To the Hon. Editor of the JOURNAL

MADAM,—I fear that my letter in the April *Journal* regarding the proxy sittings produced the emotional reactions that I hoped would be avoided. I tried not to give offence to the faithful, asking if I might “venture to suggest” and mentioning a principle, which “I suspected” ran through the mental phenomena. Nevertheless, in spite of these tentative feelers, Mr Richmond says that I make declarations and Miss Gibbes tells me that I have made astonishing assertions which are of the most preposterous kind.

My friend Mr Richmond evidently thinks that the evidence for intention on the part of the communicators in the pipes episode of the Newlove Case is strong, but I regret that I do not agree with him. Neither do the cases mentioned by Miss Gibbes appear to me to demand the importance that she attaches to them; although I admit freely that I am not acquainted with the full details. In the first instance it would seem that consultation of an obituary notice might be a good start.

I do not intend in this place, however, to ask you to print the evidence on which I base my suspicions. My original letter was intended rather as a hint to those of us still unconvinced, in spite of the flood of spiritualistic propaganda, however academic its modern appearance may be.

And now, Madam, may I be permitted to crave the assistance of your readers in another matter. But let me assure them in advance that my query is not intended as an example of what is, I understand, now so inelegantly styled the “Debunking of Daniel”.

For some months past I have been trying to discover where the famous levitation of D. D. Home took place on that memorable evening of December 13th, 1868.

Lord Dunraven tells us that it was at Ashley House, Victoria St., London (see *Proc. S.P.R.*, XXXV, p. 153). He stated the same thing in the *Weekly Dispatch* of March 21st, 1920. It will also be remembered that the heading of the report in the *Experiences* states that it was at 5 Buckingham Gate.

What does not seem generally known, however, is the fact that Lord Dunraven seems to have sent a written account with plan to Sir F. C. Burnand, in whose archives it was preserved. Here we learn that the rooms were on the first floor and that the windows faced one another, being on opposite sides of a triangle. In the *Experiences* we are told that the rooms were on the third floor

(*op. cit.*, p. 156) and there is no mention of the position of the windows.

Now I am unable to discover if any structural alterations have been made in Ashley House since 1868. The fact remains that to-day there does not seem to be any arrangement of windows to correspond with the plan mentioned above ; nor does this arrangement seem to exist in Buckingham Gate.

Can any of your readers, therefore, let me know if they remember any such arrangement at Ashley House ? If not, how are we to account for these discrepancies, apart altogether from the new light that this arrangement of windows sheds on this nineteenth century miracle ?

Yours etc.,

E. J. DINGWALL.

THE NEWLOVE CASE

To the Hon. Editor of the JOURNAL

MADAM,—In the *Journal* for June my friend Mr S. G. Soal writes about the Newlove communications. He makes a special point of the possibility that subconscious associations provoked in me an auditory illusion which led me to imagine that the words spoken in the sitting were *Bentley* and *Catlenow*.

The obvious inference would be that the evidential value of these names is suspect.

The facts can be quite simply stated. These names were *not taken down by me*, but by the expert stenographer who had, at that date, accompanied me to Leonard sittings for exactly four years. It was she who set down the names as she heard them pronounced, and they meant no more to her than they did to me.

It is regrettable that so experienced and conscientious a researcher as Mr Soal should reject sober facts in favour of easy but misleading speculations.

Yours etc.,

C. DRAYTON THOMAS.

THE STUDY OF THE PSYCHOLOGICAL ASPECTS OF MRS WILLETT'S
MEDIUMSHIP

To the Hon. Editor of the JOURNAL

DEAR MADAM,—Lord Balfour has very kindly given me permission to send to you for publication in the *Journal* the most interesting letter which he wrote to me in reply to mine which appeared in the March number. He himself does not wish to dis-

cuss the subject any further as he thinks it all so obscure and difficult that no advantage would be gained thereby. I feel sure, however, that all your readers will welcome the opportunity to learn more of his views on this most important subject.

Yours faithfully,

H. F. SALTMARSH.

AN OPEN LETTER TO MR SALTMARSH

DEAR MR SALTMARSH,—I have read with much interest your letter in the *Journal* of the S.P.R. for March 1936 on the subject of my recent Paper on the Psychological Aspects of Mrs Willett's mediumship. As you truly say, the Paper raises questions of importance not only for Psychical Research, but also for Psychology and Metaphysics.

What you call the Monadic Theory of Human Personality may take more than one form according to the extent of the scope claimed for it. But even in its most limited form it is bound to provoke the kind of questions you ask. I will do my best to provide the further enlightenment for which you plead. But the subject is obscure, and I have to confess that the conundrums you propound leave me also for the most part guessing, and unable to do more than make suggestions or indicate lines of thought.

In the limited form which it assumes in my Paper the hypothesis consists in substituting a group of true selves or monads telepathically interacting with each other, for the single unitary self of the commonly accepted view. In the normal human being one of these selves is dominant and in sole control of the organism as a whole. This dominant monad the hypothesis identifies with the "me as I know myself." Your first question is, Who then are the subordinate monads, the other conscious selves? Are they also *me*, but not me as I know myself?

I may have misapprehended the precise point you wish to raise, but surely this is not one of the many comparatively speaking side issues started by the hypothesis so much as a request to make the hypothesis itself more explicit. If so, I can only repeat what I hoped I had made sufficiently clear in my Paper, that the subordinate selves are none of them the "me" as I know myself, but in virtue of their association with the dominant me and with the organism they are, together with the organism, contributing factors in the personality which finds its resultant expression in the individual man. Each of the monads I conceive to be a "me," conscious of itself as in close relation with the organism, but not in control of the organism as a whole. The subordinate members of the group interact telepathically with the dominant self, and presumably with each

other. But it has to be admitted that in normal experience—dreaming may be an exception—the dominant self is not conscious of the interaction as an active influence, nor aware of the existence of other psychical units associated with itself and capable of being a telepathic source of modifications in its own thought-content. It is one of the serious weaknesses of the hypothesis that the evidence in favour of it rests so largely on the phenomena of abnormal psychology.

You next ask whether the hypothesis assumes that the subordinate selves are capable of existing, in any circumstances, apart from the association. In my view the hypothesis neither assumes capacity for independent existence on the part of the subordinate selves nor excludes it. Whether even the “me as I know myself” can exist independently of the body must still be regarded as an open question. To decide whether the subordinate monads can have an independent existence apart from the body, from the dominant self, and from each other, must be a yet more difficult problem. Monadic pluralism might still hold good of human personality whichever alternative we adopt.

On p. 275 of my Paper¹ I have permitted myself a bold excursion into the realm of conjecture. The passage I refer to follows one which you yourself have quoted in your letter. I give the paragraph in full:

“My own instinctive conviction is that my true self is the ‘me as I know myself,’ and that it will be as this *me*, with its formed character and stored memories, that I shall survive, if survival there be. Other selves co-conscious with ‘me as I know myself’ may also survive. If so, it is permissible to hazard a guess that as they have apparently co-operated when in the body, so they, or some of them, may continue to co-operate when no longer in the body, and that each of us may find himself in the ‘metetherial’ world the member of a group of selves with which he has already, when in the body, been associated in closer relation than with the rest of his environment.”

Speculations of this kind are evidently fanciful.

Your third question is, “Are the subordinate monads permanent members of the association, or is there a process of change there analogous to that of cells composing a living organism? . . . If so, where do they go, and whence do the new ones come?”

Here again it seems to me that any attempt to answer the question must, in our present state of knowledge, rest upon pure conjecture. You refer in this connection to a suggestion put forward by Professor McDougall in his Presidential Address (but not, I think, repeated in his *Outline of Abnormal Psychology*), that the dominant monad

¹ S.P.R. *Proc.* 1935, Part 140, Vol. XLIII.

is "not only the ruler of his society, but also the patriarch and progenitor," and that "the monads are propagated by a process analogous to budding." This suggestion may not be wholly wide of the mark, but for myself I prefer to take refuge in a confession of complete ignorance. I may, however, observe that the question itself is more apposite to the form of the hypothesis as expounded by McDougall than to the more limited form of it which is all I have argued for in my Paper. This same remark applies also to your next two questions. This will appear more clearly when I come to your question (5).

Question (4). "The dominant monad" you say, "being a complete self, possesses cognitive and affective-volitional faculties, it has its own proper train of memory. Does it perceive the external world directly? Can it employ the entire mechanism of the physical, including the sense organs, or does it make contact with its external environment only by deputy through the subordinate monads?"

When one mind apprehends the thought of another mind, I should regard the perception as being direct, but your question refers to perception of the external world. Sensory perception would certainly appear to be mediate, though even here I should hesitate to make the dogmatic assertion that no element of direct apprehension by the dominant monad enters into the process.

Question (5). The subordinate monads, you say, being selves "presumably enjoy possession of the full complement of mental faculties: do they also have full run of the physical organism or are they specialised as to function? I gather from Prof. McDougall's account that the latter view is correct, so that the subordinate monads should, strictly speaking, be considered as less than complete selves, ranging from those in the hierarchy nearest to the dominant, which would be nearly complete, down to the lowest orders whose specialised function is, say, some simple reflex."

This paragraph of your letter raises far-reaching questions, of which I was well aware, though I excluded consideration of them from my study of Mrs Willett's mediumship. By way of comment I should like to begin by referring to a passage near the end of my Paper. After summarizing the constituent factors in human personality as consisting of the dominant self, the subordinate selves, and the bodily organism, I continue as follows:

"I make no attempt to carry my analysis further, or to try to imagine in detail how the different factors in human personality work together to produce unity and order. Any such attempt would involve an enquiry not only into the relation of mind to mind, but of mind to body, with the metaphysical problem of the relation of

mind and matter looming in the background. In this Paper I have confined myself all but entirely to the direct relation of mind to mind, a subject the systematic investigation of which may almost be said to date from the foundation of the Society for Psychological Research. . . . I have tried to find a place for the principle of telepathy within the structure of human personality ; and convinced as I am that the true explanation of the lower is to be sought in the higher, and not of the higher in the lower, I am not without hope that the same principle, in a modified form, may be found applicable to the relation of mind to body also."

It will be understood from the above passage that I deliberately restricted the scope of the monadic hypothesis *so far as the treatment of it in my Paper is concerned*. The monads I had in view were minds, true selves, self-conscious Egos, comparable in every way to the "me as I know myself," and capable of telepathic communication with each other similar to that which we have valid grounds for believing to take place between different individual human beings, and some evidence for believing may take place between incarnate and discarnate minds. By thus limiting the application of the hypothesis I escaped the task of dealing with the difficulties raised by your question number 5, and inherent in the conception—which inevitably suggests itself—of a hierarchy of monads arranged in a descending scale of mentality, whose lowest members are hardly, if at all distinguishable from the elements that form the material organism. I readily admit the formidable nature of these difficulties, but anything like an adequate examination of them would require knowledge to which I have no pretension, and involve an incursion into metaphysics which cannot be attempted here.

I am glad to note that in spite of the perplexing questions which the monadic hypothesis suggests to you, you are not disposed to reject it off-hand, but on the contrary seem to think that a *primâ facie* case has been made out for it. Indeed you go so far in the direction of approval as to consider it worth while to propose an amendment in the scheme in its present form in order to meet what you feel to be a serious objection to one particular point in it. You accept the identification of the "me as I know myself" with the dominant self, *i.e.*, with the self that is in general control of the body, but would superpose upon it other selves as inferior to it in the mental scale (measured by the possession of supernormal powers), as the dominant "me" is to the subordinate monads. "The weight of evidence points," you say, "to there being a certain amount of dissociation in the large majority of cases of supernormal mental phenomena," and therefore to these powers being in the main exercised by other selves

than the "me as I know myself." And the inference you draw is that these other selves must be regarded as superior rather than subordinate to the dominant self in virtue of their possessing telepathic faculty in a higher degree.

I content myself with indicating what appears to me to be one weak point in this reasoning. You are evidently thinking of telepathic communications between minds external to one another in the sense of belonging to different individuals—what I have called in my Paper "exterior communications." But it is of the essence of my hypothesis that "interior communications," *i.e.*, communications between selves associated together in the same body, are also telepathic. There is no ground for believing that—as regards the interior, or inter-monadic, telepathy—the dominant monad is inferior in capacity to the subordinate monads. The contrary would rather seem probable. To make your inference valid; the dictum that the capacity for exterior communications is exhibited in a higher degree by the subordinate monads than by the dominant monad would have to be supplemented by the further dictum that exterior communications imply a higher mental activity than inter-monadic communications.

The amended version of my hypothesis is not inconsistent with its central tenet, namely that co-conscious selves must be regarded as distinct psychical units, and not as strata or phases of a single self. But frankly, it does not commend itself to me. So far as concerns general control of the body the superiority of the dominant self is undisputed: its inferiority in the matter of telepathic faculty is uncertain and may be only apparent. If your monads of the highest rank are not in some sense subordinate to the dominant monad, it is difficult to see in what their association with the body can consist. They would seem to be something in the nature of guardian angels.

I fear I have troubled you with a long and not very illuminating screed, but your sympathetic reception of my ideas is my excuse for inflicting it on you.

Believe me to be,
Yours sincerely,
BALFOUR.

THE STUDY OF LEONARD COMMUNICATIONS

To the Hon. Editor of the JOURNAL

MADAM,—Surely Mr Soal's may-have-beens will have occurred to any critical reader of the Newlove case. I need only note that he builds from one assumption to another, all of them dependent on a

further assumption that the records are faulty in not containing evidence for what he assumes. His positive and useful contribution lies in the observation that "such pipes as Mr Thomas describes are exceedingly common in hilly districts." This is a factor similar to the commonness or rarity of a Christian name mentioned by a medium.

Mr Soal's general suggestion, that it is useless to undertake a psychological examination of the Leonard material in the absence of complete voice-records of everything that passed at the sittings, is not nearly complete as a counsel of perfection. If we had such records, they would then be insufficient without a drum-record of the psycho-galvanometric reflex, and so on. But personally, I find much more ground for suspecting gaps in my own critical judgment than in the best of the records which we possess. And my own experience, and that of others, is not that the less accurate the record is, the better the ostensible evidence appears to be. With increased signs of accuracy and completeness in the records that we have, the evidential links appear to be more complete and coherent, and the may-have-beens fail to materialise.

I can bear to dispense with "scientific discretion" if it means that an imperfect instrument like the human investigator is not to make imperfect judgments from imperfect data, in an imperfect world. If it will comfort Mr Soal, let me assure him that I make no pretensions to a more exact method than is possible. His criticism is in essence perfectly sound, as far as it goes without supporting evidence—except in that he tends, I think, to cultivate a blind eye for the sequence and constructive effect of what comes to expression at a sitting; but he appears to be exaggerating a useful attitude of mind into an opinion which would eliminate trial and error in research and, in practice, simply barricade the path to a better understanding of our material.

