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PROCEEDINGS

OF THE

Society for Psychical Research

VOLUME XLII

(CONTAINING PARTS 133-138)

1934

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PART I

PERIODICAL, SERIAL, SOCIETY AND OFFICIAL PUBLICATIONS

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——— [Album of foreign portraits.] 4to, ff. 14. [R8 i 17]

[Containing portraits of William James, Richard Hodgson, H. P. Bowditch, W. Sturgis Bigelow, S. P. Langley, Edward C. Pickering, G. Stanley Hall, John C. Bundy, A. Blair Thaw, James H. Hyslop, Nicholas Murray Butler, E. E. Crepin, A. N. Aksakov, Alfred Backhaus, Sydney Alrutz.]

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PROCEEDINGS

OF THE

Society for Psychical Research

PART 134

REPORT ON CASES OF APPARENT PRECOGNITION

BY H. F. SALTMARSH

THE phenomenon of supernormal precognition is generally held to be one of the most puzzling, the most mysterious of all the mysteries which are presented to the psychical researcher. That a human being, conditioned in space and time, should be able under certain rather rare and exceptional circumstances to acquire knowledge of future events raises problems of the utmost importance, not only for psychical research, but also for philosophy and metaphysics and possibly for physical science itself, for it seems obvious that the solution of this problem must somehow involve a conception of the nature of time.

Obviously the first point to be determined is whether precognition ever actually occurs.

There can be no doubt that precognition of a sort is a reality ; we can foretell many future events with more or less certainty. The astronomer predicts the movements of stars and planets, the chemist knows in advance the reactions of his reagents, the meteorological office, though not so conspicuously successful, achieves a percentage of true forecasts which is in excess of what might be attributable to pure chance.

In the psychical realm we can sometimes predict a man's behaviour under given circumstances ; we are even more successful when dealing with crowds and the statistical determination of the annual figures of such things as marriage, murder and suicide, all of which are at least partially psychically determined, show a surprising degree of accuracy.

Now all these precognitions have one feature in common, they are based on inferences from past experience and rely on the assumption that the causes and conditions which have operated in the past will

continue to do so with more or less uniformity in the future. They are all instances of what may be called normal precognition. But psychical research has to do with the supernormal.

I have therefore made an examination of the records of the Society, as embodied in the *Journal* and *Proceedings*, from its beginning and have extracted and summarised all cases of precognition. It may be that I have overlooked some isolated examples of it which have occurred mixed up with other more striking phenomena or reported in a paper or article not specifically dealing with the subject, but I do not think that much of importance has been missed.

I have included all the cases I could find in which an element of precognition was apparent. As I shall show when I come to analyse them, some of these may not be true examples of supernormal precognition but may be susceptible of another explanation. Some I have had to reject altogether on the grounds that the appearance of precognition was either false or else too vague to be considered evidential; they are placed under the last two headings of my first classification, i.e. vague or non-precognitive. The remainder of the cases which are considered as susceptible of another explanation contain for the most part some supernormal element, such as telepathy or hyperaesthesia, and the explanation suggested requires the assumption that these phenomena have occurred.

It must, however, be noted that the alternatives suggested are never more than hypothetical and it may well be that if a final analysis should show good grounds for holding that supernormal precognition does sometimes occur, that interpretation may in many of the cases in question be preferred to the alternative. In the statistics which follow I have included these possibly dubious cases but have excluded those which, as I have said, I reject altogether.

The nature of the statistical facts is such that the question of alternative explanation has no relevance.

I have also studied the few papers which have been read on the matter, particularly those of Mrs Sidgwick and Mr Myers, and I here desire to acknowledge my indebtedness thereto.

Although a few examples of precognition may have escaped my net the haul is sufficiently impressive. I have been able to collect some 349 cases; some few of them are multiple, that is to say, recording more than one instance.

Having summarised these cases I then proceeded to analyse and classify them. I grouped them first under the type of impression and sub-divided each type into four headings, viz. Good, Ordinary, Vague and not precognitive.

A Good case is one where the precognition is particularly definite and detailed, or where the evidence is exceptionally good. An Ordinary case is one which, although it does not attain to the above

standard, is sufficiently evidential of precognition to be significant. The fact that it has been deemed worthy of publication is itself some guarantee for this.

I have classed as Vague those cases where there was nothing of a definite nature to indicate that a precognition had occurred or nothing conclusively to connect the event which is reported as fulfilling the prediction with the alleged precognition. For example : if A hears the ticks of a death watch and during the next few weeks a relative dies, that would be a vague case. Or if B has a vague impression of calamity or sees an unrecognised hallucinatory vision, and subsequently experiences some misfortune to which there was no obvious reference in the impression or hallucination, the case would come under this class.

