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PROCEEDINGS

OF THE

Society for Psychical Research

PART 144

IN MEMORY OF CHARLES RICHET

By Oliver Lodge

On December 3 last there died a great savant, recognised as a physiologist all over the world, and yet keenly interested in our researches. This was an extraordinary combination, and speaks well for the comprehensive character of Professor Richet's activities. For observe that a physiologist is wholly concerned with the material mechanism of the body, with its secretions, the effect of drugs upon it, with its nervous reactions, and with the working of its different organs. No explanation of its behaviour except in terms of this procedure can be contemplated or even tolerated. Professor Richet is speaking quite orthodox language when he says : " I cannot believe that memory can exist without the anatomical and physiological integrity of the brain. Whenever there is no more oxygen, whenever the temperature is either too low or too high, when there are a few drops of atropine or morphine or chloroform introduced into the blood, whenever the course of cerebral irrigation is stopped—memory alters and disappears."

Sooner than contemplate an explanation in psychic terms, Professor Richet, speaking for all or practically all physiologists, would prefer to have no hypothesis at all, and simply say that many of the facts which we adduce in support of our views are merely evidence of what he calls " cryptaesthesia," which " *can reveal to us fragments of the real*—fragments which seem to have no connexion with space and time."

And further on he says : " When I speak of cryptaesthesia I indicate a fact—the perception of reality by extra-sensorial channels. I do not seek to go beyond that, and as yet science has no right to go beyond that."

Nevertheless he admits some extraordinary phenomena, extraordinary facts, he says, that is to say Precognitions: "I by no means deny the reality of certain premonitions; I have quoted remarkable examples of them which have happened to me personally: and in the annals of our science there are astonishing examples. . . . It is an undeniable fact; and it proves to us the sheer impossibility, as yet, of finding any explanation for metapsychic phenomena."

The quotations are from Vol. XXXIV of *Proceedings*, from a short paper by Professor Richet against the possibility of survival, wherein he maintains "that the most reasonable hypothesis is the unknown hypothesis X, which it will be for the future to develop".

For a review of Professor Richet's beliefs I wish to cite this whole volume as giving incidentally evidence both for and against survival. In it I review his great "Traité de Métapsychique", which he hoped would be used some day by any University course upon the subject; and I concluded by a somewhat playful sequel as to the points on which we differed.

He has now presumably entered on that future existence established by our own investigations, and disbelieved in by him: and he will perhaps try to communicate sooner or later, and will find himself up against those difficulties of establishing his identity which I there summarise. (See vol. xxxiv. pages 100 to 104.)

Richet not only admitted the truth of what he called "subjective metapsychics" including the incomprehensible character of precognitions, he was ready to admit some of the phenomena of objective metapsychics, and thoroughly accepted those which are commonly explained in terms of ectoplasm, including telekinesis and materialisations, always however assuming that a materialisation did not represent anything surviving but must be taken as representation of something which has existed but which no longer exists; for example, a construction of the body or parts of the body of a deceased person. He quite perceived that this is wholly contrary to the scientific knowledge of the time. He says that to admit it is to enter a world absolutely unknown, and if some day we have to admit it we shall be plunged into an abyss of deeper and deeper mysteries. At present in his opinion "we have understood nothing, absolutely nothing, of all these phenomena". And in that ignorance he is content to remain, though he accepts the evidence for both the subjective and objective metapsychics, as bare facts; that is to say for the psychical as well as the physical phenomena.

In accepting the facts without any explanation he shows a remarkable openness of mind superior to that of most of his colleagues. Yet in refusing to contemplate a spiritistic hypothesis and retaining the agnostic position he puts himself in harmony with them and with orthodox science as at present understood. There is no doubt that the views of orthodox science will some day have to change. It will have to admit that it has been wholly wrong in limiting its attention to the material aspect of things, and as soon as the spiritual is generally accepted it will have revolutionised its procedure, and entered on a path to which we can affix no limits and of which it is not easy to forecast the end.

And now a few words concerning Richet the man. He was a specially accomplished man of science, and an orator whom it was a privilege to hear; he was a brilliant conversationalist as well as a renowned physiologist, and one who did not scruple to pursue truth into regions which his colleagues, and indeed he himself, regarded as unpopular and absurd. As a man of letters and an appreciator of good literature, he could hold his own, and many were the verses stored in his memory which he could pour forth at any opportunity.

Through F. W. H. Myers I made his acquaintance, and I had many opportunities of meeting him in the 'eighties and 'nineties of last century, when he came over to pay a visit to his great friends, Myers and his wife, at Leckhampton House, Cambridge. So it happened that when Richet invited Myers to come and pay him a visit at the Ile Ribaud, in the Mediterranean, off Hyères, one summer in August when he was entertaining the physical medium, Eusapia Palladino, I was included in the invitation and travelled to the South of France in the company of Myers. There I had my first opportunity of seeing some of the physical phenomena of Spiritualism, well displayed under exceptionally good circumstances. The main room of the house on the island was converted into a séance room and kept for the purpose. There we sat each evening for a couple of hours, and the phenomena occurred just as if some confederate had been introduced into the room and was free to walk about, clutch at the people present, and move things; but no confederate was present or was possible under the circumstances, and I became gradually convinced that certain phenomena had occurred, of which I have given some account in the book *Past Years*, as part of my autobiography. My record was sent home, and was ultimately printed in the *Journal* of the S.P.R., vol. vi, pp. 306-360.

During the day, and at meals, I had the opportunity of hearing a torrent of brilliant conversation between Myers and Richet, Myers thoroughly convinced of survival, and Richet, as is well known, not accepting that view, even as explanatory of the mental phenomena. To the end, Richet in public remained an agnostic and a disbeliever in the spiritual explanation. In private, he has confessed to me that he was sometimes nearly bowled over by the evidence ; but, on the whole, he adhered to his lifelong conviction of the materialistic aspect of the universe. His scientific reputation was thereby saved, and his experience was all the more valuable because it testified only to the bare facts, which, although admittedly incredible from the scientific point of view, were not employed to bolster up any spiritualistic hypothesis. On those terms we agreed to differ, and yet remained close friends. He lost a favourite son in the war, but held no communication with him, though at times sorely tempted to do so.

In telling me of his latest book in July 1932 he wrote :

" ILE RIBAUD—GIENS
PAR HYÈRES (VAR).

MON CHER OLIVER,

Merci de votre lettre. Bien entendu tout ce qui touche le S.P.R. me va au cœur.

Je vais publier un livre intitulé *La Grande Espérance.* Et, sans être résolument spirite dans le sens de Conan Doyle et Allan Kardec, je me rapproche insensiblement de vos idées. Vous dirai-je—ce qui est rigoureusement vrai—que votre profonde et scientifique conviction a eu grande influence, très grande influence.

Toutes mes amitiés fidèles à vous et à vos chers enfants.

CHARLES RICHET."

This is an indication and a justification of what I mean when I say that he felt drawn towards the hypothesis of survival more than he permitted himself to express in face of his life-long study of physiology and the material view of the nature of man.

In his physiology he studied chiefly the ordinary processes of nutrition in health and disease: he was joint discoverer of the serum treatment; and he received a Nobel prize for his investigation of the detailed effect of drugs which is known as " anaphylaxis ". At a time when the great physiologist Sir Michael Foster was President of the British Association at Dover, Richet was invited to come over to give one of the two evening discourses in French ; and brilliantly he did it, keeping his whole audience spellbound by the beauty of the language and its delivery.

Assisted by Madame Richet, he lived a complete family life ; it was very interesting to drop in on a Sunday afternoon and find him surrounded by a large family, sons and daughters-in-law. As to myself I can truly say that his attitude to me was like that of a brother.

IN MEMORY OF EVERARD FEILDING

By E. N. Bennett

I first met Everard Feilding in Oxford nearly 40 years ago, when I was in residence as a Fellow of Hertford. He was at that time deeply interested in the movement to encourage young Catholics to enter the University and in this connexion visited the city from time to time. On these occasions he frequently dined with me, and his bright and interesting conversation made him always a welcome visitor to our Senior Common Room.

A year or two before, I had, through conversations with Frederic Myers, become deeply interested in the work of the Psychical Research Society, and Everard and I found ourselves on common ground from the start. He was always ready and willing to share in any investigations and in addition to various visits to alleged " haunted houses " (including Brockley Court) we held sittings with Mrs Corner, (neé Florence Cook) and a number of physical mediums. These special researches of ours were full of interest and on one or two occasions produced phenomena which we found it difficult to explain; but in general they proved futile or, at best, inconclusive. Feilding had, however, like myself, been taught by Myers' example and precept that it was worth while to explore any really well attested phenomena on the off chance of finding some grains of gold among the dross.

There ran indeed throughout the whole of Feilding's work in Psychical Research a note of cheerful optimism, so characteristic of his free and forceful personality. Whenever I shared in his vigils in haunted houses, or sat with him to test the alleged phenomena of a materialisation seance, the tedium and generally disappointing results of such experiences were almost forgotten in such company as his. He was always ready to disregard the waste of time and money, to laugh at the exposure of trickery, and to suggest by way of consolation that after all we had gained some further knowledge of mediumistic psychology. Some critics might suggest, perhaps, that this characteristic of Feilding's indicated a rather frivolous approach to a serious subject but I feel on reflection, that such an attitude may really in the case of certain types of mind act as a saving grace. On one occasion in excusing himself from approaching the investigation of Eusapia Palladino's phenomena "in a light—shall I say even flippant spirit," he added, " I sometimes think that in this way alone one can preserve one's mental balance in dealing with this kind of subject."

Nevertheless, under this superficial impression of occasional flippancy, Feilding concealed a very deep and serious enthusiasm. In 1908 he took a leading part in the Palladino experiments at Naples and produced a report in common with Messrs Carrington and Baggally of profound interest and value. He became convinced at some of the Eusapia sittings of " the reality of the phenomena and the existence of some force not generally recognised, which was able to impress itself upon, or create the appearance of matter."

Everard shared to the full that absorbing motive which inspired the life work of Frederic Myers—an ardent desire to secure a scientific proof of man's survival after death. In an admirable paper contributed to the Dublin Review in 1925, he summed up the case as follows : " It is only by the patient accumulation of facts, disregarding sources of error which experience gradually indicates, that ultimately a probability can be built up which in the end becomes so probable as to exclude any other reasonable conclusion." He sat next to me at our Jubilee Dinner and told me how deeply he felt the importance of Mrs Sidgwick's wonderful message.

On one occasion I managed to get Everard to go with me to a remote island in the Lofoden Group which I leased for sport. He shot and fished more, I think, to please me than himself, and on one occasion when some fish had been pilfered from our boat, Feilding joined some farmers at a séance to discover the culprit's name. During this visit I realised once more Feilding's extraordinary gift for languages. Within ten days he had learnt enough Norwegian to talk to the islanders with some facility, and what is more, to understand a good deal of their Lofoden patois. Feilding loved his sojourn in this delectable island, and after our long hours in the open we sat in my wooden house beside the peat fires talking of many things.

ἐμνήσθην δ' ὁσσάκις ἀμφότεροι
ἥλιον ἐν λέσχῃ κατεδύσαμεν

but always returning sooner or later to our great mutual interest, Psychical Research.

When I last saw my old friend, exactly a fortnight before his death, his mind seemed as cheerful and as vigorous as ever. He died amid the love and esteem of all who knew him, without an enemy in the world—I know no better record for any man or woman.

Quis desiderio sit pudor aut modus
Tam cari capitis ?

(16th March, 1936.)

INDIVIDUALITY

By G. N. M. Tyrell

Mr Whately Carington's able work, in carrying the quantitative method of research into the investigation of trance-personalities, raises a very fundamental question, viz., "What do we mean by individuality?" Do word-associations, reaction-times or psycho-galvanic reflexes characterise that in the personality which is essentially the "I"? Or do they characterise those subconscious levels of the self, which in psychical research we call the "Subliminal"? Or do they characterise only the physiological and nervous mechanism of the organism? Or do they characterise a synthetic unit, which comprises all three of these?

In my ignorance of the technique of this method of research as used in normal psychology, I am assuming that they are supposed to characterise the latter; that is to say, what in common language we call the "person." How definitely and consistently does a set of reaction-times characterise such a person? Here, again, I do not know: but the Gatty experiment would seem to indicate that it cannot characterise such a total, synthetic personality with anything like the same definiteness with which the thumb-print characterises the body; for reaction-times can be changed by a mere act of volition.

That is one point. Another is that, assuming survival of death, there must, at death, be some disintegration or at least re-synthesis of the elements of the personal complex. Somewhere this complex must be severed and its surviving elements presumably readjusted. To our bodily senses it appears that the physical part only is sundered from the rest and left behind: but we do not know how much more may be happening, which our senses tell us nothing about. We do not know, for instance, how much of what we call the "Subliminal" or the "Unconscious" may enter into a new combination and survive with the ego: or whether only the individual self or pure ego survives. It may be that much of the self below the threshold of consciousness survives, with its habits, dispositions and complexes, as Spiritualists seem to believe. Or it may be that none of it, or but little, does. On the answers to these questions depends the significance we must attach to reaction-time tests and similar quantitative experiments. I do not mean to say that the validity of Mr Carington's work depends on the answers to these questions. He has merely found out what significant differences in reaction-times, etc. exist; and this we need to know in any case. But, when it comes to *interpreting* these differences, we must be profoundly influenced by the ideas we hold on the nature of personality and individuality. Hence an intensive discussion of this subject seems very desirable; and I should like to see it undertaken by those who are more competent to do it than myself.

What is *Individuality* or the "I-ness" of the self; and on what does it depend? First of all, has our individuality anything to do with the fact that we live in independent bodies, which are spatially separated from one another? Common sense (which has an amazing power of persuading us that a thing is as clear as daylight when it is really as obscure as night) certainly suggests to us that people are individuals, at least partly, because they are spatial units and live in spatially separated bodies. In fact, it goes further, and bestows a kind of pseudo-individuality on inanimate objects themselves, simply because they are spatially separated from one another. A chair, for instance, we usually speak of as being the *possessor* of such and such qualities and characteristics, thereby according it that kind of individuality which constitutes it a *possessing subject*; whereas a block of water ideally outlined in the sea is just "stuff." The sequence of ideas seems to be: (1) We are conscious individuals. (2) We live in separate bodies. (3) We should not be individuals if we were not separated from one another. (4) Chairs, stones, etc. are visibly separated from one another. (5) Therefore they must possess some sort of individuality.

But, why should we regard inorganic objects as separate *subjects* at all? We are surrounded by a continuous, though not homogeneous, environment of matter; and the surfaces of things, which mark them off from one another to the senses, are merely sense-indications of a qualitative difference in the substance of this continuum. Why is the inorganic universe to be regarded as a *many* rather than as a *one*? It is difficult to see any reason, except that of practical convenience, why we should not speak of it as *one* subject-possessor of many qualities? Spatial separateness alone cannot confer any sort of individuality.

But, if that be granted, may we not maintain that the conscious individuality of human beings depends, not on spatial separateness, but on separateness of another kind? May we not say that it depends on a numerical separateness arising out of the fact that the thoughts, mental content and, in general, all the experiences belonging to each individual are private to that individual and inaccessible to any other ; and that this constitutes a conscious being a numerical unit and hence an individual ?

It is at this point that the work of psychical research looks as if it might have something to say on the subject. At first sight, telepathy seems to challenge this alleged privacy of individual experience by making it seem probable that an experience of the agent's is actually *shared* by the percipient. But Dr Broad, who has dealt with this interesting question in his Presidential Address, has shown that it is possible to explain telepathy without having recourse to any breach of what he refers to as the principle of "Unique Ownership of Experiences." There might be a process of Interaction between the agent and the percipient, which induced in the latter an experience *similar* to, but not *identical* with, that of the agent. In this way the percipient would not actually share or directly enjoy the *identical* experience which the agent enjoyed. He would have an experience of his own, similar to the agent's, but yet remaining his unique and private possession ; while the agent's experience would be similarly private to, and uniquely owned by, himself. In the present state of our ignorance of the *modus operandi* of telepathy, we have no way of deciding this point experimentally.

There is a sort of half-way house in the matter, which Dr Broad also explains in his Address. It is possible, in some cases, that what appears at first sight to be the direct prehension by two minds of the same *experience* may be in reality the direct prehension by two minds of the same immediate *object* of experience. But I find it difficult to see how this explanation can be applied to cases in which the telepathic experience of the percipient is that of feeling within himself the whole mood and emotional tone of the agent, such as frequently occurs in practice.

But, when we come to cases of dual or multiple personality (not to mention the problems raised in trance-communications) the question of whether there is or is not a breach of the principle of the Unique Ownership of Experiences presents itself in a more acute form. Suppose, in a case of dual personality, that A is a secondary personality, co-conscious with the primary personality B, but remaining in the background. A gives evidence of knowing B's thoughts and of sharing B's experiences. Can we plausibly explain the relation between A and B on the theory of Interactional Telepathy ? We have here two individualities, and on the theory we are considering, the individuality of each depends on the numerical separateness of its content of experience. There cannot, on this theory, be such a thing as an *experience* which is common to two minds. Well, here again, our ignorance prevents us from dogmatising; all we can say is that it looks uncommonly as if there were a sharing of experience. And what has the interactional theory to say about the *cure* of such a case, when the two individualities seem to become one? Can this happen without involving the process which Dr Broad calls "Intermental Confluence"?

Assuming for the sake of argument that intermental confluence can take place, in what way would it influence our ideas of individuality? Take A and B, two individuals, at first numerically separated from one another by the privacy of their experiences, but afterwards (perhaps in a disembodied state) sharing all their experiences in common: so that each directly shares all the experiences of the other as well as his own. What effect will this abolition of the unique privacy of the experience of each have on the individualities of A and B? Will the fact that all the experience of one is *identified* with all the experience of the other result in the obliteration of either or both as individualities, by removing any criterion for distinguishing A from B? It may be said that there is now no means of identifying *two* centres of consciousness, and therefore that A has been annihilated and only B remains; or else *vice versa*. Or, it may be said, that both A and B have been annihilated and that a third individuality C, enjoying all the experiences of both, has taken their place. In general, this view would seem to indicate that if n individualities share all their experiences in common, this must result in the extinction of at least $n-1$ of them.

These suggestions strike me as being quite unplausible and absurd. If A's experience is enriched by the addition of B's, it seems monstrous to assert that this will have the effect of annihilating A.

Another suggestion which might be made is that, with the pooling of the experiences of a number of individuals, *all* the individualities concerned will disappear, merging into a kind of vaguely conceived "psychic continuum." But, this is surely going from bad to worse. It has the air of capitulation before a difficulty. This is a reversion to a purely mechanical mode of thought. The mind, brought up on the data of its senses, oscillates between the two conceptions of atomic plurality on the one hand and a continuum on the other. But both these ideas seem ultimately to be derived from the experience or conception of space, the idea of atomism being derived from the mutually exclusive aspect of space and the idea of a continuum from its prehensive aspect.

What all this points to, surely, is that we are on the wrong track in assuming that numerical separateness lies at the root of individuality. As a theory it leads to conclusions which do not square with the experience of being an individual. If we suppose that individuality does not depend on numerical separateness, we might define it as just the irreducible *character* of conscious being, without attempting to define the latter further. Why should not consciousness be as irreducible a characteristic of one kind, or phase, of being as extendedness is of another ? Then the sharing of experience by two individualities, originally numerically separate, although it removes the ground for drawing a *numerical* distinction between them, does not impair their qualitative individuality or the " I-ness " of their consciousness. It would seem that we must be wrong in speaking of individuality in the plural ; *and equally wrong in speaking of it in the singular.* Individuality would be something not subject to the category of number. It is true that one cannot form any intellectual conception of such a thing ; but why should one expect to be able to ? Why should we assume that nature will obligingly fit into the conceptual framework which she has provided for us for purposes quite other than the attainment of ultimate truth ? It may be that on these lines we shall best make a beginning in the realisation that the universe is not ultimately pluralistic.

The numerical, as well as spatial, separateness possessed by embodied human beings, with which we are so familiar, would then have to be regarded, not as constituting the basis of individuality, but as having been bestowed on the total personality by its non-conscious and less essential elements.

It is sometimes stated that individuality is dependent on memory —not on a complete and continuous chain of memory—but it is said that, if a person were to lose his memory entirely, he would cease to be the same individual. But here, again, we come up against difficulties. This seems to be another attempt to base individuality on a numerically separate possession. Suppose that an individual A, at a particular moment in time, were to lose all memory of his experiences up to that moment. On the view that individuality is dependent on memory, A would cease to exist as an individual from this moment, and would be replaced by a new individual B, who would inhabit the body formerly occupied by A. B's experiences and memories would start from the moment in time when A's ceased. But now, suppose that, at a later moment, all the memories which A had lost returned to B. (Partial returns of memory of this kind are not uncommon.) B would now become A without ceasing to be B ; and this sounds to me quite absurd. Changes in the self occur during sleep and apparent cessations of the self occur during periods of unconsciousness ; but these do not in any way impair our belief in the identity of our individuality. It is very hard to believe that individuality is dependent on the continuity of memory. It is not even easy to regard individuality as enduring in time or as being dependent on temporal continuity. It may be that temporal endurance and temporal change are less fundamental than the nature of individuality, being partly a mode of appearance, which arises out of a relational nexus into which that very individuality enters as subject. It may be that, when we think we are witnessing the fleeting and unstable nature of conscious individuality, we are in reality witnessing the unstable synthesis of a personality composed of widely differing and all but incompatible elements. It is always through such a personality that the individuality of others manifests itself to us ; and it is always on such a personality that the nature of our own subjective experience depends.

If we are seeking a test for *individuality*, how do we know that such tests as associations and reaction-times are valid for the purpose ? For if these happen to characterise, as their use in ordinary life implies, not the individuality, but the synthetic personality, (what we call in common life the " person ") it seems likely enough that an individuality manifesting in its own body in life would give different reaction-times from those it would give in its natural state after death, (if the test could then be applied) and different again from those it would give if manifesting through a medium.

REVIEW

Through a Stranger's Hands. New Evidence of Survival, compiled by NEA WALKER. Foreword by SIR OLIVER LODGE. Hutchinson, pp. 432. 16s. net.

In a well-known theological work lately published on the future life the authority gives a paragraph to psychical research. He has studied the records, he informs his readers, and they may be assured such are negligible. He makes the usual remark that all the material is on the face of it of such a trivial nature that it may safely be dismissed. Here we are faced with the common obstacle which specialisation has put in the path of science progress—few men in one science know of the latest advance in another. When they would review the whole front, outside their speciality, they fall back on work which has been superseded. This latest compilation of Miss Walker is an advance. Those who read *The Bridge*, whether they were disconcerted or concerted by it, realised here was material which would complicate our view of the universe and consciousness. It is worth noting in passing that sometimes we forget, when we are discounting (as we should) the will to believe, that there is a complementary discount to be made against the will to disbelieve, a will prompted by the strong wish to have a simple world to deal with, to observe the Law of Parsimony. Mechanism is the simplest world picture man has ever had. Hence a longing to preserve it. Against *The Bridge* could then be brought (with relief by the simplists : with reservation of judgment by the rest) the fact that telepathic leakage was certainly possible. No psychical researcher needs to be told that telepathy can never be ruled out as a possible explanation of all para-normal knowledge. The point at issue is at what point does the telepathy become so complicated that (this time on the other side) the Law of Parsimony turns up and Occam's razor begins to cut in the opposite direction. Miss Walker's technique was simple but effective in complicating the telepathic hypothesis. She received about a specific dead person, personally unknown to her, only such few distinctive facts as would permit her to recognise it was about them the medium was speaking, should the medium refer to them at all.

These facts she wrote out in letter form, read out when she was by herself, then posted the letter so read out, to Mrs Sidgwick to file as evidence. After that she travelled to have a sitting with Mrs Leonard, who of course knew nothing of the particular reason for this sitting. Then, like a good naturalist, Miss Walker sat waiting for any reply. The replies are here. Quite apart from the fact that this is a new technique which makes the telepathic hypothesis much more complicated and also confirms the bona fides of the whole procedure, the material itself is probably as remarkable as any that has been available to public inspection. The three sittings at which information purported to come from a doctor who committed suicide: the sittings at which a child killed in the burning of the Georges Philippar is claimed to be communicating: such records, quite apart from the stringent conditions under which they were received, make it quite impossible for such records to be dismissed, because on the face of it they are trivial and lacking in character. What conclusions then are possible?

The first will, for most researchers, be telepathic. It will, however, have to be owned that the old and popular idea of telepathy—one mind transmitting directly to another—is shown by these records to be as mistaken as the President's address demonstrated it to be. When there is given evidence which is neither known to sitter nor medium and those possessed of it do not know the sitting is taking place and may be far distant, the telepathic hypothesis becomes richly complex. When to that richness is added the consistent character of the communicator, often rejecting suggestions which are "out of character" and showing peculiarities of personality, we have, instead of a simple skein of memories somehow hanging together and so hauled up by medium and sitter, something much more exactly to be described as an individual communicator. That naturally persuades many investigators to adopt the second possible conclusion "conventional survival". On this side, however, we must put the confusions, vaguenesses and inaccuracies. They exist as well as the evidential material. They may be due to the medium's mind confusing transmission, on the other hand they may be due to no clear personality being present. A third provisional conclusion is then possible.

It grows increasingly probable that if consciousness survives the body it must do so under conditions largely incommunicable to us still incarnate. Further, when a mind which is so surviving would make contact with us it would have to "recondense", recondition itself. Existing say in some state analagous to the diffused state water can take on when it becomes steam, it would have to turn itself back into the definiteness of a hailstone. Such an effort would be difficult and the "frozen" state precarious for a droplet which lacked a normal condensation-point or nucleus. Certainly, reading these records, there is the feeling of a focus sharpening and fading as though it could only be retained for a few moments with any distinctness.

All conclusions, however, are obviously premature. Two things can be said : one is that Psychical Research is advancing, is enlarging our conception of what consciousness is : the other is that no one should venture to talk about survival, pro or con, till this book has been studied. Apart from all else Mr Richmond's sparse classificatory comments give these collections a peculiar value.

GERALD HEARD.

MRS HENRY SIDGWICK

AN Obituary of Mrs Henry Sidgwick by Miss Alice Johnson is in preparation and will be published shortly in *Proceedings*.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 145

PRELIMINARY STUDIES OF THE RECORDED LEONARD MATERIAL

By Kenneth Richmond

I

Notes on the Psychological Formation of Leonard Communicators

In approaching the problem of communicator-personality as it appears in Mrs Leonard's recorded trance-material, I have adopted certain fluid hypotheses which should be put down to start with. First, I adopt, provisionally, a view which is supported in part by Mr Whately Carington's quantitative investigations, that we are dealing with two chief psychological mechanisms: one an organised and habitual secondary personality, which is usually (I do not say always) the vehicle for the Feda control; and the other a dramatising function of the primary trance-mind, which adapts itself to become the vehicle for the different communicators. In saying "vehicle", I free myself from any suspicion of thinking that "secondary personality", or "dramatic pose" (to adopt Mr Carington's useful phrase), can explain or characterise the impulses that operate through these mechanisms.[1]

But these concepts can suggest an explanation, at once, for a curiously dual and fluctuating technique of the trance-mind, through which the impulse of communication for the most part operates. Sometimes Feda seems to be speaking, sometimes the communicator, sometimes you cannot be grammatically sure which, though in the context the question is not often in doubt. But Feda is regularly found speaking on behalf of the communicator. Why? Simply because the habitually organised mechanism works the

[1] "For every mental event the question must be raised as to the origin of the effective energies." K. Lewin, *Psychol. Forsch.* 7, p. 313.

more easily? I think that this is one reason, but that there is another. A secondary personality appears—and the appearance is confirmed by Mr Carington's results—to have paths of association open for use which in the primary personality are impeded by systems of resistance and inhibition. (Mrs Leonard in the normal waking state obviously would not run on as Feda does, and the Feda mind seems in many ways to run along different tracks.) The impulse of communication, wherever it comes from, thus has two alternative systems of free association through which it can operate, and the systems are "countersimilar", to borrow Mr Carington's term: one is complementary to the other. An idea or phrase that will not come to expression through one system, may be switched over to the other set of tracks and so reach its destination. I am taking it as a working hypothesis that this continually happens in Leonard material. This is not to reject the possibility that a fugitive and scrappy form of utterance may be part of a defence-mechanism. It does, in poor sittings, provide a way of slipping out of difficulties. But equally, in good sittings, it seems to provide a way of slipping into a train of association that leads to an evidential remark.

When a communicator appears to assume "direct control", the tendency of the thought-stream to be switched to and fro largely disappears. The utterance may be scrappy in many instances, but the scraps hang together in natural sequence. Impressions of character and specific personality, which are recognised by the sitter or subsequent verifier, become more frequent; but on the other hand, when evidential detail is attempted, "forgetting" and other mental blank patches shown by the communicator become more frequent also. I am not yet sure how this situation is affected as the "direct control" improves in apparent quality, when it becomes more strongly organised with time and practice; but it seems to me that the improvement consists more in steering round the limitations of the vehicle than in removing them. The ease with which evidence of paranormal faculty is produced seems to be definitely and permanently decreased in the case of "direct control", as though flexibility in switching on to the necessary associations of idea and phrase had partly disappeared. The hypothesis seems to be of use that in this case one-half only of a two-way system of association is operating, the "dramatisation" half, equipped with those association-tracks only which are open to the primary trance-personality, while the secondary trance-personality, the vehicle for Feda, is in abeyance.

This theory of a two-way system through which the impulse of communication can work may be of psychological interest, if further examination of facts does not show it to be fallaciously simple. It is a matter of common experience in psychology that definite boundaries between this and that process turn out to mark convenient divisions in the psychologist's system of ideas, rather than distinct areas in his field of observation. But Mr Carington's valuable and objective demonstration of countersimilarity between the Feda-vehicle (if he will let me use this phrase) and the ordinary system of association-tracks in the Leonard mind gives us the clear fact that the machinery exists. My conjecture about the way it works may or may not turn into a useful instrument for qualitative analysis. An interesting question for further study will be whether the striking ability for precise clairvoyant description of objects, which is characteristic of Feda at her best, seems to be expressed through the "Feda-vehicle" alone. It will complicate the theory if Feda seems at times to use both vehicles, as is quite probable. Psychological formations have a way of straying outside the diagrams marked out for them on paper. And it is difficult to say when Feda is really to be taken as operating alone. In most good cases of the clairvoyance of objects, a communicator-personality is said to be co-operating closely in the process of getting the description through.[1]

So far I have touched on hypotheses about the mental mechanisms through which expression is given to the impulse to communicate. I am not specially concerned with the impulse to convey information of paranormal origin. It is my business, in this paper, to give maximum weight to hypotheses of extra-sensory perception, where there is evidence for these, in order to be as clear as possible on the point whether anything beyond extra-sensory perception needs to be assumed. But we shall be neglecting evidence if we fail to take notice of one curious fact about the impulse to transmit extra-sensory perceptions. When these phenomena are being investigated for their own sake, and isolated from what I will call the communicator-impulse, the range of success both in quantity and quality seems to be definitely limited, in comparison with the successes observed when communicator-impulse is present. The history of experimental telepathy had produced a strong impression that this limitation existed, and recent quantitative work such as Professor Rhine's—confirmed by that of Mr Tyrrell with a more discriminating technique but a similar method of assessment—shows a peculiar

[1] *e.g.* the incident of the Adeline Genée statuette, *Proc.*, vol. xxxi, Part 81, p. 386 *et seq.* Good examples of clairvoyance by Feda when apparently operating alone seem to be all in unpublished material.

tendency of successes to hover about an average of 10% above chance. Why there should be a threshold at about 30%, when chance-expectation is 20%, does not appear; but the nature of the limitation may be provisionally explained (with some backing from the behaviour of Mr Carington's figures) as a difficulty of the mind in admitting extra-sensory perceptions into what might well be a confusing competition with normal perceptions. It is a possible conjecture that 10% is about the proportion of elementary "intuitional" judgments which the mind is organised to admit into its everyday workings. (I refer to the quite ordinary and unspecialised "intuitional" acts of the mind which seem, as a grouping, to shade off into instinctive acts and are often roughly described as instinctive.) E.S.P. experiments with young children and people from primitive races might show interesting variations.

In the study of phenomena where communicator-impulse is present, the picture appears to change completely. Numerical assessment is hitherto lacking of extra-sensory successes that occur in the course of communications; but there seems to be no question that in striking and detailed accuracy, and in the number of correct results that can be produced in one context and at one time, the paranormal material presented as evidence for communication is in a much higher class than that elicited by E.S.P. experiments. This fact suggests that there is a special "virtue" in the communicator-impulse. It need not by itself suggest that communicators are real persons. It may suggest that extra-sensory perceptions which the mind finds it difficult to admit on the same terms as normal perceptions, may gain admission very much more readily as elements in an acceptable myth. It is another question whether it is probable that a myth without basis in reality would obtain regular and consistent support from the intuitional functions of the mind. This is not found to be the case in the ordinary course of psychological research, though psychologists differ widely in the kind of reality that they discern behind the operations of imagery and symbolism.

This brings me to the difficult question, difficult, that is, to discuss in small compass, of a hypothesis about the nature of the communicator-impulse. This, of course, involves a hypothesis about the nature of "the communicator", but I should like to emphasise the importance of a step which we are apt to leave out in the discussion, after we have considered the mental mechanisms through which mediumistic utterance may work. An understanding of machinery, even when it is complete, may leave a good deal unexplained. An understanding of the machinery of a car may explain why it goes, but not why it goes to Tavistock Square. The hypothesis that there is a human driver, so highly probable in the case of the car, may supply a misleading analogy if we transfer it to all mechanisms that work in an observed direction. But a hypothesis is necessary to account for the direction as well as the mechanism—and in the case of communications, for the following of a complicated choice of paths by a process that looks very like steering. The impulse to follow a given track must be analysed, or we are in danger of jumping to one of two conclusions—either that there "must" be a spirit working the mechanism, or that the mechanism is working itself.

What is it necessary to assume about the impulse that produces communications? First, that if it proceeds from the trance-mind alone, that mind has powers not only of obtaining, selecting and arranging paranormal information, but of effective dramatisation without a model (as would be specially the case in proxy sittings); and for the existence of these powers we have nothing that can be called evidence. Second, that if it proceeds from other incarnate minds, these minds, to produce the observed phenomena, must be able to collaborate telepathically to form an integrated system of memories and characteristics, not only conveying this as a whole to the medium, but conveying, also by telepathy, a characteristic personal drive and manner of self-expression. Again, there is insufficient evidence for paranormal powers of this order. Third, that if a generalised concept such as "telepathy", or a reservoir of memories, is thought of as an effective cause, we have to endow these things with dynamic functions so much resembling personality that it becomes useless to think of them as impersonal. (I have not, so far, found it very useful to entertain the idea that more or less disintegrated wisps of memory and motive can somehow persist *in vacuo*, and influence phenomena.)

The types of assumption, in fact, which follow upon the three kinds of hypothesis which I have suggested, appear to be too wide and carefree for scientific use. I am not throwing the hypotheses aside as falsities, but trying to put them in their place as myths that may or may not prove to have some truth behind them. I have a good deal of respect for myths, and indeed, I propose to adopt one as containing the working hypothesis that seems likely to work best. This is the myth provided by mediumistic utterance itself, that the communicator-impulse proceeds from disembodied spirits.

It is a deliberate method of approach, not an ingenious but unnecessary complication, to treat the hypothesis as something that has to be disentangled from a myth. The real complication becomes visible if we accept, as a working hypothesis, the bulk of what we are told at sittings about communicators, and then see what assumptions we are obliged to make. Broadly, these are a body of spurious assumptions, many of them only half-conscious and so outside rational control, which centre in the suggested belief that the communicator of the seance-room " is " the deceased person who is being represented. If we substitute " is partly " for " is ", we are making assumptions about the nature and behaviour of personality-elements which are very likely to be misleading. It is here that we can keep closer to justifiable assumptions by thinking in terms of communicator-impulse rather than of communicator-personality. I think we can argue, with a minimum of fantasy, the existence of an impulse to dramatise on the part of the medium (a natural and respectable impulse when the mediumship is of a high grade), combined in greater or less degree with an impulse to co-operate with and assist the dramatisation on the part of a discarnate person. Essentially, we are in touch, at a sitting, with a subliminal actor in the medium (there is plenty of evidence to be gathered from hypnotic experiments for the existence of a subliminal actor in the human mind), who in turn is in more or less close telepathic touch with the original of the role. But we are not dealing, at any rate in the case of a reliable medium, with an actor depending on a memorised part ; the actor is extemporising, and we are assuming a telepathic collaborator to account for the extemporisation effectively reproducing a verifiable character unknown to the medium. A possible faint analogue to the process is the power some musicians have of extemporising duets together, without previously agreeing upon a theme. The subliminal actor, apparently, " gags " when telepathic impulse fails to assist the extemporising impulse, and probably is not at all sure which is which. This is certainly the case with automatists who obtain script with fairly clear consciousness of what comes to expression.

We may note that the sitter, on this hypothesis, is behaving with complete scientific correctness in also collaborating in the dramatic process, whether telepathically, which cannot be helped if and when it happens, or by treating the dramatised personality as entirely what it professes to be. A good dramatic critic is not a person who refuses to admit full dramatic illusion into his mind, though he has become, by practice, fully aware at the same time of dramatic technique.

The point of this hypothesis about the communicator-impulse—that it is a specialised subliminal form of the dramatic impulse common to humanity, combined with more or less of a telepathic impulse from a discarnate person—is that it imports from what I am respectfully calling the communicator-myth the minimum assumption that I think will account for the observed phenomena. It leaves untouched the deeper problems of personality-structure which are dealt with in Lord Balfour's impressive paper on the Willett mediumship.[1] But scientifically, to accept the idea of discarnate influence is a very large assumption of the unknown, based on an assertion which is part of the phenomena, and on a human intuition which is suspect of representing a desire rather than a perception. However, with the best will in the world I cannot relate the observed impulse and its outcome in evidential statement to any known factor that provides for the co-ordination of the impulse. There is no reason to suppose that extra-sensory impacts are so thoughtful as to select and integrate themselves into an illusory but effective representation of a communicator: I cannot imagine them selected and integrated, without a model, by the undirected trance-mind of the medium; while the hypothesis of telepathy from the living as a sufficient cause of communications requires paranormal abilities, added to a genius for telepathic conspiracy, with which we have as yet no excuse for crediting ourselves.

Turning from hypotheses to what may safely be described as psychological facts, I think it is of some importance to consider the fact of organisation in a communicator-personality. The term organisation is used in psychology to indicate the way in which a set of dispositions becomes integrated. A control-personality becomes pretty strongly organised; a communicator-personality, as a rule, less so. Feda is able to withstand the quite considerable psychological impacts entailed by a word-association test without noticeable change of personality (whether it is the whole of "Feda", including the paranormal functions, that has been tested, or only a comparatively mechanical "Feda-vehicle", we cannot know). The communicator whom Mr Drayton Thomas allows to be known, for brevity, as John, and other communicators whose "vehicles" (in my hypothesis, dramatic) have had years in which to become firmly organised, are similarly able—and, fortunately, for us, willing—to undergo word-association tests, with the results that Mr Carington has investigated and described.[2] Again, it is difficult to guess whether the fugitive elements of paranormal faculty, and by hypothesis of discarnate influence, may be present in the psychological formation at the time when it is being tested. (We are not sure, in any case, whether the phrase "at the time" has any particular meaning in this context.) We may be observing

[1] *Proc. S.P.R.*, vol. xliii, p. 41. [2] *Proc. S.P.R.*, vol. xliii, p. 319.

the application of the tests to an organised "vehicle" which reacts in accordance with the habits of association previously impressed upon it. The results are not the less interesting and significant if this is the case, but the possibility may affect the expectations that we form of results from other and less habitually organised communicator-formations.

For an example, the Compton communicator in one of Miss Nea Walker's proxy cases with Mrs Leonard [1] appears to have particularly good organisation, beginning from the first sitting; and the character is that of a medical man, scientifically interested in the process of communication. It would be interesting to discover whether this personality, after the quieting-down of certain emotional elements, would and could respond consistently to word-association tests; but I think it would be in no way surprising if the psychological formation proved unable to hold together under the conditions. And in general, I think we are apt to leave out of consideration the probable strength, or depth, of organisation, in many forms of psychical research, when we propose either to vary the conditions of experiment, or to apply to a new case conditions which have proved satisfactory with an old-established one. Considerations of this kind may also suggest a reason for Mrs Leonard's recent practice of giving sittings to none but old-established sitters. This suggestion leads on to the point that organisation, whether of a paranormal kind or not, must obviously subsist between medium and sitter—a fact of which the implications are often much better recognised by the investigator who relies on common sense than by the enthusiast for some particular theory. It is very much more difficult in psychical than in psychological research to vary the conditions of experiment with any approach to an idea of what changes are in fact being made in the organisations involved.

The facts of organisation may account for one curious phenomenon in communicator-psychology: that it is new communicators (such as "Dr Compton") and old established communicators (such as "John") who seem to show the most effective organisation, though they may differ in stability. In between the phases of novelty and of further development there seems often to be a flat period. This may be analogous to the inspiration of the young artist, comparatively unstable and easily upset, which appears to go underground when the technique of the art is first studied systematically, but reappears—if it survives—in maturer, more

[1] *Through a Stranger's Hands* (Hutchinson). The Compton Case. This is an exception to strict proxy technique, but the fact is not of importance here.

stable and more readily accessible form after the technique has been mastered.

In suggesting that we have a long way to go before we begin to understand the Protean forms of organisation that occur in such complex psychological events as the Leonard phenomena, I am not suggesting that it is useless to try to control the conditions of experiment. The development and success of proxy technique show that this is not the case. But while it is most desirable to bring psychical experiments into a position like that of physical experiments, where they can be repeated or varied at will, we have to recognise a wide difference between repeating the apparent conditions of a psychic phenomenon and reproducing the actual and probably obscure organisation which enabled it to happen. When the psychic phenomenon to be tested is reduced to the simplest type, such as extra-sensory awareness of an electric bulb being lit in one of five closed boxes (in Mr Tyrrell's recent experiments), the organisation involved is found to be none too simple or easy to understand; when we come to the very complicated structure of the Leonard organisations it is most difficult to experiment with any knowledge of what we are about. What is useless is to form judgments of this type: "If he (a given communicator) can do this, he should be able to do that". "He" is likely to involve an assumption that the communicator "is" the deceased person, not a complex representation; and "should" assumes that we know how the representation can and cannot operate, when in fact we know very little about it. The best method of varying the conditions of experiment seems to be to observe what variations occur spontaneously, and to select those which seem to be most fruitful and to conform most exactly to test conditions. A series of such selections can, in practice, result in a very large variation from previous conditions; but the process is gradual, and may arouse impatience in those who want immediately to try out some new experiment, which may have every chance of upsetting the organisation involved.

There is involution as well as evolution of organised psychic trends, producing stereotyped modes of thought and action. These can be useful, as in the usefulness of habit and routine, and in the establishment of junctions, as it were, in the mind, which form habitual points of reference for systems of ideas; or vicious, as in bad habits, thinking by formula, and fixations of ideas (popularly called "complexes", though often mistakenly). I approached the Leonard records with a strong impression that they would show a tendency to the use of stock formulae, on the hit-or-miss principle: not in any reprehensible way, but from a tendency which I thought would be natural in a professional medium, and thought I had observed in my own sittings in the past, to fall into a habit of using the tracks that usually lead somewhere with the average sitter, and the words and phrases that oftenest produce a response. When I came to an examination of Miss Nea Walker's records of proxy sittings, with her admirable groupings, under individual heads, of points which have special critical interest, I was not surprised to see that one of the headings she had employed was "*Clichés*". But I was surprised to see that this heading had perforce been abandoned, after points had been extracted from a number of sittings, because the pages on which the heading had been typed contained hardly anything. Organised routes for leading up to a favourite type of subject are certainly present, such as one can observe in the conversational habits of one's friends (these are easier to observe than one's own habits); but I find on careful examination that these stock openings have a remarkable way of leading each to a different track of association which is appropriate to the given communicator. I have tried to interpret this as a process in which the motivation arises in the trance-mind alone, and the deflection towards evidential fact is due to telepathic impacts from the living; but the difficulty of accounting for selection among such impacts is very great, especially at a proxy sitting, unless we assume that only selected impacts arrive, which lands us again in assuming the transmission of impulse and organisation by some fiction-factory in the minds of bereaved people at a distance.

To take an instance which occurs in Miss Nea Walker's published material, as well as in several unpublished records, there is a curious Leonard-tendency to make use of the word "mount", apparently as a feeler towards place-names containing it. This occurs several times in *Through a Stranger's Hands* and elsewhere, and is the nearest thing to a true *cliché* that I have so far observed in the Leonard material. It is a likely conjecture that almost any annotator could associate something more or less relevant with the word "mount". These allusions I should class as evidentially worthless in themselves, and I rather dislike giving any weight to them at all; but when they are dropped into contexts which appear to be far from fortuitous, the element of "fishing" seems to involve a fairly good idea where the individual trout is. At the end of *Through a Stranger's Hands*, on the last page of the van Tricht Case, references can be found to an episode in which "*the* -mount" has been associated with the name *Diemont*, not because this very fortuitous-sounding association struck Dr van Tricht as in itself compelling, but because it was so placed as to fit a given context like a piece in a jigsaw puzzle. (I am not here assuming any characteristic in pieces of jigsaw puzzles except that they fit, and make part of a pattern.)

Corresponding to certain possible *cliché* words, there are what may be regarded as *cliché* ideas or suggestions. These, again, are few: the most frequent one that I have observed will probably be recognised by a number of Leonard sitters. This is a suggestion that a visit has been paid, or has to be paid, to some vaguely indicated locality which has sad or painful associations. The conjecture is pretty obvious that this could be described as a visit-to-the-grave formula, which has become organised because it is a very likely shot. The formula, however, has often led on into association-tracks which are individually appropriate. In one proxy case (unpublished: records are in the S.P.R. rooms) details concerning a grave were given which happened to be known to Miss Walker through correspondence following a previous sitting; and on the following day these were apparently supplemented by further allusions, recognised only by the annotator in America, in the spontaneous script of an automatist who, as I can personally bear witness, knew nothing of the case, but was instructed in her script to send the script to Miss Walker for verification. The implication that effective communicator-impulse was at work does not seem to be affected by the fact that a customary formula had first introduced the subject, which was then elaborated at the Leonard sitting and elsewhere. And it has to be remembered that the subject itself is a common feature in cases of bereavement.

I have thought it of use to pursue a rather dull investigation, of which the foregoing are surface samples, into this question of organised trends in the trance-mind, because it is of fundamental importance to make sure that we are not mistaking trends of the process, in trance-communication, for evidence of intention on the part of communicators. Isolated examples of evidence for intention can be very strong (as, for example, in " The Problem of the Pipes ", an episode in Mr Drayton Thomas's proxy case of Bobby Newlove, *Proc.*, Vol. XLIII, Part 143, p. 481), but I am speaking more of the general evidence of intention that is to be inferred from veridical communications as a whole. Given a wish on the part of the medium to produce evidence of survival, and long experience in trance, with a multitude of sitters, of the lines of suggestion which are most likely to produce vivid personal associations, it is very possible for systems of safe guesses to be automatically organised, which become endowed with a great appearance of authenticity and individual quality when they are enriched by striking annotations. The intentions manifested at a sitting might be types of intention in the trance-mind alone which have been found to be readily supplemented by the associations of sitters and annotators. I think this machinery certainly exists, though much less pervasively than I imagined when I started the investigation; but I think the most interesting thing about it is the regularity with which it defeats its apparent object. In the best sittings, it is the allusions which the communicator-impulse appears to have forced away from the expected rut, that arrive at something specific in the mind of the annotator. But it is advisable to plough through a number of the worst sittings, where "stock" suggestions and feelers are chiefly to be found, in order to see how completely the most willing annotator is left groping for any trace of individual significance in what is said. I have tried deliberately to distort some of these passages into any semblance of an evidential meaning for myself, not at a glance but carefully putting down on paper everything favourable that I could think of, and I found it hard and unproductive work. Now and then an accidental association seemed to be working out brilliantly, with a little spurious encouragement, but it would regularly be put out of court by something quite incompatible in the context.

I have found in fact that the search for evidence of effective intention—effective, that is, to produce the observed results—in the organisations that seem to occur in the mind of the medium alone, is a search that appears to lead away from its objective. I think that any attentive reading of Leonard material must lead to a feeling that the element of intention in the trance-mind itself is a vaguely groping thing, continually deflected into this or that distinctive channel by intention of quite a different type; but it seemed worth while to test this feeling as thoroughly as possible for illusions due to the coincidence of mediumistic gropings, having only a vaguely organised intention, with potent associations contributed by the sitter or annotator, the whole being made the more impressive by evidence of paranormal acquaintance with facts. The point is an elementary one, and there are specific episodes which it fails to cover, but it might account for a good deal of apparent communicator-impulse. In my opinion the explanation appears to be specious, in the great majority of cases, on any close examination of the way the current of thought and speech actually flows. The vague forms of organised intention that seem attributable to the trance-mind alone, with its past experience, appear as being quite distinctly manipulated, deflected and sometimes negatived by another form of intention. This interplay and partial opposition of intentions can of course be dramatised, and on my present hypothesis are dramatised ; but, once again, extempore dramatic impulse does not seem to account for the observed content of communicator-impulse, and we have seen the difficulties of attributing a function of guidance either to "telepathy" as an abstraction or to the incarnate minds from whose unconscious collaboration it might be assumed to proceed.

One of Mrs Leonard's phenomena appears to represent communicator-impulse in a particularly concentrated form. This is the whispered, or *sotto-voce* remark, sometimes preceding a more elaborated version, out loud, of the statement (often a less evidential version), and sometimes interjected into, and interrupting or correcting, the more usual flow of speech. It is necessary to have heard these *sotto-voce* remarks to appreciate the curious impression of rapid, fugitive certainty of touch that they convey : but although I am speaking of something that cannot be put on literal record for examination, I had better note that I find it difficult to classify this, and certain other impressions of utterance in specially close touch with a motivation apart from the medium, as superlatively clever dramatic effects arranged by the subliminal actress. Not that unconscious motivation cannot produce brilliant dramatic effects—some of the hysterias have staged astonishingly convinced and convincing dramatic work ; the point is one of quality, and of acquired psychological instinct on the part of the observer for which I claim nothing except that it exists, and asserts rightly or wrongly that these particular Leonard phenomena are not much like dramatic constructs. I am prepared to find my dramatisation-hypothesis wearing pretty thin in certain important places, though I think dramatisation may still best describe the medium's unconscious contribution to the blend that usually occurs.

The moments and periods in a sitting which especially impress the hearer's mind with the feeling that something outside ordinary experience is happening, include manifestations of character and personality which are extremely difficult to deal with scientifically. It is thoroughly bad psychology to leave out of consideration things that produce strong emotional or intuitional impressions upon the observer, whatever hypotheses they appear to support ; and it is thoroughly bad method to give weight to these impressions (whether as evidence of survival or as evidence of credulity) without any means of knowing what kind of weight it is. To revert to the Compton Case, in *Through a Stranger's Hands*, the general reader, having no previous acquaintance with Leonard phenomena, receives a certain distinct feeling of impressiveness about the personality of the Compton communicator. I, having had Leonard sittings and investigated a good deal of Leonard material, receive a heightened impression through knowing the type of intuitional atmosphere, and being aware also that this personality is not only impressive but distinctive among a great number of other impressive communicators. Miss Nea Walker, being present at the sitting, and in this case having known Dr Compton slightly, receives a more direct impression, which can be gathered from her notes on the case, though she has correctly kept it in the background. The annotators, Dr "Davison" and a brother-in-law, receive a further impression which, though derived from cold typescript and unfortified by experience of Leonard sittings, is heightened in another direction by a fairly intimate knowledge of Compton characteristics.

Now, I find myself possessed of the opinion that if the Compton-Nea Walker-Leonard sittings could by some means be reproduced exactly as they occurred, with another person as auditor who both had long and critical experience of Leonard sittings, and also had known Dr Compton in his lifetime exceedingly well, that person would receive a still more definite and vivid impression that the personality of Dr Compton was in some way a participant in the being of the Compton communicator. I am not advancing this opinion as an argument, but trying to account for its existence as a psychological fact. First, it is based on a certain amount of reasonable analogy. On other occasions communicators have made their appearance, and the evidence for their authenticity has risen in impressiveness and evidential value, the more it has been weighed by people having the requisite personal knowledge and critical experience. Second, I find that I am undoubtedly influenced by this consideration: that the Compton communicator has not only "come to life" in my own mind, as a being of whom independent and characteristic reactions are to be expected (this could be said of any really well-drawn character in fiction), but that this character appears to be recognised in three different ways by three different people who knew the original. If I were a judge who had to decide a case at law, in which A was accusing B, a novelist, of having borrowed A's personality to make a fictional character, I should (I hope) study the character in the book carefully to gain an impression of the personality that was portrayed: and I should then be influenced in opinion—and so would the jury, however I directed them in law—by testimony from A's friends that they had recognised the portrait at sight, apart from facts in A's life which the novel might have reproduced more or less as they occurred. The influence of evidence which was quite unweighable objectively, except in terms of the number of witnesses saying similar things, might be regrettable in strict law ; but a moment could in fact arrive where I should know pretty positively that further witnesses would only increase the impression that the character in B's book was really based on A.

The point is not that such a moment ought to arrive in judging the credibility of a communicator, but that it does arrive, and by a process that cannot really be reduced to logical terms. It is possible to have logical evidence of character, as well as evidence of intention, by amassing details of mental and verbal behaviour, and characteristic operations of memory, which are recognised as closely resembling those of an actual person. These can be put into order on something like an evidential basis. It seems possible that Mr Carington's treatment of word-association material may establish characteristics belonging to a given communicator and not belonging to either of two independent mediums who present that communicator. This would at least demonstrate the probability of psychological " traces " having a common origin outside either medium's mind, and would be in the same field of inquiry as evidence of character. But I think it is necessary to face the fact that objective and more or less calculable evidence, in this field, cannot help being subordinate in our minds to opinion founded on personal and subjective impressions—or the lack of them. It is a truism that opinion is much less likely to bias our view of evidence if we admit and allow for it, rather than pretend that we are incapable of bias. And I have tried to suggest how this kind of opinion, based on mental processes more appropriate to the law-court than to the laboratory, can be formed on an entirely respectable basis.

This factor of opinion cannot help being of very considerable importance when we come to the study of a long-established communicator-personality such as that of " John ". In this case the organisation of communicator-impulse has had the advantage of Mr Drayton Thomas's firmly consolidated opinion, the growth of nearly twenty years, that the operative personality is that of his father, John Drayton Thomas. It would be an error of false analogy from physical science to regard this as a factor to be discounted. In dealing with insensible things and forces, the experimenter's belief in his working hypothesis does not affect his results except as it affects his own behaviour. In psychological and psychical research, the experimenter's belief is part of the active organisation that occurs. Its absence may constitute a negative suggestion which acts upon phenomena like a low temperature upon a chemical reaction. What I may call the equable temperature of Mr Drayton

Thomas's opinion is a condition in which given results occur. It is correct to examine the results with the coolest scientific scepticism, but not to complain that they occurred in whatever conditions were psychologically necessary. It may be a difficulty of the sceptic—I have traced this in myself—that he unconsciously resents phenomena that decline to occur in his own presence.

On the other hand, it is advisable to ask at the outset what results can be attributed to Mr Drayton Thomas's state of opinion, which entails a state of expectation. Expectation is always a difficult subject to handle, because the naïve assumption that "this could not have come from my own mind, as I was not thinking of it, and it surprised me", is often misleading. Experiment in telepathy suggests that the things not consciously thought of may be the easiest to convey. It becomes necessary to allow for a large, and even for an apparently improbable, field of expectation in the mind of a sitter awaiting communications from a known personality.

A striking and consistent characteristic of John is a tendency to relate paranormal evidence of many kinds to some definite association with his own typical activities in the past, or to family associations with given persons or places. To take a random instance from published material,[1] the first group of newspaper tests attempted by this communicator included a mention of soap as among the things to be found on a forthcoming page of *The Times*. This was marked as a failure, until at the next sitting, "some *name* suggesting soap" (italics mine) was suggested for the allusion. The name was clearly identified in the part of the page that had been indicated before the issue of *The Times* was out, but so far (in this instance) the emendation could be put down to normal observation by the medium after the paper was available. However, there were definite associations, dating from twenty years before, between the soap-maker's family and the Drayton Thomas family, and there had been an engagement between members of the two families, so that a piece of paranormal evidence (in this instance, containing an evidential flaw, if one chooses to think the "soap" clue a chance shot) was supported by the aptness of the given allusion to the communicator's family interests.

In this and very many similar instances, in the course of the Drayton Thomas sittings, is it arguable that Mr Drayton Thomas's latent memories could have supplied what must be called an active but unconscious expectation that these characteristic allusions would be made? There are certain cases where there appears not to have been any latent memory, but when the sitter possesses

[1] *Some New Evidence for Human Survival* (Collins, 1922), p. 216 *et seq.*

the requisite memories the evidence for telepathy between the living certainly suggests that a number of associations suitable for the communicator could have filtered through in some form, though not so large a number as are found in the records, nor in so purposive a form. It is the assumption of purposive and constructive action, either in Mr Drayton Thomas's unconscious mind contrary to his conscious intention (the mind does not work well in states of conflict), or applied by the trance-mind of Mrs Leonard to his latent memories, for which I can find no evidential justification whatever. Chance would be a very much more credible explanation for such instances of the connection of these elements of memory with paranormal data, if there were not so many of them. It seems to me extremely difficult to explain the characteristic behaviour of the "John" mentality, as the producer of a typical range of evidence through a typical mode of purposive activity, on any basis that identifies this mentality with a part of Mr Drayton Thomas's mind. The amount of speculative assumption that would have to be admitted only grows larger, the more closely such a hypothesis is followed up through the detail of the records.

It seems to be a matter of commonsense to keep evidence of character upon a dynamic as well as a factual basis. It is not difficult to be misled by a striking chance resemblance *at a given moment*, or at a small number of separate given moments, between what is presented in a communication and what would be characteristic of a particular person. What is more impressive in the long run, though it may provide less high-lights, is the persistent combination of evidence of intention with evidence of character. In reading a considerable number of records from the same communicator, day after day, I have noticed that the mind tends to become insensitive to the element of characteristic intention—one begins to take it for granted—until some new episode arouses the mental comment, "How like him!" and the critical sense immediately chimes in with an accusation of begging the question. If I were asked to say what, precisely, gives me the impression of characteristic intention throughout the "John" records that I have so far studied, I should find it difficult to give a more pertinent reply than that the whole type of intention appears to change as soon as I turn to the records of another communicator. I merely make note of the fact, that the impression of characteristic intention does increase with prolonged study of the material, and does develop a continuity which does not seem to be my own contribution.

Indeed, continuity in maintaining the hypothesis of discarnate communication, and in giving it consistent fair play, is under a handicap while one is continually and critically questioning every individual element that can support the hypothesis. " Can I think of this as not being in character ? " " Could that be regarded as accident and not intention, or as someone else's intention ? " Such a process of cross-questioning is extremely destructive, and I have been surprised at its not having a disintegrating effect upon the material as an evidential whole. I have been especially interested in the " pipes " episode already referred to (p. 27 of this paper), because it showed evidence of intention—not to say pertinacity—which could be carefully examined and then taken for granted, as a single aim at one particular objective, persisting through several sittings and a considerable period of time. The case, as Mr. Drayton Thomas has himself presented it in *Proceedings*, can speak for itself; what I am concerned with here is the effect of having this one factor of intention stabilised, as it were, in the mind while one is reviewing the tract of material in which it is concerned. The faculty of critical attention is very much more free for giving balanced consideration to other evidential factors ; and the study of this material has led me to realise to what an extent the mind is operating a complicated sliding-scale process, with every value shifting in unknown ratio to the others, when it attempts the task of scientific discrimination among diverse hypotheses at the same time.

I am inclined to think, upon a not as yet extensive experience of applying psychological criticism to the Leonard material, that a nearer approach to exact method will probably be found by taking the hypothesis of discarnate communication—or any other hypothesis that promises continuously to account for the facts—as the main line for theoretical development, and not subjecting the hypothesis to an unceasing barrage of reservations and alternatives. If the hypothesis, carefully followed up, produces a spurious psychological structure, this should be capable of demolition. And I have a strong suspicion that, in following closely and without distraction upon the trail of a communicator regarded as a human being, we shall discover a good many hitherto unnoticed facts about the processes of communication, which will be of use in understanding the necessary conditions of experiment or of further research into the most complex of all our problems.

II

An Example of Evidence of Intention in Book-test Material[1]
(The " La Vita Nuova " Case)

Early in my study of the Drayton Thomas sittings I came upon the long series of book-tests, of which a number have been published in *Some New Evidence for Human Survival*; and I noticed repeated statements at the sittings that book-tests had some general trend which would be discovered, and that a good deal was to be found out by comparing one book-test with another—something apparently of the nature of evidence through cross-correspondence. Hitherto, book-tests seem to have been examined chiefly for their evidential value as units, though as the technique of this form of communication developed, from 1918 to 1921, the units tended to become groups of tests given at one time, and having some interrelation with one another.

In Mrs Sidgwick's " An Examination of Book-Tests " (*Proc.*, Vol. XXXI, Part lxxxi) the ordinary technique of a book-test is thus exemplified :—

" Feda might tell the sitter that the communicator wants him to go to the book-case between the fireplace and the window in his study, and in the third shelf from the bottom to take the seventh book from the left and open it at the 48th page, where about one-third of the way down he will find a passage which may be regarded as an appropriate message from the communicator to him."

I think most careful investigators of book-tests, at this time, were trying to narrow down the issue to the distinctly important question whether *what was actually said at the sitting* had a really close correspondence with what was found in the passage in the book which had been so elaborately and selectively indicated. It was often said, I remember, that if only the exact words of the book could be given at the sitting, this would be conclusive. It would in fact be cogent evidence only of extra-sensory perception; and Mrs Sidgwick, with her unvarying sureness of touch in matters of evidence, concludes only from her examination of book-tests that evidence of clairvoyance appears to be established. She does not put evidence of communication out of court, but the method of inquiry is one that puts the problem of intention second to a proper treatment of the question whether these cases really are, as they seem, paranormal.

[1] Containing the substance of a lecture delivered to the Society on 29 May, 1935.

It is obvious that the more nearly we confine ourselves to evidence that the communicator "knows" what is in the book, the less likely we are to follow up any intention with which that knowledge may be used, except the simple intention to display it. However, there emerges spontaneously, for what this is worth, a manifest intention to refer, in the book-tests, to interests and reminiscences which the communicator and the sitter have in common. Such statements as, "This refers to something that was important in your life ten years ago", have a good deal of evidential interest when they are substantiated.

But we are not fully testing evidence of intention by simply deciding what to expect from what is said at the sitting, and then looking in the book to see if it is there. More satisfactory evidence of intention is something relevant that we should not have expected or, still better, could not have expected. Further, the appearance of a common aim, unsuspected by the sitters, in book-tests given to different people, would at once raise questions beyond that of a display of paranormal faculty.

Lady Troubridge and Miss Radclyffe Hall, who made a careful and thorough compilation for the Society of book-tests given at their own Leonard sittings, have noted that the subject of "psychical research" is very often the content of the book-passages referred to. If this is the fact, it looks significant, when we remember that the books selected by the mediumistic process are on all kinds of subjects, and that the chances of hitting on a passage that will bear this interpretation are few and far between. References by implication to "psychical research" could be roughly estimated, but the phrase does not provide a clear enough dividing line for purposes of classification, so after some consideration I elected to classify all the satisfactorily identified book-passages, in the different collections of Leonard book-tests then before me, according to whether they did or did not refer appositely to death and the condition of the dead, or to some relation between the dead and the living. This as a criterion of judgment is still not very exact, but it seemed in practice to be quite sufficiently so for the purpose. There were few borderline instances, and these I placed on the "No" side. It may be noted that I avoided tabulating results as I went along (since visible competition between growing columns might be a distraction) and had a general impression, before counting up the totals, that there was no more than a significant minority of positive results. Actually they turned out to be just over 60%, in a total count of 91 book-tests. Where two or more book-passages were indicated in one test, I have counted one or more positive results as one, neglecting negative results *in the same test.* It will be remembered that the remaining 40% of tests represent the competition of many other subjects, such as memories shared between the communicator and the sitter. A general control and selection of book-tests has, at various sittings, been claimed to be exercised by certain skilled communicators, who were said to advise and help others in the use of this technique. The further claim that they were selecting book-passages that would prove to have some unified and discernible trend gains possible support from the figures.

As a check-experiment covering part of the ground, I made a random collection of 500 whole-page examples (for the passages indicated in book-tests, the " target " is usually half a page or less) from books in the S.P.R. Library, grouping my selections artificially so as to make a proportional correspondence with book-tests containing more than one reference. I found that a similar estimation of these pages as containing any allusion to death and the condition of the dead, or to some relation between the dead and the living, gave under 20% of positive results. Professor E. R. Dodds has lured me into a check of the check-experiment by suggesting that the choice of S.P.R. library-books might not, as I had supposed, weight the scales in favour of the chance finding of such allusions, since the S.P.R. has many theoretical books not specially concerned with the question of survival. Actually, I had avoided technical treatises ; but I followed up the suggestion that a collection of novels might have better results,[1] and included with these some definitely spiritualistic books in the proportion of one in five, to cover the probabilities of Leonard sitters having such books on their shelves. The result was a positive count of under 12%, again taking whole pages, and making full use of any imaginative interpretations that I could think of. I have been surprised, in taking random selections from Sir Oliver Lodge's *Raymond, or Life and Death,* to find how very often one hits on a passage that no ingenuity can twist into an allusion of the type sought for. A great deal of every book is necessarily devoted to talking around its subject, and even in pages of the printed record of sittings there is a remarkably small amount, in actual bulk, of wording that fits the required classification.

Turning to more individual points of intention which may appear as common to the records of more than one sitter, I should have

[1] Here Professor Dodds removed one weight from my scales : for I think the average of words to a page, in novels, is less than in S.P.R. library-books. But the results, in any case, should be interpreted as " of the order of " the the given percentages ; hence my large precaution of taking whole pages.

been greatly prolonging my digression into book-test material if I had made a parallel study of all the different book-tests that are on record, as they were given week by week to different sitters. On the hypothesis of communicators' intention, it seems doubtful whether an extensive collating of results by different sitters would have been anticipated by the communicators. But it seemed a reasonable enterprise to look carefully through some records in which one presumably skilled communicator was specially said to have taken part in book-test material given to sitters not related to him, and in such circumstances that all the sitters concerned were in a favourable position to compare notes. I can refer without embarrassment to the fact that certain excellent sitters failed to do so, or to observe what was at least a notable coincidence, because it is pretty clear that they were misled for once by the psychical researcher's excellent acquired instinct against any comparing of notes that may lead to the spoiling of evidence.

Lady Troubridge and Miss Radclyffe Hall made a number of experiments, with interesting results, in the use for book-tests of foreign books, including Italian, since they had a possible communicator who had been an Italian scholar. It was said at sittings, however, that Dr A. W. Verrall (Mrs Salter's deceased father) would co-operate instead in these experiments with Italian books, since skill in the technique of book-tests was necessary as well as familiarity with the language. Dr Verrall was a keen student of Dante. (It has been claimed by Feda that the language in which a book is printed need not matter, because the meaning of a passage can be " sensed ", but I think there is nothing in evidence by which we can test this statement.) The claim that Dr Verrall was participating in the experiments was, so far as I can judge, in no way contradicted by the character or literary quality of the results.

In April, 1918, the experimental group of Italian books appears to have been partially dispersed, and there was no special arrangement of books in Miss Radclyffe Hall's flat in London. As in all book-test records that I have studied, there is in this case no likelihood of Mrs Leonard (then at Datchet) having had access to the books at any time. Mrs Salter was then in touch with Lady Troubridge and Miss Radclyffe Hall over the question of making proper record of the book-test episodes—we owe to them some of the best-kept and best-annotated records on the S.P.R. files—and an opportunity was, it would appear, rather specially available for Mrs Salter to be referred naturally, at a sitting, to books in this flat.

On April 23, 1918, Mrs Salter had a sitting alone with Mrs Leonard, at which these books were mentioned and a group of book-tests were given. Mrs Sidgwick quotes one of these tests which does not concern us here, and uses the words " the row of books meant having been clearly indicated " (*Proc.*, Vol. XXXI, p. 277), so I need hardly take up space by quotations to show the process by which a particular shelf was satisfactorily identified, especially as a second identification of it will be found later in these pages. Feda says that half-way down page 13 in the fourth book going from left to right on this shelf, is something that " would refer to a part of your " (Mrs Salter's) " life, when you were about twenty, as near as he can get . . . certain changes that happened about that age . . . you went through a transition. It bore fruit later, what you were going through when you were about twenty.[1] "

At this age Mrs Salter's experiments in automatism changed over from table-tilting to automatic writing, and the passage indicated in the book definitely reminded her of the period and set her looking up old records. The book was the Italian story of a wooden puppet that came to life, *Avventure di Pinocchio* (" Adventures of Pinocchio " ; extracts from an English translation were broadcast to children a few years ago by the B.B.C.). The passage that attracted Mrs Salter's attention ends two lines above a half-way division of the letterpress on page 13, and reads as follows (translated) :

" The piece of wood gave a great shake, and slipping violently from his hands, went and banged with force against the meagre shins of poor Gepetto."

" The remainder of the page " (Lady Troubridge notes when translating) " consists of an argument as to the author or cause of the movement of the wood ".

The incident of which Mrs Salter was reminded by this passage could be clearly placed by extant records. Her notes following this 1923 sitting contain the following :

" The experimenters on this occasion were my mother " (Mrs Verrall) " and myself and the date was September 1, 1903, I being then twenty years old. After some ' Messages ' had been tilted, my mother notes that ' the communicating intelligence showed an inclination to move the table and after some rocking which was tiring and useless we asked if it could move some object in the room. It said yes, and we asked what. Answer : " Pencil ". The pencil was lying on the brass table near (not the table which was being tilted) and I moved my foot away so that I might not accidentally shake the table and make the pencil move. After some time it (the table used for tilting) suddenly made a dash at me, hit my foot so that I

[1] This and all citations that follow are from the original records and notes of sittings in the Society's possession.

moved quickly, touched the brass table and set the pencil rolling. This it claimed as a success! and said in answer to questions that it was humbug and this was what was intended. Neither Helen nor I had the least expectation that it would do this sort of thing.' "

"I quote this record" (Mrs Salter's note continues) "because it seems to afford an oddly close parallel to the description nearly half-way down page thirteen of the test-book of a piece of wood which gets out of control and hits someone in the legs. But the fact which seems to me of chief interest in connexion with this book-test is that in the spring of 1903, when I was within a few months of being twenty, I did my first experiments in automatic writing. Now the change from table-tilting (which I had done before) to automatic writing, might be described as a transition which bore fruit later, for nearly all my subsequent and more interesting experiments in automatism took the form of writing."

For those who do not know the Pinocchio story, it is worth noting that the piece of wood which displays mysterious vitality is the beginning of the existence of Pinocchio the puppet, with its queer, semi-human, marionette-like behaviour. It does not seem a great stretch of the imagination to compare this with the faculty of automatism as seen from a communicator's point of view; and the whole humour and pathos of puppets, which have a place of their own in Italian tradition and literature, may easily be thought to supply an intended context to an allusion connecting puppetry with automatism. Dr Verrall was an Italian scholar, and it seems that the whole idea would be in character. I am quite aware that I am here letting imagination continue beyond the bounds of evidence, and I consider that this is a correct experimental use of the imagination. One can, equally, try to imagine a probable context of thought in a given case and find, very instructively, that the imagination is on the wrong track.

The "Pinocchio" book-test leads on to the indication of another book at Mrs Salter's Leonard sitting. The "Pinocchio" book, it has been seen, is clearly marked down at the sitting by its position on the shelf, and is subsequently established as a book that could be pointed out with intention by the communicator. Feda proceeds to put the sitter on the track of a second book, this time without precise identification by place:

"Within a span of the test-book is a book whose title gives the effect, no, not quite effect (*sotto voce*—wait a minute) 'tisn't quite right. What would you call it? It gives the idea of something that happened to your character or personality about the time your lady passed over, in a way through her passing over."

The contemporary annotation reads :

"The seventh book to the right of the test book appeared to be the only book whose title could be considered as applying in any way to Feda's words. This book was entitled *La Vita Nuova* and came well within the span." Mrs Salter's associations with this title, written down soon after the sitting in 1918, were as follows :

"My mother died on July 2, 1916, and I was married just about nine months before, on Sept. 28, 1915. *La Vita Nuova* is associated in my mind with my wedding because an old friend of my parents, Dr Butler of Trinity, gave me a beautifully bound copy of it for a wedding present with good wishes for my own 'New Life'. The phrase is also associated in my mind with my mother's death. Several friends writing to her in reply to a letter informing them of the fatal character of her illness, referred to my recent marriage, saying that my mother must be glad to think that I should not be left alone, but had now a home of my own ; and one friend, writing to me immediately after my mother's death, said she felt it had been a great pleasure to my mother to think of the 'New Life' that had begun for me. She used these words in quotation marks. It cannot of course be said that my new life in the sense of my marriage happened through my mother's death, since it preceded it by nine months, but it should be observed that (*a*) the word 'effect' which Feda first uses is rejected as not expressing what the communicator desires to convey, and the word 'through' is modified by 'in a way' ; (*b*) the break with my old life which was begun by my marriage was made complete and final by my mother's death. During the nine months after my marriage when my mother was still alive, I spent more than half my time with her at Cambridge. It was not until after my mother's death that my husband and I settled down together in the house which we now occupy. When we first married we were temporarily in rooms."

There is, clearly, a striking evidential content in these associations, with its most objective point in the existence of a special copy of the book itself as a wedding present ; the only evidential weakness, so far (where this case leaves off in the records), is the factor of chance in even a highly relevant book-title being "within a span" of a specified book. One might make very many random selections of about fourteen books in a group without finding, in any such group, one title having such clear relevance to one's personal associations, and Feda's indications of time and circumstance certainly narrow down the field ; but the thing is not outside coincidence. On the hypothesis that Dr Verrall inspired the communication, and was aware of the form in which it was conveyed, he might wish to do something further to put his intention beyond reasonable doubt; or he might have planned this allusion to Dante's *La Vita Nuova* as only one among other converging allusions—a technique which is suggested by other records as characteristic of this communicator. It is towards some such hypothesis that I have been drawn by what followed.

On the day following Mrs Salter's sitting, April 24th, 1918, and before Mrs Salter had verified her book-tests of the 23rd or communicated with Lady Troubridge and Miss Radclyffe Hall, Miss Radclyffe Hall was due for a sitting with Mrs Leonard, which she attended alone. At the very beginning the sitter mentions, for experimental reasons, a change which is being made with regard to some books not in London, and not concerned in the present case. Feda acknowledges the information, and quickly changes the subject, as follows:

"Yes, that's all right. She and Mr Arthur" (*i.e.* Miss Radclyffe Hall's communicator and Dr Verrall) "were doing a test from the London books, they was giving it to Mrs Nellie" (Mrs Salter) "yesterday, but they'd got it before yesterday." (This seems to imply previous preparation.) "It was Mr Arthur, not herself."

This clearly directs attention to the previous day's sitting, and a *sotto-voce* remark a little later, "Wait a minute—she told you Mr Arthur got one", seems to show anxiety that the point should have been got through. Feda mentions that "Mr Arthur" had told Mrs Salter that the bookshelf was "about four feet from the ground" (this had been an addition to the indications given for identifying the shelf, which was found to be four feet and three-quarters of an inch from the floor).

The eighth book from the left is then mentioned, and its 98th page, "about half way down". It is clear enough to the reader that this should be on the same shelf that was indicated in Mrs Salter's sitting, but Miss Radclyffe Hall, not knowing about this sitting, except from Feda, and being busy with her notes, naturally asks, "But where is the book?" Feda replies:

"In London where the line of books is. There are two places where books are. This is the long line of books." Feda adds that there are curtains in the room of "a dull, dark green", which is correct for the room with the long bookshelf; and Miss Radclyffe Hall feels assured of knowing exactly where to look. (If she had not been so careful, but had relied on the reference to Mrs Salter's sitting, they might have been led to compare their findings and to discover the point which we shall come upon later.)

The content of the required book-passage is given in these words : " The message is not personal : it's merely something that she thinks could be applied to the war and its effect upon conditions." The passage is described as " this quite short sentence ", and as being " not exactly half way down, a little bit lower ". It is also said that " page 98 appeared to her to fall on the right-hand side of the book ". It is, I believe, almost invariable that page 1 in a book is a right-hand page, so that all right-hand pages have odd, not even numbers ; and in this instance the relevant passage was found at the fifth line below the middle of the right-hand page 99, facing page 98. It is an odd fact that among errors in book-tests, a disproportion that seems well above chance consists in significant passages which are found in the right position on a page, but on the page facing the one of which the number has been given—the passage found being thus in actual contact with the place indicated when the book is closed, as it presumably is when any " reading " or " sensing " of its contents occurs for purposes of book-tests. The occurrence can almost be classed as an evidential error, though it must properly be regarded as halving the odds against a chance result. These odds, however, are enormously high in respect of the *placing* of a passage among a group of books, supposing the relevance of the passage, when placed, to be satisfactory.