I am glad of the opportunity not only to agree with, but to emphasise, the point in the late Mr Ernest Thomas's kindly letter on the *La Vita Nuova* case. It would have emphasised itself, perhaps, in a different way but for a slip by which he has described as "the later script," of Mrs Salter's, what in fact was the earlier of the two being described in my paper as "a much older script of hers (Feb. 24th, 1916)"—i.e., more than two years before the Leonard sittings which supplied the *La Vita Nuova* material. It would have been an absurd straining of associations if I had read into this script a prospective reference to the subsequent book-tests, and I understand that it has a quite different relevance of its own. This does not alter the fact that this script, with the phrase "triple arch," was recalled to Mrs Salter's mind by her later script, contemporary with the *La Vita Nuova* book-tests (which script Mr Thomas has spoken of as

the "earlier"): nor I think does it matter that this later script could be applied to Hood's "One more unfortunate"—or to any other context containing bridge, river, and pity. There is no reason so to apply it, and there is some reason for its application to the "bridge" references at the Leonard sitting. Further, it is a fact in evidence that the application was made, whether or not a communicator intended it to be made.

Yours etc.,

KENNETH RICHMOND.

THE SCARCITY OF SPONTANEOUS CASES

To the Hon. Editor of the JOURNAL

MADAM,—I do not know on what grounds Prof. Schiller supposes that "a subtle logical error has long vitiated the policy of the Society," resulting in a failure on the part of the Society's officers to recognise the cumulative value of inductive "proof." They may all be presumed to be familiar with Chap. iv. of *Phantasms of the Living* in which the following passage occurs (§ 20):—

"It has been urged that no accumulation of instances can make up a solid case, if no individual instance can be absolutely certified as free from flaw. But the different items of inductive proof are, of course, not like the links of a deductive chain. The true metaphor is the sticks and the faggot; and our right to treat any particular case as a stick depends, not on its being so flawlessly strong, as evidence for our hypothesis, that no other hypothesis can possibly be entertained with regard to it, but on the much humbler fact that any other hypothesis involves the assumption of *Something in itself improbable*. Third-hand ghost-stories, and the ordinary examples of popular superstitions, have no claim to be regarded as sticks at all, since the rejection of the popular explanation of them involves no improbable assumptions of any kind: at best they are dry reeds, and no multiplication of their number could ever make a respectable faggot."

Individual opinions will naturally vary as to the exact distinction between a stick and a dry reed, but a perusal of the spontaneous cases printed in the *Journal* of recent years, e.g. since the War, does not suggest that the Society is attempting to set up an excessively high standard of faggot-value.

It should be remembered that Gurney and Myers made spontaneous telepathy a live issue, a matter of widespread interest and discussion among the educated public. Many were convinced by their argument, and unless they were members of our Society, became inclined to regard the accumulation of further instances as super-

fluous. Others, unconvinced, continued to attribute cases of this type to chance or other normal causes. It cannot, I think, be doubted that, outside our membership, spontaneous telepathy no longer arouses educated interest as it once did.

But when, by the recent B.B.C. talks, contact was made with a wider public, cases flowed in freely without, to the best of my recollection, any complaint from the correspondents of want of tact or sympathy on the part of "our Secretariat."

Yours etc.,

W. H. SALTER (Hon. Sec.).

[Note: Although a reasonable standard is maintained for cases printed in the *Journal*, it is the practice of the Society to collect and file *any* evidence, either in support of or against paranormal hypotheses. This material, with a few exceptions, may be consulted by Members and Associates at Tavistock Square.—C. V. C. HERBERT.]

REVIEWS

ALEXIS CARREL. *Man the Unknown*. Pp. 346. London: Hamish Hamilton, Ltd. 1935.

The author of this book is a scientist of world-wide fame, but, as he tells us in the first words of his preface, he is "not a philosopher". Yet many of the topics discussed are essentially subject-matter of philosophical thought. The author's assertion that "man in his entirety is located within the jurisdiction of the scientific technique" will not be accepted by everyone, if it is implied that scientific technique will prove adequate to the understanding of man in his entirety. If, however, it is merely a declaration that in the investigation of man in his entirety the methods of science should be utilised to the utmost, no scientist and no philosopher need object.

The aim of this book is to summarise all that the various sciences have taught us regarding man's nature and to synthesize the fragmentary aspects revealed by each separate form of investigation so as to give us some inkling of what "man in his entirety" is like. The record of what science has taught us of man's nature is very fully told, although only in outline, and the reader's first impression is one of wonder, not at what is unknown in man, but at the enormous mass of "scientific data concerning the human beings of our time". This is especially true of the chapters dealing with the body and physiological activities, and in the account of the adaptive functions which are essential for man's life and development. Concerning these departments of knowledge the author's complaint is not so much that we do not know enough, but that we fail to make use of

the knowledge we already have, with the result that man is not even now all that he might be.

Dr Carrel's exposition of the science of man in so far as it relates to the body and physiological activities is easily recognisable as the work of an expert, but when he comes to deal with mental activities and the mind's place in nature the hand of the amateur is revealed. He reiterates many times his opinion that psychology is not a science, although in the end he so far forgets himself as to refer to it as "the supreme science". It would seem, however, that he has little acquaintance with modern psychology, and little understanding of the part it is destined to play in that "remaking of man" which he feels to be so necessary for the future of humanity.

He makes an arbitrary division of mental activities into intellectual, moral, aesthetic, and religious. Under intellectual activities he gives a prominent place to intuition as a means of acquiring knowledge. In one aspect, he says, intuition takes place quite independently of observation and reasoning, and adds that "this mode of knowledge is closely analogous to clairvoyance, to the sixth sense of Charles Richet." He expresses his firm belief in the occurrence of clairvoyance and telepathy and tells us that his belief is based upon his own observations and experiments rather than upon the opinion of others. Indeed he would seem to have but a poor opinion of the work of others in this field of investigation, and he makes the astonishing claim that "experimenters trained in clinical medicine . . . are alone qualified to investigate this subject."

In writing of religious activities, which he regards as synonymous with mystical activities he gives expression, here also, to opinions which are far from being generally accepted in the world of science. He tells us that he became interested in asceticism and "mysticity" at the same time as in metapsychical phenomena, that he has known a few genuine mystics and saints, and that he considers it impossible not to count mysticism among fundamental human activities. He believes that certain spiritual activities may cause anatomical as well as functional modifications of the tissues and organs; as, for example, in the miracles of Lourdes. Unfortunately he presents none of the evidence on which his belief is based.

The main theme of the book is that whereas the sciences of inert matter have made immense progress, those of living beings remain in a rudimentary state. The directions in which science has developed have led to "the brutal materialism of our civilization" and modern conditions of life have brought about a "state of intellectual, moral and physiological atrophy".

Dr Carrel is writing as a citizen of America and he takes as a text

for his thesis on the need for "the remaking of man" the sudden collapse of "the superb edifice of American finance and economics". It is in this connection that he asks: "Has not modern life decreased the intelligence and morality of the whole nation"? and he thinks "the day has come to begin the work of our renovation". And notwithstanding his insistence on our ignorance of man's true nature and being he declares that "to-day the science of man gives us the power to develop all the potentialities of our body. We know the secret mechanisms of our physiological and mental activities and the causes of our weakness. We know how we have transgressed natural laws. We know why we are punished, why we are lost in darkness. Nevertheless we faintly perceive through the mists of dawn a path which may lead to our salvation."

This book has been written with a purpose, "for those who are bold enough to understand the necessity, not only of mental, political and social changes, but of the overthrow of industrial civilisation and of the advent of another conception of human progress."

T. W. MITCHELL.

ADMIRAL TELEMACHUS KOURMOULIS. *The Hallucinations of Logocratia*. Pp. 243. Athens: Kyklos. 1936.

Admiral Kourmoulis's book does not indeed touch very closely on psychical research, but treats of philosophical, psychological and ethical problems, which every student of psychical research has probably meditated upon. In the preface the author defines Logocratia as "every period in the world including the period in which we are living now . . . during which philosophising people are not able to discern the existence in us of any other data of purely cognitive purpose than the data of experience." The only thinkers of importance who have been free from the fallacies inherent in most philosophical systems are, according to the author, Herakleitus and Buddha, who were nearly contemporaneous. A considerable part of the book is devoted to an analysis of the obscure sayings recorded of them, and an attempt to prove that their teaching proceeds upon parallel lines. To the "Logocratia mentality" are attributed all the evils of our present civilisation.

W. H. S.

NOTES ON PERIODICALS

Tijdschrift voor Parapsychologie: May, 1936.

Electric Phenomena of the Human Body (Dr G. Oppenheim, Frankfurt). The author attempts to show that the phenomena of telekinesis are due to electric emanations from the human body.

A Prophetic Dream (Dr P. A. Dietz). A case of a dream, dreamed two days before an event which corresponded with it in several details. [A translation of the case can be seen at the Society's rooms.]

Telepathy between Animals and Men (Dr D. A. A. van der Lek). The author gives some interesting cases pointing to the probability that there can be telepathy between animals and people—in particular, a case where the mistress of a dog claims to have been telepathically warned by the dog of impending danger to her house and children.

H. E. K.

Journal of the British Society of Dowsers. 1935, September.

Mr Cecil Maby contributes a paper entitled "The Psychological Element in Dowsing and Allied Phenomena". Mr Maby considers and rejects the purely physical explanation of dowsing. He describes various instruments, said by their inventors to respond to hitherto unknown physical influences, some of which he has constructed and tested, with negative results. Mr Maby accepts the phenomena of dowsing, and holds the view, which is shared by many para-psychologists, that they are due to psychic and not physical causes. He does not, however, bring forward the strongest of all the arguments in favour of a psychic explanation, *i.e.* the apparent success of experiments in dowsing from maps, in which, of course, the physical explanation is completely ruled out.

ERRATUM—*Journal for May*

P. 234; line 8, for "Dayton" read "Drayton."

THE *JOURNAL* IS PRINTED FOR PRIVATE
CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on the definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL

OF THE

Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 28 October 1936, at 5.30 p.m.

WHEN A PAPER ON

“THE SIGNIFICANCE OF BOOKTESTS”

(Lantern Slides will be Shown)

WILL BE READ BY

THE REV. C. DRAYTON THOMAS

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

OBITUARY

SYDNEY CHARLES SCOTT

By the death of Sydney Charles Scott on September 18, at the age of 86, the Society has sustained a loss far greater than most of our Members and Associates can have realized, for, with few exceptions, it is only members of the Council who have been in a position to appreciate the value and the full extent of his services.

His membership of the Society, which dates from October or November, 1884, lasted nearly 52 years.

Co-opted as a member of Council on March 6, 1891, he served on it until his death—a period of over 45 years.

He was appointed a member of the Finance Committee (now called the House and Finance Committee) in January 1892, and remained a member of it till the end of 1934, when increasing age made it necessary for him to limit his activities.

He was one of the trustees of the Endowment Fund from the formation of the Trust down to the time of his death.

On January 31, 1922, in recognition of his services to the Society he was elected a Vice-President.

This dull and formal record will convey little to the reader; nor can I hope to do more than give some indications of the amount of work that he did for the S.P.R., and of his efficiency and generosity.

While I cannot state it as a positive fact, I believe it to be the case that the Council have more often placed on record their gratitude to Scott than to any other member past or present. Thus, to cite some instances:—

At the Annual Business Meeting held on January 25, 1895, “thanks were accorded to Mr H. Arthur Smith and Mr Sydney C. Scott for the work they had done [in connection with the Incorporation of the Society] up to that time. The indebtedness of the Society to Messrs Smith and Scott is greatly increased by the efficient manner in which the Incorporation has been carried out, and by their kindness in saving much expense which would otherwise have been unavoidable”.

On November 1, 1895, the Council record “the great indebtedness of the Society” to Mr H. A. Smith and Mr S. C. Scott “for the large amount of time and labour which they have devoted to carrying through the Incorporation of the Society, and which has enabled it to be completed at much less cost than would otherwise have been the case”.

On December 12, 1922, the Council placed on record “their appreciation of Mr Scott’s generosity in conducting the legal

business relative to the acquisition of the Society's new premises [31 Tavistock Square] without charge to the Society ”.

In the Annual Report for 1923 gratitude is again expressed to Mr Scott for carrying out other legal business free of charge.

Once more, on March 18, 1931, the Council accorded warm thanks to Mr Scott “ for the trouble he had taken and the expense he had saved the Society ” in connexion with another matter, and also “ for general legal advice ”.

Finally in the Annual Report for 1935 we read with reference to a renewal of the lease of 31 Tavistock Square : “ As on many previous occasions the Council wishes most cordially to thank Mr Sydney Scott for conducting the legal business involved ”.

Yet the services for which Scott received the formal thanks of Council or of an Annual General Meeting by no means exhaust the list, for he rendered *many* others known only to successive Hon. Secretaries and to some of the other officers of the Society.

Whether Scott ever charged the Society for legal work I do not know, but I am quite certain that, if he ever did do so, it was but very rarely.

Yet it is not so much to his generosity in money matters that I want to draw attention, as to the generosity with which he—a man with a large professional practice—devoted much time and thought to the concerns and interests of the Society. I can only hope that the Society will be fortunate enough to find a successor to Scott, who will do what he did for it during 45 years with the same efficiency and disinterestedness.

As a member of Council he did not often take part in discussions ; but when he did intervene, in his characteristically quiet manner, it was seldom that his advice was not accepted.

Besides sound knowledge of the law, he was possessed of much shrewdness and common-sense ; but the quality which, more than any other, must, I should think, have struck most people who came in contact with him was his kindness.

Some who read this attempt to explain how much the Society has owed to Sydney Scott, may fancy that, in accordance with the not uncommon habit of writing with exaggerated politeness of the dead, I have magnified the value of his work. Disliking exaggeration in any matter, and especially in what comes under the head of Obituary Notices, I have to the best of my belief written nothing which the facts do not justify.

September 23, 1936.

J. G. P.

As regards the period since the War, I wish to confirm most

strongly everything Mr Piddington has written as to the value of the services Mr Scott rendered the Society, and as to the generosity with which, on occasions too numerous to specify, he unsparingly placed time, trouble, sagacious judgment, and professional knowledge at our disposal without any charge on our funds. W. H. S.

L. 1309.

CASE.

LOST OBJECT FOUND AS RESULT OF A DREAM.

The following case was received by Mr Saltmarsh, as a result of his letter in *The Spectator* :

Letter from Mr X (whose name and address is known to the Society) dated 1936, January 20 :

“ I send you herewith an account of an occurrence which took place in July last.