The class of non-precognitive cases are those where there is reason to think that the event referred to was simultaneous with or prior to the dream, impression, hallucination, etc. or else that the whole thing was clearly attributable to mere chance.

The following is the result of the analysis :

	Dream.	Borderland.	Impression.	Hallucination.	Mediumistic.	Crystal.
Good - - - -	76	4	14	17	20	3
Ordinary - - - -	40	3	25	45	31	3
Vague - - - -	11	—	8	23	6	—
Non-precognitive - - - -	9	—	4	2	5	—
	136	7	51	87	62	6

Before discussing the figures a few remarks on the various headings should be made.

Borderland cases—a very small class—are those where the impression occurs between sleep and waking, so that it is impossible to say whether it was a dream or waking impression. I do not think that there is any particular significance in this division of classification, but it appeared to me desirable to make it for the sake of accuracy.

Impressions are, of course, waking impressions not attaining the status of definite hallucination.

Mediumistic cases include predictions made by recognised mediums together with a very few cases of precognition in hysteria and analogous states.

Crystal cases, another very small class, are precognitions occurring

while crystal-gazing, or during some other form of scrying. It is clear that the dividing line between the classes is not in some cases perfectly hard and fast. For example, crystal-gazing cases might well be included in the mediumistic class, or an impression might almost reach the point of hallucination.

Also there are cases where several types are joined together, e.g. a precognitive dream may be preceded or followed by an impression, or else A may have a dream while B experiences an hallucination, both apparently having the same reference. Although there is this unavoidable lack of definition in some cases the majority fell into their respective classes clearly and definitely, and I do not think that any conclusions which may be drawn from the figures would be materially affected by any re-arrangement which might possibly be made.

It will be observed that, with the possible single exception of waking impressions, all the classes occur in states wherein there is some dissociation. Even in the case of impression it is not unreasonable to suppose that some slight dissociation occurs, as there are instances where the impression was so overwhelming as almost to amount to hallucination. The impression comes into the mind, not by association with any of its supraliminal contents, but apparently as imposed on it from without; it possesses an unaccountable compulsive force. It cuts through and intrudes upon the normal flow of thought. Now, if all cases of precognition are accompanied by or take place during a state of dissociation, I think it may be concluded with a fair degree of certainty that the part of the mind involved is the subliminal.

This, perhaps, is not a very striking or original conclusion; in fact it is only what we should have expected *a priori*. I merely wish it to be put on formal record at the very outset of the inquiry as it may be found later on to have an important bearing on theory. To turn now to the figures themselves. It will be noticed that the dream is a long way the most prolific source of precognitions; moreover it is among dream cases that we find the highest percentage of good cases.

Hallucinations come next in point of number but fall rather low in the proportion of good to ordinary; moreover there is an unusually high percentage of vague cases. This, I think, is perfectly reasonable. From the nature of an hallucination the details given are not as a rule as full as with a dream, so that definite reference to a particular event, the absence of which renders a case liable to be relegated to the vague class, is less likely to be found and recorded. As examples of this one might cite the fairly numerous cases where the percipient hears unaccountable knocks or ticks and shortly afterwards suffers the loss by death of a near relation. The knocks may be a vague precognitive phenomenon not reaching to the height of definite knowledge, but in the absence of any indication

of the identity of the person to whom they refer, even where the percipient regards them as a warning of approaching death or bereavement, the cases are so nebulous and uncertain that they cannot be held to be of any value as evidence.

The next class, mediumistic, presents a better average of good cases, though still falling far short of dreams in this respect, while the percentage of vagues is roughly the same. As regards this class, however, I feel that little or no reliance can be placed on any conclusions based on numbers, for I cannot regard my collection as being in any way a fair sample of the total amount of precognitive matter found in the utterances of trance mediums. I should say that the number of mediumistic cases of precognition actually published bears a very small ratio to the total of such cases. From my own knowledge of Mrs Leonard's trance utterances, a knowledge which covers only a relatively small fraction of the whole, it appears to me that instances of apparent precognition are fairly frequent. They may not be of a very striking character, but this, from the theoretical point of view, is irrelevant. Precognition of the most dull and commonplace events demands explanations just as much as does the most startling prophecy.

Mediumistic cases possess the advantage of being able to give a greater amount of detail than is possible with hallucinations or impressions, though they fall short of dreams in this respect. This, subject to the uncertainty of conclusions drawn from the number available, is shown in the somewhat higher ratio of good to ordinary cases.

As regards impressions we have to take into account a special consideration. Precognitive dreams are, as a rule, of peculiarly impressive character, which renders them more liable to be remembered and recounted by the dreamer than are the ordinary normal dreams. While it is true, as some of us know to our cost, that there are people who habitually bore the breakfast table with accounts of their dreams, to the credit of humanity it must be admitted that such malefactors are comparatively rare. The dreams they tell are for the most part very dull and ordinary.