The eighth book from the left proved to be a Spanish translation of *The Scarlet Pimpernel*, unread by the experimenters who had included it in the row, and the passage identified (and translated) refers to :

" . . . the thought of the brave man who, unknown to fame, had rescued hundreds of lives from a terrible, often an unmerciful fate."

The terrors of the war, and what would be the spiritual outcome of the ordeal, had been a subject often referred to by Miss Radclyffe Hall's communicator. In view of Feda's description, Miss Radclyffe Hall picks out the word " terrible " (Spanish *horrendo*) as having chief significance, but I think equal emphasis should be laid on the ideas of courage and of good service to others, in view of what follows.

Directly after the indication quoted above, Feda says :

" Now wait a minute, within a span of the test-book—Ladye [1] likes ' within a span ', it 'mooses her !—there's a book whose title gives in quite exact terms what the spirit world knows will come out of these terrible conditions, it's exactly right, it wouldn't have to be altered in one letter, it's what they's all working for, and what they *knows* will come out of the War."

[1] Nickname for the communicator.

The only title found relevant was that of a book two places to the right of the test-book : *Brava Gente* (" Good People "). There may be some point in the insistence of the communicator upon the literal aptness of this.[1] The association of " brave man ", a natural enough translation of the Spanish " *valoroso personaje* ", with the Italian " *brava* ", which does not directly mean " brave ", is accidental ; but where I think the word " *brava* ", *not* changing a letter, is of interest, is in the fact that it admirably represents a transition from the idea of valour to the idea of a courageous type of virtue, which could well be regarded as a hoped-for outcome of the war.

It will be noticed that in this sitting we again have one book clearly marked down by position, and another book near to it which the sitter is led to identify with what appears to be sufficient cause. After this second indication, Feda went on :

" And close to that last-mentioned book, so close that she would almost dare to say it was the next one " (the experienced Leonard sitter will suspect that this may be a Feda-dilution of a positive assertion) " is a book whose title also gives correctly what she knows will be given to the world through your work and hers " (communicator's), " that is to say that all you and she and Mrs Una " (Lady Troubridge) " and Feda is working for, will result in exactly what the title of this book says."

The next book on the right to *Brava Gente*—as will have been realised by those who habitually keep figures in their heads—was *La Vita Nuova*, the book with the wedding-present and other associations to which Mrs Salter had apparently been directed in her book-test of the previous day ; this test not yet having been verified, and Mrs Salter being still in complete ignorance of what books would be indicated by it.

If an entirely different trail was, in fact and intentionally, laid to conduct Miss Radclyffe Hall to the same book that would be found by Mrs Salter, it would have occurred to any experienced communicator, knowing his S.P.R., that the hypothesis of chance-coincidence had still not been quite satisfactorily eliminated. There are several other considerations to suggest the advisability of placing a third shot on the same mark ; for one, glancing shots may be either very skilful or quite accidental, and it would help the case if it were shown that a direct hit could also be made.

Continuing with Miss Radclyffe Hall's sitting, we have a few remarks about the titles of books, apparently without significance, unless to work in a reminder that Dr Verrall is to be taken as co-operating in the tests. " You may have to dislodge some of them "

[1] Cf. Mr Salter's comment on a somewhat similar point, p. 49.

(the books) "in order to see the title. Mr Arthur remarked that too, she says." However, I call attention not only to the re-introduction of Dr Verrall, but also to the slightly odd use of the word "dislodge". With or without exterior intention, it appears to divert the flow of speech into this channel:

"Lodge, Lodge, Raymond, that's their name (*sotto voce*, Feda's remark about Raymond was velly applopliate) 'cos in the eleventh book going from left to right (*sotto voce*, Well, what page please?) there is on page three four, thirty-four, there is near the lower part of the page, a message for Raymond's mother which she'll understand as it was touched upon in the sitting of Monday. . . . She'll understand it and be glad you sent it, the message isn't from Raymond, she'll understand if you say that. . . .

"You know, Ladye says it's so extraordinary, but she has to act upon Feda sometimes in a way Feda don't understand when she's in the medium, and she was aflaid Feda wouldn't take up the reference to Raymond, so she had to worry to get a word that would suggest Raymond to Feda; she says she's done that often and she's wondered if you had guessed she was doing that, and how carefully she has to lead Feda to a new idea. Feda knows that, 'cos when Feda's in the medium she's only got like half of Feda's own sense, she's not half so clever as when she's out of the medium!"

The eleventh book from the left was *La Vita Nuova* itself. The verification of an appropriate passage contains the same evidential flaw which we noticed in the case of the book in Spanish. The passage recognised as relevant to Lady Lodge, when the test was referred to her, occurs "near the lower part of the page" (actually within the last eight lines), not of page 34, but of the opposite page 35. Translated, it runs as follows:

"He could not do to you being dead that which, living, he would never have done to you; he lies under another sky than yours, nor must you hope ever to see him, until that day when all your fellow citizens will be visible to you, and you will be able to see their faults examined and punished by a Just Judge.

"Therefore if hatreds, angers and unfriendlinesses cease with death in whosoever dies, as is believed . . ."

Lady Lodge's statement reads:

"The message is understood by M.F.A.L." (Lady Lodge) "who knows from whom it comes.

"She has reason to think that a certain friend has been trying for a long time to set her mind at rest. He has since said through another and independent channel that he feels he has accomplished that, and is happier in consequence."

The verification shows relevance rather than points of evidence that can be put down in black and white; Feda's reference to "the sitting of Monday" is, unfortunately, not mentioned; but this selection of the book for the third time can be held to show that a direct hit could be made, so as to settle any question of real intention to pick out *La Vita Nuova.* It will be remembered that the medium's mind could not, as we can, calculate in either of the two previous instances that the eleventh book from the left was being indicated, because in both cases the indirect indications included the undetermined distance "within a span", which had to be made definite by something observed by the sitter to be uniquely appropriate in the titles within that distance. Clear objective directions, given in all three cases, would have been less evidential. The third selection of *La Vita Nuova* is not, however, supported only by the direct method of indicating the book; accepting the occurrence of an intended passage in the right part of the page opposite to the page indicated (which I think justified by the evidence in many parallel cases), the passage quoted is very definitely on the relation between the dead and the living, and is recognised as appropriate to the annotator to whom it is definitely referred, Lady Lodge, proving also not to be a "Raymond" message, in agreement with the Feda statement that it is not.

If we look at these three converging references to *La Vita Nuova*, put together, and examine the whole subject-matter that is raised by them to see whether, as a whole, it contains any evidence of design, we come upon the quite interesting fact that a general concept, "The New Life", is linked by the material with three successive views of human progress through time. First, in the material given and elicited by Mrs Salter's book-test, a retrospective picture is formed of the new life that begins with marriage and the severance of family ties; and there is presented with this the picture of another actual change, towards a more free and expressive technique of automatism, and the "new life" of that curious puppet in the subliminal which most automatists will recognise as having its strings (and occasionally its leg) pulled by various agencies.

Second, in the book-test given to Miss Radclyffe Hall, the picture extends from what were present-time conditions in the spring of 1918—war, and terror overcome by courage—to a prospective hope of "a new life" on this earth for *brava gente*; and this is closely linked with the thought of new work on the problem of communication as bringing "new life" in a further sense.

It will be noticed that the direction of thought to reminiscence in the first sitting, and to present and prospective terrestrial life in the second, could in itself come from the medium's mind normally, though without the confirmation and context that are supplied through book-passages and titles unknown to the medium. In these episodes it is part of the evidence that pointers should be given, at the sittings, to past and to present time. But in the third case, the book-test given to Lady Lodge, no indication is or need be given at the sitting to locate, as it were, the subject of reference, and we discover that the book-passage actually found (accepting the method of finding) constructively extends the sequence leading from the past to the terrestrial present and future, into the conception of a discarnate future in which thoughts of judgment and expiation pass into the thought of hatreds, angers and unfriendlinesses ceasing with death.

I should not for one moment say that this constructive appearance of the total product proves design ; the construction might be partly that of coincidence and partly my own. What I am pointing out is that it is consistent with design. If a series of three complex processes, which seem to carry intention throughout and to be directed to a common end, resulted in an end-product to which no further element of design and meaning could be attributed, there would be the less reason to give weight to the whole hypothesis of intention.

The better I may have succeeded in my attempt to set out this case clearly, the odder it may have seemed to the reader that practised investigators such as those concerned at the time did not observe the triple reference to *La Vita Nuova* and follow up the clue. Having the records before me as they were ultimately and methodically arranged for filing, I myself blundered considerably through failure to keep clear all the different elements of movement, time and place that were involved, before I had distinct in my mind such questions as who went where, did what, and noted it down when. The actual process of collecting and properly compiling book-test material requires a concentration on accuracy in detail during which it is very difficult to step back from a case and look at it as a whole ; and at the time of these sittings, and later of Mrs Sidgwick's *Examination of Book-Tests* (by which time the elements of this *La Vita Nuova* case were in separate files), the whole focus of attention was upon the success or failure of individual tests, though the *Examination* takes note of several apparent cross-correspondences. And, as I have remarked, the investigators were using the correct amount of caution against collaborating too much and so vitiating future evidence.

Another very natural question is whether, if there were design in this case, there would not be some hint of subsequent effort by the designer, after realising that a carefully prepared structure of evidence had passed unnoticed. To take things in the order in which they were observed, but not interpreted, by the investigators, this is what happened next. Mrs Salter's sitting was on the 23rd, and Miss Radclyffe Hall's on the 24th, of April 1918. On the 26th (a date which will be significant later), Lady Troubridge, Mrs Salter and Miss Radclyffe Hall met in London and went to Miss Radclyffe Hall's flat for the purpose of verifying the book-tests. We must now jump forward to the next occasion on which Dr Verrall purported to communicate at a Leonard sitting. This was on May 15th, 1918, Lady Troubridge and Miss Radclyffe Hall being the sitters. A book-test was given of which the record opens thus: "Feda don't know what Mr Arthur's come for today. . . . He's pleased to come, but he has a funny feeling that someone asked him to come, or suggested his coming. . . ."

It is possible, on our communicator-hypothesis, that the last words have to do with some committee-work behind Dr Verrall's co-operation, the "funny feeling" representing a vagueness of Feda's. At all events, it is clear that Dr Verrall is supposed to be somewhat unexpectedly present.

In the ensuing book-test, in which Dr Verrall is specifically said to be taking part, though, as usual, the communicator "Ladye" is said to be giving the instructions to Feda, the bookshelf is given as "in your room in London" (Miss Radclyffe Hall's) "not the books in one row, the books in several rows". The rest of the instructions, for the book which is of interest in our present case, are sufficiently recapitulated in the following notes by Lady Troubridge on the verification.

"She" (Feda) "tells us that upon the shelf beneath the highest shelf of the book-case, a book has been selected by Dr Verrall. This book is said to be at the end of the row nearly, on the left hand side. It is said to be a book whose title should be taken as a watchword by M.R.H., U.V.T., and Mrs Salter. In Dr Verrall's opinion it is an excellent watchword, it is something for them all to keep in front of them, and to know that 'in very truth' it will be their watchword. Dr Verrall says through Feda that they will understand why this will be their watchword, and that it is all linked up with the work which they are doing.

"On the extreme left hand side of that shelf, being in fact the third book from the left, was a book entitled *Nova Solyma*."

It is explained that *Nova Solyma*—New Jerusalem—together with the content and meaning of the book, conveys an idea of beautiful order which the annotators find relevant as a " watchword " for their work in psychical research. It may occur to us, with our present knowledge, that there is a reiterated attempt to draw attention to the *words*. The phrases summarised by Lady Troubridge include: " What's a watchword ? Keeping time ? No it isn't, he says " ; and later the actual phrasing is " in very truth it will be your watchword, he says you'll understand why ; it's all linked up with the work that you're doing ". I suggest only that this *may* be an attempt to get the verifiers and Mrs Salter to associate from *Nova Solyma* back to *La Vita Nuova*, and to ask themselves where and how *La Vita Nuova* had occurred in their work. If so, the attempt nearly had its effect, for Lady Troubridge has a later note in the records connecting *Nova Solyma* with the occurrence of *La Vita Nuova* in Miss Radclyffe Hall's sitting. But Mrs Salter's record about *La Vita Nuova* was in her own safe keeping, and the third case sent to Lady Lodge was not remembered.

After this present paper was in typescript Mr W. H. Salter saw it, and raised a point which was unknown to me, about the very special interest taken by Dr Verrall, as a scholar, in the truth of words to their etymological meanings. Dr Verrall had made it clear[1] that in the Greek dramatists a word or name is given significance as being impressively " true " to a given context or situation because of a double meaning which it bears by derivation. What in modern English would seem a mere play upon words, and in Shakespeare supplies a type of punning which to us appears strained and laborious, was to the Greek mind a part of the magic of words, so that coincidences of derived meaning could make the application of a word especially " true " (ἔτυμος). In this connexion, Mr Salter points to the use by the Verrall communicator of the words " in very truth " and their application to the book's title. (The reader may have shared my own feeling that the use of this phrase was curious, and called for some explanation.) Mr Salter remarks that if this particular idea of " truth " in derived meaning was intended by Leonard-Verrall, " the point is the etymological significance of Solyma = Salem = Peace. Very possibly Mrs Leonard knew that equation. By clairvoyance, if that faculty is to be admitted, she might be able to give the location of a book with that title, but she could not possibly know the special significance of the words " in very truth " for her communicator, except as the result of some further faculty of supernormal cognition."

[1] Notably, as Mr Salter points out, in a long appendix on the use of ἔτυμος and ἐτήτυμος, which occurs in Dr Verrall's edition of *The Seven Against Thebes*.

It seems to me that this characteristic point about Dr Verrall's sensitivity to words adds to the appropriateness of *Nova Solyma* in the whole context in which *La Vita Nuova* was given at Miss Radclyffe Hall's sitting of May 24th, 1918, with its transition from war to an outcome in terms of human and spiritual values; and in commenting upon that sitting I have referred (without knowing when I wrote of Dr. Verrall's special attitude towards derivative meanings) to the possibility of a derivative interpretation of the title *Brava Gente* (p. 44), considering the insistence at the sitting upon " quite exact terms ". It struck me that a philological point was involved, and I had in mind (and introduced into my phrasing) the parallel association of *virtus*, valour, with virtue. It can be worth while to explore these rather fine-spun contexts, when the process consists in making sure that a broad structure of evidence does not show poverty when it is followed up into its finer details.

Following upon the " in very truth " passage in the sitting, we have this:

" (*sotto voce.* You know that in forming a bridge . . . the bridge builders)—no, you haven't got it right, it's that he thinks of you as the bridge builders. . . . He says bridges can't be builted from one side only. . . ."

This passage led on to a book-test which caused the verifiers to note a number of references to Kipling (whose story called " The Bridge-Builders " will be remembered), and to discover from Mrs Salter that an interest in Kipling's work was characteristic of Dr Verrall. The book-test was evidential, and was taken from *Captains Courageous*, which somewhat echoes the theme of bravery, but its details would take us somewhat far afield. It served the function of emphasising " bridge-building " in an evidential association with Dr Verrall, with a possible intention which we shall observe in a moment.

It will be recalled that in the sitting with which we are now dealing (May 15, 1918), the first book-test opened with the introduction of Dr Verrall and the question of his reason for coming. The way in which this was put puzzled the investigators, and Lady Troubridge notes that in another part of the sitting Feda said:

" Do you know if Mrs Nellie's been anywhere ? " (This means Mrs Salter.) And again, " Feda feels it's something to do with Mrs Nellie, and that's perhaps what brought Mr Arthur here as well as the book-test."

Lady Troubridge's notes (slightly paraphrased) record the next procedure:

" On Friday May 24th, 1918, Miss Radclyffe Hall wrote to Mrs

Salter, enclosing the notes of the sitting and asking her if she could throw any light on that part of the test concerning her. On May 27th, 1918, Miss Radclyffe Hall and I called on Mrs Salter at the S.P.R., when Mrs Salter handed us her answer to the letter, which she had not yet posted. The chief interest lies in the fact that on April 27th, 1918, Mrs Salter herself had produced a piece of automatic script. It was the only piece of script produced by her between February 15th, 1918, and May 23rd, 1918, and that script opens with these words :

"The bridge and the river . . . Misericordiam Domini."

Now Mrs Salter had the impulse to write this script on April 27th, the day after she had met Lady Troubridge and Miss Radclyffe Hall and they had failed to associate the three separate references to *La Vita Nuova* with one another. I am inclined to think that the sense of "*Misericordiam Domini*" might appropriately be brought out by an exclamation mark.

The "bridge" references led Mrs Salter to look up a much older script of hers (Feb. 24th, 1916), containing the words "An arch and a bridge over a river . . . not the bridge of sighs, but it is a real bridge . . . a triple arch".

This was written in other circumstances, but the fact that Mrs Salter, an automatist, found herself associating it with the more recent script may have relevance. "Triple arch" certainly aroused associations in Lady Troubridge's mind with a curious point about the sitting at which, apparently by Dr Verrall's instigation, all this had arisen. I quote again from her notes :

"Note the words, 'A triple arch', as having a possible bearing on this book-test of May 15th, 1918. *Three* figures rather conspicuously in a part of the test. M.V.B." ("Ladye" in Feda's nomenclature) "and Dr Verrall make reference to *three* of Kipling's works. Dr Verrall selects *three* people as his bridge-builders, namely Mrs Salter, M.R.H., and U.V.T. In Kipling's story, 'The Bridge-Builders', there are *three*, and only three, principal characters, namely the chief engineer, his young assistant, and their faithful native foreman. . . ."

It would perhaps have been less interesting if the three references to *La Vita Nuova* had been observed and put together before these words were written. The recurrent threes in the material, if they had been noticed after the fact of the triple reference to *La Vita Nuova*, might have suggested coincidence-hunting. But when this fact of the recurrent threes is noticed as a curious and insistent phenomenon in a Verrall communication, and left practically unexplained, because the fact to which this insistence could be a pointer has remained hidden, we seem to have intention observed, so to speak, in the air, the observer having no point of attachment for it but still emphatically feeling it to be significant.

It may be noted that eleven years later, Aug. 2nd, 1929, the Rev. W. S. Irving was given a group of book-tests from books in Mr Salter's house at Newport, Dr Verrall being said to co-operate with the late Mrs Irving in producing the tests. Among the indications at Mr Irving's sitting with Mrs Leonard (of which an excellent verbatim record is on file) was a reference to christening and babies, related to a book which Mr Salter identified in his shelves as another copy of *La Vita Nuova*—Messrs. Kegan Paul's small edition (1903) entitled *The New Life*, with Italian text and English translation on alternate pages. The editor and translator was Dr Luigi Ricci, and the fly-leaf of this copy bears an inscription with Dr Ricci's signature paying homage to Dr Verrall as a *cultore* of Italian literature. The allusion was without significance for Mr Irving, but could well be taken as a personal touch introduced for the benefit of Mr and Mrs Salter, who were then awaiting the birth of their second child. They had had previous evidential references to the coming birth of both their children through Mrs Leonard, purporting to be given by Dr. Verrall. This " new life " reference was not stringently evidential, and was more in the nature of an appropriate by-product of the Irving material ; but again, it would have lessened the whole case for intention in connexion with Dr Verrall and *La Vita Nuova* if the allusion had been quite out of context with the material given eleven years before. As it is, Mrs Salter is first referred to " new life " in connexion with her marriage, and then again in connexion with the birth of a child, the construction of a book-test for an independent sitter providing the opportunity to give some evidential value to the latter reference.

Here, so far as I have observed in the records, the evidence in the " *La Vita Nuova* " case ends. If I were to give further rein to speculation, I should wonder whether it was thought after the " *Nova Solyma* " attempt that further efforts would be unavailing to bring its separated components, now rapidly becoming overlaid with new material of interest, together again ; or whether perhaps it was foreseen, in the fuller prescience which we are told is possessed by the discarnate, that the records would in due time be assembled, and that later still their connexion would be observed by some investigator undistracted by the rush and pressure of ever-fresh cases to be noted, annotated, and put in order for the service of research.

Eleanor Mildred Sidgwick

PROCEEDINGS

OF THE

Society for Psychical Research

PART 146

MRS HENRY SIDGWICK'S WORK IN PSYCHICAL RESEARCH

By Alice Johnson

Eleanor Mildred Sidgwick was the daughter of James Maitland Balfour, of Whittingehame, Prestonkirk, East Lothian, and Lady Blanche Balfour, daughter of the second Marquis of Salisbury. She was born in 1845, the eldest of a family of three daughters and five sons. From an early age she showed a special aptitude for mathematics, which she studied first under the village schoolmaster at Whittingehame, and later with the help of her brother-in-law, Lord Rayleigh. While he was Professor of Experimental Physics at Cambridge, 1879-1884, he was pursuing his researches on Electrical Measurements, in which work he had the assistance of Mrs Sidgwick, and with her carried out his classical measurements on the Silver Voltameter and the Latimer Clark Cell, thus establishing definitely the units of resistance, current and electromotive force. They published three papers jointly in the *Philosophical Transactions of the Royal Society*.

Lord Rayleigh, like other great English physicists of that period, habitually employed the simplest form of apparatus, making use of whatever came nearest to hand, and yet obtaining results of surprising accuracy, which were only further substantiated by more elaborate research.

Mrs Sidgwick had the same preference for the simplest kind of apparatus for whatever work[1] she was engaged in ; although interested in all mechanical devices, she was never tempted to become so far interested in perfecting them as to lose sight of the ends they were intended to serve.

[1] This was also characteristic of her brother, F. M. Balfour, in his biological work.

Like other members of her family, she took a keen interest in science generally. But she once remarked to me that mathematics especially appealed to her in early youth because she thought a future life would be much more worth living if it included intellectual pursuits, and I imagine the abstract nature of pure mathematics seemed to her specially adapted to a disembodied existence.

The last important paper contributed by Mrs Sidgwick to the *Proceedings* (written at the age of eighty-seven) was the History of the S.P.R., compiled for the Society's Jubilee in 1932, and published in vol. xli. This article must, I think, have seemed to older members not unlike the tragedy of *Hamlet* with the Prince of Denmark left out, for there is hardly anything in it about her own share in the history, beyond the general statement that she was cognisant from the beginning of what was in the minds of the founders of the Society and was herself in the inner circle of workers.

The present paper attempts to redress the balance. Yet it is impossible to draw the line between work done by Mrs Sidgwick alone and that done in co-operation with others—especially, of course, with her husband. Though their capacities and temperaments were in many respects dissimilar, the comradeship between them was so profound and far-reaching that I venture to quote below some passages from his writings—for he was always readier of expression than she—to throw light on her attitude and her doings.

Henry Sidgwick, her elder by seven years, had naturally been interested in the subject the earlier of the two. In his first Presidential Address in 1882 he says that his interest dated back for nearly twenty years—*i.e.* to the time when he was a young man of about twenty-four, already engrossed in the problems of religion and philosophy which dominated his life, and already perceiving the intimate bearing of psychical research on them.

Physical Phenomena of Spiritualism

Some ten years later, the experiments of Crookes with D. D. Home, though generally ignored by men of science, roused keen interest in a few who were sufficiently open-minded to recognise in them a *prima facie* case for investigation; and Sidgwick with several of his friends had sittings with mediums who were reported to possess telekinetic powers. The then popular estimate of the subject is shown in a letter he wrote to Myers after some experiments at Newcastle in which Hensleigh Wedgwood (cousin and brother-in-law of Charles Darwin) took part: "Wedgwood is sincerely concerned about our proposed séances at Cambridge. He thinks the Master [of Trinity] would be sustained by public opinion if he dismissed me! So there is yet a chance of one's posing as Galileo."

Sittings were also held in London, in the houses of Sidgwick's friend and former pupil A. J. Balfour, and of Lord Rayleigh. Miss Balfour (who married Henry Sidgwick in 1876) was then keeping house for her brother, and so came to join the circle in 1874, and showed at once her aptitude for the investigation. She brought to it the indefatigable patience and perseverance that such work requires; she was a keen observer and ingenious in devising simple and effective tests. She learnt much about conjuring, and was an adept in detecting tricks and codes. (When in later life she read detective stories, she lost interest in them at an early stage, because she generally guessed the solution of the mystery at the first hints given.)

In her paper, written in 1886, on "The Physical Phenomena of Spiritualism" (*Proc.*, vol. iv, pp. 46 ff.) she says that she well remembers her first sitting in 1874, "on account of the interest excited by the idea that, notwithstanding the very inconclusive character of the phenomena that occurred, we might possibly be communicating with beings belonging to another world." The possibilities of fraud, illusion and mal-observation are discussed; but in spite of much definite proof of trickery and little evidence for anything genuine having been obtained, she ends characteristically:

"It is not because I disbelieve in the physical phenomena of spiritualism, but because I at present think it more probable than not that such things occasionally occur that I am interested in estimating the evidence for them. I feel bound, however, to admit that by far the larger part of the testimony put forward as affording solid ground for a belief in them which I have been able to examine is of such a nature as to justify the contempt with which scientific men generally regard it. . . . If what I have written should contribute, in however small a degree, to the improvement of the evidence on this subject in the future, I shall feel that it has not been written in vain."

The open-mindedness of this conclusion did not save the paper from vehement attacks in the spiritualistic papers, and there was much discussion, in which both the Sidgwicks took part, in vols. ii and iii of the *Journal*.

At about this time the classical researches of Hodgson and Davey into the possibilities of mal-observation and lapse of memory were being carried on. In particular they showed that continuous observation was a practical impossibility, so that evidence which depended solely on watching the medium was always unreliable.

Davey was an amateur conjurer of extraordinary skill in imitating so-called "spiritistic" phenomena, such as "slate-writing," supposed to be produced on the inner surfaces of two slates fastened together. Unfortunately, he died of typhoid fever at the age of twenty-seven, in December 1890. Mrs Sidgwick was thoroughly conversant with all this work (for which see *Proc.*, vols. iv and viii), and often referred to it in her writings.

The Question of Fraudulent Mediums

One question in dispute with the spiritualists was whether further experiments should be tried with mediums who had been convicted of fraud. On this point Henry Sidgwick took a strong line. He says (*Proc.*, vol. iv, p. 101):

> "If it had occurred to me when I addressed the Society four years ago that we should be seriously urged to investigate the performances of 'mediums' whose trickery was proved and admitted, I should certainly have repudiated the suggestion with all the emphasis that I could command. But I then believed—and ventured to say—that spiritualists had been impressed by the 'evidence accumulated in recent years to show that at least a great part of the extraordinary phenomena referred to spiritual agency in England and America are really due to trickery and fraud of some kind.' I hoped, therefore, that educated spiritualists would generally agree with me in condemning what I called 'the obstinacy with which mediums against whom fraud has been proved have been afterwards defended,' and in regretting that such persons should, as I said, 'have been able to go on with their trade after exposure no less than before.' I never thought that we should be called upon to give direct encouragement to this trade by undertaking a formal investigation of the 'phenomena' exhibited by such persons."

(*Op. cit.*, p. 105):

> "I should certainly not [put forward results obtained under satisfactory conditions] as evidence if I knew the supposed medium to be a detected impostor. Nor should I seek evidence from such tainted sources—not because I hold that evidence involving tricksters *cannot* be raised to a pitch that would exclude explanation by trickery, except on the supposition of the investigator's idiocy; but because an extended experience has led me to regard the chance of its being so raised as too slight to counterbalance the palpable evil of encouraging an immoral trade."

Sidgwick's arguments on this point convinced his colleagues, and the Council agreed that the money of the Society should not be spent either on investigations of mediums who had once been clearly detected in fraud, or in printing reports of such investigations. This principle was adhered to throughout Sidgwick's life-time, and was adopted by Myers in selecting his material for *Human Personality*.

I lay stress on the point, because I think the attitude of the Sidgwicks on it has often been misunderstood. They never maintained that because a medium had cheated once, none of his performances could be genuine. But they condemned the tacit encouragement given by the majority of spiritualists at that time to fraudulent mediums, who knew that no exposure would prevent their continuing to drive a profitable trade; and the consequent discouragement of honest amateurs, who might object to be classed in the same category with the disreputable professional.

Foundation of the Society

To resume the chronological sequence: their repeated failures to obtain positive results from "physical" mediums had somewhat discouraged the investigators, when their hopes were revived by the successful experiments in thought-transference carried out by Barrett, with the help of Gurney, Myers and others, in 1881 and 1882, which led to the foundation of the S.P.R. in 1882. This was an event of the greatest interest to the Sidgwicks and they did their utmost for the success of the new venture. Not only did they contribute generously to its funds at different periods, both for special and for general purposes; the time and labour they gave to it were unstinted. Henry Sidgwick was elected the first President; every attempt was made to enlist supporters of all shades of opinion, and there were several leading spiritualists on the first Council—*e.g.* C. C. Massey, Stainton Moses, Dawson Rogers and Hensleigh Wedgwood. Though these members dropped off one by one, on account of the stringency of evidence demanded by the majority of their colleagues, Massey and Wedgwood, who were personal friends of the Sidgwicks, remained on terms of intimacy with them.

The study of Telepathy now became the principal, though by no means the sole, work of the Society, and we know that it is in this field—using the word Telepathy in its widest connotation—that our most important positive results have been obtained.

Phantasms of the Living

This book embodied the results of the first few years' work; begun in 1883, it was published in two bulky volumes in 1886 under the names of Myers, Gurney and Podmore. Both the Sidgwicks took a large share in the work; they corresponded with informants, interviewed witnesses, and were consulted at every stage by the authors. Mrs Sidgwick also spent much time and labour over the proof-reading.

The book deals exhaustively with the subject of Hallucination, distinguishing between the subjective type, which originates in the mind of the percipient, and the veridical type, containing an element due to that influence from without, operating otherwise than through the known sense channels, for which Myers had coined the word Telepathy. A Veridical Hallucination is deceptive in that it suggests the presence of a material object where it is not, but truth-telling in being coincident with a real event connected with the appearance, such as an apparition seen at the time of death of the person represented.

For the first time canons of evidence were formulated, applicable to a miscellaneous field of unusual phenomena, hitherto the happy hunting ground of cranks. The writers set forth the principles on which reports should be accepted or rejected. Included is an interesting "Note on Witchcraft" by Gurney, which forms a complete treatise on the subject, showing that some narratives are based on mere hearsay and others on substantial evidence, the latter being generally explicable by known causes, such as hysteria or suggestion.

Phantasms of the Dead

The first important paper in the *Proceedings* published under Mrs Sidgwick's own name was in vol. iii (1885) on "Phantasms of the Dead." These are defined as "all kinds of impressions on human minds which there seems any reason to refer to the action, in some way or other, of deceased persons." The material consisted of about 370 narratives—chosen out of a much larger number—mostly of apparitions, but excluding apparitions seen at the moment of death, or a few hours after, since these were classed with "Phantasms of the Living," as possibly due to telepathy, instantaneous or deferred, from a living person.

Mrs Sidgwick gives reasons for holding that most of the cases afforded little or no evidence for the action of the dead. Enquirers are probably more aware now than they were then of what constitutes good evidence; yet much of the criticism then put forward might still be found useful. The rest of the cases, though they did not suggest any satisfactory theory about communication with the dead and were far from proving its possibility, yet seemed to merit serious consideration. She observes that no single case, however remarkable, could prove conclusively the agency of the dead; only the cumulative effect of much good evidence could justify belief in it.

Report on Theosophy

In the same vol. (iii, pp. 382 ff.) is the "Report of the Committee on Theosophical Phenomena," of which Mrs Sidgwick was a member. The Theosophical Society was founded in New York in 1875 by Colonel Olcott and Mme Blavatsky. Mme Blavatsky came to Cambridge in 1884, was present at a meeting of the Cambridge Branch of the S.P.R. and produced a favourable impression on the leading members. She maintained the existence in Thibet of a Brotherhood with occult powers, of whom she herself was a Chela or disciple. These Brothers (Adepts or Mahatmas) were alleged to be able to cause "astral forms" of themselves to appear at a distance and to transport material objects, especially letters, supernormally wherever they chose. Hodgson was sent out to the headquarters of the Society in India in November 1884 to collect and investigate evidence for its claims, and returned in April 1885. The Report of the Committee was based to a large extent, but not exclusively, on his investigations, which resulted in the exposure of Mme Blavatsky.

Some of the evidence for fraud was furnished by Mme Coulomb, a former confederate of Mme Blavatsky's, who turned informer; but her statements were checked and a large number of other witnesses were examined by Hodgson. He investigated the "Occult Room" where "phenomena" occurred, and found there confirmatory signs of the apparatus that had been described to him, and which had been dismantled and destroyed before his visit. It consisted of a "Shrine" placed against a wall, on the other side of which was Mme Blavatsky's bedroom. The shrine had a sliding panel at the back, opening into a recess in the wall, which communicated with the bedroom through another opening, and presumably the objects found in the shrine were passed through this channel.

It also appeared that the handwriting of the occult letters had a striking resemblance to the undoubted handwriting of Mme Blavatsky. Mrs Sidgwick made a minute analysis of the resemblances, and a handwriting expert to whom the documents were submitted agreed with her opinion that Mme Blavatsky had forged the letters. The resemblances are shown in facsimiles given in the Report.

"*Spiritualism*" *in the Encyclopædia Britannica*

At about this time (1885-86) Mrs Sidgwick wrote a concise but comprehensive history of Spiritualism for the Ninth Edition (1875-1889) of the *Encyclopædia Britannica*, the volume containing her article being published in 1887. The article deals with what is called "Modern Spiritualism," the movement which started in America in 1848 with the "spirit rappings" of the Fox family by which communications from the dead were supposed to be received. The movement spread from America over many European countries; Mrs Hayden brought "Table-turning" from Boston in 1852 and D. D. Home came in 1855. Trance-speaking, both in private and public, became very common, and sometimes information unknown to the medium was given, which might be due to the action of other minds on his. But as the trance-speakers were supposed to be inspired to utter religious truths, spiritualism became a religious movement. This development became marked first at Keighley in Yorkshire in 1855, being especially prevalent among miners in the north. By 1885 there were two weekly newspapers, *Light* and *The Medium and Daybreak*, which advertised weekly meetings.

The phenomena were of two classes: (*a*) the physical, such as raps, movements of objects without contact, lights, materialisations, "direct" writing, and spirit photography; and (*b*) the much commoner automatic type, such as trance-speaking or writing admitted to be performed by the medium's own organism, but alleged to be due to spirits controlling the organism.

In the first class fraud was frequently discovered; thus, it was found that the Fox sisters produced raps with their knee and other joints; and the phenomena rarely occurred under conditions even intended to prevent fraud, while in experiments carried out apparently in a scientific spirit, the records are hardly ever precise enough to show that fraud could not have been present.

Reference is made to the striking parallelism between the proceedings of modern séances and those connected with the later Greek oracles, as described by Myers in his *Essays: Classical* in 1883.

The article gives a very full bibliography of the whole subject, but the S.P.R. is mentioned only once, in a footnote.

Premonitions

Mrs Sidgwick's paper on "Premonitions" in vol. v (1888) was based on cases received up to that date. Though mainly negative in its conclusions it was useful in showing what explanations had to be eliminated before belief in a premonitory faculty could be accepted, thus paving the way for future work.

Sidgwick commented on it in his journal (June 1, 1888) : [1]

"S.P.R. meeting and Nora's paper on Premonitions. Paper difficult to write because she does not believe in them, and yet we fear that too negative an attitude would prevent our getting the full supply of fresh stories which we want to complete our *telepathic* evidence, the simple minds of our audience not distinguishing between telepathy and premonitions. I thought she succeeded tolerably well, but Gurney thought she erred on the side of too great indulgence to weak evidence."

Henry Sidgwick's attitude as a Philosopher to Psychical Research

I insert here some passages illustrating Sidgwick's general attitude, as a philosopher, to the work :

> (Written in 1886) : [2] "I think [the S.P.R.] has done good work, as I do not doubt that thought-transference is genuine and hope it will soon be established beyond cavil ; but I see no prospect of making way in the far more interesting investigation of spiritualism. I fear our experience shows that evidence available for scientific purposes is not likely to be forthcoming ; still, having put our hands to plough this bog, it would be feeble to look back so soon."
>
> (Written in 1887) : [3] "I have been facing the fact that I am drifting slowly to the conclusion . . . that we have not, and are never likely to have, empirical evidence of the existence of the individual after death. Soon therefore it will probably be my duty as a reasonable being—and especially as a professional philosopher—to consider on what basis the human individual ought to construct his life under these circumstances. Some fifteen years ago, when I was writing my book on Ethics, I was inclined to hold with Kant that we must *postulate* the continued existence of the

[1] *Memoir of Henry Sidgwick*, p. 489.

[2] *Memoir*, p. 435.

[3] *Op. cit.*, pp. 466-467.

soul, in order to effect that harmony of Duty with Happiness which seemed to me indispensable to rational moral life. At any rate, I thought I might *provisionally* postulate it, while setting out on the serious search for empirical evidence. If I decide that this search is a failure, shall I finally and decisively make this postulate? Can I consistently with my whole view of truth and the method of its attainment? And, if I answer 'no' to each of these questions, have I any ethical system at all? And if not, can I continue to be Professor and absorb myself in the mere erudition of the subject? . . . I have mixed up the personal and general questions because every speculation of this kind ends, with me, in a practical problem: 'What is to be done here and now?' That is a question which I must answer; whereas as to the riddle of the Universe—I never had the presumption to hope that its solution was reserved for *me*, though I had to try."

A little later (*op. cit.*, p. 468) he speaks of:

"my final despair of obtaining—I mean *my* obtaining, for I do not yet despair as regards the human race—any adequate rational ground for believing in the immortality of the soul."

He discusses the question more at length, and concludes (*op. cit.*, p. 473):

"It is premature to despair, and I am quite content to go on seeking while life lasts; that is not the perplexing problem; the question is whether to profess Ethics without a basis."

Mrs Sidgwick's Editorship

Not long after these words were written, the sudden death of Gurney, the colleague with whom the Sidgwicks had most in common, laid fresh burdens on the survivors who had valued his work so highly and wished to make up as far as possible to the Society for his loss.

Among other departments, this affected the editing. No regular Editor was appointed when the Society was founded, the necessary work being done by the Literary Committee. The *Journal* (privately printed for circulation among Members and Associates) was started at the beginning of 1884 and Barrett was elected its Editor. After a year, he gave up the work and Sidgwick consented to take it. In the following year (1886) Gurney took it over. When Gurney died in 1888, Sidgwick was made Editor of both *Proceedings* and *Journal*. This meant that Mrs Sidgwick did practically all the work of editing, —of course in consultation with him—while articles in the *Proceedings* were, before publication, submitted to the Committee of Reference. From about 1890, when I became her secretary, I acted as her assistant till July 1897, when Hodgson was made Editor. In June 1899 he resigned and I was appointed in his place, with the great advantage of having been trained by Mrs Sidgwick in the work.

She was scrupulous in her treatment of documents printed in the *Journal*, however unimportant, showing precisely how far they were printed *verbatim* and where editorial modifications were introduced. Connecting sentences, if written on the MS., would be erased afterwards, and the papers put together and docketted. The collation of proofs with originals was gone through thoroughly at least once, and often twice ; and the proofs were generally read at least three times. She also had to select matter for printing in the *Journal* and arrange what varieties of type should be used, and made herself responsible for the proof-reading of the *Proceedings*.

The Brighton Experiments

Mrs Sidgwick's next important piece of work was the series of experiments in Thought-transference carried out during the four months July-October 1889 at Brighton with four different percipients in the hypnotic state, Mr G. A. Smith, the hypnotist, being the agent.

G. A. Smith was a young man with a great natural skill in hypnotism, who used to give public entertainments at Brighton, of a kind common in those days, showing the effects of verbal suggestion. Gurney having witnessed these secured his help for his own experiments. He gradually became Gurney's assistant and secretary in psychical work and made himself very useful and efficient. He found the subjects for experiment ; but though the Sidgwicks believed him to be quite trustworthy, they exercised the same vigilance that they would have done in any case, since it is of course unconscious indications, either given or received, that must especially be guarded against in such experiments. The report of them was published in *Proceedings*, vol. vi, pp. 128 ff. It describes in detail the precautions taken to guard against many possible sources of error and shows that the success obtained was considerably beyond chance.

A second series of these experiments was carried out at Brighton from January 1890-July 1891, and the report published in *Proceedings*, vol. viii, pp. 536 ff. They included the thought-transference of numbers and of mental pictures and the production of local anæsthesia by mental suggestion, a kind of experiment that had been initiated by Gurney. As her assistant in this series, I was impressed by her unwearying patience through a long sequence of tedious experiments; she never seemed to relax her efforts, and never seemed bored. She treated all the persons concerned with the utmost consideration—one might almost say respect—as if they were human beings, not mere subjects for experiment, and they all became much attached to her and liked to talk to her in the intervals about their own affairs. I remarked how different was the atmosphere on a few occasions when one or other of her helpers was in charge. Though the subjects knew that careful precautions were being taken against fraud, I believe that none of them ever resented it.

Clairvoyance

In 1891 (vol. vii) appeared a paper on "The Evidence for Clairvoyance," defined as "a faculty of acquiring supernormally, but not by reading the minds of persons present, a knowledge of facts such as we normally acquire by the use of our senses." The term is not limited "to a knowledge of present facts. A similar knowledge of past and, if necessary, future facts may be included." "The subject . . . divides itself into two main parts—clairvoyant knowledge of facts which are known to some one somewhere . . . and clairvoyant knowledge of facts which are unknown to any one in the world, such as a number drawn at random from a bag and not looked at."

The paper deals only with cases of the first type, which it is of course difficult to distinguish from telepathy, and indeed it is suggested that clairvoyance may be facilitated if it is led up to by telepathy, and that actually it may not be possible to draw the line between it and telepathy. Of the second type of cases, there was then little or no good evidence (though successful dowsing might probably be explained as due to clairvoyance, in so far as it cannot be attributed to the influence of the dowser's subliminal perception of surface indications of underground water on the movements of the rod).

Spirit Photography

A paper on "Spirit Photography: a Reply to Mr A. R. Wallace" appeared in vol. vii, pp. 268 ff. Much of the material used in it had been collected by Mrs Sidgwick in 1885-1886, for her article on "Spiritualism" in the *Encyclopædia Britannica*, though it had not been used in that article. She had not brought it before the Society earlier, because her conclusions were chiefly negative. But in 1891 Wallace had published a paper on "Objective Apparitions," maintaining that Spirit photographs afforded the most complete and crucial test as to the subjectivity or objectivity of apparitions and that the S.P.R. had completely ignored the evidence for it, which he thought superior to any that they had collected for any supernormal phenomena. In reply to this challenge, Mrs Sidgwick published her examination of the evidence dealing with the most famous spirit photographers of the period, most of them being professionals, who made money out of it, such as Mumler in America, Hudson and Parkes in London, and Buguet in Paris. Of these, Mumler, Hudson and Buguet had been clearly convicted of fraud. Parkes produced photographs of very suspicious appearance, but gave investigators little opportunity of examining the methods by which he produced them, so that there was no evidence of their genuineness. She points out that the mere fact that trickery has not been discovered is not enough to establish genuineness; also that the evidence of recognition, on which spiritualists chiefly relied, is very dubious. She quotes from Stainton Moses, himself a convinced spiritualist (*op. cit.*, p. 277): "Some people would recognise anything. A broom and a sheet are quite sufficient to make up a grandmother for some wild enthusiasts. . . . I have had pictures that might be anything in this or any other world sent to me and gravely claimed as recognised portraits; palpable old women authenticated as 'my spirit brother dead seventeen years, as he would have been if he had,' etc." (From my own experience, I should say that this is no exaggeration.) Various methods of fake photography are described, and tricks of substituting one plate for another.

Principalship of Newnham College

In 1892, on the death of Miss Clough, the first Principal of Newnham College, Mrs Sidgwick was urgently invited to succeed her. She had been for many years on the Council and on a number of Committees and was also Treasurer from 1876 to 1919. She remained for the rest of her life a member of the Council, and never slackened in her devotion to the College. For two years, 1880-1882, she had been Vice-Principal, when she and her husband had lived in limited quarters in Newnham and now again they consented to give up their own home for the sake of the College, which owed its existence mainly to Henry Sidgwick.

The reasons which led to their consent are best given in his words (*Memoir*, p. 514):

> "We have been engaged in anxiously deliberating whether to do what the Newnham College Council unanimously wish and

agree that Nora shall take the office of Principal of Newnham College. The die is cast and she has written to accept. . . . It was difficult to refuse; and if I am—as I still am—doubtful whether she has done right in accepting, it is almost solely on account of the S.P.R. . . . We could not resist the unanimous wish of the Council, and of course it is a great pleasure to us (while at the same time it increases the sense of responsibility and difficult duty of 'coming up to the mark') to find that the staff and the students are pleased. What *we* feel most strongly is that after Miss Clough's death it is the duty of all who have given their minds to Newnham to 'close ranks' and take the place that others, moved by the same interest, assign to one. We hope it will be for the good of the College."

(*op. cit.*, p. 522):

"(May 2, 1892) On Saturday Nora was made Principal of Newnham, and to-day she is dining for the first time as Principal at the Hall called by her name. I am doubtful whether she did right in accepting, but only for a reason which does not occur to any of the friends and relations who write about it. I fear that she may not find time for the work of the S.P.R., for which I think her uniquely fit—much more fit than I am. If it turns out that she must sacrifice some of this work, I shall have to take her place; but my intellect will be an inferior substitute for this work, and I shall give up with reluctance the plans of literary work for which I am better fitted. Still, if it must be so, I shall give them up without hesitation, just as I should give them up to fight for my country if it was invaded."

I may add that when at this juncture I expressed to Mrs Sidgwick, in spite of my own affection for Newnham, my fear that the extra administrative work involved must interfere with her work for the S.P.R., she listened attentively to all I had to say, and at last merely replied, "Well, if you ever find me getting slack about the S.P.R., you must pull me up." She was actually then in the middle of one of the heaviest tasks she had ever undertaken for the S.P.R., viz. the Census of Hallucinations, which, begun in 1889, was published in 1894. Her remarkable powers of concentration helped her to carry on these two departments of work—for the College and for the S.P.R.—concurrently. Very few of those associated with her in either had any adequate realisation—many, I think, were entirely ignorant—of her interest in the other. This was partly due to her constant tendency to talk to people about their affairs, not her own;

and her habit of appearing at liberty to attend to anything brought before her.

Census of Hallucinations

The object of the Census of Hallucinations was to discover the frequency of hallucinations experienced by sane and healthy persons in a waking state, and the proportion of them occurring at a time when some marked event was happening to the person represented, *e.g.* an apparition seen, or a voice heard, at the time of death, with a view to finding whether such coincidences occurred more often than they might have done by chance. Thus it was attempted to apply to spontaneous telepathic cases the same principle as had been applied to experiments in telepathy.

The method employed was to ask as many people as possible whether they had ever had a hallucination, getting details from those who answered in the affirmative, and to tabulate the results. Gurney had already attempted such an enquiry, but had only got answers from about 5,700 persons, which he thought too small a number for the purpose.

In 1889 a committee was appointed by the Council of the S.P.R. to start a fresh enquiry on a larger scale. The undertaking was approved by the International Congress of Experimental Psychology held in Paris, which entrusted the enquiry to Sidgwick's direction. The interest taken in it by the Congress led the committee to enlarge its scope, so as to include not only the points of special interest to the S.P.R., viz. the further testing of the hypothesis of telepathy, but also points of interest to psychologists generally, who might be studying the phenomena of hallucination. (The great value for psychological study of the material in the Report on the Census was emphasised in a paper by Mr Piddington, published about twelve years later, in *Proceedings*, vol. xix, pp. 267 ff.)

The committee consisted of six persons, the Sidgwicks, F. W. H. and Dr A. T. Myers, Podmore and myself. The Report, which occupies almost the whole of *Proceedings*, vol. x, was written chiefly by Mrs Sidgwick, who also worked out the statistical calculations, while Sidgwick acted as Chairman of the Committee. There were frequent meetings of the whole committee, and the other members worked at collecting information and enquiring into the cases received, both through correspondence and interviewing witnesses. Answers were received from 17,000 persons, and many interesting and well-authenticated cases of coincidental hallucinations were reported.

The calculation as to whether these coincidences were due to chance was based on the average annual death-rate for England and Wales for the ten years 1881-1890, which was 19·15 per thousand. This gives the probability that any one person taken at random will die on a given day—for this purpose taken as the day on which his apparition is seen and recognised—as about 1 in 19,000. We should then expect that out of 19,000 apparitions of living persons, one would occur on the day of death of the person seen.

Now, in order to make sure that we are not exaggerating the case against explanation by chance, it is necessary to weight it in favour of chance: viz. by making as much as possible of the total number of apparitions and as little as possible of the coincidental ones. In particular, we have to allow for a large number of the apparitions seen having been forgotten and therefore not reported, and for the coincidences having been exaggerated.

It was calculated, then, that the number of apparitions reported should be multiplied by 4, making them 1,300, and that only the best-authenticated cases of death coincidences should be reckoned, which reduced them to thirty. This gives thirty coincidences out of 1,300 cases, or about 1 in 43, as compared with the 1 in 19,000 that might have been produced by chance, and 1 in 43 is equivalent to about 440 in 19,000 or 440 times the most probable number.

This is, of course, a very brief summary of the argument, which is given at length with detailed reasoning as to all steps of it in the Report, of which I quote the final paragraph (vol. x, p. 394):

"Between deaths and apparitions of the dying person a connexion exists which is not due to chance alone. This we hold as a proved fact. The discussion of its full implications cannot be attempted in this paper;—nor, perhaps, exhausted in this age."

Eusapia Paladino

The next important investigation—that of the celebrated Neapolitan medium, Eusapia Paladino—was less satisfactory in its upshot. It was alleged (*Proceedings*, vol. ix, p. 219) that Eusapia had never been detected in fraud, and therefore appeared a suitable subject for investigation. Later it transpired that at least highly suspicious circumstances had been noted by several observers.

In July 1894 sittings had been held in the south of France by Richet, who, with various other eminent scientists, both French and English, including Sir Oliver Lodge and Myers, had been convinced of Eusapia's powers. About a month later, the Sidgwicks attended a further series of six sittings and were impressed, though not convinced, by what they saw. Mrs Sidgwick said (*Journal*, vol. vi,

p. 339) : "As far as they go, my experiences with Eusapia Paladino entirely confirm Professor Lodge, though they do not go so far—for the phenomena I witnessed were never, I think, such as could not have been produced by normal means had her hands alone been free. . . . The evidence, so far as my own experiences go, entirely depends upon whether her hands were efficiently held." Sidgwick concurred in this view.

The report (by Sir O. Lodge and others) of these sittings was severely criticised by Hodgson (*Journal*, vol. vii, pp. 36 ff.) on the ground that the control, *as described*, was insufficient to guard against fraud. Though the sitters were no doubt familiar *theoretically* with most of the tricks described by Hodgson, probably no member of the Society approached him in both theoretical and practical knowledge of conjuring and mediumistic frauds, nor do I think that any have rivalled him since. Every educated person assumes the fallibility of human observation and human memory ; but it is not easy to make full allowance for it till one has discovered by practical experience how far one's own fallibility goes in both respects.

Hodgson's criticisms confirmed the Sidgwicks in their view that further experiments were necessary before coming to a conclusion. They therefore arranged with Myers a series of sittings at Cambridge in August and September 1895. This series began with high hopes. Eusapia stayed for seven weeks at Myers's house, being treated there with the utmost kindness and hospitality, which she exploited to the full. As few of their neighbours could speak Italian, the Sidgwicks went constantly to the house to help in entertaining her.

Efforts were made to induce her to submit to some form of mechanical control more satisfactory than the mere holding of her hands, but she refused all such suggestions. I well remember her rage when it was proposed that she should sit before the open doorway of a room with a net curtain stretched over the doorway, and the objects to be moved put on a table on the other side of the curtain. She would also be constantly calling for "less light" as the sitting proceeded.

Altogether twenty sittings were held. At the first few phenomena occurred which the sitters were unable to explain, but suspicious circumstances began very early to appear, suggesting that Eusapia was getting one hand free by a method that Richet had in fact already guessed. Nevertheless the attempt was continued to get genuine phenomena under unimpeachable conditions rather than to discover fraud.

Among other sitters, the celebrated conjurer Maskelyne was invited, and though he was not, I think, convinced that anything supernormal was occurring, he was not able to make any new suggestion as to how the phenomena were produced. Later Hodgson came from America to assist, and his experience proved conclusively what we had already suspected as to the method by which Eusapia got one hand free by making the other do duty for two, so that the sitters on each side of her were both holding the same hand. She had great muscular strength, which enabled her to force the sitters' hands into the position most suitable for the substitution. This muscular strength often struck me, and I saw her more than once, outside the sittings, lift great weights with no apparent effort.

In the end Mr and Mrs Myers, the Sidgwicks, Hodgson and myself unanimously came to the conclusion that nothing but deliberate trickery had been at work in the Cambridge series of sittings.

While Myers and Sir Oliver Lodge, unlike the Sidgwicks, retained their belief in the earlier series, it was agreed that the records of these should not be treated as part of the evidence put forward by the Society in favour of the genuineness of physical phenomena. The records were therefore printed in the *Journal* only, and not published in the *Proceedings*.

Replies to Criticisms

Next year (1896) an attack was made on the validity of the Brighton experiments by two Danish psychologists, Lehmann and Hansen, who, on the strength of some experiments of their own, maintained that the successes at Brighton could be explained by "involuntary whispering" on the part of the agent, combined with hyperæsthesia in the percipient.

Playing in turn the part of agent and percipient in 500 experiments in the transmission of numbers, they found that each was able to guess many of the numbers through hearing the other's (so-called) "involuntary" whispering. This might have been expected, but the point was that when they guessed wrong, they found that the same "substitutions"—guesses of one number for another—occurred as had occurred at Brighton.

In the first Brighton report (*Proceedings*, vol. vi, p. 164) the possibilities of unconscious whispering and hyperæsthesia had been discussed and the mistakes had been analysed from the point of view of finding whether they could be due to mishearing—the mistaking of one number for another similar to it in sound, such as five for nine. It had been found that mistakes of that kind were not more prevalent than any other mistakes.

Lehmann and Hansen, however, in whose experiments the agent and percipient were much nearer to one another than was customary at Brighton, found that numbers could be transmitted by faint whispering with closed lips, in such a way that a by-stander, in a slightly less favourable position for hearing than the percipient, might neither hear a sound nor see any sign of movement of the speech-organs of the agent.

It was the custom at Brighton for one of the investigators to watch the agent, and another the percipient, in order to guard against signals being given or received—either consciously or unconsciously. The Sidgwicks now made a large number of experiments with G. A. Smith, Mrs Verrall and myself, to see what results could be got by the methods of Lehmann and Hansen.

It was found that faint whispering—more or less audible to the percipient—could be produced with no visible movement of the lips, but not without discernible movements of the neck and throat. Nevertheless, the results confirmed what had always been assumed —that if the case for telepathy rested solely on experiments with agent and percipient in the same room, it could not be considered conclusive.

The Danish psychologists, however, maintained that comparison of their wrong guesses with the Brighton ones provided strong positive evidence that the same mode of transmission was operative in both cases.

Now, among the Brighton experiments there was a series with agent and percipient either in different buildings or separated from one another by at least two closed doors and a passage. In this series the successes were not beyond chance, so that they must be regarded as mere guesses. Yet the "substitutions" in this series corresponded with those in the successful telepathic experiments, at least as much as the latter corresponded with the Danish series. And since in the series of pure guesses there was no transmission at all, the cause of the correspondence in "substitutions" could not be, as had been maintained by the critics, that the same mode of transmission was operative in both cases.

The report of the Sidgwicks' joint examination of the subject was read by Sidgwick at the International Congress of Psychology at Munich in August 1896, and published for simplicity under his name alone in *Proceedings*, vol. xii. Actually the greater part of the paper, and in particular the calculations involved, were the work of Mrs Sidgwick.

Criticism of the Census of Hallucinations

Another notable example of Mrs Sidgwick's method of dealing with criticism is her defence of the Report on the Census of Hallucinations against the strictures of a German psychologist, Edmund Parish, whose pamphlet, *Zur Kritik des Telepathischen Beweis Materiels*, was read before the Psychological Society of Munich in 1897.

Parish argued that many reported cases probably did not occur at all, but were due to illusions of memory; that, if they did occur, the recognition of the figure seen was probably imported into the narrative afterwards by memory adaptation; that the reason why the coincidences of apparition with death appear to be beyond chance is that the coincidence causes dreams to be remembered as waking hallucinations; finally that the content of a hallucination may be due solely to association of ideas, not to telepathy.

Mrs Sidgwick in her review of the book in *Proceedings*, vol. xiii, pp. 589 ff. points out that all these possible defects of memory had been considered in the Report, and allowance made for them; and that the questions of recognition and of whether the percipient was awake or asleep had been carefully examined. She remarks that Parish does not seem to grasp the purely subjective character of the hallucinatory process: that it is self-suggestion, rather than pseudo-memory, that determines many of the details, making hallucinatory figures or objects look as real ones would in similar circumstances. It was constantly urged throughout the Report that the percipient's ideas affected the form of the hallucination—indeed in the very cases which Parish cites as proof of the influence of association of ideas. Even when the fundamental idea—the idea of the person seen—was to be attributed to telepathy, such details as dress, attitude and position in space, were, according to the Report, determined by self-suggestion or association of ideas.

But, whatever influence this may have on the form of the hallucination, it has no bearing on the question whether the coincidences are due to chance or not. For the known existence of one cause of an event cannot exclude the operation of another, unless the first can be shown to be absolutely necessary and sufficient under all conditions.

Furthermore, Parish maintains that, since there cannot be a causal connexion between the death and the hallucination, we must turn the statistical argument round. The Report found that thirty death coincidences occurred among 17,000 informants, while 1 in 19,000 hallucinations is the number that chance would produce. So the real number of apparitions seen must have been $30 \times 19{,}000$, or 570,000. It would follow from this, according to the figures given in the Report, that the informants must have forgotten or ignored more than 98 per cent. of the hallucinations that had occurred to them a fortnight or less before they were asked if they had ever experienced one.

Mrs Sidgwick concludes: "If Herr Parish finds himself able to adopt so extravagant a conclusion, there is no more to be said."

Early Piper Sittings

The chief single subject investigated in the last few years of last century was the trance phenomena of Mrs Piper. She had first been introduced to the S.P.R. by William James, and had come to England in 1889-1890. She had visited Cambridge, and stayed for some time in the houses of Mr and Mrs Myers and Mr and Mrs Sidgwick; and had also visited Sir Oliver Lodge at Liverpool and had sittings with Walter Leaf and others in London. It was a moment of special interest, as the first occasion on which a considerable number of successful sittings had been held with a medium who appeared beyond suspicion and who, as a matter of fact, has never been convicted of deliberate fraud.

Many of the Piper Reports which appeared in vols. xiii and xiv of the *Proceedings* were written in consultation with Mrs Sidgwick; she studied the details, both published and unpublished, with extreme care, and herself attended a number of sittings.

An illuminating "Discussion of the Trance Phenomena of Mrs Piper" was written by her in 1899, and published in *Proceedings*, vol. xv, pp. 16 ff. She accepted the conclusions arrived at by the majority of sitters with Mrs Piper that the supernormal knowledge displayed in her trance was not acquired by fraudulent means, and that it could not as a whole be drawn from the mind of the sitter. Hodgson, who had been the principal investigator, was convinced that communication from the dead through Mrs Piper was practically proved. Mrs Sidgwick thought this too tremendous a conclusion to be based on the study—however prolonged—of a single medium. But she considered the evidence for it sufficient to allow her to adopt it as a working hypothesis. And, granting communication from the dead, she proceeded to argue for Telepathy *versus* Possession—which was Hodgson's theory—as the mode of communication.

It has often been assumed that if telepathy be true, it would be almost impossible to get evidence for communication with the dead. On the other hand, it is the discovery of telepathy that affords the strongest presumption for the possibility. For if telepathy is a spiritual process—a communication between mind and mind in which the physical world is not concerned—it would tend to establish the existence of mind apart from body, with the further probabilities that the mind may continue to exist after the death of the body and continue to be able to communicate with minds in the body; and, indeed, that it is by telepathy that disembodied spirits communicate with one another. Mrs Sidgwick, then, uses the word Telepathy in the sense of any communication between mind and mind otherwise than through the recognised channels of sense, whether the communicating minds be in the body or not. She maintains that the intelligence actually communicating with the sitter by voice or writing is Mrs Piper herself in some condition of dissociation of consciousness, in communication of a supernormal, but partial and uncertain kind, with other minds—of the living and of the dead—and conveying by voice and writing the information thus obtained, mixed up with much that has been acquired or imagined in a normal way.

It is noteworthy that the "controls" exhibit themselves very realistically in bad as well as in good sittings, which would naturally be the case if they were part of Mrs Piper herself. And this hypothesis would account for the frequent failures, absurdities and false statements that are difficult to reconcile with the supposition that the "control" is actually the deceased person that he purports to be.

Some people, as is well known, are "good" sitters, in the sense that they get good results from a medium, and others are "bad" sitters. Mrs Sidgwick remarks that this fact suggests that the sitter is an important factor in the case; that if Mrs Piper got all her information telepathically from him, her failures might be due simply to his being a bad agent, as we find in thought-transference experiments that many people seem to be bad agents. But a good sitter seems in some way to make the process of transmission easier, even when it is difficult to suppose that the information comes from his mind. Hodgson attributed the failure of sitters to such causes as want of attention, or of desire to succeed; and he thought that the attitude of mind of some persons might be actually repellent to the efforts of their deceased friends to communicate. On this Mrs Sidgwick remarks: "I speak as one who has uniformly failed, whether as agent or percipient in thought-transference experiments, or as sitter with Mrs Piper and other mediums, and I am sure that the cause is not want of sympathy or desire to succeed, or belief that success is possible. After years of failure the inevitable absence of hope may have a damping effect, but this will not account for failure when hope is fresh. I think it is certain that the sitter does not serve only as an attraction to his deceased friends, but that . . . there are subliminal qualities in the sitters which [affect the results]. Now this is not, I think, consistent with the hypothesis that the communicator . . . uses Mrs Piper's organism directly [*i.e.* the hypothesis of Possession]; for if so, why should it be more difficult for him to speak or write for A than for B? We require a hypothesis which allows for all three minds—the minds of the deceased friend, of Mrs Piper and of the sitter—being subliminally concerned in the result."

There is positive evidence that Mrs Piper sometimes draws ideas telepathically from the sitter's mind, *e.g.* when mistaken ideas of the sitters are reproduced. There is also some evidence of telepathic communication between dead and living persons in a normal state—though at that date not nearly so strong as the evidence for telepathy between living persons. And it is probable that many telepathic communications do not rise above the subliminal consciousness of the percipient.

It is possible then that Mrs Piper may be in telepathic communication with the sitter, and that the sitter may, without knowing it, be in telepathic relation with his dead friend, and that his function is to be a channel of communication between his friend and Mrs Piper. The latter, on this hypothesis, plays the part of a bad mirror, reflecting very imperfectly the contents of the sitter's subliminal consciousness, coloured and distorted by the contents of her own. Sometimes there would be no evidence of any communication from the dead, only the sitter's own ideas being reflected. Also it would not be impossible that Mrs Piper herself, as well as the sitter, should sometimes receive communications direct from the dead. Also her trance personality might, in the course of a number of sittings, gradually acquire, as part of its own stock of ideas, a definite conception of the deceased person, and as this happened the intermingling of ideas from all three sources might become more and more indistinguishable.

Memoir of Henry Sidgwick

The next year (1900) brought the great tragedy of Henry Sidgwick's illness and death. Mrs Sidgwick, with the courage that never forsook her, carried on meanwhile to the utmost of her power all the responsibilities that she had undertaken, till she was persuaded by her friends at the end of the year to go abroad for a few months' rest. On her return, in addition to her work at Newnham, she devoted herself to completing as far as possible her husband's literary schemes. She brought out new editions of three of his philosophical works, with four volumes of unpublished material left by him. Also, in conjunction with his brother, she wrote a *Memoir* of him and brought out a volume of *Miscellaneous Essays*. For the philosophical work she had the help of specialists, but she read all the proofs herself, and this took up so much time that she was not able to write much for the S.P.R. till after the appearance of the *Memoir* early in 1906. After her husband's death, she was for the first time elected a Member of the Council, and remained on it for the rest of her life.

Automatic Scripts

Mrs Sidgwick took a keen interest in the new developments that occurred almost immediately after the death of Myers in 1901. Hitherto prolonged systematic investigations had been carried on exclusively with professional or semi-professional mediums. But now a band of automatic writers, of whom Mrs Verrall was the first, began themselves to experiment. They were educated and intelligent women, sometimes as able as the investigators to criticise the results obtained, and in any case willing and anxious to have their work rigidly scrutinised and tested, and giving all the help they could to this end.

To those who had been accustomed to deal with professional mediums, who resented criticism and required to be humoured like children, the complete change of atmosphere was an untold relief. One could talk to these automatists with frankness, as man to man, and discuss freely without fear of giving offence any question that turned up, for they were not concerned with their own fame, but simply with the pursuit of scientific truth.

As time went on, "Cross-correspondences" between the scripts of the various automatists began to appear. In a typical Cross-correspondence, there is no exact reproduction in the script of one automatist of a phrase in the other, or even a different expression of the same idea, which might be attributed to telepathy between them. There is a fragmentary utterance in one, which seems to have no particular point or meaning, and another fragmentary and apparently pointless utterance in the other. But when the two are put together, or when a link between them appears in the script of a third automatist, it is seen that they supplement one another and form one coherent idea underlying all the scripts, though only partially expressed in each. This suggests that the idea originates in an intelligence external to the minds of the automatists, which inspires them to utter the fragments. Thus it seemed that the Cross-correspondences furnished the best evidence so far obtained for communication from the dead; but the supposed scheme was not obvious at first, and only through much accumulation of material and prolonged study did it appear. Nor is the evidence of a kind that can be easily summarised, depending as it does on a multitude of details interlocking with one another. Naturally, any single Cross-correspondence can be explained away as due merely to chance; but in course of time the evidence steadily increased and improved, and the case for the theory rests on its cumulative force.

In this work, it was always Mrs Sidgwick's opinion of the value of the evidence that was most eagerly awaited by the investigators. All felt that they had scored a point if they could satisfy her.

Meanwhile work with Mrs Piper was going on, and a number of important cases of multiple personality were published, which afforded further material for the study of automatism.

Obituary of Hodgson

At the end of 1905, the Society suffered another great loss in the death of Richard Hodgson. Mrs Sidgwick's obituary notice of him in *Proceedings*, vol. xix, p. 356, describes his immense zeal, energy and enthusiasm. Hodgson was emphatically a man who had *les défauts de ses qualités*, and while this gave piquancy to his character, it hardly detracted, as she shows, from the value of his work. He had devoted his life whole-heartedly to psychical research. His powers were often applied to necessary but destructive work in exposing trickery. But his experimental investigation in conjunction with S. J. Davey into the possibilities of mal-observation and lapse of memory, mentioned above, has a constructive side, establishing psychological data of great importance for the estimation of evidence, and is of lasting value. Through his study of Mrs Piper, which almost entirely absorbed the last eighteen years of his life, he added largely to the positive evidence for supernormal phenomena. He collected an immense quantity of material about it and published important reports, but unfortunately died, leaving a large mass unpublished.

Reply to Morselli on Eusapia

In vol. xxi (pp. 516 ff.) appears an instructive discussion of Prof. Morselli's *Psicologia e Spiritismo: Impressioni e note critiche sui fenomeni medianici di Eusapia Paladino.* Morselli was specially interested in demonstrating that the spiritualistic hypothesis in this case was absurd ; but while admitting occasional trickery, he maintained that some unknown supernormal force was present. Mrs Sidgwick replied that it is difficult to see how any serious student could suppose that the spiritualistic hypothesis would render the phenomena more intelligible ; but that the more important point is to prove the absence of trickery, not the absence of spiritual agency. She thought that Morselli had not sufficiently attended to this, or allowed enough margin beyond what is normally possible to justify the assumption of supernormality.

Illustrating the case by her own experiences in the Cambridge sittings, she shows point by point the inadequacy of the precautions described by Morselli to guard against fraud, explaining how this was only gradually discovered at Cambridge. She remarks that Morselli seems to think that when fraud has not been discovered by sensible persons responsible for watching the medium, the presumption is that it has not occurred : in fact, that the *onus probandi* is with those who doubt the genuineness of the phenomena. She says : "I have not the same confidence that he has in my own or any one else's powers of continuous observation, especially in darkness or semi-darkness ; and when it has been shown that a medium systematically practises trickery, the presumption, I think, is that on any particular occasion when an unexplained phenomenon takes place, an opportunity for trickery unobserved by the investigator has been found."

Hon. Secretary and President

In 1907 Mrs Sidgwick undertook the office of Hon. Secretary of the S.P.R. which she retained till 1932. During some of these years another member of the Council shared the work, till Mr Salter, her latest colleague, took sole charge of it.

For the two years 1908 and 1909 she was President. She could only be persuaded to give one Presidential Address, which is published in *Proceedings*, vol. xxii, for her forte lay rather in the detailed examination and digestion of a mass of material than in a general survey of a wide miscellaneous field. But whenever she attempted the latter, she had a gift for picking out salient points and treating them with independence, if not with originality.

In this Address she sketches briefly the departments of work envisaged in the original syllabus of the Society and the progress made in each. Of one, clairvoyance, she says (*cf.* above, p. 64) that successful dowsing seems to afford the only evidence so far obtained for pure clairvoyance or telæsthesia as distinguished from telepathy, the difference between the two faculties being of great theoretic importance.

She goes on to discuss automatism, as the external expression of subliminal mentation, and the great advances made in the understanding of it through the work of Myers and the hypnotic researches of Gurney, in addition to those of medical men at home and abroad, many of whom were actively interested in the S.P.R. In regard to telepathy, she urges the constant need for fresh evidence both to confirm what we already have, and to help in discovering, if possible, the laws that govern it.

She remarks that there is one department in which no real progress has been made—the physical phenomena of spiritualism, or telekinesis. This seems to stand where it did twenty-six years ago, when the Society was founded. The phenomena are still swamped in fraud, and still occur for the most part in the presence of professional mediums who are sooner or later detected in trickery. On the other hand, impressive evidence is still occasionally forthcoming. The work in this field, however, has not been entirely barren, for we have a much more definite experimental knowledge of the possibilities of mal-observation than we had. And our greatly extended knowledge of motor automatism has shown that the possibility of complicated subconscious muscular action has to be reckoned with even more than was thought.

She urges strongly that in order to further the investigation, fraud should be more seriously discouraged. If telekinesis is genuine, we should probably know more about it if it had not been mixed up with fraud. The reason for fraud is that it pays so well, for the medium generally runs no risk, even of loss of credit, through discovery : he is as much in demand after exposure as before. If such a person were at once dropped or ignored, the trade would cease to be profitable, and the ground for investigation—if there are genuine phenomena to be investigated—would be considerably cleared. It is also to be noted that while, in other branches of the subject, the hope of the founders of the Society that private mediums would come forward and give their help in investigation has been fulfilled and progress has been largely due to this fact,—in the case of telekinesis no disinterested persons have come forward apparently possessing this power, and taking enough scientific interest in it to carry them through the tedium of careful experiment.

Mrs Sidgwick next mentions the importance of the study of hallucination in relation to its possible bearing on our evidence and quotes a case which had excited much interest among scientific men a few years earlier. A French physicist of repute, while investigating Röntgen rays, thought he had discovered a new kind, which he named N-rays, whose presence was shown by an increase in luminosity of feeble sources of light. Physicists all over the world tried to repeat these experiments, but though a few could see the effects described, most could not. Finally an American physicist, Professor Wood, visited the laboratory of one of the successful seers to test his results. It was said that a hand interposed in the path of the rays intercepted them, but Professor Wood found by a large number of experiments that the effects observed varied, not according to what was really happening, but according to what the observer thought was happening. The difference in sensation was, in fact, a hallucination produced by expectation.

Mrs Sidgwick thought that the possibility of hallucinations, caused by suggestion, at sittings for physical phenomena was greater than is generally realised. We know that hallucinations can be induced in many persons by crystal-gazing, or by hypnotic suggestion, and the N-ray experiments showed that rudimentary hallucinations could occur in a darkened room to persons in a normal state.

She passes on to the most encouraging branch of our investigations,—Telepathy, and reiterates the necessity for getting constantly fresh and better evidence for it. She remarks that any evidence we can obtain of survival after bodily death is likely to throw light on the nature of telepathy and *vice versa.*

Lastly, she refers to the recent advance made through the scripts of Mrs Verrall, Mrs Holland and others; saying that at least we have in them material for extending our knowledge of telepathy, while the form and matter of the cross-correspondences between the scripts of automatists at a distance from one another afford considerable ground for supposing the intervention behind the automatists of another mind independent of them. If this be the mind of a person who has survived death, it would mean that intelligent cooperation between discmbodied minds and our own, in experiments of a new kind intended to prove continued existence, has become possible,—a new and very important stage of the Society's work.

Further Piper Sittings

Mrs Sidgwick had meanwhile been taking part in this new work as a member of the committee for experiments with Mrs Piper, who had been brought to England after Hodgson's death, and gave seventy-four sittings, at nineteen of which Mrs Sidgwick was in charge, from November 1906 to June 1907.

The committee decided that their main objects should be, (*a*) to encourage the development of certain " controls " who had already been manifesting in her trance, purporting to be Sidgwick, Myers and Hodgson, and (*b*) to try to bring about cross-correspondences with the other automatists. The controls continued, but only Myers showed a marked advance in dramatisation and in the *vraisemblance* of the personation. A number of striking cross-correspondences occurred between the scripts of Mrs Piper and those of Mrs and Miss Verrall and Mrs Holland. A full report, drawn up by Mr Piddington, occupies almost the whole of *Proceedings*, vol. xxii, with a short separate article by Mrs Sidgwick at the end.

In the next two volumes occur further studies of the Hodgson control in the English Piper sittings, and of the cross-correspondence experiments carried on in the spring of 1908 between Mrs Piper in America, under the charge of G. P. Dorr, and the automatists in England. The reports of these, by Mrs Sidgwick, Mr Piddington and Mrs Verrall, include a number of successful and instructive cases, which must be read in detail to be appreciated.

Obituary of Podmore

In her obituary notice of Frank Podmore in vol. xxv, Mrs Sidgwick emphasises the great value to the Society of the critical work with which his name had chiefly come to be associated, although he had contributed much to the collection of evidence for telepathy. She observes : " Ignorant criticism we can get plenty of, but when not harmful it is usually quite useless. What it is not easy to find is a man with unflagging energy in keeping his knowledge up to date, unflagging belief in the importance of the investigation, who yet can put himself outside it and view it from an impartial, impersonal and mainly critical standpoint. . . . The Society will be fortunate indeed if it finds another critic equally friendly, learned, painstaking and accurate, to take Mr Podmore's place, and put the brake on when there are signs of running too fast."

Reply to Dr Tanner's " Studies in Spiritism "

In striking contrast to Podmore's methods was a book by two American psychologists, *Studies in Spiritism*, by Dr Amy Tanner, with an Introduction by Professor Stanley Hall, reviewed by Mrs Sidgwick in vol. xxv, pp. 102 ff. On the strength of six sittings with Mrs Piper, carried out in a different spirit from that of the S.P.R., the writers attempted to discredit all the results obtained by the latter. They tried deliberately to mislead the controls, so as to confuse their statements—telling them, in fact, what the Hodgson control justly described as "awful whoppers." They also subjected Mrs Piper, without her consent, to a number of disagreeable and painful experiments to test the degree of anæsthesia in trance, the effects of which lasted after waking and were greatly resented by her.

Mrs Sidgwick was not often moved to anger, but this behaviour, so completely at variance with her own straightforward and considerate treatment of mediums, excited her indignation. She limited herself, nevertheless, in her review to a dispassionate dissection of the misrepresentations and inaccuracies of the account of the English sittings given in the book; finally observing that it may impress persons who derive their knowledge of the evidence discussed from it alone, but that a very different view would be formed by any one who would check Dr Tanner's version by reference to the original sources.

Reply to Dr Maxwell on Cross-Correspondences

The next article to be mentioned deals with a critic of another stamp, Dr Joseph Maxwell (author of *Les Phénomènes Psychiques*), whose paper on "Les Correspondances Croisées et la Méthode Expérimentale" had been published in *Proceedings*, vol. xxvi, pp. 57 ff. Mrs Sidgwick replies to it in the same vol., pp. 375 ff. That Dr Maxwell had studied the reports with care is shown by his animadversions on a number of detailed cases which he quotes, and such discussions must always be instructive. Mrs Sidgwick admits the justice of some of his criticisms, *e.g.* that it would be easier to judge of scripts if they were printed entire, instead of only partially. But she takes exception to some of his views on methods. He thinks that too much dependence is placed on the automatists' good faith and that automatists should not be co-investigators. He imagines that both investigators and automatists start with the desire to prove some particular thesis, and frequently misunderstands what the reports are actually trying to prove. Thus, when they try to find the source of some literary allusion or quotation, he imagines that they give it as evidence of the automatists' supernormal knowledge, whereas it is of course generally evidence of their normal knowledge and the chief object of finding the literary source is to see whether it strengthens or weakens the cross-correspondence.