“ One afternoon my wife and I took a run into the country, with the intention of having a picnic and the expectation of a little fishing for myself.

“ On arrival at the place I left my wife near the bridge which crosses the stream and went off fishing. The stream runs through a wood, hardly a mile long, but very dense in parts and thick with undergrowth. There are no proper paths, as it is very little frequented, being far away from any village. On returning about two hours later, happening to put my hand in a pocket, I found a hole and that a small key which locked a drawer where I kept certain papers was missing. When and where the key had fallen I could not say. If here in the wood, there was no use looking for it. It would be worse than looking for a needle in a haystack. So the matter dropped.

“ Next morning my wife said she had had a dream. In her dream she was in the wood sitting on a fallen tree trunk, and near a mossy stone she saw the key.

“ So impressed was she with the reality of the dream that in spite of my dissuasion she went there again that day, went through the wood, guided as it were by some force, until she saw the tree of her dream. Then she sat down on it for a few minutes, and then saw the key beside the stone exactly as she had dreamt. I was greatly impressed when she came home in the evening with the key. She had not gone through the wood previously and had no knowledge where I had gone.

“ I may say that my wife has told me before of like happenings, but this one I can verify as being absolutely true.”

Mrs X writes : " This is to confirm that all my husband has written regarding the loss of his key, together with my dream and the finding of the key, is quite correct."

This case is of great interest, in that the dreamer was not present when the article was lost. One hears of so many cases where the loser himself finds the lost object, as the result of supposed clairvoyance, when normal unconscious memory of the loss would provide a much more probable explanation. Here, one is tempted to consider the possibility of normal unconscious memory of the loss being transmitted telepathically from Mr X to Mrs X.

A NOTE ON " RADIAESTHESIA " IN FRANCE

BY COUNT PEROVSKY-PETROVO-SOLOVOVO

DR ROBERT RENDU, a former hospital-attendant at Lyons, has published in the July-August number of the *Bulletin* of the Société médicale de Saint-Luc, Saint Côme et Saint Damien (61 rue Madame, Paris 6^e), a paper on " Radiesthésie, Science et Morale " since reprinted in pamphlet form and which will well repay perusal. (" *Radiesthésie* ", it should be added perhaps, is in France the fashionable term for dowsing and pendulum performances, whilst the *Société* in question is a Roman Catholic medical society.)

There are two chapters in Dr Rendu's pamphlet. With the second (" *Radiaesthesia and Morals* ") I will not concern myself, though it too contains valuable observations and considerations of interest. For our immediate purpose it will be enough to reproduce the author's conclusions at the end of chapter first on " *Radiaesthesia and Science* " :

" As far as I am concerned, I have now been trying for more than twenty years to submit dowisers' (" *sourciers* ") statements to experimental control, convinced as I am that whilst nothing must be denied *a priori*, nothing must be on the other hand asserted without proof. If so far as radiaesthesia is concerned, we retain but those facts which can be easily and immediately controlled ; if we are attentively on the watch so as to prevent the dowser from availing himself of any extra-radiaesthetic data ; if we take care to undertake experiments of a simple character and apt to be easily repeated ; if especially we take into account the percentage of successes due to the laws of chance—we are driven to the conclusion that hitherto, at least, the results obtained by those radiaesthesists who did consent to submit to a scientific control are to a great extent identical with those derived from the plain calculus of probabilities " (p. 22).

These conclusions—they will not please everybody, the author admits—are supported by a mass of evidence which certainly tends to throw on the (French) radiacsthesists' claims a lurid light. Cases suggesting insincerity are—to me—the most disheartening. These are but too numerous. And even when we are not compelled to postulate *complete* insincerity, there are distinctly unpleasant incidents. There is, *e.g.* at Lyons, we are told, a friar, who has specialised himself in pendulum researches over plans and has made in the same way marvellous discoveries in the realm of biology and medicine. Dr Rendu wants to make with him conclusive experiments. Alas, all his proposals are declined, and at last he is told: "My competence is universally known, and I do not wish to humiliate myself so as to make experiments." To this Dr Rendu replies that never would a Pasteur, a Claude Bernard or a Branly have thought he was humiliating himself by submitting to experimental control. Insistence proved of no avail, and as Dr Rendu was withdrawing the friar blurted out: "After all these experiments are but snares which are being laid to me" (p. 17).

It should be added that the French dowsers' claims are so far-reaching as to be distinctly bewildering. They seem to extend practically to *everything*! To quote one of them, Father Frédéric de Bélinay writing in the *Etudes* (a Paris review, with nothing humouristic about it and which is edited by Jesuits!) "All underground riches, water, coal, petroleum and metals, an aeroplane which fell down in the prairies, guns camouflaged in a forest, submarines diving, codfish shoals escaping before a fisherman's flotilla, Incas' treasures, tombs of the quaternary era, public notaries in flight (!), colonies of microbes, the hidden beginnings of cancer . . . everything we may conceive as being material, physiological" is "within reach of the powers with which the pendulum is endowed" (p. 2). For my own part, when reading such preposterous statements I can but shrug my shoulders or hold up my hands in utter amazement; and I think many other Psychological Researchers will agree with me.

The French Medical Society of which Dr Rendu is a distinguished member is, as I have said, a Roman Catholic Society. The independence of its attitude before claims put forward by ecclesiastics (rather numerous in this domain) is the more to be commended. However I do not quite sympathise with Dr Rendu when I find him saying (p. 30):

"How much we wish the ecclesiastical authorities would renew [as to dowsing, etc.] the cautious (?) prohibitions of bygone days!"

I do not quite agree because such prohibitions should not be

multiplied unduly. Do we not already live and move in a world saturated with *verboten* ?

In a welcome postscript Dr Rendu adds he is in a position if necessary to substantiate the smallest facts (alas, all negative !) mentioned in his valuable paper. We congratulate him on the scientific and critical spirit manifested in this and other statements of his and think he has done in writing "Radiesthésie, Science et Morale" a very useful piece of work.

PEROVSKY-PETROVO-SOLOVOVO.

CORRESPONDENCE

LEVITATION

RUDI SCHNEIDER'S CURTAIN MOVEMENTS

To the Hon. Editor of the JOURNAL

MADAM,—I read with great interest the article on, "Some Recent Sittings with Continental Mediums" by Mr C. V. C. Herbert in the *Journal* for May.

As regards the Sittings with Rudi Schneider, I suggest a possible explanation of this curtain movement, which may seem fantastic and far-fetched, but which, nevertheless, bears, I submit, a grain of debate.

Many years ago, when I was quite a youngster, I was serving at Sitapur, a small military station some fifty-six miles north of Lucknow. One day the District Superintendent of Police invited a party of friends to lunch, and to entertain them afterwards he engaged the services of a Brahmin, a self-styled holy man, who performed certain acts of magic or conjuring. One of his performances was very remarkable. A member of the party, a stout, phlegmatic Major in the Royal Army Medical Corps related it to me the following day.

It will be seen from the above that I was not present at this party, which consisted of ten or eleven male guests all aged in, or about, the forties. They sat in chairs in the shade of the garden, while the Brahmin did his performance on the grass lawn in brilliant sunshine. The act in question was as follows :

Four short posts, or stakes, were stuck into the ground by his assistants (two or three in number). On these posts was poised a cushion, and on the cushion was set a small Indian boy, aged five or six years. The Brahmin proceeded to mesmerise the boy. When he was completely hypnotised, the assistants pulled away the posts, and the boy remained sitting on the cushion in the air without any

support. The Brahmin passed long rods all round and over and below the boy, to prove there was no attachment. Presently the posts were put back, and the boy de-hypnotised. The act was over.

How was it done? The Brahmin said he could only do it with a boy under seven years. Some of the guests thought that *they* had been hypnotised, and that the posts had never been removed from under the cushion. But as the Major said to me, "I was smoking a 'Trichy' (Trichinopoli) cheroot throughout the proceedings." And he certainly did not strike one as being an easy subject for mesmerism.

The easy solution is that the Brahmin controlled the boy with some powerful form of hypnotism. If that was so, he must have controlled the cushion also.

I have seen a certain amount of Indian juggling, some of it extremely clever, but I have never heard of a similar case to this. I am also well aware that there are many amazingly tall and unsubstantiated stories of Indian tricks going about. I cannot vouch for the accuracy of this case, but I have the Major's word, and I believe he was too mystified at the time to have given way to exaggeration. Anyway, it happened thirty years ago and cannot very well be authenticated now.

When I read Mr Herbert's article, it set me thinking of this case, and I am beginning to wonder if it may not be possible to will-control inanimate things. May not the strong concentration of the will power of one person, or better of several persons, cause furniture to move and curtains to sway? In this case of Rudi Schneider the sitters were warned to count five and then "look out" at the curtains. This concentrated a number of will-powers, all expectant of seeing something—probably a movement—on the curtains. Is it not possible that this concentration, in some way little-known to us as yet—an undeveloped normal faculty—may have affected the movements of the curtains by perfectly natural means?

Yours faithfully

C. R. L. FITZGERALD, LT.-COL.,
Indian Army (retired).

To the Hon. Editor of THE JOURNAL

MADAM,—I have read with very great interest Colonel Fitzgerald's letter on the subject of the Schneider curtain movements, but I must confess that his suggested explanation does not seem to me to help us very much in understanding these phenomena. We have no evidence whatever that any such manifestation of combined "will-power" has ever been noticed before, whereas, if it were really so, one would expect kindred phenomena to occur spon-

taneously when large crowds were assembled with a common purpose. Again, it would only account for such of the Schneider phenomena as were expected by the sitters. It would not account for the overturning of the stool, mentioned in my paper, which came as a complete surprise to me, and, so far as I know, to all the other sitters.

None the less, Colonel FitzGerald's account of the Indian performance is of very great interest, and suffers only from the defect that he himself was not an actual witness of the occurrence. I have lately received, however, an account by Lord Halifax (printed below) of what seems to have been a somewhat similar experiment carried out in 1928 or 1929, and of which he was an eye-witness; and as he has been kind enough to give permission for his account to be printed, I send it to you herewith, in the hopes that it may be possible to print both accounts together, when they will, I feel sure, add greatly to each other's value. I remain, Your obedient servant,

C. V. C. HERBERT (Research Officer).

LEVITATION EXPERIMENT

We witnessed the performance of an Indian conjuror at Udaipur in 1928, or 1929, and therefore my recollection of the details may be incorrect, but, so far as I can recall it, what happened was as follows.

The performance took place in the Resident's drawing-room, the furniture being pushed back, and the performance taking place about 20 feet, or so, from where we were sitting. The conjuror had with him an Indian boy, whom he enveloped in a blanket secured on the outside by cords over the boy's arms and hips. He then made passes over the boy, who appeared to go into trance, and then to rise from the ground to the height of about a foot. The conjuror then passed his wand below, behind and above the boy to show that he was unsupported and unattached. After a short interval of half-a-minute or so with further passes he lowered the boy to the ground again, and in due course brought him out of his trance.

This is to the best of my recollection what occurred, but unhappily I did not write it down at the time, and my memory may betray me in particulars. (signed) HALIFAX.

"MEMORY AND TELEPATHY"

To the Hon. Editor of the JOURNAL

DEAR MADAM,—Mr Carrington's letter, published in the June issue of the *Journal*, deals with subjects of supreme importance.

I doubt, however, whether the majority of medical men believe

in the "engram" or "trace" theory of memory. The ordinary materialist perhaps would rejoice if such a theory were true to fact, for he would be able to say, "Let us eat, drink, and be merry for tomorrow we break our record."

Lately I happen to have been writing an article on "fixed memories" and in that article I introduced the following personal illustration as follows: "Nearly fifty years ago I recited at a school concert 'The Vulgar Boy' from *The Ingoldsby Legends*. Since that date I have not had a copy of that work in my possession, nor have I seen a copy. Nevertheless I am still able to repeat a dozen verses of the poem with hardly a mistake."

Now, taking this illustration for my text, let us imagine, for the sake of argument, that the little chap fixed himself with the associated poem, to an "engram" in my brain fifty years ago. By an "engram" one means I suppose the ultramicroscopical particle of electricity within the nucleolus of a brain cell. So far so good; we have the little chap firmly attached to an "engram". But here comes a difficulty. Brain cells grow and divide to form daughter cells, the nucleolus, which is the most important and smallest part of the cell, must partake of this division; for the daughter-cells, though immature, resemble the parent cell in all material particulars at any rate. Presumably the engram would also divide. "What becomes of our little chap, then?" If he also divides in the course of fifty years we might have millions of parts of little vulgar boys floating about the brain and jostling millions of other memories conscious or unconscious; under these conditions unity of normal mind could not be maintained.

It is true, of course, that there are certain ganglionic nerve cells which control certain vital processes such as respiration, but these act automatically, and act in a definite manner throughout life, though their material structure is changed many times. These cells, however, are not under control of the will.

But by our wills we can do many things: we can, for instance, visit and visualize places we have formerly lived in, etc., etc.

"Isn't it far easier to consider our minds as immaterial unities using the brain as instruments to play upon?"

The handing down of millions of memories from cell to cell seems such a cumbrous method of preserving memories, and not in accordance with wisdom or reason.

It is far easier to understand telepathy by assuming the unity of mind than by giving it a cellular structure.

Yours faithfully,

WM. A. CARDEN, M.R.C.S., L.R.C.P.

THE QUALITATIVE STUDY OF TRANCE PERSONALITIES

To the Hon. Editor of THE JOURNAL

MADAM,—In her letter in the *Journal* for April Miss Naumburg reports that Mrs Garrett was under the impression that, when Mr Besterman was working with her, “the galvanometer failed.” Mainly on this ground, it would appear, she implies that the method in general, and my conclusions in particular, may be invalid. Since the galvanometer and ancillary apparatus were used for studying psycho-galvanic reflexes, whereas my conclusions were based entirely on reaction times and reproduction tests,¹ this would seem to be a *non sequitur*, but I cannot allow the suggestions made to pass unchallenged.

First, however, I must apologise for a verbal carelessness which may possibly have been misleading. I ought, no doubt, to have written that “Mrs Garrett’s reflexes were less satisfactory than any I have previously studied,” or words to that effect, instead of the passage quoted; but I think that my clear implication that it was Mr Besterman who collected the data (*Q.S.T.P.*, i, p. 117) should exonerate me from any deliberate suggestion that I did the work myself.

Next, it is not quite clear to me whether Miss Naumburg wishes to suggest that the accurate collection of data was beyond Mr Besterman’s powers; but, if so, I can assure her that the task is not really at all so formidable as it sounds. It is admittedly more difficult than the leisured reading of, say, a clinical thermometer, and requires a certain amount of practice (which Mr Besterman had had); but I venture to say that almost any competent person, not unduly intimidated by apparatus as such, could easily acquire the kind of rhythmic sequence of motions, etc., which enables observations to be made in an orderly and accurate manner.