The peculiar characteristics possessed by the true precognitive dream induces the dreamer sometimes to record it in writing, sometimes to regard it as a warning to be acted upon, but more often to recount it to other people with special emphasis. Mediumistic precognitions are for the most part embodied in the records of sittings, while hallucinations are sufficiently uncommon and striking events to be specially noted and remembered. Impressions, however, may range from the vague feelings of disquiet which, I suppose, everyone is liable to experience, up to those so relatively massive and detailed as to amount almost to hallucination.

It would occur to no one to note and remember every passing impression, and thus it must happen that many which might possibly be precognitive are passed by and forgotten. Even of those which are remembered and recognised as having been fulfilled, many are likely to be deemed unworthy of being reported. It is only the most impressive that would be considered sufficiently interesting to be placed on record.

There are cases where the impression is definitely felt to concern some particular person, there are others where it is so strong as to be regarded as a warning to be acted upon. It is significant that there is a larger proportion of warning cases with impressions than with any of the other types.

I think that we are safe in assuming from these considerations that the reported cases of impression do not represent so high a percentage of the total number of such incidents as with dream or hallucination. One would have expected, *a priori*, that the proportion of good to ordinary would be lowest among impressions, whereas, though last but one in the list, it is considerably above hallucination.

I think that this may be accounted for by the fact that the poorer cases would be thought not worth reporting, or, if reported, not worth publishing.

As regards the remaining two types, viz. Borderland and Crystal-gazing, the number of these cases is so small that the figures can have no significance as statistics.

The percentage of cases rejected as non-precognitive has no interest.

We are left then with the following supply of material :

	Dream.	Borderland.	Impression.	Hallucination.	Mediumistic.	Crystal.
Good - - - -	76	4	14	17	20	3
Ordinary - - - -	40	3	25	45	31	3
	116	7	39	62	51	6

or 281 cases in all.

In rejecting the vague and non-precognitive cases I do so for safety's sake ; if, on the evidence of those remaining, supernormal precognition is found actually to occur, then some of these rejected cases may well have been real instances of it.

I have made a further classification of the good and ordinary cases under the headings of the type of incident to which the precognition refers. This is as follows :

	Dream.	Borderland.	Impression.	Hallucination.	Mediumistic.	Crystal.
Death from natural causes	28	2	3	29	10	—
Death from accident - -	9	—	4	5	6	3
Course of illness - - -	6	—	4	3	5	—
Accident not involving death - - - -	11	1	3	3	2	—
Accident to material things, such as fires, etc.	17	1	11	6	2	—
Trivial incidents - - -	35	—	6	5	21	3
Incidents not trivial - -	3	1	4	3	1	—
Arrival cases - - - -	5	1	2	8	2	—
Winning numbers, winners of races, etc. -	6	1	2	1	6	—
	120	7	39	63	55	6

It will be observed that the totals in this classification are in some instances slightly higher than the total number of cases given above. This is, of course, because some of the cases exhibit precognition of more than one type of incident.

There is a certain amount of ambiguity between some of these headings. For example, an accident to a material thing such as a fire or carriage accident, might be classed as such or as an incident, trivial or not, according to its nature. In actual practice, this particular ambiguity has not been felt. I have experienced no difficulty in classifying under these headings except perhaps, in a very few cases, certainly not sufficiently numerous materially to affect results.

Of course, the distinction between trivial and not trivial is very vague. However, in most cases no doubt arose. One would not class precognitions of the Great War under the heading of trivial incidents, or even such a matter as the failure of the bank wherein the dreamer had her money. On the other hand, the call of a commercial traveller selling books could hardly be called anything but trivial.

Under accidental death I have classed the few cases of death by murder.

Under arrival cases I have included arrivals of persons and messages, such as letters and telegrams. Under winning number cases, numbers drawn in lotteries, games of chance, also some cases of precognition of the numbers drawn by men called up for military service, precognition of cards drawn from a pack, winners of races and so on.

The other headings are, I think, sufficiently clear to need no comment.

There is perhaps one point concerning death from natural causes which should be mentioned. There are one or two cases where the actual cause of death was a natural one, but brought about or intensified as the result of an accident. In these I have used my discretion in deciding whether the accident or the illness was the predominant cause.

Taking all the types of precognition together, the most numerous of the incidents foreseen was death, either natural or accidental, being 72 natural and 27 accidental. Running it very close, however, come incidents, 70 trivial and 12 not trivial. It is, perhaps, worthy of notice that hallucinations give a far higher ratio of precognitions of death to those of incidents than do any of the other classes, and this is significant (see Appendix B). This seems explicable by the greater emotional tone involved, which may reasonably be held to have some connection with the generation of hallucination. The statistics of phantasms show conclusively that a state of crisis is somehow connected casually with their occurrence, and it appears likely that a similar nexus may exist with precognitions.