To the ordinary educated Englishman, his propensity to find in the spiritualist paper *Light* the sources of such phrases as "Crossing the Bar," "seven times seven" or "Pharaoh's daughter" must appear simply grotesque. But, whatever their source, a careful reader can hardly overlook the extent to which both cryptomnesia, on which Dr Maxwell lays such stress, and a margin for subliminal action of all kinds on the part of the automatists has been allowed for by the investigators.

As a general disquisition on cross-correspondences and what they point to, this paper of Mrs Sidgwick's is particularly valuable. She remarks : " Regarded merely as proof of telepathy, their importance can hardly be over-rated, and much more may ultimately be proved by them. . . . I myself think the evidence is pointing towards the conclusion that our former fellow-workers are still working with us."

Psychology of the Piper Trance

Mrs Sidgwick's " Contribution to the Study of the Psychology of Mrs Piper's Trance Phenomena," which occupies the whole (676 pp.) of vol. xxviii (1915) [1] is one of the most important studies of the relation of psychology to psychical research since the publication of Myers's *Human Personality.* It excludes the evidence for Mrs Piper's supernormal powers, of which so much had already been published ; but simply attempts to throw light on the trance consciousness, and the question whether the intelligence that speaks or writes in the trance, and sometimes exhibits supernormal powers, is other than a phase, or centre of consciousness, of Mrs Piper herself.

The question had already been discussed by Mrs Sidgwick in the paper in vol. xv, referred to above. Here it is examined afresh in a broad survey of the whole ground, with a wealth of illustrative detail.

Hodgson had, in a number of sittings, tried to get from the communicators their own theory of the phenomena, and their description of the conditions under which they were working and of the life they live. He had intended to write an account of this, but unfortunately died without even leaving any notes to indicate what his treatment would have been. There is no doubt, however, that he tended to take the statements of the communicators far more at their face value than did Mrs Sidgwick.

If we are ever to approach an understanding of communication between the living and the dead, it must obviously be through a medium who provides good and abundant evidence at least of telepathic power. Mrs Piper was chosen because we have fuller records of her phenomena than of any other medium, and it is more

[1] The synopsis of contents at the beginning of the volume will be found a great help to readers.

enlightening to compare statements made at different times by the same medium than to range over the whole field, picking out points from a number of different automatists.

Mrs Sidgwick begins her discussion with a useful definition of the two words " Control " and " Communicator " which had hitherto been treated as synonymous. She defines " Control " (a word of course common in spiritualistic parlance) as the intelligence which is or professes to be in direct communication with the sitter by voice or writing ; and " Communicator " as the intelligence for which the control professes to act as amanuensis or interpreter, or whose remarks the control repeats to the sitter. The roles of control and communicator may be interchangeable ; but the main idea underlying the conception is that Mrs Piper's spirit is supposed to be temporarily absent, and her body meanwhile occupied by the control. It is asserted that successful working with the medium's organism needs special skill and practice ; hence the necessity for a control intervening between communicator and sitter.

The controls tend to be more or less eminent historical characters, such as J. S. Bach, Mrs Siddons, Sir Walter Scott and Moses (known as " Moses of old " to distinguish him from Stainton Moses). They fail, however, to produce evidence of their identity. The most important group are the " spirit guides " of Stainton Moses. They use the pseudonyms—Imperator, Rector, etc.—which he gave them to conceal their exalted or semi-sacred character, but they never succeeded in revealing their real names. Nevertheless, in some way for which it is difficult to account, they seem to help the phenomena to emerge, and it was often found convenient to allow them ostensibly to take charge of the proceedings. Of course, this may have been simply due to Mrs Piper's belief in their existence.

There are also controls who control for their own dead friends, such as " G. P." (George Pelham) or Hodgson, and these persistently maintain the reality of the others. It was " G. P.'s " statement that convinced Hodgson of this in his life-time, and the Hodgson control carried on the belief. Mrs Sidgwick observes that if the consistency either of " G. P.'s " personation, or of the personation of the spirits he guarantees, breaks down, we are left practically without support for the hypothesis that the controls are independent spirits. But this would not tend to show that there was no real " G. P." in the background, helping to inspire the personation of him.

Much space is devoted to showing the resemblances between Mrs Piper's trance and the hypnotic state as regards changes and dissociations of consciousness. But what psychical processes are involved in these changes we do not know. We may represent it as some sort of shuffling or re-arrangement of centres of consciousness, interconnected but to some extent independent, with one of them dominant enough to keep order, as it were, and secure the kind of stability that is exhibited in the trance.

Mrs Piper's trance seems to be a sort of self-induced hypnosis, in which her hypnotic self personates different characters, either consciously or unconsciously, believing herself to be the person she represents, and sometimes probably in a state of consciousness between the two. In the trance state, as often in hypnosis, her normal powers are superior in some respects to those of her ordinary waking state. And further she can obtain, imperfectly and fragmentarily, telepathic impressions. Or it may be more correct to say that such impressions are not only received by her telepathically, but rise partially or completely into the consciousness of the trance state and so are recognised.

In so elusive a subject, it would be rash to try to come to a conclusion on this case alone. Much more study is wanted of hypnotic and other dissociated states, and of cases of automatic writing, especially of course where there is evidence of supernormal powers like Mrs Piper's.

I may add that Mrs Sidgwick's hope of a similar psychological study of the non-evidential matter in the trance utterances of an automatist through whom excellent evidence has been forthcoming was fulfilled some years later in Lord Balfour's "Study of the Psychological Aspects of Mrs Willett's Mediumship," in vol. xliii. This was the result of many years' observation of the case, with which Mrs Sidgwick also was well acquainted, as Lord Balfour was with the case of Mrs Piper; so that the two reports are in a sense supplementary to one another and form together a most interesting disquisition on what Lord Balfour calls "process,"—the constitution of the Ego and the relationship to one another of different centres of consciousness in the same individual. The two reports together are all the more instructive because of the contrast between the two mediums,—Mrs Willett being a highly intelligent and well-read woman, with a wide experience of life, probably incapable of harbouring in any stratum of consciousness the kind of illiterate nonsense that was not infrequent in Mrs Piper's trance.

A similar but less comprehensive study of the trance phenomena of Mrs Osborne Leonard was made by Mr Drayton Thomas in *Proceedings*, vol. xxxviii.

Abridged Edition of "Phantasms"

In 1918 Mrs Sidgwick brought out a revised and abridged edition of *Phantasms of the Living*, which had long been out of print. The two bulky volumes were now reduced to one. The abridged version omitted the important "Note on Witchcraft" by Gurney, mentioned above, a long "Note on a Suggested Mode of Psychical Interaction" by Myers, the substance of which was later embodied in *Human Personality*, and the Supplement, containing the less well-evidenced cases. Otherwise the book was substantially as the authors had left it, no new cases or new discussions being added.

The original book must always rank as a classic of psychical research, but the general reader may find it convenient to have an abridgment of it, selected by so expert a researcher.

"Book-Tests"

An important new line of research, the so-called "Book-Tests," was the next subject taken up by Mrs Sidgwick. These were obtained through the mediumship of Mrs Osborne Leonard, which was first brought to the notice of the Society by Miss Radclyffe-Hall and Lady Troubridge in 1918, and which stimulated the interest of many members who had not hitherto taken an active part in the work.

A full report of their sittings by Miss Radclyffe-Hall and Lady Troubridge was published in 1919 (*Proceedings*, vol. xxx, pp. 339 ff.). Later Mrs Leonard's control "Feda" gave to these ladies and other S.P.R. sitters correct statements relating to passages in books unseen by the medium; and "An Examination of Book-Tests obtained in sittings with Mrs Leonard" by Mrs Sidgwick was published in 1921 in vol. xxxi, pp. 242 ff.

Two typical instances may be cited in brief:

(*a*) The sitter, Mrs Salter, was alone with the medium, and her father, Dr. Verrall, purported to communicate. "Feda" indicated a certain row of books in Miss Radclyffe-Hall's flat, where Mrs Leonard had never been, and said that in the second book from the left, on the first page, there was a kind of play on Miss Radclyffe-Hall's surname; that the first page was not a full page, but only part.

There was nothing relevant in the second book on the shelf; but if "second book" is taken to mean second from the one previously mentioned by "Feda"—which there is some reason to believe was the communicator's method of counting—this was found to be *Il Salotto della Contessa Maffei*. Only half the first page is covered with print and the word *Salotto* occurs in it six times. The Italian dictionary consulted gives it under *Sala*, thus : " *Sala*. dim : salotto, etc., a *hall* or saloon." Mrs Salter had never been in the flat and knew nothing about the books there.

(*b*) Sitters, Miss Radclyffe-Hall and Lady Troubridge. " Feda " indicated a certain shelf and the fifth book from the left, and said that on p. 14 something gave her a feeling of heat about halfway down the page ; it might be heat like a hot fire, or it might be great anger spoken of as heat. The book indicated had not been read or opened by the sitters. Its pages have thirty-three lines. On l. 16 of p. 14 occur the words " ardent patriot," expressing heat in the metaphorical sense ; on l. 15 of p. 15 occurs the word " bonfire," giving heat in the literal sense. Pp. 14 and 15 face each other, and when the book is closed, the words " ardent " and " bonfire " almost touch.

It is clear that correct statements of this kind cannot be explained by telepathy from the sitter, since the facts are not within his knowledge. It looks more like pure clairvoyance—knowledge of physical appearances outside the range of any one's senses—but if so, is it " Feda " who has this power, or some disembodied spirit who communicates it to her ?

Mrs Sidgwick analysed about 532 cases, obtained from thirty-four sitters, and concluded that 92 could be classified as successes, and 100 approximately so, in all about 36 per cent. But she remarks that any classification is difficult and uncertain, and that the degree of success beyond chance must vary enormously in different cases, so that any calculation of probabilities—such as can be applied to simple experiments in telepathy—is out of the question.

Sometimes details about the surroundings of the book were given, which might be explained by telepathy from the sitter ; but cases where the sitter knew nothing of the surroundings were sometimes equally successful. Another question might be whether the knowledge was possessed by any living person who might be supposed in touch with the sitter or with the medium. Again, was the knowledge shown within the possession of the communicator before his death ? If so, it would be evidence of his surviving memory.

It is only after exhausting these possibilities that we are driven to assume pure clairvoyance. " Feda " ascribes it to clairvoyance exercised by the communicator. There may be three sources of " Feda's " supernormal knowledge—the sitter, other living persons and the memory of the communicator—all working together, or any two of them may work and fortify one another.

Some of the most interesting cases are cited in full and carefully analysed. Clairvoyance is a matter which is at present disconnected with all our established knowledge, and is therefore hard to believe. And it is very hard to estimate whether the successes of the Book-Tests are more than we can suppose chance to have produced. On the whole, however, Mrs Sidgwick thought that they afforded a *prima facie* case for clairvoyance. They also suggest a close connexion between telepathy and clairvoyance,—not a new theory, but one which might lend itself to practical experimentation. *E.g.* if telepathy operates in the finding of the book and the perception of its surroundings, it might facilitate the clairvoyant perception of the inside of the book which sometimes follows.

The paper includes a "Note on Chance in Book-Tests," giving the results of an experiment by Mrs Sidgwick to see whether, in specified places in twelve books taken at random, a passage would occur corresponding to a "Test" given by "Feda". Only one of the twelve was found to have any relevance to it. These experiments, though too few to be conclusive, tend to show that chance is not an easy explanation of "Feda's" successes.

Imitation Book-Tests

In a later volume (xxxiii, pp. 606 ff.) Mrs Sidgwick reports on a series of experiments, in which sixty members of the Society took part, to test chance in book-tests. They were asked to choose ten books at random, and after doing so, were given three "tests" and asked to search for each of the three in a specified part of each book. *E.g.* is there a passage which is particularly relevant to your father in the top quarter of p. 60 in each book? In all, therefore, 1,800 passages were examined, 600 for each "test."

The experimenters recorded the results in detail, stating whether or not they considered the "tests" successful, *i.e.* whether the passages were relevant or not. But naturally different people had different standards of what should count as a success. So another member of the Society, Colonel C. E. Baddeley, undertook to analyse all the results and draw up percentages of success or failure. In order to compare these chance results with the successful Book-Tests obtained through Mrs Leonard, as calculated by Mrs Sidgwick, it was necessary to know how her standard of "success" compared with Colonel Baddeley's. So she also studied all the "sham" ones in the same way. She agreed with him on the whole, but thought he tended to allow things to count too much as success. This, however, would be a fault on the right side in an experiment of this kind, since, in relation to real Book Tests, it would allow chance to account for more than it probably did.

As mentioned above, Mrs Sidgwick calculated that about 36 per cent. of the Leonard Book-Tests were successful. By as nearly as possible the same standard the success of the sham tests was 4·7 per cent. Thus, as far as can be estimated from these experiments, the Leonard results are considerably above what chance would be likely to produce.

New Collection of Phantasms of the Living

In 1922 Mrs Sidgwick made a collection of cases of telepathy which had been printed in the *Journal* only and were therefore not available for the general reader. It is contained in a paper of about 400 pp. in vol. xxxiii, entitled "Phantasms of the Living: An Examination and Analysis of Cases of Telepathy between Living Persons printed in the *Journal* of the Society since the publication of the book *Phantasms of the Living* in 1886."

The period covered is from June 1886 to the end of 1920, and the collection is made from seventeen volumes of the *Journal*, omitting all cases in which the interval between the experience and its record exceeds five years, and those in which the evidence seemed too weak to be worth publishing. A Table of Contents at the end explains the classification adopted and makes it easy to look up any type of case wanted.

It forms a complete treatise on Telepathy, being a continuation to date of the original *Phantasms of the Living* in the light of the research that had been going on since the publication of that book. It appeals once more for the continued collection of new first-hand material, of which there can never be too much, if it is well-evidenced.

Hindrances to Telepathy

A paper by Mrs Sidgwick on "Hindrances and Complications in Telepathic Communication" was published in 1923 (vol. xxxiv, pp. 28 ff.). She points out that the confusion so often apparent in "messages" from the dead is not *by itself* a reason for doubting their validity, since communication from the dead is presumably of the same nature as telepathic communication between the living, which frequently exhibits similar confusion. Cases of telepathy between the living are here examined from the point of view of imperfection of transmission, in order to discover, if possible, what causes the imperfection.

We have no evidence of obstruction outside the two communicating minds, such as we should have if the transmission were physical, like the passage of light. The difficulties, then, must be sought for in the mental processes of the two individuals.

One important difficulty seems to lie in the passage of the idea from one stratum of the mind to another. We get glimpses of double mentation at work in dreams, in automatic writing, in sensory hallucinations, such as crystal visions, in hypnotism, etc. In emerging from a subliminal stratum, an idea or intention may be curtailed or distorted. This can be seen clearly in cases where there is no question of telepathy ; *e.g.* when a hypnotised subject is given a verbal order to do or experience something on waking, such as to see a specified vision in a crystal. Sometimes the vision may be seen but not recognised. For instance, Myers told a hypnotised subject (P.) the story of Robinson Crusoe finding the footprint and fearing savages, and said he would see it in a glass of water. Awakened and set before the glass, P. at once exclaimed, " Why, there's Buffalo Bill ! He's dressed in feathers and skins round him ; almost like a savage. He's walking about in a waste place. I can see something else coming from another part—it's a blackie. Look at them now, how they're arguing ! Buffalo Bill and his black man."

P. failed to see the foot-mark, on which Myers had chiefly dwelt. He had read Robinson Crusoe, but was more familiar with Buffalo Bill, who was at the time touring the country with his show of the Wild West of America.

Here the vision seemed clear as far as it went, with the important omission of the foot-mark, but the message which reached the supraliminal was misinterpreted by it. It would appear that an idea successfully impressed upon one stratum of consciousness is liable to get distorted before it emerges in another. Possibly all telepathic communications first reach the subliminal and have to run the gauntlet of passage from one stratum to another before they can emerge. Even with normal transmissions there may be a want of understanding between two parts of our mind, as in misspellings of familiar words and inability to remember familiar names.

In telepathy between the living, we have of course the advantage of being able to get information about the working of both minds, while in the case of " messages " from the dead, we have no means of knowing what passes on the supposed agent's side, but can only surmise from the contents of the message as received. Even in messages from the living we can rarely discover anything of the mental working of the agent, except in experimental cases ; and of these the most instructive for the present purpose will be the partially successful ones ; for if the idea gets through promptly and completely, no light will have been thrown on the obstacles that may have been overcome. On the other hand total failures cannot reveal what the obstacle is.

Voluntary effort on the part of the agent to transmit some particular idea is not always necessary: the percipient may have impressions corresponding unmistakably to his thoughts, and here the activity seems on the percipient's side. In Mr and Mrs Newnham's experiments, he asked mental questions and her automatic writing made appropriate responses, indicating that she had apprehended the question subliminally. But if the question related to something that only he knew, her answer was only slight and fragmentary; though generally appropriate, it was not correct, but pure invention. This suggested that the subliminal was unwilling to confess its ignorance and tried to invent something plausible, as mediums often do. That this attempt at deception was not the work of some outside spirit seems clear from the facts that the writing intelligence always asserted that it was Mrs Newnham herself, and that no true information unknown to both of them was ever given.

Sometimes when a picture is to be mentally transferred, the percipient may see some items and misinterpret them. Thus, in the Brighton experiments, a sailing boat on the sea appeared as cliffs seen above the boat, then joining the boat, and at last identified as its sails. Again, a snake with its tongue out was seen as something like a snake and a snake charmer playing with it,—an added detail clearly due to association of ideas in the percipient. Since association of ideas plays a large part in normal perception and in the interpretation of perceptions, we might expect the same to occur in supernormal perception.

In fact we often find that telepathic messages, when received, are incomplete; they may be received piecemeal and the percipient may fail to grasp the total idea It is to be supposed that the same kind of failure may occur with messages from the dead.

Analysis of Professor Murray's Experiments

Mrs Sidgwick's examination of telepathy was continued in 1924 in a "Report on further Experiments in Thought Transference carried out by Professor Gilbert Murray" in *Proceedings*, vol. xxxiv, pp. 212 ff. She remarks that these are perhaps the most important experiments ever brought to the notice of the Society, both on account of their frequently brilliant success and on account of the eminence of the experimenter.

They were carried out with Professor Murray's own family, sometimes joined by friends; Professor Murray himself was generally the percipient, and one of his daughters the agent. Though he spoke of his attempts to reproduce the agent's ideas as guesses, no mere guessing could have produced the amount of success obtained. Telepathy or some other agency must have been at work. Professor Murray distinguishes between three things—the impressions that seem to come to him from without, his inferences from these impressions, and his guesses to supplement them. The impressions, which are probably telepathic, differ in intensity and clearness, and the one which is strong and clear and comes promptly is usually, but not always, right. Sometimes a faint and dim and slowly developing impression may be equally correct.

Probably the impression generally comes in a mixed way, partly as an idea not of a sensory kind, and partly as a visual or auditory image. The different avenues used are not always distinguishable by the percipient, and their use probably depends on the make-up of his own mind, whether he is a good visualiser and so forth. Granting that the subliminal mind plays an important part, we see that error may come in at four stages:—the impression may get into the subliminal in any degree of incompleteness; it may there be further distorted by false associations and inferences; loss may occur again in emerging into the supraliminal, through inhibition or otherwise; finally, the supraliminal may reject some images and misinterpret others.

If telepathic impressions come through the subliminal, one important quality in a good telepathic percipient may be the power of drawing easily on the contents of his own subliminal.

These experiments are instructive for comparison with the automatic scripts containing cross-correspondences, because the experimenters, like our automatists, are persons of wide reading and strongly marked literary tastes, so that quotations and literary associations tend to crop up abundantly in their minds.

In 1932, the year of the Society's Jubilee, when Sir Oliver Lodge was President, Mrs Sidgwick was elected by the Council President of Honour. It was then that she wrote the History of the S.P.R., referred to above, and its concluding sentence, that, upon the evidence before her, she herself was " a firm believer both in survival and in the reality of communication between the living and the dead," must be fresh in the memory of all of us.

As long as her strength permitted, she went up to London for S.P.R. meetings and wrote occasional book-reviews and minor articles. She read zealously all the Society's publications, as well as any important new books on psychical subjects. As a member of the Committee of Reference, she studied carefully the proofs of articles for the *Proceedings* and was in constant consultation with the Society's officers and other leading workers.

Her continued openness to new conceptions was remarkable. "If this is so, we may have to revise our ideas," she used to say, and clearly there was no opinion that she would not have revised, if the freshly alleged facts had satisfied her critical mind. There are, one hopes, many people of whom one could say the same, but not, I think, many who, like her, are always looking out eagerly for new facts—whatever they may lead to—whose intellectual curiosity remains insatiable up to the end of so long a life. She died on February 10, 1936, within about a month of her ninety-first birthday.

Yet it must not be supposed that the essence of her nature was pure intellect. She valued human beings far more than ideas, or even than the causes for which she would spend herself. Her own standard seemed above the reach of ordinary mortals, but she had an infinite tolerance and charity for other people's weaknesses, and a most generous appreciation of their capacities and achievements. Her life might perhaps be summed up in the words :

> "Mercy and truth are met together ;
> righteousness and peace have kissed each other."

SUPPLEMENT

[As an Officer of the Society who had been associated with Mrs Sidgwick in its most recent work, Mr Salter undertook to supplement Miss Alice Johnson's paper; and Mr Besterman begged the Editor to include in the Memorial Part a tribute "from the youngest generation."]

I

By W. H. Salter

A word should be said about the very great services Mrs Sidgwick rendered to the Society as Hon. Secretary during the difficult period which followed the War. Several of her most important contributions to *Proceedings* were made during this time, but I shall confine myself to the administrative side of her work.

The emotional after-effects of the War brought into the Society a large number of new members with no knowledge of its past history and traditions, and, in many cases, no deep-seated or enduring desire for research on scientific lines. At the same time new types of phenomena were requiring investigation. It was therefore essential that the direction of the Society's affairs, so far as they depended on any one individual, should be in the hands of one who had the fullest possible knowledge of past events in the Society, and a very firm grip of the essential principles of psychical research, combined with the will and the ability to apply these principles to the new problems that were arising. In the possession of the required qualifications Mrs Sidgwick stood, of course, without a rival, but the resignation by Miss Alice Johnson of the post of Research Officer during the War, and shortly afterwards the resignation by Mr Feilding of the post of joint Hon. Secretary considerably increased the burden of administrative work thrown upon her. If in the circumstances existing she were to continue to hold the post, she would necessarily be immersed in a mass of petty administrative detail, from which any one of her age, with her record of service, and with the pressure of the research work she was doing, might reasonably have claimed to be excused. With characteristic willingness, however, and disregard of her own convenience, she consented to remain in an office which she held and the duties of which she carried out with the greatest thoroughness until within a few years of her death.

Miss Newton, who as Secretary was brought into the closest possible touch with Mrs Sidgwick, has emphasised in conversation with me how difficult were the years that immediately followed the War, and has expressed her opinion that nobody except Mrs Sidgwick could have carried the Society through them with success. This I can readily accept, as it tallies with my own experience as a colleague of Mrs Sidgwick from early in 1921, when I became Hon. Treasurer. It was an immense support to me in my inexperience then, and later when I became joint Hon. Secretary, to be able to rely in any difficulty that might arise on her experience and advice. However small the matter on which she was consulted, her advice was invariably prompt, clear, supported by cogent arguments and patently the product of much careful thought. Whatever storms arose (and for many years after the War the Society encountered rough weather) she steered us through them with imperturbable cheerfulness and confidence.

Perhaps in those years none of Mrs Sidgwick's qualities was of more service to the Society than her absolute candour. She regarded every piece of work objectively; whoever did it, if she had to give her opinion on it, she gave it, kindly of course, but so as to leave no doubt whether she thought the work well or badly done. The impersonality of her judgments was quite compatible, and in fact associated in a high degree, with sympathetic understanding of people whose opinions and outlook differed widely from her own.

Since the War a new generation of researchers has grown up in our Society. There is not, I suppose, one of them who would not readily acknowledge that his warmest encouragement, his soundest advice had come from Mrs Sidgwick, and that she had been a constant inspiration to good work, not only by her long record of achievement, but by her eagerness to collaborate actively in any new and promising research. If any of them ever felt impatience at the caution of her methods, they soon realised that the "guides' pace" she set was a measure of the height of her aims and of her resolve to attain them.

In the beginning of 1932, the Society's Jubilee year, increasing physical infirmity caused her to resign office as Hon. Secretary, but she became President of Honour jointly with Sir Oliver Lodge for that year. Her Jubilee paper was the last of a series of contributions to *Proceedings*, in which the whole history of our Society may be traced. She still remained an active member of the Committee of Reference, and only a few weeks before her death, when the Council's draft Annual Report for 1935 was read to her, she took great interest in it and said she thought it very satisfactory and encouraging.

It is the greatest gratification to us all to know that she, in whose life the Society had played so large a part, should have been able to retain to the last her confidence in its future.

II

By Th. Besterman

I first met Mrs Sidgwick in the autumn or winter of the year 1926. She had already entered her ninth decade, and I looked at this great lady of whom I had read and heard so much across the chasm of nearly sixty years. It was a Committee Meeting at which somewhat uninteresting technical details were being discussed. Mrs Sidgwick said little, but she was very attentive and her gently, but accurately observant eyes studied the speaker. It was clear that she was considering not only the merits of the case, but also the motives and intentions of the proposer. At the end of the meeting Mrs Sidgwick, having said less but thought more than anyone else, quietly put forward a motion which met the situation to everyone's satisfaction.

In the following years this experience was often repeated and this first impression was as often confirmed. Mrs Sidgwick put her faith more in reflection than in superfluous discussion, and character was at least as important to her as ability. This feeling was in a sense a projection of her own personality, for Mrs Sidgwick's greatness was in fact due to the union in her of brilliant ability and outstanding character. These things were so perfectly blended in her that one was often at a loss whether to admire more the accuracy or the justice of her judgment.

The accident of birth and the process of time added to Mrs Sidgwick's mental and moral qualities what was almost as valuable, an exceptional range and depth of experience. Thus it is not surprising that those who had the privilege of knowing her quality took Mrs Sidgwick as their touchstone. Certainly during the years in which I was an official of the Society, I avoided many a hasty decision, many an unwise step, by testing it against what I knew of Mrs Sidgwick's opinions. And when I did act in a manner of which I knew she would not approve, that knowledge was the obstacle most difficult to overcome. This was true, I think, of every one who was capable of appreciating her singular *fineness*, a quality, in my experience, peculiar to her, and one which seemed unaffected by the passage of time.

History will no doubt record some, at least, of the perceptible influence exercised by Mrs Sidgwick on psychical research. Greater even than this was her personal influence on those practitioners of psychical research who have most usefully contributed to this nascent science. If part of the subject matter of psychical research ever attains to the dignity of general recognition, this result will be due to no one individual anywhere so much as to the calm enthusiasm, the steady persistence, the bold but sober judgment, and the intellectual quality and charm of Eleanor Mildred Sidgwick.

// PROCEEDINGS

OF THE

Society for Psychical Research

PART 147

FURTHER RESEARCH IN EXTRA-SENSORY PERCEPTION

By G. N. M. Tyrrell

Introduction

Since the inquiry into modes of perception other than those recognised as "normal" began to develop an experimental side, the tendency has been for it to become more and more a quantitative research. This appears to be due to the necessity for so framing experiments that statements made by the subjects can be easily verified and an estimate made of the probability that such statements, when true, are attributable to chance. Such a method of experiment entails the use of events which are quite trivial, easily controllable and amenable to calculation; and, since an ordinary pack of cards provides such a series of events, playing-cards have been used by several investigators in this field.

The history of the piece of research, which is to be described here, began with some experiments with cards, which were made in the year 1921. In that year the writer had already met Miss G. M. Johnson, who was then a girl, taking no particular interest in scientific research or even in psychic matters, although she possessed in a half unconscious way very remarkable psychic gifts. The experiments resulted from a long stay, which Miss Johnson made with the writer and his family in the country, and an account of them is to be found in the Report which appeared in the *Journal* S.P.R. for June, 1922, under the title *The Case of Miss Nancy Sinclair*. This Report was published anonymously because at the time, Miss Johnson did not wish, for personal reasons, to disclose her identity. The part which is especially relevant to the present issue is the account given on pp. 318-327 of seven experiments with cards, which provide strong evidence for clairvoyance; while three of them have the appearance of being precognitive. These experiments were remarkable because of the many signs which accompanied them, showing the subject's departure from the normal state of consciousness, and the peculiar and characteristic subjective side of them which showed a state as far removed as could be from mere guessing. These card experiments form an early, though brief, attempt to apply the quantitative method to Extra-Sensory Perception, and the figures show that the results cannot possibly have been due to chance.

To summarise these experiments roughly, Miss Johnson on seven occasions gave the denominations, but not the suits, of from five to eight consecutive cards in a pack of ordinary playing-cards. In the four clairvoyant cases, I had shuffled the pack very carefully, unseen by myself or by anyone else, just before she gave them. In the three precognitive cases, I shuffled the pack in the same way, but *after* the cards had been given. On being turned up, the cards proved to be nearly all right, except for some minor errors. Details will be found in the Report.

Taking all these seven experiments together, the probability that they are due to chance is of the order 10^{-36}, that is to say, the odds against their being chance is about a billion, billion, billion to one: and the explanation that Miss Johnson did them by a superlative feat of conjuring (which seems the only alternative to genuine Extra-Sensory Perception) will probably not appeal to many, and is quite out of the question for myself, partly because I know that Miss Johnson cannot conjure in the very simplest way and would not if she could, and partly because I had my eyes fixed on the cards every moment after the shuffling had been done.

Before leaving these early experiments, I should like to call attention to a curious point about the last one (p. 324). If we accept Miss Johnson's word about this experiment, a sequence of eight cards, previously given, turned up in the shuffled pack, not merely once, but *twice* on two consecutive days. It is curious to note this; but the evidence is not strong enough to lay much stress on.

After this, many years elapsed during which circumstances prevented the resumption of experiments between Miss Johnson and myself. Since, as my colleague and the principal percipient in the present experiments, she is to figure largely in the following account, it has been thought suitable that a few words describing her paranormal gift, as it displays itself in everyday life, should be inserted here.

Miss Johnson possesses a well-balanced mind, strong common sense and a sceptical tendency towards the general mass of mediumistic and spiritualistic phenomena. She can produce automatic writing easily at will but no very striking material has ever emerged from it. She can also enter states of trance which tend to maintain their own independence and are largely resistant to suggestion and impervious to attempts at communication by the operator. She has no "control." She possesses also a general faculty which may be called "paragnostic," "intuitive," "extra-sensory-perceptual" or whatever the reader chooses, but which is certainly a "normal" factor in her psychological constitution in the sense that it is constantly present and plays what is, for her, an indispensable rôle in the affairs of daily life. This faculty provides her with a kind of half-realised "knowledge" of things and events which, though seldom clearly present in the field of normal consciousness, is nevertheless powerful enough to determine many of her actions, which it seems to direct from a seat somewhere on the fringe of consciousness. It is of the greatest practical utility and everything seems to lie within its scope, from the making of an important decision to the finding of some trifling object.

It would be useless here to give examples of the practical working of this faculty, interesting though such examples are, for they occur spontaneously and cannot as a rule be clearly attested by independent witnesses or clearly proved to be beyond the range of chance. Their power to convince depends upon their cumulative effect when seen at close range, and still more upon their practical utility. The pragmatic test is probably the most convincing to common sense, but not to the critic who stands aloof. A demonstration of the wide-spread success of a faculty in matters of daily life is apt to call forth the criticism that the detailed items of it are not logically perfect as evidence : while the production of a logically perfect case calls forth the opposite criticism that no single case can be convincing unless it is shown to belong to a wide-spread genus. And so the pendulum swings backwards and forwards between two different conceptions of evidence.

Thus, it will probably be more useful to describe here the subjective aspect of Miss Johnson's faculty, since, if her experiences are reliable, their coincidence with events which seem to be paranormal on other grounds constitutes in itself evidence of a different kind ; and Miss Johnson, being an unemotional subject, may be taken to be reliable in the account which she gives of her feelings. The following extracts from a paper which she read to the S.P.R. on 29th January, 1936, will serve to illustrate this subjective aspect. "The supernormal," she says, " is stamped with a feeling which is like nothing else. This stamp or feeling is all-important. I invariably trust to it in the affairs of daily life and have never known it to fail ; but it is practically impossible to describe it. All I can say is that it has a settled feeling of finality about it. It impels you and simply does not let you doubt it. If one ever tries to go against it the feeling is strongly unsatisfactory ; but if one does as it impels, there is a very satisfactory feeling of accomplishment and rightness." And, with regard to the experience she has of feeling a certain quality about the people she meets : " When you are in a very sensitive state, this is like being exposed to the weather and being, in a sense, bare to the telepathic climate. While it is true that the presence of certain people puts you in the psychic sunshine, it is also true that the presence of other people puts you in the psychic East wind. It is extremely difficult to explain these feelings in words. There are some people who seem to raise me up to a height. As soon as I am in their presence, I have a feeling of enthusiasm and happiness and everything feels what I can only describe as 'light.' There is a feeling of *rightness* about it too—a satisfactory feeling of things being as they should be. And there is more even than that. There is a feeling of being brought into touch with *reality* ; and this 'reality-feeling,' if I may call it so, is the hall-mark of this kind of person ; and also of all knowledge which comes in a genuinely supernormal way. There is even something of this feeling of being in touch with reality in the trivial act of opening the right box in the machine when the success is not due to chance. But nothing which comes to one in the normal way has this 'reality-feeling' about it.

" And this feeling has its opposite. There is an atmosphere which lacks all 'lightness,' from which joy and enthusiasm seem to ebb away and all seems heavy and difficult. One's supernormal faculty will not come into play under these conditions ; it retires into its shell. And so one of the most important conditions, if one wishes to get experimental results, is to encourage the former atmosphere and to avoid the latter."

My experience amply bears this out. The condition which Miss Johnson here describes as " light " is the key to success in experimental work. It is this which really matters more than anything else ; and the investigator who is genuinely searching for truth cannot afford to ignore it. It may be that there are some people who regard such subjective conditions as mere personal " fancies," improper and inimical to the impersonal dignity of scientific research. If so, I am certain that such people will find that they have sacrificed relevant facts of primary importance to conventional ideas of decorum. To ignore such facts is not only to court failure in practice, but also to run the risk of making oneself ridiculous ; for it is an attempt to dictate to nature and to force the facts of psychical research to conform to ideas of what they ought to be, instead of admitting them to be what they are. We cannot pick and choose among the material which nature offers us. If we do, we may find ourselves exemplifying the saying, " Nature abhors a mugwump."

The reference to opening the right box alludes to the experiments which are to be described below. Miss Johnson says that in doing these experiments she can, as a rule, distinguish between those successes which are due to the exercise of her own faculty and those which are due to chance.

It was not until the summer of 1934 that events made it possible for Miss Johnson to rejoin me in starting again on the experimental work which we had begun in 1921 ; and this time we were fortunate in being able to start in London.

During the intervening years, a good deal of work on quantitative lines had been done elsewhere, one of the most notable examples being the experiments carried out by Dr J. B. Rhine at the Duke University in North Carolina and published by him in America in 1934. He had used several subjects and had obtained what seemed, in view of the previous experience of others, astonishingly high rates of success. He had differentiated clairvoyance, as Miss Johnson and I had also done, from a combination of clairvoyance with telepathy.

It is from Dr Rhine that I have adopted the term " Extra-Sensory Perception," using it to cover all modes of cognition which are paranormal ; and for brevity I shall in future refer to it by its initials, E.S.P.

In October 1934 I was particularly struck by a feature in Miss Johnson's faculty, viz. the extreme satisfaction—one might almost say the excitement of satisfaction—which accompanied the finding of any lost object. If one could create such an excitement, even in a mild way, and make the condition of its satisfaction the performance of some experiment on a measurable basis, it seemed that it might be possible to harness it in the work of demonstrating E.S.P. It was this which led me to construct, in the Autumn of 1934, an apparatus in which a pointer, representing the hidden object, was thrust into one of five small boxes from behind, while the subject raised the lid of one of the boxes from in front, trying to find in which box the pointer was hidden. It was easy with this arrangement to observe successes and failures, while the " finding interest " was sustained through series of trials which were carried out with great rapidity.

A description of these experiments and their results was published in the *Journal* S.P.R. for April, 1935.

The experiments now to be described were carried on from the point where this Report comes to an end. They include the extension of Miss Johnson's work to trials with other agents or operators; the tests of other subjects besides herself; some experiments with cards, and the construction and employment of a more elaborately developed machine arising out of the simple box-apparatus worked with the pointer. These experiments are given in the historical order in which they occurred, and the reader must understand that the progress of events was more in the nature of an unplanned adventure than of a previously thought out experiment. The new apparatus was partly improvised as events moved on, and this, combined with the constantly fluctuating rate of scoring, makes it very difficult to present the results in a clear and logical way. I can only hope that the development of events will be sufficiently intelligible in the account that follows.

The Quantitative Method of Experiment

This method received a great impetus when Miss Ina Jephson published her *Report on the Evidence for Clairvoyance in Card-Guessing* in *Proceedings* S.P.R. 109. She has earned the gratitude of all workers in this field by the able way in which she showed that the question of chance can only be dealt with by the systematic use of the theory of probability: and in doing so she strengthened this research at its weakest point, for an unknown chance-factor has, in the past, rendered many results in psychical research nebulous, and has given the scientific mind an excuse for ignoring the subject.

The quantitative principle may be applied to the field of Extra-Sensory Perception in two ways: either by using (1) the Collective method of experiment or (2) the Individual method.

The Collective method aims at flinging a net as widely as possible over all and sundry; in pooling the information thus caught and in sifting it for significance by mathematical analysis. The Individual method, on the other hand, aims at making a close-range study of one, or at most of two or three, specially selected subjects. Both methods, no doubt, have their uses and may be distinguished as belonging to their respective fields.

(1) *The Collective Method.* This appears to be applicable to two purposes: (*a*) as a means for testing the theory that E.S.P. may be a faculty spread widely and evenly amongst mankind, but present in each individual only in a small degree. Then a large number of

trials will be needed to reveal the faculty and these will best be obtained by pooling the results from a number of subjects. (*b*) As a means for testing how the faculty is distributed among mankind, if it should prove not to be equally possessed by all.

(1) (*a*) An important point to notice is that, if the theory of the even spread of the faculty is not true and if there should be a few individuals here and there who possess it in fairly marked degree, while there are hundreds who do not possess it at all, then the Collective method will merely mask the fact of its existence by swamping the scores of those who possess it with the merely chance scores of the majority who do not.

If any individual subject scores well above chance expectation when tested alone, there is no reason whatever for including such a subject in a group. To include a subject, who scores well alone, in a group, may easily lead to an absurdity : for if the group is large enough, and entirely composed of non-scorers except for the one good subject, it may easily happen that the latter's score is not greater than could be accounted for by chance within the large, collective total. The verdict of the collective experiment would then be "probably chance"; but the verdict of the individual experiment would be "certainly not chance." It is clear that the collective verdict in such a case would have to be rejected, for it would be absurd to claim that an experimental fact could be altered by merely placing it in association with a number of other disconnected facts. By including the subject in a number of different arbitrary groups, the verdict could be made to go, now one way and now the other, and the results of an experiment would become fluid instead of fixed. The probability that a George Washington will speak the truth is not affected by thinking of him in connection with any number of liars. Therefore it is not wise to include a subject who scores alone in any kind of mass-experiment.

(1) (*b*) The use of the Collective method for discovering the distribution of the faculty of E.S.P. amongst mankind raises two difficulties : (i) Suppose that a group of subjects show collective significance and only slight individual significances. It will be said that there will be a danger in accepting the latter as genuine unless they are much greater than can reasonably be referred to chance within a block of results of that size. But, if it is illegitimate to pick out the best scorers from among the group as the possessors of E.S.P. faculty, how can the Collective method help to tell us anything about the distribution of the faculty ? Apparently, the highest scorers would have to be abstracted from the group and retested individually with much larger numbers of trials. In this way, also,

good subjects might be discovered: but it would be a laborious way of doing it, and not nearly so practical, one would think, as personal inquiry based on common sense. (ii) The distribution of E.S.P. involves the question: "What percentage of the population possesses the faculty?" Is this question capable of receiving a definite answer? Put in this form, it makes the assumption that the faculty is one which has an objective fixity, like being over six feet high or weighing over twelve stone. But experience points to the faculty as being something dependent on variable mental and physiological conditions in the subject. Although it is no doubt true to say that, on the whole, some people possess the faculty in a more marked degree than others, yet there is no reason to expect that its distribution could be expressed in any satisfactory way as a percentage: and it is difficult to see why importance should be attached to discovering such a figure. What we are concerned to know is the existence of the faculty and something about its nature; and whether 10% of the population possess it in workable degree or 90% does not seem to be of first importance.

(2) *The Individual Method.* This, on the other hand, possesses great advantages over the Collective. In the first place, the investigator, being in touch with the subject, is able to *foster* the faculty he wishes to observe and is not obliged merely to accept the material that chance throws in his way. With the Collective method, he must take what comes to him and make the best of it, and unless he is extremely fortunate, his material will probably consist of "low-grade ore." Indeed, the very usefulness of the Collective method depends on there being a widespread mass of low-grade ore to deal with. But this means that positive results, if there are any, will lie near the level of significance; and there is a danger in relying on small deviations from chance-expected values unless one is very certain that the conditions assumed in the experiments hold with great exactness. The experimenter is, indeed, likely to be right about the conditions in the main; but it is very difficult to be sure that nothing has been overlooked which might make a small difference to the probability of success. For this reason, in the present experiments, no stress has been laid on any results unless the figure for the anti-chance probability is a long way above the commonly accepted significant value.

Again, the Individual method allows of the use of apparatus by means of which each subject can be put through a very large number of trials. But the Collective method cannot make use of any but the simplest apparatus and hence cannot test its subjects under the best conditions.

On general grounds the Individual method seems a more practical way of going to work than the Collective, for, if there is one thing that experience reveals, it is the paramount importance for the investigator to keep in close, personal touch with his subject. Nature has rivetted human attention on the channels of normal perception and has placed subconscious barriers against the flow of paranormally acquired knowledge into the consciousness: and, as a result, the main task of the experimenter is to find a way of surmounting these subconscious barriers by discovering and allaying resistances and inhibitions and by preventing their growth. The essential avenue of approach to psychical research is that of the psychologist and not that of the statistician; and the main use of mathematics in this subject should be for the evaluation of chance in any particular experiment.

Tests with a Number of Subjects

In March 1935, I was asked by the Council of the S.P.R. to experiment with a number of subjects in order to test whether the results obtained with Miss Johnson could be repeated with others; and also to find out whether a repetition of experiments on the lines followed by Dr Rhine, and published in his book *Extra-Sensory Perception*, would show a percentage of successful subjects at all comparable with his. Accordingly, between the 3rd April and 4th November, 1935, 30 subjects were tested with the Pointer Apparatus and 21 out of the same group with " Zener " cards of the kind which Dr Rhine had used in his experiments. Miss Johnson entered into these experiments on an equal footing (at least as regards external conditions) with the other subjects, working in all cases with an agent other than myself, since all the other subjects were working with a stranger.

These experiments were carried out at the Rooms of the S.P.R. I regarded this use of the Pointer Apparatus as a preliminary test from which to select subjects for further tests with the Electrical Apparatus afterwards; although there would be this additional point of interest, to find out what percentage of a group of subjects gathered *ad hoc* would be successful. It was considered unnecessary to have witnesses at these trials, since there was no way in which the subject could score by misusing the machine without it being observed by the operator. Also, they were preliminary, and it would be sufficient to call witnesses for the final trials. The argument that there should always be a witness, not as a check on the accuracy of the record, but as a guarantee of the good faith of the experimenters, seems logically to lead to the necessity for a second witness to guarantee the good faith of the first, and so on *ad infinitum.* Thus, witnessing from this point of view becomes something of an absurdity. In these experiments, the operator has also been the recorder, unless otherwise stated.

With this apparatus, subjects have the opportunity of seeing the pointer in the box, when the right one is opened, and so of knowing each time they score a success: but, unless the subject is sitting forward and the light is good, it is not always easy to see the pointer in the fraction of a second during which the box is open. Many subjects say that they only see the pointer occasionally, and one subject said that she never even looked for it.

In some experiments, the signal for the subject to open a box was given by the operator saying the word " In "; in others by his tapping on the floor with his foot. This is indicated in the Table by the words " In " and " Tap " respectively.

Abbreviations

In giving experimental results, the following abbreviations will be made use of:

No. = the serial number of the result.
T = the number of Trials.
S = the number of Successes.
d = the deviation from Chance Expectation (above or below).
% = the Successes as a percentage of the total number of Trials.
X = the Deviation divided by the Standard Error.
P = the Probability that the result is due to Chance.

Those taking part are indicated by their initials as follows:

G.J.	Miss G. M. Johnson.
C.V.C.H.	Mr Herbert.
C.H.	Lord Charles Hope.
H. de G.S.	Mrs Salter.
G.H.	Mr Gerald Heard.
H.F.S.	Mr Saltmarsh.
G.W.F.	Mr Fisk.
G.N.M.T.	Mr Tyrrell.

The method of carrying out the experiments and of recording them is the same as that described in the *Journal* S.P.R. 514 for April 1935 on p. 56, from which the following paragraph is quoted. " 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." The percipient was separated from the operator by a large wooden screen and the sound of the pointer was rendered negligible by careful padding of the boxes.

(For readers who are not conversant with mathematical symbols, it may be as well to add that a probability is always expressed in the form of a fraction. Thus, the statement that the probability of success is 1/5 means, in common language, that the odds against success are 4 to 1. If the probability that a result is due to chance is less than 0·01, it is arbitrarily taken to be significant.

A probability of 0·01 or 1/100 is equivalent to odds of 99 to 1 against. A probability of 0·001 or 1/1000 or 10^{-3} is equivalent to odds of 999 to 1 against, or approximately of 1000 to 1 against, and so on.

The figures for P in the probability column are only approximate. By the time we reach figures sufficiently low to be reliable, their accuracy has ceased to matter: the order is sufficient. They are taken from a curve plotted from tables of the Probability Integral given in Dr R. A. Fisher's book, *Statistical Methods for Research Workers*.)

TABLE I

RESULTS WITH THE POINTER APPARATUS

Date, April 3 to November 4, 1935. Operator, G.N.M.T.

No.	Subject	T	S	d	%	X	P	Conditions
1	Baker, Miss - -	1500	301	+1	20·0	—	—	Tap
2	Bramley-Moore, Mrs	1600	374	+54	23·4	3·37	0·0003	Tap
3	Carruthers, Miss -	800	165	+5	20·6	0·4	0·34	In
4	Coates, Miss - -	800	179	+19	22·4	1·7	0·04	In
5	Douthett, Mrs -	100	26	+6	26·0	—	—	In
6	Firmstone, Mrs -	400	79	−1	19·7	—	—	In
7	Fisk, Mr - -	1600	313	−7	19·4	0·44	0·33	In
8	Ganz, Mrs - -	800	170	+10	21·2	0·9	0·18	In
9	Goldney, Mrs -	1200	245	+5	20·4	0·36	0·36	In
10	Heckle, Miss - -	1400	287	+7	20·4	0·46	0·32	In and Tap
11	Heckle, Mr - -	1000	208	+8	20·8	0·63	0·46	In and Tap
12	Hemingway, Mrs -	2000	504	+104	25·2	5·81	10^{-8} to 10^{-9}	Tap
13	Herbert, Mr - -	2000	409	+9	20·4	0·5	0·32	In
14	Hope, Lord Charles	1500	210	+10	20·6	0·6	0·27	In
15	Humphreys, Mr -	100	19	−1	19·0	—	—	In
16	Hutchinson, Mrs -	3200	674	+34	21·0	1·5	0·07	In
17	Johnson, Miss -	2000	494	+94	24·7	—	—	Op. C.V.C.H. In
18	,, ,, -	400	89	+9	22·2	—	—	Op. C.H. In
19	,, ,, -	1300	351	+91	27·0	—	—	Op. H. de G.S. In
20	,, ,, -	3800	902	+142	24·0	—	—	Op. G.W.F. In
21	,, ,, -	7500	1836	+336	24·4	9·7	10^{-21}	

No.	Subject	T	S	d	%	X	P	Conditions
22	Miller, Mr - -	900	219	+39	24·3	3·25	0·0006	Tap
23	Miller, Mrs - -	400	80	0	20·0	—	—	Tap
24	Minns, Mrs - -	1500	318	+18	21·2	1·1	0·136	In
25	Morton, Dr - -	1600	325	+5	20·3	0·3	0·39	In
26	North, Mr - -	1200	252	+12	21·0	0·87	0·91	In
27	Nunn, Miss - -	2300	481	+21	20·9	1	—	In
28	Richmond, Mr -	400	73	−7	18·2	—	—	In
29	Richmond, Mrs -	800	181	+21	20·9	1·8	0·03	In and Tap
30	Tennant, Mrs -	2300	495	+35	21·5	1·8	0·03	In and Tap
31	Tubbs, Mrs - -	2100	433	+13	20·6	0·7	0·24	In
32	Turner, Miss - -	100	18	−2	18·0	—	—	In
33	Varvill, Mrs - -	3100	641	+21	20·7	0·9	0·16	In and Tap
34	Wiley, Mrs - -	400	77	−3	19·2	—	—	In

There is no reason to treat these results collectively. They are tests of individual subjects, and each test is independent of all the others. The result shows that four of the 30 subjects have scored significantly, while the rest have not. Nothing but confusion would result from considering these four successful subjects in connection with a group. The significant results are Nos. 2, 12, 21 and 22 ; and the two outstanding ones are Nos. 12 and 21, obtained by Mrs Hemingway and Miss Johnson respectively. Miss Johnson's results were obtained with four different operators.

It may be as well to insert here the complete figures of Miss Johnson's scores with operators other than G.N.M.T. There were six of these ; but two of them experimented with her when acting as witnesses in her previously reported experiments, and so have not been included in the above Table. When they are included, the totals with all operators other than G.N.M.T. are as follows :

TABLE II

POINTER APPARATUS

G.J. with various Operators

No.	Operator	T	S	d	%	X	P
35	H.F.S. - - -	400	121	+41	30·2	5·1	10^{-7}
36	G.H. - - -	600	169	+49	28·1	5·0	10^{-7}
37	C.V.C.H. - -	2000	494	+94	24·7	5·2	10^{-7}
38	C.H. - - -	400	89	+9	22·2	1·1	0·13
39	H. de G.S. - -	1300	351	+91	27·0	6·3	10^{-9}
40	G.W.F. - - -	3800	902	+142	24·0	5·7	10^{-8}
41	Totals - - -	8500	2126	+426	25·0	11·5	10^{-29}

This Table shows clearly that Miss Johnson's scoring is not the outcome of some condition which is dependent on one particular operator. She has scored a long way above chance with five out of these six operators, and the total with all operators is colossally above chance.

The largest number of trials was done with Mr Fisk. He says, with reference to his detailed scores, which are omitted here for lack of space, " If the figures are examined it will be noticed that Miss Johnson's scoring deteriorated somewhat after the first two weeks of the experiment. This is probably due to the fact that I deliberately tried to make it difficult for Miss J. to score. After the first couple of sittings I realised that there was no particular object in continuing the experiment merely to confirm the work Mr Tyrrell had already done in over 30,000 trials. She could obviously score well above chance with me as operator in place of Mr Tyrrell. So I ceased to give Miss Johnson what might be termed a fair deal. I watched her guesses very carefully to see if I could detect any numerical habits and then did my best to thwart her. Occasionally I purposely neglected one or two of the boxes for a whole series of trials. She however kept on scoring well above chance. I was most successful in my opposition of 28th August when I used pre-selected numbers that slowed down the operation very considerably. I had also tried chaffing her a little before the trials and for this or other reasons her score dropped to chance expectation only for that day. On the previous day (23rd August) I had used pre-selected numbers (but without any previous attempt to ruffle Miss J.'s feelings) and after guessing two series at a chance score (20, 20) she managed a 27 and 34. Her lowest score was 14 on 24th Sept. . . . From the result I do not see how one can escape from the conclusion that some factor other than chance is involved." Further, he says : " The one important consideration is the question of the validity of the assumption that chance-expectation of success in every trial is 1/5. Is this assumption undermined by the possibility that any particular operator may have ' number-habits ' and does the percipient learn these habits, consciously or unconsciously, and so begin to score above chance expectation ? If so, the fact that Miss J. has made high scores *at once* with new operators would seem to show that if number habits exist we all of us have precisely the same habits. Were that so we should surely be able to detect them. But analysis of successive numbers chosen by different operators has failed to show any favourite sequences. The fact that Miss J. also scores with pre-selected numbers (chosen by dice or other mechanical means) is evidence against any number-habit explanation."

Three different operators used numbers which had been selected beforehand by mechanical means, with results as in following Table:

TABLE III[1]

MECHANICALLY SELECTED NUMBERS WITH THE POINTER APPARATUS

No.	Operator	*T*	*S*	*d*	%	*X*	*P*	
42	C.V.C.H. -	700	142	+2	20·3			"Out."
43	G.W.F. -	1300	301	+41	23·1			"Out."
44	G.N.M.T. -	704	201	+60	28·5			"Home."
45	Totals -	2704	644	+103	23·8	5·0	10^{-6}	

Two things appear from this Table:

(1) That this score, which is certainly not due to chance, cannot be explained by any kind of number-habits; for the probability of success in this case is quite certainly 1/5. (2) That there is a markedly higher rate of success in No. 44 than in Nos. 42 and 43. This is evidently due to the difference between working under the "Home" condition with a familiar operator and working under the "Out" condition with a comparative stranger.

Effect of Signal for opening Boxes

In the case of three of the best scorers, the Tapping signal was used, viz. with Mrs Hemingway, Mr Miller and Mrs Bramley-Moore. With some subjects, both methods were tried alternately, and two scored at a slightly higher rate with the Tapping than with the In signal; and one slightly higher with the In than with the Tapping. A fourth scored almost at the same rate with both; so that there is no evidence that it made any difference in which way the signal was given.

The Question of Help from the Auditory Sense

A note may be interposed here with regard to the suggestion that the sound of the pointer may give some guide to the percipient. In March 1936 an experiment was carried out with a makeshift pointer consisting of a pencil with a piece of flannel wrapped round it, and used in bare-wooden boxes. There were slight, audible sounds; but the percipient could not locate them or make any use of them. Therefore with the carefully padded boxes in this apparatus all help from supposed sounds must be out of the question.

The Probability of Success with the Pointer Apparatus

The system of allowing the operator to choose his boxes, as opposed to the method of using pre-selected numbers, does admittedly make

[1] See also footnote on p. 151.

it impossible to say exactly what the distribution of the choice of boxes has been. And it may be argued that, as there is a departure here from the random selection of pure chance, there will be a tendency to raise the probability of success. This question of what factors affect the probability of success will be gone into presently in greater detail when the Electrical Apparatus is dealt with: but it may be pointed out here that the scores of the 26 subjects who have shown no sign that they are getting anything but chance results are, in fact, clustered very closely about the value 20%. Thus, if they are chance scorers, the probability of success when the operator is choosing his boxes, must be very close indeed to 1/5. I am aware that this argument may be attacked on logical grounds, since the four good scorers have first been removed from the group; but, since the great majority scored at the same rate and this rate was 20%, it seems in accordance with common sense to conclude that this confirms the assumption that the probability of success is almost exactly 1/5.

Mr Fisk's Method of Scoring

Another question which will have to be raised is whether it is possible to score with the Pointer Apparatus according to the method discovered by Mr Fisk. What this method is will be explained later:[1] but the opinions of some operators who have used the apparatus in practice as to the likelihood of scores being obtained (*a*) by the operator following the habits of the subject, or (*b*) by the subject's using the Fisk method, are appended here.

These operators were asked:

(1) Do you consider that the scores obtained when you operated with the Pointer Apparatus might have been obtained by your choosing the boxes in the same order in which you had observed the subject to be choosing them?

(2) Do you think it possible that the subject could have scored by continuing to open one box at a time until a success was obtained; going on to another box and continuing to open that until a success was obtained and so on, without your observing this and avoiding the box which was being repeatedly opened?

The answers were:

C.V.C.H. (1) Possible, but unlikely. (2) No.

[1] A full explanation of this will be given below in the section entitled "A New System of Scoring" (p. 153); but it may be said here that it consisted essentially in the repeated opening of the same box by the percipient until a success was obtained.

H. de G.S. (1) I never consciously observed the percipient to have a preference for any particular order and it seems to me most unlikely that I observed anything of this kind unconsciously. I have not the records of the experiments by me, but so far as I remember, there was no marked improvement in the later experiments such as one might expect if I was observing the percipient's number habits and acting on them.

(2) I am pretty certain that if the percipient had done aything of the kind I should have noticed it. It was immediately apparent to me when the percipient opened the same box twice running (which she did from time to time). I did notice that she very rarely, if ever, in my experiments opened the same box three times running.

G.W.F. (1) No. No preferences were observed although I was continually on the watch for them. Had I observed any preference it would have told *against* the percipient's score as I should have been able to " dodge " her more successfully.

(2) Quite impossible. During my experiments the percipient rarely opened the same box twice in succession and never, to the best of my remembrance, thrice.

Results with Cards

A pack of " Zener " cards was used for these experiments of a kind similar to those used by Dr Rhine and described in his book *Extra-Sensory Perception*. These cards contain five diagrams, a Cross, a Rectangle, a Circle, a Star, and Wavy Lines. There are five cards with each diagram, making 25 cards in the pack.

The method adopted in the experiments was as follows: the subject and operator sat in the same positions as for the Pointer experiments, that is, one on each side of a narrow table divided by a wooden screen measuring 26″ by 26″. The operator shuffled the pack of cards behind the screen before each experiment and placed it face downwards on the table in front of him. He then said the word " Ready " or " Now " to the percipient, and turned the cards up one by one, placing each in turn rather noisily face upwards on the table and looking at it as he did so. On hearing the card put down on the table, the percipient called and the operator entered the call and the actual card side by side in parallel columns. The agent did not speak at all during the experiment, and the percipient did not see or touch the cards. The probability of success was assumed to be 1/5.

The number of trials done by each subject varied according to the time and opportunity which each was able to provide.

TABLE IV

CARDS

Date, April 3 to November 4, 1935. Operator G.N.M.T. except where stated

No.	Subject	T	S	d	%	X	P	Conditions
46	Baker, Miss -	375	75	0	20·0			
47	Carruthers, Miss	150	31	+1	20·6	0·20	0·42	
48	Coates, Miss -	100	23	+3	23·0			
49	Fisk, Mr -	350	64	−6	18·3			
50	Goldney, Mrs -	200	36	−4	18·0			
51	Heckle, Mr -	125	22	−3	17·6			
52	Heckle, Miss -	325	78	+13	24·0	1·8	0·03	
53	Herbert, Mr -	225	40	−5	17·3			
54	Hope, Lord Ch.	275	60	+5	21·8	0·75	0·24	
55	Hutchinson, Mrs	550	102	−8	16·7			
56	Humphreys, Mr	75	17	+2	18·5			
57	Johnson, Miss	1000	228	+28	22·8	2·21	0·013	Operator: G.W.F.
58	Minns, Mrs -	75	10	−5	13·5			
59	Miller, Mr -	100	16	−4	16·0			
60	Morton, Dr -	300	57	−1	19·6			
61	North, Mr -	225	36	−9	16·0			
62	Nunn, Miss -	225	38	−7	16·8			
63	Richmond, Mr	100	19	−1	19·0			
64	Tennant, Mrs -	400	79	−1	19·7			
65	Varvill, Mrs -	325	81	+16	21·8	2·22	0·013	
66	Wiley, Mrs -	100	17	−3	17·0			
67	Totals - -	5600	1131	+11	20·2			

It will be seen that none of the pereipients has seored signifieantly above ehance expectation. Mrs Varvill's and Miss Johnson's seores alone reach to just about the point of signifieanee ; but neither is high enough to support any positive eonelusion. Treated eolleetively, the group shows very exaetly the ehanee rate of seoring.

Whereas I was the operator in all eases exeept one, Mr Fisk was the operator in Miss Johnson's ease. He says : " From 27th June 1935 to January 1936 I eondueted a series of Card Guessing tests with Miss Johnson, using Zener eards. Conditions were varied from time to time. Thus, the pereipient was sometimes sereened from a view of the eards ; sometimes she was allowed to see the eard turned up after her guess ; some tests were for pure elairvoyanee, others for elairvoyanee and telepathy mixed, ete., the objeet being to diseover, if possible, whether Miss J. would be able, in any variation of the method used, to obtain a significant score. After over 1000 mixed trials results were a little promising; successes were significantly above chance expectation ($X=2{\cdot}66$, $P=0{\cdot}004$) and we were encouraged to continue, but she did not seem to show any marked preference for any particular set of conditions—she scored impartially a little above chance with them all.

"But at this juncture Mr Tyrrell and I discovered that it is possible to obtain apparently significant scores with Zener cards under certain conditions by using systematised guessing." (This system will be described later.)

Mr Fisk goes on to say: "We adopted the following method. G.W.F. shuffles and cuts the pack of 25 cards and holds the pack face downwards in the left hand. Sitting behind a screen with G.N.M.T. he takes the first card, turns it over, places it face upwards on the table and says 'Now.' Miss J.—the other side of the screen and unable to see the operation—names a card. G.N.M.T. records her guess. The same procedure is followed right through the pack except that after the first card no word is spoken as Miss J. is able to hear the card being placed on the table (put down purposely with a bang). After 25 trials G.W.F. picks up the upturned pack—taking care to preserve the order of the cards—reverses it and calls out each card for G.N.M.T. to record against Miss J.'s guesses. Successes are then counted. The pack is run through as rapidly as G.W.F. can turn the cards."

(The results obtained by this method are No. 57 in Table IV.)

Mr Fisk continues: "As operator I was impressed by the way that occasionally Miss J. would score nothing at all for quite a considerable number of trials and then, suddenly, begin to score at a high and consistent rate. She seems either to be very bad or very good at it. There was also confirmation of what Mr Tyrrell has previously noted, viz. her difficulty in expressing any paranormal feeling in words. There were several instances when from behind the screen I could hear her begin to say, for example, 's-s-st' (an evident attempt to pronounce the word 'star') and then, as though giving it up, changing to 'cross' or 'oblong,' etc. In, I think, every case her first attempt would have been correct. Sometimes, too, she would correct a call with 'I really meant to say so and so' and here again she was generally, though not always, correct. Needless to say corrections were not counted in the score."

The fact that we have in G.J. a subject who scores well beyond the significance point with the Pointer and is one of the only two who approach this point with the Cards suggests that there is something more difficult about the card method than about the pointer method.

In fact, it suggests that we may have been testing the card subjects under conditions which were just too difficult. If the card method is inherently more difficult than the pointer method, it is also true that it has not been given the advantage of "Home" conditions. Miss Johnson never concentrated on them with myself as operator: the subjects met by appointment, doing a few trials once or twice a week. There is evidently a great difference between what may be called "Home" and "Out" conditions, as can be shown by reverting for a moment to the figures supplied by Miss J. with the Pointer Apparatus.

Table V

No.	Conditions	Rate of Scoring %
68	G.N.M.T. operator under "Home" conditions - - - - - -	31·2
69	Two witnesses of experiments (G.H. and H.F.S.) act casually as operators under "Home" conditions - - -	29·0
70	Three strangers (C.V.C.H., C.H. and H. de G.S.) act as operators under "Out" conditions - - - -	25·2

And the same thing is shown by the differences between No. 44 and Nos. 42 and 43 in Table III.

If the subjects in the Card experiments had had the opportunity of working for some time in their home surroundings with an experimenter with whom they were quite familiar, would some of them have scored? It seems not improbable. If Miss J. had worked on the cards for some time at home, she would very likely have scored in the end.

This raises a point which has a bearing on a possible criticism which might be made of these experiments, viz. that they should have been carried out on a larger scale; and that with more extensive results they would have made a more valuable comparison with those of Dr Rhine. The figures just quoted show, however, that the conditions possible with a single subject are not possible with a number; and, in fact, that it is just the difference between what has been called "Home" conditions and what has been called "Out" conditions that shows itself in the score. This difference may be a subtle one and not easy to put down in black and white; but it is there. And the question which raises itself is whether the conditions of Dr Rhine's experiments were more comparable with those in which Miss J. scored 31% or with those in which she scored 25%.

One cannot assess an intangible thing like a mental atmosphere. One cannot even bring its existence home to some people; yet it makes all the difference to the results. But there is, I think even an insensitive person might admit, a difference between coming once or twice a week to a dull room to go through an experiment with a stranger, and doing an experiment in a university as an undergraduate with other undergraduates. The latter may be nearer to the "Home" than to the "Out" condition. Hence, one must ask whether, if these experiments had been very much extended, they would still have come anywhere near to being a duplication of Dr Rhine's? And whether any figures about the "distribution" of the faculty of Extra-Sensory Perception could safely be based on the number of subjects who happened to score above chance. It certainly seemed to me better to take the two or three subjects who showed promise and to concentrate attention on them.

There is another consideration affecting the use of cards in general which these results suggest, when taken in conjunction with the success with the Pointer Apparatus. It seems possible that all the card experiments so far made have begun too high up in the scale of complexity. No doubt, people first began to use playing-cards for telepathic experiments because they were easy to obtain: but, with their two colours, four suits, with court cards separate from the others and no striking differences between the arrangements of the pips, they are obviously unsuitable for work in E.S.P. The Zener cards are a great improvement on playing-cards; but even they are far from presenting the simplest possible kind of event for a perceptual faculty to take hold of. It is true, of course, that we do not know whether clairvoyance is at all analogous in its processes with visual or any kind of normal perception; but we know at least that paranormal knowledge, however acquired, must emerge; and in its emergence it usually takes sensuous forms. So that we have some ground for supposing that the simpler the event presented for clairvoyant cognition, the better chance it will have of getting through. Now, the simplest event one can present would seem to be a bare contrast, such as that between black and white or light and dark, etc.; and this is what the Pointer Apparatus does and also the Electrical Machine, to be described later. The Pointer Apparatus presents the contrast between a white box empty and a white box with a dark pointer in it. And the Electrical Apparatus presents the contrast between a lamp which is alight and a lamp which is out. But the Zener cards demand the recognition of a diagram, and, although the diagram may be a simple one, it makes a much greater demand on any perceptual faculty than does a simple contrast. Suppose, for instance, that one were asked to look at something exposed to view for only a brief fraction of a second in order to say what it was, would it be easier to say whether it was just something light or dark or whether it was a cross or a circle ? Of course, it is possible that clairvoyance may work in such a way that it does not make any difference what the event is that is open for its perception ; but it would be on the safe side to assume that it does.

A simple contrast could be arranged with cards, no doubt. The subject might be asked to distinguish between a card with a white face and a card with a black one, or between a blank card and a card with a black disc on it : but the process of working through cards like this would be much slower and more tedious than working with a machine and would yield a smaller number of trials, so that it does not seem to be worth while, if one has the easier method to work with.

The Electrical Apparatus

General Description

The Pointer Apparatus was an experiment, obviously capable of improvement in many respects. Some arrangement was needed which would differentiate between Telepathy and Clairvoyance and would select the order of events mechanically. An electric signal, for opening the boxes, which would give an invariable sound, was also needed ; and an automatic recorder which would work at any reasonable speed and be fraud-proof at the same time, was badly needed. A high speed of working was required, both to meet the obvious psychological demand for it and also to avoid tedium, which certainly militates against success, by inculcating a dislike which is always the father of a resistance. The more automatic the machine becomes the more both operator and subject are freed from the necessity of giving attention to the purely routine part of the experiments ; and, in point of fact, the speed and ease of working through 100 trials with this electrical apparatus in a few seconds is a revelation of how simple an experiment in E.S.P. can be. Also, with the machine, hundreds of trials can be accumulated rapidly and easily ; and the importance of this will be appreciated when it is realised how slow is the process by which a new condition is accepted by the subliminal of the subject. The tedium of going through all this with a pack of cards might well postpone acceptance of the new condition indefinitely.

The Pointer Apparatus had embodied at least four improvements over the method of using cards, viz. speed, rhythm, the presentation of the event as a simple contrast instead of in the complicated form of a diagram, and the externalisation of the paranormal knowledge by motor action instead of by speech.

The new apparatus, now to be described, embodied these further features : (1) An automatic Recorder marking successes and failures, but not recording each lamp lit and each box opened. (2) Electric lamps as the events inside the boxes, with provision for changing these for other pieces of apparatus if desirable. (3) An electric signal from operator to percipient. (4) A mechanical Selector for selecting the lamp-circuits in a purely chance order. (5) An alternative arrangement of five silent Keys by which the operator can himself select the lamp-circuits. (6) A Commutator which could transpose the connexions between the Keys and the Lamps in various orders, which can be either known or unknown to the operator. (7) A Delay-action Relay, for selecting the lamp to be lit beforehand, but postponing the actual lighting until a box is opened. (8) Arrangements of switches by means of which any desired condition can be instantly brought into play. These features were not all embodied in the apparatus as it was at first constructed ; some of them were added from time to time as experience suggested them.

Technical Description of Electrical Apparatus

(The references are to Diagram A)

Percipient's Table. This measures 30″ by 17″ and supports the Box Unit and Screen. It is divided longitudinally underneath by a screen of Essex board reaching from the underneath of the top to the floor and coming round on either side so that the percipient's legs are in a sort of bay.

The Box Unit. The five boxes are of mahogany and measure $1\frac{1}{2}''$ in width with spacings of $\frac{1}{2}''$ between them. The lids slope downwards towards the percipient and overhang the boxes $\frac{1}{4}''$ in front so that they can be raised by lifting with a padded stick. They are held down by springs of rubber bands, one on either side, passing over projections in the sides of the lids, which form cranks. The pressure is therefore strongest when the lids are closed and weakens as they are opened. The lids are rabetted and faced with fine velvet where they make contact with the box, and an undercut flange projects into a space under the hinge, thus rendering the lid light-tight.

Arranged on a shelf above and behind the boxes is a double row of electric contacts (C1-C10) so adjusted as to be opened by an arm projecting backwards from the side of the lid when the latter is closed. When the front of the lid is raised as much as $\frac{1}{16}''$ these contacts automatically close. One of them for each box brings in the Trial Recorder (TR) which registers the trial by making a short dash on a moving strip of paper tape, while the other causes a relay to close, which renders possible the recording of a success if the right lid has been raised, but not otherwise. The five boxes are open at the back to allow of the introduction of small carriages which contain the electric lamps. Each is secured in position by the turning of a button, and when each is pushed home the lamp is automatically brought into circuit.

The Screen. A screen of 3-ply wood 30″ square fits into grooves in the sides of the apparatus and comes down to the table on either side and so divides the entire table longitudinally into two halves. The top of the screen is then 5 feet above the floor and the percipient can see nothing of what is going on on the other side of the table. And, as has been said, the screen is continued under the table to the floor and is also brought round to the sides. The percipient's table is connected to the operator's table by flexible wires 23 feet long, and can thus be moved about the room.

The Operator's Table. This contains most of the apparatus.

The Relay Switches (R1-R10). There are ten of these relays of the 1000 ohm P.O. type operating on a current of from 5 ma. upwards, and the object of them is to enable successes to record themselves automatically. Five of these are connected, one each, in circuit with the box-lid contacts ; and five are connected, one each, in circuit with the lamps in the boxes. The contacts of these relays are connected in pairs in series, one box-lid relay and one lamp relay from the same box, making a pair. This gives the arrangement that when a pair are closed together, the Success Recorder (SR) is brought into action, and a second dash is made on the tape parallel with the dash recording the trial. A double dash thus indicates a success. The arrangement will be understood at once by reference to Diagram A.

Since one relay is in circuit with each lamp, it is necessary that there should be no distinctive sounds from them which might betray to the percipient which lamp is alight. The relays are carefully silenced with rubber and are placed in a stout oak case faced with velvet where it is pressed against the table.

The Multiple Cut-out (MC). This is a device to render it impossible for the percipient to score a success by raising more than one box-lid at a time. The possibility of opening one box for a preliminary peep is secured against by the early closing of the Trial Recorder circuit which records the trial before the percipient can see into the box.

But, unless some device were provided to prevent it, it would be possible to secure a success by opening some or all of the boxes together. The Multiple Cut-out consists of a relay with five windings on one core, and it is provided with a break instead of a make contact. One of these windings is included in circuit with each of the percipient's relays, and the tensions are so adjusted that the current passing through one winding is insufficient to work the relay, while the current passing through two or more windings is sufficient to work it. Thus, if two or more box-lids are opened, the relay works and breaks the circuit of the Success Recorder, thus rendering the recording of a success impossible. The Trial Recorder works as usual and records a failure.

The Recorders. These are made by adapting a machine known in the Post Office as a "Morse Inker." This machine drives by clockwork a strip of paper $\frac{1}{2}''$ wide and there is an electro-magnet, which, when energised, brings an ink-wheel into contact with the paper and marks a dash on it. This machine has been adpated to comprise two ink-wheels side by side marking on the same strip of paper, each operated by its own independent electro-magnet. One of these is the Trial Recorder, which makes a mark each time any box-lid is lifted; the other is the Success Recorder, which makes a second mark parallel to the trial mark each time any box is opened which has a lamp alight inside it.

The Keys. Five Keys are arranged in a row so that the pressing of any key lights a lamp in one of the boxes. Each key makes contact with a globule of mercury contained in a brass cup, which renders it silent.

The Commutator. The wires from the keys do not pass directly to the lamps but pass first through a Commutator. This is, in effect, a rotary switch capable of transporting or mixing up the five connexions in various ways. The five wires from the keys can, by its means, be joined to the five lamps in ten different ways, seven of which transpose the connexions in various orders, while the remaining three join them to the lamps in a straightforward order of one to one, two to two and so on. This Commutator switch is driven by an electric ratchet mechanism. It is in fact a "Rotary Line" switch as used by the Post Office in automatic telephone exchanges, and the pressing of a button causes it to rotate and come to rest in one of the ten positions. The switch is enclosed in a box, which has a door facing the operator, so that when the door is shut and the button is pressed, the switch comes to rest in a position which is unknown to the operator. After that, the operator, as he presses each key, does not know which lamp he is lighting. As the switch rotates rapidly and the button is usually pressed three or four times there is no possibility of the operator's guessing where it may come to rest.

The Selector. In addition to the five keys there is a mechanical Selector, consisting of a rotating switch of the same kind as that used for the Commutator; but in this only a single arm operates and selects one out of the five lamp circuits. The Selector contains 25 contacts and these are arranged in a semi-circle with a double arm passing over each in succession. When one arm has swept over the semi-circle of contacts the diametrically opposite arm takes over and follows it. The arm is driven by an electro-magnetic and ratchet device and the 25 contacts are grouped in 5 groups of 5 each. In each group the wires from the five lamps are connected to the contacts in a different order, the corresponding numbers of each group are then connected together. Hence, the Selector, when worked, stops with one of the five lamp-circuits selected and connected up, the choice being purely a random one. The key working the Selector is so arranged that when it is pressed, the breaking of the back contact stops the Selector and thereby selects a circuit, and then the making of the front contact of the key completes the common part of the lamp-circuits. Thus no current passes through the Selector while it is moving.

The Selector is rendered silent by encasement in three boxes filled with wrappings of various sound-deadening materials, and is placed in a gallery at the far end of the room, 20 feet from the operator's table. It is for all practical purposes inaudible and is used as an alternative to the keys, being brought into circuit by the operation of a simple switch ready to the operator's hand.

Tests of Selector. The Selector is tested from time to time to make sure of its impartiality. The results of the last test of 2000 trials are as follows: 410, 405, 400, 370, 415 for the frequencies of the five circuits.

The Delay-action Relay (DAR). This device secures that, when desired, the pressing of the operator's key shall select which lamp is going to be lit next, without actually lighting it. It consists of a relay of which the electro-magnet is in circuit with the Trial Recorder, so that each time the latter works this relay closes. The contact which thus closes is placed in the common return wire of the lamps, so that, in the ordinary way, no lamp will light until this relay has been actuated. Thus, when the operator presses one of his keys, or works the automatic Selector, he will have selected a circuit, but the lamp will not light because the common circuit of all the lamps is broken at the Delay-action Relay. As soon, however, as *any* box-lid is opened, this relay comes into operation and the selected lamp lights up. When it is not desired to use this arrangement it is thrown out of action by simply short-circuiting the relay-contacts by a switch. The object of the device will be described later.

Synchronising Lamp (SL). Since working is done at high speed, it is essential that the operator should have some method of keeping in step with the percipient. He must be able to know that he is pressing his key at the same time that a box-lid is open, otherwise no successes can be recorded. A small lamp is therefore placed in front of the operator, and is lit up by the Trial Recorder each time a box-lid is opened. The operator has only to watch the synchronising lamp and press his key when it is alight.

Sounder (S) and *Counter* (C). As he presses the key with his right hand, the operator raises a small lever with his left, which performs two actions simultaneously, (*a*) it closes the circuit which causes the Sounder to give a click under the percipient's table, and (*b*) it moves on a Counter on which it records each trial and tells the operator when he has reached 100. This also avoids the necessity for having to count the total number of trials on the tape.