As regards the main point, Mr Besterman will, I know, bear me out in saying that at no time did the galvanometer, or any other part of the apparatus “fail”; if it had done so, it would instantly have been detected. The apparatus was so designed that its proper functioning could be tested at any moment, and we even had the equivalent of a “dummy subject” as an additional means of testing the whole circuit. Actually, as a precautionary measure (which was, as it proved, supererogatory) we had the whole apparatus checked up by the makers on two or three occasions, but in each case it was found in perfect order.

¹ The only conclusion I drew from the reflexes was, tentatively, that the phenomenon of “countersimilarity” probably extends to them, in the case of Uvani, as well as to reaction times. (Cf. *Q.S.T.P.* ii, p. 340.)

The suggestion that, if only "a technician" had been called in, different results would have been obtained, can, if I may venture to say so, proceed only from ignorance of the apparatus, its method of use, and the contemporary circumstances. And to speak of the galvanometer failing because the reflexes rapidly fell off is about equivalent to saying that a telescope failed because clouds passed between it and a star.

Further, if Miss Naumburg will consult again the report by Dr Brown to which she refers, she will see that an entirely different type of experiment was involved. But at least one feature of Mrs Garrett's behaviour is easily identifiable, for it was—to speak colloquially—the failure of the reflexes elicited by the stimulus words to make headway against the steady and relatively rapid rise in resistance shown in Dr Brown's paper that our difficulties were largely, or at least partly, due.

There can, in short, be no doubt whatever that the unsatisfactory nature of Mrs Garrett's reflexes was *not* due to "lack of a functioning galvanometer," but to psycho-physiological causes at her end. I accordingly repudiate with vigour the suggestion that the effects observed were due to "lack of observation and training. . . ."

And this view is supported by the fact that very satisfactory reflexes were obtained in the Rudi-Olga experiment, whereas those obtained from Mrs Salter and Mr Gatty at later dates were very bad, despite the intervening additional practice with Rudi.

On the other hand, it is not less important to insist that the "unsatisfactoriness"—from the computer's point of view—of Mrs Garrett's reflexes constitutes no sort of reflection on her or her mediumship. For aught I know to the contrary, this type of response to word-association tests (resistance high and rapidly rising; reflexes rapidly fading out) may be the hall mark of high-grade mediumship, though I fancy the fading is mainly a matter of "boredom" (cf. *Q.S.T.P.*, ii, p. 351). When a dozen other mediums of equal quality (if such there be) have proved so obliging as Mrs Garrett we shall know more about it; at present it would be unwise to generalise from a single case.

I hope the foregoing will set Miss Naumburg's mind at rest; for it seems unfortunate that she should be led to discount results obtained from one class of data because of Mrs Garrett's erroneous, if natural, impressions concerning the collection of another class.

As regards the exclusion of certain words of universally high emotional value from my list, I should have thought the reasons for this would have been clear enough. If we wish to study the

differences between two objects, we naturally do not study, but carefully ignore, the points which we know in advance they are likely to have in common. We do not seek to identify a criminal on the ground that he has two legs, ten fingers, one heart and thirty-two teeth: otherwise Miss Naumburg, or I, might well find ourselves in the electric chair as being indistinguishable from Public Enemy No. 1. On the contrary we concentrate on attributes which are likely to vary greatly from one individual to another.

In my early work I found that the six words giving the biggest reflexes (on which I was mainly concentrating at that time) were Kiss, Love, Marry, Divorce, Name and Woman: while the six giving the smallest were Swim, Pencil, Pond, Flower, Give and Glass. Now it is evident that the first six are likely to be of almost universal interest and emotional value, while the last might give very diverse effects according to the particular experiences of the persons tested. These, then, would be most likely to bring out individual differences which might easily be overshadowed by the effects of the first six if these were included in the list.¹

I quite agree that it would be most interesting to repeat the tests with words of this character included; but I think there can be no doubt that it would have been unwise to use them in the first instance.

Miss Naumburg suggests also that "the effect of the stimulus words must surely die down," and cites Mrs Garrett's subjective impressions in support of this contention. But the proof of the pudding is in the eating, and if this were to occur to any considerable extent we could not possibly obtain, as we do obtain, significant Similarities, Differences and "Individualities"—the latter, in particular, effectively demonstrating the consistency with which the various words are, in fact, differentiated in the case of a given subject. I fear that "must surely" cannot over-ride the facts as represented, for example, by results 11 and 14 of *Q.S.T.P.* ii, Table I. which show overwhelmingly the consistent differentiation between words by Mrs Garrett and Uvani.

Yours, etc.,

W. WHATELY CARINGTON.

N.B.—I cannot, of course, speak with authority about Mr Hereward Carrington's work, but internal evidence strongly suggests that what I have said above applies to his as well as to ours.

¹ Cf. *The Measurement of Emotion*, pp. 38 and 110.

REVIEWS

HANS BENDER. *Zum Problem der Aussersinnlichen Wahrnehmung.* Leipzig 1936. Pp. xi, 116. Price RM. 4.80.

HANS BENDER. *Psychische Automatismen.* Leipzig 1936. Pp. ix, 135. Price RM. 5.50.

It is no reflection on Dr Bender to say that the most striking thing about these two little books is the fact that they set out the results of work done in the Psychological Institute of the University of Bonn, with the financial and moral support of the *Notgemeinschaft der Deutschen Wissenschaft*. Such a change of attitude on the part of official science will be most significant to those who know of the intensely conservative attitude of the German universities to the whole field of psychical research. Dr Bender could ask for no higher tribute to his integrity and ability.

The former of the books mentioned above, though published first, is actually a sequel to the second. This latter book embodies the results of a series of investigations planned to demonstrate the value of the psychic and sensory automatisms for the study of the unconscious. These experiments are a sort of compact summary of the work in this field carried out during the past half-century, and they yielded the same types of results as always before. What is more interesting is that they suggested, as so often before, that paranormal phenomena sometimes emerge during the course of such experiments. Dr Bender had the courage to follow up these indications, and his findings are described in the first-named book.

In brief, Dr Bender's experiments, in their final stages, were carried out with series of cards bearing the letters of the alphabet, each card being covered with cellophane in order to avoid any suspicion of tactile hyperesthesia on the part of the subject. These cards were prepared by a third person, and were picked out by Dr Bender and handed to the subject entirely under cover. Thus nobody present knew what card was being examined by the subject, and there appears to have been no normal source of leakage. There were variations in the procedure, the conditions here described having been the *least* secure; thus the cards were sometimes so covered as to render the letters on them invisible. Under these conditions Dr Bender obtained the following results, which he himself analyses much more elaborately:

Series 1; 34 trials; completely accurate indications, 7; chance expectation, about 1.3.

Series 2; 69 trials; completely accurate indications, 15; chance expectation, about 2.5.

Series 3 ; 17 trials ; completely accurate indications, 8 ; chance expectation, about 0.7.

Series 4 ; 14 trials ; complete accurate indications, 7 ; chance expectation, about 0.5.

Total of the four series : 134 trials ; completely accurate indications, 37 ; chance expectation, about 5.

These figures, which are not Dr Bender's, but which are derived from his tables, are pretty conclusive, even allowing for the small number of the trials, the writer's justification of which is not altogether convincing. Another defect is the fact that the entire experiments were conducted with a single subject (though in the preliminary series one or two others were used). It is also right to record the fact that a series of trials conducted early in 1934 with materials (numbers, geometrical figures, letters and words, carefully enclosed) provided by the present reviewer, in his then capacity of Investigation Officer, proved a complete failure. This fact is duly set out by Dr Bender, though he does not describe the results in detail. These reservations may possibly cause some students to suspend judgment, but I am myself satisfied that the only loophole in these experiments lies in the statistical difficulties inherent in their small number. Dr Bender states more than once that his sole purpose was to convince official psychology that clairvoyance must be taken seriously and that normal psychology is bound to be defective if paranormal phenomena are overlooked. It may seem absurd to hardened psychological researchers that it should still be necessary to demonstrate these things : but the necessity is unquestionable, and there can be no doubt that Dr Bender ought to succeed in his admirable purpose.

It should be stated that Dr Bender has also entered into some distinctly interesting theoretical considerations mainly based on the introspections of his subject. I think, however, that Dr Bender has not given sufficient consideration to the danger of generalising from the self-analysis, always dangerous even in the mass, of a single subject. Dr Bender is continuing his researches, with special attention to scrying, and his report will be looked forward to with the keenest interest.

THEODORE BESTERMAN.

HEREWARD CARRINGTON, *Loaves and Fishes*. Charles Scribner's Sons Ltd. Price 7s. 6d. net.

In this book, Mr Carrington examines the Bible accounts of miracles (mostly those of the New Testament) in the light of modern psychological and parapsychological knowledge. The Bible miracles, regarded solely from the point of view of psychical research, can be

of little interest to us, since our knowledge of them is based more on tradition than on contemporary record. But none the less the book will prove of value to at least two classes of persons: *i.e.*, those who accept the whole of the Bible, or the whole of the New Testament, as "gospel truth" (a class possibly larger in America than in this country); and those who reject all the miracles as impossible.

Dr Carrington has trodden a difficult and dangerous path with considerable skill. He has avoided treading too heavily on religious corns, without, on the other side, unduly offending the sceptics. There is certainly nothing in the book which should shock or pain a reasonable person. There are, no doubt, passages with which many readers will disagree. For example, it is clearly questionable that the Christian religion would have come to nothing if it had not been for the "seeming resurrection of Christ" (page 184). Again, some will consider that the Author overstates the certainty with which the parapsychological phenomena have been established at the present day.

We must, then, congratulate Dr Carrington in having succeeded where many people would have failed: though it should again be stressed that any permanent value that the book may have, lies in the field of theology, and not of psychical research.

C. V. C. H.

ERRATUM

Journal for July, page 261, line 38, for 'inferior' read 'superior.'

THE *JOURNAL* IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Associates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on the definite assurance that the *Journal* is, as stated on the cover, issued "For private circulation among Members and Associates only." The Council hope that all Members and Associates will continue to co-operate with them in maintaining this privacy.

JOURNAL
OF THE
Society for Psychical Research

NOTICE OF MEETING

A Private Meeting of the Society

WILL BE HELD IN

THE SOCIETY'S LIBRARY

31 TAVISTOCK SQUARE, LONDON, W.C. 1

ON

WEDNESDAY, 25 November 1936, at 5.30 p.m.

WHEN A PAPER ON

“THE GHOST AS A PSYCHIC PHENOMENON”

WILL BE READ BY

DR H. GODWIN BAYNES

N.B.—No Tickets of Admission are issued for this Meeting. Members and Associates will be admitted on signing their names at the door. Tea will be served from 4.45 p.m., to which Members and Associates are invited.

NEW MEMBERS

*(Elected on 27 May 1936)***Macintyre, Donald**, Moanalua, Honolulu.**Standaart, Dr A. W. J.**, 445 Mathenesserlaan, Rotterdam, W., Holland.*(Elected on 8 July 1936)***Gray, Mrs E. F.**, Ripple Hall, Tewkesbury, Glos.*(Elected on 30 September 1936)***Gibson, Edmond P.**, 1009 Oaklawn St. N.E., Grand Rapids, Michigan, U.S.A.**Godden, Miss L. E.**, Heathfield, Lynchford Road, S. Farnborough, Hants.**Moxey, Louis W., Jr.**, 230 North Camac Street, Philadelphia, Pa., U.S.A.**Pierson, Miss Jocelyn**, Sterlington, New York, U.S.A.**Shearer, F. M.**, King William Street, Adelaide, S. Australia.

THE RESEARCH OFFICER

THE Council have appointed Mr C. V. C. Herbert, who has acted as Research Student since September 1935, to be Research Officer of the Society.

Mr Herbert will be glad to see members of the Society, or others who may wish to discuss matters relating to the Society's work, on any Tuesday or Thursday from 11.0-1.0 and 2.30-4.0, except when he is unavoidably absent on the Society's business. Should these hours be inconvenient in any individual case, Mr Herbert would endeavour to arrange to make a special appointment.

MEETINGS OF THE COUNCIL

THE 342nd Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1., on Wednesday, 27 May 1936, at 4.15 p.m., THE HON. MRS ALFRED LYTTTELTON, G.B.E., in the Chair. There were also present: Mr W. H. Salter, and the Rev. C. Drayton Thomas; also Mr C. V. C. Herbert, Research Student, and Miss Isabel Newton, Secretary. Although there was no quorum the members present decided to transact the business before them, subject to confirmation at the next Meeting of the Council.

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

Two new Members were elected. Their names and addresses are given above.

THE 343rd Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 8 July 1936, at 5 p.m., THE PRESIDENT in the Chair. There were also present: Mr Whately Carington, Lord Charles Hope, the Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mrs W. H. Salter, Admiral the Hon. A. C. Strutt, R.N., and the Rev. C. Drayton Thomas; also Miss I. Newton, Secretary, and later Mr C. V. C. Herbert, Research Student.

The Minutes of the last Meeting, when the Council failed to secure a quorum, were adopted and confirmed.

One new Member was elected. Her name and address are given above.

Mr C. V. C. Herbert was appointed Research Officer to the Society.

THE 344th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 30 September 1936, at 4.15 p.m., THE HON. MRS ALFRED LYTTELTON, G.B.E., in the Chair. There were also present: Sir Lawrence Jones, Bart., Mr W. H. Salter, Mrs W. H. Salter, Admiral the Hon. A. C. Strutt, R.N., the Rev. C. Drayton Thomas, and Miss Nea Walker; also, Miss Isabel Newton, Secretary.

The following Resolution was moved from the Chair, and carried unanimously:

“The Council, who have learned with the deepest regret of the death of their colleague, Mr Sydney Scott, wish to express to Mrs Scott and the family their warmest sympathy with them in their bereavement, and at the same time to place on record their appreciation of the very great services which on numerous occasions over a long period of years Mr Scott rendered to the Society, and their sense of the heavy loss which the Society has suffered by his death.”

Five new Members were elected. Their names and addresses are given above.

On the proposal of Miss Nea Walker, seconded by the Rev. C. Drayton Thomas, a vacancy among the elected Members of Council was filled by the appointment to it of Lord Charles Hope, hitherto a co-opted Member.

MEETINGS OF THE SOCIETY.

The 185th General Meeting of the Society was held at Manson House, 26 Portland Place, London, W. 1, on Wednesday, 8 July 1936, at 6.30 p.m., THE PRESIDENT in the Chair.

SIR ERNEST BENNETT, M.P., read a paper on "Some recently received Cases of Haunted Houses" (cases received in response to Sir Ernest Bennett's broadcast of March 1934).

THE 142nd Private Meeting of the Society was held in the Society's Library on Wednesday, 27 May 1936, at 5.30 p.m., the REV. C. DRAYTON THOMAS in the Chair.

MR J. C. MABY read a paper on "The Flower Medium: Miss Hylda Lewis". A summary of this paper will be printed later in the *Journal*.

The 143rd Private Meeting of the Society was held in the Society's Library on Wednesday, 30 September 1936, at 5.30 p.m., the HON. MRS ALFRED LYTTELTON, G.B.E., in the Chair.

MR W. H. SALTER read a paper on "Statistical and Other Technicalities in Psychical Research".

NOTES ON RESEARCH UNDERTAKEN DURING THE SEASON 1935-1936

A SERIES of sittings with Miss Frances Campbell was arranged by the Society, the Assistant Secretary, Miss Horsell, acting as note-taker. A report on these is in course of preparation.

An important experiment with the well-known subject, Mrs Garrett, is being carried out on the lines of Pratt's work at Duke University.¹ The first series of sittings have all been held, but it will be some time before the necessary work on the annotation and evaluation of the scripts can be finished.

A recently elected Member, Mr Eric Cuddon, in collaboration with the Research Officer, is carrying out some interesting experiments in post-hypnotic suggestion. Mr Cuddon has an excellent hypnotic subject, and it is hoped that his work may throw light on the psychological mechanism of "mediumship".

Some 60 "spontaneous" cases have been investigated, of which six have been judged sufficiently evidential of paranormality for printing in the *Journal*.² The documents relating to most of the others may be seen by Members and Associates at the Society's Rooms.

¹ See Boston Society for Psychical Research, Bulletin XXIII, March 1936.

² See *Journal*, S.P.R., No. 511, p. 2; No. 512, p. 33; No. 524, p. 209 and p. 213; No. 526, p. 239; No. 528, p. 272.

Various sittings with "mediums" were attended by officers of the Society, who also witnessed demonstrations of so-called sightless reading by Mr Kuda Bux, and Mr Kolb. Reports of these are available for inspection.

An interesting case of "clairvoyance" in Holland was investigated by Mr Whately Carington, who with the assistance of the Research Student carried out a quantitative experiment in paragnosis. The results were not significant of supernormal faculty.

An experiment with Mrs Leonard has been begun in which a technique has been evolved for reducing the well-known "book-tests" to a quantitative basis, so that the odds against chance may be exactly calculated. The Rev. W. S. Irving has kindly cooperated in this.

Messrs. Gatty and Irving have continued their work with Mrs Leonard, and Mr Gatty has prepared a paper on his results. The Rev. C. Drayton Thomas has carried out further researches with the same subject.

Mr Kenneth Richmond is continuing his study of the large amount of Leonard material that has been put on record by members during 20 years. The structure of trance-personalities remains his chief line of research in an investigation which also includes further work on proxy sittings and on the validity of book- and newspaper-tests.

Mr G. N. M. Tyrrell is also continuing his research in Extra-Sensory-Perception. The Report on this subject published in *Proceedings*, xliv, Part 147, covered a period of research from 13 May 1935 to 13 March 1936, but a footnote on p. 151 carried the results of one class to 2 July 1936. This latter class dealt with the condition in which, using the electrical apparatus described in the report, mechanically selected numbers were passed through the keys by the operator, the connexions from the keys to the lamps being in all cases unknown to him. Results with this arrangement, as is shown both by theoretical calculation and by practical tests, cannot be obtained by using the Fisk method of scoring (see pp. 153-158 in the Report), the method being in fact evidentially on a par with the use of the mechanical selector itself. The chance-probability figure for this class of results, which on 2 July was 10^{-14} , has since then (up to 12 September) been reduced to 10^{-56} , that is to say that the odds against this class of results being due to chance is of an astronomical order.

Results with the mechanical selector itself have begun to be obtained. At present they form a comparatively small class, for which the chance-probability figure is 0.0016 (also up to 12 September). It is interesting to note that this class of experiment appears to be

passing through the same phases as the other classes which were dealt with in the above report, viz., that of gradual acceptance by the subliminal of the subject after a long period of rejection.

A new device has been added to the machine by which the selector is operated automatically by the subject herself in the act of opening the boxes, and as this arrangement, when tested out, gives a chance distribution of events, a new and interesting form of the pre-cognitive experiment is opened up by it. Several successes have been attained with it, Miss Johnson being the percipient.

A hypnotised subject was tried but she failed to score above chance. A child aged six is now under test.

Any member, or the friend of any member, living in or near London, having mediumistic or clairvoyant gifts, would be welcomed as subjects if they would care to try. It is of special interest to try with children, for whom the apparatus forms a kind of guessing game.

The Society has kept in touch with The British College of Psychic Science, The International Institute for Psychical Research, and The University of London Council for Psychical Investigation, each of which bodies has been of material use to the Society on more than one occasion.

CORRESPONDENCE

EXTRA-SENSORY PERCEPTION

To the Hon. Editor of THE JOURNAL

MADAM,—May I venture to make a few comments on Mr G. N. M. Tyrrell's "Further Research in Extra-Sensory Perception", published in the *Proceedings* for June?

First, I would like to say how I admire the scientific detachment with which he presents his findings, whether they are disconcerting (as in the case of Mr Fisk's method of scoring) or otherwise. It has been said that too often those who engage in psychical research exhaust their energies in establishing the mere existence of the phenomena. Here, at least, is a triumphant exception. Long may this painstaking collaboration of Miss Johnson and Mr Tyrrell continue!

They probably have their programme mapped out, but there is one experiment I should like to suggest. If the conditions entailed do not inhibit Miss Johnson's faculty it is possible that it might yield interesting results. The hypothesis it would test may possibly have been suggested before. It first occurred to me when reading Upton Sinclair's book *Mental Radio*. As I pointed out in a letter

published in the *Listener* of August 16 1933, certain puzzling facts recorded in that book would be explained if the results were attributed not to clairvoyance, but to precognition. It may be remembered that Upton Sinclair made drawings of simple objects, placing them in numbered envelopes. His wife then attempted to reproduce these drawings one at a time by alleged clairvoyance. He reported that a number of attempts bore a striking resemblance not to the corresponding originals, but to subsequent ones. Not easy to explain by clairvoyance, but understandable if the percipient obtained her results by perceiving her own future examination of the original drawings.

Another justification for suggesting the above hypothesis is that it may shed light on the research described in Dr J. B. Rhine's *Extra-Sensory Perception*. He says (on pages 147-8, American ed.), "It may be said now, I think, on good experimental evidence, that in clairvoyance and telepathy we are dealing with the same basic process. . . . Not only do the subjects possess both clairvoyant and telepathic capacity, but, what is more meaningful still, they score in both conditions at about the same rate." He draws the conclusion that "as referring to distinct processes there is probably no clairvoyance and no telepathy. There is just this mode of perceiving, extra-sensorially". I should prefer to say that while telepathy probably exists as a distinct process, the one basic process was precognition.

Returning to Mr Tyrrell's results, on first inspection they seem to confirm the hypothesis. For instance, total 138 gives 23.8% for 10,050 trials when the conditions excluded telepathy, while total 140 gives 23.9% for 2255 trials when the conditions excluded telepathy and clairvoyance.

On the other hand, his results surprised me in another direction. I had assumed that scores above chance depended on the percipient seeing the correct card or box after each trial. Or, in other words, that precognition of an event took place only when action based on information obtained thus did not prevent the event itself taking place. Such a procedure was followed presumably by Dr Rhine (it may explain the rather higher scores obtained by his subjects), but Mr Tyrrell's work shows that this assumption was unjustified. In its place I venture to put forward another. Where the percipient scores significantly without seeing the correct box after each trial a possible explanation is this. At each trial the percipient selects one box at random. By precognition the success or failure of this intention is perceived, and in the case of the latter another choice is made which is acted upon.

But enough of speculation ! The whole matter can be solved by experiment or it would be meaningless. If Miss Johnson can score significantly when her action of raising the box lid switches off the light before it is normally perceptible, then the hypothesis is disproved. At the cost of further complication, but with the advantage of retaining apparently normal conditions, it would be desirable to arrange that the change from conditions permitting precognition should be operated by a mechanical selector selecting one trial in, say, four.

Mr Tyrrell is very cautious, so that, while I do not think his published conclusions can be construed to suggest the above hypothesis, I hope I have not trespassed hastily where he is advancing cautiously. His own theory of clairvoyance is, I think, one which would be classified by Dr C. D. Broad as "non-sensuous prehension", and would not give rise to such a suggestion.

Yours faithfully, T. A. STRANGE.

To the Hon. Editor of THE JOURNAL

DEAR MADAM,—In thanking Mr Strange for his letter, may I say that I entirely agree with him in believing the distinction between the different modes of E.S.P. to be arbitrary rather than real? A point mentioned in my Report, but perhaps not sufficiently stressed, is that in the great majority of cases the lighting of the lamp and the opening of the box were simultaneous, so that the percipient's mental act of choice as well as initial muscular movements towards the box must have occurred before the lamp was actually alight. If a cinematograph record of the whole process had been possible, I am sure it would have shown the percipient's hand moving towards the box before the lamp began to glow. Like Dr Rhine, I have been impressed by the undifferentiated character of the extra-sensory faculty; there are no signs of anything to mark off clairvoyance from precognition, a reversal of the time-order of the events being treated with indifference.

It would certainly be interesting to try the experiment which Mr Strange suggests, but it might be some time before the subject could be induced to accept the change.

Yours faithfully, G. N. M. TYRRELL.

INDIVIDUALITY AND EVIDENCE

To the Editor of THE JOURNAL

MADAM,—Clear-headed philosophical discussion generally brings out points of practical importance, and a very clear statement on

mental events by Mr Saltmarsh¹ has a vital bearing not only on the theory of individuality, but on our practice in estimating evidence of individual intention, as distinct from evidence of paranormal knowledge.

“Every mental event”, Mr Saltmarsh writes, “occurs against a background and is to some extent coloured by that background. The background is made up of elements from the entire past history of the individual experiencing the event. . . . I suggest therefore that every mental event contains within itself elements derived from the whole past history of the individual.”

Apart from the general assent which we may be inclined to give to this proposition, the analytical branches of psychology supply abundant and increasing evidence that it is true. And the truth of the proposition involves a clear fact about the evidence for any mental event—in particular, the presentation of any piece of paranormal information.

Directly this mental event is separated from its “background” (if it has been given in a context referring to the past history of a communicator), it automatically loses “colour” as a mental event appertaining to that communicator. The paranormal detail is, like other items of experience, “not a simple, clear-cut mental event with sharp edges and clearly defined boundaries,” to quote Mr Saltmarsh again; “rather it is an inseparable part of an organic whole which, if forcibly taken out of that whole, becomes something different.”

Probably by reason of training and habit derived from the physical sciences, especially chemistry (the science of thought is riddled with irrelevant chemical metaphor), we find ourselves possessed of an idea that if we “isolate” the “elements” of a psychological whole, we shall somehow have explained it. Even in chemistry we isolate elements by wiping out compounds with their distinctive properties; and in psychical research we cannot remove a piece of evidence from its context without every risk of abolishing the properties which it has within that context.

Mr Drayton Thomas's recent exposition of newspaper-tests is a case in point. Separate these events from their close interweaving with the past history and experience of “John”, and you have evidence of extra-sensory perception without any hint that it is being displayed with any particular intention. It is as easy to presuppose intention on the part of Mrs Leonard's trance-mind and Mr Drayton Thomas's subliminal as on the part of a communicator.

¹“Some comments on Mr Tyrrell's Paper on Individuality.” *Proc.*, Part 148, Oct. 1936, p. 188.

But restore these leaves of extra-sensory evidence to the stem of "John's" history and the grouping of his interests from which they emerge in the tests themselves—still more, refer the whole to the roots that can be traced out in the complete records of the Drayton Thomas sittings—and there is positive and coherent evidence, which needs to be criticised as such, of an organic whole which has the characteristics of an individual.

It seems to me important that this question of context and coherence in psychical evidence should not be regarded as a speciality of a fad of those who are interested in it. I am quite prepared, myself, to follow up any rational hypothesis whatever of "the communicator"; but it seems sheer waste of time to pull organic evidence to pieces, make selections from the pieces, and if these still show too much meaning reduce them to still smaller shreds, so as to demonstrate in the end that there is nothing worth talking about in material that has been effectually destroyed. And this destructive energy could be most usefully employed in demolishing faulty evidence and showing where and why it is not truly organic.

Yours, etc., KENNETH RICHMOND.

REVIEWS

Harry Price : Confessions of a Ghost-hunter. Pp. 386. Illustrated. Demy 8vo. Putnam's : Price, 10s. 6d. net.

Mr Price has written a book which, while intended for the general reader, will be found interesting and in many places instructive by those who have a fair amount of knowledge of psychical research. It is written in a readable style and covers a wide field, though naturally those aspects of psychical research, with which the author has had the greatest experience, receive the fullest treatment.

In the first five chapters Mr Price relates some of his experiences in investigating poltergeists and haunts, and he follows these with an account of the "Talking Mongoose" on a Manx farm, a very curious incident falling within the same general class of phenomena. A later chapter on "How to test a Medium" contains many sound hints for beginners, and a long chapter on "Secrets of 'Spirit' Photography" treats this subject very fully and drastically. The author explains in some detail various methods which have been, and others which might be, used by the fakers of photographic extras.

But Mr Price's attitude is not invariably negative. There is a chapter entitled "Convincing Experiments with a French Clairvoyante" (Mlle Jeanne Laplace), and in another chapter he reiterates his belief in the genuineness of the earlier phenomena of Rudi Schneider.

Later sections deal with "Stage Telepathy", Marion, (of whom we are glad to learn a report by Mr S. G. Soal is to be expected), the Indian Rope-trick as performed by Karachi (differing considerably from the elusive rope-trick of tradition) and Kuda Bux's exploits, both in reading with blind-folded eyes and in performing the fire-walk; further reference to the latter exploit is made below.

There is also a chapter on the much discussed Brocken experiment, when (in honour of Goethe!) the ritual of the "Blokberg Tryst" for converting a he-goat into a young man was followed by Mr Price and Mr Joad as nearly as the unobliging absence of the moon permitted. Everyone seems to have enjoyed it except the goat, who, to judge from the photograph facing p. 338, adopted a very non-cooperative attitude. He has our sympathies.