Operator's Screen. The operator's table is situated in the corner of the room, so that one wall is behind him and another on his right hand, where the end of his table shuts against the wall. The back of the table and its outer end are surrounded by a wooden screen, reaching to the floor and standing 5 feet above it. There is only a space 30″ wide left for the operator to get in and out of the corner in which he works. Thus, nothing whatever can be seen of the operator or his table or any of his apparatus from any part of the room where the percipient's table is placed. And, in addition, there is the screen already described on the percipient's table itself.

Reliability of the Apparatus. One question which naturally presents itself is this. Could the failure of any part of the machine to act properly give a falsely high score ? The most likely thing to happen is for the operator to get out of step with the percipient when the experiment goes fast. The effect of this will be to shorten the time during which a key is pressed and a box is opened simultaneously. This shortens the length of the dash indicating a success. If there is no common time at all during which a key is pressed and a box opened together, a success becomes impossible, and only the dash of the Trial Recorder appears. Thus successes may be lost by getting out of time, but they cannot be gained.

Failure of the box-lid contacts to open when the lids are closed results in a continuous line being drawn on the tape, giving no separation of trials and therefore no series of experiments. Their failure to close when the lids are opened gives blank spaces on the tape, so that it simply results in no trials at all being recorded.

The only way in which unearned successes could be recorded would be if the Relays stuck in the closed position, failing to release when the current was off. This, as a matter of fact, has never happened, but a simple and rapid test can always be applied to show whether they are working properly. This test is frequently carried out and is always applied after any strikingly good result has been obtained. It has been found that any fault shows itself immediately on the tape record.

Types of Experiment which are Possible with the Apparatus

(1) *The Selector.* This renders it possible to do experiments in which the events on one side are certainly distributed according to a purely chance arrangement. Since the wires from the Selector to the lamps pass through the Commutator, they are further re-arranged there, but this makes no difference.

(2) *The Keys.* These may be used to give several different conditions.

(i) They may be connected to the lamps in a straight one to one order. This arrangement divides into three sub-sections : (*a*) where the arrangement is known to the subject and to the operator ; (*b*) where it is known to the operator only ; (*c*) where it is known neither to operator or subject.

(ii) Key connexions transposed by the Commutator. This can also be subdivided into (*a*), (*b*) and (*c*). And, in addition, it is possible for all these to apply either to the case in which the mere fact that there is a transposition is known ; or to the case in which the actual order of the transposition is known also.

(iii) It is possible, with the keys, for the operator either to select each trial at random or to take the trials from a list of mechanically selected numbers taken beforehand.

Any case in which the operator knows the order in which the keys are connected to the lamps allows of the possibility of telepathy as well as of clairvoyance. Any case in which he does not know this rules out the possibility of telepathy, and leaves only that of clairvoyance, or possibly of precognition.

(iv) It is also possible to use any of the above arrangements in conjunction with the Delay-action Relay.

(v) It is possible to test for precognition by getting the percipient to open the box at each trial *before* the lamp-circuit has been selected by the operator when keys are being used, or by the mechanical Selector.

Some Possible Criticisms of the Apparatus

The more obvious points of criticism are dealt with here :

Knowledge by Normal Means

(i) *Visual.* Until 16th November 1935, the keys were on the percipient's table behind the screen. Nothing could be seen of them unless the percipient got up and peeped round the edge of the screen, which she could not do from her seat ; and this would stop the experiment in a very obvious way. Even this would be no help when the Commutator was in use. It must be remembered that the opening of each box is accompanied by the click of the Recorder and that even a slight hesitation in this rhythmical sound is noticeable.

The Recorder is on the operator's table, and even if it were visible, would give no help to the percipient. Actually, the record is made under a roller and no trial is visible until it has emerged over the roller about two trials later.

When the keys were removed to the operator's table, the latter was surrounded by a high screen.

There is a board of resistance-lamps for working the apparatus off the mains, and the light of these lamps fluctuates each time a key is pressed. The fluctuation is exactly the same so far as can be detected whatever key is used, but, on the advice of Professor Adrian, a metal screen was placed over this board in the beginning of January 1936.

(ii) *Auditory.* The sound of the relays in the operator's circuits, if the percipient could hear them and could distinguish the sound of each, would give an indication of which lamp was alight. The Commutator would not help to do away with this, as each of these relays has to be connected to its own particular lamp. These relays, as has been said, were carefully silenced so as to be practically inaudible with the cover removed and with the ear close to them, and absolutely silent with the cover on. The percipient would have to hear them when seated at a separate table from three to nine feet away, with the thick case over the relays and screen between as well. But what is even more important is that nearly all work with this apparatus is done at high speed, that is to say from 60 to 80 trials a minute, when it is impossible to avoid working in a rhythm. This means, that whether it is intentional or not, one finds that one is pressing the key in synchronism with the raising of the Sounder-lever and the percipient also opens the box synchronously with the click of the Sounder. The result is that the action of the operator's relays occurs simultaneously with loud sound of the Recorder, and even if they made a slight sound, this would be drowned by the

Trial Recorder. In none of those cases in which the key has been deliberately pressed some time before the opening of the box has there been any increase in the rate of scoring, which should have been the case if the sound of the operator's relays, whether perceived hyperaesthetically or normally, had been the cause of scoring. There is also the important effect of the Delay-action Relay, which entirely prevents any possibility of this sort and to which I shall refer presently.

The sound of the keys themselves has been eliminated by providing them with mercury contacts. It has been suggested that the very small spark which takes place on the mercury surface might be heard by the percipient ; but, from the percipient's table this is certainly impossible. And these again, in normal working, are drowned by the noise of the Recorder. Also there is not the slightest reason to suppose that the sound of the sparks differ from one another, or that if they do, the difference is constant. And in any case, the Commutator would do away with any possible help these might be imagined to give.

Unconscious whispering. This would have to be very loud to overcome the noise of the Recorder and extremely rapid, and observers could scarcely fail to have noticed it. Also the Commutator again would render it useless.

(iii) *Tactual.* Until the keys were moved to the operator's table, operator and percipient shared the same table, sitting one on each side of the screen. Their legs might therefore come into contact and a critic might say that information could be conveyed by touch, though this could scarcely be unconscious. Therefore on 1st September 1935 a complete screen was provided as above described.

Hyperaesthesia

The foregoing arguments which apply to the acquisition of knowledge by normal sight and hearing apply also to hyperaesthesic sight and hearing. Hyperaesthesia does not help to distinguish a faint sound which is covered by a louder one. The only sense which might be alleged to be helped by hyperaesthesia is the visual one. If there were any escape of light past the lids of the boxes, this would of course be a guide to the percipient. Or it might be said that the lamp which was alight made the box slightly warmer, and that the temperature-sense was a guide. But, as has been said, the opening of the box and the lighting of the lamp are practically simultaneous. In any case, the lamp is alight only for a fraction of a second, and it may be mentioned that the lamps are not lit to their full brightness. Suppose that the heat of the lamp were to pass through the bulb and begin to warm the lid of the box, it would probably not have its effect until the trial was over and the next lamp was alight, since it takes time for anything to get warm ; and then it would simply be misleading.

It might be suggested that some unknown form of radiation from the lamp passes through the wood of the box and affects some unknown sense-organ of the percipient in some unknown way. It is of interest in this connexion to observe that highly significant scores have been obtained when the boxes were opened well in advance of the lighting of the lamps (see No. 134, Table XXIV).

But in any case the Delay-action Relay sweeps away these objections at one blow. As before stated, this device delays the lighting of the lamp, after it has been selected, until after the box-lid has been opened, so that when the percipient selects the box, the lamp is not yet alight. It is, in fact, the percipient's own action which lights it. Yet, so far as the percipient can see, there is no difference whether the delay-action is in operation or not.

To return now for a moment to the possibility of help from the sounds of the operator's relays dealt with above, the delay-action also does away with this objection, since these relays are actuated when the lamps are lit, viz. *after* the box-lid is opened. The results in Table XXII show about equal success with the delay-action and without it ; so that all these criticisms are swept away together.

To sum up. The Commutator and the Delay-action Relay together eliminate the following possible criticisms. All sensory aid from parts of the apparatus, whether hyperaesthesia be present or not. All sensory indications which could be given by the operator, such as unconscious whispering, etc.

Fraud

The percipient has no opportunity to score by any fraudulent means. The operator is surrounded in front and on one side by a screen 5 feet high and by the walls of the room on the other two sides, except for a narrow space about 30″ wide by the wall. This screen entirely intervenes between the two tables. In order to see anything of the operator or the contents of his table the percipient would have to get up and leave her table and peer over the screen without causing a hitch in trials going at the rate of 70 a minute. This source of fraud is therefore absurd. And further, even if the percipient could see the operator's table, she would as a rule get no useful information from it.

The percipient cannot peep into the boxes beforehand because as soon as the lid of a box is raised even a small fraction of an inch a trial has been recorded. Nor can she score successes by raising more than one box-lid at a time for this cuts out the Success Recorder and registers a failure. If any box fails to close completely, the Trial Recorder remains in action and draws a continuous line on the tape and thus automatically wipes out the experiments. The percipient is therefore powerless to do anything except carry out the experiment in the legitimate way.

Chance in Connexion with the Apparatus

The next question to be considered is how chance affects the apparatus which I am now describing—whether there is anything in the course of the experiments which is likely to alter the assumed probability of success and, if so, in what way and to what degree.

In the Electrical Apparatus, with which we are now dealing, there are ten events ; five of which are the lighting of lamps by the operator, and five the opening of boxes by the percipient. Let the lamps and the boxes be numbered from left to right, as seen by the percipient, from 1 to 5. Then let p_1, p_2, p_3, p_4, p_5 be the respective probabilities of the lamps being lit, and q_1, q_2, q_3, q_4, q_5 be the respective probabilities of the boxes being opened in any particular trial, the sum of the p's being unity and the sum of the q's being unity.

We are not concerned with the different ways in which these events can distribute themselves in any block of trials, but only with the *coincidences* between lit lamps and opened boxes. The probability of there being a coincidence between a lit lamp and an opened box is the product of the separate probabilities of each event occurring. For instance, the probability that No. 1 box will be opened while No. 1 lamp is alight is p_1q_1, and so on with the other lamps and boxes. But what we are particularly interested in for our purpose is the probability of there being a coincidence somewhere along the line between a lit lamp and an opened box in any particular trial. This probability is given by the sum of the probabilities of the particular coincidences. It is the Probability of Success. Let us call it p.

Then, $$p=(p_1q_1+p_2q_2+p_3q_3+p_4q_4+p_5q_5),$$

which is true generally, whatever values the p's and q's may have.

Let us first suppose that the lighting of the lamps and the opening of the boxes is so distributed that there is no favouritism on either side, so that any lamp is equally likely to be lit and any box is equally likely to be opened at any particular trial. That will mean that the probability that any lamp will be lit and that any box will be opened is 1/5; and the probability that any lamp will fail to be lit or any box fail to be opened is 4/5. But this raises a curious point which has some practical importance. What is meant by saying that any one of five events is "equally likely to happen"? It is certainly a point open to discussion; and it may be that there are two distinct meanings to the phrase, one of which is relative to knowledge while the other refers to objective fact. At any rate, the latter meaning is the one we are now concerned with. That any one of five events is equally likely to happen means, in this sense, that if the trials are continued, the tendency is indefinitely towards the occurrence of each of the five events an equal number of times, that is, one-fifth of the total, *in the long run.* It is these words, "in the long run" which are important, and we shall have to return to the point later.

The condition of "equal likelihood" on both sides, which we have assumed, means that,

$$p_1 = p_2 = p_3 = p_4 = p_5 = q_1 = q_2 = q_3 = q_4 = q_5 = 1/5,$$

so that the probability of a success in any one particular box is 1/25. And the Probability of Success,

$$p = (1/5)^2 + (1/5)^2 + (1/5)^2 + (1/5)^2 + (1/5)^2 = 5/25 = 1/5.$$

The first thing to be noted about this formula is that, so long as each of the five events is equally likely to happen on one side, it does not make any difference to the probability of success what the distribution of events is on the other side; the probability of success still remains 1/5. This could be proved generally, but it will be sufficient to illustrate it by taking an extreme example. Suppose that on one side the events are distributed in a chance order (say the lighting of the lamps), while on the other one box (say No. 1) is opened every time, while the others are neglected.

Then, each of the ps $= 1/5$; $q_1 = 1$; $q_2 = q_3 = q_4 = q_5 = 0$.

$$p = (1/5 + 1 + 0 + 0 + 0 + 0) = 1/5.$$

Even in this extreme case the probability of success is not altered. In order that it shall be altered, *both* sides must depart from the chance distribution of events. Even then, it does not follow that the probability of success will be raised: in fact, it is more likely that it will be lowered.

If we know what frequency preferences there are in the operator's and percipient's habits, by checking each box opened and each key pressed, we can calculate from the above formula what effect these will have on the probability of success. In an actual case in which this has been done, the following is the result. Graph III, Nos. I and II, shows a record of 100 trials in which G.N.M.T. was the operator, using the keys, and L.H. was the percipient. This block of trials was checked completely from both the operator's and the percipient's ends, and No. 1 shows by the dots each key which was pressed in turn and No. 2 shows by the dots each box which was opened in turn. The successful trials are shown by rings round the dots. The frequencies are shown by the total figures at the bottom of each table; and they indicate a slight tendency to favour the middle number (No. 3) on the part of both the operator and the percipient. Inserting these frequencies in the above formula will show what effect this favouritism has on the probability of success.

$$p_1=\frac{17}{100} \qquad p_2=\frac{20}{100} \qquad p_3=\frac{25}{100} \qquad p_4=\frac{20}{100} \qquad p_5=\frac{18}{100}$$

$$q_1=\frac{15}{100} \qquad q_2=\frac{18}{100} \qquad q_3=\frac{24}{100} \qquad q_4=\frac{22}{100} \qquad q_5=\frac{21}{100}$$

$$p=\left(\frac{255}{10^4}+\frac{360}{10^4}+\frac{600}{10^4}+\frac{440}{10^4}+\frac{378}{10^4}\right)$$

$$p=\frac{2033}{10^4}=0{\cdot}2033.$$

This is a departure from the assumed value of 0·2 or 1/5 which is quite unimportant.

Graph III, No. IV shows a case in which G.J. was the percipient. It does not pair with No. III, which is a record taken independently from the mechanical selector, but with another table in which G.N.M.T. was the operator which is not shown in the graph, but in which the frequency totals were 23, 23, 23, 18, 13. The probability of success, when worked out by the same formula, comes to 0·1995, showing, instead of a rise owing to the human departure from random choice, a slight but quite insignificant fall. Such frequency preferences as actually occur are such as to cause only quite negligible deviations from the assumed 1/5 value for the probability of success. Indeed, frequency preferences in order to make an important difference would have to be glaringly obvious. If both the operator and the percipient chose one number fifty times out of a hundred, the odds are four to one against their choosing the same number. If they chose different numbers, distributing their choices as evenly as possible over the other four, the result would be actually to *reduce* the probability of success from 0·2 to 0·1719. Only if they chose the same number would the probability of success be raised. It would be raised to 0·3126; and it is interesting to note that this would be just about the value it would have to have in order to account for the successes with the Pointer Apparatus by frequency preferences. And no one who has used the apparatus could suggest that this kind of preference on both sides could pass unnoticed.

Number Sequence-Habits. There might be no particular frequency preferences on either side and yet there might be sequence number-habits. Both sides, for instance, might constantly use the order 3, 2, 1, 4 or some such order of sequence.

Here again, such a habit would make no difference if it were only on one side ; and again, also, it would make no difference on both sides unless the sequences coincided. It could only be effective on those occasions on which there was a success with a 3. Again, to think of an exaggerated case as an illustration, suppose both sides went continually from one end of the row to the other, they could easily miss each other altogether by being out of step. Only if they coincided when going in the same direction would they score 100% successes. With any sequence-habits, in order to alter the probability of success, (1) operator and subject must have identical habits, and (2) the sequences of both must start together from a success. So that it will be realised how easy it is to exaggerate the importance of number-habits by talking about them loosely.

Another important point about sequence habits is that the Commutator would break them up. If we take, for instance, the straight sequence 1, 2, 3, 4, 5, this would be transformed by the Commutator into 3, 5, 2, 1, 4 ; 2, 3, 1, 5, 4, and so on in seven ways, so that, if the percipient did manage to coincide with a habit of the operator's, the Commutator would render this habit worse than useless ; and if sequence number-habits had been the cause of scoring, the Commutator should have brought the scores down to chance values. Hence, neither frequency nor sequence number-habits will account for the rates of scoring actually attained.

Those who have been good enough to give me valued criticism and advice have urged that the recording mechanism of the apparatus ought to have been constructed so as to record each box opened and each lamp lit. With this criticism I am in agreement up to a point. That is to say, I agree that it is necessary to have a representative selection of blocks of trials recorded in this way, but I do not think that it would be either necessary or convenient to have all experiments so recorded. The wealth of detail would be cumbersome as the records ran into hundreds. And if only a few are required they can be obtained by getting two recorders to check by hand. But undoubtedly automatic checking is better, and the ideal arrangement would be to have both kinds of recorder and to use either at will.

The reason why a complete recorder of this kind was not provided from the beginning is that the electrical apparatus, like, I suppose, most pioneer appliances, was a patchwork growth rather than a complete plan. And in the beginning, a mechanism for registering every trial would have been more elaborate and expensive than the dual recorder actually used, and more difficult to install. There were too many mechanical problems to be dealt with at first for more to seem desirable. And later, when the need for the complete recorder became clearer, it seemed an unwise policy to ask the percipients to stand aside while the apparatus was dismantled for the considerable time which would have been necessary to get a new piece of apparatus to work satisfactorily.

Chance in Connexion with the Experiments

In the kind of experimental work which is here being dealt with, the estimation of the probability that any result is due to chance is fraught with special difficulties, and it is no easy matter to present these probabilities in any satisfactorily clear and logical fashion. But there is on the other hand a compensating fact, for the anti-chance figures in most cases turn out to be so large as to render the theoretical difficulties formal rather than practical, and to leave an investigator who is in touch with the phenomena in no doubt as to the verdict. As an example of the kind of difficulty which arises, we may consider how to deal with the results of an experiment which has been done on one particular day. It may be that these results show significance when dealt with alone; but the difficulty is that if we deal with this day's results alone they may be regarded as a selection taken from a series which ought to have included the results obtained on other days as well. The only fair way to deal with these results, according to this argument, would be to take the chance-probability figure for the results of all the days together. Then, if the results on the other days had been blank or poor, it might quite possibly be found that the day which showed significant results when considered alone, would have to be regarded as a freak of chance which could occur in the larger total, showing in itself no significance on the whole. In the same way, a man who, while on his way to visit a friend, stepped into the very 'bus in which that friend was travelling, might think at first that some special influence had caused the meeting; but when he considered the large number of occasions on which such a coincidence might have happened but did not, he would see that it could well be accounted for by chance.

There is thus in the estimation of chance the principle which makes it necessary to deal with the relevant whole: but the difficult question in practice is to decide what the relevant whole is. If the method of estimation deals with too narrow a basis, it runs the risk of taking results to be significant which are really due to chance: but if it deals with too wide a basis, it runs the risk of confusing the issue with figures which are meaningless.

It is necessary to divide the experiments into groups, each group consisting of results obtained under the same conditions, and to calculate the chance-probability figure for each group. But it is obvious that the different groups must be kept apart and not included together in the same total, otherwise the chance-probability figure will have no intelligible meaning. For example, one group of experiments may have been done under condition A while another group has been done under condition B; and it may be that A was a condition which permitted the phenomenon to occur, while B was a condition which prevented it from occurring. Clearly a probability figure for A and B combined will not be illuminating. The difficulty which occurs in practice is that of being sure that, in dealing with what appears to be a group, we are not in reality dealing with a composite group containing more than one set of conditions. There is a difficulty in this subject which does not occur in physics, for the relevant conditions are not only physical and external; they are also psychological and internal to the subject; and, of the two sets of conditions, the psychological ones are probably the most important. But as these lie to a great extent beyond the operator's control, they must perforce be ignored for the purpose of probability calculations. That conditions exist within the subject which profoundly influence the results, is shown by the fluctuations in the rate of scoring which occur when the external conditions are kept the same. And even when dealing with the external conditions alone there are apt to be minor sub-divisions which have to be discounted to avoid undue complication.

The principle which is here adopted is to regard as a group all experiments which have been done under the same general external conditions and to take the temporal limits of such groups as coinciding with the period under review. (See Table XXV.)

These considerations lead to the conclusion that chance-probability figures should only be used to indicate the odds against the results of certain groups of experiments being due to chance in a way which may be called "by and large." It is, moreover, dangerous to proceed to induction and to correlate the anti-chance figures with the experimental conditions in such a way as to infer causal relationships ; and it is dangerous to lay stress on anti-chance figures which are near the point of significance. It is necessary to use common sense, and above all to take into account the guiding indications which arise from contact with the subject.

But it is not a case of dealing merely with the chance-probabilities of group totals ; for a striking feature of these experiments is not the totals themselves only, but also the manner in which the totals arise. To illustrate this, tables have been given showing the comparative rates of scoring belonging to two different sets of experimental conditions carried on simultaneously, but not extending to the full temporal scope of the groups. The comparisons are made, as a rule, by giving not the chance-probability figures, but the percentage rates of scoring ; but sometimes, when they seemed to be of interest, probability figures have been given for series of experiments which are less than an entire group (as in Table XV). The reader, if he distrusts these, may compare them with the figures for the group-totals in Table XXV.

Experimental Results with the Electrical Apparatus

As soon as this apparatus was sufficiently complete, experiments were begun with it, a mechanical Selector being used instead of the method by which the operator selected the events by his own choice. The Selector first tried was impartial as amongst the five lamp circuits, but it was noisy and unreliable in its working and had the effect of putting the subject off to such an extent that she soon felt a strong dislike for it. She also said that it felt " mechanical " and had not the " human feeling " which the Pointer Apparatus had. The subject's table, also, was some 12 feet from the operator's and this gave her a strange and lonely feeling. These things, perhaps, sound childish ; but the fact is that the state in which the scoring is done is easily influenced by feelings of this kind and they have to be recognised.

A set of experiments was carried out with this early form of Selector between 13th May and 1st June 1935, but without any positive results. As an additional drawback, the subject suffered from an influenza cold for ten days of this period. The results were as follow :

Table VI

Subject G.J. Operator G.N.M.T. Auto. Recording

No.	*T*	*S*	*D*	%	Conditions
71	8000	1540	−60	19·25	First Selector.

As the trials in these experiments went on without success, the subject became more and more disheartened and this no doubt militated still further against success. My policy of persevering with the same condition through as many as 8000 trials was a mistaken one.

Those who took part in these experiments will be referred to by their initials as follows:

G.J.	Miss G. M. Johnson.
L.H.	Mrs Hemingway.
B.-M.	Mrs Bramley-Moore.
C.M.T.	Mrs Tyrrell.
G.W.F.	Mr G. W. Fisk.
G.N.M.T.	Mr G. N. M. Tyrrell.

Introduction of Keys

Since the Selector had so far been a failure, it was decided to re-introduce the human element by providing the operator with five Keys for lighting the lamps in the boxes, so as to return as nearly as might be to the conditions of the Pointer Apparatus. The only difference now from the latter would be that the operator pressed a key which he chose at random instead of putting a pointer into a box; and the percipient would see the lamp alight inside the box instead of seeing the pointer. The Keys, as has already been said, had been rendered silent by the provision of mercury contacts.

The subject's table was brought near the operator's to help to restore the feeling of "human confidence," which the subliminal seemed to demand, being so placed that the screen on it, 30 inches square, hid the operator and his table completely from the percipient. The screening was quite satisfactory, but it should be noted that it was afterwards made doubly effective by providing a screen round the operator's table as well. At this time, before the second screen was added, the opportunity (if opportunity it can be called) of getting normal information from the operator's table was at its best, but all the results were failures.

TABLE VII

3rd to 7th June, 1935

No.	*T*	*S*	*d*	%	Conditions
72	1305	276	+15	21·1	Keys and Lamps.

This is entirely a chance result, yet the conditions differ from those with the Pointer only in the fact that the operator does not know what boxes the percipient is opening. It might be said that this supports the hypothesis that, with the Pointer Apparatus, the operator had been watching the percipient's habits of opening boxes and had fallen in with them, either consciously or unconsciously. But the sequel negatives this.

Reversion to the Pointer

As the Keys and Lamps had failed, I resolved to go back to the beginning again and use the Pointer. The original Pointer Apparatus not being available, as it was being used elsewhere, I took the lamps out of the boxes and muffled a pencil by wrapping a piece of flannel round it, and used it as a pointer. It was not quite so silent as the original pointer as the boxes were not padded, but it was moderately so.

The subject, after a brief holiday, came back and realised this as the old Pointer Apparatus again and sat down to it with the " don't care " feeling, which was bred of the confidence of past successes ; and success returned.

TABLE VIII
10*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
73	100	35	+15	35	Pointer inside Boxes.
74	100	45	+25	45	,, ,, ,,
75	100	23	+3	23	,, outside ,,
76	100	21	+1	21	,, inside ,,
77	100	21	+1	21	,, outside ,,
78	20	11	+6	55·0	,, inside ,,
79	80	31	+15	58·7	,, outside ,,

Now, when the pointer was thrust into the boxes the conditions were exactly the same as with the original Pointer Apparatus, except that it was not quite so silent.

When the pointer was put *outside* the box the padded end was merely rested on the shelf behind the box and opposite the opening. The percipient could not see it here because of the depth of the box, and, in fact, never saw anything.

The return to the old conditions partly restored the scoring, and the first criticism would be that this was due to the sound of the pointer entering the box, for Nos. 73 and 74 show success when it is put in and No. 75 shows failure when it is put outside. But No. 76 shows failure when it is put inside again ; and Nos. 78 and 79 which are a split run of 100 show a high rate of success *both* with the pointer inside and outside the box.

It is instructive to notice here how success and failure do not range themselves with changes in the external conditions of the experiments but cut right across them. Thus, if the subject were scoring by the sound of the pointer No. 76 should go up and No. 79 should go down. But if success comes at all, it seems to be oblivious of external conditions.

Return to Lamps and Keys

Since scoring had again appeared, I went back to the Lamps and Keys, but again they proved a failure.

TABLE IX

10*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
80	406	93	+12	22·8	Lamps and Keys.

What the subject said about them was that, when she came back to the Lamps and Keys, the " don't care " feeling she had with the Pointer left her, and she felt concerned about the result. So failure again supervened. We therefore went back to the beginning again and used the Pointer.

TABLE X

11*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
81	100	23	+3	23	Pointer inside Boxes.
82	100	39	+19	39	,, ,, ,,
83	20	9	+5	45	,, ,, ,,
84	80	31	+15	38·7	,, outside ,,

The pointer again restored the scoring and again it did not matter whether it entered the boxes or not. Hence, scoring cannot have been due to the sound of the pointer when it did enter. If any normal means of scoring was being used, the difference between the scores in Nos. 81 and 82 remains unexplained, and the sudden drop after Nos. 73 and 74 in Table VIII. A score of 39 in 100 cannot be due to chance.

Another return to the Lamps and Keys was then tried.

TABLE XI

11*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
85	100	13	−7	13	Lamps and Keys
86	100	26	+6	26	,, ,, ,,
87	100	16	−4	16	,, ,, ,,
88	100	26	+6	26	,, ,, ,,

Here was again failure when I went to the Lamps and Keys. Of course, to do this, I went back to the operator's table to work the Keys, whereas I worked the Pointer from the back of the percipient's own table. G.J. (or her subliminal) did not like the new condition of my being further away ; so I removed the Keys to the back of her table, where they were, if anything, more effectively screened than on my own table.

However, I did not at first use the Keys but went back to the Pointer again, removing the Lamps from the boxes. But, in order to take a step in the direction of accustoming the subject to the "mechanical feeling" of the new apparatus, I connected it so that the automatic Recorder would work even with the Lamps removed. This Recorder makes an audible click at every trial. But for this, the conditions were exactly the same as those with the Pointer experiments.

Table XII

11*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
89	100	40	+20	40	Pointer inside Boxes.

The Pointer had immediately restored success, and I went straight on to replace the Lamps in the Boxes, and stopped using the Pointer : and then, at last, after 3100 trials, success had began to carry over from the Pointer to the Lamps.

Table XIII

No.	*T*	*S*	*d*	%	Conditions
90	94	16	−2·8	17·0	Keys on Percipient's Table and Lamps.
91	94	43	+24·2	45·7	,, ,, ,, ,,
92	120	52	+28·0	43·3	,, ,, ,, ,,

A special test was given to the apparatus after these trials and it was found to be working perfectly.

This is very instructive, for the condition with the Lamps and Keys, which has now begun to give success, is exactly the same as that which had given persistent failure for so long. The process is illustrated graphically in Graph I, which shows the condition with Lamps failing at first alongside the successes with the Pointer, and gradually rising to parity with it. It shows the gradual yielding of the subliminal and its slow acceptance of the new conditions, and challenges an explanation in terms of anything but the faculty of Extra-Sensory Perception. It also provides a lesson on the danger of forming hasty conclusions in this inquiry. At first everything begins to look as if the Pointer must have afforded some loophole which has been stopped by the stricter conditions of the new method with the Lamps. And if the machine did not provide a means of going on through a large number of trials, a critic might well go away with the impression that this was so. But perseverance shows that it is nothing of the kind. It is merely an internal resistance to change.

But it has been mentioned that there was one change in the external conditions which was made before scoring began with the lamps. The Keys had been moved from the operator's to the percipient's table; and there might be some lingering doubt as to whether this had given some hints by sound. But the next table shows a return to failure with the Lamps and Keys in this position.

TABLE XIV

12*th June*, 1935

No.	*T*	*S*	*d*	%	Conditions
93	331	64	−2·2	19·4	Keys on Percipient's Table and Lamps.

Any normal assistance to scoring would be a constant factor and would not be likely to oscillate as the actual results do.

The next block of trials shows the condition of the Lamps accepted and very high rates of scoring attained with them, although there is still the usual sporadic rise and fall.

TABLE XV

13*th*-14*th June*, 1935

No.	*T*	*S*	*d*	%	*X*	*P*	Conditions
94	97	37	+19·4	31·8			Keys and Lamps
95	100	26	+6·0	26·0			,, ,, ,,
96	76	26	+10·8	34·2			,, ,, ,,
97	100	42	+22·0	42·0			,, ,, ,,
98	100	20	0	20·0			,, ,, ,,
99	59	17	+5·2	28·8			,, ,, ,,
100	100	30	+10·0	30·0			,, ,, ,,
101	632	198	+71·6	31·3	7·1	10^{-11}	(Totals.)

In spite of the oscillation between chance and high values, the totals of this block of results show clearly that it is just as possible for G.J. to score with the Electrical Machine as with the Pointer

Apparatus, once it has been accepted : and scoring with it is maintained in future.

Health of the Subject. Notes taken at this period show that the Spring of 1935 was wet and cold, and the subject seems to have had a succession of colds throughout May and the first half of June, which no doubt acted generally as a delaying factor.

Commutator

From 15th June, 1935, a Commutator was introduced as an integral part of the apparatus. This instrument has been described in the technical account of the apparatus and it will be sufficient here to state its function. Up to this point, the wires from the operator's five Keys had led directly to the five Lamps in the boxes, being connected to them in a straightforward, one to one order. Hence the operator knew, when he pressed any key, which lamp he was lighting. This made telepathy possible between operator and subject and caused all results to be " undifferentiated " as between telepathy and clairvoyance. It also left open the criticism (however highly improbable) that the operator might convey information unconsciously to the subject by " unconscious whispering " etc. The Commutator is an electrically operated switch, enclosed in a box, which transposes the connections between the Keys and the Lamps in ways that may be known or unknown to the operator at will. If unknown, there can be no telepathy, since he does not know what lamps he is lighting.

Experiments with Keys will be divided into Known (K) and Unknown (U) classes. Experiments with the mechanical Selector will be denoted by (Sel). It is possible that either (K) or (U) may be " Mixed " or " Straight."

Table XVI shows the effect of introducing the Commutator.

TABLE XVI
15th June, 1935

No.	*T*	*S*	*d*	%	Conditions
102	84	17	+0·2	20·2	(K) " Straight "
103	92	44	+25·6	47·7	(K) " Straight "
104	100	41	+21·0	41·0	(K) " Straight "
105	100	14	−6·0	14·0	(K) " Mixed "
106	100	45	+25·0	45·0	(K) " Straight "
107	100	26	+6·0	26·0	(K) " Mixed "

The condition so long rejected by the subject's subliminal of scoring with the Lamps still goes on here attaining high rates of success. The new condition now is the mixing of the connexions by the Commutator, and Table XVI shows that it is not favoured with high scores.

I must apologise here for rather stupidly introducing an unnecessary complication by looking into the Commutator-box each time to see whether it had stopped on a " Straight " or a " Mixed " contact. This means that with logical strictness, the telepathic element is not removed from the " Mixed " cases. I did not consciously remember the various mixed connexions, which were noted in a book ; but I had known them, so that it could be argued that telepathy was possible in these cases as well as in the " Straight " ones. Strictly speaking, the important difference is between the Known and the Unknown cases, and Straight and Mixed arrangements, when equally Unknown, may be classed as the same. In Table XVI the subject knew which arrangement was being used, which might account for the differentiation between them.

The difficulty of comparing the Known with the Unknown conditions lies in the continual variations in scoring that one gets with the same condition. Therefore in the next table I tried quickly changing from one condition to the other in the middle of a run without letting the subject know which half was which, although I could not prevent her knowing that a change was being made on account of the slight, necessary pause.

TABLE XVII

15*th June*, 1935

No.	*T*	*S*	*d*	%		Conditions
108	50	3	−7	6		(K) Straight
	50	7	−3	14		(K) Mixed
109	100	24	+4	24		(K) Mixed
110	50	22	+12	44		(K) Straight
	50	13	+3	26		(K) Mixed
111	100	17	−3	17		(K) Mixed
112	100	53	+33	53		(K) Straight
	(Totals for Tables XVI and XVII)					
113	1076	326	+110·8	30·3	$X=8{\cdot}4$	$P=10^{-16}$

These results are extraordinary. The subject seems to have known telepathically or guessed which were the Straight conditions and to have done everything possible to avoid scoring with the Mixed conditions. No. 108 is the lowest score she has ever obtained and appears to be deliberately subnormal. It almost looks as if the subliminal was not sure which half embodied the new, Mixed condition, and resolved to score as low as possible with both in order to be on the safe side. In No. 109 there is a chance to score with the Mixed condition. In No. 110 the Straight condition seems to have been picked out and the score goes up, while the Mixed condition is given a chance score. Then the Mixed condition, tried alone for the second time, is again met with a chance score. Finally the Straight condition, offered alone, is seized on for the highest score on record (No. 112). That the subliminal had been very active during this trial was shown by the fact that the subject had to lie down afterwards, feeling the nervous strain. This may have been partly due to the subject's state of health, for a contemporary note says, "G.J. still with heavy cold, continually sneezing. Weather bad; raining every few minutes. Cool for June; almost cold."

The next batch of results on 18th June shows the same thing continuing, only towards the end there is one score of 30% with the new, Mixed condition as an indication that it is about to be accepted. On 19th June, 675 further trials showed the same tendency to score with the Straight and not with the Mixed arrangement.

Electric Sounder

On 19th June an Electric Sounder was installed for giving the signal for opening the boxes instead of saying the word "In." It was fortunately accepted almost at once and was always used after this date.

A break in the experiments then occurred until August 1935. Then the subject came back after a holiday and tried with the Commutator in a Mixed, and this time also Unknown, position and scored immediately.

Table XVIII

22nd August, 1935

No.	*T*	*S*	*d*	%	Conditions
123	90	35	+17	38·9	(U) Mixed

The probability of this being a chance result is about 5 in a million. The condition of Mixed connexions within the Commutator is at length beginning to be accepted after a long period of reluctance, just as the Lamps were. And on this particular occasion the connexions were not only Mixed but also Unknown.

During the remainder of August the scoring was poor, both with the Straight and Mixed arrangements.

TABLE XIX

24th to 31st August, 1935

No.	T	S	d	%	Conditions
124	1656	371	+39·8	22·4	(U) Mixed
125	700	135	−5·0	19·3	(K) Straight

The best scoring is here with the Mixed and Unknown condition, the figures for it being just significant (0·007); while the figures for the Straight condition are pure chance.

G.J. went away for a short time, during which certain improvements were made to the apparatus.

(1) The Keys were provided with deeper mercury cups, so that by no possibility could brass come into contact with brass.

(2) An electrically worked Sounder was placed in front of the subject under the edge of the table, which gave an audible click as the signal for opening a box. It was worked by raising a small lever beside the operator's Keys, which, at the same time, worked a counter to register the trials. Henceforward, this Sounder was always used, and the operator spoke no word except in the case of Precognitive trials.

(3) A screen, reaching from the table top to the floor was inserted, so that the legs and feet of the operator could not touch those of the percipient.

(4) A Delay-action Relay was installed, the purpose of which has already been referred to and will also be described later.

These appliances, together with the Commutator, were designed to rule out every possibility of normal sources of leakage and also every chance of leakage through visual or auditory hyperaesthesia.

G.J. came back well on 14th September, 1935, but most unfortunately, through an accident, caught a cold on 15th September, which lasted till 11th October. The results secured in the meantime must be looked at from two points of view: (1) from that of the progress of scoring with the Unknown Mixed condition, (2) from that of the introduction of the Delay-action Relay device.

Comparison of Known and Unknown conditions between 18*th Sept. and* 12*th Oct.* 1935.

TABLE XX

12*th October*, 1935

No.	T	S	d	%	X	P	Conditions
126	1900	464	+84	24·4	4·8	10^{-6} to 10^{-7}	(K)
127	4200	1018	+178	24·2	6·8	10^{-11}	(U)

This shows clearly that the Unknown condition of using the Keys has now become accepted by the subconscious of the subject and is being used on an equality with the Known condition, after the long resistance shown in its introduction on 15th June and after. Here is a repetition of the same process which for so long a time refused to score with the Lamps instead of the Pointer, but finally accepted them. There is a strong confirmation in all this that the subject has not been scoring by any normal means; for, if there had been any source of leakage, such as unconscious whispering or the like, or any number-habit raising the probability of success, which must have been cut out by the use of the Commutator, then the score would have fallen to chance whenever the Commutator was used.

Such a normal explanation would not account for the gradual rise in scoring with the Unknown condition until it rose to parity with the Known condition. Each time that a new external condition is introduced it is met with failure at first and success later. There is, therefore, some condition which is gradually changing; and this is clearly not an external condition; therefore it must be an internal condition. The whole process is shown graphically in Graph II, where the dotted line represents the Unknown condition and is far below the full line at first, but rises together with it at the end.

The Delay-action Relay

The same results have to be examined from the point of view of the delay-action. The Delay-action Relay operates as follows. When the operator depresses one of the Keys, a circuit passing through one of the lamps is selected: but the lamp is not lit because there is a break in the common return-wire of the lamp circuits. When the box-lid is opened, this energises a relay, which closes this gap in the lamp-circuits and the pre-selected lamp is lit. Thus, the selected lamp is actually lit by the percipient's own action in opening the box-lid. (See DAR on the diagram of connexions, A.) The effect of this is three-fold: (1) It removes the ostensible event which is exposed for the clairvoyant faculty, viz. the lamp lit in the box beforehand; so that, when the Commutator is used as well, the knowledge required for telepathy from the operator is not there; and also the ostensible event presented for clairvoyance is not there either. (2) It removes all possibility of knowledge being gained through the percipient's normal senses, whether hyperaesthetic or not. There is first the auditory sense. It will be remembered that it was said when the apparatus was being described that the five relays in the lamp-circuits are each connected in circuit with its lamp, and that it was necessary to silence these relays very carefully, because, if the sound of each could be heard and was distinctive and recognisable, it would give a clue to the lamp which was being lit. With the delay-action device, these relays, like the Lamps themselves, are brought into action by the percipient's own action in raising the box-lid. Therefore, if it be supposed that these relays are hyperaesthetically audible, they could now give no help in scoring. (3) The question of the leakage of light past the box-lids is solved by this device, since there can be no leakage of light before the lamp is lit. And the same applies to any supposed radiation of whatever kind which might be supposed to pass from the lamps through the wood of the boxes to the percipient, there to affect some unknown sense-organ in some unknown way. Or to any guide given by a supposed rise of temperature of the box-lid when the lamp is alight inside, even were this not ruled out by the speed of working. All possible help from sense-stimuli of any sort is rendered impossible by the use of the Delay-action Relay.

It is interesting to speculate as to what differentiation is left for the percipient's faculty to work on when the delay-action is used. When the key is closed by the operator, although no current flows, the wires connected to the Keys are charged up as far as the break in the circuit at the relay ; and it seems at first sight as if the lamp that is going to be lit will be raised to a higher potential than the others and that there might be this differentiation for the faculty to work on. But an examination of the connexion shows that this is not the case. There is a common wire connecting all the Lamps together, so that, when any key is pressed, all the lamps are charged up simultaneously. The only differentiation is the closing of the gap in the key pressed ; and in order to know by clairvoyant perception which lamp will be lit, it is necessary to know what are the connexions inside the Commutator.

Since this group of results is fairly balanced as between the Known and Unknown classes, it may be used as one group for comparison between Delay-action and Non-delay-action classes.

Table XXI

18th September to 12th October, 1935

No.	T	S	d	%	X	P	Conditions
128	2450	620	+130	25·3	6·56	10^{-10}	Non-delay action
129	3750	856	+106	22·8	4·33	10^{-5}	Delay-action

The figure with the delay-action is not as good as that without it, but it is amply significant. Only the loosest reliance can be placed on a comparison of external conditions based on a comparison of such figures, since there is no guarantee that conditions other than the one which the experimenter has deliberately altered have remained constant. Probably the nearest approach which can be made towards comparing external conditions in this kind of work is attained by altering the condition in the middle of a block of 100 trials. This was done in order to compare the delay-action and non-delay-action conditions, as the change from one condition to the other can be made by merely moving a small switch which does not interrupt the experiment. Sometimes the delay-action was introduced into the first 50 trials and sometimes into the last, the percipient being always in ignorance of which was which.

TABLE XXII

Trials between 12*th October*, 1935 *and* 25*th January*, 1936

No.	T	S	d	%	X	P	Conditions
130	1090	306	+88	28·0	6·7	10^{-10}	Non-delay-action
131	1105	302	+81	27·3	6·1	10^{-9}	Delay-action

These figures give about the best comparison that can be made between the two external conditions, and the result shows that, although both conditions give highly significant results, there is practically no difference between them. However the scoring is done, therefore, it cannot be by normal sense-perception or by hyper-aesthesia.

It is interesting to note that, although with the delay-action, a third new factor has been introduced, there seems to have been no protest against it as there was at first against the Lamps and against the Commutator. It is accepted from the outset, or rather ignored, which is remarkable, as one would have thought that it would make more difference to the operation of any faculty in the least like sense-perception than the mere substituting of a lamp for a pointer. The resistances appear to be rather against a condition giving the feeling of strangeness than against what we should consider to be solid difficulties placed in the way of Extra-Sensory Perception itself.

Improvements to the Apparatus

On 16th November, 1935, the Keys, Synchronising Lamp and Counter were removed from the percipient's table and put back on the operator's table. A screen 5 feet high from the floor was fitted round the two sides of the operator's table which were outwards towards the room, the table being situated in the corner so that the operator sits back to the wall. The wires connecting the two tables were lengthened so that the tables could be placed as much as 8 or 9 feet apart.

During the early part of January the contacts of the box-lids were renewed, more efficient ones being substituted for those provided by the maker. The resistance-board of lamps was covered with a metal screen; and early in February, all the circuits of the apparatus were made independent of one another so as to increase its reliability.

Exposure of the Event for Clairvoyance

At the usual rate of working with G.J., 100 trials occupy from 60 to 70 seconds; and, at this speed, operator and subject cannot help working in a steady rhythm, which is no doubt helpful to the phenomena, but which soon results in the box being opened at the same instant that the key is pressed, without any interval during which the lamp is alight before the box is opened. It therefore seemed desirable to test whether any difference could be perceived between trials in which the lamp was alight well before the box was opened (when the clairvoyant faculty has a well-marked event to work on) and in trials in which the lamp was not alight until just after the box was opened. Accordingly, some blocks of trials were done slowly, the key being pressed from 3/4 to 1/2 second before the signal was given to open the box. In the middle of the run, *i.e.* after 50 trials, the switch was thrown over which brought the delay-action into operation, and prevented, without interrupting the series, the lamp from being lit until the box was opened. Sometimes the delay-action was in the first half of the series and sometimes in the second, the subject never knowing in which half it was. Telepathy on the part of the operator was in all cases ruled out by the use of the Commutator. This gave the following result:

TABLE XXIII

12th October to 23rd January, 1936

No.	T	S	d	%	X	P	Conditions
132	845	242	+73	28·6	6·3	10^{-9}	Lamp exposed before box opens
133	855	224	+53	26·2	4·5	10^{-5} to 10^{-6}	No lamp till after box opens

The results when the lamp was " exposed " are a little better than those in which it was cut out ; but there is no evidence that the lamp alight in the box beforehand is essential to success. Both results are far above chance. This tells us that success is possible when there can be no telepathy and when there is no ostensible object for clairvoyant perception until afterwards. It has the appearance of precognition ; but a sufficiently elaborate theory of combined clairvoyance and telepathy might be put forward instead.

" *Precognitive* " *Results*

Certain blocks of trials were done in the following way. The box was opened each time by the percipient about half a second before the key was pressed. The Commutator was used on every occasion to eliminate telepathy ; and the tape record shows in the case of every success that the box was in fact opened before the key was pressed by the amount that the trial line overlaps the success line (sample of tape).[1] These trials are called " Precognitive " because they appear to be so ; they may be and, I think, probably are ; but it is not stated that there is no possible alternative explanation. The results were :

TABLE XXIV

13th October to 13th February, 1936

No.	T	S	d	%	X	P	Conditions
134	2255	539	+88	23·9	4·6	10^{-5} to 10^{-6}	" Precognitive "

Since these cannot be due to chance, it is of interest to ask by what other means, if not by precognition, they could have been obtained. There can have been no telepathy because the Commutator was used on all occasions. If there were contemporary instead of precognitive clairvoyance, it would have had to be the operator who exercised it, and not the subject. The operator must have known, perhaps by telepathy, which box the subject had opened. After that, the operator would have to resort to clairvoyance to penetrate into the recesses of the enclosed Commutator, there to discover the cross-connexions linking the Lamps to the Keys. Having sorted out the right connexions, the operator must then have pressed the key corresponding with the box which the subject had opened.

The great difficulty in this explanation is that the writer was in most cases himself the operator and cannot believe himself to be endowed with the psychic powers which this explanation demands. These powers are entirely absent when he occupies the percipient's

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chair; and, if he had them, surely they would not allow his subjects to fail as most of them do!

TABLE XXV

Period covered, 13th May, 1935 to 30th March, 1936

Operator G.N.M.T. Percipient G.J.

No.	T	S	d	%	X	P	Conditions
135	8700	1666	−74	19·1	—	—	Mech. Selector
136	3261	707	+55	21·7	2·4	0·008	Keys Mech. Seld. Nos. unknown to Op.
137	17842	4612	+1044	25·8	19·5	10^{-80}	Keys known to Op.
138	10050	2399	+389	23·8	9·4	10^{-20}	Keys unknown to Op.
139	5768	1379	+226	23·9	7·4	10^{-12}	Keys Delay Action
140	2255	540	+89	23·9	4·7	10^{-15}	Keys Precog.
141	47876	11303	1728	23·6	19·5	10^{-80}	Total

Table of Totals

The Tables so far given have been for purposes of comparison or to illustrate particular phases of the experiments. Table XXV contains all the results obtained under the conditions specified under each heading between 13th May, 1935 and 30th March, 1936, which is the period dealt with in the present report. But these groups entail a certain amount of cross-classification, since it is impossible to form groups which do not contain some element in common, and this robs the figures of complete clarity. For example, although No. 139 contains no results which appear in Nos. 137 and 138, it does consist of cases in some of which the connexions to the lamps were known and in others unknown. It therefore cuts across Nos. 137 and 138 and at the same time it withholds results which would have been included in these latter, but for the special, additional feature of the delay-action. There are also important conditions, such as changes in the emotional tone or health of the subject, which undoubtedly influence the result and which would cut right across the above classification; and these have been omitted. Hence it comes about that any attempt at a causal analysis of the results by dividing them into groups and attributing success to the various groups in proportion to the rate of scoring is defeated by the confusion introduced by the inevitable cross-grouping. Except under very favourable circumstances, and then only very roughly, group-conditions must not be correlated with rates of scoring.

There is a danger in publishing such tables as the above that the reader may extract more information from them than they contain. The most favourable case for comparison is between Nos. 138 and 139, for there the condition was in some cases quickly changed in the middle of the groups of trials without the subject's knowledge, and the indication is that the difference in the conditions, though profound, made no appreciable difference to the rate of scoring.

Little more can safely be inferred from Table XXV than that some factor other than chance has manifested itself in all cases other than that in which the mechanical Selector was used. It looks at first sight as if the mechanical Selector must have imposed some condition which rendered scoring with it impossible. But one learns to be wary in judging from first appearances. The reader is referred to the remarks about the Selector experiments which were made on p. 135. Also the results given in No. 136, which were made, not with the Selector itself, but with numbers taken from the Selector and transmitted through the Keys, have attained significance. These experiments, although covering the period under review, were in fact all made between 4th and 30th March, 1936, which was a period of low scoring all round, and it would be unsafe to regard them as representative.[1] Again, results with mechanically selected numbers were obtained when earlier experiments were being made with the Pointer Apparatus in 1935, but which do not come under the present period of review and these showed the much higher rate of scoring of 28·5%. On the whole it would be safer to suspend judgment for the present on the negative result of No. 135 and the barely significant result of No. 136 in Table XXV.

No. 141, which is the final total of all the previous groups in Table XXV, includes the whole of the results dealt with in this report. It must not be taken as indicating more than that the anti-chance figures shown in the separate groups are not in themselves chance fluctuations within the whole. This, indeed, is sufficiently obvious in any case. Since many different conditions are pooled together, the average rate of scoring in this table cannot be taken to mean anything in particular.

[1] Subsequently to the period covered by the report, further results were obtained in this class. About the middle of May, 1936, the inhibitory influence of the Fisk shock began to disappear and Miss Johnson showed signs of returning to her normal rates of scoring. Thus, between 4th March, 1936 and 2nd July, 1936, the scores for the condition in No. 136 were: Trials 7809; Successes 1841; Average rate of scoring 23·5%; X 7·89; P 10^{-14}.

Also, since 18th June, 1936, positive scoring has begun with the mechanical Selector itself. These results are of course impossible with the Fisk method of scoring.

Grouping of Successes

A point of considerable interest shows itself on the tape records. Where series of trials have been successful, the successes are distributed very unevenly, tending to bunch together in certain places; and it has also been noticed that these batches of consecutive successes correspond with times when the subject has the experience of losing herself in the experiment. It should, in principle, be possible to determine whether this grouping of successes is greater than could be accounted for by chance, but this problem has not yet been gone into. On the surface, it looks as if the above chance scores of the successful series of trials were due to these short periods of exceptionally high rates of scoring super-imposed upon the more evenly distributed successes due to chance.

Indication of the Deep Level of Extra-Sensory Prehension

The way in which the results have emerged, beginning with series of failures and leading up to later successes, show that there is a reluctance to accept new conditions, but that this is not due to any inherent difficulty of the perceptual act itself but rather to hindrances or resistances in the path of emergence. The indication is that the variations in performance are due to variations in the conditions on the lines of communication and not to conditions at the source of the perceptual process itself. Such a process must therefore take place deep down in the personality of the subject and there is no indication as yet that any condition has been discovered which materially affects it.

Other Percipients

Besides Miss Johnson, the three other percipients who scored above chance with the Pointer Apparatus were Mrs Hemingway, Mrs Bramley-Moore and Mr Miller. Mrs Hemingway's results will be dealt with below. Mrs Bramley-Moore did 1400 trials on the electrical apparatus, but only attained a chance score. Mr Miller has only quite recently begun experiments on the Electrical Apparatus and it is too early to say what the result will be.

Relation of Health to Scoring

It is extremely difficult to determine any clear relation between Miss Johnson's health and the rate of scoring with the apparatus, because her health often seems to be not of the best but in an undefinable way. The following table, however, taken from contemporary notes, shows how continually colds interfere with results.

Date	Remarks
13th May, 1935	Began work with Electrical Apparatus.
20th May, 1935	G.J. with headache.
23rd-26th May, 1935	G.J. in bed with bad cold and cough.
27th May, 1935	G.J. up but not well.
3rd June, 1935	G.J. lying on sofa with headache. Got up to do experiments.
12th-13th June, 1935	Slight cold.
15th-19th June, 1935	Heavy cold.
24th June, 1935	G.J. left for holidays.

The weather in May and the first part of June was exceptionally wet and cold. Unfortunately Miss Johnson and I had to be away at different times, which entailed a long break in the experiments.

22nd-31st Aug., 1935	Experiments resumed. Then G.J. went away for a short time again.
14th Sept., 1935	G.J. returned very well to resume work.
15th Sept.-13th Oct., 1935	G.J. caught severe cold and chill which lasted for a month.
13th-27th Oct., 1935	G. J. well during this period. Scoring good at average rate of 24·3%.
28th Oct., 1935	Shock of Fisk discovery spoils scoring.
5th-23rd Dec., 1935	G.J. again with cold and cough.

Ill health again supervened from the end of January to the 10th March, 1936. The only really good time during these two months was the fortnight in the middle of October, which was cut short by the shock of the Fisk discovery. This shows why work has been so slow.

A New System of Scoring

I now come to an interesting and rather extraordinary discovery which is due to Mr G. W. Fisk, who has kindly devoted a good deal of time and trouble to taking part in this investigation. Mr Fisk had acted as one of the 30 subjects tested with the Pointer Apparatus, but had been unsuccessful in scoring. Afterwards he acted as one of the agents with this apparatus when Miss Johnson was the percipient; and also as the agent with her in the experiments with Zener cards. The results of all these have been dealt with above.

It was when trying the card experiments with Miss Johnson that it occurred to him to try again as percipient, but this time with the Electrical Machine. He had evidently been thinking things over and the results he produced astonished us all.

TABLE XXVI

28th October, 1935

No.	T	S	d	X	P	Conditions
142	100	24	+4	—	—	(K)
143	100	35	+15	—	—	(K)
144	100	30	+10	—	—	(K)
145	100	27	+7	—	—	(Sel.)
146	100	31	+11	—	—	(Sel.)
142-144	300	89	+29	4·19	10^{-4}	(K)
145-146	200	58	+18	3·19	0·0006	(Sel.)

In Nos. 142-144, with the ordinary straight, known arrangement of Keys, he scored at an average rate of 29·6%. I then, without saying anything, switched on the Selector, and he continued to score with this at approximately the same average rate, viz. 29·0%. The odds against this latter score being due to chance are about ten thousand to six.

Mr Fisk's own explanation of his success was the system by which he was scoring. He selected one box and kept on opening it until a light appeared. Then he immediately went on to another box, which he chose at random, and continued to open that until a light appeared there also. He then went on to another, and repeated the operation through the series. I shall call this method the Fisk Flexible System. It was not at all easy to see why it should account for the high rate of scoring, and I was sceptical. Mr Fisk's theory was that, after a success had been obtained in one box, there was a slightly greater chance of obtaining one at another box than at the same again. But it was not at all clear why this should be so, since the operator would probably repeat a key about as often as the mechanical selector would repeat it. In fact, I felt sure that this was so in my own case as I had compared my own repetitions with those in the tests of the Selector and was conscious of making them about as often.

But the fact of the high scoring was plain; so on 2nd Dec., 1935, four methods of scoring were compared, Mr Fisk being the percipient. (1) The Fisk Flexible Method. (2) The same method, except that the passing on to the next box was done in a rigid and predetermined order, thus excluding all scope for guessing. I shall call this the Fisk Rigid System. (3) Going from box to box in a predetermined order without ever repeating a box. These three with myself as operator, using the Keys. (4) Method (1) with the Mechanical Selector. These gave the following results :

Table XXVII

No.	T	S	d	%	P	Conditions
147	200	62	+22	31·0	0·00002	Method (1)
148	200	59	+19	29·5	0·0002	Method (2) Mixed Cds.
149	300	71	+11	23·7	0·056	Method (3)
150	298	52	−7·6	17·4	—	Method (4)

This showed that the Fisk Rigid System scores nearly as highly as the Flexible System. It showed that the Selector causes the system to fail. And it showed that a rigid system which does not wait for the light to appear in a box before going on, fails to score above chance. The scoring therefore depends upon waiting for the light. These conclusions were confirmed by further experiments.

Mr Fisk and I then did two sets of experiments, using his Rigid System, in which we changed places :

Table XXVIII

No.	T	S	%	p	Operator	Percipient	Conditions
151	2000	607	30·3	0·303	G.N.M.T.	G.W.F.	Fisk Rigid System
152	1400	431	30·8	0·308	G.W.F.	G.N.M.T.	,, ,, ,,

In this table p stands for the Probability of Success ; that is, the number of Successes divided by the number of Trials.

It is very remarkable that these come out almost exactly the same, the inference being that Mr Fisk and I must space our selections of keys in just the same way ; and it is possible that every operator would do the same, and that there may be a constant difference in this respect between a human operator and a mechanical one.

Further experiments were made, which are not here recorded, repeating these results, and they bring out clearly one very striking difference between scoring which is done by assuming a false probability of success and scoring that is done by an E.S.P. faculty. The former is perfectly reliable and occurs every time an experiment is made. The latter is most unreliable and is continually varying.

It was puzzling to know in what way the human selector differed from the mechanical one, and four blocks of 100 trials, in which I acted as operator, were checked completely, key by key and entered on squared paper in the form of a graph. These gave the following information :

Frequencies

TABLE XXIX

13th February, 1936

FREQUENCY OF SELECTION OF KEYS

Operator G.N.M.T.

No.	Block	1	2	3	4	5
153	1	20	22	24	19	15
154	2	19	22	25	18	16
155	3	18	19	24	21	18
156	4	17	20	25	20	18
157	Averages	18·5	20·7	24·5	19·5	16·7
			MECHANICAL SELECTOR			
158	1	26	23	20	15	16
159	2	23	19	20	19	19
160	3	23	16	23	18	20
161	4	19	20	25	19	17
162	Averages	22·7	19·5	22·0	17·7	18·0

There is a slight tendency on my part to favour the middle Key, but it is not enough to be serious. Nor do the graphed records show any signs of sequence number-habits.

Having graphed records both from the human operator and from the mechanical Selector, it is easy to go through them according to the Fisk Rigid System on paper without actually trying them in practice. Using the same graphed blocks of records as in Table XXIX :—

Fisk Rigid System on Operator and Selector Records

TABLE XXX

SUCCESSES, RIGID SYSTEM

No.	1	2	3	4	Mean	
163	29	28	28	36	30·3	Operator G.N.M.T.
164	19	24	22	22	21·7	Selector

A sample of the graphed result No. 156 (4) is given above No. 159 (2). (See Graph III, Nos. I and III.) In Graph III the line which runs through each table represents the mode of scoring with the Fisk Rigid Method according to the fixed order 1, 2, 3, 4, 5. Beginning with Box 1 the line represents the repeated opening of the same box until a success is attained and then the removal to the next box which is shown by the line going on to the next column, and so on.

Examination shows that the difference between the two is not that the human operator selects one number more often than the Selector does, or has any sequence number-habits, but that he distributes his choice more evenly amongst the five numbers than pure chance would do. The result is that, if the percipient continues to open the same box until a light appears, he will not run the risk of having to wait so long with the human operator as he will with the mechanical selector. It is not that the operator falls into preferences ; it is that he is too impartial ! Until Mr Fisk thought of it, I have never heard any critic make this suggestion. If the human operator behaved as the mechanical operator does in fact behave, he would feel that he was favouring some numbers at the expense of others. So he is more impartial than the Selector *in the short run* of 100 trials ; but the selector evens things out and is impartial *in the long run.* If Graph III be examined, it will be found that in I there is no space in any column longer than 8 trials : in III there is a space of 21 trials and one of 13. It is this difference in the spacing which allows the Fisk system to score.

The reader may be reminded that when dealing with Chance, the meaning of the words " equally likely " was considered. It was said, " That any one of five events is equally likely to happen means, in this sense, that if the trials are continued, the tendency is indefinitely towards the occurrence of each of the five events an equal number of times, that is one-fifth of the total, *in the long run.*" But " equally likely " events will not be equally distributed in a short number of trials.

Mrs Hemingway

The subject who had scored next best after Miss Johnson with the Pointer Apparatus was Mrs Hemingway, Her average rate of scoring with this had been 25·2% over 2000 trials, and the chance-probability between 10^{-8} and 10^{-9}.

When tried with the Electrical Apparatus, her rate of scoring was low, though the total result after 4400 trials was just significant ($P=0{\cdot}006$). I wished to increase her rate of scoring by giving a suggestion which might help the subliminal; so I suggested to her, after Mr Fisk's success with his method, that for one occasion only she should adopt the Fisk method of scoring and I told her that it would automatically bring up the result, but that this would not be due to her own faculty but to a change in the probability of success. This may have been rash; but I did not then believe in the Fisk method and merely used it to give colour to the suggestion that her score would go up. It did go up, of course, although at the time I believed it was owing to the suggestion. After that I told her not to use the Fisk method again, but I am afraid, without success. The scores obtained were as follows:

TABLE XXXI

28th November, 1935. *Mrs Hemingway and the Fisk Method*

No.	T	S	d	%	X	P	Conditions
165	500	123	+23	24·6	2·57	0·005	With Fisk Method Suggested. Keys.
166	7439	2093	+605	28·1	17·5	10^{-67}	After Fisk Suggestion. Keys.
167	2381	471	−5	19·7	—	—	Selector.

This shows that the Fisk method sent the score up and that it continued to go up after the method had been suggested; also that there was complete failure with the Selector.

Four blocks of 100 trials each of Mrs Hemingway's were completely checked and graphed (one of these is shown in Graph III, No. II) and these showed that she was using a mixture of the Fisk method and guessing. The scores in No. 166 must therefore be regarded with suspicion as probably due to the use of the Fisk method. Mrs Hemingway must have been scoring without it to get her results on the Pointer Apparatus, with which the Fisk method cannot be used, and seems to have been scoring positively at a low rate with the Electrical Apparatus before the 28th November; but the large increases in scores after that date are almost certainly due to the Fisk method and must be discounted.

Miss Johnson and the Fisk Method

To what extent does the discovery of the Fisk method of scoring affect Miss Johnson's results? I may as well admit that it would have been better had I obtained some results which were checked through in detail earlier in the investigation. But I think that the arguments against Miss Johnson having used the Fisk method are very strong. They are :

(1) Miss Johnson herself asserts that she never used it before it was explained to her. When, after that, she tried it, it simply put her off and reduced her score. She said that the rule of keeping to one box till a light came merely confused her because she had a feeling all the time that a lamp was alight elsewhere.

(2) The gradual acceptance of new conditions, when she simultaneously scored with an old condition and failed to score with a new one, until finally she scored with both, are very hard to reconcile with the Fisk mode of scoring.

(3) The extreme uncertainty and variability of her scoring do not point to the Fisk method.

(4) Operators who have used the Pointer Apparatus are agreed that the Fisk method cannot be used with it.

(5) The results which Miss Johnson obtained with mechanically selected numbers could not possibly have been scored on the Fisk system. (See Table III and footnote on p. 151.)

(6) In the checked block of 100 trials shown in Graph III, No. IV, there is no trace of the Fisk method. (Compare with Mrs Hemingway's result in Graph III, No. II.)

While the Fisk system was being discussed and tested (*i.e.* after 28th October, 1935) Miss Johnson did get into the way of mixing it with her ordinary mode of scoring : and it had the effect of sending her rate of scoring *down* instead of *up*. With Mrs Hemingway it had the opposite effect. This fact is shown very clearly in Table XXXII, in which the scores for 2000 trials *before* and *after* the Fisk discovery are compared (*a*) for Miss Johnson and (*b*) for Mrs Hemingway.

Table XXXII

Miss Johnson

No.	T	S	d	%	X	P	Conditions
168	2000	501	+101	25·6	5·6	10^{-10}	Before Fisk discovery.
169	2000	440	+40	22·0	2·2	0·01	After Fisk discovery.

Mrs Hemingway

No.	T	S	d	%	X	P	Conditions
170	2000	445	+45	22·2	2·5	0·01	Before Fisk discovery.
171	2000	607	+207	30·3	11·5	10^{-29}	After Fisk discovery.

It may be said, therefore, that while Mrs Hemingway's results with the Electrical Apparatus must be discredited on account of the probable large admixture of Fisk method, there is no reason to discredit Miss Johnson's.

Coincidence between Fisk Method rate of scoring and Miss Johnson's

It is remarkable that the rate of scoring with the Fisk Rigid Method and Miss Johnson's average rate of scoring when at her best are both close to 30%. This, at first sight, looks a very suspicious fact, the more so since Miss Johnson's rate of scoring fell off very much after the Fisk method was discovered. But it must be just one of those coincidences which seem to be set like traps for the hasty and unwary. The figures of Table III and at the foot of p. 151 are alone enough to disprove it. The most plausible line for the critic to take would be to allege that there had been a mixture of the Fisk method with guessing; but personally I do not believe that there ever was before the Fisk method was suggested to Miss Johnson. The system merely balks the genuine faculty with her.

The Fisk method is quite easily prevented (*a*) by the use of the Selector or of mechanically selected numbers, or (*b*) by arranging that the percipient does not know when a success has been scored. In the latter case, there is no reason to suppose that the human operator is not exactly on a par with the mechanical Selector.

Psychological Effect of the Fisk Discovery

The effect of the Fisk discovery on Miss Johnson's scoring was most unfortunate. In fact, from one point of view, the most important result of this discovery has been its psychological effect as a deterrent and inhibitory influence on the subliminal. The suggestion was made suddenly that there was some method by which scores of the same order as those which Miss Johnson had been getting by means of her faculty could be got by a quite normal and, as it were, "trick" method. The exact explanation of the method and the bearing it would have on the work of the past six months was not yet clear. No better example could be staged of the kind of thing which it is the paramount duty of the experimenter to guard against or of anything more likely to create a subconscious resistance against the experiments as a whole. In point of fact, Miss Johnson's power of scoring dropped at once and from that date (28th October, 1935) to the present time has never recovered. And it is interesting to note that the falling off is not with regard to one condition only, but to all alike. On 13th February, 1936, the rate of scoring showed signs of picking up again, but illness sent it down soon after; and at the present time (March, 1936) Miss Johnson can scarcely get more than chance scores with any condition, as a result of the depressive influence of the Fisk shock or of bad health or of both.

The Fisk Method and Zener Cards

It is possible with the Fisk method to score with Zener cards under the same conditions as with the human operator in the apparatus. The simplest way to test this is to lay the shuffled pack out in line face upwards on the table and, having decided the rigid order to be used, go through the pack counting the successes. Different orders can then be used with the same lay-out. Very high "anti-chance probabilities" are obtained if the probability of success is taken to be 1/5.

This was tested by both Mr Fisk and myself and Mr Herbert kindly made some systematic tests which confirmed them.

Using an ordinary pack of 25 Zener cards Mr Herbert obtained the following results:

(1) Using the Fisk Rigid System and going through the pack in 10 different orders. p assumed $=1/5$.

TABLE XXXIII

No.	T	S	d	%	X	P
172	2500	620	+120	24·8	6·0	10^{-8} to 10^{-9}

or, put in a form giving the actual probability of success,

$$S/T=p=0{\cdot}248.$$

(2) When the orders were mixed by changing them within the 25 trials, the scoring on the whole sank to something like chance values.

TABLE XXXIV

No.	T	S	d	%	X	P
173	2500	527	+29	21·2	1·45	0·07

(3) Putting four packs of Zener cards together to make one pack of 100, and going through with the sequences as in (1) gave the following result:

TABLE XXXV

No.	T	S	d	%	X	P
174	2500	519	+19	20·7	0·95	0·17

This is a chance result and indicates that a pack of 100 cards is large enough to give something near enough to random shuffling for practical purposes, although a pack of 25 is not.

(4) Finally, packs of 25 cards were drawn at random from a well-shuffled pool of 100 cards and the same process of sequences gone through again. Results:

TABLE XXXVI

No.	T	S	d	%	X	P
175	3000	611	+11	20·4	0·5	0·3

This again gives a chance result.

To sum up these results, it is possible by using the Fisk method consistently to obtain highly significant scores with a pack of 25 Zener cards, but if a pack of 100 Zener cards is used, or more, the effect of the Fisk method is practically wiped out. Or, alternatively, if packs of 25 Zener cards are drawn from a well-shuffled pool of 100 cards or over the same applies. Changing the rigid order in the course of an experiment, waters down the Fisk effect.

The reason of this Fisk effect with Zener cards has, of course, nothing to do with the thoroughness of the shuffling to which the pack is subjected. It is the result of the fact that it is impossible to arrange the pack in a truly random order because the condition is imposed that five cards of each kind must be included in a pack of 25. In a truly random arrangement, this will very rarely be satisfied; and the Fisk method scores because a shorter average space exists between any diagram and a repetition of that diagram than would be the case if the cards were arranged in a truly random order.

The Fisk Method and Dr Rhine's Results

The question arises how far the discovery of the Fisk method affects Dr Rhine's results. As has been said, the possibility of using the method is easily annulled, whether by using a pack of 100 or more cards, or by arranging that the percipient does not know when he scores a success. But, in any case, as each call is entered as the percipient makes it, it would be quite easy for Dr Rhine to know whether any percipient had been using the method or not: and as it would be very obvious and he does not mention it, presumably his percipients did not use it. It would be very unnatural to use it unless it had been suggested. It would, however, be advisable in any future experiments that all investigators should rule out the possibility of the Fisk method being used.

CONCLUSION

One surprising fact which has come into prominence in the course of this investigation is the absence of any general disposition among investigators to regard the search for unknown modes of perception as fundamentally a problem in psychology. One would have thought that stress laid on this point of view, or emphasis given to the primary importance of psychological conditions in an inquiry into a cognitive faculty, would have been in the nature of a platitude: but it seems, on the contrary, that no truth is more in need of emphasis. If it is alleged that success in E.S.P. depends upon a delicate balance of psychological forces within the subject; or if insistence is laid on the necessity for guarding the subject against adverse suggestions, or on the necessity for fostering favourable emotional conditions; or even if a departure is made from the bare numerical totals to show how the paranormal factor emerges in the curious way in which the experiments have developed, the attitude of a common form of criticism is to regard all this askance as the excuse of a prejudiced advocate, who is trying to cover repeated failures. Repeated failures do not need to be excused: they are merely a fact which investigation discloses. The point is that the boot is on the other leg, for this kind of critical attitude is in itself no centrally poised, scientific position, for it is not even ready to accept on an equal footing every kind of condition which experience shows to be necessary for success. In its bias against the psychological and its over-emphasis of the physical, it is, in effect, making the demand that a psychical phenomenon, in order to prove its reality, must show itself to be a physical phenomenon. It is an attitude which would seem to be based, not on scientific principle (for that, in its essence, is pure empiricism), nor on any particular speculative or philosophical position, but simply on an ingrained conviction, taken from practical life, that the world of normal sense-perception supplies the criterion of what is probable throughout the universe. Perhaps it goes even further, and suggests that the sensuous world, because it is so familiar, must of necessity be all-inclusive. In any case, the next step is that, non-physical causes being unlikely, non-physical conditions are unimportant. It is a standpoint based on a suggestion rather than on an argument, for its underlying idea is not explicit but exists as an unformulated postulate secreted in the background. No suggestion could be more inimical to pioneer work in psychical research than this, for it destroys at the root the mental freedom which allows itself to be influenced equally by all kinds of experience.

Probably no one can be in touch with facts such as those dealt with in Extra-Sensory Perception without reaching convictions based on grounds which cannot be logically formulated; and the writer feels that he would be failing to give a complete account of the phenomena of E.S.P., as they have fallen within his experience, if he did not stress the conviction which he has of being in touch with something very real. The demand for logical proof, based on experiment, in such a matter as this, is, of course, an obvious necessity; but it is doubtful whether, in the last resort, people are as much impressed by logic as they are by subtle differences in the characteristics of things with which they have grown familiar. There hovers, somewhere behind the normal consciousness, a wide potentiality for knowing, which, in moments when the subject is raised a little above the humdrum level of common life, and the frequently jarring elements of the self unite in a more concordant synthesis, can reveal itself uncertainly in outward speech or action; and this state of self-togetherness is marked by a feeling of internal harmony. It is a state which one gets to know more certainly from personal contact than from any numerical proof; and it is this, together with the other psychological factors which have been detailed above, which form the background of the subject. The writer's conviction is that future success in this field is likely to depend largely upon the extent to which investigators and critics are both willing to accord these conditions adequate recognition. Without this, the state of affairs is not very hopeful; for the conditions which make it possible for these phenomena to occur are being disregarded, while the conditions which would render them evidentially valid if they did occur are jealously maintained.

The account of the experiments which has been given above should be regarded as an interim report on an investigation which is still in progress, and not as a summary of one which is complete. So regarded, these experiments, in the opinion of the writer, warrant the following conclusions: (1) That Extra-Sensory Perception is an unquestionable fact. No other explanation will reasonably cover the whole of the evidence. (2) That E.S.P. takes place when the possibility of telepathy, as ordinarily defined, is excluded. (3) That there is evidence pointing strongly in the direction of Precognition. (4) That the faculty appears to be able to externalise its material in the form of motor action, without a *necessary* conscious accompaniment; and that consequently it is doubtful whether the faculty falls completely within the definition of a cognitive mode. (5) That the records, as well as the subject's experience, point to the cause of above-chance scoring as being the existence of short periods of mental dissociation in which the rate of scoring successes greatly exceeds chance and often approaches 100%. Experience shows that the occurrence of these brief periods of dissociation depends upon a psychological background which is largely unknown; but that it is influenced by (i) ideas or suggestions which have been accepted by the subject's subliminal and have taken root in it. These may be of the most trivial nature, quite without rational foundation and may be simultaneously disbelieved by the subject's normal consciousness. (ii) By a general emotional state including interest, enthusiasm and happiness on the one hand or disappointment, depression and boredom on the other. (iii) By the subject's state of health, which must be good if the right psychological state is to supervene. (6) That the external conditions of the experiments should include: (i) The possibility of working at a high rate of speed for two reasons, (*a*) to allow the paranormal impulse to run the gauntlet of conscious associations without having time to become entangled with them, (*b*) to avoid tedium. (ii) The possibility of dealing rapidly with large numbers of trials in a way which makes the minimum demand on the conscious attention of the subject. (iii) The possibility of changing the conditions instantly, if possible without the knowledge of the subject, and also without a pause, in the middle of a series of trials. (7) That it is necessary for the investigator to deal with every subject individually and not to attempt to treat subjects *en masse*. (8) Rhythm in working through a series of trials is desirable. (9) The only method which adequately fulfils these demands is a mechanical instrument.

Extension of the Research

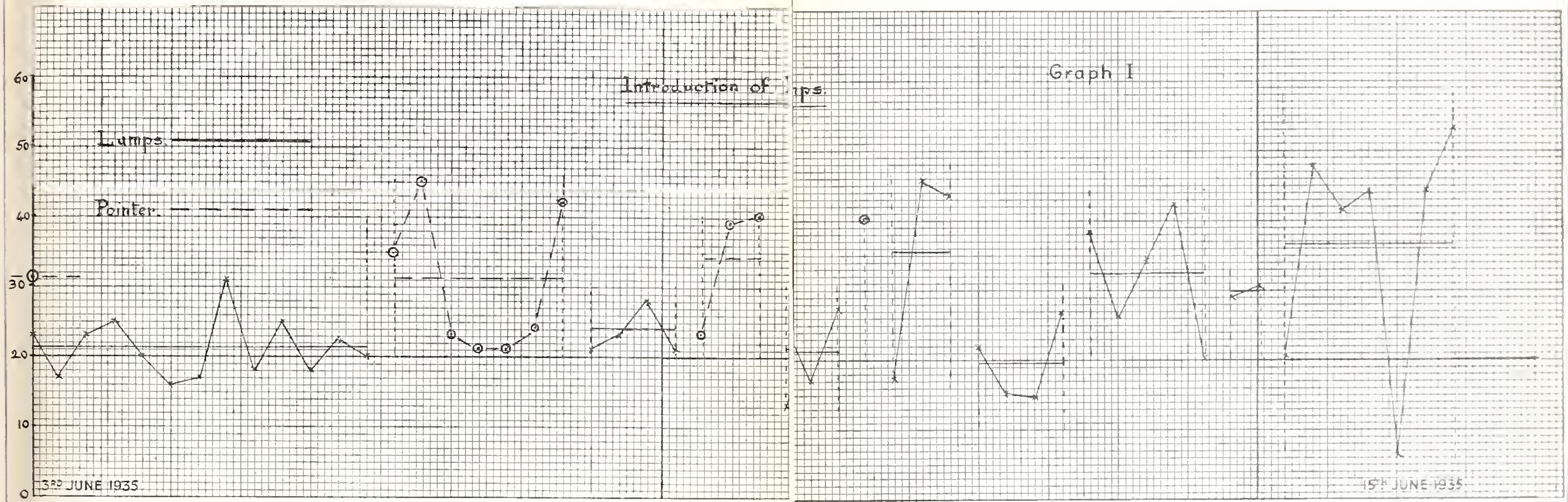
The writer's experience suggests the advisability of dealing with each subject separately. But then, it may be asked, if this is done, how is the extra-sensory faculty to be shown to be a human possession in the sense of arising out of the normal structure of human personality at large, and not to be a pathological freak. The Collective method of research may, on occasion, prove to be a useful auxiliary; but, in the main, it seems that the method must consist in the establishment of a number of centres in each of which an investigator makes an intensive study of two or three selected subjects. In this way the various lines of research could be specialised.