It will be seen that Mr Price succeeded in the intention announced in the Preface of making these *Confessions* both diverse and readable. But the Preface seems to suggest that the book has some serious bearing on the question of survival, and this is far from being the case. There is hardly a matter treated in the book which any disputant who knew his case would raise if debating that question. This, and too great a readiness to sniff at the efforts of other investigators are unfortunate blemishes in a book which deserves to be widely read.

Who wrote the Mahatma Letters? By H. E. and W. L. HARE.
Pp. 326. London: Williams & Norgate. 1936. 10s. 6d.

The authors are to be congratulated on having written an extremely interesting book. In the year 1923 the complete series of letters received by Mr Sinnett, and purporting to have been sent to him supernormally by Indian Mahatmas living in Tibet, was published, and in the following year a complete collection of Madame Blavatsky's letters to Mr Sinnett appeared. It therefore became possible for the first time to institute a comparison between Madame Blavatsky's letters and the Mahatma letters, and this the authors have proceeded to do with devastating effect. Drawing parallels between the two series of letters it becomes obvious that the similarities between them are too close and too numerous to be attributed to any cause other than common authorship. They share the same range of ideas, and the same general literary style, particularly the use of gallicisms natural to Madame Blavatsky, who was always more familiar with French than English; similar mistakes also occur as regards the transliteration into English of Indian names, and in the spelling even of common English proper names.

The question of Madame Blavatsky's motives has from the early days of her Society been much debated. Hodgson's suggestion that

she was a Russian spy is now known not to be correct, though not far off the mark, since the principal reason why she did not follow this career seems to have been lack of appreciation of her abilities by her own Government. The authors of this book make the following three "interpretative suggestions": "First, we trace the power-seeking motive as the original and main cause of her deceptions: second, we note an animus against Christianity, both exhibited and avowed, which led her to the extremity of abuse and to tactics of opposition void of moral scruple: third, we perceive that her earlier deceptions placed her in positions which in time became untenable, so that necessity compelled her to adopt greater and more unabashed measures of defence." W. H. S.

The Philosophy of Religion versus The Philosophy of Science. By ALBERT EAGLE. Pp. 352. "Obtainable through all Booksellers from Simpkin Marshall Ltd." Price 5s.

Mr Eagle is Lecturer in Mathematics in the Victoria University of Manchester, and has held several scientific appointments. The sub-title of his book, "An Exposure of the Worthlessness and Absurdity of some Conventional Conclusions of Modern Science" fairly describes its nature. Many of his criticisms can be read with interest, even by those who differ widely from him on many points. It is surprising to observe that in a book which touches on many of the problems to which the evidence of psychical research is extremely relevant, the author seems to be as oblivious of that evidence as any of the typical scientists whom he criticises. W. H. S.

NOTES ON PERIODICALS

The International Institute for Psychical Research. *Bulletin III*: June, 1936.

In 1933 Dr Watters published an account¹ of a series of experiments, performed in America, in which he claimed to have photographed, by means of a Wilson Cloud Chamber, the "immaterial bodies" of certain animals, liberated at the moment of death, and rendered visible by the condensation on them of water vapour in the Cloud Chamber. The photographs, as reproduced in Dr Watters' paper, were not sufficiently clear to enable the reader to form a judgment as to the validity of the claims; and, not unnaturally, serious students of parapsychology were unwilling to accept such an extraordinary phenomenon without corroborative evidence.

¹ *Bulletin* of the Dr William Bernard Johnston Foundation for Psychological Research, October, 1933.

The International Institute is greatly to be congratulated in having gone to the trouble and expense of repeating Dr Watters' experiments. This *Bulletin*, written by B. J. Hopper, M.Sc., gives a summary of the Watters paper, and describes the apparatus that was set up at the Institute, and the conduct of the experiments. It will be obvious to the reader of the *Bulletin* that every effort was made to duplicate the American results; but no "immaterial body" was ever detected. It seems likely that Dr Watters was mistaken in his interpretation of his photographs, and that what he claimed as an "immaterial body" was in reality nothing more than stray formations in the cloud in the Chamber, due to "slight unevenness in the thermal conditions".

All workers in this field will be grateful to Mr Hopper and the Institute for an important piece of research. C. V. C. H.

Psychic Science, 1936, Vol. XV, No. 2, July. Price 1s.

This number contains an important paper by Mr Eric Cuddon, describing experiments in which a "control" was produced by means of post-hypnotic suggestion. It now seems probable that even if the messages received from "mediums" do originate in the survived personalities of departed spirits, the mechanism of transmission is, none the less, dependant on unconscious personalities in the subject's mind, *i.e.* that the spirit (if it is a spirit) makes use of the psychic organisation of the subject, as well as of his or her physical body. For this reason, Mr Cuddon's experiments should be of the greatest interest both to investigators and to confirmed spiritualists.

Mr Cuddon is greatly to be congratulated, though the very curious Editorial Note to his paper suggests that those to whom Mr Cuddon has communicated his results are not worthy of the labours of so excellent an investigator.

Another experiment by Mr Cuddon is described in a paper by Mrs Hankey. Though it has a less direct bearing on psychical research, it should be of the greatest interest to students of psychology and the phenomena of hypnosis. C. V. C. H.

Revue Métapsychique, January-February 1936.

(1) *Charles Richet* by Dr E. Osty.

Dr Osty gives a brief but comprehensive survey of Richet's career as a physiologist and as a metapsychic researcher.

(2) *Supernormal perception of things unknown to any living person*: by M. Prosper de Szmurlo of Warsaw.

M. Stefan Ossowiecki describes in detail the unusual and various

contents of a packet made up by a man long since dead, and unknown to anyone living.

Chronique. An interesting article on "The part played by spiritualism in the recent English elections."

An article on the stopping of hemorrhages by verbal formulae.

The disappearance from public view of Theresa Newmann, of Konnersreuth, who is presumed to have been hidden in a convent by the ecclesiastical authorities in consequence of her utterance of some criticism of the present German authorities.

Revue Métapsychique. March-April 1936.

"Haunted Houses." DR E. OSTY.

Dr Osty has never been able to witness phenomena in a haunted house, but he gives a detailed account of the different kinds of haunting (auditory, visual, tactile, olfactory and physical), both spontaneous and induced.

Dr Osty suggests that it would be valuable if evidence could be obtained of haunting with no person present in the house (this might be accomplished by methods developed during the investigation of Rudi Schneider's mediumship).

"Account of Haunting." MME DE M.

Mme de M. had frequently the impression of a "presence" in her bedroom, when her dog and parrot showed signs of fear. Blows, as with a fist, were heard, also heavy footsteps. There were numerous unexplained fatalities among domestic animals. Nothing succeeded in the estate, and Mr M.'s business all went wrong.

"Haunted Houses before the Law." M. C. DE VESME.

Details are given of the haunting of Mr Stephen Phillips' house at Egham, and the resulting litigation. Signor Bozzano ran risk of proceedings for writing of phenomena which are said to have occurred in a room sealed by law officers after a suicide. The question of the effect of hauntings on leases, of haunting as dealt with by modern Italian law, of the part played by metaphysics in cases of hauntings (explaining or alleviating the phenomena), cases of "poltergeist"; hauntings of empty houses.

"Catherine Fillgung's Stigmata." DR WITRY OF METZ.

Catherine Fillgung, a Dominican nun, was operated upon for a tumour while in ecstasy. The operation lasted half an hour. In her ecstasies she claims to have suffered the pains of Christ's passion, which she seemed to witness. She also claims a miraculous cure of another illness, which she attributes to the intervention of the

Virgin Mary. Several doctors bear witness to her lofty character and to the increased activity for good following her ecstasies.

“Notes on M. F. Cazzamalli’s Recent Experiments.” M. H. AZAM.

These experiments deal with the physical reactions resulting from thought or emotion.

In the *Chronique* is an obituary notice of Mrs Henry Sidgwick. There is also an account of Mr Harry Price’s attempt to broadcast the voice of a ghost.

Bulletin du Conseil de Recherches Métapsychiques de Belgique (Jan-April 1936).

In an article “Le Glas du Spiritisme” (The Knell of Spiritism) Maurice Schaerer bids farewell to Professor Richet and exalts Metapsychic Research at the expense of Spiritism.

An article by Maurice Schaerer on F. A. Baker, the biologist, and his work.

A translation of F. A. Baker’s lecture at the Royal College of Science on “Finality and Natural Selection.”

Zeitschrift für metapsychische Forschung, 5th February, 1936.

On the theory of reincarnation. The editor sums up the discussion on reincarnation.

Mysticism and occultism in German literature by Prof. Johannes Kasnacich, of Graz. (continuation and conclusion).

The author of the article quotes a number of German prose-writers and poets to show how the German literary mind looks upon mysticism and occultism.

Vampirism. A discussion of vampirism in the widest sense of the word: Not only the traditional form of vampirism is discussed, but also the overweening influence (with physical effects) of one individual on another.

Dr Schröder writes an interesting (illustrated) article on cross-correspondences.

Zeitschrift für metapsychische Forschung. April 1936.

“The Problem of the Penetration of Matter. FRANZ ETTIG.

A discussion of the phenomena of apport.

“Thinking Animal” Series. MARIA SCHRÖDER. (Berlin)

A remarkable account of apparitions of a pet cat, “Hiddi,” claimed to have been seen by the author’s mother, and also on occasion by her father and a maid. Other cats in the house seemed to perceive the apparition.

“Schelling and the Oeeult.” P. WALLIS (Pastor).

Schelling's view was: “There is certainly a spirit world independent of man, just as there is a natural world independent of man.” Man is the link between the two worlds.

There is an obituary notice of Dr T. Glen Hamilton, “the physician, the man and the metapsyehical researeher”; also of Peter Johannsen and August Bruns, well-known German psyehieal researehers.

Tijdschrift voor Parapsyehologie. Mareh 1936.

“Dr W. H. C. Tenhaeff on Mediums and Mediumship.”

Dr Tenhaeff, continuing the exposition of his views, dilates on natural and indueed mediumship.

In Dr Tenhaeff's opinion people “see” ghosts because the ghost “controls” them. Thus eases of ghost-seeing become eases of indueed mediumship.

The main work of the parapsyehologist is to establish faets, but a tendency has lately become noticeeable towards the investigation of side-issues, such as the disposition or nature of mediums. The writer examines the question of the relation of nervous disorder (hysteria) to mediumship.

Mediums are generally of an exeitable, nervously unstable type, very emotional.

In a note he touehes on the relations of mediumship to madness, genius to madness and genius to mediumship. H. E. K.

ERRATA

Journal for July, p.268, l. 20, for “bring forward” read “develop.”

Journal for Oetober, p. 279, l. 1, for “Qualitative” read “Quantitative.”

THE *JOURNAL* IS PRINTED FOR PRIVATE CIRCULATION

The attention of Members and Assoeiates is drawn to the private nature of the *Journal*, from which no quotations should be made without the previous consent of the Council. Ever since the first issue of the *Journal* in 1884, much of the material appearing in it has been contributed on the definite assurance that the *Journal* is, as stated on the cover, issued “For private eireulation among Members and Assoeiates only.” The Council hope that all Members and Assoeiates will eontinue to co-operate with them in maintaining this privaey.

JOURNAL

OF THE

Society for Psychical Research

FORTHCOMING MEETING

THERE will be no Meeting of the Society in December, but particulars of the Meeting in January, which will be held on *Wednesday, 27 January 1937*, will be given in the next number of the *Journal*.

NEW MEMBERS

- Arbuthnot, Miss Mary E.**, Davies' Hotel, 10 Brompton Square, London, S.W. 3.
- Colquhoun, Miss B. M.**, 2 Kendall's Mews, George Street, London, W. 1.
- Craik, Lady**, 5a Dean's Yard, Westminster, London, S.W. 1.
- Farkas, Captain Gustav**, Albertville, Belgian Congo.
- Farrer, Mrs**, 2 Somerset Road, Wimbledon, London, S.W. 19.
- Holm, Knut H.**, c/o Messrs Anderson, Clayton & Co., Barranqueras, Argentine.
- Hydari, The Right Hon Sir Akbar, P.C.**, Hyderabad, Deccan, India.
- Taylor, Captain H. B., R.N.**, United Service Club, Pall Mall, London, S.W. 1.
- Wendt, Mrs Henry**, 126 Chapin Parkway, Buffalo, N.Y., U.S.A.

Elected, 25 November 1936

- Flower, Mrs Fordham**, 3 Smith Square, Westminster, London, S.W. 1.
- Leslie, Mrs J.**, Stragglethorpe Hall, Brant, Broughton, Lincoln.
- Littlewood, Professor J.**, Trinity College, Cambridge.
- Narain, Narsingh, B.A., P.C.S.**, Hardoi, United Provinces, India.
- Richmond, Mrs Kenneth**, 82 North End Road, London, N.W. 11.
- Sitwell, Mrs**, 114 Grosvenor Road, London, S.W. 1.

MEETINGS OF THE COUNCIL

THE 345th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 28 October 1936, at 4 p.m., THE PRESIDENT in the Chair. There were also present: Mr Gerald Heard, Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., The Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mr H. F. Saltmarsh, Admiral the Hon. A. C. Strutt, R.N., and the Rev. C. Drayton Thomas; also Mr C. V. C. Herbert, Research Officer, and Miss Isabel Newton, Secretary.

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

Nine new Members were elected. Their names and addresses are given above.

THE 346th Meeting of the Council was held at 31 Tavistock Square, London, W.C. 1, on Wednesday, 25 November 1936, at 4.15 p.m., THE PRESIDENT in the Chair. There were also present: Lord Charles Hope, Miss Ina Jephson, Sir Lawrence Jones, Bart., The Hon. Mrs Alfred Lyttelton, G.B.E., Mr W. H. Salter, Mr S. G. Soal, Admiral the Hon. A. C. Strutt, R.N., the Rev. C. Drayton Thomas and Miss Nea Walker; also Mr C. V. C. Herbert, Research Officer, and Miss Isabel Newton, Secretary.

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

Six new Members were elected. Their names and addresses are given above.

Mr Herbert reported that at the meeting of the Research Committee held on 29 October 1936, the following were co-opted as members of the Research Committee: Mr Eric Cuddon, Mr C. C. L. Gregory, Lord Charles Hope, Mr Kenneth Richmond and Mr G. N. M. Tyrrell.

PRIVATE MEETINGS

THE 144th Private Meeting of the Society was held on Wednesday 28 October 1936, at 5.30 p.m., MR H. F. SALTSMARSH in the Chair.

THE REV. C. DRAYTON THOMAS read a paper on "The Significance of Book Tests", which was illustrated by lantern slides.

A discussion followed in which the following members took part: Mr J. W. Findlater, Mrs Footner, Rev. J. W. Hayes, Mr C. V. C. Herbert, Mr J. Hettinger, the Hon. Mrs Alfred Lyttelton, G.B.E., Count Perovsky-Petrovo-Solovovo, Mr Kenneth Richmond, Mr A. W. Trethewy, Mr G. N. M. Tyrrell.