Since E.S.P. constitutes a psychological problem, it is important that the co-operation of psychologists should be secured in the attempt to solve it. What is needed perhaps more than anything else at the present time is for the psychologist to devise a technique for the effective control of those levels of the self below the conscious threshold, which are concerned in the externalisation of extra-sensory material. Such increased control would probably lead to greatly enhanced results with the sensitive type of subject and might even lead to the yielding of results by ordinary, "unpsychic" persons. We have above an interesting example in the case of Mr

Fisk (see Table XXVI, Nos. 145, 146), who appears to have been exalted, in a moment of emotional enthusiasm, *based on a rational belief*, to the pitch of scoring with a genuine faculty. If this has not happened, there must have been here a remarkable freak of chance. We need to know how to do artificially what occurs spontaneously at rare moments such as this; and the incident is one to bear in mind as it gives the valuable suggestion that the extra-sensory faculty may not be at all rare *as* a faculty of perception, but may, on the contrary, be universal and inherent in the make-up of every human being. The rare feature may be only its intrusion into conscious life, that is its externalisation. On this view the sensitive would not be a person who owns a rare faculty of perception, but only one in whom the path of emergence is not completely blocked. Will not psychologists help us to work out a technique which will shorten the tedious process by which the above results were obtained? Will they not give us some readier means of controlling the self beneath the threshold? It is a work well worth the attention of orthodox science, since it promises not only to light up the recesses of the human mind, but also to widen the perceptual channels on which all knowledge of the outer world depends. We should like to know whether the "dissociated" states of sensitives are not, perhaps, many; and if there may not be one of these which in particular is necessary for the production of extra-sensory material. We should like to know how to establish the particular state we want. We should like to have, in general, a comparison of the make-up of the sensitive with that of the non-sensitive, and some sort of map, however rough, of the country we are trying to explore. If the fashion which regards extra-sensory perception as scientifically disreputable would become out of date, a field would be opened to psychology of the greatest interest and importance to mankind.

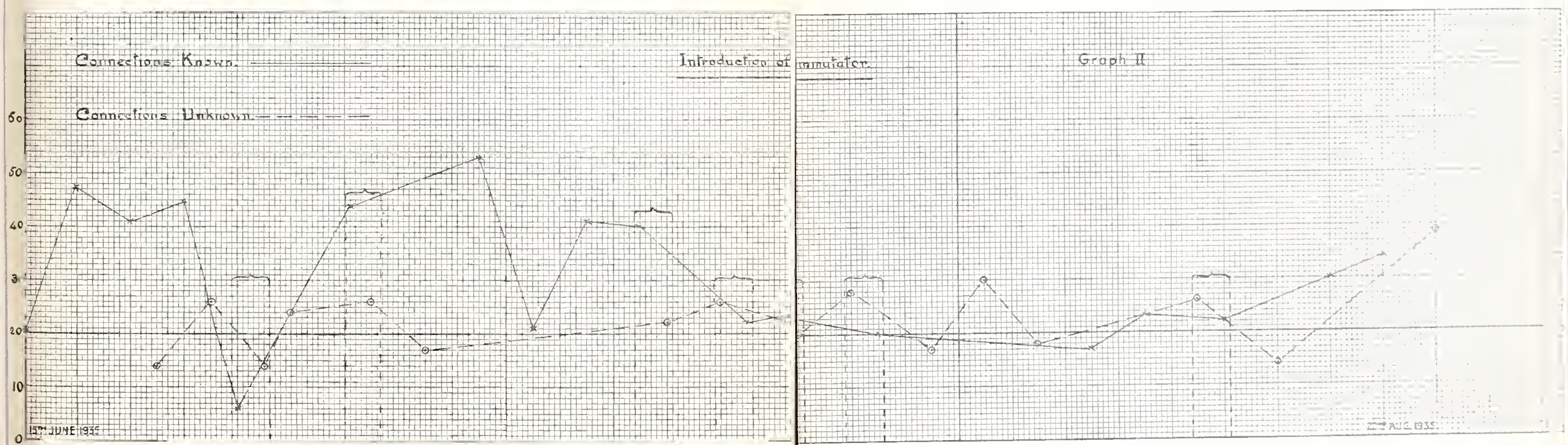
Finally, I should like to express my sincere thanks to the President as well as to the Council of the Society for the kind and generous help which they have given me, and to various members for their encouragement: to Mr Saltmarsh for his constant co-operation and advice and for assisting with his handiwork in the construction of the apparatus: to Mr Fisk for his assistance and for his valuable discovery: to Dr G. C. Poole of New College, Oxford, and to Dr R. A. Fisher for mathematical advice; and to Mrs Hemingway as well as to all who have so kindly given their time in acting as subjects.

Gh I



Lumps
Pointer
Introduction of ps.
Graph I
3RD JUNE 1935
15TH JUNE 1935
0
10
20
30
40
50
60

Gr II



Connections Known.
Connections Unknown.
Introduction of mmutator.
Graph II
15TH JUNE 1935
22ND AUG 1935
0
10
20
40
50
60

Graph III

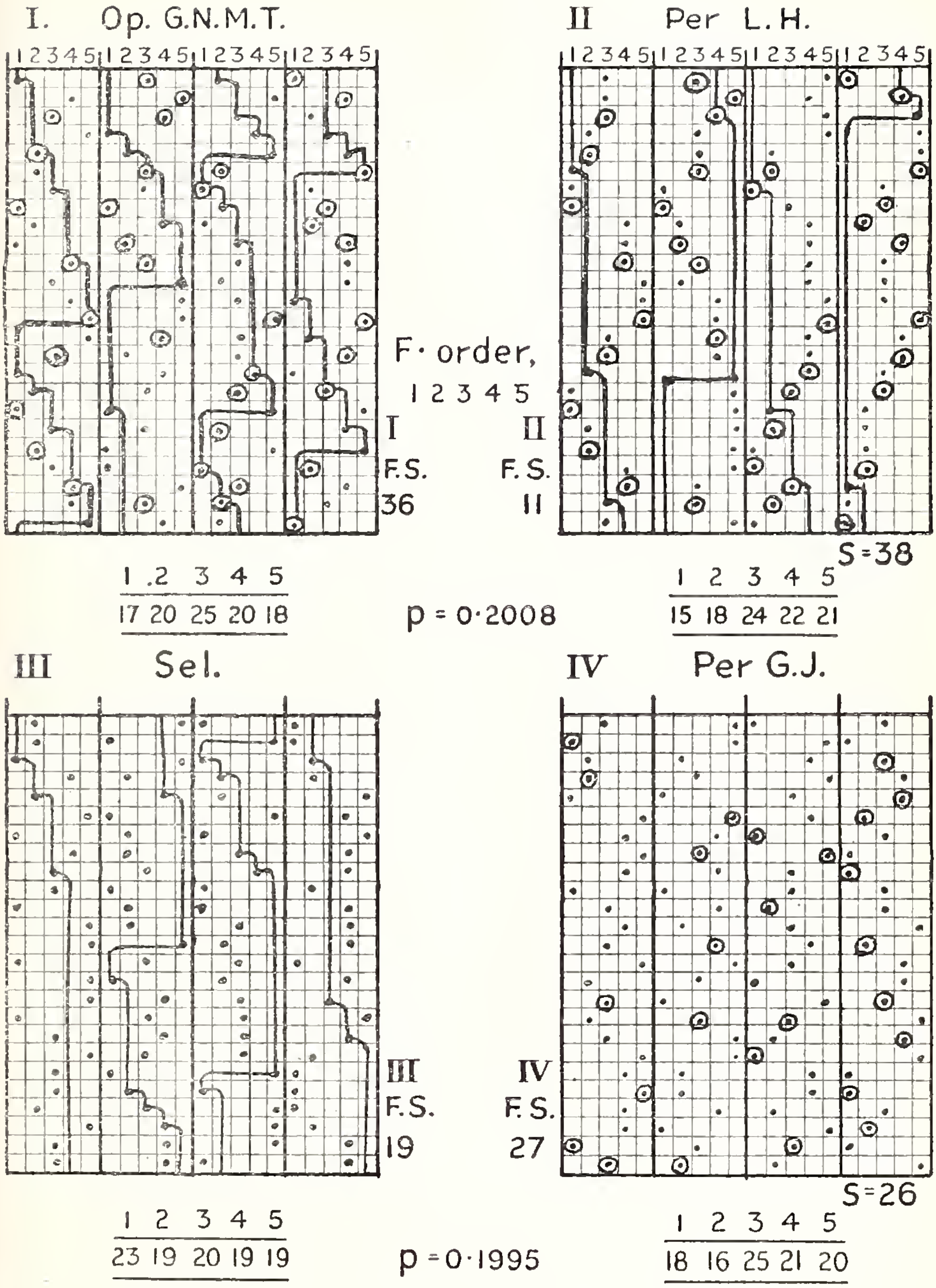
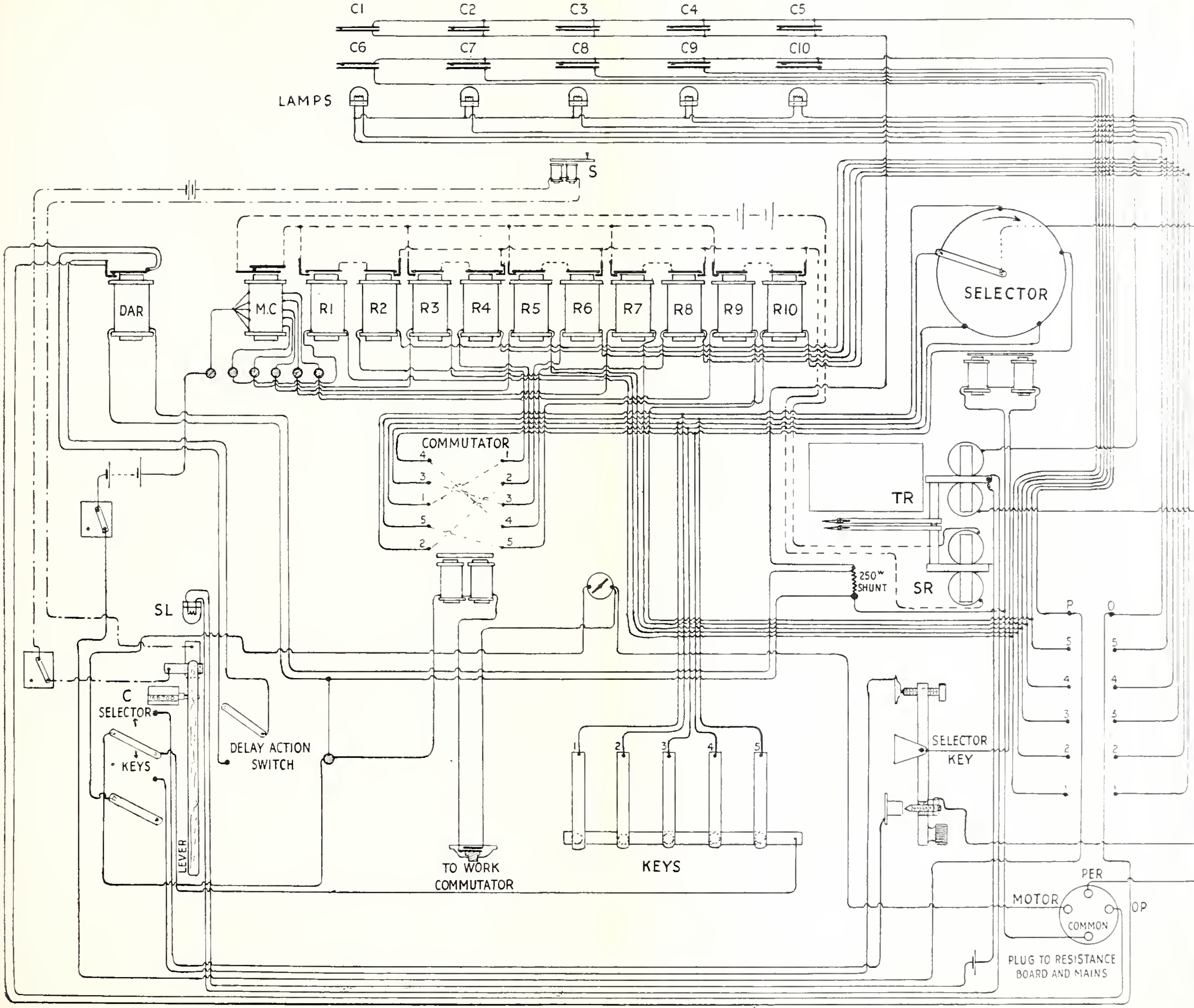

I. Op. G.N.M.T.
II Per L.H.
F· order,
1 2 3 4 5
I F.S. 36
II F.S. 11
S=38
1 2 3 4 5
17 20 25 20 18
p = 0·2008
1 2 3 4 5
15 18 24 22 21
III Sel.
IV Per G.J.
III F.S. 19
IV F.S. 27
S=26
1 2 3 4 5
23 19 20 19 19
p = 0·1995
1 2 3 4 5
18 16 25 21 20

DIAGRAM A.


C1
C2
C3
C4
C5
C6
C7
C8
C9
C10
LAMPS
S
DAR
M.C
R1
R2
R3
R4
R5
R6
R7
R8
R9
R10
SELECTOR
COMMUTATOR
TR
250W SHUNT
SR
SL
C
SELECTOR
KEYS
LEVER
DELAY ACTION SWITCH
TO WORK COMMUTATOR
KEYS
SELECTOR KEY
P
O
MOTOR
PER
OP
COMMON
PLUG TO RESISTANCE BOARD AND MAINS

PROCEEDINGS

OF THE

Society for Psychical Research

PART 148

SOME OBSERVATIONS ON EXTRA-SENSORY PERCEPTION

(A Preliminary Note)

By J. Cecil Maby, B.Sc., A.R.C.S., F.R.A.S.

Four points in Professor C. D. Broad's recent presidential address (*vide Proc.* S.P.R., Part 142, vol. XLIII) stimulate me to put forward certain considerations in connection with extra-sensory perception, resulting from a series of experiments and observations of a qualitative kind that I have made during the past few years [1] in a personal attempt to satisfy myself, first, of the genuineness of such phenomena and, second, of their psycho-physical *modus operandi*. Special attention has been paid to the possible connection between such cryptopsychic processes as telepathy and clairvoyance and common-place sensorial perception. Recent observations and speculations by Prof. Hans Driesch, Prof. Broad, Dr Rhine and others have, in fact, strengthened my own findings and ideas; but I feel that the time is, as yet, premature for their full discussion. It may, however, not be amiss for me to advance one or two personal observations and ideas in relation to Prof. Broad's paper without treating the matter upon a statistical basis, despite the present mathematical vogue.

(1) . . . Prof. Broad questions whether it would be possible, say, for an Englishman, ignorant of the French language, to induce telepathically in a Frenchman, who knows no English, " a cognition of a fact which the Englishman knows or a proposition which he

[1] As yet unpublished, except in two recent lectures to the Oxford University Psychical Research Association. These experiments were commenced some years ago, before the writer had seen the results of Dr J. B. Rhine's and Mr G. N. M. Tyrrell's recent experiments.

cognises". He further recommends experimental investigation of such a case, as did Mr Saltmarsh also in his report on the Warren Elliott case, (*Proc. S.P.R.* vol. XXXIX, p. 128).

Without having made experiments in this particular direction, it occurs to me that, as most of us tend mentally to visualise our ideas quite as commonly as we verbalise them, such an agent's conception might happen to be translated in *visual* terms, or even directly, as pure meaning-content, perhaps, without any recourse to verbal symbology. Moreover, after its primary (subliminal ?) reception, the percipient might, conceivably, interpret the initial recept in terms of his own sensory and/or verbal symbology ; *e.g.*, the French language. Such a process would, admittedly, be somewhat roundabout, but it should not, for that reason, be either impossible or improbable, judging by the various forms that veridical hallucinations are found to take—especially in spontaneous cases, but also in the seance room and even in the laboratory. As Mr Saltmarsh has privately pointed out to me, however, there are some " meanings " of a generalised nature (he cites emotional moods, tastes, smells, etc.) that might not be translatable into visual symbology. But in answer to that I would ask : Can emotional moods be supposed ever to exist in a non-specific sense, abstracted from concrete sensations and ideas ? Personally, I do not think so. Moreover, it is begging the whole question to assume that meanings, as such, cannot be transmitted directly without translation into sensory symbols. Indeed, evidence is not lacking to indicate that such may be the case, in some instances at least.

(2) Prof. Broad very justly stresses several seemingly insuperable difficulties in the way of any purely physical explanation of the facts of clairvoyance—which category may, I believe, be fairly extended to cover many of the phenomena of dowsing, divination and "psychometry".

As one who has not only carried out a fair number of experiments in telepathy, clairvoyance, dowsing and what Dr Rhine conveniently terms undifferentiated extra-sensory perception, (in many of which we were fortunate to score a good and significant proportion of successes), but also acted as percipient in the majority of those experiments, I should like to take this opportunity generally to confirm Prof. Broad's statement as well as the findings of Dr Rhine, as recently given in his excellent treatise on E.S.P. Perhaps, too, it is not amiss to add the following qualitative observations, in the hope that they may stimulate others either to confirm or to disprove certain speculations that my own experiments and records have suggested to me.

In our tests, we variously endeavoured to " get " a given object either by *pure clairvoyance*, as with shuffled drawings or with pictures in an unknown and unopened book ; by *undifferentiated e.s.p.*, as with pictures, drawings, or words actually held and observed by a supposed telepathic agent ; and third, by *pure telepathy*, as when the agent only concentrated his attention on a given mental idea or image, not sensorially upon a physical object. In all instances where the selected object or image was a visual and pictorial one, the extra-sensory perception (if successful) was also a *visual* one, seen against the greyish blank of the vacant mind—much as was lately described by Mrs Upton Sinclair, and as in visions of successful scryers, etc. When, however, the agent's " message " was a verbal one, the percipient's resultant " hallucination " (=e.s.p.) was almost invariably an auditory one, or else a somewhat vague idea of the content of the " message ", expressing itself in verbal rather than visual symbology. This was particularly noticeable in spontaneous instances of telepathy of a simple kind. But it was also found in experiments in which one of the keys of a piano or else one volume out of a case of books of various colours had been mentally preselected by one or more agents, that an extra-sensory percipient tended to decide correctly upon the *identical* key or book, if the agent(s) selected an object visually and specifically ; but that he was often misled into deciding upon *any* one of the keys of a given name, or upon *any* one book of a given colour, when the agent(s) thought was in *verbal* rather than visual terms.[1] That is to say, certain agents tended to transmit generalised and abstract, as concontrasted with particular and specific ideas ; corresponding to their individual tendency to think in words and verbal symbols rather than in " objective " sensory images.

It should be added that the foregoing observations have not been repeated a sufficient number of times or with enough subjects to be deemed conclusive or worthy of statistical analysis ; but our qualitative observations, so far as they go, would appear to be both positive and significant. In any case, the connection (if any) between the initial form of a given object or " message " of e.s.p. and the ultimate form taken by the e.s.p. itself—or else the sensory hallucination, should the e.s. stimulus happen to be especially intense —is, I think, worthy of careful study. For it would, surely, be a fact of the first importance, that there should exist a positive and automatic inter-relationship between the object of extra-sensory perception (whether existing physically and objectively, as in cases

[1] This confusion may, of course, have been due to corresponding indecision in the agents' minds, though they themselves denied such indecision.

of clairvoyance, or else psychically and subjectively, as in cases of telepathy) and the derivative e.s.p. itself. The present note, however, is intended merely to draw attention to such an eventuality; for it may well be that, in the qualitative and somewhat limited investigation in question, we have voluntarily predisposed ourselves towards forms of sensory "hallucination" (*via* e.s.p.) corresponding in kind to the prime aspects of the original phenomena by virtue of expectation and the selected aims and conditions of the tests. I cannot quite believe, however, that the story ends there; for it is noticeable, in accounts of spontaneous veridical cases of telepathy and "travelling clairvoyance", that there is a remarkable tendency towards such a co-relationship as I have cited. Moreover, I have been able to trace such a relationship in the majority of spontaneous instances of my own collection. It would be a labour well repaid, therefore, for one of our members to re-examine, in the light of the foregoing hypothesis, all the veridical cases of extra-sensory perception collected by the S.P.R., as well, perhaps, as some other major collections, such as that of the late M. Camille Flammarion. This enquiry I would gladly undertake myself, were it not for pressure of much other work of an unavoidable kind.

As examples of striking agreement between the nature of the original object or phenomenon and its e.s.p., the following first-hand examples may be of interest, stated as briefly as possible.

(*a*) *Visual e.s.p.*

On a certain occasion I held in my hands, in complete darkness, a large illustrated book, without opening it or having any normal knowledge whatever of its contents; nor was the particular book with its illustrations and the experiment conceivably foreconscious in the mind of any other living person at the chosen time, since I had drawn it at random and in the dark from a book-case containing volumes previously unknown to me, and that, too, without the normal cognisance of any other living person.[1] Despite the strictness of these conditions, I succeeded in visualising (though but dimly, and without the clarity of normal perception or even that of a vivid dream or hypnagogic illusion,) and describing correctly in writing, before opening the volume, some 33% of the ninety illustrations it contained. Several of these were, admittedly, repetitive in general type (*e.g.*, pictures of mosques and pagodas); subsequent examination of the other twenty-three illustrated volumes of the

[1] Actually Vol. 12 of the 14th edn. *Encyclopaedia Britannica*, of twenty-four illustrated volumes, that had freshly arrived and not been previously opened by me or others in the house.

case, however, showed that my clairvoyant perceptions could only have been said to agree—and then merely in a general and less particular way—with from 1% to 15% of the illustrations in the several volumes. Such a result was, therefore, highly significant. Note that a score of 33% successes is about equal to Miss Johnson's average score in Mr Tyrrell's experiments in undifferentiated e.s.p.: though the chances of success by normal guesswork were very much lower in the present instance, I believe, in view of the detailed nature of the perceptions involved. I should perhaps add that I tend about equally to visualise and to verbalise my thoughts and imaginations in everyday thinking. Some of these pictures were, moreover, of a sufficiently peculiar kind to be immediately and specifically recognisable on opening the volume for normal visual inspection; for instance, plates opposite pp. 96 and 558, which were very remarkably described as "a kind of stained glass window, containing several mediaeval figures in colour" and "a number of very curious shapes that I cannot quite understand, including a large X, a row of little ovals, like miniature frames, with squares below, and what looks like a close view of the human eye". Reference to the volume in question will at once show how dramatically correct these dim perceptions were. They were, in fact, at once and indubitably recognised upon opening and examining the book.

Note, moreover, that the pictures were seen (*a*) as if "in a glass, darkly", (*b*) of approximately their proper dimensions, (*c*) normally orientated, though some were actually face downwards as the book was held [1]; and (*d*) that, no effort having been made to discern any of the verbal matter, no such matter was perceived extra-sensorially, even in a blurred and undifferentiated way:—which confirms the observations of Dr Rhine and others, namely, that e.s. attention can be directed at will to any one particular object concealed among many others opaque to the light and of a mutually conflicting kind.[2]

[1] In other experiments *re* the perception of drawings by undifferentiated e.s.p., however, the objects were sometimes perceived incorrectly orientated or even in fragmentary form. In fact, ordinary spatial relationships were commonly discounted in the e.s.p. (*Cf.* also similar observations by Richet, Upton Sinclair and other previous investigators.)

[2] It would appear that the skilled diviner or dowser effects the same selection unconsciously when he carries a specific sample of the object of enquiry, or else consciously when he simply concentrates mentally on a single objective or substance. But I have purposely omitted any consideration of such (highly relevant) phenomena from the present paper, since they appear to call for a separate and more detailed study, dealing with the employment of a rod or pendulum for the purpose of recording delicate muscular automatisms in telepathic and clairvoyant experiments.

Note, finally, that in another set of tests, the percentage of successes was nearer sixty, or nearly doubled, when isolated illustrations (as judged by the different textures of the leaves of the, then opened, book) were considered individually by e.s.p. This observation—which, of course, also requires repetition on a quantitative basis—suggests that e.s.p. may be (*a*) aided by some sort of direction sign, and (*b*) dependent upon spatio-material factors after all; though tactile hyperaesthesia was not the cause, as I avoided any contact with the sheets except at margins.

The following example of successful undifferentiated e.s.p. is also very instructive; though I might equally well have chosen others for their special informativeness:

My assistant, completely concealed from me, though in the same room, secretly thought of and drew a conventional heart on a piece of paper. As, however, one half was slightly misdrawn, he re-drew it in a deeper outline than the other side. He also, of course, concerned himself mentally more with that half than with the other.

By e.s.p. I correctly visualised and drew *the darker half of the figure only*. I represented it both correctly oriented and upside down.

Note that, as this was an undifferentiated case, it is impossible to be sure whether clairvoyance or telepathy was responsible for the result. I may say, however, that, in other similar experiments on the transference of drawings by undifferentiated e.s.p., the results as a whole pointed to more or less perfect clairvoyance as *modus operandi*. But there were also instances—especially with certain agents, whose cerebral processes are known to be relatively fecund and intensive—wherein evident telepathy occurred; for an alternative object, that the agent had also *thought* of, but not actually *drawn*, was successfully registered by the percipient instead of that drawn on the agent's sheet. So too, if the psycho-galvanic reflex is used to detect which object, letter or numeral out of a series has been mentally pre-selected by a subject, a *first* choice will generally be more readily detected than a second one, should the subject happen to change his or her mind during the test.

(*b*) *Auditory E.S.P.*

One quiet Sunday morning my wife and I were lying in bed awake, but not talking, at about 8 a.m. Our bedroom door and that of our small boy, R, then aged three, were both ajar to a common landing. We could hear R humming and talking to himself in his cot. Suddenly it occurred to me to endeavour mentally to "project" myself to his room; which I did forthwith, without word to my wife or any accompanying sounds. In fact, I intently and suddenly imagined myself at R's bedroom door, rapping on it loudly. In instant response there came a call from his room: "Who's dat?" and then "Daddy?", in an enquiring and startled tone. I explained to my wife what had happened on my side, then went in to R, and asked him what was the matter. He said: "Someone knocked on the door. *You* knocked, daddy!"

It only remains to add that I did not normally knock thus on the child's door, and that his immediate response and comments were equally unusual and dramatically convincing. Note, moreover, the excellent agreement between the determination to *knock loudly and suddenly on the door* and the resultant e.s.p. I had, in fact, concentrated on the imaginary *act* itself and not on transmitting any mere *idea* to R. My wife, incidentally, neither heard nor suspected anything.

Several equally successful examples of auditory e.s.p. between R, my wife and myself have been recorded, but the foregoing will suffice.

(*c*) *Tactile E.S.P.*

One evening when he was in the (normally) deepest period of sleep, I went very quietly into the same child's (R's) bedroom, which was dimly lit by a light shining in from a summer evening sky. I first stood silently in the room for some three minutes to make sure that R was sleeping soundly and undisturbed by my presence. Then, without any actual movement or disturbance, I suddenly imagined myself to be laying a cool hand on his forehead. After two or three seconds, the child sighed, stirred in his sleep, turned his head once or twice from side to side—as one would be inclined to do in the circumstances, if real—and finally turned over on to his other side. At the same time, in imagination, I withdrew my hand: he then continued to sleep peacefully and I left him.

This and several similar experiments made with this child and others possess, I believe, an additional significance in that telepathic transference was successfully achieved during the period of profound and apparently dreamless slumber, between one and three hours after the subject had fallen asleep; suggesting very strongly that the psychic organisation remains accessible throughout the deepest physiological repose. The short delay between the application of the psychic stimulus and the resultant physiological response (also observed on other occasions in two human subjects and a dog) might very well be attributed to physiological rather than psychological sluggishness; it taking, let us suppose, some moments—depending upon the depth of bodily slumber—for the psychic will to bestir the organism and sufficiently to lower synaptic resistances within the nervous system in order to permit of impulse passage and neuro-muscular control.

(*d*) *Undifferentiated E.S.P.*

A Miss W., an undergraduate of Celtic race and temperament, known to possess a fair degree of sympathetic " psychic " faculty, was staying in our house. One Sunday evening she was studying alone up in her bed-sittingroom while my wife and I were downstairs in our drawing-room. Miss W., be it noted, was not in the habit of coming down to our room, except very occasionally, on some particular errand, nor had we any reason to expect her at the time in question. Now it happened that, unknown to Miss W., my wife and I had been discussing telepathic phenomena, and we decided, there and then, to see if we could will our guest to come to the drawing-room. Accordingly we both concentrated our thoughts on our subject, fancying her in the room upstairs and wishing her to come down to see us. In less than a minute Miss W. knocked at our door, and entered. I said : " Hullo, M., do you want something ? " She was, however, very hazy as to her reason for coming down, and excused herself, saying that she had " wanted to ask us something, but really didn't know what it was " ; and she thereafter seemed to search vainly in her mind for an explanation, looking confused and nonplussed. We then explained what we had done. Miss W. agreed that our wishes had undoubtedly been the cause of her coming, but could recollect no special sensation or hallucination other than a sudden urge, of a vague and non-specific sort, " to come down and see us or ask about something".

Subsequently, the experiment was twice repeated successfully and twice failed—probably owing to mental preoccupation of the subject. On each occasion the motor impulse emerged in the subject's supraliminal mind as some trifling excuse of her own imagining, and there would appear to be little doubt that the extra-sensory impulse was itself received subliminally ; any subsequent or concomitant sensory-motor act being seemingly analogous to the autosuggested acts and hallucinations that have been so frequently recorded of suitable subjects during hypnotic and post-hypnotic states.

(3) The foregoing experiments with Miss W. (and others) also remind me that Prof. Broad referred, in his address, to the lack of introspective evidence of any primary awareness in e.s.p.—*i.e.* feelings equivalent to those experienced in connection with the use and *modus operandi* of the normal sensory channels—on the part

of clairvoyants, say. Such a statement might be extended to cover many cases of both spontaneous and experimental e.s.p., in which actual sensory hallucinations do not occur, but merely generalised (usually rather vague or else simple) " feelings " and ideas ; also automatic script, other than that of the " telephone conversation " variety, tiptology, divining by rod or pendulum, and other motor automatisms. But it should be borne in mind that we have no right, and it is, in fact, begging the question, to term " *clairvoyance* " that form of metagnomy, common to trance mediums, psychometrists, dowsers and diviners, in which the visual sense is not stirred into activity. Indeed, the experiences of certain exceptionally sensitive dowsers, as well as the utterances of mediums such as Mrs O. Leonard, and many instances of spontaneous telepathy all point the same moral ; namely, that e.s.p. is primarily a subliminal and unconscious affair, which subsequently and quite secondarily, by auto-suggestion and bestirring of the imaginative faculty, tends to create a variety of sensory hallucinations, of which visual and auditory forms are most common—presumably by reason of our habitual chief usage of those senses.[1]

Now, if extra-sensory perception is, indeed, always primarily subliminal and unperceived, as such, then Dr Rhine's convenient expression (*e.s.p.*) is seen to be unjustified and undesirable in the same way that " *clairvoyance* " and " *psychometry*", as generally used, are undesirable. Had we not, then, better restrict ourselves to one of the well established and more general terms, such as *paragnosis*, *metagnomy* or *cryptopsychy ?* Of the three, CRYPTOPSYCHY is, I would suggest, to be strongly recommended on account of its non-committal nature.

(4) Prof. Broad ridicules the idea that there can exist such a psychic state as one of " unconscious thought " (*vide* bottom of p. 433 to top of p. 434 of his address, *loc. cit.*) : this with reference, I take it, to the supposed mind-reading of his given passive " agent " (M) by an active " percipient " (N). Thus he says with reference to discursive cognition : " In most cases it seems certain that the person from whom the cognition was telepathically derived was not

[1] It would be interesting, in this connection, to find out whether—as one supposes—the pictorial artist and strong visualiser would be prone to clairvoyance as a form of e.s.p. ; whereas the musician, the poet and the literary dramatist, say, tended to clairaudience as a general rule. Blind and deaf (acquired not born) persons might also be worth consideration. In Beethoven, for instance, the imaginative power of the " inner ear " remained unimpaired—possibly it was enhanced—by the decay of the peripheral organs of hearing : a condition analagous to that of the maimed soldier, who feels pains in a limb that no longer exists.

thinking at the time of the fact or proposition concerned". And of the idea of unconscious, or subliminal, thought on the part of the "agent" he remarks: "This, however, would be a wholly gratuitous assumption for which there is no independent evidence, and I shall ignore it."

By denying the existence of unconscious activity, Prof. Broad would appear to deny those multifarious activities of the dreaming, subliminal,[1] hypnotic and entranced mind and of sub-personalities, etc., that have been so prolifically recorded by students of Psychology and Psychic Science. Of those copious phenomena Prof. Broad himself is, naturally, well aware; and to anyone who has witnessed them their reality is beyond all manner of doubt. Yet Prof. Broad would appear to maintain the belief that psychic acts are invariably and solely supraliminal; although I understand that he is prepared to accept the existence and "potential energy" of subliminal memories in a latent and quiescent state—as memory traces, not as active thoughts, ideas and sensations, that is to say. Critical analysis, however, shows, as J. Luys, in his work on the functions of the brain, argued many years ago, that every single unit—or moment-content, as Sidis termed it—of conscious thought is so complexly conditioned by and dependent upon a co-existent multiplex system of other mental (and/or cerebral) units that it cannot possibly be considered as an isolated or momentary phenomenon.

Now suppose that a moment-content of (fore)consciousness be likened to one out of a succession of notes forming a tune as played by an automatic pianola; then the pianola keyboard, strings, etc. may be taken to represent the animal's entire central nervous system, the electric motor or other source of power to represent its vital energy, and the perforated roll to be equivalent to the whole subliminal, but cöexistent ideational content of the mind, that conditions and precisely determines each individual note of the melody or thought-train. But most significant of all, the initial composer of the music will find his counterpart in the individual Will or Central Psyche of the animal personality in question, whose purposeful and, in some unknown manner, energetic guidance and control first determine the melody, and then utilising the mechanical energy, precipitate, as it were, into *space* and *matter* (as bodily

[1] In fully developed automatism—as I have satisfied myself by means of an automatic recorder connected to a specially arranged tiptological device—the foreconscious mind, or primary personality, if you will, can occupy itself conversationally, say, while the other component involuntarily spells out coherent "messages", and supplies, may be, cryptopsychic information into the bargain. Some, if not all, of these messages obviously come from the automatist's own mind, though quite subliminally and unconsciously.

action and sensation) what was previously "potential" in *time* and *mind*. But it is important to note that ideas and mental units must, surely, first possess some real energy of their own before they find supraliminal expression in consciousness and physical action, no matter whether such energy be termed potential or actual!

From this analogy—in the general correctness of which I, personally, place my faith, despite such crudities and imperfections as it may entail—we see that there is every reason to assume the cöexistence of vast ideational complexes or thought trains in what may conveniently be termed, with Myers, the subliminal mind. Moreover such psychic entities should not even be thought of as *serial* structures—except for the artificial purposes of psychoanalysis. They are, rather, integrated and unified in the sense that a natural panorama is, in fact, integrated and unified; and they only appear to possess a serialism or "grain" (like the pictures of a cinema film or the perforations of a pianola roll) when spatio-materialised and analysed in the form of speech and writing or other pantomime. Continuity, however, may be re-constituted when these symbols have been somehow absorbed and translated back into thought in the mind of another animal of the same race and species; though seldom in perfect agreement with the original, unfortunately.

The sum of the foregoing argument is this: that there is every reason, logically speaking, to suppose that the spot-light of what we term attention or awareness only illuminates a very small and trifling patch of the whole vast content of the otherwise sensorially subliminal mind; much as the policeman's bull's-eye throws light upon but a trifling fragment of some great city, of the rest of the machinations of which he is, at that instant, blissfully ignorant. Now, for the policeman to state that the rest of the world was non-existent or inactive, simply because he had no perception of it, would clearly be unpardonable. In like manner, I suggest, that it is unpardonable to speak of the subliminal mind as being necessarily altogether quiescent or latent—much less non-existent—simply because the moving beam of foreconsciousness happens to illuminate it only point by point. Indeed, had not our primary psychic intelligence a very much wider grasp of the mental structures under its control than we imagine it to have, when we deny the existence of unconscious thought, etc., coherent and logical reasoning would be impossible; still more so the "inspirations" of genius.

I gather that Professor Broad, while admitting the existence of the subliminal mind, with its memories, as a sort of reservoir or more or less materialistic filing cabinet, would differ from the view here sponsored, in that he would deny actual activity to the subliminal mind, and prefer to regard it as a potential state of knowing, or "disposition" to know, certain facts—*i.e.*, memories and experiences.

Everyone is familiar with the experience of having had some argument or idea all ready worked out in the back of his mind for exposition, and then accidentally lost the tag or clue to the whole affair, while knowing full well that it was still there and active, in the sense of being "on the tip of the tongue"; though temporarily "potential" and somehow inhibited from emerging into full foreconsciousness:—then we may safely affirm that subconscious ideas and aggregates can and do exist in a subliminal phase, latent and "unconscious"[1] if you will, but none the less real and "vibrant". Their dynamism, however, happens either to be below the energy level at which they cause nerve impulses to overflow at the synapses, thus setting in action various neuro-muscular mechanisms, and expressing themselves objectively and consciously, or else they simply happen to lie, temporarily, out of the immediate focus of interested attention.

If, then the entire mind, with all its memory and ideational content be termed the subliminal, "subconscious", or "unconscious" mind; the supraliminal or foreconscious region may be said to consist merely of a relatively minute and peak-like portion, which has, through force of subjective or objective circumstances, momentarily risen to such an energy level—to use a physical expression—as to surge up and "spill over" or, "precipitate itself" in the form of neuro-muscular and sensory activity. Moreover, we find ourselves vaguely aware at the same time of a sort of conditioning environment of other associated ideas that have just expressed, or are just about to express, themselves explicitly, in like manner. We are also yet more dimly aware of an indifferentiated and increasingly "distant" background of thoughts that we know to belong to the thought train, or psychic melody, at present in process of self-expression. We cannot, however, usually quite define or grasp this great residuum; though we can, with a little introspection, sense that it gradually and in many diverse directions links up with and roots deep into the rest of our potential, latent or subliminal mind. We also know that the pattern and content of each association plexus is subject to subliminal modification and that fresh associations are built up without any conscious and voluntary attention on our part, as primary personality or "observer number one".

[1] Naturally I prefer the term subliminal to either subconscious or unconscious, as it appears more truly to fit both psychological and physiological facts.

The entire mind, therefore, may be supposed—and, indeed, directly " felt "—to exist at an infinite number of energy levels, of some sort, proportionate to the relative intensity of what we call consciousness. In that case, zero level might be said to correspond to absolute ignorance or absolute forgetfulness (if there be such a thing) of a given phenomenon; *i.e.* non-existence of any co-relative idea in one's mind. Reasoning thus, it is conceivable that a crypto-psychic percipient should successfully " tap " and cognise the contents of a given " passive " agent's mind, provided that such contents possessed a fair positive energy value, even though they were below the limen of consciousness of their proper owner at the given time. In that case, " mind reading " might be accepted as a very fair term to describe the process.

Finally, it seems likely to me, all things considered, (1) that *consciousness may very well be concomitant with the passage of nervous impulses across a synapse*:—no overflow, no consciousness or awareness; and (2) that *the relative intensity of consciousness may be represented as a joint function of the relative resistivity of the active synapses and of their applied nervous potentials.*[1]

Consciousness, thus regarded, is equivalent to a physiological process: the precipitation, so to say, of something out of mind and time into matter and space, or the materialisation of an idea. In these circumstances also, when a sensory impulse across the synapses, due to the stimulation of the peripheral nerve endings, occurs, the reverse thing will happen: consciousness will again arise in the organism's body-brain system, and a memory thereof be stored in the immaterial and temporal psychic organisation. In what manner such translations may occur I do not, of course, pretend to know: and I freely admit the difficulties of comprehension that lie in the way of any such hypothesis as that ably put forward by the two Bousfields in their provocative work " The Mind and its Mechanism". Similar difficulties, however, exist in modern physics in regard to the problems of material radiation and absorption of energy. So that we need not be unduly disheartened by any mere difficulty of comprehension, especially since there appear to be good reasons for the purely mechanistic of physical memory traces to fall into disrepute, as McDougall and others have already pointed out.

But besides auto-consciousness, thus considered as a semi-mechanical affair, there is also, undeniably, a second factor, namely *attention*,

[1] The magnitude of the resultant nerve current might be roughly equivalent to the intensity of sensation, and a law similar to Ohm's law for electrical conduction might be found to express the whole situation, speaking from the physiologist's point of view.

which is clearly controllable by the Will. And this is true to such an extent that either (*a*) elements that would otherwise remain unconscious, or but dimly so, can be voluntarily stimulated into action by proper searching and focusing with the spot-light of attention, or (*b*) strongly auto- and sensorially stimulated elements can be more or less disregarded in favour of weaker ones, by a voluntary diversion of attention. I mention this aspect of consciousness, in passing, lest it should be thought that I favour a purely mechanistic and materialistic view of the nature of Mind and Will—which is far from being so. I do believe, however, that one cannot safely afford to neglect the nervous mechanism—memory traces or no memory traces—and the various formations, limitations and repercussions that it patently impresses upon the mind and spirit of its own metaphysical possessor and, one may perhaps believe, original creative architect, the individual Ego or central psyche. In other words, the central psyche may be supposed to represent not only the initiator and foundation stone of the living organism, considered both mentally and bodily, but also the final recipient and beneficiary of the psychophysical experiences of that organism during its incarnate existence.

SOME COMMENTS ON MR TYRRELL'S PAPER ON INDIVIDUALITY

By H. F. Saltmarsh

In his most interesting and suggestive article on Individuality published in *Proceedings* XLIV, Mr Tyrrell discusses a problem which is fundamental both for psychical research and philosophy. As a full and free discussion of this matter may possibly result in some light being thrown upon its obscurities I venture to make a few comments on his arguments and to put forward a few suggestions. Though I should succeed in accomplishing nothing towards a solution of the many difficulties, it may be that, if the ball be kept rolling, others far better qualified than myself will be tempted to take a hand in the game even if only for the satisfaction to be derived from exposing the errors of other people.

Mr Tyrrell's first question, " What is it that is characterised by word association tests ? " contains, as questions frequently do, several tacit assumptions. He assumes, for example, that some form of dualism as regards body and mind hold good. If one adopted a monistic theory the answer to his question is obvious. As there is only one element in personality it must be that which is characterised by the word association tests, assuming, as I suppose we do, that they actually characterise something.

However it seems pretty clear that whatever type of theory be adopted, whether, that is to say, one is a monist, a dualist or a pluralist, whether one accepts emergence or not, the physical, if there be such, must play some part in the word association tests, the psychogalvanic reflex and all the rest ; for the response to these tests is given by means of the physical organism just as the tests themselves are received through it. Even if one be a subjective idealist of the purest water and deny altogether the reality of matter there is always that inconvenient illusion of matter which will mix itself up in all our doings ; and even an illusion, especially one which is so good an imitation of reality, can produce its effect. Further, Mr Tyrrell assumes the stratification hypothesis, *i.e.* that hypothesis which postulates levels and thresholds in the mind. I do not doubt that his question could be framed to make it applicable to the monadic hypothesis equally as well.

His second question, which deals with survival, contains several other assumptions besides those mentioned above.

If the stratification hypothesis be adopted we cannot assume without examination that the division between supra- and subliminal persists after death. It may quite well be that this is imposed on the mind solely as the result of its incarnation and that it is not an intrinsic feature of mental structure. On the monadic hypothesis it may be the whole hierarchy of monads which survives, or the dominant and some of the subordinates, the dominant alone or else some supra-dominant monad, if such there be—what Myers called the transcendental self. I suppose that it is theoretically possible that the only survivor might be one or a group of the subordinate monads, though it is doubtful in such case whether that would be called a survival of the personality. There are other possibilities which I will not mention lest I become too tedious.

Further Mr Tyrrell assumes that the separation between mind and body is complete at death. Doubtless the gross physical body is destroyed but I do not think that Sir Oliver Lodge's suggestion of an etheric body can be summarily dismissed. I do not know of any positive evidence for its existence but it is at least a possibility. And, if I may make an even more fantastic suggestion, it is theoretically possible that some very small part of the physical body escapes at death and remains attached to the mind. Until incredibly accurate and apparently impossible experiments in weighing, etc., just before and just after death have been performed this cannot be entirely ruled out. I admit that the suggestion is perhaps absurd and not to be taken seriously, all I claim is that it is not impossible.

My object in mentioning these alternatives is to show that the whole matter is so complex, that what little knowledge we have is so vague and unreliable, that speculation is, to say the least, premature ; besides we are by no means sure that there is survival at all.

Mr Tyrrell then asks, " What is individuality ? "

Now it seems to me that Individuality is a relative term. What may properly be termed an individual from one point of view and for one purpose of discourse, is equally properly termed a plurality from another point of view and for another purpose. A chair is undoubtedly individual if I want to sit on it, but to the cabinet maker who is making it each leg is an individual. Mr Tyrrell says that a block of water is " just stuff ", but some blocks of water, such as, for example, waves on the sea, may be highly individual and must, in certain circumstances, be treated with the respect due to an individual, as those who are accustomed to sailing in small boats know only too well. If they are not so treated they may make their individuality felt in unpleasant fashion by breaking aboard and filling you up. The lines of demarcation by which we divide up the external world into individuals are purely arbitrary and this applies to some extent to human beings as well as to inanimate objects. We may legitimately speak of Parliament as an individual, or of the various subdivisions thereof, such as the Cabinet or the Opposition, as individuals, yet they are all made up of individual members.

But as regards the self I think that the position is somewhat different. In spite of all the facts which seem on the face of them to point to the opposite conclusion, I have an inescapable conviction that I am, somehow or other, fundamentally one and only one person. Behind the phenomena of dissociation, multiple personality, subconscious mentation etc. there looms the unitary I.

It may be that this instinctive conviction of unity is nothing more than an illusion, or perhaps an *a priori* form of thought. If the category of number be imposed on reality by the mind in the same way as Kant taught that time and space are imposed, then the real personality is neither one nor many. However, as we cannot help thinking in these terms it seems that we are ultimately forced back to unity if we carry our analysis far enough, for the simple reason that, having arrived at unity, we can go no further. The unity at which we arrive may not, of course, be a personality. Atomistic theories of consciousness are rather discredited nowadays, but there are others more respectable which take this standpoint, *e.g.* Professor Broad's theory wherein the self is held to be made up of a physical body and a psychic factor, neither of which, if capable of a separate existence, would be a self. Further, it may be that the unity of the self is an unity of form or structure rather than an unity of substance, just as the unity of the physical body persists in spite of the change which goes on in the cells which compose it, or as Parliament remains an unity though some members may resign or die and be replaced by others. But then the question arises, Is not, in the last resort, everything which exists only form and is pure substance anything more than a metaphysical abstraction?

MacTaggart defined substance as that which possesses qualities and stands in relation yet is not itself a quality or a relation. If this be a correct definition it seems that individuality can only attach to substance in virtue of the qualities which it possesses, that is to say in virtue of its form.

However if Bergson be right, form is continually changing, so we cannot base individuality on the persistence of form. All that is left for us is some sort of continuity of history. I do not profess to be competent to form an opinion on the validity of Bergson's doctrine, but, whether it be universally true or not, I think that it is obvious that most of the individuals with whom we are acquainted are forms in a state of flux and that the usual criterion which we apply is the presence or absence of continuity of history. It is true that we do not always demand unbroken continuity when applying this criterion but are willing to overlook some gaps. I think that this would give the answer to Mr Tyrrell's question about the man who lost and then recovered his memory.

After all, as I have said above, individuality is relative to the point of view and this may be well exemplified by the hypothetical case which he cites. Whatever view a psychologist might take as to the persistence of individuality in such a case, there is no doubt that a lawyer would hold that loss of memory would not entail loss of individuality, that rights, such as ownership of property, and duties, such as liability for debts, would remain unaltered.

The psychologist, however, would very probably not accept the legal standpoint, unless he happened to be one of the man's creditors, and it would then depend on what criterion he employed in judging individuality and how strictly he applied that criterion. If continuity of memory be adopted, he must be prepared to decide whether the gap is or is not sufficiently large to constitute change in individuality, for gaps of some size he must in any case overlook. But he might take continuity of moral and intellectual character as his criterion, he might say for example that the man remains morally and intellectually the same individual, subject, of course, to the effect of the shock of loss of memory. But as his character is always changing under the influence of events which happen to the man, this shock, though exceptionally far-reaching and sudden in its effects, can be considered as essentially the same in kind as any other of the events which are continually causing change.

That there are difficulties in the matter of individuality no one will deny. Consider the case of the amoeba which propagates by simple fission. Is the parent cell identical with both or either of the two daughter cells or not? From one point of view it is clearly not identical, yet Weisemann founded his doctrine of the potential immortality of the cell on this phenomenon.

The phenomena of multiple personality are even more puzzling, in particular those cases where two of the secondaries have been fused into one by treatment. We might perhaps sidestep this difficulty by holding that where two apparently separate secondary personalities are thus fusible, the separateness is only illusory and they are really different phases of the same personality. So far as my recollection of the cases goes, successful fusion has always been between secondaries which were more or less complementary to one another: I doubt whether any attempt at fusion would be made by the physician were this not so.

But I think that it may be said that these and other equally well-established facts do, at the least, show that the ordinary common-sense view of personality as an indivisible entity cannot be accepted. What they do *not* show is the alternative which should be accepted as correct: hence all the array of different theories.

I have mentioned so far only the monistic and the dualist types, there are, however, theories which postulate more than two different kinds of element.

It seems to me that there is much to be said for a theory somewhat on the lines of the old-fashioned orthodoxy of body, soul and spirit. Such a theory is to be found in a highly elaborated form in the Vedanta. There the Atman, or real spiritual self, is what I suppose must be called supra-individual, it transcends all categories when freed by enlightenment from the bonds of illusion. It is that which gives unity to the individual when conjoined to and informing the combination of the other factors, viz. body and life. The theory, as presented in the Vedantic books is extremely complex and difficult to understand, perhaps impossible for most Western minds, but the general idea is clear and might form a basis for a less elaborate hypothesis which would possibly be acceptable.

Myers speaks of a transcendental self and if one takes this to belong to an order of nature totally different from the physical and psychic factors we have the materials for a pluralistic hypothesis. We should then have the physical body and the psychic factor, whether in the form of a stratified consciousness or a hierarchy of monads, forming a living organism; this would be informed by the transcendental self, which, though in its essential nature supra-individual, would contribute to the whole that unity of which, I imagine, we all have an inescapable conviction. The relation between the transcendental self and the living organism might be thought of as an unilateral relation of coconsciousness, that is to say, the transcendental self is coconscious to the fullest extent with the monads or stratified mind, but they are not coconscious with it.

As a rough illustration of what I mean one could imagine an actor who threw himself so thoroughly into his parts that while he was actually acting he almost became for the time being the imaginary personality whom he was portraying. Suppose that it was Irving playing Hamlet, then Hamlet-Irving would represent the "me as I know myself" but would know nothing of the real Irving; the real Irving, however, would know all about Hamlet-Irving.

I refrain from elaborating this idea any further, but I imagine that Mr Tyrrell must have had something of this sort in his mind when he wrote, on page 11, " It would seem that we must be wrong in speaking of individuality in the plural and *equally wrong in speaking of it in the singular.* Individuality would be something not subject to the category of number".

On such an hypothesis one could imagine that the relations between the monads or strata, whichever they may be, which constitute the second factor, would be telepathic, as has been suggested by Lord Balfour, and I suggest further that such telepathy would be Interactional Telepathy. As regards the relations between the transcendental self and the psychic factor I would suggest that these also are of the nature of telepathy, but in this case they would be of the kind which Professor Broad calls Intermental Confluence; it would, however, be non-reciprocal, a sort of one-way traffic.

This brings me to the last point in Mr Tyrrell's paper on which I beg leave to make some comment. On page 9 he speaks of " sharing an experience " and " direct prehension by two minds of the same experience " ; on page 10 he says " it looks uncommonly like as if there were a sharing of experience".

May I venture to suggest that he has overlooked an important consideration ? Any experience whatsoever is not a simple, clear-cut mental event with sharp edges and clearly defined boundaries, rather it is an inseparable part of an organic whole which, if forcibly taken out of that whole, becomes something different. Every mental event 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. Moreover, an event, to be in consciousness at all, must be recognised as such and such however vague such recognition may be and this recognition is determined largely by past experience.

I suggest therefore that every mental event contains within itself elements derived from the whole past history of the individual. I hate to use the word " impossible", it smacks too much of omniscience, so I will content myself with saying that it is highly improbable that any two numerically distinct individuals should have identical histories and, in consequence, exactly identical backgrounds against which their experiences occur.

Of course all this is simply a restatement of the fundamental principle of " The Unique Ownership of Experience " as laid down by Professor Broad.

In view of these considerations I cannot see how two persons can share an exactly identical experience and, if this be so, it seems needless to discuss hypotheses based upon such a sharing.

PROCEEDINGS

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PART 149

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

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

III

INTRODUCTORY

1

General: Towards the end of my last paper on this subject (*Q.S.T.P.*. II, *Proc. S.P.R. Part* **141**) I briefly described a preliminary attempt to investigate the autonomy of the two communicating controls known as "John" and "Etta". This depended on obtaining reactions (times and reproductions) to word association tests given to the personalities concerned when manifesting through two different mediums, Mrs Leonard and Mrs Sharplin. I attempted to eliminate the effects due to the mediums themselves by the method of "partial correlation", and I obtained positive results which indicated that some extraneous cause was at work other than any similarity which might exist between the mediums themselves.

Fortunately, I guarded myself fairly thoroughly against possible imperfections of the method used, for Professor Fisher has kindly pointed out that it is not to be relied upon in the context. The reason, as I understand it, is broadly speaking this: that wherever the eliminated variables (in this case the data obtained from the two mediums in their normal states) are themselves liable to error, the resultant partial correlation coefficients are always likely to be too large; precise results are obtainable only when the values of the eliminated variables are known with exactitude (*e.g.* dates). The figures in question must accordingly be discarded, but the work has at any rate served a useful purpose in encouraging the experiment discussed below which has led to extremely promising results.

The main material used consists of the data obtained by Mr Drayton Thomas in five special sittings with Mrs Leonard and five with Mrs Sharplin held on various dates in September, October and November 1935. I have also, as will be seen, made certain comparisons of importance between this material and that obtained from Mrs Leonard and Feda, and from John and Etta, in 1933 and from the two first in the Irving experiment of 1934.

The list of words used in the Leonard-Sharplin tests was the same as that for the last-mentioned experiment; that is to say, it was derived from the words of the original (1933) list by substituting for each a word more or less closely associated with it. (Cf. *Q.S.T.P.*, II, p. 349.)

It may be said at once that, to superficial inspection, the Sharplin data were singularly unpromising. Mr Drayton Thomas, from a study of the responses, reported to the effect that he could find nothing at all suggestive of either John or Etta, while I myself, after a preliminary examination of the figures, regarded a negative outcome as a foregone conclusion to be worked out and recorded only for the sake of formal completeness. That results so well-marked as those actually obtained should ultimately have emerged constitutes a remarkable testimonial, so to speak, to the power and sensitivity of the method ultimately employed, for which I am more indebted than I can easily express to Mr W. L. Stevens of the Galton Laboratory. I must also record here my gratitude to Mr L. H. C. Tippett (Author of *The Methods of Statistics*) for timely help at a critical moment.

I do not propose to overburden this paper (which in any event is bound to be almost unreadable) by describing in detail how, despite prolonged residence abroad, I maintained the old English characteristic (according to my Continental friends) and "tried all the wrong ways first"; but I think the final stages will be more comprehensible if I first deal with certain "routine" evaluations and comparisons. This will at least show the hopelessness of applying ordinary methods to the main problem involved; moreover some of the results are of very considerable intrinsic interest, and appear to illuminate to an appreciable if incomplete extent the nature of the psychological mechanisms with which we are dealing.

2

Notation, abbreviations, etc.: In what follows, RT stands for Reaction Time(s), RPN for Reproduction(s), or for tests on these observables. L stands for Mrs Leonard and S for Mrs Sharplin in their normal states (I have not concerned myself further with the " prepared " state of Mrs Leonard). F is Feda and S′ is " Silver ", Mrs Sharplin's corresponding control. J and E stand for John and Etta as purporting to manifest through Mrs Leonard ; J′ and E′ the same through Mrs Sharplin. Towards the end, notably in Table V, I use the contractions 33-35′ to indicate the comparison of the J and E (Leonard) data of 1933 with the J′ and E′ data (Sharplin) of 1935, and like contractions *mutatis mutandis.*

x stands for any quantity of the kind we happen to be considering at the moment.

$\bar{x}$ is the mean (average value) of all quantities like x.

$S(x)$ is the sum of all such quantities.

$S(x^2)$ is the sum of the squares of all such quantities.

$\sim$ has the usual significance of " approximately " or " difference between " according to context.

$>$ is " greater than " ; $<$ is " less than ".

RN is Result Number(s).

The results are numbered in much the same way as those of *Q.S.T.P.*, II, but the first is 3001, the initial 3 corresponding to the number (III) of this paper ; the results of *Q.S.T.P.*, II, may be thought of as numbered from 2001 to 2203, and those of the next will begin with 4001. The same system of decimalisation of results in the same line of a Table, or otherwise closely related, is adopted as before (*Q.S.T.P.*, II, p. 323).

Part I

Preliminary Survey of Material

A. Individual Consistencies

3

In Table I are given the values of the various Individual Consistencies (previously called " Individualities ") shown by the personalities examined.

I have introduced what I think is an improvement here by giving, in addition to the values of z, quantities which may be regarded as percentage consistencies. The z's are mathematically indispensable, but they are not very informative to the untutored eye, whereas a percentage conveys something to almost everyone.

Any given z can be transformed into a correlation coefficient, in cases where we are comparing two different personalities, by the relationship

$$r=(e^{2z}-1)/(e^{2z}+1),$$

and this is easily shown to be preeisely equivalent, in such cases, to

$$r=(\mathrm{W}-\mathrm{WP})/(\mathrm{W}+\mathrm{WP}),$$

in the notation of *Q.S.T.P.*, I, Appendix II, the letters standing for the mean squares of the quantities eoneerned. *Mutatis mutandis,* I here take

$$r=(\mathrm{W}-\mathrm{OW})/(\mathrm{W}+\mathrm{OW}),$$

and, in eaeh case, the pereentage similarity or eonsistency as $100r$.

This gives results whieh are both intelligible and in accordanee with eommon sense ; for, if there were no similarity or consistency, the pereentage thus obtained would be zero, while if it were perfect the pereentage would be 100.

4

The most striking feature of the Table is the high consistency of the eontrols, Feda and Silver, as eompared with their mediums. This is observable in every case with the Leonard-Sharplin material, though it is untrue in three eases out of four for the results of the earlier experiments, whieh I have given on the right of the Table for purposes of eomparison. I do not quite understand this discrepancy, but there seems no doubt as to the genuineness of the effect in the present ease ; for we have, taking RT and RPN together :

	Means	Differences	S.E.	P	RN
L -	- ·20995				
F -	- ·34875	F-L ·13830	·1005	·17	.........3089·1
S -	- ·16970				
S′	- ·48675	S′-S ·31705	·1005	<·005	......3089·2

while eombining the figures for mediums and controls gives :

Mediums	·18980				
Controls	·41775	C-M ·22795	·07106	<·005	...... 3089·3

Taking the First Thomas and Irving Experiments (RT & RPN) together gives :

L -	- ·28912				
F -	- ·33095	F-L ·04183	·05431	44	...........3089·4

whieh is altogether negligible, while for the difference between the Medium-eontrol differenees, we have

·22795 – ·04183	·18612	·08832	·04	............3089·5

so that while we can hardly dismiss the discrepancy as fortuitous it may fairly be regarded as due to some masking cause in the earlier experiments and not to any essential change in the character of the personalities.[1]

I infer that, over the comparatively short periods of a few weeks each, which cover the durations of the experiments concerned, controls, or at any rate these controls, are significantly more consistent than their mediums. This implies the not-unimportant conclusion that they are derived from, or based upon, much more stable strata of the total personality or " psyche " than those which determine the reactions of the normal consciousness. We shall find further confirmation of this, in the case of L and F, when we come to consider the Intra-group comparisons.

The communicators in the Leonard-Sharplin Experiment come between the mediums and controls, the comparable figures being

J & E - ·21870; J′ & E′ - ·25435

while for the First Thomas Experiment the comparable figure is

J & E - - - - ·9490

which is less than the value for either L or F. There is accordingly here a suggestion that, in this respect, communicators resemble normal mediums more than they do controls; but the difference is easily shown to be insignificant (though it may, of course, be real despite this), while the Irving communicator " Dora " (D) is, faintly, anomalous. Pooling all data and taking means all round, we have

Mediums ·2295. Communicators ·2667. Controls ·3743.

The communicators (which here include D) clearly favour the normal mediums, though only to a slight extent; the fact may, perhaps, be taken to suggest that communicators are more like normal people than they are like secondary personalities, such as I consider Feda and Silver to be. But it would be unwise to attach appreciable weight to this.

Apart from the rather poor showing of Leonard on RPN and Sharplin on RT, there is nothing more here that is worth noting, unless it be that Silver's figure of 52·7 per cent. for RT is the highest consistency yet recorded.

[1] In the above S.E. is the Standard Error of the difference concerned. We may also here conveniently note the following *errata* in *Q.S.T.P.*, II, Table II: In RN 42, for ·2378 read ·2738; in RN 43, for ·4642 read ·4588.

B. Intra-Group Comparisons

5

The relationships of similarity and difference between the personalities forming the Leonard and Sharplin groups, within those groups, are shown in Table II. I have added the figures for percentage similarities, obtained by the method already indicated, but I have not thought it worth while to work the covariances, such as were given in *Q.S.T.P.*, II. These are laborious to evaluate and, as a rule, of very little interest. At the foot of the Table I have added a number of percentage similarities from earlier experiments for purposes of comparison. The symbols, etc., have their usual meanings, but I have halved the probabilities for similarity, as indicated (P/2), so as to show the chance of a value of the observed magnitude *and* in the observed sense (similar or countersimilar) being obtained accidentally.

I have restricted the work to the more evidently important types of comparison. We are naturally interested in the relationships of controls and communicators to their mediums and of the communicators to each other; but the relationships between controls and communicators seem of little importance at the present time, and nothing is to be gained by overburdening the work with irrelevant figures merely for the sake of achieving a formal completeness.

6

The first point to be noted is that, on RT, Feda is again countersimilar to her medium; thus clinching if need be, the point made in *Q.S.T.P.*, II, regarding her status as a secondary personality.

Silver, it will be noted, is not countersimilar to normal Sharplin, but his similarity is so altogether trifling, that it seems more reasonable to ascribe it to chance errors than to maintain that he is of a different psychological type from Feda and to create a new category for him.

Actually, Feda is slightly similar, in this case, to J and E (the figures are not given in the Table); so that, taking all known comparisons within the Leonard manifold into account, including those with " prepared " Leonard and those of the Second Thomas Experiment, we have:

	+ve	−ve	Total
Feda Comparisons	4	9	13
Other ,,	13	2	15
Total	17	11	28

which gives, using " Yate's adjustment " for small numbers,

$\chi^2 = 6{\cdot}93$. P less than ·01 3090

as the chance of the effect being accidental.

7

We may next consider the LL and FF comparisons (RN 3017 & 8, 3027 & 8). These are between the normal Leonard and Feda data respectively for this (Leonard-Sharplin experiment), and the Irving Experiment of 1934, in which, as already stated, the same list of words was used.

Note first the negligible similarity and highly significant difference between the two sets of L reaction times, and contrast with these the very high similarity and negligible difference of the corresponding figures for F. Just as Feda was much more consistent than Leonard over the short period covering this experiment, so she shows herself enormously more stable over the (approximately) nine months interval separating the two experiments. The difference is significant, for we have :

Difference in z's = ·3822. S.E. = ·14213. P = ·01 3091

Perverting the poet, we might almost say of Feda " Time cannot wither, nor custom stale, her infinite consistency ". She is evidently derived from strata far less susceptible to the influence of current events than those which we recognise as " normal Leonard ".

The effect is not nearly so marked in the case of RPN. This is in accordance with the view, which I have always maintained, that the reproduction test penetrates to deeper levels than does that for reaction times. It thus strikes more permanent elements in Leonard than RT does ; but this circumstance is irrelevant in the case of Feda, because, when *she* is tested, whether on RPN or RT, the deep layers from which she is derived are already uppermost, so that the superior penetration of RPN has no scope.

8

Connected also with the foregoing is, presumably, the markedly greater differences which the controls F & S′ show from their mediums, in respect of RT, compared with those shown, on the whole, by communicators. The latter are somewhat " freer ", as judged by this criterion of difference, at the RPN than at the RT level, while the reverse is significantly the case with the controls. Mean figures are :

	RT	RPN	Diffce.
Communicators and Mediums -	·0709	·1441	– ·0732
Controls and Mediums - -	·3544	·1114	·2430
Difference - - - - -	·2835	·0327	·3162

The differences – ·0732 and ·0327 are evidently insignificant ; for the significance of the others, we have :

·2430 has S.E. ·09675 ;	whence P/2 is	$\sim$·01	..........3092·1.
·2835 ,, ,, ·0684	,, ,,	$< 10^{-4}$	.........3092·2.
·3162 ,, ,, ·1185	,, ,,	$<$·005	.........3092·3.

We conclude that the controls really are freer on the RT level than are communicators, or than they themselves are at the RPN level. The reverse is the case with communicators, but I doubt whether we are entitled to take the significance of the value ·3162 as assurance that this reversal is real. There is, however, a pretty strong suggestion to the effect that at the deeper level reached by RPN controls *converge* while communicators *diverge* with respect to their medium, the former becoming more and the latter less like her.

So far as it goes, this affords fairly strong evidence of the autonomy of communicators and the fundamental unity with the medium of controls ; but the figures are not coercive, while the Irving experiment is definitely anomalous. Pending confirmation, or an explanation of the anomaly, the indication must not be taken too seriously.

9

When we turn to the relations between communicators and mediums from the more ordinary point of view, we find that, whereas (on RT) J & E are sharply differentiated from L, J′ & E′ are quite indistinguishable from S. Mrs Sharplin's " controlled " state (Silver) is particularly well defined, but it is clear that when, so to say, she abandons this for the " communicator " condition she reverts, at the RT level at any rate, to her normal condition, which is but little affected by J′ and E′. The value of ·4314 for S and E′ is, indeed, the highest reaction time similarity ever recorded.

Leonard shows the same tendency, though to a much less extent, for we have, taking the means for this and the First Thomas experiment together :

	Similarity	Difference
LF - - -	– ·11795	·3150
LJ & LE - -	·0962	·1383

According to the point of view we ultimately adopt, we may attribute the difference between Leonard and Sharplin here either to the lack of practice which the communicators have had with the latter medium, or to the short time that has been available for building up the " histrionic poses " of the alternative hypothesis.

Leonard has shown an appreciable, though barely significant, improvement in this respect, since the first experiment ; for we have, for the LJ and LE means :

	Similarity	Difference
First Experiment -	·1937	·0297
This " -	–·0013	·2469
Difference - -	·1950	·2172

The differences have standard errors ·1086 and ·0875 respectively ; so that, for their significances, we have :

For similarity : $P = {\cdot}07$ ....3093·1
For difference $P = {\cdot}02$ ....3093·2

while if we take the two together, which is probably legitimate, we find

Mean of differences $= {\cdot}2061$. S.E. of mean $= {\cdot}0697$. $P < {\cdot}01$ ...3093·3

Points such as these are, however, of comparatively minor interest at the present stage of our investigation, though they may perhaps be useful straws to indicate the direction of the wind.

C. Inter-Group Comparisons

10

Much more interesting and of potentially greater importance for our present investigation are the inter-group comparisons given in Table III. I have again confined myself to the more obviously interesting comparisons, for I see little to be gained at present by comparing, say, J with S′ or E′ with F. I have, however, included the " cross-comparisons " JE′ and EJ′, because of the known close similarity, from *Q.S.T.P.*, II and Table II of this paper, between J & E and J′ & E′. If these are autonomous entities and very like each other, we might reasonably expect the cross-comparison as well as the direct similarities to be high. As before, I give percentages for similarity and halve the probabilities, " for sense ", in the case of this quantity.

Probably the most striking feature of the Table is the extraordinary resemblance between Feda and Silver in respect of RT. This is repeated, though to a much less extent, by the reproductions also, and is only rendered the more remarkable by the significant differences also found in each case. Leonard and Sharplin themselves show no kind of resemblance to each other—they are, indeed, insignificantly countersimilar—as regards RT, where their difference is highly significant; while their similarity and difference are alike negligible in the case of RPN.

It is tempting to rush to the conclusion that there is a specific "control state" common to all mediums; but this, I think, would be a far too hasty generalisation. More probably it is only some more or less coincidental likeness of character and experience that has caused L and S to split off such highly similar secondaries. None the less, so striking a resemblance between a young Hindu girl and an erstwhile American brave (as they are alleged to be) is, to my mind, quite incredible; the similarity in question accordingly greatly weakens, in my opinion, whatever claims either might have to be accepted at his or her face value.

I may say here that among the data collected by Mr Drayton Thomas from Mrs Sharplin were three sets of reactions from a personality purporting to be Feda manifesting through her, as well as a number of observations on "Jinnie" who is a kind of secondary control. I have not yet made any study of these data, thinking it better to concentrate on John and Etta for the time being; but I hope to examine them in due course, when I attempt a more thorough study of controls generally than I have yet been able to undertake.

11

It will be noticed that, on RT, the communicators "follow their mediums" in the sense that both JJ′ and EE′ similarities are feebly negative, like LS; but I doubt whether any weight should be attached to this circumstance which is probably quite accidental. The cross-similarities are about equally feeble in a positive sense. It is evident that little support for the hypothesis that J and E are autonomous is to be expected from a study of RT; these data, at any rate, give none at all.

On RPN, on the other hand, the position is somewhat more promising, for JJ′ & EE′ similarities are both appreciably higher than that for LS. The Difference, however, is not significant, for taking means, we have

	Mean
Mediums	·1152
Communicators	·2848
Difference	·1696
S.E.	·1231

whence Diff./S.E. is 1·378 and P/2 is ·085 ....3094·1

Even if we count in the cross-comparisons, we have

	Mean
Mediums	·1152
Communicators	·26825
Difference	·15305
S.E.	·1123

whence Diff./S.E. is 1·363 and P/2 is ·09 ....3094·2

I give these values, which are of no intrinsic significance, mainly for the sake of emphasising that, *even if they were highly significant*, we should not be entitled to draw any positive conclusions from them regarding the autonomy of John and Etta. We have just received an unmistakeable warning. in the shape of the remarkable resemblance between Feda and Silver, to the effect that, however different two mediums may be in their normal state they are quite capable of achieving a striking similarity when in trance. Thus, broadly speaking, *no* " straight " resemblance between J & J′ or E & E′, however strong and however much greater than that between L & S would justify us in doing more than conclude that there is a stronger likeness between the " communicator " states of the two mediums than between those we know as normal.

Still, we may reasonably conclude, despite the foregoing and the feebleness of the indications, that *if* there is any good evidence for the autonomy of John and Etta to be found anywhere we are more likely to find it among reproductions than among reaction times.

D. Inter-List Comparisons

12

It will be remembered that in the course of *Q.S.T.P*, II (*q.v.* p. 350) I compared the reactions given by the same personalities (N, P, & F) in response to the two different lists of words used in the First Thomas and Irving experiments. That is to say, I compared the reactions given by normal Leonard when tested with List A with those given by her when tested with List B, and similarly for prepared Leonard and Feda.

As already explained, the words of List B were derived from those of List A by substituting for each a word more or less obviously associated with it, and I rather expected that there would be some reasonable degree of similarity shown when the reactions to the two lists by the same personality were compared. In this I was, in the first instance, disappointed; but I have since made a considerable number of comparisons of this kind, of which the more important are given in Table IV. I should perhaps explain that in recomputing the results of the original comparisons I have incorporated corrections for practice and fatigue, which I had previously ignored and, in the case of Feda, I have rejected the reproductions of the first sitting in which, evidently, she did not realise what was required of her, and gave only one correct reproduction in seventy-five; this last accounts for the considerable difference between RN 3051 and 2134.

13

As was perhaps to be expected, most of the similarities found are somewhat slight, particularly as regards RT. Here, indeed, the only really striking result is RN 3058, which shows Feda as again significantly similar to Silver, despite the change, so to say, of list. It is distinctly curious that she should be, as it were, more faithful to him than to herself (3052) though this is reversed for RPN. Something odd is evidently involved here, though frankly I do not understand just what it is.

It is perhaps just worth noticing in passing that the RT values for JJ′ and EE′ (3055 & 6) are both positive and, as such, better than the straight intra-list values (3039, 3040) of Table III; but the figures are too small to be significant.

On RPN they are negligibly inferior to the straight comparisons (3045 & 6); EE′ is significant as it stands and JJ′ not far off it; while if we take the mean, we have

Mean$=\cdot 2389$. S.E.$=\cdot 0822$. P/2$=\cdot 0017$ 3095

which is very definitely significant. It is tempting to find here a further and stronger indication of autonomy, for it is difficult at first sight to understand just how likeness between the two mediums can produce the effect when two different list of words are involved. But the general considerations of section 11 above still hold and it may well be that by forcing the comparison to a deeper level, as the use of inter-list data undoubtedly does, we have merely shifted to a region where the mediums are more alike than they are superficially.

The fact remains, however, that comparison of J′ & E′ with their prototypes of the 1933 list gives results just about as good, on the whole, as comparison with the Leonard versions tested with the 1935 list. It was this discovery that led me to use the 1933 material in the analysis of Part II of this paper, with results, as will be seen, of considerable interest.

14

The whole question of interpreting these intricate and somewhat varying results is, I find, extremely difficult; yet I am confident that they contain important clues to the proper understanding of the psychology of trance mediumship, particularly as regards the status and nature of controls.

I have said above that the use of inter-list data " forces the comparison to a deeper level " and it is possible that some amplification of this remark may help us to envisage more clearly the kind of problem with which we are concerned.

I am speaking, of course, of what I may call " associated " lists, such as these, in which each word of the one is linked with the corresponding word of the other by some not too remote association. Thus the first word " Head " of the 1933 list is evidently connected with the first word " Hair " of the 1935 list; the second words " Green " and " Grey " respectively are both colours (colloquially speaking at any rate), and have the additional link of both beginning with Gr . . . . The last mentioned element might tend to produce some likeness of response on the most superficial level without the meanings of the words being at all apprehended by the subject; but in general this is not the case, so that likeness of reaction will only be brought about the evocation of associated memories, concepts or what-not common to the two words. It is in this sense that I use the term " deeper level " in this context; but there seems no *a priori* reason for supposing that it is " deeper " in the same kind of way as the level at which RPN operates is " deeper " than that reached by RT. In fact, it fairly evidently is not; otherwise, I think, we should find Feda inter-list RT similarities (3051 & 2) of the same high order as those for RPN (3061 & 2); and, again, if the two " deepnesses " were of the same kind, we should expect F to be significantly similar to S′ on inter-list RPN (3068) if she is so on RT (3058). And why in the name of all that's perplexing should she be significantly similar to S′ on inter-list RT, but not to herself?

I can only propound the conundrum and hope that some more penetrating psychologist than myself will produce the answer.

PART II

ELIMINATION OF EFFECTS DUE TO MEDIUMS FROM J, J′ AND E, E′ DATA

A. EXPLANATION OF METHOD

15

I have already pointed out that no "straight" comparison of the data obtained from the communicators purporting to manifest through two different mediums can possibly lead to valid conclusions regarding their autonomy, because any likeness found can always be attributed to likeness of the relevant trance states as such. I must now add that it is equally hopeless to attempt to overcome this difficulty (as I did in Part VI of *Q.S.T.P.*, II) by introducing data obtained from the *normal* mediums into the calculations, for the "communicator" state of trance may be very different from the normal, just as the "control" state has been shown to be above.

We must accordingly work only with the data obtained from the ostensible communicators and devise means for eliminating any effects of likeness, howsoever arising, due to the trance states in or through which they are functioning.

The right way of doing this (for which, as already stated, I am deeply indebted to Mr W. L. Stevens of the Galton Laboratory) is not particularly easy to understand, but I will do my best to make it clear with the aid of simple illustrations, and I think the best plan will be to build up a set of imaginary readings for, say, a group of five words and then analyse these. The process, if not completely illuminating, should serve to show the kind of way in which the method operates.

16

By way of preliminary, we must note the following points. If we are presented with a set of, say, reaction times from the Leonard-Sharplin experiment of 1935, we find that it consists of 400 entries, namely 100 from the Leonard version of John (J), 100 from the Leonard version of Etta (E), 100 from the Sharplin version of John (J′) and 100 from the Sharplin version of Etta (E′). To each of these entries three factors (apart from fortuitous error) have contributed and to the operation of these three factors, plus error, all differences between them are due.

The first factor is the Medium, hereinafter denoted by M. By this is meant, of course, the medium in the relevant trance state—

not the medium in the "normal" state or in that produced by hashish or a new hat. Thus the communicator state *as such*, and regardless of which communicator is functioning, might be responsible for a generally longer reaction time on the part of Leonard than on the part of Sharplin, or *vice versa*, and this fact would contribute to the variability of the data taken as a whole.

The second factor is the Communicator, hereinafter denoted by C. This may refer either (on the spiritistic hypothesis) to an actual extraneous entity with attributes of its own exerting an influence on the average length of reaction time, just as M does, or (on the purely psychological) to the "rendering" by the medium of the character of John or Etta as the case may be. It makes no difference to the analysis; on either hypothesis, C may make just the same kind of contribution to the total variability of the data as M.

The third factor, of course, is the Word, hereinafter denoted by W. The different words of the list will, in general, produce differing effects which may be common to the two mediums as such, to the two communicators as such, or to all four personalities.

The last remark, which may perhaps sound a trifle obscure, leads to the consideration that we must not in our analysis deal with these three main factors alone, but also with what is usually termed their "interaction" with each other. Thus the contribution of C to the total variability of the data may differ as between the two mediums—or, if you prefer it (since there is nothing mathematically speaking to distinguish between the data from mediums and communicators) the contribution of M may vary as between the two communicators. We will denote this interaction by MC, and will similarly use the letters MW and CW to refer respectively to the contributions made to the total variability of the data by the variation of the mediums as such, taken together, with respect to the words, and to the corresponding variation of the communicators as such, taken together.

Thus the total variability of the data is made up six definite (and calculable) contributions, *viz.* M, C, W, MC, MW, CW, together with an indefinite amount of error. Our business is to find how much of the total observed variability can be ascribed to each of these six components and to see whether those that interest us (in this case only CW) are significantly greater than the residuum (error). This will, I hope, become clearer when we have constructed a set of imaginary data *ab initio*.

17

We will try the effect of building up a set of imaginary reaction times for the four personalities J, E, J′ and E′ for five words, *a*, *b*, *c*, *d* and *e*, using arbitrary units.

We will start by supposing that one of the mediums (L, say) has a " basic rate " of reaction time of, say, one unit, while the other has a basic rate of 2. Then our first schedule of contributions to the final times will be :

WORD	J	E	J′	E′	TOTAL
a	1	1	2	2	6
b	1	1	2	2	6
c	1	1	2	2	6
d	1	1	2	2	6
e	1	1	2	2	6
TOTAL	5	5	10	10	30

Now the " total variability ", otherwise known as the Variance, is simply the sum of the squares of the differences of the quantities concerned from their mean. Here we have 20 entries with sum 30 and mean 1·5 ; since 10 of them are 1 and the other 10 are 2, it is easy to see that the total variability, or variance, will be $20 \times \cdot 5^2$, or 5·0.

The rule for finding the contribution made to the total variance from any source may be stated as follows : Take the sum of the squares of the totals of the entries contributed by the two or more factors concerned ; divide this sum of squares by the number of entries which go to make up each total thus squared ; subtract the product of the Grand Total and the General Mean ; the remainder is the contribution required. I cannot give here the theoretical proof of this rule ; but those who are acquainted with statistical methods will probably recognise an old friend, even if somewhat disguised.

Now at this stage we have only one source of variation involved, namely M—*alias* the differing " basic rate ", as we have called it, of the two mediums. Leonard, we have supposed, has contributed the 10 entries under J & E, which have a total of 10 ; Sharplin the 10 entries under J′ and E′ with a total of 20. The sum of the squares of these totals is $100+400=500$; each of the totals which has been squared is made up of 10 entries, and dividing by this number gives 50 ; the grand total is 30 and the general mean is 1·5 with a product of 45 ; subtracting this from 50, leaves 5·0. This, as we have just seen, is the total variance ; so that at this stage the total variability is found to be composed exclusively—as it should be, because we have arranged it so—of the variations due to the difference in the " basic rates " of the mediums—that is to say, to M.

18

Now let us suppose that " John "—whether because he is an autonomous extraneous entity so constituted, or because the

" renderings " of his character given by the two mediums agree in this respect—has similarly a " basic rate " of 2 and " Etta ", subject to the same remarks, of 3. Adding these to what we have for the mediums, our schedule becomes :

Word	J	E	J′	E′	Total
a	3	4	4	5	16
b	3	4	4	5	16
c	3	4	4	5	16
d	3	4	4	5	16
e	3	4	4	5	16
Total	15	20	20	25	80

We may as well find the variance now by the regular method instead of by inspection ; we have, where x refers to any entry,

$S(x)=80$; $S(x^2)=330$; $\bar{x}=4{\cdot}0$; $\bar{x}\,S(x)=320$; $S(x-\bar{x})^2=10{\cdot}0$; the last-named quantity (10·0) being the variance, obtained by subtracting $\bar{x}\,S(x)$ from $S(x^2)$.

(*N.B.* $S(x)$ is the Grand Total and $\bar{x}$ the General Mean.)

Applying the same procedure as before for determining M, we have

Sum of all Leonard entries 35·0 ; sum squared -	1,225·0
,, ,, Sharplin ,, 45·0 ; ,, ,, -	2,025·1
Total - - - - -	3,250·0
Dividing by 10 as before gives - - - -	325·0
Subtract xS(x) - - - - - - -	320·0
Remains - - - - -	5·0

It will be noticed that the contribution of M is unaffected by what we have done.

Applying exactly the same procedure to determining C, we have :

Sum of all John entries 35·0 ; sum squared - -	1,225·0
,, ,, Etta ,, 45·0 ; ,, ,, - -	2,025·0
Total - - - - - - -	3,250·0
Dividing by 10 gives - - - - - -	325·0
Subtract - - - - - - - -	320·0
Remains - - - - -	5·0

which is the contribution made by the difference between communicators.

Thus the total variance at this stage, namely 10·0, is completely accounted for (as it should be) by the two contributions M & C. As it happens, the simple figures chosen make the two contributions equal; but there is no reason, in general, why they should be so, and they are equally well dealt with by the same process whether they are or not.

19

We will not trouble to introduce an element of "interaction" between mediums and communicators, and I fear I am not clever enough to build up a complete set of life-like data without at one stage or another upsetting the arrangements I have, so to speak, already made. It will be sufficient, I hope, to construct a reasonably plausible set and then to show how it is in practice analysed.

Let us suppose that the words *a*, *b*, *c*, *d*, *e* are such that they respectively produce increments of time 7, 3, 1, 5 and 4 units, *for all personalities*; then to the existing figures in each column of our schedule we shall add 7, 3, 5, 1, 4 in that order from top to bottom. On the assumption that John and Etta really are extraneous entities of some kind, let the words further be such that they produce respectively increments of 3, 1, 0, 2, 1 in the case of both versions of John, and of 1, 4, 1, 5, 2 in the case of both versions of Etta. Finally let the mediums have their own idiosyncrasies such that the words give, similarly, increments of 6, 1, 3, 1, 1 in both tests of Leonard, and of 1, 4, 4, 2, 6 in both tests of Sharplin.

We have now catered for all possibilities—namely, for variations in the average times of mediums and communicators over all words (by the "basic rates" dealt with in the previous sections); for differences between the words common to all personalities—*i.e.*, effects common to both mediums—for those affecting each communicator individually; and for those affecting each medium individually. We will refer to these last three quantities involving the words as W, CW, and MW respectively; but it is only CW that really interests us. If there is "nothing there" the two versions of each communicator can have nothing in common except what chance will give, and the value of CW will not significantly exceed error; but if there really are communicators behind the scenes, then their personal variation with respect to words will tend to leave a significant value for CW, after we have allowed for any seeming similarity due to variations of the mediums individually or variations common to all personalities.

Reverting, we need not trouble to put down in detail just how the data for each personality are built up, but will content ourselves with the single example of J (the Leonard version of John).

These data will now be made up as follows :

Word	B	W	J	L	Total
a	3	7	3	6	19
b	3	3	1	1	8
c	3	1	0	3	7
d	3	5	2	1	11
e	3	4	1	1	9

The attentive reader will doubtless recognise the figures under B as the " basic rates " already discussed ; those under W as the contributions from the words common to all personalities ; and those under J and L as the increments postulated as characteristic of John and Leonard respectively.

Repeating this process with the other three personalities, we obtain as our final set of data for analysis :

Word	J	E	J′	E′	Total
a	19	18	15	14	66
b	8	12	12	16	48
c	7	9	9	11	36
d	11	15	13	17	56
e	9	11	15	17	52
Total	54	65	64	75	258

It should be borne in mind that, in composing this set of data, we have assumed that there is no error (*i.e.*, we have not introduced any casual irregularities but only quantities having a definite *raison d'être.* Thus, when we have removed all the variability (variance) due to M, C, W, MC, MW and CW, we ought to find nothing left.

20

We start by determining the total variance to be analysed. The Grand Total is 258, the General Mean 258/20=12·9, and their product 3328·2.

The sum of squares of all 20 entries is 3566 ; subtracting the above product leaves 237·8 ; this is the variance.

The contribution made by the mediums, communicators and their interaction (M, C and MC) is determined as follows. First we square and sum the four personality (column) totals, obtaining the figure 16,862 ; but since each item squared contains five entries, we must divide by five (obtaining 3,372·4) before subtracting the product (3,328·2) which leaves 44·2. This quantity, however, contains all the variance that can be due to mediums, communicators and their interaction ; so that to obtain MC we must calculate M and C separately and subtract from 44·2. Repeating the ritual already exemplified in previous sections we obtain the values of 24·2 for M amd 20·0 for C. We then have:

M - - - - - -	24·2
C - - - - - -	20·0
MC - - - - - -	0
TOTAL - - -	44·2

We find to our surprise that there is no interaction between M and C, but soon realise that this is because we omitted to introduce any.

To find the contribution made by the differences between Words as such, *i.e.*, the differences common to all personalities, we square and sum the Word (row) totals, obtaining 13,796. We divide this by four, because each total squared contains four items before subtracting the product from the quotient (3,449). The remainder amounts to 120·8 and is W.

For the contribution made by the differences between the words as shown by the mediums we take, say, Leonard first, add the two entries for J and E in the case of each word (obtaining 37, 20, 16, etc.), square and sum these totals, obtaining 3,101, and repeat the process for Sharplin, which yields 3,949. We add these to get the total effect, divide by two, because each item squared was made up of two entries, and subtract the product. This leaves 196·8, which is the total contribution to the variance made by the mediums, the words and their interaction. To obtain MW we must subtract M and W from this and we accordingly have:

M - - - - - -	24·2
W - - - - - -	120·8
MW - - - - - -	51·8
TOTAL - - -	196·8

In the case of CW, we proceed precisely similarly, except that instead of adding and squaring the pairs of entries belonging to the same medium, we add and square the pairs belonging to the same communicator. The sums are 2,964 and 4,016 for John and Etta respectively; adding these, dividing by two and subtracting the product, we have the figure 161·8 which, analogously to the case of the mediums and words, contains the contributions made by C and W as well as CW. So in this case we have:

C - - - - - -	20·0
W - - - - - -	120·8
CW - - - - - -	21·0
TOTAL - - -	161·8

We may now tabulate the contributions as follows :

M - - - - - -	24·2
C - - - - - -	20·0
W - - - - - -	120·8
MC - - - - - -	0·0
MW - - - - - -	51·8
CW - - - - - -	21·0
MCW - - - - -	0·0
TOTAL - - -	237 8

It will be seen that the total variance is precisely made up, as it should be, by the contributions thus determined, so that nothing is left for MCW, which would be error for our purpose.

In practice, of course, this is far from being the case, and the question arises as to whether the quantity CW is significantly greater than the residual MCW. If it is not, then the effect is attributable to chance ; if it is, then the operation of *some extraneous factor not ascribable to the likeness between the relevant trance states of the mediums, or to chance,* may be regarded as established. We have extracted and set in its proper place, as W, the effect due to whatever the words have in common for all personalities ; and we have done the same (MW) for whatever is common to the two (presumably closely similar but not necessarily identical) states of each of the two mediums ; if there are no communicators, then chance alone can give us whatever we find for CW, and it is only a matter of routine statistical technique to ascertain the probability of this having occurred.

21

The way this is done is by means of the ordinary " z test ", which I have already explained to some extent in *Q.S.T.P.*, I, Appendix II, p. 214. We obtain the mean squares of CW and MCW by dividing each by the appropriate number of degrees of freedom and take z as half the difference of the natural logarithms of these quotients. Dividing this z by its standard error and consulting a table of normal deviates gives us the probability required.

This needs a good deal more amplification than I can give here. It must be sufficient to say that the number of degrees of freedom associated with any of the elementary factors M, C and W is one less than the number of entities, so to speak, classified under the heading in question. Thus, in the example just worked, we have two mediums and two communicators, so that M and C have only one degree of freedom each ; but we have five words, so that W has four degrees. For " compounds " such as MW and CW the number of degrees is the continued product of the numbers for the constituents ; thus MC would have $1 \times 1 = 1$ degree, CW has $1 \times 4 = 4$, and MCW has $1 \times 1 \times 4 = 4$ degrees. So long, in fact, as we are dealing only with two mediums and two communicators the number of degrees for CW and for MCW will always be equal. This saves a certain amount of trouble in practice, for we do not need to work out mean squares but merely divide CW by MCW (or vice versa, if MCW happens to be the greater) and find the logarithm of this quotient. In practice also it reduces the likelihood of error not to halve the logarithm but to use double the standard error instead when determining the probability. When the degrees of freedom are equal, as in these cases, the standard error is the square root of the reciprocal of the number of degrees.

22

I greatly regret that I have not been able to produce a more elegant example than that given above. I should have liked to have been able to compose a set of data such that the analysis would show us taking out (as in practice it does enable us to do) precisely the same amounts of variance, under the different headings, as we had previously put in. But it is one thing to understand a method well enough to use it, and to follow fairly clearly the manner of its operation ; but quite another, as I have found to my cost, to be able to work it backwards well enough to construct an example of such a kind. As it is, I can only hope that the example I have given, imperfect as it is, may enable the reader to form some appreciation of the way in which the method breaks up the total variability of the data into all possible constituent parts and assesses each with precision, leaving nothing to chance except that which chance has introduced.

It is interesting to note that, if we had only a single communicator, even though manifesting through two mediums, we could never get to grips, on these lines, with the question of autonomy at all, because any similarity at all between the (now only two) sets of data would be taken out under W. It is only the possibility of applying a kind of differential method as between John and Etta on the one hand and Leonard and Sharplin on the other that gives us the possibility of disentangling whatever effects may really be due to outside influence from those due to the mediums and their trance conditions.

B. Application of the Method

23

So soon as the method just discussed was brought to my notice I hastened to apply it to the observations of the Leonard-Sharplin experiment. In the first instance I used the " bulked " data, that is to say, the sums of the (scaled) reaction times, or failures in reproduction, for all five occasions on which the personalities were tested, and I analysed the whole list of 100 items in a single calculation.

The results, which need not be discussed in detail here, were singularly unpromising. Much the same applied when I broke the data up into the " blocks " of 25 words[1] in which they are actually divided, for convenience of recording, during the tests. I also wasted a great deal of time trying all manner of combinations and permutations of analysis, such as testing the reactions times obtained from one medium against the reproductions obtained from the other ; or either or both against a third quantity—Variety of Response (V) —which I have lately been studying in a preliminary sort of way. (In passing, it looks as if this is going to be a valuable indicator, but a great deal of work will have to be done on it before it is ready to make its debut.) I even embarked, with more heroism than prudence, on the full analysis of the five-dimensional manifold MCWOT (where O stands for Occasion and T for Test—*i.e.*, RT, RPN and V) having 5,999 degrees of freedom.

But it was only when, abandoning these rash projects, I laboriously took the whole outfit to little bits, so to speak, and applied the MCW analysis to each group of 4×25 data, block by block and occasion by occasion, that light began to dawn.

24

The results obtained by this procedure were very much more promising, at least as regards RPN ; the RT results were uninspiring, as was to be expected from the preliminary survey.

Encouraged by this and by the rather surprisingly high similarities of the inter-list comparisons, I decided to add to my stock of results by comparing the Leonard data of 1933 with the Sharplin data of 1935. For reasons given in *Q.S.T.P.*, 1, p. 191, the material

[1] In what follows, Block A refers to words 1–25, Block B to words 26–50, and so on. The contents of the blocks are always the same, but the order of the words is varied within the block from occasion to occasion. The occasions (sittings) are numbered from I to V.

of 1933 is limited to 75 words instead of 100, so that only blocks A, B and C could be analysed here.

It further occurred to me that the method described knows nothing and cares less, so to speak, of the label attached to the data, so that the observations obtained from Sharplin might just as well be obtained from Leonard so far as the mathematics is concerned. If the method can eliminate effects due to similarities between Leonard and Sharplin, it can equally well eliminate effects due to the similarity between Leonard on one set of occasions (1933) and Leonard on another set of occasions (1935). I accordingly have analysed these data also for both RT and RPN.

Each such analysis follows precisely the ritual given in the example of sections 16 to 21 above, except that there are 25 words instead of only five.

Using a convenient notation in which the figures stand for the years and a dash (′) indicates Sharplin, I thus obtained 20 RT and 20 RPN values of z from the 35-35′ material, 15 of each from the 33-35′ and 15 of each again from the 33-35, making a total of 100 in all.

25

These 100 values of z (actually of $2z$, *vide supra*) are displayed in Table V, and the all-important point to remember is that, if there were no extraneous influence at work, so that the values of CW from which they are derived were wholly chance determined, these 100 values would be randomly distributed about zero with variance 4/24 or ·16667.[1]

Little more than a glance at the Table is necessary to make us suspect that this condition is far from being fulfilled. It is true that, if we pool all the results, the general mean is not far from zero nor the variance from what it should be, but if we look at the arrangement of the quantities and the signs of structure, so to speak, which they display, we are tempted to declare roundly that the random distribution is definitely not realised. We must, however, be extremely cautious here, for it is remarkable how, in a mass of data of this kind, chance alone will throw up effects which look very striking but can be shown by exact tests to be not at all unlikely to occur.

[1] The original true z's have variance 1/24 and S.E. equal the square root of this ; the fact that we simply do not halve these quantities cannot alter their significance ; therefore the S.E. of the double z's must be double that of the single z's, and their variance four times as great, since the variance is the square of the S.E.

Actually we have

General mean$=\cdot 03451$. S.E.$=\cdot 040825$. P$=\cdot 40$ ....3096·1

which is negligible, and for the variance (using the " z-test ")

$$\frac{\text{Sum of squares of all entries}}{\text{Expected value}}=\frac{16\cdot 33202}{16\cdot 66667}=\cdot 9799,$$

whence z is $-\cdot 0203$. S.E.$=\cdot 1000$. P$=\cdot 84$ ....3096·2

which is even worse.

It should be noted here that, although the variance taken over the whole 100 entries differs negligibly from its expected value, it varies considerably from one group of data to another, though never to a significant extent. This also is to be expected. In what follows, I use the theoretical variance throughout; this is technically correct, because we *know* the variance of the " population " to which the data belong, whereas to use the observed variance—which might appear to be keeping closer to the facts—would be merely to *estimate* what we want from data themselves under test.

Reverting: On the other hand, at least one of the groups (35-35′ RPN) has a mean which differs significantly from zero, while some of the others are not altogether negligible and we have to consider the likelihood of these arising by chance in a random sample of six such groups. We shall return to this in a moment.

26

But the two most striking features by far are, first, the preponderance of negative results in the 33-35 (Leonard-Leonard) material, and, second, the tendency for whatever is going on to become stronger in the course of the sitting. That is to say, if the group as a whole shows a positive result, then there is a tendency for the positivity to increase from block A to block C or D. Compare for example RPN 35-35′; the Total for block A is only ·0749, but there is a fairly steady increase through B and C till for D it reaches the high value of 2·5901.

This " practice " effect is very striking and extremely difficult to reconcile with the idea of random distribution; it must, however, be tested for significance before being accepted as authentic.

27

We will first consider the tendency on the part of the Leonard-Leonard analyses to give negative results.

Taking RT and RPN together we find that she gives 20 negative items out of 30. If chance alone were at work, we should expect 15 ; the difference is 5, whence

$$\chi^2 = 3{\cdot}333. \quad P = {\cdot}065 \quad \ldots\ldots\ldots\ldots\ldots\ldots 3097{\cdot}1$$

Alternatively, and preferably, we have

	Positive	Negative	Total
Leonard-Sharplin Comparisons -	40	30	70
Leonard-Leonard ,, -	10	20	30
TOTAL - - -	50	50	100

whence $\chi^2 = 4{\cdot}762. \quad P = {\cdot}03$ .... 3097·2

which leaves little or no doubt that something non-chance is going on, which is all that we are interested in at the moment.

Again, the mean of all Leonard-Leonard data differs significantly from the mean of all Leonard-Sharplin data, for we have

Mean of all Leonard-Sharplin data	·09729	with variance	·00238
,, ,, Leonard-Leonard ,,	−·11196	,, ,,	·00556
Difference of means	·20925	,, ,,	·00794
and S.E.	·08911,		

whence $P < {\cdot}02$ 3098

Strictly speaking, I think that these two results (3097 & 3098) are independent and thus in some sense cumulative, for there seems no reason why the mean should not differ to the extent they do and yet the L-L material be of the same predominant sign as the L-S ; but I am not quite sure of this point, so will not claim it in the final assessment.

28

To ascertain whether the six means themselves are likely to have been produced by chance in a random sample of six groups we must first calculate the probability of each individually being a chance effect, and then combine the probabilities by the " negative logarithm " method of Fisher (*Statistical Methods for Research Workers*, 21·1, pp. 97-8, IVth Edn.).

We then have

Group	Mean	S.E.	P	$\text{Log}_e 1/P$	
RT 35 – 35′	– ·00298	09129	·97	·0306	...............3099·1
33 – 35′	·13515	·10541	·20	1·6094	.... ,, ·2
33 – 35	– ·09847	·10541	·35	1·0498	.... ,, ·3
RPN 35 – 35′	·21781	·09129	·017	4·0745	.... ,, ·4
33 – 35′	·03241	·10541	·76	·2744	.... ,, ·5
33 – 35	·12544	·10541	·23	1·4697	.... ,, ·6
	Total	-	-	- 8·5084	

Multiplying by two gives - - 17·0168

which is a χ^2 with 12 degrees of freedom, whence

$P = \cdot 15$ 3100

where P is the probability that six such means arising by chance in a random sample. The figure is suggestive, but far from significant.

29

To investigate the tendency for " whatever is going on to get more so as the sitting proceeds ", we must determine the slope of the best fitting straight line that can be drawn through the block totals (or means) of the group concerned, find its standard error and the probability of its having arisen by chance, and combine the results as above.

The value of the slope (" b ") is found by the usual methods of the text-books ; its sampling variance is shown by Fisher (*loc. cit.*, p. 125), to be $\sigma^2/S(x-\bar{x})^2$, where σ^2 is the variance of the quantities dealt with, and x has the meaning usual in such contexts (here the number of the block). Applying this procedure, we have

Group	b	S.E.	P	$\text{Log}_e 1/P$	
RT 35 – 35′	– ·05836	·08165	·47	·7551	...............3101·1
33 – 35′	·26271	·12910	·031	3·4738	.... ,, ·2
33 – 35	– ·00923	·12910	·94	·0621	.... ,, ·3
RPN 35 – 35′	·14103	·08165	·084	2·4766	.... ,, ·4
33 – 35′	– ·04065	·12910	·75	·2876	.... ,, ·5
33 – 35	– ·28261	·12910	·029	3·5404	.... ,, ·6
	Total	-	-	- 10·5956	

Multiplying by two gives - - 21·1912

which again is a χ^2 with 12 degrees of freedom, whence

$P = \cdot 05$ 3102

where P is the probability of six such slopes arising by chance in a random sample. The figures would usually be taken as indicating a real effect.

30

If we wish, as we should, to err, if at all, on the side of over-stringency, we may regard the six tests of the means and the six tests of the slopes as twelve independent tests of significance of qualitatively similar material and enquire as to the likelihood of twelve such results arising by chance in a random sample. We add the doubled totals and obtain a χ^2 with 24 degrees of freedom, thus :

$\chi^2 = 38{\cdot}2080$. P $= {\cdot}03$ 3103

which may be taken as significant.

We should obtain this result just the same even if the slopes were randomly distributed with respect to the means ; but actually there is a strong association between them. The slope has the same sign as the mean in five cases out of six, while, if we work the correlation between mean and slope, we find

$r = {\cdot}783$. P $= {\cdot}065$ 3104

I must leave the question of how best to combine these various probabilities to a later section, and must make a short digression to enquire into the possibility of the effects observed being artifactual ; that is to say, due to causes not eliminated by the methods employed.

31

The only possibility seems to be this : that for reasons too intricate to discuss here it is impracticable to apply corrections for practice and fatigue in the same way as is done when we are estimating the ordinary similarities and differences between personalities. It might, accordingly, be suggested that similar rates of slowing of reaction time, for example, might produce illusory likenesses between the L and S versions of J and E and thus raise the value of CW relative to MCW in the analyses (or reduce it if the rates of change were dissimilar). It is a little difficult to see just how this would work out, for (unless I am mistaken) the first order effects common to the two mediums will be eliminated by the method, leaving only second order effects to operate ; but it is certainly not mathematically excluded from possibility. The only way to make perfectly sure of this point is to correct every individual reading to the value it would have if no practice or fatigue were operative, and then recalculate the whole outfit. There are 5,500 relevant observations at present and life has hitherto proved too short for so formidable an enterprise ; but I propose to undertake it almost immediately, when I receive the additional 2,000 observations which Mr Drayton Thomas is collecting from Mrs Garrett at the time of writing.

Meanwhile, there are a number of considerations which completely assure me, humanly speaking, that the results are not due to anything of the kind; on the contrary, the random incidence of practice and fatigue effects is more likely to have obscured than to have generated the sort of thing we have been looking for.

1. The relevant corrections are always negligible in the case of RPN; but the RPN results obtained above are more, rather than less, impressive than those for RT.

2. In comparing the 1933 material with the 1935 I have used the same five 1933 sittings (namely I & III to VI) as for the comparisons of *Q.S.T.P.*, II (*q.v.*). This means that the orders in which the corresponding words were given were different (except in the case of the first sitting) in the two years; so that the effects in question are practically eliminated here; yet the 33-35′ and 33-35 results are not less impressive, on the whole, than the 35-35′.

3. Although it is not practicable to apply corrections, it is not difficult to find the amount of "inflation" to which CW would be subjected as a result of the unremoved practice or fatigue effects. If these effects are responsible for the results obtained, there should be a significant correlation between the amount of the inflation and the value of the corresponding z. I have tested this on RT 35-35′, which, as unaffected by the considerations of 1, and 2, above, is the most suspect group. The result is altogether negligible, for the coefficient of correlation is only ·126.

I have also performed analogous calculations to see whether there is any tendency for the "inflation" to increase (or diminish) as between blocks A and D of RPN 35-35′, and a number of other tests in addition. The results are quite negligible in all cases.

We may accordingly conclude with assurance that the results observed are *not* due to uneliminated practice or fatigue effects.

32

The problem of how probabilities obtained from different tests or experiments should be combined so as to give an over-all result is distinctly tricky, and I have yet to see any satisfactory account of it.

Of course, if we are given two samples of qualitatively similar raw material (*e.g.* measurements of the heights of Scots and Dutchmen), we should simply pool the data and treat them statistically as a single sample. Or if we know the "end-product probabilities", so to say, of a number of such (qualitatively similar) investigations, we use the method employed in 28 and 29 above to determine the chance of such a batch of probabilities arising as a result of random sampling.

But it is not of such cases of these that I am thinking; the question is rather one of combining the outcomes of tests or experiments which are *qualitatively different*, or independent. And by "independent" I mean such that it is impossible to infer the outcome of the one from a knowledge of the outcome of the others *without invoking the truth* (*or falsity*) *of the hypothesis under test.*

Thus, if we wished to test the hypothesis that inter-planetary space is filled with granulated cheese, we might approach the problem either by a spectroscopic method or by analysing the accretions of meteorites; and these two methods would be independent in the above sense. If our technique was so imperfect that statistical methods had to be employed to assess the results, we should obtain (perhaps) two probabilities of ·05 each in favour of this intriguing hypothesis. It would not be correct, I think, to combine these by the negative logarithm method and arrive at the over-all result of ·2 as the final probability; in fact, such a conclusion would be absurd on the face of it.

I submit that in the case of independent experiments, or their equivalent, *provided we are sure of the independence*, the proper procedure is that of the "witnesses" formula for concurrent testimony as given in the text-books (*e.g.* Hall & Knight, *Higher Algebra*, p. 396 IVth Edn.). The argument may be presented as follows:

In all cases of the kind we are considering, we are enquiring whether there is a "real" non-chance cause or influence at work, or whether the effects observed can be ascribed to chance alone, and the probablility of the latter supposition being true is the value of P, which we obtain at the conclusion of any test on the subject.

Then, since there is certainly either a non-chance cause or not, the probability of there being one, plus the probability of there being none, must together equal unity; but the value of the second probability is P, so that the value of the probability of there actually being a "real" cause is $1-P$.

Now suppose that we perform two independent experiments and obtain two values p_1 and p_2 as the probabilities that their results are due to chance alone. Then $(1-p_1)$ and $(1-p_2)$ are the respective probabilities of the results being due to something other than chance.

Equivalently, and more conveniently, we may regard the outcomes of the two experiments as two statements to the effect that chance alone is operative, with probabilities respectively of p_1 and p_2 of being true and of $(1-p_1)$ and $(1-p_2)$ of being false.

Then, if chance alone *is* operative, both statements are true, and the chance of this occurring fortuitously is the product p_1p_2; but if a "real" influence is at work then both are false, and the chance of this occurring by accident is the product $(1-p_1)(1-p_2)$. Thus, the probability (so far as the evidence of these experiments goes) of the results observed being due to chance alone is to the probability of their being due to a real influence as p_1p_2 is to $(1-p_1)(1-p_2)$.

Whence the over-all probability that chance alone is responsible is

$$P=\frac{p_1p_2}{p_1p_2+(1-p_1)(1-p_2)}.$$

The same reasoning shows that if we have a number of probabilities $p_1, p_2, p_3, \dots p_n$, then the combined chance will be

$$P=\frac{p_1p_2p_3\cdots p_n}{p_1p_2p_3\cdots p_n+(1-p_1)(1-p_2)(1-p_3)\dots(1-p_n)}$$

The above argument appears sound, and it is easy to assure ourselves that what I may, by analogy, term the "boundary conditions" of the problem are satisfied. For example, if every value of p were ·5, the resultant P would also be ·5, no matter how many p's were involved; and this is as it should be, for a value of ·5 means that chance is exactly as likely to be responsible as not, so that the experiment throws no light on the subject. Similarly, the value of any probability will be left unaltered by combining with it another of value ·5, which is also as it should be. Again, if two experiments were to yield probabilities of, say, ·1 and ·9, the combined probability would be ·5; which is right, for each experiment precisely contradicts the other, thus leaving us in our original state of *a priori* nescience.

Finally, just as a succession of constituent p's less than ·5 will progressively diminish the combination P, so a succession of constituent p's greater than ·5 will progressively increase it till in the limit it becomes unity (which is precisely equivalent to the chance of the alternative hypothesis becoming zero); but a succession of the latter kind is just as unlikely to occur by chance as one of the former, and a succession of random values of the constituent p's, such as the operation of chance would in fact give, would cause P to approximate increasingly closely to ·5.

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In the present context we are concerned solely with the question of whether the values of $2z$ given in Table V are or are not randomly distributed around a mean of zero with variance ·16667.

From the point of view of the last section it would appear that the RT and RPN results are " independent " in the relevant sense; for to use our previous knowledge that a failure in reproduction is usually associated with a prolongation of reaction time, or other like information, would be tantamount to invoking the (falsity of the) hypothesis we are testing, namely that there is no cause, other than chance, at work behind the scenes, *i.e. no* extraneous personality possessed of the characteristics concerned. But I shall not trouble to separate the two tests here, for the results are sufficiently coercive without doing so.

Taking the two tests together we have the following items of evidence, P being the probability in each case that the effects are due to chance alone on the hypothesis under test.

	RN	P
1. Probability of the six means being due to random sampling - - - - -	3100	·15
2. Probability of the six " slopes " being due to random sampling - - - - -	3102	·05
3. Probability of the observed association between means and slopes being due to chance	3104	·065
4. Probability of the difference between the means of L – S and L – L results being due to chance - - - - - - -	3098	·02

Applying the formula of the preceding section, we have

$$P=\frac{\cdot15\times\cdot05\times\cdot065\times\cdot02}{\cdot15\times\cdot05\times\cdot065\times\cdot02+\cdot85\times\cdot95\times\cdot935\times\cdot98}$$

$$=\frac{\cdot000009750}{\cdot739922500}$$

$$=1\cdot3\times10^{-5} \qquad \ldots\ldots\ldots\ldots 3105$$

which is sufficiently significant to meet all requirements.

Even if we were to adopt the ultra-conservative (and, as I hold, erroneous) course of treating these four items as if they were independent " tests of significance ", instead of independent pieces of evidence bearing on the point at issue, we should still have

No.	RN	2 $\text{Log}_e 1/P$
1	3100	3·7944
2	3102	5·9914
3	3104	5·4662
4	3098	7·8240
TOTAL	- - -	23·0760 a χ^2 with 8 D.F.

P less than ·01 ....3106·1 and the same would be true if we followed on from RN 3103, adding merely the last two items, for we should then have

$\chi^2 = 51{\cdot}4982$ with 28 D. F., and P again less than ·01 ...3106·2

It would appear, therefore, that the statistical significance of the results is not in doubt and that the effects observed cannot reasonably be ascribed to chance.

Summary and Conclusions

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In the first part of this paper, while considering various routine comparisons, we have occasion to note the remarkable stability of the controls " Feda " and " Silver ", particularly as regards RT, as compared with the normal personalities of their mediums ; in the case of Feda, this applies to long periods of months as well as to the shorter period covering a set of sittings. The implication is that the controls are derived from relatively " deep " and permanent strata of the total personality, of a nature not easily disturbed by the " changes and chances of this mortal life ".

Somewhat similarly, we find that the reproduction test appears to penetrate to deeper and more permanent levels than does the reaction time ; but there are certain anomalies, and it would be unwise to suppose that the two sorts of " depth " thus indicated are of the same kind.

Very remarkable is the striking resemblance of Feda to Silver, especially on RT ; in the case of inter-list tests Feda carries this to the extent of being more like Silver than she is like herself—an anomaly which calls for explanation.

In the second part we have seen how it is possible to devise and apply a method which will eliminate from the reactions of two communicators tested with different mediums all effects due to similarities between the communicator states of those mediums themselves. When this is done on so detailed a scale that we can deal with the statistics of the resultant statistics, we find that the results are not attributable to chance, while there is no reason to suppose, and ample reason to doubt, that they are artifactual.

When Leonard material is tested against Leonard, instead of against Sharplin, negative results predominate ; the reason for this is at present entirely obscure to me, but it should be noted that the effect is apparently not of the same kind as the countersimilarity previously noted in the case of controls. When we fully understand the implications of this curious fact (which, frankly, I do not at the moment) and can produce a plausible theory to account for it, we shall, I fancy, have made a considerable advance in our understanding of trance mediumship.

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As regards the main point at issue, namely whether there is or is not adequate evidence for the operation of some extraneous influence (presumably—though not, perhaps, inevitably—something in the nature of what John and Etta claim to be), I can only adopt this position :

If nothing more important that a few million pounds or the fate of a couple of nations were involved, I should feel disposed to declare flatly that the operation of some such extraneous influence had been established, and to leave it at that. But since the admission of such a conclusion, arrived at for the first time in history by the use of exact quantitative methods, would open up prospects beside which the achievements of relativity theory would be of no more than parochial interest, I prefer to make precaution doubly cautious and not to commit myself (if ever) till I have reworked the entire calculation, with the additional refinements indicated and the additional material now being collected.

If we then obtain the same results, or more so, we may reasonably conclude that there is "*something* there", and apply ourselves to the more delicate task of deciding what it is.

Table I

Individual Consistencies (" I ")

RN	Personality	z	P	%	Percentages from Earlier Experiments
			Reaction Times		
3001	L	·3342	10^{-4}	32·2	L_1 26·7 ; L_3 42·9
2	F	·3831	10^{-6}	36·5	F_1 21·0 ; F_3 41·3
3	J	·1944	·01	19·2	J_1 15·2
4	E	·1485	·06	14·7	E_1 10·2
5	S	·1024	·20	11·0	
6	S′	·5861	10^{-9}	52·7	
7	J′	·1990	·013	19·6	
3008	E′	·2610	·001	25·5	
			Reproductions		
3009	L	·0857	·28	8·5	L_1 26·6 ; L_3 15·0
10	F	·3144	10^{-4}	30·4	F_1 25·5 ; F_3 39·3
1	J	·2570	·001	25·2	J_1 28·0
2	E	·2750	·001	26·8	E_1 39·0
3	S	·2370	·003	23·3	
4	S′	·3874	10^{-6}	36·9	
5	J′	·3801	10^{-5}	32·5	
3016	E′	·1773	·025	17·5	

(*N.B.* Above, the suffix 1 refers to the First Thomas Experiment of 1933, the suffix 3 to the Irving Experiment of 1934.)

N.B. All values of P are approximate ; in most cases the exact values are somewhat less than those given.

TABLE II

INTRA-GROUP SIMILARITIES AND DIFFERENCES

RN	Pair	Similarity z	%	Difference P/2	z	P
		Reaction Times: Leonard Group				
3017	LL	·0241	2·4	·405	·3531	10^{-5}
8	FF	·4063	38·5	10^{-4}	·1105	·16
9	LF	−·1959	19·3	·025	·4227	10^{-7}
20	LJ	·0590	5·9	·28	·1931	·015
1	LE	−·0617	6·2	·27	·3007	10^{-4}
2	JE	·2426	23·8	·01	·0380	·65
		Reaction Times: Sharplin Group				
3023	SS′	·0174	1·7	·43	·4332	10^{-7}
4	SJ′	·3520	34·0	·001	−·0747	·35
5	SE′	·4314	40·6	10^{-5}	−·0531	·50
6	J′E′	·3232	31·2	·001	·0523	·50
		Reproductions: Leonard Group				
3027	LL	·2086	20·5	·02	−·0238	·76
8	FF	·2610	25·5	·005	·1873	·06
9	LF	·3048	29·6	·001	·0099	·90
30	LJ	·1339	13·3	·09	·1392	·08
1	LE	·3195	30·9	·007	·0396	·62
2	JE	·3537	34·0	·001	·0519	·51
		Reproductions: Sharplin Group				
3033	SS′	·1994	19·7	·025	·1587	·04
4	SJ′	·3789	36·0	10^{-4}	·1221	·12
5	SE′	·2536	24·8	·01	·0408	·61
3036	J′E′	·3446	33·2	·001	·0779	·33

N.B. All values of P/2 and P are approximate; in most cases the exact values are somewhat smaller than those given.

For Comparison: Percentage Similarities from Earlier Experiments.

RN	Pair	% RT	% RPN	RN	Pair	% RT	%RPN
2034	GU	8·9		2074	LE	4·2	
41	GU		5·8	89	LE		11·9
72	LF	−3·9		115	LD	5·8	
87	LF		40·3	125	LD		34·2
114	LF	−3·1		80	JE	23·2	
124	LF		12·1	95	JE		45·8
73	LJ	33·2					
88	LJ		11·5				

Table III

Inter-Group Similarities and Differences

RN	Pair	Similarity z	%	P/2	Difference z	P
		Reaction Times				
3037	LS	– ·0500	5·0	·31	·3134	10^{-4}
8	FS′	·3577	34·3	·0002	·2040	·01
9	JJ′	– ·0105	1·0	·46	·2113	·01
40	EE′	– ·0946	9·4	·175	·1866	·02
1	JE′	·0078	·7	·47	—	—
2	EJ′	·0084	·9	·465	—	—
		Reproductions				
3043	LS	·1152	10·1	·125	·1002	·21
4	FS′	·1738	17·2	·04	·2385	·003
5	JJ′	·2998	29·1	·003	·1584	·04
6	EE′	·2698	26·3	·005	·0638	·62
7	JE′	·2236	22·0	·013	—	—
3048	EJ′	·2798	27·3	·003	—	—

N.B. All values of P/2 and P are approximate; in most cases the exact values are somewhat smaller than those given.

Table IV

Inter-List Similarities

Pair	RN	z	P/2	RN	z	P/2
L '33 – L '34	3049	– ·0705	·26	3059	– ·0434	·355
L '33 – L '35	50	·0632	·295	60	·0579	·31
F '33 – F '34	1	·0404	·365	1	·2578	·013
F '33 – F '35	2	·0957	·205	2	·3849	·001
J '33 – J '35	3	·0743	·26	3	·1023	·19
E '33 – E '35	4	·1352	·12	4	·3181	·003
J '33 – J′	5	·1322	·125	5	·1814	·06
E '33 – E′	6	·0616	·30	6	·2964	·005
L '33 – S′	7	– ·0167	·445	7	·0333	·385
F '33 – S′	3058	·3472	·002	8	·0086	·47

N.B. As before, values of P/2 are approximate.

Table V

Summary of Analyses (MCW) for Blocks and Occasions: All Combinations

N.B. The figures given are double z's, each with Variance ·16667 and Standard Error ·40825.

Reaction Times

RN	Block	Occasions I	II	III	IV	V	Total
Data:			35 – 35′				
3069	A	– ·0863	·2336	·6599	·0000	– ·1477	·6595
70	B	– ·1911	·2035	– ·0871	– ·0194	– ·3170	– ·4111
1	C	·3382	– ·0645	– ·3708	·2454	·0649	·2132
2	D	– ·4305	·0602	– ·5597	·0824	·3264	– ·5212
	Total -	– ·3697	·4328	– ·3577	·3084	– ·0734	– ·0596
Data:			33 – 35′				
3073	A	– ·0084	– ·3440	– ·1669	– ·0862	·1102	– ·4953
4	B	·2219	·4104	– ·5165	·4670	– ·1920	·3907
5	C	– ·1123	·4074	·7251	1·3026	– ·1910	2·1318
	Total -	·1012	·4738	·0416	1·6834	– ·2728	2·0272
	Total -	– ·2685	·9066	– ·3161	1·9918	– ·3462	1·9676
Data:			33 – 35				
3076	A	– ·3347	– ·3602	– ·3707	·1741	·1770	– ·7145
7	B	– ·5298	·0117	·4202	– ·1983	·3404	·0442
8	C	– ·2468	·1908	·0696	– ·2539	– ·5665	– ·8068
	Total -	– 1·1113	– ·1577	·1191	– ·2781	– ·0491	– ·4771
Grand	Total -	– 1·3798	·7489	– ·1970	1·7137	– ·3953	·4905

Reproductions

RN	Block	Occasions I	II	III	IV	V	Total
Data:			35 – 35′				
3079	A	·2765	– ·0176	·0938	·2876	– 5654	·0749
80	B	·1776	– ·2687	·1084	·3857	– ·3215	·0815
1	C	·4128	·7041	– ·3677	·3662	·4943	1·6097
2	D	·6386	·3409	·6733	·1163	·8210	2·5901
	Total -	1·5055	·7587	·5078	1·1558	·4284	4·3562
Data:			33 – 35′				
3083	A	– ·4143	·4314	·7719	·2304	– 1·0609	– ·0415
4	B	·3895	·2844	– ·2940	– ·0429	·6387	·9757
5	C	– ·4930	– ·0349	·2064	·0697	– ·1962	– ·4480
	Total	– ·5178	·6809	·6843	·2572	– ·6184	·4862
	Total	·9877	1·4396	1·1921	1·4130	– ·1900	4·8424
Data:			33 – 35				
3086	A	·3329	1·0212	·4987	– ·1034	– ·2151	1·5343
7	B	– ·5726	– ·4104	– ·5154	– ·4306	– ·1951	– 2·1241
3088	C	– ·1657	– ·0844	– ·5477	– ·3295	– ·1645	– 1·2918
	Total -	– ·4054	·5264	– ·5644	– ·8635	– ·5747	– 1·8816
Grand	Total -	·5823	1·9660	·6277	·5495	– ·3142	2·9608
Grand Total, all entries				-	-	-	3·4513

PROCEEDINGS

OF THE

Society for Psychical Research

PART 150

REVIEW OF MR WHATELY CARINGTON'S WORK ON TRANCE PERSONALITIES [1]

By Robert H. Thouless

I. Introduction

My function in the present paper, as I understand it, is not primarily to express my own opinion as to the conclusions that may legitimately be drawn from Mr Whately Carington's investigation of trance personalities.[2] My more important task is to try to give such an account of his methods as may enable even those who are not familiar with the mathematical devices which he employs to understand these methods sufficiently for them to judge the experiments for themselves. The importance of the experiments is obvious. Whatever the answer may prove to be, it is a considerable achievement to have devised a method of submitting to quantitative and statistical test, the problem of whether the alleged spirits which communicate through mediums are the autonomous personalities they claim to be or whether they are merely organisations within the medium's own personality.

[1] This paper was read in an abbreviated form at a Private Meeting of the Soeiety on June 30, 1937.

[2] "The Quantitative Study of Tranee Personalities," I, II and III, W. Whately Carington, *Proceedings of the Society for Psychical Research*, xlii, pp. 173-240 ; xliii, pp. 319-361 ; and xliv, pp. 189-222. These are hereafter referred to as TP 1, TP 2 and TP 3 respeetively.

We must all admire the industry with which the author has laboured, the boldness and originality of his design, and the perseverance with which he has overcome difficulties. I understand that he knew little of statistical methods when he started and has had to acquire this knowledge in the course of his work. If he has made mistakes, as I think he has, this is less remarkable than the fact that he has acquired mastery of a complicated mathematical technique which he required as a tool for his investigation.

I think that such work deserves that we should examine and criticise it with as much care as the author has put into the investigation. Probably many of us who would be willing to do this have found the task rather a formidable one. Certainly I found it so myself. The author has not made it easy for the reader to follow him, and many readers probably feel themselves wholly debarred from following him by the fact that he uses statistical methods which make his pages fearful with formulae.

Unfamiliarity with statistical methods is really no reason why any reader should not be able to form his own opinion as to the conclusions of the work. Statistics are only an intermediate step in drawing a conclusion as to fact. The final step is a logical one. If we are told what relationships are indicated by statistical methods, we need not be mathematicians to see what conclusions can be drawn from these relationships. My purpose then is to demonstrate the statistical methods used by W. W. C. so far as is necessary for understanding what sort of conclusions they point to, to say whether I think the statistical reasoning is sound, and finally to put my readers into a position in which they can ask for themselves the final logical question: " What does it all prove? "

II. Problems and Methods

The ultimate problem of Mr Whately Carington is whether the controls and the communicators in mediumistic séances are, as they claim to be, autonomous personalities, or whether they are secondary personalities of the medium. A completely convincing answer to that question may, of course, be remote, and the more immediate aim of his research has been to develop a technique of experimentation and of treating the raw data of his results, and to discover whether there is reasonable hope that this kind of experiment will be able ultimately to provide a complete or partial answer to this question. A secondary object of the research was that, if it did not provide evidence for the existence of communicating spirits, it should give information about the psychology of trance states.

The first of these problems we may call (in W. W. C.'s terminology) the problem of the "autonomy of communicators". There are two main alternatives to be tested (with certain intermediate possibilities). First, there is the spiritist explanation of communicators, that they are really different and autonomous personalities who can communicate through one medium or through another. The other possibility is that they are merely unconscious creations of the medium in the trance state, or at best secondary personalities of the medium.

When we speak of the possibility that John and Etta are different personalities, we mean primarily that they are different in the same sort of way as any two living persons are different. The matter is complicated by the fact that, if we judge difference of personality by difference of character and interests, we all know that one and the same person may differ considerably in these respects at different times. Also, of course, truly different personalities may appear more or less similar in any characteristics that we can use to identify them. The changes in a single human being may be so great that we speak of "double personality". This simply means that, within the limits of a single human body, there are differences of personality as great as we commonly find between two bodies. In considering how great a difference must exist between communicators before they can be considered to be different personalities, it may be necessary to bear in mind the possibility (mentioned by W. W. C.) that the separateness between personalities may be less complete in the spirit world than in this world. J. and E., even though both were autonomous communicating spirits, might have more in common than J. and E. had in this life. This would not prevent W. W. C.'s latest method (developed in TP 3) from identifying them as autonomous personalities provided that anything remained peculiar to J. and to E. respectively through whatever medium they communicated. If, however, there were after death a continuing existence in which all individuality was lost and all personalities were fused in one world-soul, I see no possibility that evidence for such continuance could be obtained in this way. Nor would there then seem to be any sense in which we could speak of autonomous spirit personalities. The method can only reveal spirit communicators who, communicating through different mediums, retain some individual characteristics in which they differ from other communicators.

Mr Whately Carington began his enquiry by devising methods of measuring the amount of similarity or difference between different trance personalities by finding out how much they resembled each other or differed from each other in their performance in the "word-association test". While the methods of using the data obtained were changed a great deal in the course of the investigation, the use of the word-association test as the source of the data was retained throughout.

This is a test well known in psychological laboratories, although application to this problem was altogether new when W. W. C. started his work.[1] The method was used by Jung in order to investigate the emotional complexes of his patients. A series of words was called out to the subjects who were required to respond, as quickly as possible, with the first word that came into their minds. The time taken for the response was noted by means of a stop-watch. This is the "reaction time" (W. W. C.'s RT). At the end of the experiment the same list was called out again and the subject was asked to try to respond with the same word as before. This is the "reproduction test" (W. W. C.'s RPN). The fact that a word had a hidden emotional significance was said to be shown by the fact that it has an unusually long reaction time, and that there was a strong tendency for a failure in reproduction, that is, for a different word to be given as response on the second reading of the list. Another measure of the emotional significance of the word may be obtained by connecting the subject with a suitable system of electrical apparatus and noting the change in his electrical resistance when he responds to the word (the psycho-galvanic reflex or PGR). High emotional significance is shown by a large drop in resistance.

Although W. W. C. used the PGR in his early experiments, he did not find it a satisfactory tool for his purpose, and, in this survey, we need not bother about it but confine our attention to reaction times and reproductions. I shall use the word "response" as a general term to cover both RT and RPN. We must remember that unusually long reaction time indicates the same kind of thing as failure of reproduction.

I think the experimental psychologist would feel more happy about Mr Whately Carington's work if somewhat fuller details could be given of the exact methods employed in applying the test, and if a small sample could be given of the actual raw results of the test. Perhaps both of these deficiencies may be remedied in a future publication.

The same test was applied, at (I think always) the same sitting to the control and to different communicators who were communicating through the medium, and results were obtained generally

[1] Since then, the method has also been used by Mr Hereward Carrington.

on six different occasions. These six sets of readings for any "personality" were treated together for the subsequent mathematical working out of results.

III. The Course of the Investigation—Recantations and conclusions

With admirable open-mindedness, Mr Whately Carington has been very ready to change his opinions between successive publications. No one will blame him for this, but it does make it rather difficult for the reader to get an idea of the investigation as a whole, since in reading TP(n), he must bear in mind what part of it has been repudiated in TP($n+1$), and having read the last of the series he may wonder how much of that will be repudiated in the still unpublished next one. This source of difficulty could, of course, have been avoided if the author had felt himself able to postpone publication until all criticisms by himself or others could have been considered, and it is to be hoped that when the investigation is brought together in its final form it will be given the self-consistency of which circumstances have now deprived it.

Until then, we must try as well as we can to bring the parts into relationship with each other, and I will try to give a brief account of the conclusions which have been advanced, and which have been later withdrawn. The author has very kindly indicated to me what parts of TP 3 will be repudiated in TP 4.

First, in TP 1 we have a description of experimental and statistical techniques, which, with a few modifications, were used throughout. Methods of calculating indices of "similarity" and "difference" are explained in an appendix. This paper is written with the assumption (that seemed to W. W. C. to be justified by his own earlier experiments) that one and the same personality will always produce sets of reactions which are not significantly different.

The most important change of opinion in TP 2 is that this assumption is given up. Mr Gatty tested by Mr Besterman had shown that he produced significantly different sets of reactions by merely imagining himself in different life-situations. This makes W. W. C.'s problem much more difficult since it is no longer possible to conclude the autonomy of a communicator merely from the fact that his reactions are significantly different from those of the medium, and more complex methods of solving the problem must be sought. We may indeed notice that, since Mr Gatty was not tested in really different but only imagined different situations, and since he does not appear to be a particularly unstable person, it may be doubted whether the range of " differences " between different phases of the same person may not be much greater than is revealed by the Besterman-Gatty experiment.

In Part III of TP 2 an important argument is developed that there is a relationship of " countersimilarity " between mediums and controls and that this is evidence that controls are secondary personalities of the medium. This argument is dealt with later in the present paper. It is not modified in any way later by W. W. C.

In Part V of TP 2, the author tackles the autonomy of communicators tentatively by a new method. The same communicators are tested but with different mediums. The communicators turn out to be " similar " to themselves even when the mediums are different. This cannot be taken directly as a measure of the real similarity of the communicators to themselves unless we can first eliminate the effect of the " similarity " of the mediums. This W. W. C. here attempts to do by the method of partial correlation. He puts forward his conclusions very tentatively with some doubt as to the legitimacy of the method.

In TP 3, he decides (as a result of Professor Fisher's criticism) that the method of partial correlation cannot legitimately be used for this data. This does not mean a complete abandonment of this kind of enquiry (the " inter-medium method ") but only of the particular statistical method used in TP 2. Another method based on analysis of variance is used in TP 3 on the inter-medium data. At the end of TP 3, the author suggested that evidence for the autonomy of communicators had become very strong although he was not satisfied that it was sufficiently so for complete conviction. Further criticism from Professor Fisher caused this claim also to be abandoned.[1] Unfortunately, these conclusions were largely based on " reproduction " material. A reproduction score must either be +1 or 0, so it cannot be distributed even approximately in the " normal " distribution assumed in the mathematical method of " analysis of variance " used by W. W. C. While relations of difference and similarity may be indicated by the z for " reproduction " data obtained by the analysis of variance, neither the amount of this z nor its calculated standard error can be relied on. This objection does not apply to reaction times and, presumably, TP 4 will deal with the work that W. W. C. is now engaged on,

[1] I am here reporting recantations and new work not yet published by W. W. C. which he has kindly communicated to me by letter. I should like to take this opportunity of acknowledging the friendly help which has been given to me by the author in all my study of his work.

of seeing how far the same results emerge from reaction time data alone.

W. W. C. also considers that a mistake was made in this section in the inclusion of two sets of data from the same medium (Mrs Leonard). Since the object of TP 3 was to find out whether a communicator, ostensibly the same individual communicating through different mediums, gave evidence that he was really the same, the inclusion of two sets of data from the same medium was obviously a mistake, because a positive result would have shown only that J_L (John communicating through Mrs Leonard) at one time was the same as J_L at another time, which would have been much less strong evidence of the autonomous existence of John than would demonstration of the identity of J_L with John as communicating through another medium.

One rather important recantation of the methods of TP 3 is of W. W. C.'s unfortunate excursion into Hall and Knight's algebra in paras. 32 and 33 for a method of combining independent probabilities. This he now gives up altogether.

To sum up. The general method of experimenting remains the same at the end as at the beginning. For different reasons, both the "psychogalvanic" responses, and the records of reproductions, have been now discarded. This really matters very little; W. W. C. needed only one of the three kinds of response for his purpose, and he has found out by experience that the reaction-time records best serve his needs. The general method of argument in TP 3 is retained although many of its details are now regarded by W. W. C. as mistaken. This method, simplified and amplified and applied to reaction time data, will form the foundation of his next contribution. The conclusion in TP 3 is no longer defended on the evidence there presented. The conclusion that controls are countersimilar to mediums seems alone to have suffered no vicissitudes and remains now as stated in TP 2.

There is one point about these recantations that I should like to make clear. They might easily be represented by an unsympathetic critic as merely a process by which Mr Whately Carington has blundered from one error into another. I think, on the contrary, that they are a process by which his technique of enquiry has passed from a relatively poor one to a good one. The methods used in TP 3 are enormously better than those in TP 1 although TP 3 happens to be disfigured in its later part by serious errors. Probably every scientific investigator improves his methods of working by a process of trial and error. It is only unfortunate that the "error" part of this process has in W. W. C.'s case been immortalised in print.

IV. The Measure of "Similarity"

We may remind ourselves of the nature of the results for any one "personality". For each of the six sittings there was a set of "reaction times" which were the times in seconds that he had taken to respond to each word. Certain devices of scaling were used (clearly explained and adequately justified by the author in TP 2) in order to get rid of irregularities caused by abnormally long reaction times. The reaction times, so corrected, were the raw material for what W. W. C. calls the RT data. The raw material for his RPN data were marks of 1 put opposite words that had been correctly reproduced on the second reading of the list, and of 0 against those that were incorrectly reproduced or not at all.

It is clear that two lists of responses (for the sake of clearness let us think of reaction times) obtained at any sitting from two personalities, may either resemble or differ from each other to any degree. Also it is clear that there is more than one respect in which they may be said to be similar or to differ. Two sets of reaction times in which every word caused exactly the same time of response to both personalities would obviously be exactly similar on any method of reckoning. Let us suppose, however, that one personality always took exactly twice as long as the other in responding to each word. The two lists would then be dissimilar if we took the mean time of response as our criterion of similarity, but would be exactly similar if we took as our criterion the general shape of the pattern of response time, that is, the proportion of each response time to the average time for the personality making it.

It is in the second sense that W. W. C. measures "similarity". Two lists are "similar" if a word which causes in one personality a response which is long for him, causes in the other personality a response which is similarly long for him, irrespective of whether the absolute times are or are not the same. His reason for choosing this criterion of similarity is, I think, that he finds in practice that it is a better indication of how much the personalities really resemble each other than would be, let us say, a comparison of their average times. Also I think that this might reasonably be expected and is the most commonly used criterion of resemblance in everyday life. We say, for example, that a small boy resembles his father because his features, although they differ much in absolute size from those of his father, are related to each other in the same way as his father's; his nose is absolutely much shorter than his father's but, like his father's, it is long in proportion to the other features of his face, and so on.

The detection of similarities of this kind is a common problem in applied psychology. It is the measurement of a " correlation ". If three boys had marks in a Greek examination of 70%, 60% and 50% respectively, and in a Latin examination of 85%, 80% and 75%, we should say that these two sets of results were completely correlated. A boy who did well or badly in Greek would do correspondingly well or badly in Latin. The conclusion we should be likely to draw would be that the two examinations measured the same mental capacity. If on the other hand, a boy who did well in Greek proved always to do correspondingly badly in Latin, we should say that the two sets of results were completely negatively correlated. If there were no relationship either positive or negative between the two orders they would be uncorrelated.

W. W. C.'s method of measuring " similarity " is, in effect, the measurement of a correlation. The average size of response of one personality A to the first word of the word list, his average response to the second, to the third, and so on, form one series. The other series is formed by the average responses to these same words given by a second personality B. The amount of correlation between these two series is a measure of the similarity of the two personalities in the sense in which W. W. C. is speaking of similarity. The responses used for this measurement may be reaction times, reproductions, or psychogalvanic reflexes, so for the same pair of personalities there may be calculated an RT similarity, an RPN similarity and a PGR similarity.

The most familiar way of calculating a correlation is by the use of the " Bravais-Pearson " formula which gives a measurement (the correlation coefficient or r) which is $+1$ for complete positive correlation, -1 for complete negative correlation, and 0 for no correlation, and, of course, some intermediate value between 0 and $+1$ for such degrees of similarity as are commonly met with in practice which fall short of perfect correlation in various amounts.

W. W. C. does not, in fact, use the Bravais-Pearson formula but obtains a measurement of the amount of correlation by the use of the method of analysis of variance. This gives a quantity z which also measures the degree of similarity of the mean responses of the two personalities concerned to the same words of the word list. This quantity z can be derived from r by a simple arithmetical transformation and differs from r in the fact that for perfect correlation it would be infinite instead of being unity. Those who are used to thinking of degrees of correlation in terms of the correlation coefficient r may find it helpful to remember that, for small values, z is only slightly greater than r, the difference being less than 10%

when z is 0·5, so below this value we shall not be far wrong to think of z as equal to r.

Realising that, to many of his readers, very little idea of the degree of similarity is conveyed by the statement that z is, let us say, 0·43, W. W. C. in his last paper transforms this into a percentage of similarity by converting it into the corresponding r and multiplying by 100.[1] Thus the above similarity would be expressed as a 40% similarity. The formula used [$r=(e^{2z}-1)/(e^{2z}+1)$] is strictly only the correct one for deriving r from z when (as in "similarity" measurements) only two series are compared and not, therefore, for the measurement of "individualities" (see Section VII) for which it is also used. This, however, does not, I think, matter since W. W. C. is quite justified in defining what he means by "percentage individuality" as $100\times(e^{2z}-1)/(e^{2z}+1)$, whether or not this is equivalent to $100\times r$.

The occurrence of a positive measurement of similarity is, of course, a merely mathematical fact whose interest for us lies in its indication of the psychological fact that the two personalities resemble each other. Since confusion may arise by using the one word *similarity* for the mathematical indication of a fact and also for the fact it indicates, I shall use inverted commas for "similarity" (or W. W. C.'s symbol "S") when what is meant is the mathematical fact that there is a positive value of z in the similarity measurements, reserving the word similarity without inverted commas for the fact of resemblance. As will be discussed in more detail later, "similarity" must exceed a certain minimum value (the lowest value for *significance*) before it can be treated as evidence of similarity.

If we measured the "similarity" of human faces by the method suggested as analogous to W. W. C.'s measure of "similarity" in responses to the word-association test—*i.e.* by measuring the correlation between the measurements of the features of any two faces—we should find that some faces, such as those of relatives, resembled each other closely while others resembled each other less but that in no one was resemblance altogether absent. Mr Whately Carington's face and my own are not very similar as human faces go, but would be much more "similar" than would be either of our faces to that, let us say, of a chimpanzee. The common human form would itself give a certain degree of "similarity". Thus if, from the "similarity" of a face seen at one time to that seen at another time, we wanted to prove that they were one and the same face, we could not be content merely to infer this from the fact that our measurements showed high "similarity". It would be

[1] TP 3, p. 192.

necessary to show that this " similarity " was greater than that found in a large range of comparisons between different people's faces.

There is the same reason for expecting very generally some degree of " similarity " between different persons in their responses to a word list. Most people, for example, may be expected to give longer reaction times to " dead " on W. W. C.'s list than to " window " (TP 1, p. 223). So far as they react to words of common human interest, they will tend to be " similar ". One man's individual history may, however, give strong emotional significance to " ship " which does not exist for another. Such individual differences will tend to make their reactions differ. If, however, there are words to which all personalities tend alike to give prolonged responses, there will be a general tendency for all personalities to show more or less positive " similarity ", tending, of course, to be greater if the personalities really closely resemble each other. This general tendency to resemble each other is what W. W. C. calls the " common humanity " factor.

Since the resemblance due to common humanity may be present in any comparison between two personalities, we cannot, merely by measuring their similarity and finding it high, prove that a personality A communicating through one medium is really the same as one claiming also to be A communicating through another medium. This difficulty was fully realised by W. W. C. in the later stages of his research (in TP 3) when he adopted a method of demonstrating identity less simple but entirely free from this objection. The seriousness of the difficulty was certainly not realised by him at the early stages of the research. In the first pages of TP 1, for example, he compared the quantities obtained by the reaction time test with the measurement of finger-prints for the identification of criminals. Unhappily the analogy is imperfect in its most important quantitive aspect. By comparing a small number of points of coincidence between finger-prints, identity can be established because the similarity is greater than could be found once in a billion times between two different people. In a mental test on the other hand, at best, the degree of resemblance between two performances by the same person is no greater than can be found in a not very large range of different people. This seems to be particularly true of the word association test, as may be seen by the very low measurements for self-consistency obtained by W. W. C. (TP 2, table II). The attempt to identify personalities by their reaction time responses is as different as it can well be from identifying them by their finger-prints.

We have already said that " similarity " as measured in this way will tend on the whole to be greater in those personalities which most closely resemble each other. This, however, is a different thing from saying that the amount of a " similarity " measurement measures the degree of similarity between the two personalities. Correlation coefficients are, in fact, often used as measures of degree of resemblance but this usage is generally agreed to be improper. In truth, a measure of correlation depends partly but not wholly on the degree of similarity of the series compared. This means that we cannot properly conclude that two personalities A and B resemble each other more than do C and D from the fact that A and B have a higher z for " similarity " than have C and D. It seems, therefore, that much of the discussion in TP 1, paras. 21 onwards, of the amount by which different personalities in a manifold resemble each other, is based on mistaken premises and should be discarded or attempted by other methods. A significant " S " measurement indicates that there is similarity between the personalities compared, but the relative sizes of the z for different personalities is a very uncertain indication of the degree of similarity between them.

I do not think that the conception of " counter-similarity " should offer any obstacle to understanding. This is the relationship more commonly known as " negative correlation ". If we found that those people who took the most medicine were on the whole in the worst health, this would be a " negative correlation " or " counter-similarity " between medicine-taking and good health. This would be as definite evidence of a causal connection between medicine-taking and health as would be a " positive correlation " or " similarity ", thus differing altogether in its indication from the mere absence of correlation which suggests no causal connection.

The features of any two human faces are " similar " ; a human face and a plaster mould of a face would be " counter-similar " whereas the features of a human face and the roughnesses on the surface of a seed potato would, I expect, show zero similarity. In these three cases we should find the z calculated for " similarity " to be positive, negative and zero respectively.

V. The Measurement of " Difference " by Analysis of Variance

W. W. C. makes an estimate not only of the " similarity " of the personalities investigated but also of their " difference ". The method of doing this is explained also in Appendix II of TP 1.

It does not appear from such enquiries as I have made that many of his readers have understood this part of his work. The matter is difficult in itself, particularly to those who have no previous acquaintance with the mathematical methods used. Its difficulty has not been reduced by the not infrequent obscurity of W. W. C.'s exposition and the occurrence of too frequent misprints. I shall try in this section to clarify the issues involved. It is possible that I may not succeed and then the criticisms I have directed against W. W. C.'s obscurity must fall equally on my own head.

One of the most obvious difficulties which readers have met is that of understanding what is meant by " difference " in W. W. C.'s measurements. In ordinary speech, *similarity* and *difference* are opposites. If we say that two faces are similar, we mean the same as if we say that they are not different. More exactly (since similarity and difference may exist in any degree) we should say that the more similar are two faces, the less different they are, and *vice versa*. With these ordinarily accepted meanings of *similarity* and *difference* in our minds, we may find ourselves puzzled by some of the relationships indicated in W. W. C.'s papers. That some pairs of personalities appear to be both similar and different is not a real difficulty. Any two things that are not completely similar will be similar to some extent and different to some extent. What, however, are we to make of such observations as that of Mrs Leonard (normal) and Mrs Leonard (prepared) [TP 1, p. 200] who are stated to be neither similar nor different? Does " difference " here mean " dissimilarity " or does it mean something else?

We can only discover exactly what is meant by these terms by examining how they are measured. I think it may save perplexity if I indicate here what seems to me to be the true answer to the above question, which will be more fully justified later. W. W. C.'s " difference " measurement is not a measurement simply of *difference* but of *consistency of difference*. We can put this in other words and say that it measures not simply *dissimilarity* but *consistency of dissimilarity*. The quantity z obtained in comparing any two personalities for difference may be small because the reactions of the two personalities do not much differ or because, although they differ, they do not show consistency in their differences on different occasions. If the D measurement is low only for the first of these two reasons, the S measurement is likely to be correspondingly high, but not if it is lack of consistency that makes the D measurements low. Lack of self-consistency in either of the personalities compared will also reduce the S measurement for that comparison. So it may easily happen that both S measurement and D measurement are low for the comparison between any two personalities. This does not mean that the personalities compared are neither similar nor different, but that any similarity or difference that exist have been obscured by the lack of self-consistency in the measurements. W. W. C.'s phrase on the page cited above: "The two states may be regarded as quite neutral, so to speak, with respect to each other" seems to me, therefore, to be incorrect. The peculiarity revealed is a peculiarity of the measurements and not of the states measured.

It is a perpetual source of discouragement to those who try to read modern investigations in experimental psychology (and in other biological sciences) that their pages are full of statistical calculations. Except for those who have been endowed with a perverse taste for mathematical puzzles, these are generally regarded as parts to skip. In great part, there is no reason why they should not be skipped by most readers. They are merely (as has already been indicated) necessary preliminaries to a logical step and it is this logical step in which we are primarily interested. Statistical methods are used to discover whether a set of figures indicate anything, and, if they have been honestly and competently performed, we may take their results for granted and go on to ask what the figures do indicate. Sometimes, however, the statistics and the logic of an argument are not sufficiently independent for it to be possible to know what conclusions are indicated unless we have some rough idea of the preceding statistical steps. That is, I think, the case in this investigation. It is not necessary that a reader should understand the method of using the analysis of variance either in the sense that he sees why successive steps are taken or even in the sense that he could use it competently himself. It is necessary, however, if he is to form a reasonable opinion of the work, that he should see the kind of thing the analysis of variance is getting at.

No very great mental effort is required for this. The basic problem to be solved by the analysis of variance is a simple one, and the method of solving it is also fundamentally simple. Although in analysing a large table, the figures dealt with may be uncomfortably big, the arithmetical operations themselves are only those of multiplication and addition which are familiar to the average child of ten.

We may illustrate the arithmetic of the method by an example drawn from a more concrete field than that of W. W. C.'s data. Let us suppose that a group of seven poultry-keepers differ as to whose hens lay the most eggs. A sceptical onlooker says that none of them is a more successful poultry-keeper than another, that sometimes one and sometimes another is lucky, and gets most eggs in any particular period, but that on the average there is no difference between them. The dispute is referred to an impartial referee.

In order to keep this case as closely parallel as possible to W. W. C.'s, we shall suppose that the referee is not required to decide a dispute between the poultry-keepers as to which is the most successful, but the dispute between all of them and the sceptical outsider as to whether there is any real difference between them with respect to their success. In any case, this is the first question. Unless there is a real difference, there can be no question of which is best.

The referee begins by making a random selection of five representative hens from each yard and keeps them under his observation for a fortnight, and records the number of eggs laid by each. He can write these in a table (as Table I below), in which after the name of each poultry-keeper he writes the number of eggs laid by each of his five hens (columns 1-5).

TABLE I

	(1)	(2)	(3)	(4)	(5)	(6) Totals	(7) Averages	(8) Sums of squares of deviations from average	(9) Σ dev. $^2/4$
Brown	8	8	8	6	5	35	7	8	2·0
Smith	5	8	5	5	7	30	6	8	2·0
Jones	5	6	6	7	6	30	6	2	0·5
Scott	4	2	7	3	4	20	4	14	3·5
Briggs	4	8	3	9	6	30	6	26	6·5
Webb	7	11	11	7	4	40	8	36	9·0
Thomas	4	7	4	6	4	25	5	8	2·0
							Average = 6	Total = 102	Mean = 3·64

These are his data. He must now perform the necessary calculations to discover what (if any) conclusions he can draw from them. His first step is the obvious one of finding the average number of eggs obtained from the five hens of Brown, from the five of Smith, and so on. These he writes under the word "averages" in the seventh column.

This column 7 is obviously the important one for the referee. If he were sufficiently a simpleton, he might be content to notice that these averages differed amongst themselves and conclude that this was in itself sufficient evidence that there was a real difference between the different owners with respect to the laying power of their hens, and so award the victory to them against the sceptical outsider. This would, however, obviously be fallacious since the referee would have failed to take into account the fact that the different hens of the same owner had laid different numbers of eggs, and that even if they had been seven sets of five hens drawn from the same yard, their averages would not have been identical, for we cannot expect identical averages from samples whose members differ amongst themselves. Even if there is no real difference between the samples, their averages will differ by an amount which depends on how much the individual hens of the same owner differ amongst themselves.

The task of the referee is, therefore, to decide whether these differences between the average number of eggs per hen obtained by the different poultry-keepers are greater than the differences that would result merely from the fact that each is an average from a set of numbers differing amongst themselves. Unless they are, it is obvious that the results of the test provide no evidence whatever that one poultry-keeper is better or more successful than another.

In order to establish whether or not this is the case, the referee must have some way of measuring the "scatter" (*i.e.* the degree of difference) of a set of differing numbers, and also he must know how much scatter will be caused amongst a set of averages by a given amount of scatter in the figures from which they have been calculated.

In order to get a measure of the scatter of any set of figures, he first finds out how much each of the figures differs from the average of the set, he squares each of these amounts and adds all the squares together. He then divides by the number of "degrees of freedom", *i.e.* the number of the figures which could be independently varied while keeping the same mean. This divisor is, therefore, one less than the number of figures in the set.

For example, the set of averages given in column 7 of Table I have a mean of 6. Their separate deviations from this mean are: +1, 0, 0, −2, 0, +2, −1. Squared, these become: +1, 0, 0, +4, 0, +4, +1[1]. The sum of these squares is 10, which divided by 6 (one

[1] If the referee is using a calculating machine, however, he may prefer to find the sum of the squares of deviations from the mean by summing the squares of the figures themselves and subtracting from them the square of their mean multiplied by their number. This is frequently the actual operation carried out by W. W. C. This, however, is merely an alternative way of getting the sum of squares of deviations from the mean and involves no difference in principle from the above operation. In the present case, this would give us $262-(7\times 36)=10$, which is, of course, the same result as above.

less than their total number of 7) is 1·66. This then is a measure of the scatter of the averages of different owners. It would be zero if all of these averages were the same ; the more they differ amongst themselves, the bigger it becomes.

This quantity (when calculated from a distribution of figures for which it is an appropriate measure of scatter) is called the *variance.* Very commonly, the square root of this quantity is calculated and is called the *standard deviation* (or σ). All that need be remembered is that the variance, the standard deviation, or any other measure of scatter, is simply a measure of how much a set of quantities differ amongst themselves.

The referee now knows the actual scatter of the average scores of the different poultry-keepers. He must now compare this actual scatter with the amount of scatter of the averages which would have resulted from the individual differences between the hens. That is, he must know how much the averages for different owners would have differed between themselves even if there had been no real difference between the laying power of hens of different owners.

If, and only if, the observed variance of the averages (1·66) is sufficiently greater than this, he will be able to give an affirmative answer to the question of whether there is a real difference between the laying power of the hens of different owners.

He begins by measuring, in the same way, the scatter between the scores of the five hens from any one poultry yard. He does this separately for each separate poultry yard and takes an average of the seven variances he has obtained. Thus for Brown, the deviations of the hens from the average (7) for this yard are : $+1$, $+1$, $+1$, -1, -2. The sum of the squares of these deviations is $1+1+1+1+4=8$, which divided by 4 (one less than the number of hens in the yard) is 2, which is therefore written in the last column of table under the heading $\Sigma\,\text{dev.}^2/4$ (in which Σ is the sign for summation). The variance for each of the other yards is written below this in the same column. This column is then added up and divided by 7 and is found to give an average of 3·64.[1] Let us call

[1] The referee might also have got this quantity by a different method. If he had added together the squares of all the scores for the 35 separate hens, he would have got 1,412. By subtracting the square of the general average (6) multiplied by the total number of hens in the experiment, he would have got $(1412-36\times 35)=152$. By further subtracting the sum of squares obtained earlier (from the averages of scores of different owners) multiplied by the number of hens in the experiment belonging to each owner, he gets $(152-(10\times 5))=102$ which divided by 28 (*i.e.* by $7\times(5-1)$) gives 3·64 as above.

That this must give the same result as the simpler procedure above may be shown by simple algebra. It may seem a laborious and roundabout way of this quantity the "mean variance within poultry yards" in order to distinguish it from the measure of scatter obtained earlier which was the variance of the average scores of the different owners (the variance between the averages for poultry yards).

The simple proposition that the referee uses is that if there were no real difference between the owners, then the mean variance within poultry yards would be about equal to the variance of the average scores of the different owners multiplied by the number of hens for each owner from which this average score has been calculated (in this case, five). The variance of the average scores of the different owners is, as we have seen above, 1·66. The quantity required will, therefore, be $1{\cdot}66 \times 5 = 8{\cdot}33$. We may call this the "total variance between poultry yards" or simply the "variance between poultry yards". If these two quantities (the mean variance within poultry yards and the variance between poultry yards) were equal or nearly equal, we could conclude that the hens of different owners differed no more in their power of laying eggs than they would if all had been drawn at random from the same poultry yard.