An extract from Mr Drayton Thomas's paper, and a summary of one by Mr Maby, read at the Private Meeting held on 27 May, are printed below.

THE 145th Private Meeting of the Society was held in the Society's Library on Wednesday, 25 November 1936, at 5.30 p.m., THE PRESIDENT in the Chair.

Dr H. Godwin Baynes read a paper on "The Ghost as a Psychic Phenomenon".

SUMMARY OF A PAPER BY MR. J. CECIL MABY ON
"THE FLOWER MEDIUM: MISS HYLDA LEWIS"

In explaining the reasons which led a group of Oxford investigators to invite Miss Hylda Lewis, the "Flower Medium," to give sittings to them in the Autumn of 1935, Mr Maby referred to the unfavourable opinion on her phenomena formed by many investigators who had already sat with her, including several members of the S.P.R., and particularly to an occasion in the Summer of 1935 when she had signed a "confession", which was published in the press.

Many of Miss Lewis's friends, however, considered that she had not been fairly treated on that occasion, and that in fairness to her she should be given an opportunity of having her powers tested by a group combining critical judgment with a sympathetic and friendly atmosphere. It was accordingly arranged that Miss May Walker, in whose company Miss Lewis had visited various investigators at home and abroad, should bring her to Oxford towards the end of September 1935, where she should live as Mr Maby's guest in his house, No. 3 Rawlinson Road. While living at Oxford it was intended that she should enjoy complete liberty of action and movement, except during the actual sittings, being, at the same time, kept unobtrusively under as close observation by Mr Maby and the other members of his household as the circumstances permitted. This intention was duly carried out.

Mr Maby's household consisted of himself and his wife, their infant son, a laboratory assistant (Mr Meyer), and a maid (Miss Loose). The investigating group included Miss May Walker, the late Mr E. S. Thomas, Prof. H. H. Price (all members of the S.P.R.), Col. Dr E. F. G. Tucker, a former member, Mr F. A. Baker and Miss C. M. Bryson. Care was taken that the medium should get to know all the sitters in the ordinary way of sociability before giving them sittings.

Miss Lewis arrived in Oxford on Saturday, 28 September, 1935, and remained there till the following Wednesday, 2 October. She gave several sittings under appropriate control conditions, and also produced phenomena at times when no sitting had been arranged. Her phenomena could be classified under three heads (1) supposed supernormal "communications"; (2) apports or materialised objects; (3) lights.

Mr Maby and his fellow investigators came to the conclusion that none of the phenomena were certainly genuine, and that most of them certainly were not. Thus the supposed communications contained no information not derivable from a Family Bible and various papers to which the medium, as guest, had easy access, or from conversations at meal times.

The apports or materialisations consisted of apples of a variety grown in Mr Maby's garden and of flowers and small toys such as could have been purchased in the Oxford shops, which she had opportunities of visiting. Mr Maby gave in detail reasons for concluding that objects so purchased were later produced at sittings. Thus flowers were found concealed in the bedroom exactly corresponding to those produced at two subsequent sittings, and later again cut stalks, loose petals and wilted flowers were discovered in circumstances that seemed to Mr Maby, who is a trained botanist, to leave no doubt that they were the relics of apports or materialisations already observed.

The investigators were unable completely to elucidate the problem of the lights without violation of the conditions they had agreed to against forcible stripping and search of the medium, but Mr Maby mentioned several observations regarding them which seemed strongly to negative any supernormal hypothesis.

Mr Maby's paper can be seen in full at the Society's rooms. Mr Maby has also kindly deposited with the Society a fuller report of the Oxford investigations, which has been placed with the considerable volume of other reports, several of them confidential, on the same medium received by the Society.

EXTRACT FROM A PAPER BY THE REV. C. DRAYTON THOMAS ON
"THE SIGNIFICANCE OF BOOK TESTS"

"DR V. J. Woolley, while the Hon. Research Officer of the Society for Psychical Research, devised a method by which we could experiment with a book, the identity of which should meanwhile remain unknown to anyone on earth.

Described briefly his method was as follows : He asked a friend to send him a parcel of books without information about the contents. He asked others to do the same. This brought into his possession packets containing a variety of books. These he placed unopened in a canvas sack. Taking this into a dark room Dr Woolley inserted his hands into the sack and opened the parcels ; all the books were thus mingled together. He then took a smaller sack and placing its mouth over that of the larger pushed a single book up into the small sack. This smaller sack he then closed by a cord passing through eyelets round the rim, and placed a lead seal with his private stamp over the cord. It was then put into a large envelope which was gummed up and then secured by rice paper labels bearing marks of identification, and this was finally wrapped in brown paper for protection. The larger sack containing the remaining books for future use was then secured in a similar manner, and was taken charge of by Mrs Brackenbury, Dr Woolley's assistant, and remained in her possession till required again. The single book in the parcel was handed to me for the experiment.

It is perhaps worth noting that the three people who supplied the books were not known to each other and did not meet during the course of the experiment, so that no list of the books in the sack could have been compiled.

We now had a book taken at random in such a way that no earthly mind could be aware of its title.

I placed it in a position easy to describe—in the bookcase behind my study door, third shelf up and at end nearest the door.

Arrangements being thus complete I asked my father if he would endeavour to ascertain a few facts about this book, sufficient to make clear that he had gained access to it. He agreed to try and at a subsequent sitting told me his findings. The five statements were taken down by my stenographer and, when typed out, were handed to Dr Woolley, who then in my presence opened the packet, after satisfying himself that the seals were intact. Here in brief form we shall see what was stated and how it compared with the sealed book.

(1) "It is not complete, should have other books with it although it appears like a complete volume."

This book has a loose cover which reads "No. 457, Everyman's Library"; and the final words of the book are "End Vol. 2."

(2) "There is a person's name beginning with 'G' composed of several letters, like the writer or owner of the book."

On the fly leaf there is printed a single sentence, and underneath it the author's name, Glanvill.

(3) "The outside of the book would suggest things of the old

world and of the new world, the old style and the new, old days and present days."

This was most apposite. The title reads "Socratic Discourses by Plato and Xenophon. Introduction by A. D. Lindsay M.A. Classical. Everyman's Library. J. M. Dent & Sons Ltd. E. P. Dutton."

(4) "Old style print right at the beginning when you look at it you would say they don't put it like that nowadays. This was at the very beginning of the book."

This is entirely correct. The title page is surrounded by heavy black conventional design and the printing imitates ancient wood type. The whole thing aims to look antique.

(5) "Counting from the commencement on the 13th page find something to do with geometry."

Counting as directed brought us to page xiii of the Introduction. The first seven lines of this page read as follows :

"For if we have learnt geometry and housebuilding, we are in having done so housebuilders and geometers. That is why Socrates inquired what justice is, and not how and from what conditions it comes into being. Now this is perfectly right in the theoretical sciences ; for astronomy and natural science and geometry are concerned with nothing but the knowledge and contemplation of the nature of the subject of these sciences."

It will be seen that geometry is named three times in this quotation. I can find no further reference to geometry, either in the context or elsewhere in the book.

The success of the above experiment indicated, what I had already proved to my own satisfaction, that my communicator could give me information about a book which I had not read. We can therefore . . . proceed with entire certainty that the sitter's memory *need not be* the source from which book test information is derived."

Seven other experiments were carried out under the same conditions, of which four were complete failures. The second and third experiments (described by Mr Drayton Thomas in his paper), although they presented points of interest, were by no means as successful as the first. Mr Thomas concludes : "One is reminded of the way in which the book tests, given to Mrs Leonard's sitters in 1917-18 so successfully, later began to fail in quality and finally ceased. It is as though interest diminished somewhere or in some one, or possibly some aspect of the mediumistic mind resented being used in this way. The fact remains that a particular line of experiment seems to play itself out and cannot be successfully revived again until after a period of rest."

Mr Drayton Thomas included in his paper two other successful book tests. One of these, concerning a place in which he and his father, the communicator, had worked together, is of interest because "it brought together facts which were unknown in their entirety to any single mind on earth and included some that were personal to my father and me." This test has already been described in a book by Mr Thomas now out of print and he hopes to re-publish it in a book which is to appear shortly.

The remaining test was concerned with a volume of Byron which had been the property of Mr Thomas's grandfather, and showed evidence of having been "devised and carried through by the memory and observation of my grandfather, assisted by his son Alfred in whose possession the book had been." This test also Mr Thomas hopes to publish in detail later.

SPONTANEOUS CASE. A PRECOGNITIVE DREAM

IN response to the appeal this year that Members should send in accounts of Spontaneous Cases more freely, the following account of a precognitive dream came in letter form from Lt. Colonel C. R. L. FitzGerald, a new member. Unfortunately, although corroborated in some degree, the date of the occurrence is 1913, and there is no contemporary record; so that the case cannot be assigned the importance possibly really due to it, and is therefore not numbered. Being, however, interesting in its subject matter, it has been thought worth printing. Moreover, in sending it, Colonel FitzGerald remarked, "I met one [of the two ladies mentioned in the account, page 309] quite recently in the street, after an interval of twenty-three years. She recognised me, and the first thing she said was—'Do you remember that dream you had about Colonel Rowlandson's death?'" This led to a request that Colonel FitzGerald should trace the lady's address and ask for her version of the story without reminding her of further details, for the conversation they had had on meeting had not entered further into the story than her spontaneous reminder. Colonel FitzGerald very kindly took this trouble, and his friend, Mrs Bromley, equally kindly responded, sending her account direct to the Hon. Editor of the *Journal*, and it is printed after the story of the dream. The date in Mrs Bromley's account is 1911, but she indicates in a covering note that she is not sure of the year, adding however that "it was a curious happening and impressed itself on my memory."

The correspondence and original letter are all filed at the Office of the S.P.R. In some preliminary pages of the letter in which he relates the dream, Colonel FitzGerald says that, while home on

leave in 1910, he heard that a certain Colonel A. T. Rowlandson was being transferred to command the regiment to which he himself (then a Captain) belonged—the 126th Baluchistan Infantry of the Indian Army; and that, by chance, he discovered that his own grandfather and Colonel Rowlandson's grandfather had been very great friends.

This the then Captain FitzGerald never mentioned to Colonel Rowlandson, since he felt it might seem like courting favour with a new commanding officer, and although the correspondence shows that it is likely that Colonel Rowlandson did hear of it he never mentioned it to the junior officer. Colonel FitzGerald says that he “attaches importance to the link of the two grandfathers, though it may not be relevant to the character of the dream,” and for this reason the link is briefly indicated above. He then goes on to tell his story, saying—

“In any case, the Colonel became very attached to me: it would be no exaggeration to say that I was markedly his favourite in the regiment.

In 1912 in Hong Kong he made me his adjutant, and we worked in close co-operation and understanding until his death. In July 1913, I think on the night of 11th-12th (the exactness of the date does not appear to be material) I had a remarkable dream. It was definitely divided into three dream pictures in which I was taking no part—merely an observer. They were, all three, very vivid.

In the first, I saw an open, grass-grown space, the grass short as in a playing-field rather than a meadow. There was nothing on it, and it was bordered in the near distance by a pale, rather hazy blue, which I presume to have been sky.

Suddenly from behind my right I saw Colonel A. T. Rowlandson ride out on to the grass. He was mounted on a light grey, or white, pony and was dressed rather peculiarly—a white cotton polo shirt, and *white flannel tennis trousers which were strapped at the knees to facilitate riding*.¹ The picture was then abruptly blotted out. I saw nothing further.

Almost immediately a second vivid picture appeared. The same field, Colonel Rowlandson was lying on his back, his head almost touching my feet, his right leg crossed under his left leg. He wore the same clothes as in the first picture. His eyes were closed and he did not move. Then I heard, coming apparently from behind me, in a clear and level voice, the words: “Take care for the Colonel's head.”

¹ Italics here and on p. 310 are mine, since the dress was, I learn, “very unusual” in respect of the white flannel tennis trousers strapped at the knee.

That dream too disappeared abruptly, and a third scene took its place.

Again the grass-grown space or field, nothing on it. Then again from behind my right came a dhoolie (Indian litter used as ambulance) covered with a dark green material and borne by two bearers in the uniform of the military hospital. They were bearing the dhoolie away. It was covered up. I could not see who was in it. Then the same voice behind me spoke again and said: "The Colonel is dead."

I awoke. It was early—about 6 a.m. The dream-pictures left a very clear impression on my mind. All that morning I had an almost indescribable sensation. Great clearness of mind, and yet the feeling of being in a dream, a sort of elation as though I had been in touch with a much superior intelligence.

Before the dream, I had known that the Colonel was riding in some sports that afternoon, but it had never dawned on my mind that anything might happen to him.

All that morning, at breakfast and later in the regimental office, I could not get the dream out of my mind. The Colonel and I worked in the office till nearly one o'clock, when he rose to go.

At the door of the office he turned to me and slapping his thigh with his cane, said, "I'm very gay today, FitzGerald, I'm luncheoning with the —s, riding in the gymkhana this afternoon and dining with the —s tonight."

At that moment I had a strong urge to tell him of my dream, but I knew him too well to think that for a moment he would consider it seriously. He was not made that way. And then he was gone.

After lunch I was delayed by some duties and did not start for the sports' ground till towards 4 p.m. As I passed the gate on to the ground, a low roar went up from the crowd of soldiers, sepoy and others who were looking on. Something had happened.

I hurried forward to the outer fringe of the crowd. Two ladies, whom I knew, were standing there. I joined them and on tiptoe looked over the heads of the crowd. On the far side of the ground Colonel Rowlandson was lying on the grass beside his horse, but he got up and began to brush his white sun helmet with his arm. I was glad to see that he seemed to be fairly all right, and I quite cheerfully told the two ladies that I was relieved to see him thus, because I had dreamed early that morning that he had had a serious accident.

Then I invited the ladies to have tea with me, and we moved to the tea tent. I selected a table at the side of the tent, the wall of which had been taken away, so that we were sitting right on the edge of the fairway past the tent.

In a very short time the Colonel came by. *He was dressed as I had seen him in my dream.* He looked very pale and carried his head very high. He did not look our way. He was looking straight in front of him.

I said to the ladies, "I am glad to see that he is not seriously hurt, because I dreamed that he had been killed." I had hardly spoken the words when I saw some people hurrying up. On the ground, within a yard of our table lay Colonel Rowlandson, on his back, his head towards us, his eyes closed and his right leg crossed under his left, exactly as I had seen him in my second dream-picture.