TABLE II

	Degrees of freedom	Sum of squares	Mean square (*i.e.* variance)	$\frac{1}{2}\log_e V$
Between poultry yards	6	$5 \times 10 = 50$	8·33	1·060
Within poultry yards	$7 \times 4 = 28$	102	3·64	0·646
Total - -	34	152	4·47	$z = +0{\cdot}414$

If, however, we turn to the figures that we have actually obtained (see Table II), we find that this is by no means the case. The variance within poultry yards was only 3·64 while that between poultry yards was 8·33. Thus the scatter of the mean scores of different owners is more than twice as great as we should expect it to be if it were caused only by the differences between different hens. Can the referee then return an affirmative answer to the question as to whether there is any real difference between the success of different poultry-keepers?

getting the result, but this is only because we are working with an artificially simplified set of figures which can be easily worked out in our heads. With the kind of data generally found in practice, and if results are obtained by the use of a calculating machine, the second method is the easier and was, in fact, used by W. W. C.

Unfortunately, not yet. There is a further possibility that he must take into consideration. We said earlier that if there were no real difference between the skill of the poultry-keepers the variances between and within poultry yards would be equal *or nearly equal.* Certainly they are not equal, but are they sufficiently nearly equal for it to be reasonable to attribute to chance the higher variances between poultry yards? This is the next question the referee must ask himself.

In order to answer it, he must calculate a quantity z which is half the difference between the Naperian logarithms of the two variances. The required quantities are given in the last column of Table II under the heading "$\frac{1}{2}\log_e V$", and the required value of z is seen to be $(1{\cdot}060 - {\cdot}646) = {\cdot}414$.

The referee may or may not understand the reason for taking this step. All that is necessary is that he should carry it out correctly and understand the use of the figure he has obtained at the end. His reason for calculating z is not (as perhaps some of the competitors imagine) that he wants to go through a mathematical ritual which will make his results incomprehensible to a layman. It is because he knows the danger that an apparent indication of the truth of the hypothesis that he is testing (that there is a real difference between the success of different poultry-keepers) might occur as an effect of chance, and, like all others who have to make practical use of statistics in research work, he has at his elbow a copy of Fisher's *Statistical Methods for Research Workers.* His intention is to turn up the appropriate table in Fisher's book in order to discover what is the likelihood that a value of z as large as or larger than the one observed might have been produced by chance if there had been no real difference between the poultry-keepers. When he looks up Fisher's Table VI, he finds that there is a ·05 probability that a z of ·447 might have been produced by chance in these figures. Since the observed value is rather less than this, and a probability of ·05 is the minimum criterion of significance the only reply the referee can give is: "I do not know whether there is any real difference between your skill as poultry-keepers. I think so. If you made another test with more hens or over a longer period, I might be able to give you a definite answer."

The procedure so far described would be correct if (and only if) the hens from each poultry yard had been selected in no systematic way. A slightly more complicated method would, however, be necessary if the experiment had been arranged in a different way, which also, if it is practicable, is a better way. Let us suppose that each of the poultry-keepers kept hens of the same five different breeds, and that, to allow for the possibility that some breeds might be better layers than others, it was arranged that hen 1 of each poultry-keeper was, let us say, a Plymouth Rock; No. 2 a Brown Leghorn; and so on.

The referee must, in this case, perform the additional operation of estimating the amount of scatter due to difference of breed. He adds therefore, under Table I, the total number of eggs laid by hens of each breeds, and then works out the average for each breed. The new lines are as follows:

	(1)	(2)	(3)	(4)	(5)
Totals for each breed -	37	50	44	43	36
Averages for each breed -	5·29	7·14	6·29	6·14	5·14

He then works out the sum of the squares of the deviations of these means from their general average (of 6) exactly as he did for determining the variance between the averages of poultry yards. The sum of squares is, therefore:

$$(-\cdot 71)^2+(1\cdot 14)^2+(\cdot 29)^2+(\cdot 14)^2+(-\cdot 86)^2$$

which equals 2·65.[1] The number of degrees of freedom is four (one less than the number of different breeds), so the variance of the averages is 2·65/4. This must be multiplied by 7 (since each average has been calculated from the score of 7 owners), giving

$$(7\times 2\cdot 65)/4=18\cdot 55/4=4\cdot 64$$

for the total variance between breeds. The referee can now write down a new table of analysis of variance (Table III) showing in the first line the variance between poultry yards, in the second that between breeds, and in the third the variance from all remaining sources. This third line is obtained by subtracting the sum of squares between breeds from that obtained earlier within poultry yards (Table II) and similarly subtracting the number of degrees of freedom.

The z is then obtained as before by subtracting $\log_e$ of the residual variance (in the third line) from $\log_e$ of the variance between poultry yards. The reason for going through this additional process is that any difference between the laying power of different breeds will not affect the scatter between poultry yards (since its effect has been eliminated by taking one hen of each breed from each poultry yard). It will, however, increase the scatter within poultry yards,

[1] Alternatively, if we are using a calculating machine we may prefer to add $(5\cdot 29)^2+(7\cdot 14)^2$, etc., and subtract (5×6^2) at the end, or to add the squares of the totals, $(37)^2+(50)^2$, etc., subtract (5×30^2), and divide by 49. Both of these processes will give the same answer as before, *i.e.* 2·65.

TABLE III

	Degrees of freedom	Sum of squares	Mean square (*i.e.* variance)	$\frac{1}{2}\log_e V$
Between poultry yards -	6	50	8·33	1·060
Between breeds - -	4	18·55	4·64	
Residual variance (or error variance) - - -	24	83·45	3·48	0·6235
Total - - -	34	152	4·47	$z=0{\cdot}4365$

and will, therefore, unless its effect is eliminated, lead us to over-estimate how much scatter between poultry yards might be due to chance causes. This might have led the referee, if he had ignored this factor, to have missed a real difference between the different poultry yards. He is now comparing the scatter between poultry yards with what is, in effect, the scatter within poultry yards after the effects of difference of breed have been removed.

In this particular case, it does not make much difference. It is true that the z is now somewhat larger (·4365 instead of ·414), but since the number of degrees of freedom in the residual variance is less than it was before, the size of z with a ·05 probability of chance occurrence is also greater, and is now ·460. The z obtained is, therefore, still too small to come up to the minimum criterion of significance and the referee can still only give the same answer as before—that he thinks there is a real difference between the poultry yards but cannot be sure without a more extended investigation.

The reason why there is no appreciable increase in the significance of z in Table III is simply that, in constructing this example, I purposely made the difference between the breeds negligibly small (in order to keep it closely parallel to W. W. C.'s data). If the difference between breeds had been large, the significance of the z obtained for the difference between poultry yards would have been much increased. Although he gets no positive advantage by carrying out the more complicated process of Table III, the referee was undoubtedly right to do this, since he knows that there is a possible cause of scatter in his " within yards " variance that is not present in his " between yards " variance, and he cannot know how much it is affecting his results except by going through the calculations necessary for its elimination.

Let us now turn from poultry yards to trance personalities. In

W. W. C.'s data, the different words of the word-association test correspond to the different poultry yards, and the figures on which the analysis is carried out are not the number of eggs laid by a particular hen, but the difference between the response of the two personalities compared. The different occasions on which the test is administered correspond to the different breeds in our example. To show the parallelism between the two cases, we can make a table like Table I, showing W. W. C.'s data. Let us call the two personalities compared A and B, and the successive words of the words-association test, 1, 2, 3, 4, etc. Then A_1 can stand for, let us say, A's reaction time to word 1, B_1 for B's reaction time to word 1, A_2 for A's reaction time to word 2 and so on. The table for working out W. W. C.'s *z* for the measurement of what he calls " Difference " is as follows :

Occasions	I	II	III	IV	V	VI
Words						
1	$(A_1 - B_1)$	$(A_1 - B_1)$	$(A_1 - B_1)$	$(A_1 - B_1)$	$(A_1 - B_1)$	$(A_1 - B_1)$
2	$(A_2 - B_2)$	$(A_2 - B_2)$	$(A_2 - B_2)$	$(A_2 - B_2)$	$(A_2 - B_2)$	$(A_2 - B_2)$
3	$(A_3 - B_3)$	$(A_3 - B_3)$	$(A_3 - B_3)$	and so on.		

Each of these entries is of course a number—the difference between the reaction time of A and the reaction time of B to that word on that occasion. The scatter between the averages of the rows is obtained exactly as in our example, and all the remaining steps are carried out as in the example, the effect of difference of occasion being eliminated in the same way as in the example we eliminated the effect of the difference of breed. If a significant value of *z* is obtained at the end, this proves that there is a real tendency for the rows to differ. Now each row is a series of differences between the personality A and the personality B for a particular word. If A and B did not tend to react differently to different words all the (A – B)'s would be about the same and *z* would be about zero. It would also be about zero if A and B did react differently to different words, but did not show any consistency in their differences on different days, if, for example, A reacted much more strongly than B to word 1 on the first occasion but much less strongly than B to the same word on the second occasion, and so on. Thus both difference between the reactions of A and B and consistency between these differences are necessary for a significant value of *z*, and this measure would be more correctly described as one of " consistency of difference " than simply as one of " difference ". I shall use for this measure W. W. C.'s letter " D ", but we may bear in mind that D stands for " consistency of difference " and not merely for " difference ".

There is another measure obtained by the analysis of variance of which little use is made, and which, I think, offers no special difficulty to understanding. This is *covariance* which (as used by W. W. C.) means the extent to which the different occasions of testing produce an effect of the same kind on the responses of the personalities compared. The existence of positive covariance would seem to indicate the same kind of thing as the " similarity " measurements, *i.e.* a relationship of resemblance between the two personalities. On the other hand, absence of covariance may indicate nothing except that neither personality shows variation of response depending on occasion. " Covariance " was rarely found, so it is of little importance for our present purpose.

VI. Tests of Significance

We cannot infer the existence of either " similarity " or " difference " merely from the fact that we obtain a positive value of z in the S and D measurements respectively; it is also necessary to show that the z which has been obtained is larger than might have arisen by chance from figures which do not really indicate the relationship in question. The usual minimum criterion for regarding z as significant is that it shall be larger than a value which might have arisen by chance once in 20 times. This may be expressed by saying that P (the probability that in a collection of figures not showing the relation in question, the observed value of z might have arisen by chance) is less than ·05. This is a minimum criterion of significance. We shall feel more confidence in the result of any observation if it satisfies a more severe criterion than this; we may prefer, for example, that P should be less than ·01.

W. W. C. generally makes the appropriate tests for the significance of his results and attaches to each of the measurements of S and D in his tables, the corresponding value of P. It is unfortunate that the P generally printed by him is not the quantity generally meant by this letter but one half of it, *i.e.* it is the likelihood of a quantity of at least the observed value *and of the observed sign* arising by the chances of sampling from a population randomly distributed about zero. This is erroneous (as W. W. C. now agrees), since " similarity " measurements might vary significantly in either direction, that is, either a $+ve$ or a $-ve$ value might be significant of a real relationship. Since the requirement that P should not be greater than ·05 is the minimum requirement for significance, it is necessary to remember that this division has taken place, since otherwise in the " similarity " measurements, we shall be accepting as evidence of relationship, amounts which might have arisen by chance once in ten times (instead of once in twenty times). This division by two would, however, only have serious consequences when we were using the minimum criterion of significance (of $P < \cdot 05$).

In the case of "difference" measurements, on the other hand, no meaning is to be attached to negative z's. These could only arise as random deviates, so in estimating the significance of a positive D, we should be quite justified in asking what was the likelihood of a value of that sign, and, therefore in dividing by two the value of P found in a table of normal deviates. Unfortunately, when W. W. C. divides P by two for one set of measurements and not for the other, it is for the "similarity" measurements that he divides by two while the "difference" measurements remain undivided.

The method by which W. W. C. estimates this quantity P is by calculating the standard errors of his S and D measurements (by formulae given in TP 1, pp. 215 and 219 respectively), dividing the z by the standard error so obtained, and looking up the P for the value so obtained in a table of normal deviates. If this ratio *i.e.* (measurement)/(standard error), is 2 or over, in any normally distributed quantity, the value of P is less than ·05, so the z can be taken to be significant. Thus in the comparison between Mrs Leonard (normal) and Feda in TP 2 (Table II, RN 72), the z's for S and D are $-\cdot 0400$ and $+\cdot 2073$. By using the formulae as given by W. W. C., we find that the standard errors of these are ·116 and ·092 respectively. The ratios we want are, therefore, $\cdot 0400/\cdot 116 = \cdot 345$ for the S measurement and $\cdot 2073/\cdot 092 = 2\cdot 25$ for the D measurement. Now turning up a table of normal deviates we find that for a ratio of ·345, $P = \cdot 73$, and for a ratio of 2·25, P is ·025. The "difference" measurement for these personalities is, therefore, significant since it would only occur once in forty times by chance if there were no real "difference", whereas the "similarity" measurement is of a magnitude which would be more likely than not to occur by chance if the real value of z were zero, so we cannot infer from it that there is any real similarity between Feda and Mrs Leonard. The values of P given by W. W. C. are, of course, half those given above, *i.e.* ·37 and $< \cdot 015$.

There are few other points deserving notice in connection with the use of P as a criterion of significance, since they arise in various parts of W. W. C.'s work. First, although it is true that from such a value of z as the S measurement for Feda and Mrs Leonard given above, in which P is ·73, we cannot conclude that there is any similarity (either positive of negative) between the two personalities compared, it does not follow that no conclusion whatever can be drawn from this figure. Since the standard error is ·116, we can conclude that the true value of z for similarity lies somewhere between the observed value plus twice the standard error and the observed value minus twice the standard error. The value obtained, therefore, for z is definitely inconsistent with the true value of z being a positive value greater than about ·19 or a negative value beyond about $-{\cdot}27$. It frequently happens that what we want to know is not merely whether an observed value is or is not consistent with the hypothesis that the true value is zero, but also within what range the true value is likely to lie. For this reason, I think the standard error is more informative to the reader than is the value of P, and that W. W. C.'s tables would be clearer if standard errors were included.

Secondly, although we cannot conclude from the single S measurement from Feda and Mrs Leonard discussed above that there is between them any relation of similarity, either positive or negative, we might be able to draw such a conclusion for a series of such measurements even though in all of them the z was too small for significance. Let us suppose, for example, that this was one of a series of ten measurements of similarity of the same two personalities, all negative, all too small for significance, and all independent of one another (*i.e.* none are in any degree causally connected with any of the others). We could then obviously consider that if the values were randomly distributed about zero, the probability of all ten being of the same sign by chance would be $2 \times (\frac{1}{2})^{10}$ which is ·002, an amount which is undoubtedly significant. So long, therefore, as proper care is taken, perfectly valid conclusions can be drawn from combinations of data themselves separately not significant.

Thirdly, we have sometimes to decide what is the significance of a figure which is itself significant if considered separately but which is actually the best one of a number of observations of the same kind. Let us suppose, for example, that we had ten measurements of " similarity " for a single pair of personalities and that the best of these gave a P of ·01. If it were legitimate to treat this separately, we should say that the odds against it occurring by chance if there were no real similarity would be 100 to 1. This would be to ignore, however, the actual conditions of the observation. The probability of such a value occurring at least once in ten trials is not ·01 but ·095.[1] The odds against this occurring by

[1] Because if ·01 is the likelihood of the value turning up by chance in one trial, the likelihood of it not turning up by chance in one trial is ·99, and the likelihood of it turning up at least once in ten trials is, therefore, $1-({\cdot}99)^{10}n=$ ·095. Generally, if P is the probability for single value, the likelihood of a value equal to this or greater turning up at least once in n trials is $1-(1-\mathrm{P})^n$, which is approximately nP, if P is small.

chance are, therefore, only about 10 to 1, which is not enough for significance.

Before leaving the discussion of significance, it is perhaps worth while to consider what was wrong with W. W. C.'s now discarded attempt in TP 3 to combine independent estimates of significance by a method which resulted in a fantastic overestimate of significance.

The beginnings of this mistake are to be found much earlier than TP 3. In para. 26, in TP 1, W. W. C. considers two observations which have P's of ·005 and ·01 respectively. He wants to discover the value of P for the two observations taken together, *i.e.* to estimate how strong is the evidence of the combined fact of both observations for the hypothesis under consideration. There is a recognised and legitimate method of doing this (the negative logarithm method used later by W. W. C. in TP 3). If we could treat the two observations in question as independent, and if we could ignore the fact that they are the selected best from a number of observed values, then the negative logarithm method would tell us that the likelihood of two values at least as great as these arising by the chances of sampling from a population randomly distributed about zero is ·0005($\chi^2 = 19{\cdot}9$ with four degrees of freedom).

W. W. C., however, starts the argument by stating the meaning of P erroneously, and it is, I think, in this misstatement that the origin of the mistake lies. He says : " The chances of these being accidental were about 1 in 200 and 1 in 100 respectively." He then works out that the combined chance is about 1 in 19,000. There seems to be nothing wrong with this argument except the premiss. If the two P's were the probabilities that the two values were accidental, then the probability of both being accidental would be their product (about 1 in 20,000). The combined P would thus be ·00005 which is only one tenth of the value obtained by the proper method. Since a smaller P indicates a more certain result, this mistake in method leads to an overestimation of significance, which in other cases is greater and more serious than it is here.

W. W. C. also sometimes states the meaning of P correctly. In TP 1, p. 214, for example, he says that P " represents the chance of getting a z of this magnitude by accident ". This is quite correct, but it is a very different thing from saying that P is the probability that the z in question has been produced by accident (which is what he says in the passage quoted earlier) a misstatement which produces its most spectacular effects at the end of TP 3. I do not suppose that W. W. C. is the first to be guilty of the misstatement ; it could probably be paralleled in many accounts of statistical methods.

It is his misfortune that he has been clear-sighted enough to drive the misstatement to its logical conclusion and to derive from it a method of combining P's from different observations which would be a correct one if P were the likelihood of a given z having been produced by chance.

When we say that a particular value of z is significant because P is ·02, we mean that if the real value of z (*i.e.* the mean we should get if we could take an infinite sample of such z's) were zero, then the odds would be 50 to 1 against a value as large as the one observed having arisen by chance. This gives us a reasonable conviction that the observed z did not arise in this way. Let us be clear that this is not the same thing as saying that the odds are 50 to 1 against the observed z having arisen by chance.

We may suppose that I am waiting on the outskirts of Cambridge for a walker whom I have never seen before but who I know has slept the previous night at St. Ives (12 miles away). At 10 a.m. I see someone approaching along the road with a rucksack on his back. Taking into consideration the distance from St. Ives and the probable time of starting, I do not, however, bother to ask the man whether he is the one I am waiting for since I consider that if he had started from St. Ives that morning the odds would be (let us say) 100 to 1 against my expected friend having yet got so far. At 11.30, I see another man approaching on foot. It still seems rather unlikely that my friend would have got so far in such a short time, but now I estimate that the odds are only 3 to 1 against him having got so far (*i.e.* P=·25) so it seems reasonable to consider that it is sufficiently likely that the man is the one I am waiting for, for it to be worth while to ask him.

The above P is exactly analogous to the P calculated in an estimate of significance. Here it is the probability that if my friend had started from St. Ives that morning he could have got at least as far as this point by this time. In testing significance, P is the probability that if the true value of the quantity observed were zero, a deviation at least as big as the observed value might have occurred by the chances of sampling. The only essential difference between the two situations is that in testing significance we are generally interested in trying to establish not that the observed walker might have come from St. Ives but that there is no reasonable likelihood that he did.

The erroneous use of P which I am here criticising would be paralleled in our illustration by supposing that the judgment that there is a probability of ·25 that if my friend had started from St. Ives this morning he would have got as far as this, was equivalent to the quite different judgment that there is a probability of ·25 that the observed walker is my expected friend. Clearly this is a different judgment and any opinion on this probability would have to be founded on quite different data. The observed walker might have come from Huntingdon, from Godmanchester, from Fen Stanton or from some intermediate village. Other things being equal it is clearly more likely that any particular walker came from some less distant village than that he came from St. Ives. In fact we could not even form an estimate of the likelihood that it was from St. Ives that he came without knowing a great deal more about populations, relative distances, etc. We can only be sure that it is likely to be a figure very different from the ·25 calculated as the probability that, starting from St. Ives, he would have got at least as far as this.

We may take an even simpler illustration. A man is arrested and charged with murder. He pleads " not guilty ". The judge may consider that if he were guilty the odds would be 50 to 1 that he would say he was not guilty. This is plainly not the same as supposing that the fact that he pleads " not guilty " shows that the odds are 50 to 1 that he is really guilty.

I have dealt with this mistake at some length because it is an insidious one, and because, if this line of reasoning were admitted, all kinds of erroneous conclusions could be drawn from these papers and from most other statistical studies. I certainly do not wish it to be imagined that this criticism undermines the whole of W. W. C.'s work. It means simply that the arguments of TP 1, para. 26, of TP 2, p. 355, and of TP 3, paras. 32 and 33 must be rejected. None of these is of great importance, and the discarding of them leaves the remainder of the three papers intact.

VII. The Measurement of Individuality or Self-Consistency

We have already seen (in Section V) that W. W. C.'s measure of D is not simply a measure of " difference " but of " consistency of difference ". Although the way in which consistency affects the S measurement is less obvious, I think it is equally true to say that this too is not simply a measure of " similarity " but to some extent also of " consistency of similarity ". Let us remind ourselves of what S is. It is a measure of the degree to which the average responses to different words of two subjects were correlated (*i.e.* if one subject showed to a particular word a response which was large compared with his own average, the other subject also tended to show to the same word a large response compared with his average). I think it should be clear that if any personality was inconsistent on different occasions, showing a large response to one word on one occasion, but to quite different words on other occasions, there could be little that was characteristic of him either in his responses on any one occasion or in the average, which will simply be a blurred resultant of many different patterns of response. Nor can we reasonably expect either any single set of responses or the average of them to show close correlation with the pattern of response of any other personality. This, I think, becomes obvious if we reflect that if a second personality showed close " similarity " to any one set of the inconsistent personalities responses, he could not also to others. We cannot, in fact, expect to find that a personality is more " similar " to another personality than he is to himself.

This tendency of low self-consistency to produce low correlations is well known to educational psychologists who use the word " attenuation " to describe this effect.

We have the situation then that a low D measurement may mean small difference between the personalities compared or it may mean low self-consistency in one or both ; similarly a low S measurement may mean little similarity or it may mean low self-consistency. Actually, we are interested only in similarity and difference, and from the point of view of this enquiry, lack of self-consistency is simply a factor whose importance for us lies in the fact that it may obscure real relationships of similarity and difference.

We cannot interpret low S and D measurements, then, unless we have some way of determining the degree of self-consistency of the personalities compared. This is given to us in the measurement called " individuality " or " I " by W. W. C. I should have preferred that this should have been called (as W. W. C. also suggests) " self-consistency ".

This is a quantity obtained in the same way as the " difference " measurement from a table in which the items are simply the responses of one personality to the different words on different occasions. It may seem paradoxical that the method which was previously used to measure a difference between personalities is here used to measure the resemblance of a personality to himself. This, however, is an obvious consequence of the nature of the numerical items in the two cases. What was measured in the previous case was the extent to which the difference between the responses of two personalities on different occasions was consistently greater for some words and less for others ; what is measured in this case is the extent to which the responses of one personality on different occasions are consistently greater for some words than for others. We said that the " D " measurement indicated the " consistency of difference " between two personalities ; in the same way, we may say that the " I " measurement indicates the " consistency of individuality " of one personality. It measures how far this individual (to quote W. W. C.) " always gives a longer time to GOAT, say, than to PIG, and to PIG than to CAT, during the period covered by the tests ".

It will be seen from what has been said earlier that it is of some practical importance to have a measure of self consistency and I think that much more practical use might be made of it than W. W. C. has made. Let us suppose, for example, that we turn to the tables at the end of TP 2 in which a series of " S " and " D " measurements is given for different pairs of personalities in three experiments. If we ask ourselves which personalities resemble each other and which differ from each other, we shall be bewildered. The same pair of personalities seem to be differently related in different applications of the test, and many seem to have the meaningless relationship of being neither similar nor different.

It is only when we begin to take into account the " I " measurements that order begins to appear in the chaos. We can take, for example, the comparisons between Mrs Garrett and Uvani (reaction times) in Table I. For three of these (RN 30, 32 and 34), " I " measurements are given (RN 9 and 12, 10 and 13 and 11 and 14 respectively). The " similarity " measurements present no difficulty. All three are insignificantly small, so we may conclude that the test shows no appreciable resemblance between the two personalities. The " D " measurements are more difficult. The first is ·31 (fairly large and easily significant) the second is ·05 (insignificantly small) and the third is ·48 (even larger than the first). Are we to conclude that the two personalities do or do not differ? We shall probably all guess that they really do differ, but this is not a very satisfactory conclusion unless we have some ground for rejecting the middle value other than the fact that it does not agree with the other two. When we examine the " I " measurements for Mrs Garrett and Uvani we find that they are satisfactorily large for the first and third comparison (G=·25 and U=·30 for the first, and G=·55, U=·48 for the third). For the second comparisons, however, I is small for Mrs Garrett (·15) and practically zero for Uvani (−·07).

We may make a more general examination of how far this appearance of personalities being neither " similar " nor " different " is a result of low self-consistencies in the personalities compared. I have divided the " S " and " D " measurements in Table II (TP2) into two classes; those in which both of the personalities compared have " I " measurements of ·2 or over, and those in which one or both " I " measurements fall below that value. The first class is thus composed of personalities with good self-consistency, the other has one or both personalities with bad self-consistency. The comparisons in both classes are then grouped as showing significant similarity and no difference (S+Do), significant difference and no similarity (So D+), and so on. The results for the whole of W. W. C.'s Table II are as follows: [1]

TABLE IV

	(1) S+Do	(2) SoD+ or S−D+	(3) So Do	(4) S+D+	Totals
Comparisons between personalities with " good " self-consistency - - -	3	17	0	4	24
Other comparisons	11	9	13	1	34

There are two striking features of this table. First, the much greater proportion of comparisons showing significant difference in those calculated from personalities with good self-consistency (21 out of 24 as against 10 out of 34), suggesting that a principal effect of lack of self-consistency is to obscure differences that really exist. Secondly, there is a complete disappearance in the comparisons with good " self-consistency " of the anomalous indication that two personalities are neither " similar " nor " different ".

This confirms the opinion previously suggested that these cases do not indicate a relationship of " neutrality " between the personalities who neither resemble each other nor differ from each other, but simply that lack of self-consistency prevents us from measuring any real similarity that exists. Our " S " and " D " measurements are quite correct in their indications that the two personalities show neither " consistent similarity " nor " consistent difference ", and only become misleading if we equate these with " similarity " and " difference ".

There may, at first sight, appear to be inconsistency in the indications of column 4 where the same personalities appear to be both

[1] When this table was made, I did not realise that all W. W. C.'s P's had been divided by 2, so I took as my criterion of significance that P (W. W. C.'s value) should be ·05 or over.

similar and different. This, however, is altogether reasonable. If two personalities are not completely similar, they will be more or less different. So it is to be expected that a measure of similarity together with a measure of difference will be found in all comparisons, with a tendency for the difference to be greater if the similarity is less and *vice versa*. It may generally happen that only " S " or only " D " will be significant, but there is no reason why both should not be.

It would be interesting also to enquire whether, if only data of good self-consistency were used, inconsistency of indications between different experiments would be reduced or disappear. This probably would happen, but there are not enough comparisons of the same pair of personalities who have both been self-consistent in more than one experiment for a test to be possible.

The suggestion I wish to make here, is that if we require " S " and " D " measurements as indications of the relationships of similarity and difference between personalities, they should be calculated from test results which show a reasonably high self-consistency or " I " measurement, because only then will " S " and " D " indicate adequately similarity and difference respectively. I think the " I " measurements might also be used to determine the satisfactoriness of different methods of scoring. Many of the results given in TP 1 become very different when rescored for TP 2. I have no doubt that the later methods of scoring were better and that the results indicated by them are more reliable than the earlier ones. I suggest, however, that this superiority might best be shown by finding that the improved methods of scoring gave increased " I " scores when they were used.

What has here been said is not meant as a criticism of the work of W. W. C. It is, on the contrary, a suggestion that his results are much more informative and internally consistent than they appear at first sight. There is another way in which it might be possible to deal with the difficulties I have here discussed. I have explored only the way in which the results could be improved by eliminating those calculated from personalities showing low " I " measurements. Such a procedure would involve sacrificing a lot of the data. It would be better, if possible, to calculate indices of " S " and " D " which, in some way removed the effect of low self-consistency. This would be equivalent to the practice in educational psychology of " correcting for attenuation ". Whether this could be done and how it could be done, I must leave to the statisticians. If it were practicable, it would plainly be better than jettisoning all the data calculated from personalities with low " I ", but until it can be done, I think the sacrifice of even two-thirds of the data would be worth while, if it left (as it would) much more clear indications in what remained.

VIII. Countersimilarity of Controls

A secondary object of Mr Whately Carington's research was to throw light on the psychology of the trance condition. Outstandingly the most important of these is his study of the relationship of the controls to the medium and to other personalities in the same manifold.

In measuring a correlation, as in W. W. C.'s measurements, we may find a positive correlation indicating similarity of the two things measured, no correlation at all indicating absence of similarity, or a negative correlation indicating a relationship which W. W. C. calls " countersimilarity ". Similarity is the relationship between two faces resembling each other in shape ; countersimilarity may be expressed as the relationship between a human face and a hollow cast of a face. Countersimilarity (*i.e.* what is indicated by a significant negative z) is, as W. W. C. truly points out (TP 2, p. 333), no less evidence of some causal connection between the reactions of the two personalities compared than is a positive similarity.

In TP 2, pp. 329 ff., an argument is developed that controls show the relationship of countersimilarity to other personalities in the psychological manifold to which they belong whereas communicators do not. From this fact is drawn the important conclusion that controls are parts of the medium's personality split off by repression and not autonomous personalities.

Let us begin by making a general survey of the evidence on which this conclusion is based. First, that of Feda the control of Mrs Leonard. There is no clear evidence of countersimilarity in the Feda " similarity " measurements published in TP 1 (pp. 200 ff.). These results are recomputed for TP 2 by a better method and examination of the more extensive observations published in Table II of TP 2, suggests at once that there is a tendency for Feda to show negative z's for similarity in the reaction time results although not in those for reproduction. It is, in fact, entirely from the reaction time results that W. W. C.'s argument is drawn.

The fact that Feda's countersimilarity does not appear in the material published in TP 1 need not bother us. The recomputation for TP 2 was by a better method (not, of course, devised to demonstrate countersimilarity, but for other reasons) and it may easily happen that a genuine relationship is obscured by an inferior method of scoring.[1]

Nor need we worry about the fact that the relationship does not appear in " reproduction " results. If a relationship is significantly shown by any one set of results, then that relationship really exists and the mere fact that other sets of results do not show it is not, in itself, reason for supposing that it does not exist. Actually there are two possible reasons for the failure of the reproduction results to show the countersimilarity shown by the reaction time results. First, it may be, as W. W. C. suggests elsewhere (TP 2, p. 345), that the RPN results are measuring something different, and, secondly, it might be due to the general untrustworthiness of the indications of RPN data (see Section III of this paper).

Further examination of Feda's negative similarities (in Table II, TP 2) shows that, with one exception (RN 116) they are insignificantly small. Although this means that we cannot draw any conclusion as to the reality of countersimilarity from any one of these insignificant values taken separately, it does not necessarily mean that we cannot do so by taking them together. As has already been pointed out (in Section VI), a number of independent indications separately insignificant may, as a whole, give a significant indication.

Let us now turn to W. W. C.'s arguments in TP 2. Sections 12 to 17 are more or less preliminary. It is in sections 18 ff. that the important part of the demonstration is to be found. I think that W. W. C. in his use of statistical analysis in these sections lays himself open to charges of over-optimism and of insufficient use of statistical methods as a critical check on this tendency. These also seem to me to be the main faults in his use of statistics where he went wrong at the end of TP 3. I think it is unfortunate here that a tendency to over-state his case may obscure the fact that there is quite a strong case anyway.

W. W. C. begins by pointing out (TP 2, p. 334) that in the reaction time " similarities ", Feda gives negative values proportionately more often than do other personalities. This certainly is the case as is seen in the following table from this page :

	Positive	Negative
Feda comparisons - - -	2	8
Other comparisons - - -	11	1

[1] It is, nevertheless, rather awkward that Feda and Mrs Leonard (prepared) show a significant positive similarity on p. 201 of TP 1 ($z = +\cdot 2740$, $P = \cdot 018$) which becomes insignificant in Table II of TP 2 ($z = +\cdot 1471$, $P = \cdot 20$). It is less easy to understand how an inferior method of computation can make a really insignificant indication of relationship into a significant one.

It is obvious that in this table, the Feda comparisons show the greater proportion of negative " similarities ". As always, we must ask whether this indication is significant, *i.e.* whether the disproportion is so great as to render it unreasonable to suppose that it might be due to chance. W. W. C. does this by the standard method of the text-books for this type of problem (in which a quantity called χ^2 is calculated) and shows that it is unquestionably significant.

Undoubtedly it is, but we must also ask: what exactly does it signify? Plainly it does not signify what is here required for the argument—that Feda has a real tendency to be countersimilar to other personalities. Negative values of z might arise in two ways, either from a real countersimilarity of Feda to other personalities, or by the fact that she had zero real similarity to other personalities so that negative values were as likely as positive. In fact, Feda's " S " values do tend to be much lower than the others so the mere fact that she gives more negative values is consistent with the second hypothesis and does not necessitate any real countersimilarity.

It may be objected, however, that this is proved by the fact that Feda gives more negative than positive values. This certainly is an argument in favour of countersimilarity, but it is a different argument. The important question is not whether she shows more negative values than the other personalities but whether she shows more than are to be expected on the hypothesis that her z values are normally distributed about zero. The table should be:

	$+ve$	$-ve$
Observed Feda comparisons - - - - -	2	8
Expected Feda comparisons on hypothesis of zero similarity - - - - - - -	5	5

The inequality is not significant but this may be because the total number of F comparisons is small. Let us, therefore, add together the Feda and the Uvani comparisons and treat in the same way.

We get:

	$+ve$	$-ve$
Observed comparisons involving controls -	3	12
Expected observed comparisons involving controls on hypothesis of zero similarity - -	$7\frac{1}{2}$	$7\frac{1}{2}$

Applying the χ^2 test for significance (with Yates's correction for small numbers), we find that χ^2 is 4·26 which makes P between ·02 and ·05. This is sufficient for significance.

This would be valid evidence for a real tendency to countersimilarity if we were justified in treating all the comparisons as independent. I do not think we are. If they are not independent, the significance of the results will be much less than the above calculation would lead us to suppose. Since questions of what items of evidence can be regarded as independent occur several times in these papers, it may be worth while to consider shortly the problem of what constitutes independent evidence.

Let us suppose that we toss a penny fifteen times and that tails turn up twelve times, the above calculation would tell us how unlikely it is that this event would happen by chance. Suppose, however, that (still wishing to discover whether the coin is biassed) we recorded as separate events the fact that the figure of Britannia was uppermost, that the trident was uppermost and that the date was uppermost. In five tosses we found that each of these three events happened four times and failed to happen once. If we added these together and said that our hypothesis of bias had been confirmed twelve times out of fifteen, we should be guilty of an obvious fallacy. Actually only five tests have been made (not fifteen) and the results have been four of one kind and one of the other (a disproportion which is not significant since it could very easily have happened by chance). The fallacy would be that of treating as independent, items of evidence that are not independent.

In such a case as this, where the interdependence of the events tested is complete, no one is likely to make a mistake. There is another more difficult case where the occurrence of one event only makes more probable the occurrence of another and does not make it certain. We may suppose that we are tossing several mutilated pennies some of which have a date but no trident, others have a trident and no date, while others have both trident and date. Then the turning up of the trident makes the turning up of a date more probable but not certain, and *vice versa*. We still should be in error if we treated the turning up of the trident and of the date as independent events, for the turning up of the trident a large number of times by chance would make more likely the turning up of the date. The significance of any observed concurrence of these events would be greatly exaggerated if we treated them as independent. If, moreover, we were in the position of not knowing how many of our coins were mutilated (and, therefore, how intimately these events were causally connected) we should be unable to estimate how much the significance of the results had been overestimated by treating them as independent.

It seems to me that the different " S " comparisons of the same personality in any one series of experiments are not independent for much the same reason that the turning up of the trident and the date in the last example are not independent. It must be remembered that all of these " S " comparisons are made with the same set of Feda measurements. It may happen by chance that in any single experiment, Feda produced a set of reaction times countersimilar to some other personality. This set will then, to some extent, tend to be countersimilar to any third personality positively similar to the first one (just as a hollow cast countersimilar to W. W. C.'s face will also tend to be countersimilar to mine). All the *-ve* Feda comparisons in the first Thomas experiment are truly independent pieces of evidence that this particular set of Feda measurements shows a tendency to " countersimilarity " but not that Feda measurements in general show countersimilarity. For that proposition they are, taken together, one piece of evidence. Another piece of evidence is provided by the results of the second Thomas experiment and another by the Irving experiment. In exactly the same way these three experiments are three independent pieces of evidence that Feda shows countersimilarity but are only one piece of evidence for the proposition that controls in general show countersimilarity.

Nor do I think that it is justifiable to treat (as W. W. C. does) reaction time results and reproduction results as independent. In general these are correlated (as W. W. C. has himself shown) and, therefore, a countersimilarity that appears in one will tend also to appear as a dependent fact in the other.

I do not, of course, know how much our previous estimate of significance would be affected by taking into account these interdependences of the Feda comparisons. There was not a very large margin of significance to begin with, and, since we know that the significance must have been overestimated to an unknown extent, it seems safer to reject altogether the evidence for the countersimilarity of Feda and Uvani got by combining the values separately insignificant, and to enquire whether we can get satisfactory evidence from values which are large enough to be significant.

For Feda, there is only one such value, the " similarity " of Feda and Leonard (prepared) in the Irving experiment (No. 116 of Table II, TP 2). Here z is $-\cdot 3310$, with a standard error of $\cdot 1$, so the value of P is less than $\cdot 001$. This value of z is unquestionably significant. It is very improbable that it arose as a chance deviation from zero. It is true that this is only one of many z's calculated for Feda and the chance of such a value arising at least once in several tests is larger than that of it occurring in a single trial. Even, however, if we multiply by 10 to get the chance of such a value occurring at least once in the ten reaction time comparisons of Feda, P would still be less than $\cdot 01$ and would remain significant. We may note also that the fact that we have treated as independent comparisons which really are not independent, will, in this case, cause us to underestimate the significance, so P must be considerably less than ·01 and undoubtedly significant.

We must now consider how this indication fits in with other indications in the same table. We may probably disregard the fact that the reproduction test on the same material makes Feda and Mrs Leonard (prepared) significantly similar. If, of course, two apparently reliable lines of evidence were to tell us, one that Feda and Leonard (prepared) are similar and the other that they are countersimilar, it would be impossible to draw any conclusion on this point; we could only conclude that there was something wrong with the evidence. We have, however, seen earlier that there may well be sufficient reason for accepting the testimony of the " reaction time " date without expecting that of the " reproduction " date to be consistent with it.

This, however, leaves the same difficulty at a different point. The RT similarity between Feda and Leonard (prepared) is insignificant in both Thomas experiments, insignificantly $+ve$ in one ($z = +\cdot 1471$), and insignificantly $-ve$ in the other ($z = -\cdot 1478$). This does not merely mean that these experiments do not confirm the conclusion of the Irving experiment on this matter; they are definitely inconsistent with it. As has already been pointed out, the fact that a value of z is insignificantly small does not mean only that we cannot conclude that there is any relationship between the items compared; it also means that this relationship, if it exists, cannot exceed a certain positive value. The testimony of the two Thomas experiments is that the countersimilarity of Feda and Leonard (prepared), if it exists, cannot be of the size indicated by the Irving experiment, no less definitely than the Irving experiment indicates that it cannot be zero. There is a clash of testimony which must leave us uncertain which to accept unless we can show reason why one line of evidence is of less value than the other.

I think there is such a reason. If we look at the " Individuality " or self-consistency measurements for Leonard (prepared) in the Thomas experiments, we see that both are negative. It is obviously improbable that we shall find any relationship with another set of figures, from a set of figures which is a mean of results which are negatively self-consistent with one another. A true countersimilarity between Feda and Leonard (prepared) might therefore have been obscured by the negative self-consistency of Leonard (prepared) in these experiments. The Irving experiment, in this respect, is in a somewhat (but not very much) better position. The " I " measurement for Leonard prepared is $+\cdot 0306$. This is positive but very small. It is indeed surprising that such large measures of S and of D as are found in RN 116 could have been obtained from data with so little self-consistency. Since, however, the low self-consistency would have tended to make these both nearer to zero, it is no reason for rejecting their indications as it was in the two Thomas experiments.

There appears then to be sufficient evidence that in reaction time measurements, Feda and Mrs Leonard (prepared) are countersimilar. The evidence here rests on one observation (that of the Irving experiment) and depends on the validity of the reasons I have suggested for rejecting opposing evidence from other experiments. This conclusion is confirmed by the observation made in TP 3 that Feda is again countersimilar to Mrs Leonard (p. 194 and Table II). The P here is ·05 which taken by itself is significant but unimpressive, but taken in conjunction with the results in TP 2 is very strong evidence of the reality of the relation of countersimilarity. In the reproduction results, Feda is again not countersimilar to Mrs Leonard as she was not in the Irving experiment of TP 2.

The evidence from Mrs Garrett and her control Uvani (Table I, TP 2), mostly calculated from Mr Hereward Carrington's data, is of the same order and points in the same direction. There are five P.G.R. measurements of similarity between Mrs Garrett (G) and Uvani (U) and five reaction time ones. Of these, three PGR and four RT similarities are $-ve$. One PGR negative " S " is significant (No. 19, $z = -·3079$, $P = ·002$), one RT is nearly significant (No. 31, $z = -·3083$, $P = 06$). No positive " S " measurements between G and U are significant. The evidence here seems to me to be at least as good as that derived from Mrs Leonard and Feda.

On the whole, it seems reasonable to conclude that there is a genuine relationship of countersimilarity between Feda and Mrs Leonard and between Uvani and Mrs Garrett. It is a pity that this important conclusion could not have been established in a more convincing way from data freer from inconsistencies. Even as it stands, however, I think the evidence is good enough for reasonably strong conviction. Also it seems reasonable to accept W. W. C.'s explanation of this fact that these controls are not independent personalities but are dissociated parts of the medium's personality (or secondary personalities of the medium).[1]

[1] There is also an argument in TP 3, p. 198, against the autonomy of controls drawn from the fact that there is a very strong resemblance between the controls Feda and Silver, which seems more likely to be explained by the fact that the two mediums have split off similar secondaries than to a real resemblance between a Hindu girl and a Red Indian warrior.

Whether or not this relationship holds generally for all mediums and their controls could not, of course, be decided without investigating a greater number. W. W. C. seems to treat this result as following from the countersimilarity of medium and control in these two cases. The fact of two controls being countersimilar to the mediums cannot, however, be proof that all are. The fact that Rudi Schneider and his control Olga showed, on the contrary, a strong similarity (TP 1, pp. 187 ff.) need not be considered an exception to the rule of countersimilarity since it is apparent that there were other grounds for not regarding Olga as a control. There is, however, a real exception in Mrs Sharplin and her control Silver (TP 3, p. 194 and Table II RN 3023) who show no significant similarity or countersimilarity. If, in the future, it is shown that countersimilarity is a general rule amongst controls, a satisfactory explanation of this exception may be found. So long, however, as the generality of the countersimilarity of controls is in question, Silver must be treated as an exception weakening the evidence for this conclusion. Whether or not the relationship of countersimilarity is a general characteristic of controls (or of controls of this type) can only be decided by further research on larger numbers. In view of the interest of this question, it is to be hoped that this further research will be carried out.

I do not think it is possible to admit any logical force to the argument on p. 348 of TP 2 that such communicators as John, Etta and Dora are not secondary personalities because they do not show countersimilarity. Even if countersimilarity were established as a general property of controls, it would not therefore be shown to be a general property of all secondary personalities. We cannot argue that because two monkeys are found to have tails (or more precisely because two out of three animals claimed as monkeys have tails) that therefore any animal without a tail cannot be a monkey.

There is another unfortunate argument on this page. Against the suggestion that communicators are histrionic poses, W. W. C. says that they do not show the association of reaction time and reproduction which seems to be the hall mark of a single and undivided personality and is shown by the two Gatty poses. W. W. C. has failed to notice that in one of the Gatty poses (Gatty, Oxford) on p. 344, the association between reaction time and reproduction is strongly negative. Even if the Gatty figures are a misprint (as seems likely), the statement that communicators do not show association between RT and RPN is based on two only out of three communicators (the third does show such association).

Obviously the number is far too small to support the general conclusion that communicators do not show association between RT and RPN. Moreover, the test of significance on p. 345 of TP 2 is erroneous because W. W. C. has treated separate tests of the same communicator as independent pieces of evidence, and also because he has used a method of calculation which is generally agreed to be invalid if the number of cases expected in any class is less than five.

The conclusions that I think may safely be drawn are these: there is evidence of sufficient but not of coercive strength that the controls Feda and Uvani are negatively correlated to the mediums and to other personalities of the same manifold; this may be most easily explained by supposing that they are dissociated parts of the medium's personality (for which there is other evidence); whether this relationship of countersimilarity is true for all controls is a problem for future research; there is no evidence of this relationship of countersimilarity being found amongst any of the trance personalities that are not controls.

IX. Intermedium Work

In the last section we saw that a very strong presumption had been established in TP 2 as to certain of the controls. They appeared not to be autonomous personalities but secondary personalities of the medium. This, I think, is the most important positive conclusion from Mr Whately Carington's research. The problem of the status of communicators was, at this stage, not going on so well. If we look at the relationships of similarity and difference listed in Table II of TP 2, we see an impressive array of figures but little indication of what relationships they are pointing to amongst the personalities compared. It is certainly true that, once we admit the force of the argument at the beginning of TP 2, we can no longer hope to be able to settle the problem of the autonomy of communicators by any mere examination of coefficients of "similarity" and "difference". We might, I think, still have hoped that these would give us strong indications of how the personalities in a manifold were related. It does not appear that they do. Even if they had done so, it would be necessary, however, to look in a different direction for final proof. It is with the devising and carrying out of better methods for investigating the problem of the autonomy of communicators that TP 2, pt. IV, and TP 3 are mainly concerned.

The first venture in this direction in TP 3 need not delay us since the author discarded the method immediately afterwards. The method here employed is to obtain readings for the word-association experiment from the communicators John and Etta through another medium (Mrs Sharplin) and to compare these with the reactions of the same communicators when communicating through Mrs Leonard. If the communicators could be proved to be identical with the same communicator communicating through another medium, their autonomy would be proved. This intermedium comparison of communicating personalities who are ostensibly the same has remained the basic method of all the later part of W. W. C.'s work although the method of making the comparison has been much improved since the first crude attempts of TP 2.

The distinctive feature of the now discarded method of TP 2 is that (realising that a simple similarity between, let us say, John communicating through Mrs Leonard and through Mrs Sharplin might be due to similarity of these two mediums) the attempt was made to get rid of the effect of the similarity of the mediums by the method of " partial correlation ", *i.e.* to get a measure of how much John (through Leonard) resembled John (through Sharplin) when the effect of the similarity of the mediums was eliminated.

W. W. C. thought, at the time of publishing TP 2, that the indications of autonomy of John and Etta were positive but he was wisely suspicious of an indication obtained so easily and published the results with reserve, suggesting that it might prove that there was a flaw in the argument.

In actual fact the final similarities shown on p. 355 of TP 2 are not significant either separately or together. W. W. C. was only misled into thinking they were significant by repeating the mistake (already discussed in Section VI) of multiplying together their separate probabilities in order to get their combined probabilities, a method which (as we have seen) enormously overestimates the significance. Here he gets a combined P of ·0014 (instead of the true value of ·1). The matter is not important since W. W. C. had realised by the time TP 3 was published that the results would have proved nothing about the autonomy of communicators if they had been significant.

The method was rejected at the beginning of TP 3 as a result of Professor Fisher's criticism that the partial correlation method was not to be relied on when the eliminated variable was itself (as here) subject to error. I think that there is a more fundamental objection than this to the method as used in TP 2. If the partial correlation method had been applicable to these data and if its results had been significant they would have shown only that J_S resembled J_L and that E_S resembled E_L more than could be accounted for by the resemblance of the mediums. But, as we have already seen, any two sets of responses to the word-association test may resemble one another more or less whether or not they are produced by the same personality, and this possible cause of similarity is not eliminated by the partial correlation method. It would be necessary also to show that J_L resembled J_S and that E_L resembled E_S significantly more than J_L resembled E_S and than J_S resembled E_L.[1] Unless this were done, the partial correlation method could not have proved the autonomy of J and E ; if it were done, it seems to me that the partial correlation method would be unnecessary since the same pair of mediums would have been used in all four comparisons.

The next method devised by W. W. C. is not open to this objection. It is, in fact, a method very close to that suggested above but is neater since coefficients of " similarity " are not calculated but the required comparison is calculated by the method of analysis of variance direct from the table of word-association responses. It is, I think, fundamentally sound, and its devising is a feat very creditable to Mr Whately Carington. That much of the superstructure of TP 3 is worthless does not alter the fact of the essential soundness of the foundations.

If we agree that mere resemblance, however great, cannot be proof of identity, I think there should be no difficulty in seeing what kind of evidence would prove identity. Perhaps we can state it most simply as follows : we must prove not merely that a given communicator is like himself but that he is more like himself than he would be if he were not himself. More exactly, we must be able to show whether or not X_A (communicator X communicating through medium A) is more like X_B than he would be if the two X's were not the same personality in the two cases. W. W. C.'s third article is devoted to discovering whether or not this can be demonstrated.

If the problem as above stated is borne in mind, the general nature of this part of the enquiry should be clear even though the details may remain obscure. Let us begin by considering an analogous case in which no spirits enter. Let us suppose that I received two years ago a letter from someone who knew me in the past and now needs my help. Let us further suppose that he is an illiterate man who has not composed the letter himself, but has given a secretary the general idea of what he wanted to write and has left

[1] Here and hereafter, I am using J_S as a convenient contraction for John communicating through Mrs Sharplin, J_L for John communicating through Mrs Leonard, and E_S and E_L similarly for Etta communicating respectively through the same two mediums.

her the task of expressing it in her own words. Let us suppose further that I received yesterday a letter ostensibly from the same man but composed by a different secretary. I have some reason for suspecting that the writer of the second letter is an impostor, and is not really the same person as the writer of two years ago. How am I to establish whether or not the two letters have identical originators?

Naturally, the two letters will not be identical; they may not even be closely similar. Each of them has many individual peculiarities which reflect the personality of the secretary and not of the originator. On the other hand, they are not likely to be wholly dissimilar. Any two human beings in similar circumstances may give somewhat similar instructions to the person who is writing a letter for them, and any two secretaries writing letters under similar circumstances may be expected to compose somewhat similar letters. With this amount of data, indeed, the problem will be insoluble. Even though I can measure how much the two letters resemble each other, I cannot draw any conclusion as to the identity or non-identity of the originators, since I do not know how much the two letters might differ although the originator of both was one and the same person, nor do I know how much the two letters might be expected to resemble each other although the originators of them were different.

If, however, I were so fortunate as to have, written by the same two secretaries, two more letters written in similar circumstances but not written by the alleged individual of whose self-identity I am in doubt, I should have the data for the solution of my problem. In order that the problem may be as closely as possible analogous to that of W. W. C.'s research, we may suppose that the second letter of each secretary was written at the same time as the first, and that I am as doubtful about the common authorship of this pair of letters as I am about the common authorship of the other pair.

Let us call the two secretaries A and B, and the two alleged authors (who may prove to be four), X and Y. I want to know whether X_A is the same individual as X_B and whether Y_A is the same as Y_B. Comparison of X_A with Y_B and of X_B with Y_A will tell me both how much the style of the two secretaries has in common and how much two letters written by different individuals in the same circumstances have in common. This knowledge will enable me to allow both for the factor of the resemblance of secretaries and for the common human factor. If now I find that the letter of X_A resembles that of X_B strikingly more than the pair of comparisons I made previously, I may conclude that X_A and X_B are one and the same person ; similarly for Y_A and Y_B.

I have so far supposed that the problem of the authorship of the letters was decided qualitatively, by my mere impression of the degree of resemblance of the different pairs of letters. Such impressions, however, are unreliable, and, as a scientist, I may prefer to get some quantitative measure of the characters of the letters. I might get this by counting some characteristic features, the number of unusual grammatical constructions, the proportion of compound to simple sentences, etc. I should then have a set of numbers to compare and need rely no longer on my own impressions. Then I should be driven to judge the question by a method which was equivalent to that used by W. W. C. or was identically the same.

Essentially the method is quite general in its nature. The above example shows that it could be applied to problems outside the special field of psychical research. It seems to me that it might be used, for example, in a purely agricultural problem, to test, let us say, whether two bags of different kinds of artificial manure bought at one shop were identical in composition with two bags ostensibly of the same kind as the first pair but bought at a different shop. It is an advantage of a quantitative statistical enquiry that the method of proving the self-identity of bags of manure is the same as that of proving the self-identity of spirits. The criteria of validity are independent of the emotional importance to the enquirer of the subject of research.

There is one obvious limitation to the validity of this test. Let us suppose that in our analogy of the letter writers, one claimed to be a man I had once employed as a gardener who was asking me for a loan, while the other claimed to be a woman asking me for a subscription for the R.S.P.C.A. There might well be a great deal of resemblance between X_A and X_B and between Y_A and Y_B that did not exist between X_A and Y_B or between X_B and Y_A even though neither the two X's nor the two Y's were really the same person. They would be playing the same rôle and would therefore tend to resemble each other in superficial respects. Both X's might talk about apple-trees and both Y's about homeless dogs. For that reason it would be necessary to make the test for self-identity on some characteristics such as grammatical constructions, complexity of sentences, which were not affected by the rôle.

Similarly, W. W. C.'s test would not be valid if applied to similar superficial characteristics of responses which might be characteristic of any medium (or secondary personality of a medium) playing the part of a clergyman or of a young woman. The actual words used as responses might, for example, show theological interests when J (a clergyman) was communicating through two mediums, even if there were no real J behind the communications. It is very unlikely that any similar spurious indication of self-identity would vitiate the test when reaction times are used because the medium (or trance personality of the medium) would have no theory as to what would be the characteristic reaction times of a clergyman or even indeed any knowledge of the fact that reaction times are affected by dominant emotional interests.

It is not particularly easy to explain simply the method employed in TP 3. Essentially it is a form of the analysis of variance which has already been described in Section V. Each occasion is, however, treated separately. The rows are, as before, formed of responses each to a particular word in the word association list. There are four columns of the responses of John through Mrs Leonard, of Etta through Mrs Leonard, of John through Mrs Sharplin and of Etta through Mrs Sharplin. The table for the analysis of variance is, therefore, as below :

		J_L	E_L	J_S	E_S
Word 1	-	a_1	b_1	c_1	d_1
Word 2	-	a_2	b_2	c_2	d_2
Word 3	-	a_3	b_3	c_3	d_3
etc.					

The entries a_1, b_1, c_1, etc., are the usual quantitative expressions of the word-association responses made by the personality entered at the head of the column to the particular word of that row (thus in the reaction time data they are the times in seconds that the personality has taken to react). As before, the problem is to discover how much of the scatter of these responses is contributed by different factors. In this case, the factor we are interested in is the respect in which J responses differ from E responses. The actual arithmetical operations are explained very clearly by W. W. C. in TP 3, pp. 202-210. Those who wish to understand the method cannot do better than work out for themselves the examples he gives.

We can, I think, best express in words what each z is by saying that it is a measure of similarity (as was the earlier " S " measurement). Briefly, it measures the extent to which each ostensible communicator is more similar to himself (through another medium) than to the other communicator. More exactly, it shows how far the similarity between J_L and J_S and between E_L and E_S is greater than that between J_L and E_S and between J_S and E_L. If all four comparisons : between J_L and J_S, between E_L and E_S, between J_L and E_S, and between E_L and J_S showed differences which were about the same, z would be zero or only insignificantly different from zero. Nothing would appear to belong specifically to J or to E. If, however, the first two of these comparisons showed consistently less difference than the last two, z would be positive, and this would indicate that ostensibly the same communicator, even communicating through two different mediums, showed a degree of resemblance that could only be explained if he really was the same communicator.

A separate z is worked out for each occasion and for each block of twenty-five words. Thus in the comparison between the Leonard and Sharplin material of 1935, where there are five occasions and four blocks of twenty-five words, there are twenty z's for the reaction time data and twenty for the reproduction material. All the z's of all comparisons are given in Table V at the end of TP 3.

The procedure has, so far, been statistically correct, and, as an attempt to devise a satisfactory method of investigating the problem, it deserves high praise. Two defects of the data have already been mentioned in Section III. These are: the inclusion of two sets of observations from the same medium obtained at different times, and the use of " reproduction " material which may not be sufficiently nearly normally distributed to give reliable results. These, however, are not defects of the method.

The result to be expected from this experiment, if John and Etta communicating through Mrs Leonard are identifiably the same John and Etta as communicate through Mrs Sharplin, is that the z's obtained at the end should be significantly positive. If responses ostensibly from the same communicator through different mediums have no more in common than have responses from different communicators, then the z's will be randomly distributed about zero. No reasonable meaning can, I think, be attached to negative z's. They would suggest that responses ostensibly from the same communicator through different mediums have less in common than they would have if they were from different communicators. This does not seem reasonable on any hypothesis.

The question of the autonomy of communicators is, therefore, to be decided by finding out whether the z's tend to be significantly positive or whether, on the other hand, they tend to be randomly distributed about zero. If the first alternative is true, the autonomy of communicators is indicated; if the second, then the experiment provides no evidence for the autonomy of communicators.

Which of these two expectations is fulfilled is decisively shown in para. 25. The mean value of the z's is $+\cdot035$ with standard error of $\cdot041$. The likelihood of such a deviation from zero occurring by chance in a set of values randomly distributed about zero is ·4, that is, it is likely to happen nearly as often as not in a series of tests. There is no evidence here that there is any tendency of the z's to differ from zero.

It is true that W. W. C. mentions that one of the sets of " reproduction " z's has a mean which differs significantly from zero. This is the mean of the twenty z's obtained by the comparison of communications through Mrs Leonard and those through Mrs Sharplin in 1935. The mean value is +·218 with standard error ·091. If this were taken as an isolated value, P would be ·017 making it easily significant. It is, actually, the selected best of six means, and the probability of at least one value as high as this occurring as a random deviate from zero in six trials is about ·1. This is not enough for significance, but if there were no grounds for doubting the reliability of RPN results (as there were none when W. W. C. published this paper), it would make us hesitate to accept the otherwise clear negative indication of the results. Since, however, there is grave reason for doubting the reliability of RPN evidence (see Section III) and since the RT results for the corresponding comparison are completely insignificant (−·003 with S.E. ·091), there seems no reason for trying to draw any conclusion from this one unexpectedly high mean.

It says much for the honesty of the investigator that he presents so clearly such strong evidence against the hypothesis which, at the time of writing, he believed that his investigation supported. Unfortunately, he did not stop at this point but went on to an ingenious but mistaken attempt to wrest from the data a significance which they do not possess. I do not want to dwell too long on the errors of TP 3 (sections 27 onwards) since these are now recognised by W. W. C. It is necessary, however, I think, to indicate them shortly to prevent others from being misled by them.

We need not trouble much about the problem dealt with in para. 27 (the excess of $-ve$ z's in the comparison between the two sets of data obtained from Mrs Leonard in different years), since it is not relevant to the hypothesis under consideration. Significantly negative z's could (as we have seen) only mean that responses ostensibly from the same communicator have less in common than they would have if they were from different communicators. This is an unintelligible result in any case, and is certainly no easier to explain on the hypothesis that the John and the Etta of 1933 are the same personalities respectively as the John and the Etta of 1935, than on the hypothesis that they are not.

In para. 28, W. W. C. reconsiders the means of the z's obtained in his six sets of comparisons. These, certainly, are data relevant to the hypothesis under investigation, but they have already been treated as a whole and appeared then to be randomly distributed. They still remain random (with the possible exception dealt with above) when split up. W. W. C. obtains a combined P of ·15, which he says is suggestive but not coercive. Actually the P is somewhat overestimated by this method since it treats $+ve$ and $-ve$ deviations as equally providing support for the hypothesis tested, whereas, as we have seen, only $+ve$ deviations are evidence for the autonomy of personalities. A P of ·15 is not, however, even suggestive. A total irregularity that could occur once in seven times by the chances of sampling is quite consistent with the hypothesis that all the means are random deviates from zero.

In para. 29, W. W. C. calculates the " slopes " of the six sets of values, *i.e.* the rate of change of the mean for all occasions from block to block. Some of these are fairly large and the total irregularity would appear to be just significant if we treated the " reaction time " and the " reproduction " results as independent (which does not appear to be justifiable). Also in assessing the significance of these values, it must be borne in mind that they are only one of many characteristics of the data that might have been calculated (*e.g.* the means of the blocks, the slopes showing the rate of change with occasion, etc.) and the likelihood that at least one of a set of characteristics will show a given deviation from expectation is considerably greater than would be its likelihood if it were the one appropriate characteristic to measure for the testing of the hypothesis in question. If the slopes were significant, it does not seem that they would indicate the autonomy of John and Etta and I find it difficult to see what they would indicate.

This is also true of the correlation between slopes and means worked out in para. 30 which seems to indicate a tendency for " whatever is going on to get more so as the sitting proceeds ". The correlation is ·78 which nearly satisfies the criterion for significance.[1] If another experiment confirmed this correlation, it would indicate something curious about the data. Again it is difficult to say what, but there seems no reason for attributing it to the autonomy of communicators, particularly since the relationship seems to hold for negative means (which contradict this hypothesis) as well as for positive ones (which are in its favour).

[1] At first sight, the correlation seems to be much less than this but that is because there is an obvious misprint in the list of means on p. 215. A minus sign has been omitted before the last mean.

In paras. 32 and 33, a mistaken method of eombining the probabilities from the above four lines of evidenee is developed (giving for the eombined P's the startling value of ·000013). This has already been listed as one of W. W. C.'s reeantations and no more need be said about it. He also, however, uses the orthodox "negative logarithm" method (p. 220) and gets an overall probability of ·01 in support of the thesis that eommunieators are autonomous. This would be a perfeetly eorreet proeedure if all four pieees of evidenee were independent (whieh they are not eompletely) and if they were all evidenee for the hypothesis in question (whieh we have seen they are not). Sinee these neeessary eonditions are not fulfilled, the eonelusion here indieated must also be rejeeted.

This may seem to be somewhat destruetive eritieism. Any diseussion of this part of TP 3 must be so. I do not, however, wish it to be eoneluded that nothing ean be established from this work. Having eleared away this unfortunate superstrueture of fallaeious argument, there remain the faets of paragraph 25 in whieh it is seen that the *z*'s are randomly distributed as they would be if there were nothing peeuliar to the alleged same eommunieators when eommunieating through different mediums. In other words, the results of the experiment are not merely that it fails to show that the eommunieators are autonomous personalities, but that it eonforms remarkably well to the hypothesis that they are not. It eannot, of eourse, prove that the alleged John and Etta eommunieating through one medium are not the same personalities as John and Etta eommunieating through a different medium but it does show that if there is anything peeuliar to John and to Etta, the word-assoeiation test fails to reveal this faet.

The imperfeetions of the data mentioned above do not give a valid reason, I think, for rejeeting this negative eonelusion although they would provide reason for being suspieious of a positive one. The inelusion of two sets of data from Mrs Leonard might have produeed (but, in faet, did not produee) spurious indieations of autonomy, but they would not have masked genuine indieations of autonomy from the other eomparisons. The same is true of the inelusion of "reproduetion" material. I do not know whether the unsuitability of this material for the mathematieal treatment whieh was used would have made the *z*'s obtained from it too high or too low. If they were too high, they might have produeed spurious indieations of autonomy, but if they were too low they would not have masked genuine indieations from the "reaetion time" data. It would eertainly be a good thing to have eonfirmation of the result by more extensive material from whieh data of the kind eritieised has been excluded, but the evidence as it stands is good and its indications are unquestionably negative.

They are negative in the sense that they show that the experiment does not give evidence in favour of the autonomy of communicators. How far can they be claimed as evidence that there are no autonomous communicators? Obviously no failure to detect autonomous communicators could be final evidence that there were no autonomous communicators to detect, but it might provide indications in that direction of greater or lesser evidential value. How great is the evidential value of such indications must obviously depend on how likely it is that the test would reveal the autonomy of communicators if they had been autonomous. This depends on whether the word-association test (under the condition of communication through a medium) obtains results that are really characteristic of the personality under investigation, and, if it does, on how sensitive is the method of distinguishing personalities used in the inter-medium work.

It seems that the sensitivity of the method could be tested without much difficulty in some such way as that of carrying out the same procedure with the undoubtedly separate personalities of living subjects, and discovering whether their separateness is shown by significantly positive z's. I understand that, in a future publication by W. W. C., some such test of sensitivity will be made.

Even, however, if the procedure is shown to give a sufficiently sensitive test of the autonomy of personalities tested directly by the word-association test, it might be the case that the necessity for communicators to respond through a medium prevented the responses from adequately characterising the communicator. This possibility appears to be more difficult to investigate, and unless it can in some way be eliminated, it seems that W. W. C.'s results may provide no evidence for autonomous communicators but also no very strong evidence against autonomous communicators.

At the time of publication of TP 3, its author supposed that its evidence supported the hypothesis that communicators were autonomous personalities. His conclusion was that, on the question of an extraneous influence corresponding to John and Etta, "If nothing more than a few million pounds or the fate of a couple of nations were involved, I should feel disposed to declare flatly that the operation of some such extraneous influence had been established" (WC 3, p. 222). But in view of the importance of the issue, he preferred not to commit himself. Nevertheless, it seems a fair inference from these words, that he regarded the conclusion as being pretty well established (presumably he would not lightly sacrifice either a few million pounds or a couple of nations).

This conclusion, I understand, he is no longer prepared to defend. If, however, the evidence of the later sections of TP 3 had been (as W. W. C. supposed it was) reliable, independent and relevant to the hypothesis in question, the conclusion would have been the one indicated by the evidence. Although W. W. C. was certainly wrong on this point, it does not mean that the enquiry was not worth while or that nothing has been achieved by it. The original question was worth asking, and the methods might have given a positive answer to it. In any case, negative conclusions are no less worth while to scientific progress than positive ones although they are less satisfactory to those who have done the work for them. It would have been more exciting for Mr Whately Carington (and for us all) if he had made the first quantitative demonstration of the reality of spirit communications through mediums. It is, however, no less creditable to him that he has devised a satisfactory technique for testing this problem although the result of the test appears so far to be entirely negative.

X. Conclusions

We saw at the beginning that Mr Whately Carington set out to solve three main problems. First, to devise a quantitative method of experimenting on trance personalities. Secondly, to use this method to find out whether it could provide evidence for or against the autonomy of spirit communicators. Thirdly, to find out as much as possible about the psychology of the trance state.

The first aim has been achieved. In TP 3 and more satisfactorily in later work still unpublished, a method has been devised which is practicable and fundamentally sound. Its general principles could obviously be used with other material than the reactions obtained by the word-association test. It could, in fact, be used with any test whose results could be expressed quantitatively and whose numerical results were distributed approximately normally. The only obvious limitation of its validity would then be that it must not be used on material (such as the actual words given as responses in the word-association tests) in which similarities between the same communicator through different mediums might be the results of the mediums' knowledge of salient features of the alleged communicator's character and interests.

The second aim seems also to have been achieved. The results obtained are those which would be expected if there were no real communicating spirits who communicated sometimes through one medium and sometimes through another. No negative conclusion can be final, and it is reasonable for those who believe in the autonomy of communicators to try other tests by the same method. If these are rigidly applied and rigidly evaluated and are found to give positive results, the existence of autonomous communicators will be proved. If these tests too give negative results, the conclusion that there are no autonomous communicators in séances will become a very probable one.

Finally, there is the third object of throwing light on the psychology of trance states. There are many indications, of which I think the most important is the countersimilarity of controls indicating the possibility that these are secondary personalities produced by repression. Most of the evidence on the psychology of trance states seems, however, to require reconsideration and amplification.

NOTE ON PROFESSOR THOULESS' PAPER

It was Samuel Butler, I think, who observed that " Life is like playing the violin in public, and learning the instrument as one goes on ". This aphorism, which is only too true in so many contexts, I have found acutely applicable throughout the work discussed by Professor Thouless. Certainly I have had to " learn the instrument " as I went along, and it is evident from his most valuable and conscientious criticism that I have made very many mistakes of all orders of magnitude. Still, it is something to feel, as I think I am justified in doing, that I have shown the instrument to be worth playing and even, perhaps, to have produced " . . . something remotely resembling an air ", however far short of my original hopes the ultimate results may have fallen. In so far as I may unwittingly have misled many of my readers, I can only express my deep regret, and plead with Dr Johnson " Ignorance, Madam, sheer ignorance ".

Broadly speaking, I accept nearly all of Professor Thouless' criticisms at their face value, my differences, if any, being on minor points of wording rather than essentials. There are, however, a few points on which comment seems worth while :

(1) The credit for devising the basic method used in TP 3 should, as there stated, be given to Mr W. L. Stevens, of the Galton Laboratory, and not to me.

(2) Whereas it is true that the reproduction data, being necessarily 1 or 0, cannot legitimately be treated by analysis of variance, for the reasons given, this does not imply that they are valueless. They may be dealt with by appropriate methods of the " contingency " type and accordingly represent valuable data worth the collecting. I hope to study them anew whenever opportunity offers, although I greatly doubt whether they will yield results appreciably different from those given by reaction times.

(3) It is true that I used the " finger print " analogy in TP 1 and that this suffers from the defect mentioned by Professor Thouless. But this was *not* because I did not realise the importance of the point in question, as is shown by my comparison with Bertillon measurements which has not this defect. It was a concession, apparently misguided, to the fact that hundreds of people have heard of finger prints for one who is familiar with the earlier (Bertillon) technique.

(4) As regards the important question of the counter-similarity of controls, there seems not much doubt—even after Professor Thouless has done his righteous worst—that this is a genuine phenomenon, to be interpreted as I have maintained. I hope, however, to be able to re-examine this question with greater particularity, working the results by separate occasions and applying corrections to blocks of 25 words instead of to the sitting as a whole. The fact that the RPN data, though correlated with RT, do not in general indicate countersimilarity, is probably due to their inappropriate treatment by analysis of variance.

(5) I venture to think that Professor Thouless is definitely in error when he says (p. 269) that " no reasonable meaning can be attached to negative z's ". Actually, it is not difficult to show that a negative z is likely to result if a certain proportion of J and E times are interchanged ; that is to say, if, fairly often, we actually get an E time when we think we are getting a J, and *vice versa*.

But this is a topic to be discussed in a later communication, in which I hope to deal with an extension and revision of the inter-medium R.T. work. I may say at once, however, lest future hopes be aroused, that at present there seems no prospect of reversing Professor Thouless' conclusion, which must, I think, be unequivocally accepted, so far as the data at present available are concerned.

In conclusion, I must express my most sincere thanks to Professor Thouless, not only for the great amount of trouble he has taken, but for the singular combination of ruthlessness and sympathy he has brought to bear on the work.

The above, I should like to emphasise, is no empty form of words ; for, although I have yet to hear that fruit-trees enjoy the process of pruning, all that matters is that their fruit should ultimately be sound.

W. W. C.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 151

SUPERNORMAL FACULTY AND THE STRUCTURE OF THE MIND

BEING

THE FREDERIC W. H. MYERS LECTURE, 1937

(delivered October 27, 1937)

By C. A. Mace, M.A.

It is a paradox that should delight the Hegelian philosopher and the Freudian psychologist alike that the defences we erect within ourselves against prejudice and superstition themselves tend so to encrust and petrify the mind that it becomes increasingly resistant to novel truths. No one has had better reason to be conscious of this paradox than the student of psychical research in his efforts to invoke co-operation from orthodox working scientists in relevant and allied fields of investigation.

The distinguished exceptions to this generalization are in large measure standing witnesses to the greatness of the achievements of Frederic Myers whose scholarly and balanced presentation of the facts first impressed the more discerning and continues progressively to undermine the resistances in the body of working scientists as a whole. So much progress has been made that now it is hardly possible for the working psychologist, at any rate, to proceed with his proper task without taking serious cognisance of the evidence for the supernormal aspects of the personality of man.

Accordingly, I propose to take this opportunity to pay my tribute to the memory of Myers not by presuming to speak as one who has himself graduated in the more specialized and technical fields of psychical research, but by speaking as one such ordinary working psychologist who, though concerned in general with the normal functions of the normal mind, finds it necessary to adjust his concepts to the facts which psychical research has brought to light.

It serves to raise important issues straight away if the question be bluntly put : How far does the professional student of the normal mind accept the findings of psychical research? Does he believe that telepathy occurs? Does he accept the evidence for clairvoyance, for survival, for any of the alleged material phenomena? If he does, why do none of these important matters find a place in the textbooks of his science? If not, where precisely do his objections and his difficulties lie?

Speaking for myself, but trusting that my answer is not eccentric or unrepresentative, what I should begin by saying is this :

Strictly, the attitude of *belief* is one that plays a very much smaller part in scientific thought than is apt to be supposed—a fact which often makes the scientist appear more evasive and non-committal than in fact he is. If we take *any* important current scientific hypothesis and ask the scientist whether he accepts or believes it, he will almost certainly say that he does not. He will say : From the nature of the case, from the fact that it is only an hypothesis, I do not *believe* it. All I can say, as a scientist, is that I seriously *entertain* it. I am prepared to treat it with the respect due to any hypothesis put forward by a fellow scientist whose intellectual powers and whose integrity I acknowledge, and I am prepared to devote my time to conducting investigations which will contribute to its verification, its modification or rejection. This is the essential form in which a scientific hypothesis obtains recognition. And this, I take it, is the kind of recognition that Myers wished to see accorded to the hypotheses of the field in which he was a pioneer.

To say all this, however, is to give only a part of the answer to the question I have raised. Let us consider in a little more detail the problem of telepathy which in this respect provides a representative, relatively simple, but undoubtedly critical case. Is telepathy an established fact? The answer depends upon an analysis of the empirical and the hypothetical elements in the assertion that a process of telepathic communication has occurred. Clearly, all that is observationally verified in the most direct way in any particular case is a certain detailed parallelism of events. Suppose that to one person, *A*, a complex series of events occurs—he is involved, for example, in an unusual kind of accident. Simultaneously, or at approximately the same time, a detailed representation of this sequence of events is present to the mind of *B*, another person situated at some considerable distance from *A*. Each event, or complex of events, may be empirically verified. *A* himself, or a witness, can verify the details of the accident; *B* can verify the presence of the representation to his own mind. In accordance with principles of evidence which we find satisfactory in almost every other connection each of us can know that this parallelism of events has occurred. Admittedly there are difficulties here but they are difficulties of an uninteresting kind. In principle, the evidence may be such that an "incredible" occurrence may have to be believed; and anyone who refuses to accept this evidence, whether he be scientist or not, will thereby remove himself from court to enjoy the unenviable reputation for caution which we accord to those who choose to question the historical existence of Napoleon.

Granting the parallelism of events, what more do we need? Obviously something more. The assertion that the parallelism is the parallelism of communication contains implications which can be verified only in ways much more complicated than those by which we establish *particular* facts. At the very least the assertion embraces two negative propositions: (i) that the parallelism was not a chance coincidence; (ii) that it did not arise in consequence of any one of a certain set of causal processes which we might specify by enumeration.

These two assertions are connected. To deny coincidence is to assert causal connection. The two possibilities are alternative and disjunctive. It cannot be both and it cannot be neither. If it is not a coincidence the connection must be causal, there is no third alternative. The second assertion is of the kind that the formal logician would describe as a compound negative proposition. The parallelism does not arise from sight, hearing or smell, nor from memory, nor from inference, nor from any other of the commonly recognised processes through which ideas come to be present to a mind.

Here, then, is an obvious source of some of the residual doubts with which the accumulating evidence for telepathy continues to be received. To establish the contention that a parallelism of events arises from a supernormal mode of communication we have to establish a causal proposition; and we have further to establish a complex combination of negations.

In principle, two lines of procedure are open. In the first we do not formulate any specific theory concerning the nature or the *modus operandi* of the causal factors involved, but try to prove each of the constituent negative propositions one by one. The alternative procedure is the logically more direct but practically no less prodigious undertaking of attempting to discover the positive character of the process.

To the former of these procedures belong the now highly technical applications of statistical methods. In these we have an instrument which enables us to prove that a parallelism must be in some way causal without knowing *what* the causal connection is. It is not in the least to disparage work along these lines to say that it is subject to inherent limitations, and that it cannot in itself provide all the information we require. To say this is merely to repeat what statisticians themselves have emphasised and what those who have used these methods would readily endorse.

The progress of science, generally, depends upon the preservation of the appropriate balance between empirical observation and theoretical construction. To this the science of the supernormal can be no exception. And here again our thoughts must turn to Myers in whose monumental work we find a combination of factual record and cautious construction strikingly contrasted with pedestrian empiricism on the one hand and with undisciplined speculation on the other.

That the case for telepathy is not more widely admitted is, I think, in some measure a consequence of the fact that in recent years the construction of hypotheses has not kept pace with the accumulation of facts. What we need, I would suggest, is not so much quite new hypotheses but hypotheses which are richer in detail, hypotheses we can test, and refute, then modify and test again.

It is my hope that such hypotheses will be found (and here, perhaps, I must ask you to make allowances for the predilections of one whose experience is restricted in the main to the study of the normal) not by starting *ab initio* with radically novel conceptions of the human mind, but by the piece-meal and progressive modification of concepts which have proved their utility in more familiar fields. A psychologist who is conscious of what he is about will always have it in the back of his mind that sooner or later revolutionary hypotheses may well be required, but these are not the hypotheses with which to begin. For this reason, I propose in what follows to try to make more explicit some of the more ordinary concepts with which the orthodox psychologist tries to interpret the facts which come his way, so that we may proceed to ask where more precisely new formulations are likely to be required.

Piecing together relevant data drawn from many sources we are led to picture the constitution of man as an immensely complex mechanism the salient characteristic of which is the capacity to respond to stimulation. As such it is a mechanism consisting of two broadly contrasted parts—a mechanism of reception and a mechanism of response.

As a receptive apparatus man consists of a set of organs each of which is selectively sensitive to its own appropriate type of electromagnetic, chemical or grosser physical mode of stimulation ; and as a responsive apparatus he consists of a set of organs used to produce effects upon things in his environment.

In the cycle of response to stimulation we have presented for analysis a causal chain the main links in which can be broadly enumerated as follows :

(i) First we have an initiating event—the *remote* stimulus, such as the vibrations in a bell or a change in the temperature of a filament. (ii) There is then an external transmissive process through which corresponding changes—sound, light, heat, etc.—are propagated through a more or less homogeneous medium. This constitutes the immediate or proximate stimulus. (iii) Then occurs a change in the appropriate receptor organ of the body as the transmitted impulse impinges upon it. (iv) There follows an internal transmissive receptive process wherein another type of change is propagated through the more or less homogeneous conductive medium of the afferent nerves. (v) The receptive process, physiologically conceived, is completed by a central process which can in part be located in certain definite areas of the sensory cortex to which the afferent paths proceed. (vi) There follows a further central change which initiates the second half of the process—a central responsive process which, too, can in part be located in another definite area of the brain. (vii) A second internal transmissive process ensues—the internal transmissive responsive process through the conductive medium of the efferent nerves. (viii) Completing the organic sequence there occurs a set of changes in specialized effector organs which either directly produce effects upon the external environment or initiate further changes in the organism itself. In the former case we can normally distinguish two final phases in the total cycle, viz., (ix) A second external transmissive process through an appropriate conductive external medium, and (x) a terminal change in some remote object. This object may, of course, be another organism ; in which case the terminal phases of one cycle are the initiating phases in a second cycle of the same general type.

This is one of the simplest cases, but of course it is by no means the only type of process exemplified in the normal interaction between the individual and his environment.

The problems which arise in the attempt to elucidate in detail the course of events here summarily described are of extremely varied and extremely difficult kinds. The solutions of these problems depend not only upon specialized physical, biochemical, physiological and psychological research but also upon the clarification of fundamental philosophical and logical issues. I shall restrict myself, however, in this discussion to those points which are most germane to the problem of supernormal faculty.

To begin with we may note the extraordinary lack of homogeneity in the concepts required even to *describe* this sequence of events. Three antitheses in particular call for scrutiny.

First there is the antithesis between the strictly physical and the strictly psychological concepts. It is a matter of pretty general agreement that the constitution of physical objects and the nature of physical processes can be described in terms of a very limited number of fundamental concepts, and it is further agreed that in terms of these concepts we can not describe the states of consciousness or experiences of individuals. My preliminary description of the process of response to stimulation has been given mainly in physical terms, or in terms which could in principle be reduced to the fundamental concepts of physics. But it is clear that in any adequate account of this cycle of events we should have to make statements concerning the *experiences* of the individual who responds to stimulation, and such statements would introduce the distinctive concepts of psychology.

Next, and not less important, is the antithesis between the concepts employed in a strictly physical description and those which enter into the plain language of everyday usage. It is, I think, important to stress the fact that this second distinction is fundamental. It is not merely that scientific language is more precise, or that it is " technical " ; the difference is one of kind—of a kind most clearly illustrated in the difference between the common-sense description of light and colour and the scientific description of the " corresponding " electro-magnetic waves. Traditionally, this distinction has commonly been assimilated by philosophers either to the distinction between matter and mind, or to that between the " primary " and the " secondary " or higher order properties of material things. For the present I wish merely to note the antithesis without making any special assumption regarding its nature.

The third antithesis to which I would draw attention is that between the brute empirical, or observational, data incorporated in our account of response to stimulation and the non-empirical elements—the hypotheses, constructions and methodological assumptions by the utilization of which the otherwise detached empirical data are assembled to yield the picture of a coherent causal chain. It would take more than the time available for this discussion even to state the many hypotheses relevant to the account given of each phase of the process—physical hypotheses concerning the nature of the stimulating object and the constitution of the external media, physiological hypotheses concerning the receptor processes and internal conduction, and psychological hypotheses concerning the experiences of the individual who responds to stimulation.

What in the present connection is of particular importance is the distinction within the non-empirical elements between the special hypotheses of each of the relevant sciences and the more general assumptions—of a methodological character—concerning the nature of the causal nexus, as such, through which each phase of the process is linked to its neighbours. It is from such general presuppositions that we are led to introduce for example the concepts of physical and mental " dispositions ".

It is clear that we cannot account for the causal process simply in terms of a sequence of *events* or occurrences. We cannot account for the connections either in terms solely of the *actual* occurrences observed or solely in terms of these together with other *actual* occurrences hypothetically assumed. In addition to occurrences we postulate causal factors of a persistent (as opposed to an occurrent) nature.

The action of a stimulus or agent upon any material substance would seem most commonly to produce two types of effect—firstly a more or less transient event, and secondly a more or less permanent change of state. Subjecting a bar of tempered steel to heat, for example, produces a change of temperature in the bar and it produces a persistent change in its elasticity. Stroking a bar of iron with a magnet produces transient events in that bar and at the same time induces in it persistent magnetic properties. Similarly, the fact that a burnt child dreads the fire suggests that the temporary application of a nocive stimulus induces, firstly, a temporary change in experience and behaviour—the immediate " response "—and, secondly, a persistent disposition to behave in certain ways under certain conditions.

We may picture to ourselves what happens in the case of the magnetized bar of iron by saying that this bar really consists of a multitude of smaller natural magnets in higgledy-piggledy order, and by supposing that stroking the bar caused first the occurrent movements of these component magnets (which movements formed part of the immediate effect) and second it caused these component magnets to assume and remain in a new kind of arrangement—this being a standing arrangement which will persist until something else happens to alter it again.

Similarly, we may be tempted to picture the dispositional change in the burnt child by saying that the original stimulus, in addition to producing such events as the movements and experiences which constituted the immediate response, produced some new " standing arrangement " in the elements of his nervous system. But here the theory of dispositions is complicated by the antithesis between physical and mental properties. An alternative account is provided by saying that the long period effect is to be explained by reference to a change in the " standing arrangements " of the child's mind. This is a complication which will occupy us further in the sequel.

Notwithstanding the obscurity and the hypothetical character of much that it contains, this account of the general constitution of man provides a convenient starting point for an inquiry into the nature of the supernormal faculties.

We may note one small promising feature straight away. The varied forms of supernormal process for which the strongest claims have been advanced fall quite naturally into two main classes corresponding with those into which normal powers have been divided—the receptive and the executive. Telepathy, clairvoyance, and precognition exemplify supernormal reception ; levitation, materialization and allied phenomena suggest the operation of powers of action and response other than those that employ the mechanisms of the normal neuro-muscular and glandular systems. Of course, the antithesis must not be too sharply stressed. Telepathy, for example, may be regarded either as an executive or as a receptive function according as we suppose its distinctive feature to consist in a peculiar supernormal act of the agent initiating the communication or in a peculiar supernormal type of sensitivity in the recipient. In point of fact it is not improbable that telepathic communication depends upon two distinct supernormal faculties, corresponding to these two cases, each of which calls for separate investigation. In my present discussion, however, I shall be almost exclusively concerned with cases in which the element of supernormality enters primarily into the receptive process.

The question next to be raised is : In what way, and at what point precisely, does supernormality intervene? Clearly, intervention must be located somewhere in the *intermediate* phases of the process. The remote or initiating stimulus is not itself supernormal. In clairvoyance, telepathy or divination, for example, what is apprehended and is the initiating cause of the act of apprehension is a natural event, commonly something happening in the material world which from some vantage point can be presented through the normal channels of sense perception. So, too, is the terminal phase of the process, in its intrinsic nature, an event of perfectly normal constitution—a cognitive experience which can be described in the terminology accepted by psychologists of the normal. There is nothing in the *nature* of a perception or a thought as presented to introspection which enables us to distinguish the normal from the supernormal. The distinction lies in the circumstances under which the perception or thought occurs.

In the case of conceptual, ideational, or generally nonperceptual types of awareness, there is a further observation to be made which also helps us to restrict the field in which the intrusion of the supernormal must be sought. When someone is conscious of an event not sensibly presented there are three things to be distinguished: (1) the bare presence of the idea of this occurrence to mind, (2) the belief in this occurrence, and (3) the further conditions which are satisfied when the occurrence of the event is not merely entertained, not only believed, but in addition is *known.* Now in the case of supernormal consciousness of the non-perceptual type there is, so far as I can see, nothing supernormal in the genesis of belief, and there is nothing supernormal in the epistemology of the case. Given that the idea is present to mind, the fact that it is *accepted as true* can in principle be explained by general psychological principles, and when it is *known to be true* this will be because it satisfies the conditions which the epistemologist formulates in the ordinary way for the purpose of distinguishing knowledge from mere belief.

These considerations all serve to simplify and restrict our problem. They reinforce what has earlier been said to the effect that what is critical and essential in the assertion that supernormal communication has occurred is the causal implication of the statement. If I am not over-simplifying the issue—and even on this view it remains, in all conscience, sufficiently complicated—our inquiry resolves itself into a study in causality. The supernormality of supernormal cognition resides in the supernormality of the causal process upon which cognition depends. Accordingly our next step will be to examine in somewhat greater detail the precise nature of the causal nexus as it is exhibited in the normal process of reception.

So long as we try to formulate an account of the sequence of events in terms acceptable to reflective common sense and not in terms of the more sophisticated special philosophical and scientific theories, we postulate, to begin with, a number of particular substances or things to which and in which these events happen. These events are in general changes in the internal states of these things or changes in their spatial or other relations to one another. Between such changes we assume that the things in question persist in certain states and relations.

We have observed that the characters of these things may be divided into material characters and mental characters, and further that we can distinguish primary and secondary properties. Accordingly we are compelled to make certain assumptions concerning the distribution of these properties. We should preserve the maximum freedom in the construction of interpretative hypotheses by assuming that all possible combinations of these properties could be exhibited in one and the same thing, and that anything might be deficient in one or more of these types of property. In point of fact most theories are constructed on the assumption that the possible combinations are more limited than this. Partly on *a priori* grounds, and partly on empirical grounds it is most generally assumed that some pairs of properties are invariably disjoined and others invariably conjoined.

It has, for example, been very generally assumed, since the time of Descartes at any rate, that nothing combines material and mental characteristics, from which it follows that the cycle with which we are concerned is one which involves some purely material things and some purely mental things in causal, quasi-causal, pseudo-causal or some other more mysterious relation. Personally, I consider this to be extremely dubious, and am more favourably disposed toward some of the recent theories according to which the things we are concerned with may possess both or neither of the types of characters which are defined respectively as material and mental. But since I am concerned to formulate an account of the normal sequence of response to stimulation so far as may be in terms acceptable to reflective common sense and the most widely current opinions of the day—and not in terms of special philosophical theories—I provisionally lay down that we are concerned with a set of substances some of which are assumed to have material properties but not assumed to have mental properties, and others of which are assumed to have mental properties but not assumed to have material properties. I need not assume, however, that nothing has neither.

Regarding the distinction between primary and secondary properties—which I shall assume to be equivalent to the distinction between spatio-temporal and other relational properties on the one hand and *qualities* in the narrower sense of this term on the other—I shall assume that for ordinary scientific purposes we shall need to refer only to the primary properties of material things, but that in the analysis of the psychological phases of the process secondary properties will enter into our descriptions.

Accordingly, the *structural* basis of the response to stimulation will be constituted by, *firstly*, a set of discrete material things possessing characteristic internal states and standing to one another in certain spatial relations and connected with one another by certain media, which media may be conceived to be continuous (like a jelly) or made up of a compact mass of smaller particular discrete substances (like a bag of lead shot), and, *secondly*, by a set of mental substances or individual minds. The latter, unlike the material substances, are not spatially adjoined, except indirectly or in a special sense through their relations to bodies, nor are they, on ordinary views, connected by a mental medium picturable as a jelly or as a bag of shot. There are, however, reasons for supposing that what is commonly regarded as a single discrete individual mind either is itself, or is part of, a collection of minds, one member of which collection is the central conscious being we introspect, the others constituting secondary or subconscious personalities, or, on certain views, a set of subordinate "monads". But whether we are concerned with the central conscious self or with any particular subordinate personality or monad, it is assumed that this self is linked throughout life to a material body or some part of such body. Commonly the mode of linkage is conceived to be something *sui generis*. It is unlike the mode of linkage exhibited by the material parts of a material thing; it is unlike the mode of linkage which holds between two properties of the same thing. That is to say, it is neither any sort of cohesion or spatial adjunction, nor any sort of co-characterization.

Within certain limits we can conceive the total system constituted by a set of material things, some of which are linked with minds, as being at rest, *i.e.* as maintaining the same unchanging states throughout a period of time. We can do this more easily for the material part of the system. We can picture a potentially stimulating object remaining at a constant temperature and we can picture the material media, through which a change of temperature might be communicated, as undisturbed—at least we can do this so long as we conceive the state of affairs in common-sense qualitative terms.

But even at the unsophisticated common-sense level we encounter difficulties when we try similarly to picture the psychological part of the system as at rest. The difficulty of maintaining even for a moment or two a literally and strictly unchanging mental state disposes common sense readily to accept such statements as that "all consciousness is consciousness of change" or that "all consciousness is changes of consciousness". We can evade these difficulties for the moment by thinking of a temporal cross section of the state of affairs or of the state of affairs in the whole system at a certain time. This yields us the picture of the structure of the system as a whole—a picture of certain material things in certain positions and in certain states, of certain minds presented with certain sensory data and images and further characterized by feelings and desires, and with their bodies ready for the reception of further stimuli and prepared for the execution of a suitable response.

Let us next consider certain general features of the causal process when the machinery begins to work. In the simplest case the cycle of events is initiated by a change in one object which operates on the remote stimulus, say a change consisting in a specific type of oscillation in the particles of this object—an oscillation which is precisely defined by specifying such variables as the amplitude and rate of movement. This change or some other more or less similar change is then taken up by an adjacent member of the system, the same or a similar change is taken up by another member adjacent to the second, and so a series of corresponding changes is propagated through the system as a whole.

The expression " this change or some other more or less similar change " draws attention to a general character of the causal nexus which requires some discussion. The parallelism of events characteristic of communication, whether normal or supernormal, arises in virtue of the fact that effects resemble their causes in certain respects. Several historical controversies have been concerned with the kind and degree of similarity which holds between cause and effect. The truth of the matter I believe to be that effects are both similar and different from their causes. They are similar in certain respects and they differ in certain respects. Clarity requires that we should endeavour to specify the respects in which they are similar and the respects in which they differ, and the circumstances under which similarity and difference will themselves differ in degree and kind.

That some special kind of similarity holds between events causally connected follows, I believe, from the definition of causality or from the criteria by which we distinguish causal connection from coincidence. We assert causal connection with greatest assurance on the basis of observing concomitant variations. But the vague phrase " concomitant variations " covers two things, first the spatio-temporal or other type of adjunction between the events, and second some similarity of form or parallelism between two series of changes.

This similarity or parallelism varies in degree. The limiting case of exact similarity in all respects is one to which a pair of occurrences approximate under certain special conditions. The closest approximation is found when the substance in which the effect is produced is exactly similar in internal constitution and closely adjacent to that in which the causal event occurs—*i.e.* in the case of conduction through a homogeneous medium. Difference between cause and effect arises where there is spatial separation or difference in internal constitution between the things to which the events happen.

In conduction through a homogeneous medium, where difference is only difference in spatial position, the effect will resemble the cause in proportion to the proximity of the "patient" to the "agent", and the greater the distance between them the greater the difference between the two events. Commonly the precise character of both the differences and the similarities will be defined by a specific formula—such as a law of inverse squares. When the causal process consists in the transference of some change from an agent of one type of internal constitution to a "patient" of a different type of constitution other differences will be superimposed. Here, too, in simple cases, the character of the transformation can be precisely defined—as when, for example, the character of the effect can be referred to the curvature of a mirror or lens, or to the refractive index, the density or the elasticity of a transmitting medium. Very small differences in internal constitution are sufficient to make the differences between cause and effect much more prominent than their similarities. But even in extreme cases some similarities may be found. In fact, it is by reason of their detection that causal relations are discerned.

A critical case is that in which, on traditional assumptions, the physical and physiological cycle produces changes in the mind. Electromagnetic oscillations being followed by certain corresponding changes in the cerebro-retinal apparatus of sight are in turn followed by the appearance to the percipient of a flash of coloured light. On the traditional assumptions, within the framework of which we are endeavouring to conceive the process, the second transition like the first is causal in nature. On any view this second transition contains much that is peculiar and mysterious. I want to emphasize this point because the admitted mystery in normal psychology may turn out to be bound up with some of the mysteries of psychical research. For one thing, there is on any view something peculiar about the spatio-temporal nexus. In the purely physical phases of the process spatial continuity is linked with temporal continuity, so that events proximate in time are proximate in space, and events relatively remote in the temporal order are also relatively remote in the spatial order too. Now what leads us to assign the emergence of a visual sensory datum to a definite position in the causal series is the fact that it can be given a fairly definite position in the *temporal* series. We assign it to the moment at which the physiological conductive-process has reached a certain place in the cerebro-retinal apparatus. On the analogy with normal physical causation the unsophisticated may accordingly be so misled as to link spatial with temporal proximity by saying that the sensory datum has its being inside one's head. The less unsophisticated, seeing the objections to this, are content to say that the datum is in one's mind, or more guardedly still, in the presentational or phenomenal field. In addition to the spatio-temporal peculiarities affecting this phase of the process there is a peculiarity with respect to what we may call the substantival factors in the causal process. It is admitted that variations in the character of the sensory datum are so linked with variations in the stimuli as to suggest causal connection. Clearly, however, these variations are not explained by the stimuli alone, nor by the stimuli together with the pre-existent properties and states of the body as physically defined. If, therefore, we persist in the endeavour to adhere to the general principles of causal explanation we have invoked elsewhere—accounting for a change by reference to an external occurrent factor operating upon a substantial thing of a certain nature, referring to the latter all that is not accounted for by the former—*we are led to postulate some non-physical substance upon which the stimulus acts and within which or to which the effect event occurs*. That is to say, just as we account for the fact that an electro-magnetic stimulus produces a chemical change in the cerebro-retinal apparatus by reference to the constitution of that apparatus itself, so we account for the fact that the latter chemical action in the nervous system produces a change of sensory experience by postulating something of substantival type, the nature of which contributes to the effect to be explained. Sometimes the substantival factor has been identified with the mind itself and a sensory experience regarded as one of its " states ". A variant of this view is illustrated by the hypothesis of James Ward, who distinguished this substantival factor from the "self", regarding it as a " psychoplasm " analogous in some respects to the plastic material of which bodies are composed, but possessing the distinctive properties required to account for the observed effects. Contemporary philosophers of many otherwise diverse schools are content to describe sensory data as neutral (as between matter and mind) and they try to avoid committing themselves to any assertion to the effect that sensations are states of any substance whatever. In fact, such an account would often be explicitly denied. In this they may be right; but the point I wish here to press is that to make this denial entails a consequential modification of our views as to the kind of causality implied when we speak of our sensory experiences as being caused by happenings in the brain. On any ordinary view of causality what happens in the brain can be only a contributory causal factor—and some other factor must be postulated on any view which is not content to leave the matter wrapped in impenetrable mystery.

In point of fact, the circumstances are essentially similar to those in consequence of which we postulate dispositions. It is rather curious to observe that whilst factors of substantival type are freely postulated in all explanations of normal memory it has not been so commonly or so explicitly recognized that factors of the same type are required for any reasonably complete account of normal sensation. In neither case is the existence of such a factor to be asserted on grounds of direct observation. In both cases the grounds reside in the adoption of certain causal postulates. It is also only on account of such postulates that we can assert that the contributory factor in its intrinsic nature is mental or that it is physical. But I cannot clearly see any well-established principle which would justify either of these assertions. In fact I believe both views are open to serious objections. If this be so, that is, if we are led by general considerations to postulate such a factor, and further general considerations prevent us from asserting it to be mental or physical, we are accordingly led to postulate something which is neither mental nor physical or which we can, if we are so disposed, picturesquely describe as belonging to a " third realm of being ". Philosophically such language is no doubt objectionable, but it has the practical psychological advantages of freeing our mind from misleading associations and of conferring a certain plasticity upon the ideas we require in order to develop some of the much needed hypotheses of psychical research. Accordingly without more ado, and subject to all the qualifications which the philosophically minded can supply for themselves I shall permit myself in what follows to speak of this possible third order of being—referring to it noncommittally as the *Tertium Quid*, to signalize the fact that we neither assert it to be mental nor assert it to be material—as a factor which seems to be required both as a contributory cause in the genesis of sensation and as a vehicle of dispositions. If I am right, and if the factor is required at all, it is required for the explanation of normal experience. The question then arises : Granting the existence of this cause factor can it further assist in the construction of any intelligible theory concerning supernormal modes of communication?

The foregoing lines of thought have led us to a general conception of the causal structure of the world within the framework of which we must try to fit both normal and supernormal types of communication. Causality in general is a source of parallelism between series of events. Many examples of parallelism are afforded in the process of reception and in the larger process of response to stimulation. The events in the external transmitting medium parallel events in the originating stimulus object. The events in the cerebral cortex are parallel with those in the receptor organs. There is that particularly interesting parallelism between events in the brain and events in the mind which is assumed by the traditional doctrine of psycho-physical parallelism and in the doctrine of " isomorphism " of the Gestalt psychologists. In general there are as many special parallelisms as there are pairs of links in the causal chain involved in response to stimulation. Such parallelisms, however, are in varying degrees imperfect. Imperfection in the parallelism arises from the attenuation of effects with spatial and temporal distance, from the discontinuities which issue from the differences in intrinsic nature between the components in and to which the cause and effect events occur, and it arises in even greater measure in virtue of the considerations which inductive logicians have dealt with under the heads of " composition of causes " and the " intermixture of effects ". But both the parallelism and the divergence from the parallelism are accounted for by three constituent types of mechanism in the system, (1) mechanisms whereby parallel similar effects are produced at *places* different from those at which an agent operates, (ii) mechanisms whereby parallel similar effects are produced at *times* different from those in which the causal event occurs, and (iii) mechanism whereby parallel but *dissimilar* effects are produced at remote places or times. We may designate these mechanisms mechanisms of diffusion, mechanisms of retention and mechanisms of translation. The conduction of light, sound and electricity under simple conditions illustrate the mechanisms of diffusion. The persistence of physical, physiological or mental dispositional properties on all theories of traces and " engrams " illustrate the mechanisms of retention. The operation of a proximate stimulus upon a receptor and the production of sound by electrical impulses illustrate the mechanisms of translation.

In virtue of the fact that translation processes are sometimes reversible and sometimes cyclical, *re*translation may occur, so that similar effects are produced at remote places and times in spite of dissimilarities in the intervening stages of the process. Such processes are illustrated in photography, in mechanical systems for reproducing sound, in broadcasting systems, and most notably in the normal mechanism whereby ideas in one mind are reproduced in another mind.

Accordingly, we do not need to extend our survey beyond the realm of the normal to realize that the constitution of our world is such that there is no reason, in principle, why a sequence of events at one place and time should not reflect itself in a parallel sequence at any other place or time. There is nothing, in principle, to prevent a sequence of events occurring in Athens two thousand years ago being reflected in Tooting to-day, if a Tooting historian cares to reconstruct the Athenian events in his mind The normal psychology of cognition has the appropriate concepts of diffusion, retention and translation of effects to account for the fact. The parallelism of events, however, is subject to restrictive conditions. Restrictive conditions are implied no less by supernormal than by normal modes of communication. The nature of these restrictive conditions, as they are empirically ascertained, will provide our main sources of suggestion in the construction of detailed hypotheses for psychical research.

Telepathy and clairvoyance, both spontaneous and of the kind which is studied by Mr Tyrrell and by Dr Rhine, the occurrence of apparitions, post-cognitive experiences of the type recorded by Miss Moberley and Miss Jourdain, precognition and other modes of supernormal intuition all fall within the scope of the general principles which govern the parallelism of events. Little as we know as yet concerning any of these things there is ample empirical evidence that they are not of a wholly random character, but are subject to restrictive conditions. What is more interesting is the fact that we can specify to some degree what the restrictive conditions are.

In telepathic communication of the typical sort—I nearly said " of the normal sort "—there appear to be no restrictive conditions with regard to place. Agent and recipient of the communication may be seated on the same settee, or they may be separated by the diameter of the earth. Nor need one party to the transaction even know where the other may chance to be. Whether there be any restricting conditions of the temporal sort the evidence is not clear, but in the most representative cases approximate simultaneity of the parallel events is generally supposed to hold. The most definite evidence of restrictive conditions in these cases pertains to the alterable mental states of the agent and recipient and the emotional relations between them. There is also some evidence of the existence of restrictive conditions affecting the subject matter of communication ; but in this connection there are important differences between spontaneous and experimental telepathy. The most striking cases of the former are cases in which the communication concerns matters of emotional significance to one at least of the participants, whereas in the majority of experimental studies what is in general transmitted is information of no importance to either the participants or, for that matter, to anybody else. Another matter calling for investigation is the possibility of restrictive conditions with regard to volition. In normal psychology there is reason for supposing that some powers are facilitated by voluntary effort and that others are inhibited by effort in accordance with a principle picturesquely formulated by Coué as " the law of reversed effort ". It would be of interest to inquire whether telepathic communication falls into the latter category. There are, in short, many specific questions of a psychological nature on which co-operative research on the part of students of the normal and students of the supernormal could hardly fail to be to the advantage of both.

In the case of hauntings and supernormal cognition of the kind recorded by Miss Moberley and Miss Jourdain, the restrictive conditions suggested by a first analysis are of a very different kind, and indicate in consequence the operation of an entirely different kind of causal mechanism. A survey of representative cases does not encourage the belief that communication is restricted to individuals of any particular mental type or to individuals in some peculiar mental condition, or that it occurs only between agents and recipients between whom there is any pre-established rapport. In the typical " ghost story " emphasis is placed upon the fact that the apparition is presented to normal, matter-of-fact, observers—observers very frequently having no acquaintance with or even knowledge about the person who is presumed to be manifest in the phenomenon. On the other hand, there are very severe restrictions of a spatial kind. It is a particular house that is haunted and commonly a very narrowly restricted region of the house. Spatial restriction, in fact, is very frequently the most strongly emphasized circumstantial detail. The kind of apparition that appears to walk just above or just below the level of the floor (in defiance of the structural alterations which have affected the behaviour of normal pedestrian residents) presents the sort of problem that the psychologist is only too ready to pass on to someone else. This is a kind of restrictive condition which, if it does not suggest any particular causal hypothesis, at least indicates the field in which an hypothesis must be sought. Whereas telepathy and clairvoyance, and other phenomena of the class above distinguished, bristle with problems of a psychological nature one's feeling in these space-restricted phenomena is that they contain something very important with which the psychologist as such has little competence to deal.

Precognition presents yet another type of problem. Whilst the case is covered by the general description " a parallelism of events ", the parallelism is one which does not admit of any plausible causal explanation apart from rather drastic modification either of our concepts of causality or of our concept of time. No doubt there are purely psychological questions to be investigated here, questions similar to those raised concerning telepathy, but I do not think that these are the central and crucial problems in studies of precognition. The utility of the normal psychologist to psychical research is enhanced in proportion to the degree to which he addresses himself to its problem with a full consciousness of the extent to which his own special competence is restricted. A good deal can be done in the way of getting clear concerning what is specifically psychological in the situation and what must be referred to philosophy or to one of the other special sciences.

Reviewing them generally, however, and taking them at their face value, empirical observations of the kind with which we are concerned do, I think, point to the probable necessity for the development of our provisional hypothesis of a residual substantival factor which resembles the postulated vehicle of mental dispositions in its capacity to produce parallel events after a lapse of time, but which in its capacity to produce such parallels throughout periods of time which transcend the individual life has some, at least, of the properties attributed to an " immortal soul ", and which further in its capacity to produce these parallel series in remote localities beyond the range of normal personal influence is analogous to the " ether " of unsophisticated physics. It is for these reasons, presumably, that untutored popular opinion in its first attempt to assimilate the facts of supernormal communication has produced that peculiar psycho-physical hybrid—the notion of a kind of " psychic ether ", which can be vehicle of " thought waves " and retain the engrams of emotionally changed events, which later can be ecphorized in ghostly apparitions.

Before we shout down this theory in scientific horror let us try to define a little more precisely where its error lies. To give it its due this kind of theory has the virtue of concreteness, and I think it is an advantage in the earlier stages of our science to try to work in concrete, and so far as possible, picturable terms. It is better to be crude than obscure. It is better to employ the pounds, shillings and pence of everyday notions than to try to deal with the more refined conceptual currency of higher physics as this currency emerges fresh from the mint. Even in physics common sense preceded Newton and Newton came before Einstein. In psychical research we await our Newton.

The chief defects of the theory of a " psychic ether " lie, I think, less in its theory of diffusion and retention than in the theory of translation, and less in the account of how impressions are engraphically recorded than in its account of how they are later ecphorized ; and it is defective not in supposing these events to occur, but in providing no suggestion as to the conditions of their occurrences.

Personally I am of the opinion that we can, with a good scientific conscience, postulate the existence of a medium which records impressions of all sorts of patterns of events, and which later or elsewhere may produce a corresponding pattern. We need not ask : what is the intrinsic constitution of this medium ; we need not yet ask how it does it. The postulated medium, in fact, needs only to be endowed with the one virtue expected of all hypothetical entities —the virtue of doing exactly what it is told. In order to be non-committal in respect of what is unimportant I have labelled our hypothetical medium the *Tertium Quid*, and I would if I could be even more non-committal still. But, however non-committal we may choose to be we are bound to ask : Under what conditions do these events occur? Under what conditions does this *Tertium Quid* receive impressions and under what conditions will these impressions be ecphorized or revived.

Let us, then, construct hypotheses. Let us say, to begin with : *No* special conditions are required. The medium is such that any sort of event will produce its appropriate impression. This hypothesis may recall to mind a well-known passage in which William James tries to frighten us into good behaviour by an argument based on a highly generalized theory of physiological dispositions. " Every smallest stroke of virtue or of vice ", he writes, " leaves its never so little scar. The drunken Rip Van Winkle, in Jefferson's play, excuses himself for each fresh dereliction by saying ' I won't count this time '. Well! he may not count it, and a kind Heaven may not count it ; but it is being counted none the less. Down among his nerve cells and fibres the molecules are counting it, registering it and storing it up to be used against him when the next temptation comes. Nothing we ever do is, in strict scientific literalness, wiped out."

Let us, then, for the moment throw caution to the winds and go one better than James. Let us suppose that somewhere in our *Tertium Quid* everything that happens anywhere or to anything is duly recorded. This is an extravagant hypothesis, but I suggest that, while we are about it, we take all we want and more, and return later what we cannot use.

Given, accordingly, that all that happens produces its own engram, we ask next : Under what condition will ecphory occur, *i.e.* under what conditions will the engram reveal its existence in a series of events parallel to those it records. Suppose we answer again : No special conditions are required. An impression recorded at one place or time will express itself, without further conditions being fulfilled at all other places and all other times. But this, of course, is demonstrably nonsense. It is to say that, for example, any casual sneeze at one place and time will reflect its characteristic pattern at every other place for all time. We know that it does not. One hypothesis at any rate can be quickly disposed of. Suppose, next, that we go to the other extreme of caution and assert that whilst every event records its impressions upon the *Tertium Quid*, the conditions to be realized before ecphory can occur are probably so many and so complex that in practice the production of parallel events will hardly ever, if ever, occur. Our caution has led us here into the assertion of another kind of nonsense. What we have asserted in one breath we have denied in the next any possibility of verification. The existence of engrams is postulated only on the grounds of their manifestation in ecphorized events. To deny the possibility of the latter is to deprive ourselves of our sole reason for asserting the former. It may be entertaining to think of Nature as engaged in writing a very long book not a single word of which will ever be read, but such a hypothesis, however entertaining, has no scientific function.

Any sensible hypothesis must fall somewhere between these two extreme suggestions. In all theories concerning engrams, traces or dispositions, we have to begin with a choice between certain abstract possibilities. We can postulate universal engraphy—within the relevant field—together with restricted or selective ecphory ; we can postulate selective engraphy with universal ecphory or we can assume the more complicated case of selective or restricted engraphy and selective or restricted ecphory. In the case of memory, for example, this third alternative is assumed by common sense—*i.e.* that some, but not all, of the things we perceive produce memory traces, and some, but not all, of those traces later admit of revival or recall. An important school of psychologists, on the other hand, favour the view of universal retention with selective or restricted revival. It is suggested by these psychologists that every perceptual experience is in a sense retained, but that apart from some special technique of hypnosis or psycho-therapy only a restricted portion of these memories can be revived. The third alternative cannot be dismissed offhand, viz. that we are restricted in respect of what the memory can assimilate, but what it can assimilate it can in principle revive. But whichever hypothesis we choose to adopt we begin to mean business with this hypothesis when, and only when, we attempt to specify the principles of restriction or selection. So, too, we shall begin to mean business with our hypothesis of the *Tertium Quid* when we commence to formulate the specific conditions by which the supernormal parallelism of events is in some measure restricted.

The point I wish to stress is that between the two extreme and absurd hypotheses to which I have referred there are already some fairly well-defined and generally accepted hypotheses of intermediate and restricted type designed to account for facts regarded as normal. To modify these hypotheses to cover the supernormal does not involve any very fundamental question of principle. We can extend the range of application of these hypotheses by removing specific restrictions. This logically and methodologically is a matter of detail ; and the removal of the restrictions can be carried out by a piecemeal, step by step, procedure.

We may start, for example, from some such formulation as we find in the hypotheses of Richard Semon, whose grotesque but useful terminology I have already drawn upon. This hypothesis is formulated in two principal laws. The law of Engraphy reads : " All simultaneous excitements in an organism form a connected simultaneous excitement complex, which, as such, works engraphically, *i.e.* leaves behind a connected engram complex which in so far forms a whole." The Law of Ecphory reads : " The partial return of the energetic situation which formerly worked engraphically operates ecphorically on a simultaneous engram complex."

This, as it stands, states the hypothesis in a form required for mnemic facts as described in physiological and biological terms. If we wish to describe psychological processes the laws may be restated in terms which are approximately equivalent to the traditional laws of association. But, in either case, the laws are restricted in their application to occurrences in *one and the same individual organism or mind.* If, however, we are impressed by the evidence which writers such as Jung adduce for supposing that ideas or impressions elaborated in primitive minds may be reinstated in later generations, we can formulate a doctrine of the " collective unconscious " in a form which makes this simply a modification of Semon's hypothesis. We remove the restriction to one individual and suppose that an engraphic record in one person may be ecphorized in a lineal descendant.

What degrees of kinship are allowed would be, of course, a matter for empirical research. I do not in the least wish to commit myself to any such hypothesis. My point is merely that it is open to no objection in principle. It is one that could be established by certain kinds of empirical fact.

Continuing step by step we could, if the facts supported us, progressively relax our conditions, extending the range of kinship allowed. A more radical but still, in principle, legitimate extension of the hypothesis could be made so as to bring within its scope facts of the telepathic type. In Semon's hypothesis an engraphic record is ecphorized at a *different time* in the same person, or (in a sense) in *the same place*. A more general principle might be suggested which would allow also an engraphic record to be "ecphorized" at the same time but in a different place. Such a principle would be very similar to certain principles which are in fact suggested in the formulæ of Gestalt psychology. The general assertion that a change in one part of the phenomenal field involves a reorganization of the field as a whole opens the possibility of certain types of simultaneous parallelism in virtue of which we can say that the events in one place may be reflected in a different place. Such laws are in general expressed in purely phenomenological terms and they are, of course, restricted to "places" within the presentational field of one and the same individual. But they might be restated in a form involving a specific reference to what I have called the "substantival factor" required in the complete theory of presentation, and we might here, as elsewhere, suppose this substantival factor to be identical with that which serves as the vehicle of temporal dispositions.

From this point we could again proceed step by step to remove specific restrictions. If we remove the restriction to the single individual or organism, we then have the hypothesis that an excitement affecting the substantival factor in one individual may be simultaneously ecphorized (in a generalized sense of "ecphory") in another individual. If we remove our restrictions gradually by reference to degrees of kinship then, quite in its early stages of its extension, our hypothesis would appear to cover the queer facts recorded by Galton concerning the coincident thoughts of twins, and it would accord well with the facts which have been recorded concerning the greater frequency of telepathic communication between close blood relations.

All hypotheses of this type amount to saying, in more concrete picturable terms, that whilst actual experiences are private and restricted to individual minds, what I have called the *Tertium Quid* or the vehicle of dispositions is something that is individualized in other ways. Such dispositional factors on these hypotheses are in some measure shared.

I need hardly say that I attach no special importance to any of the particular hypotheses to which I have referred. My aim rather has been to suggest a certain general schema of possibilities within which such hypotheses will fall, and to suggest a general procedure whereby hypotheses may be framed and modified. In every case, the line of advance will be suggested by directing attention to restrictive conditions. I have mentioned in passing some of the types of restriction which the records seem to indicate, and it would be possible to mention many more. Most of these can be empirically tested, and here I think is one well-defined field of co-operative research for psychologists of the normal and students of psychical research.

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*Evelyn, C. J. A., Wotton House, Dorking, Surrey.
*Ezra, Sir Alwyn, 143 Esplanade Road, Fort, Bombay, India.
*Falk, Mrs, 7 Sion Hill, Clifton, Bristol 8.
*Farkas, Captain Gustav, Kongolo, Belgian Congo.
*Farrer, Mrs, 2 Somerset Road, London, S.W. 19.
Felkin, Mrs, 119 Grosvenor Road, London, S.W. 1.
*Fermor-Hesketh, Lord, Easton Neston, Towcester, Northamptonshire.
*Findlater, J. W., 16 Cutenhoe Road, Luton, Beds.
*Fischer, S., 305 Broadway, New York, U.S.A.
*Fisk, George W., The Spinney, Ditton Grange Close, Ditton Hill, Surrey.

*Fitch, Hugh B.
*FitzGerald, Lieut-Colonel C. R. L., D.S.O., 5 Parkholme, Fairfield Road, Eastbourne.
*Flagg, Don Perley, M.D., 3102 La Salle Avenue, Los Angeles, Cal., U.S.A.
*Fleetwood-Hesketh, Major C. H., Stocken Hall, Stretton, Oakham, Rutlandshire.
*Fleming, Miss A. E., 3 Crossfield Road, London, N.W. 3.
*Fleming, Ian L., 22B Ebury Street, London, S.W. 1.
Flugel, Professor J. C., 11 Albert Road, London, N.W. 1.
*Fodor, N., Ph.D., 47 Beverley Court, Chiswick, London, W. 4.
*Foot, Miss Katharine, 31 Brookfield Mansions, London, N. 6.
Footner, Mrs, 122 Langford Court, Abbey Road, London, N.W. 8.
*Francis, Mrs Francis.
Freeman, Miss Adelaide C., Queen Anne's Mansions, London, S.W. 1.
*Freeman, Rev. Canon, Bentley, Clifton Hill, Bristol.
*Frith, Mrs W.
*Fry, Miss Agnes, Orchard Hill, Brent Knoll, Highbridge, Som.
*Gantz, Mrs W. L., 35 Hasker Street, London, S.W. 3.
*Garton, Wilfred, M.R.C.S., L.R.C.P., 353 Romford Road, Forest Gate, London, E. 7.
*Gatty, Oliver, 6 Lowndes Square, London, S.W. 1.
*Gavorse, J., 329½ West 21st Street, New York, U.S.A.
Gay, The Hon. Mrs C. H., Higham Hall, nr Rochester, Kent.
Gellert, J. W., 38-40 Grenfell Street, Adelaide, S. Australia.
*Gharagozlou, Mme. Naghi Khan, Khiaban Arbab Jamshid, Teheran, Iran.
*Gibbes, Miss E. B., 25 Jubilee Place, London, S.W. 3.
*Gibson, Edmond P., 1009 Oaklawn Street, N.E., Grand Rapids, Michigan, U.S.A.
*Giglio, E., 21 Mincing Lane, London, E.C. 3.
*Gilbert, A. Stuart, 7 Rue Jean du Bellay, Paris, IV.
Giles, Mrs, 19 Highfield Road, Edgbaston, Birmingham.
Gilson, R. Cary, Quilters, West Chiltington Common, by Pulborough, Sussex.
*Glanville, Mrs E. A., Gatehurst, Gate End, Northwood, Middlesex.
Glasson, J. W., I.C.S., Biniaraitx, Soller de Mallorca, Baleares, Spain.
*Godden, Miss L. E., Heathfield, Lynchford Road, S. Farnborough, Hants.
*Goldney, Mrs, 157 Rivermead Court, Hurlingham, London, S.W. 6.
*Goldschmidt, Mrs de, Lane End, Burchetts Green, Berks.
Gooch, Dr G. P., South Villa, Campden Hill Road, London, W. 8.
*Gore, Miss K. C., Peacock Hall, Little Cornard, Sudbury, Suffolk.
Gough, A. B., Ph.D., Briar Cliff, Sevenoaks, Kent.
*Gower, Sir Robert, K.C.V.O., O.B.E., Sandown Court, Tunbridge Wells.
*Grant, Malcolm, The Mall House, Castle Townshend, Skibbereen, Co. Cork.
*Grant-Suttie, Colonel H. F., War Office, Whitehall, London, S.W. 1.
*Gray, Mrs E. F., Ripple Hall, Tewkesbury, Glos.

*Green, Lady, Gotwick Manor, East Grinstead, Sussex.
Green, Mrs Alan B., Acton Castle, Marazion, Cornwall.
*Greenwood, L. H. G., Emmanuel College, Cambridge.
*Gregory, C. C. L., F.R.A.S., 86 Friern Park, London, N. 12.
*Griffith, Mrs W. S. A., 19 Cheyne Walk, London, S.W. 3.
Grignon, Miss A. E., 41 Filton Avenue, Horfield, Bristol.
*Grisman, Instructor-Commander J. R., R.N., Selhurst, Gordon Road, Camberley, Surrey.
Grosvenor, Hon. Mrs Norman, 2 Upper Grosvenor Street, London, W. 1.
Grottendieck, W. G., rue v/d Noot 8, Brussels-Molenbeek, Belgium.
*Grubbe, Hubert H., 22 Park Lane, Southwold, Suffolk.
*Grugeon, C. L., The Chestnuts, Henley-on-Thames.
*Guénault, P. H., 31 Lyddon Terrace, Leeds.
*Gwyn, W. J., 8 Netherhall Gardens, London, N.W. 3.
Haldar, Professor Hiralal, P 49 Manicktolla Spur, Calcutta, India.
Hall, Wilfred, 9 Priors Terrace, Tynemouth, Northumberland.
*Hamilton and Brandon, The Duchess of, 25 St Edmund's Terrace, London, N.W. 8.
*Hamilton, Mrs T. Glen, 185 Kelvin Street, Winnipeg, Canada.
Hammond, Miss Winifred B., 2034 S.E. 51st Street, Portland, Oregon, U.S.A.
*Handley-Seymour, Major J. B., 1 Wadham Gardens, London, N.W. 3.
*Hannen, E. C., Ouseleys, Wargrave, Berks.
*Hanson, Mrs, The Pleasaunce, 4 Grassington Road, Eastbourne.
*Harding, Norman, 18 North Common Road, London, W. 5.
*Hare, Dr A. W., 59 York Road, Birkdale, Lancs.
*Harrington, E. J., Greensand, Heath Road, Petersfield, Hants.
Harris, Alan C., c/o Messrs. Morgan, Harjes & Co., 14 Place Vendome, Paris, France.
Harris, Mrs W. F., 13 Westbourne Avenue, Hull.
*Harrison, V. G. W., Ph.D., Beverley Court, 23 Queen's Gate, London, S.W. 7.
Harrison, William, Downs View, Tattenham Corner, Epsom.
Hart, Mrs H. H., The Old School House, Bodninick-by-Fowe, Cornwall.
*Hart, Hornell, Ph.D., 72 Sherman Street, Hartford, Conn., U.S.A.
*Haslam, Oliver H., Cairngill, nr Dalbeattie, Kirkcudbrightshire.
*Hawkins, J. Gordon, 480 Ellis Street, San Francisco, Cal., U.S.A.
*Hayes, Rev. J. W., Towerville, The Crescent, Loughton, Essex.
*Head, Mrs Geoffrey, 24 Charles Street, London, W. 1.
*Heard, Rev. A. St J., The Rectory, Caterham, Surrey.
*Heard, Gerald, 28 Portman Court, London, W. 1.
Heaton, Guy, 51 Westcliff Road, Bournemouth.
Hemenway, Mrs Augustus, Readville, Mass., U.S.A.
*Henderson, Miss Hester M., M.B., 25 Ferndale, Tunbridge Wells, Kent.
Henderson, J. J., c/o Mrs Ditges, Freehold, Greene County, New York, U.S.A.
Henderson, Miss Lilian, North Lane Gables, The West Gate, Canterbury.
*Herbert, C. V. C., 3 King's Mansions, Lawrence Street, London, S.W. 3.

*Hercod, Ernest, 38 Avenue de Rumine, Lausanne, Switzerland.
*Hettinger, John, Broseley, 63 Drewstead Road, London, S.W. 16.
*Hichens, Mrs W. Lionel, North Aston Hall, Oxfordshire.
Hildyard, F. W.
*Hill, Miss Marianne, Claremont, Thornton, Bradford, Yorks.
Hoare, Fred H., 37 Fleet Street, London, E.C. 4.
*Holdsworth, H. H., Westholme, Sandal, Wakefield, Yorks.
*Hole, Rev. Donald, 35 Selby Road, Ealing, London, W. 5.
*Hollick, Captain A. J., Kelsall Lodge, Sunninghill, Ascot, Berks.
*Hollick, Mrs A. J., Kelsall Lodge, Sunninghill, Ascot, Berks.
Hollins, Mrs A. E., Dunsfold Rectory, Godalming, Surrey.
*Holm, Knut H., c/o Anderson, Clayton & Co., Barranqueras (Chaer), Argentine.
Hookham, Philip, Shottery Cottage, Shottery, Stratford-on-Avon.
*Hope, Lord Charles M., 26a North Audley Street, London, W. 1.
*Hoppe-Moser, Dr Fanny, Franz Josefstr. 19, Munich, Bavaria.
*Hoseason, A. G., The Bungalow, Tanworth-in-Arden, nr Birmingham.
Hotblack, Frank A., Great Frenches Park, Crawley Down, Sussex.
Howden, Mrs, 11 Eton Terrace, Edinburgh.
*Howe, E. Graham, M.B., B.S., 146 Harley Street, London, W. 1.
*Howell, Mrs Philip, 5 Carlyle Square, London, S.W. 3.
Howgrave-Graham, Major A., 116 Johnston Street, Pretoria, S. Africa.
*Hume, Lieut.-Colonel W. J. P., C.M.G., Brinksway, Lynchmere, Sussex.
Hume-Rothery, J. H., Mendip House, Headington Hill, Oxford.
Hurwitz, W. A., Ph.D., White Hall 8, Ithaca, N.Y., U.S.A.
Hutchinson, F. W. H., Grove Lawns, St Albans, Herts.
*Hutchinson, Mrs, Spelmonden, Goudhurst, Kent.
*Huxley, Professor Julian S., Zoological Society of London, Regent's Park, London, N.W. 8.
*Hydari, Rt Hon. Sir Akbar, P.C., Dilkusha, Hyderabad, Deccan, India.
*Hyland, C. W., Ph.C., 300 Commissioner Street, Johannesburg East, Transvaal, S. Africa.
*Hynes, Miss G., 41 Haverstock Hill, London, N.W. 3.
Imamura, Prof. Shinkichi, Clinic for Psychiatry, Imperial University, Kyoto, Japan.
*Innes, Lady Rose, Kolara, Gibson Road, Kenilworth, Cape, S. Africa.
*Irving, Rev. W. S., Oxenhall Vicarage, Newent, Glos.
*Istituto di Studi Psichici, Via Monforte No. 4, Milan, Italy.
*James, Mrs Bayard, 405 East 54th Street, New York, U.S.A.
*James, Colonel E. A. H., R.E., British Embassy, Tokyo, Japan.
James, Henry, 36 West 44th Street, New York City, U.S.A.
*James, Miss S. Boucher, 2 Whitehall Court, London, S.W. 1.
*Jameson, David.
*Janson, E. W., Balfour House, 119-125 Finsbury Pavement, London, E.C. 2.
*Jay, Miss G. de L., Aynho, Station Road, Nailsea, Bristol.
Jaye, William R., Beldornie Tower, Pelham Field, Ryde, I.W.
*Jephson, Miss Ina, Ladyoak, Flimwell, Kent.

Johnson, Miss F. C., 25 York Street Chambers, London, W. 1.
*Johnson, Miss G. M., 32c Harrington Gardens, London, S.W. 7.
*Johnson, James MacNeill, Aberdeen, North Carolina, U.S.A.
*Johnston, Sir Reginald F., K.C.M.G., C.B.E., LL.D., Eilean Righ, Loch Craignish, Kilmartin, Argyll.
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*Johnston, Mrs W. B., Giverny, par Vernon, Eure, France.
*Jones, Professor B. Melvill, Engineering Laboratory, Cambridge.
*Jones, Sir Lawrence J., Bart, 39 Harrington Gardens, London, S.W. 7.
*Jones, Lady, 39 Harrington Gardens, S.W. 7.
*Jones, Lawrence E., 59 Pall Mall, London, S.W. 1.
*Judah, Noble B.
*Jyotirbhusan, L. M., Rangpur, North Bengal, India.
*Kakucs, Baroness de, The Estate House, Heytesbury, Wilts.
Keeble, Lewis B., Highworth, Byng Road, Tunbridge Wells.
*Kennedy, Miss H. E., St Margaret's, Wick Lane, Felpham.
*Ketner, Dr C. H., Den Helder, Holland.
*Khakhar, Dr H. M., Khakhar Buildings, C.P. Tank Road, Bombay, India.
*Kingsley, Mrs, The Knoll, Silverhill Park, St Leonards-on-Sea.
*Kingston, Leonard J., 22 Spencer Park, London, S.W. 18.
*Kiralfy, G. A., M.B.E., 47 Lowndes Square, London, S.W. 1.
Knight, Charles N., 7 Marlborough Buildings, Bath.
Laing, R. M., "Ogilvie," 37 Macmillan Avenue, Christchurch, New Zealand.
Lamb, Charles George, D.Sc., 65 Glisson Road, Cambridge.
*Lambert, G. W., 64 Onslow Gardens, London, S.W. 7.
*Lambert, Mrs Helen C., 12 East 88th Street, New York City, U.S.A.
*Lambert, Rudolf, Haigst 42, Degerloch bei Stuttgart, Germany.
Leaf, Mrs A. H., Woodcroft, Oxted, Surrey.
Leaf, Miss E. M., Leafield, Augustus Road, London, S.W. 19.
*Leaf, F. A., West Acre, Harrow-on-the-Hill, Middlesex.
*Le-Apsley, James H. M., M.D., 1340 Highview, Kaimuki Honolulu, Hawaii.
Lee, Blewett, P.O. Box 152, Station C., Georgia, U.S.A.
*Lee, Dr H. D. C., 32 New North Road, Huddersfield, Yorks.
*Lee, Roger I., M.D., 264 Beacon Street, Boston, Mass., U.S.A.
*Lees, Norman D., A.I.C., Norton Hall, Norton-on-Tees, Co. Durham.
*Leggett, Douglas M. A., Dytchleys, Coxtie Green, Brentwood, Essex.
*Lemon, Mrs, 8 Bryanston House, Dorset Street, London, W. 1.
Leon, Mrs Philip, 4M Montagu Mansions, London, W. 1.
Leopold, Dr H. M., Oranjelaan 7, Hilversum, Holland.
*Leslie, Mrs J., Stragglethorpe Hall, Brant, Broughton, Norfolk.
*Lester, Mrs M. C., Keynes Place, Horsted Keynes, Sussex.
*Lewis, David J., 328 Fayette Street, Cumberland, Maryland, U.S.A.
Librarian, Public Library, Adelaide, S. Australia.
*Librarian, Adyar Library, Adyar, Madras, S. India.
Librarian, Amsterdam Free Library, Amsterdam, N.Y., U.S.A.

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Librarian, Selskabet for Psykisk Forskning, Ny Vestergade 7, Copenhagen, Denmark.
*Librarian, Iowa State Library, Des Moines, Iowa, U.S.A.
*Librarian, Glasgow Society for Psychical Research, 102 Bath Street, Glasgow.
Librarian, The University, Glasgow.
Librarian, Pennsylvania State Library, Harrisburg, Pa., U.S.A.
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Librarian, Public Library, Newcastle-upon-Tyne.
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Librarian, Public Library, Jersey City, New Jersey, U.S.A.
Librarian, Public Library, New York, U.S.A.
*Librarian, Public Library, Omaha, Nebraska, U.S.A.

Librarian, Norsk Selskap for Psykisk Forskning, Parkveien 49, Oslo, Norway.
Librarian, Leland Stanford Junior University, Palo Alto, Cal., U.S.A.
Librarian, The University of Pennsylvania, Philadelphia, Pa., U.S.A.
*Librarian, Public Library of Philadelphia, Middle City Station, Philadelphia, Pa., U.S.A.
Librarian, Natal Society, Pietermaritzburg, Natal, S. Africa.
*Librarian, The University, Reykjavik, Iceland.
*Librarian, Public Libraries, Rochdale, Lancs.
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Librarian, Mercantile Library Association, St Louis, Mo., U.S.A.
*Librarian, James Jerome Hill Reference Library, St Paul, Minn., U.S.A.
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Librarian, Public Library of New South Wales, Sydney, Australia.
*Librarian, University of Illinois, Urbana, Illinois, U.S.A.
Librarian, Library of Congress, Washington, D.C., U.S.A.
Librarian, Public Library, Washington, D.C., U.S.A.
Librarian, Wellesley College, Wellesley, Mass., U.S.A.
*Littlewood, Professor J. E., Trinity College, Cambridge.
*Llewellin, Mrs W., Upton House, nr Poole, Dorset.
*Lloyd, Miss Edyth M.
Lloyd, Miss Julia.
*Lloyd-Jones, Mrs, 104 Draycott Avenue, Kenton, Middlesex.
*Lodge, F. Brodie, Floore House, Floore, Northants.
*Lodge, Mrs F. Brodie, Floore House, Floore, Northants.
*Lodge, Sir Oliver, F.R.S., Normanton House, Lake, nr Salisbury.
Lubbock, Mrs Geoffrey, Glenconner, North Berwick.
Lyon, Mrs, 49 Holland Park, London, W. 11.
*Lyttelton, Hon. Mrs Alfred, G.B.E., 18 Great College Street, London, S.W. 1.
*Maby, J. Cecil, Bourton-on-the-Hill, nr Moreton-in-Marsh, Glos.
Macdonald, Miss Isabelle M., M.B., 47 Seymour Street, London, W. 1.
*Mace, C. A., M.A., 12 Cavendish Road, London, N.W. 8.
*Machin, Mrs H. A. C., Kenora, Ontario, Canada.
Mackay, N. Douglas, M.D., Dall-Avon, Aberfeldy, Perth.
*Mackenzie, Mrs J. O., 17 Great Cumberland Place, London, W. 1.
*Mackeson, Mrs Peyton, 1 Eldon Road, London, W. 8.
Macklin, Miss H. E., 23 Norland Square, London, W. 11.
Madders, Mrs H. F., 87 Hampstead Way, London, N.W. 4.
*Magnus, Mrs Laurie, 34 Cambridge Square, London, W. 2.
*Magrane, Mrs Victor, 4 Grand Parade, Portsmouth.
*Mahony-Jones, Mrs, M.B., 1 Culverden Gardens, Tunbridge Wells.
*Mallet, E. Hugo, 14 St. James's Square, Bath.
*Malone, Dr Wilfred, 1 Alleyn Park, West Dulwich, London, S.E. 21.
*Mander, Geoffrey Le Mesurier, Wightwick Manor, Wolverhampton.

*Manning, Miss H. T., 1009 Kenyon Avenue, Plainfield, New Jersey, U.S.A.
Mansell, A. E., Bundella, Dromedary, nr Hobart, Tasmania.
Mantell, Colonel A. M., 5 St James's Square, Bath.
*Manuel, Alexander G., M.D., 110 William Street, New York, U.S.A.
*Marsden-Smedley, Mrs, Lea Green, Matlock, Derbyshire.
*Marshall, Miss G. F., Enfield Lodge, Pluckley, Kent.
Marston, Sir Charles, 4 Camden Park, Tunbridge Wells, Kent.
Marten, Miss A. R., Osbornes, Liphook, Hants.
*Marten, Ven. G. H., The Rectory, Godstone, Surrey.
*Mather, Rev. Herbert, Royal Automobile Club, Pall Mall, London, S.W. 1.
*Matthews, Very Rev. W. R., Dean of St. Paul's, London, E.C. 4.
*Maxwell, Sir John Stirling, Bart., Pollok House, Pollokshaws.
Maxwell, Dr Joseph, 37 rue Thiac, Bordeaux, France.
Mayor, R. G., 36 Campden Hill Gardens, London, W. 8.
McConnel, H. W., M.B., M.R.C.S.
McConnel, Mrs H. W.
*McDougall, Miss C. J., Appleton-le-Moors, York.
McDougall, William, F.R.S., D.Sc., M.B., Wellwick House, Wendover, Bucks.
*MacIntyre, Donald, Moanalua, Honolulu, Hawaii.
McKeever, Buell, The Chicago Club, Michigan Ave. and Van Buren Street, Chicago, Ill., U.S.A.
McLauchlan, G. M., c/o Dr J. J. Dunne, Port Alfred, C.P., S. Africa.
Meck, Maximilian de, 93 Kenilworth Avenue, Wimbledon, London, S.W. 19.
Meebold, Alfred, Heidenheim, Wurtemberg, Germany.
Mehrji, M. H., M.D., Yusuf Building, Esplanade Road, Fort, Bombay, India.
*Mellor, Miss J. V., 26D Ladbroke Gardens, London, W. 11.
*Merritt, O. K., Mt. Airy, North Carolina, U.S.A.
*Micklethwait, Richard K.
*Millard, Mrs Almira B., 269 Canyon Crest Road, Altadena, Calif., U.S.A.
*Miller, G. B., Brentry, Romsey, Hants.
*Minns, Christopher, Hammondswood Cottage, Frensham, Surrey.
*Minns, Mrs C., Hammondswood Cottage, Frensham, Surrey.
*Mitchell, T. W., M.D., Hope Meadow, Hadlow, Kent.
*Morris, Miss H. L., 6 Fore Hill, Ely, Cambs.
*Mortimer, Mrs Stanley, 4 East 75th Street, New York City, U.S.A.
*Morton, Dr Eva, 24 Park Crescent, Portland Place, London, W. 1.
*Mosher, Mrs Howard T., 216 Alexander Street, Rochester, N.Y., U.S.A.
*Moss, C. A., 277 Hamlet Court Road, Westcliff-on-Sea, Essex.
*Mounsey-Wood, Mrs, 16 Alexandra Road, Reading, Berks.
*Moxey, Louis W., 230 N. Camac Street, Philadelphia, Pa., U.S.A.
*Muir, Mrs W. E., Rowallan, Haslemere, Surrey.
*Mullins, Colonel W. B., Ambersham House, Midhurst, Sussex.
*Mumford, Captain W. C., Sugwas Court, Hereford.
*Mürer, Johan, Furnlundsvei 7, Bestum, Oslo.
*Murphy, Professor Gardner, Ph.D., Columbia University, N.Y., U.S.A.

Murray, Professor Gilbert, LL.D., Litt.D., Yatscombe, Boars Hill, Oxford.
*Myers, Harold H., Ovington House, Ovington Square, London, S.W. 3.
*Myers, L. H., 19 St. James's Square, London, S.W. 1.
*Narain, Narsingh, Hardoi, U.P., India.
*Nash, Miss Diana, Point of Pines, Tryon, N. Carolina, U.S.A.
*Naumburg, Miss Margaret, c/o Messrs Straus & Kenyon, 475 Fifth Avenue, New York City, U.S.A.
Neustadter, Louis W., 6845 Odin Street, Hollywood, Cal., U.S.A.
Newton, Miss F. E.
Newton, Miss I., 31 Tavistock Square, London, W.C. 1.
*Newton-Davis, Mrs, M.B., 8 Temple Gardens, London, N.W. 11.
*Nicholl, Iltyd B., The Bath Club, 34 Dover Street, London, W. 1.
*Nicholson, Mrs Scoble, Gatehurst, Northwood, Middlesex.
*Nicol, J. Fraser, 2 Observatory Road, Edinburgh, 9.
*Nicoll, Mrs De Lancey.
*Nisbet, B. C., 42 Iverna Court, London, W. 8.
*North, Sidney V., Poste Restante, Southwick, Sussex.
*Odell, A. E., 10 Knights Park, Kingston-on-Thames, Surrey.
*Oldfield, Miss F., The Glen, Farnborough Park, Kent.
*Oliver, C. R., 61 Courtfield Gardens, London, S.W. 5.
*Osborn, A. W., Elizabeth Street P.O., Melbourne, Australia.
*Osborne, Miss H., 10 Empire House, London, S.W. 7.
*Osmaston, Dudley F., Lowfold, Wisborough Green, Sussex.
*Owen, A. S., Keble College, Oxford.
*Paget, Captain A. W. L., 19 Bryanston Square, London, W. 1.
*Paget, Mrs A. W. L., 19 Bryanston Square, London, W. 1.
*Palmer, John W. G., 13 New Road, Brighton, Sussex.
*Palmstierna, H.E. Baron, 27 Portland Place, London, W. 1.
*Parkin, John, Blaithwaite, Carlisle.
Parsons, N. M., 65 Bedford Gardens, London, W. 8.
*Parsons, Miss P. C., The Drift, Cave End, Reading, Berks.
Paul, J. Rodman, 505 Chestnut Street, Philadelphia, Pa., U.S.A.
*Payne, Mrs, M.B., C.B.E., 143 Harley Street, London, W. 1.
*Peake, C. W.
Pease, Mrs J. R., 82 Queen's Gate, London, S.W. 7.
*Pennington, Mrs Henry, 279 Trinity Road, London, S.W. 12.
Percival, Philip E., Old Priory, Brightwell, nr Wallingford, Berks.
Perkins, Miss S. R., 74 Princes Square, London, W. 2.
Perry, Sir E. Cooper, M.D., Seighford, Mill Road, West Worthing, Sussex.
*Phillimore, Hon. Mrs, Kendals Hall, Radlett, Herts.
*Phillimore, Miss M., 16 Queensberry Place, London, S.W. 7.
Phillips, Mrs C. G., Kazamgula, P.O. Sinska Bridge, George, C.P., S. Africa.
*Pickard, Mrs Fortescue, c/o Guaranty Trust Co. of N.Y., 50 Pall Mall, London, S.W. 1.
*Piddington, J. G., Fishers Hill, Woking, Surrey.
*Piercy, Major B. H., 94 Piccadilly, London, W. 1.

*Pierson, Miss Jocelyn, Sterlington, New York, U.S.A.
Pigou, Professor Arthur Cecil, King's College, Cambridge.
*Pilcher, Mrs G. T., Treen, Frith Hill, Godalming, Surrey.
*Pillai, R. B., c/o Thos. Cook & Son, Berkeley Street, London, W. 1.
Piper, John E., LL.B., 10 Herondale Avenue, London, S.W. 18.
Pithapuram, H.H. The Maharajah of, Pithapuram, Madras Presidency, India.
*Plimmer, Mrs R. H. A., 52 The Pryors, East Heath Road, London, N.W. 3.
*Pocock, Miss F. N., 34 Gerard Road, Barnes, London, S.W. 13.
*Pollock, A. N., M.B., Ch.B., 4 Ventnor Villas, Hove 3, Sussex.
*Power, F. Danvers, 25 Woodside Avenue, Burwood, N.S.W., Australia.
Powles, Lewis Charles, Little Cliff, Rye, Sussex.
*Preston, E. M., Slaugham Park, Haywards Heath, Sussex.
*Price, Harry, Arun Bank, Pulborough, Sussex.
*Price, H. H., Trinity College, Oxford.
Pym, Leslie R., Penpergwm Lodge, Abergavenny, Wales.
*Quinby, Rev. John W., East Bridgewater, Mass., U.S.A.
*Rabb, A. L., 1350 Consolidated Building, Indianapolis, Ind., U.S.A.
*Radcliffe-Whitehead, Ralph.
*Radclyffe-Hall, Miss M., The Black Boy, Rye, Sussex.
*Raikes, C. S. M., Northlands, 124 College Road, London, S.E. 19.
Ramsden, Miss, Marley House, Haslemere, Surrey.
*Rashleigh, John C. S., M.D., Throwleigh, Okehampton, Devon.
*Rattey, C. C., Downside, Merrow, Guildford.
*Rayleigh, Lord, Terling Place, Chelmsford, Essex.
*Rees-Roberts, J. V., F.R.S., M.D., 90 Fitzjohn's Avenue, London, N.W. 3.
*Reeves, E. A., F.R.A.S., 56 Hillway, Highgate, London, N. 6.
*Reeves, Mrs M. S., 31 Pembroke Square, London, W. 8.
Rendall, Rev. Dr G. H., Dedham House, Dedham, Essex.
*Rendell, Francis G., 19 The Drive, Henleaze, Bristol.
*Renwick, A. E., St-y-Nyll, St Brides-super-Ely, Glamorgan.
*Reutiner, Miss A. H., Fountain Court, Westminster, London, S.W. 1.
*Rhine, J. B., Ph.D., Duke University, Durham, N. Carolina, U.S.A.
*Richmond, Mrs Kenneth, 82 North End Road, London, N.W. 11.
*Rickman, John, M.D., 11 Kent Terrace, London, N.W. 1.
*Riddle, Mrs, Hillstead, Farmington, Conn., U.S.A.
*Ridley, Henry N., F.R.S., C.M.G., 7 Cumberland Road, Kew, Surrey.
*Rinehart, Mrs Stanley M., 630 Park Avenue, New York City, U.S.A.
*Ripon, Rt Rev. The Bishop of, The Palace, Ripon.
*Ritchie, A. J., Oriental Club, Hanover Square, London, W. 1.
*Ritson, F. A., 10 Rubislaw Den South, Aberdeen.
Riviere, Mrs Evelyn, 4 Stanhope Terrace, London, W. 2.
*Robson, Major J. S., Hales Place, Tenterden, Kent.
*Rogers, George F., M.D.
*Romanes, F. J., The Brick House, Duton Hill, Dunmow, Essex.
*Ross, Robert, British Consulate General, 360 N. Michigan Avenue, Chicago, Ill., U.S.A.

*Rothschild, Miss Miriam L., 4 Palace Green, London, W. 8.
*Röthy, C., 1 Zuhatag-Gasse 5, Budapest, Hungary.
*Rowntree, W. S., 15 Chatsworth Road, Brighton, Sussex.
*Russell, Dr A. V., 4 Oaks Crescent, Wolverhampton.
*Ryan, Mrs.
*Ryley, Mrs Beresford, 37 Victoria Road, London, W. 8.
*St. Aubyn, Hon. Mrs., 61 Onslow Gardens, London, S.W. 7.
*Salter, F. R., Magdalene College, Cambridge.
*Salter, W. H., The Crown House, Newport, Essex.
*Salter, Mrs W. H., The Crown House, Newport, Essex.
*Saltmarsh, H. F., Woodcote, Lynton, N. Devon.
*Saltmarsh, Mrs H. F. Woodcote, Lynton, N. Devon.
Samaldas, Hon. Sir Lalubhai, 99 Apollo Street, Fort, Bombay, India.
*Sassoon, Mrs Alfred, Weirleigh, Matfield Green, Kent.
*Sassoon, Mrs Meyer, 6 Hamilton Place, London, W. 1.
Savill, Mrs, M.D., 7 Devonshire Place, London, W. 1.
*Scotland, Douglas C., L.R.C.P., Church Lane, Brighouse, Yorks.
Scott, Miss A. D., 60 Hornsey Lane, London, N. 6.
Scott, Rev. D. D., C.F., The Manse, Khandallah, Wellington, New Zealand.
Scott, Captain J. E., c/o Barclays Bank, 140 King's Road, London, S.W. 3.
*Selborne, The Earl of, K.G., Blackmoor, Liss, Hants.
Shastri, B. G., Kala Mehta's Street, Sagrampura, Surat, India.
*Shaw, Mrs Bernard, 4 Whitehall Court, London, S.W. 1.
*Shearer, F. Myers, c/o I. J. Butcher, Kither's Buildings, King William Street, Adelaide, S. Australia.
*Siepmann, Mrs, 4 Wells Road, Regent's Park, London, N.W. 8.
Simpson, Miss E. C. Price, Beech Barns, Alton, Hants.
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*Sitwell, Mrs, 114 Grosvenor Road, London, S.W. 1.
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*Smith, Marion, 80-11th Street, N.E., Atlanta, Georgia, U.S.A.
*Smith, The Lady Sybil, Mitford House, Lennox Gardens, London, S.W. 1.
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*Soal, S. G., Scratton Lodge, Brook Road, Prittlewell, Essex.
Soley, Mrs, 66 Holbein House, Sloane Square, London, S.W. 1.
*Sorabji, K. S., 175 Clarence Gate Gardens, London, N.W. 1.
Southern, H., 3 Crescent Road, Beckenham, Kent.
*Sowrey, Wing-Commander J., R.A.F., Yeoveney, Staines, Middlesex.
*Spender, J. A., Well Hill House, Chelsfield, Farnborough, Kent.
Spens, William, Corpus Christi College, Cambridge.
*Spinney, G. H., 4 Overhill Gardens, London, S.E. 22.
*Spranger, John A., 4 Via Micheli, Florence, Italy.
*Sprott, W. J. H., 116 Portland Road, Nottingham.

*Standaart, Dr A. W. J., 382 Mathenesserlaan, Rotterdam, W., Holland
*Stansfield, C. E., 70 Northcourt Avenue, Reading, Berks.
*Steane, G. A., 5 Queen Victoria Road, Coventry.
Stephens, W. F., Mahé, Seychelles, Indian Ocean.
*Sterling, Miss F. M., Home Wood, Hartfield, Sussex.
*Stevens, Rev. W. H., Eastville, Haslingden, Lancs.
Stevenson, Mrs A. F., 72 Heath Street West, Toronto, Canada.
Stevenson, A. Creery, Rose Cottage, Farnborough, Hants.
Stewart, Miss M.A., Queen Anne's Mansions, London, S.W. 1.
Stoehr, Miss, Alexandra Club, Cape Town, S. Africa.
*Strachey, Mrs J. St Loe, 39 St Leonard's Terrace, London, S.W. 3.
*Strange, T. A., Christowe, Minchinhampton, Glos.
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*Strawson, A. H., 27 Norfolk Road, London, N.W. 8.
*Strutt, Rear-Admiral the Hon. A. C., R.N., 1 Cambridge Square, London, W. 2.
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*Strutt, Hon. Charles R., Terling Place, Chelmsford, Essex.
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*Thaw, A. Blair, 3255 N. Street, Washington, D.C., U.S.A.
*Thibodeau, William A., 20 Chapel Street, Brookline, Mass., U.S.A.
*Thomas, Rev. C. D., South Hill Lodge, Bromley, Kent.
Thomas, Miss Edith J., Nantyfelin, Criccieth, N. Wales.
*Thomas, Mrs Gale, 3 Morland Close, Hampstead Way, London, N.W. 11.
*Thomas, John F., Ph.D., 18295 Oak Drive, Detroit, Michigan, U.S.A.
*Thompson, Mrs E. Roland, Oak Hayes, Crewkerne, Somerset.
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*Thomson, Lady, Bell Cottage, Kynance Mews, London, S.W. 7.
*Thorburn, John M., University College, Cathays Park, Cardiff.
Thornley, Miss F. J., The Pantiles, Brean Don Avenue, Weston-super-Mare.
Thornton, Mrs, 5 Belgrave Place, Edinburgh.
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Thurston, Rev. Herbert, S.J., 114 Mount Street, London, W. 1.
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Tipping, Miss K., 7 Lansdowne Circus, Leamington.
Tottenham, Miss Mary T. A., Ballycurry, Ashford, Co. Wicklow, I.F.S.
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*Tyrrell, G. N. M., 32c Harrington Gardens, London, S.W. 7.
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*Vandy, G. E., 94 Essex Road, London, E. 12.
*Van Deren, H. S., Hume Fogg Building, Nashville, Tenn., U.S.A.
Van Renterghem, A. W., M.D., 298 Heemraad-Singel, Rotterdam, Holland.
*Varvill, Bernard, M.R.C.S., 92 Harley Street, London, W. 1.
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*Warburton, Mrs J. R., Arley, Rydens Road, Walton-on-Thames, Surrey.
Ward, Hon. Kathleen, Moorings, Menai Bridge, Anglesey.
*Warner, Hon. Mrs W. W., 52 Hans Place, London, S.W. 1.
Warrender, Miss Margaret, 50 Wilton Crescent, London, S.W. 1.
*Warrick, F. W., 6 Raymond Buildings, London, W.C. 1.
*Watkins, Miss K. E., 53 All Souls' Avenue, London, N.W. 10.
*Watts, Mrs James, Branton, Bollin Hill, Wilmslow, Manchester.
*Weldon, Arthur, 23 Westminster Palace Gardens, London, S.W. 1.
*Wellesley, Sir Victor A. A. H., C.B., 12 Ranelagh Grove, London, S.W. 1.
*Wendt, Mrs Henry, 126 Chapin Parkway, Buffalo, N.Y., U.S.A.
*West, M., Cornerways, Baughurst, nr Basingstoke, Hants.
Whitaker, Mrs Joseph J. S., Villa Malfitano, Palermo, Sicily.
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Wilkins, Mrs, 13 Buckleigh Road, Streatham, London, S.W. 16.
Wilkins, C. F., Three Trees, Hillingdon, Middx.
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*Wilson, Mrs C. Stuart, Mena House, The Pyramids, Cairo, Egypt.
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*Wilson, S. R. W., Two Fields, Whelpley Hill, Chesham, Bucks.
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*Winby, Lieut.-Col. L. P., 11 Trevor Square, London, S.W. 7.
*Winchilsea and Nottingham, Edith Countess of, Dower House, Ewerby, Sleaford, Lincs.

*Winterstein, Dr Baron Alfred von, Wattmanngasse 38, Vienna xiii, Austria.
*Wisdom, John, 16 Clarendon Road, Oxford.
*Wodehouse, Miss Helen M., Girton College, Cambridge.
*Wood, Christopher, 28 Portman Court, London, W. 1.
*Wood, Mrs St Osyth, 105 Hallam Street, London, W. 1.
*Wood, T. Eugène, Redcliffe, 14 Chine Crescent Road, Bournemouth.
Woodhull, Miss Zula M., Norton Park, Bredon's Norton, nr Tewkesbury, Glos.
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Woods, Miss C. E., Graythorpe, Kingswood, Surrey.
Woollett, Lieut.-Colonel W. C., Grand Hotel, Gibraltar.
*Woolley, Mrs Cornell, 950 Park Avenue, New York, U.S.A.
*Worsfold, Mrs Basil, 3 Plowden Buildings, Temple, London, E.C. 4.
*Worcester, Dr Elwood, 186 Marlborough Street, Boston, Mass., U.S.A.
Wrangham, W. H., 78 Barmouth Road, London, S.W. 18.
*Wright, Dr A. F.
*Wright, H. R., 15B St Mildred's Road, Lee, London, S.E. 12.
*Wright, Maurice B., M.D., 86 Brook Street, London, W. 1.
*Yardley, R. B., 3 Paper Buildings, Temple, London, E.C. 4.
Younghusband, Sir Francis, Currant Hill, Westerham, Kent.
*Zeigler, Major C. H., Springfield, Breinton, Hereford.

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For the sake of brevity such qualifications as "supposed," "alleged," etc., are omitted from this index. It must, however, be understood that this omission is made solely for brevity, and does not imply any assertion that the subject-matter of any entry is in fact real or genuine.

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