Soon the rumour got over the ground that I had dreamed this dream. The Second-in-Command, Major Wooldridge¹ came up to ask me about it. I said, "the Colonel will die." He said, "Nonsense, Fitz, I don't believe it."

Colonel Rowlandson never regained consciousness. A little later he was carried from the ground in a green-covered army hospital dhoolie by two bearers. The hospital was close by. He died immediately after reaching it.

There is one curious discrepancy in this dream, which it would interest me to have explained. I dreamt that the Colonel was riding a light-grey or whitish horse, but actually his horse was a very dark brown, and I knew this well. I had previously had a light-grey Arab pony and this is the only connection I can make with the matter. But the Colonel had never ridden that pony of mine."

CORROBORATIVE STATEMENT SENT BY MRS H. BROMLEY FROM
5 WESTMINSTER, SHAFTESBURY ROAD, SOUTHSEA

DATED JUNE 10, 1936

"In the summer of 1911, my husband and I attended a Gymkhana given by the officers of the 126th Baluchis, stationed at Kowloon, Hong Kong.

We were watching the sports in company with some friends, Colonel and Mrs Stewart, when we were joined by Captain—now Colonel C. R. L. FitzGerald.

He was unhappy and disturbed over a vivid dream he had had the previous night—a dream which had very much impressed him, and which he then related to us.

In his dream he saw the Sports in progress, just as it was, at the moment of speaking. When the pig-sticking event took place, he saw his Colonel taking part—as he rode round the field, he was thrown

¹ Died soon after the Great War. C.R.L.F.

heavily. A crowd rushed to assist him but he was fatally injured and was carried dead off the field.

Our conversation turned on the fallacy of relying on dreams, and the folly of allowing them to affect us, etc. etc., as we could see that Captain FitzGerald was anxious, and nervous.

After some minutes the pig-sticking event came on the programme, and among the officers taking part was the Colonel of his regiment.

He came galloping past us, and on—when suddenly he took a violent toss, and was thrown headlong from his mount. A crowd rushed to assist him. After lying on the ground for some minutes he was helped to his feet, and strode off as if unhurt.

With a great sense of relief, we proceeded to the marquee for tea.

Just as we sat down, we saw the Colonel coming walking towards the marquee. As he came in line with our table he stumbled and fell heavily.

He was carried off on a stretcher, and died on his way to hospital."

NOTES ON RESEARCH UNDERTAKEN

AN interesting case of "*poltergeist*" manifestations in a house near Leeds was ably investigated on behalf of the Society by Mr P. H. Guénault of Leeds University. Mr Guénault's report may be seen at the Society's rooms.

The quantitative "book test" experiment is being continued with the co-operation of Mrs Leonard and Mr Irving. Two tests have now been carried out.

A few "spontaneous cases" have been received, one of which, it is hoped, may prove of sufficient evidential value for printing. The investigation of this case is being carried out by the Secretary, Miss Newton.

A claim to have discovered a connection between certain current events and forecasts made by astrological methods is being investigated. It is not yet possible to say if the connection is real or imaginary.

C. V. C. H.

CORRESPONDENCE

MEMORY AND TELEPATHY

To the Hon. Editor of THE JOURNAL

MADAM,—It is a pity that Mr. Carden, in his letter in the October *Journal*, should make such an extraordinary error as he does. He asserts that "Brain cells grow and divide to form daughter cells."

It is, however, well-known that nerve-cells, once they have become differentiated, never divide. This holds for all higher animals so far investigated.

When, however, he further identifies an engram with "the ultra-microscopic particle of electricity within the nucleus of a brain-cell," one may be pardoned for not taking his views very seriously. Whatever the material basis of memory be called, there are few who would deny that it must exist: but an elementary knowledge of physiology should make it clear that even the simplest memory-trace must involve a very large number of nerve-cells.

Yours etc.

JULIAN S. HUXLEY.

SCARCITY OF SPONTANEOUS CASES

To the Hon. Editor of THE JOURNAL

MADAM,— I am very much obliged to Mr Salter for quoting, in the *Journal* for July, from *Phantasms of the Living* an excellent passage which seems wholly to exonerate the founders of the Society from the insidious logical error I was castigating. At the same time I also had definite cases in mind when I censured certain officials of the Society, though, fortunately, none of them is any longer responsible for the handling of "spontaneous" cases. For example, a review by me of one of Mr Podmore's later and ultra-septical books was not allowed to appear, because I had made against him the point in question, that of not allowing the evidence to accumulate. Another, much later, case was that of an attractive story, involving an apparently supernormal photograph (by an amateur), which was rejected because, the official assured me, he had hundreds of much better cases he had not published. "Well", I retorted, "if so, I can only say that you are suppressing a great deal of valuable evidence!" The tendency to treat each case as standing alone is a real danger, which is always recurring.

I am, Madam, etc.

F. C. S. SCHILLER.

3710 GLOBE AVE., LOS ANGELES, CALIF.

REVIEW

Lord Halifax's Ghost Book. With a foreword by Viscount Halifax, K.G. Geoffrey Bles. London 1936. Price 8s. 6d. net.

The impression produced by this book is twofold. On the one hand its publication is to be welcomed, for it seems at first sight calculated to make our researches more popular and more widely known; also to prove that even bearers of illustrious names do not eschew ghost stories at times, nay, like dabbling in them. On the other hand, *Lord Halifax's Ghost Book* leaves much to be desired from the standpoint of the quality of some of the records.

Of the narratives a few are good but others are distinctly unsatisfactory.

To begin with, two narratives (pp. 196 and 201) have nothing to do with ghosts, whether genuine or fraudulent. The second of these two in particular ("The Bordeaux Diligence") is even an obvious joke.

Of the last narrative in the book ("Colonel P's Ghost Story") we read (p. 211) that it "makes no pretence at authenticity, but was of Lord Halifax's own invention."

The story entitled "The Gentleman with the Latch-key," was "told by Lord Falmouth to Lord Grenfell, Lord Methuen and his son Paul (the present Lord Methuen). Lord Falmouth had been told the story by a friend, who had heard it from the man to whom it happened" (p. 193). So far as Lord Halifax is concerned the story was therefore at least third, possibly fourth-hand.

On page 183 there is the beginning of a story told Lord Halifax in December 1920 by a nephew of his, Mr Charles Dundas, who heard it from a military aviator named Villiers. Such as it is, it might therefore be regarded as third-hand—which is bad enough—were it not for the fact that Villiers himself was certainly no eye-witness. Let us not, however, regret too much that the case cannot be properly authenticated: the idea that an airman, be he an Australian, is capable, if a loser at poker, of wreaking an awful *post mortem* vengeance on his co-players is rather too repugnant to be entertained!

In such a story as that of Miss Johnes whose future is forecast by Lord Lytton (p. 209) there are no ghosts whatever. It is true that Lord Lytton's predictions are alleged to have been quite correct, but—the story was sent to Lord Halifax by Lady Margaret Shelley, about whose relation to Miss Johnes we know nothing.

In some cases the incidents described may be termed puzzling rather than suggestive of something supernatural; *e.g.* the ones

concerning the Rev. Speneer Nairne and "evidently" written by himself, where mistaken identity seems after all a possible explanation (pp. 111-117).

This list of unsatisfactory cases in *Lord Halifax's Ghost Book* is unfortunately not complete, but the instances quoted are sufficient—and sufficiently characteristic.

Let us now say a few words about the more or less satisfactory cases. These are not numerous, but there are some which deserve attention.

I will mention the alleged hauntings in a house at Lille (pp. 11-19); the story entitled "The Grey Man at Wrotham," where—exceptionally—corroborative evidence is supplied by a nurse (pp. 39-45); the case of the ghost seen at Benishaw (the country house of the Sitwells, in Derbyshire) by two persons successively on the same day (pp. 123-125); the case described, in a letter to Lord Halifax, by a Charles G. S. (pp. 143-147); possibly also the story narrated on pp. 141-143. This last case is, it should be said, second-hand, but Lord Halifax tells us: "To his dying day my old friend Reginald Easton, the artist, persisted in the truth of the following story." In this case the ghost of an old murderess appears in a haunted room on seven consecutive nights, thus presenting us with a nut somewhat hard to crack. But perhaps this is an insufficient reason for disbelieving altogether in Mr Easton's account.

Finally I will mention the "monk" seen in August 1912 in Bolton Abbey by the present Marquis of Hartington. King George V was staying at the Abbey at the time.

The very dramatic "Telephone at the Oratory" case is, I think, susceptible of a normal explanation. Our readers will find it in the *S.P.R. Journal*, Vol. XIX, p. 83, but I may be permitted to make a few comments on it. Let us note first that Father C.'s experience cannot, in any case, be explained by a *post mortem* agency, since Mrs P. was alive at the time. The experience in question may therefore have been due to an "orthodox" telepathic impact. But even such an explanation may seem superfluous. Father C. had undoubtedly been strongly impressed by Mrs P.'s state; he tells us that he regretted having promised her doctor not to administer the last rites on his first visit; it is perfectly conceivable that such a state of mind may have induced in him a "borderland" hallucination or quasi-hallucination connected with the telephone message. The coincidence may therefore have been purely fortuitous.

My attention has been particularly attracted by a short story (alas, third-hand) called "The Haunted Bungalow," because such as it stands it seems to confirm a hypothesis I have long thought

plausible with a view to explaining some at least of the alleged hauntings. In 1871 a friend of the above-mentioned Mr Dundas, Troward by name, and his wife, when in India, had to spend the night in a bungalow which had been hastily got ready for them and which it seems had a bad reputation with the servants.

“ In the middle of the night Mr Troward was awakened by a loud report, followed by terrified cries and screams from his wife. When he asked her what was the matter, she said that a man in a grey suit had come up to the side of the bed and leant over her, saying : ‘ Lie still, I shall not hurt you.’ He had then fired a pistol or gun across her over the bed ” (p. 187). In the morning it was ascertained by the Trowards that a Mr de Courcy, formerly Commissioner at Hoshiarpur, had shot himself in the middle of the night in this very bungalow, leaning across the bed and saying to Mrs de Courcy : “ I shall not hurt you.”

Let us suppose that the incident occurred as described. Surely it suggests much less continuous action on the part of the unfortunate dead man, senselessly repeating the same fatal act and the same moving words, than perception of what occurred once by a person of peculiar psycho-physical organisation. *How* and *on what* was produced the enigmatic impression, later at times perceived by individuals specially qualified, we do not know and possibly will never know. But—again supposing that such episodes did really occur and still do occur—surely such an “ explanation ” is more plausible than any direct “ haunting ” in the proper sense of the term.

I am not aware of the name of the critic who was the first to frame such a hypothesis, but I think it is one of the most rational ever formulated in the realm—alas not too devoid of very fantastic theories!—of Psychological Research.

PEROVSKY-PETROVO-SOLOVOVO.

NOTES ON PERIODICALS

Revue Metapsychique : July–August 1936.

“ Impressions of Presence.” Dr F. Moutier.

(Lecture given at the Metapsychic Institute, Paris, Dec. 17, 1935).

The lecturer gives an interesting case of projection of presence by a patient of his, perceived by himself, and a number of cases of apparitions of the dead. He discusses the circumstances in which phenomena of the kind usually occur, their nature and the reasons for their appearance. He is inclined to regard them all as subjective.

“Le Curé d’Ars” : (St. John Baptist Vianney), by Dr E. Lenglet.

The extraordinary manifestations which occurred in connection with this modern saint are discussed—his power of reading the minds of his penitents, his miracles (a tumour cured by his touch), his struggles against demons who troubled him, his visions, his levitation while at prayer.

Zeitschrift für Metapsyehische Forschung.

August 1936.—“Can a burnt object be restored?” Dr E. Pap von Chengery. Dr Chengery describes (among other phenomena) the burning before his eyes by a medium in Riga of a marked piece of paper, which was then restored.

“Ways to the searching out of Fate.”

By D. Hans Hanig. A paper on Astrology. The author argues that astrology should be set among the sciences.

“Verkettungen” (Chaining together).

By Dr Schwab. This is an account of two (astrologically) linked lives.

“Mysticism and Occultism in German Literature.”

By Professor J. Kasnacich. A further article in the series. Mystical passages from the works of a number of German poets and prose writers are given.

“The Problem of Thinking Animals.”

By the Editor. The homing faculty and sense of direction in animals. The author discusses the homing faculty and sense of direction of birds, dogs and cats. He considers that desire and will make the bird or animal find its way home and ascribes to this homing phenomenon a psychic origin.

“Dr Moser’s ‘Occultism.’”

A controversy concerning Dr Fanny Moser’s attack on the medium Frau Maria Silbert.

Among the *Reviews* the most interesting is the review of Professor Hermann Mandel’s book: *A Systematic Contribution to the History of Human Belief and Religion*. It has an appendix entitled “Human Belief in Immortality.” It is published by J. A. Barth, Leipzig.

He regards the supernormal consciousness as the means by which man is and has always been able to come into contact with the invisible world.

“Metaplastism in the ‘Kate Goligher Circle.’”

This is an account of sittings which took place with Kate Goligher (Donaldson) in March 1936. The sitters (experimenters) were her husband, Mr Donaldson, Mr Stephenson and Mr Warrick.

Short Original Communications.

A note on electric emanations from Count de Berenyi, which are said to move the needle of a compass and light an electric Neon lamp which takes a current of at least 130 volts to light it.

A case of foresight and a note on the “Grey Ghost of Neustrelitz.” There is also a note on the perception by animals of the supernormal, and an article (a continuation of the series “Our Knowledge of Life after Death”) by Prof. C. Schröder. It gives details of “spirit-artists” and “spirit-pictures.”

Tydschrift voor Parapsychologie. Dr P. A. Dietz.

“Critical considerations concerning retrograde psychoscopy.”

This article gives an account of some remarkable experiments with the Mexican trance-psychometrist, Senora de Reyes de Z. The author criticises severely some of her results but is impressed by a case in which she gave correct particulars of a deceased person from a sealed letter enclosed in another sealed letter. Nobody in the room knew the contents of either letter. The covering letter had been written in Tokio. The man who wrote the *inner* letter had gone down in the *Lusitania*. (The article gives very full particulars of precautions and details of dates etc.). The psychometrist described the scene on the ship before the disaster and after it. She described (accurately) the writer of the letter. After waking from trance she gave many correct details concerning the writer of the letter and his widow. The author of the article discusses this case very fully from every point of view.

“Foresight or Chance.” J. C. M. Kruisinga.

A carefully tabulated series of impressions and their fulfilments are given. Prefaced to this is a discussion of the probabilities of coincidence.

H. E. K.

ERRATA

Journal for November, 1936, page 294 line 10, for " a speciality of a fad," read " a speciality or a fad."

Page 295, line 6 ; the item referred to had been already printed in *Journal* for June, page 252.

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