The ratio of death to incident in mediumistic cases is somewhat lower than with the other types; but, as was pointed out above, statistics in this class cannot be considered reliable.

I do not think that there are any other points in connection with these figures which merit special mention at this stage.

A further classification has been made in which the numbers of certain special types have been extracted. This is as follows:

	Dream.	Borderland.	Impression.	Hallucination.	Mediumistic.	Crystal.
Collective - - -	2	—	2	9	—	—
Recurrent - - -	6	—	1	5	2	—
Symbolic - - -	6	—	—	9	—	—
Warning - - -	12	2	9	10	1	—

Of these, collective dreams are more of a curiosity than of much practical importance. Neither of the two cases which I have are of the good class.

In the first, *Proceedings*, xiv. 253, Dr Howard and another student of Columbia University, both dream on the night before the boat-race that Columbia would win. Presumably they were all excited about the race, so that there is nothing very remarkable in two of them dreaming that the same crew would win. The full report of the case reads better than my bald abstract, but it is scarcely, if at all, beyond chance.

The second case, *Journal*, xiii. 118, Mr E. J. and his wife have apparently simultaneous dreams of his mother. In Mrs J.'s dream she is told that the mother will not live for another three months. Fulfilled. However, the mother was 83 years of age and expected to die at any time. Again a very poor case. In fact, neither of them would have been mentioned but for their interest as curiosities; as evidence of precognition they are almost worthless, apart from the small additional value afforded by the fact of collectivity.

It is conceivable that collective dreams might occur owing to telepathic suggestion from one dreamer to another, or by the operation of an external cause acting on both sleepers simultaneously, and it would be of interest to know if any such cases, whether precognitive or not, could be found.

Collective hallucinations are fairly common, so it is not surprising that some instances should be found among precognitive cases. The problems raised by the cases do not directly concern the matter of precognition. Whether they are due to telepathic suggestion from one of the percipients to the other, or to the operation of an external agency acting simultaneously on more than one person, need not be discussed here. The fact of collectivity, however, gives a certain added impressiveness to the phenomenon.

Recurrent cases, whether of dream or any other type, certainly give the impression of a persistent cause, either external or internal. A dream repeated two or three times is more likely to be remembered and noticed, and where the evidential conditions are good, the case gains greatly in impressiveness. It is true that the hypothesis of an illusion of memory may sometimes have a certain plausibility here, inasmuch as it might be suggested that the dream really occurred only once and that the apparent repetition was due to a false memory. However when dreams occur on different nights or at fairly long intervals on the same night, or even where the dreamer wakes and is sufficiently impressed by the first dream to note it particularly and be perturbed by it, this hypothesis is hardly tenable.

Apart from illusion of memory these cases are of particular value because the possibility of explanation by chance is much reduced.

It might be due to chance that I should dream of an event which is going to happen next day, but that I should do so twice running is far more unlikely.

That there should be some amount of symbolism, particularly among the dreams and hallucinations, is only what might have been expected. In fact, I am rather surprised that the number of such cases is not higher. As has been conclusively shown by the investigations of Freud and Jung, symbolisation plays a very important part in dream formation. It is a device frequently resorted to by the subliminal mind, and, as I suggested in my report on the Warren Elliott sittings, it is part of the mechanism of transference from one stratum of consciousness to another. The fact that it is not more in evidence, being only 5 to 6% in dreams and 14% to 15% for hallucinations, should be noted. It may be that this is partly due to the symbols not being recognised as such, though in many cases the precognition takes the form of a more or less accurate picture of the event foreseen wherein no question of symbolism seems to be involved. It is possible that were the symbolism of our dreams more fully recognised and correctly interpreted we should find many instances of precognition.

Warning cases have a double importance. First, as regards evidential value and secondly for theory. I will not discuss the latter now. As regards the former, it is, I think, clear that the fact that the dream, impression, etc., has been regarded as a warning and in many cases has been acted upon, lends weight to the evidence. It shows that the percipient has been impressed by his experience beyond the ordinary, for one's conduct is not, as a rule, governed in any way by one's ordinary dreams.

It is therefore more likely for the account given to be full and accurate. One of the peculiar characteristics of dreams, and to some extent of all subliminal phenomena, is that memory of them tends to vanish more rapidly than in the case of waking experience. Unless the memory be fixed and reinforced by special attention it is likely to vanish completely in a short time.

I have, therefore, in assessing the evidential value of any case, allowed considerable weight to instances of warning.

This is perhaps as suitable a place as any to give some account of the methods which I adopted in assessing evidential value. I made five classes for evidential status, as follows.

E 1. Impression recounted beforehand, confirmed by documents or reliable independent witnesses.

E 2. Acted upon beforehand. Warning cases.

E 3. Told afterwards and confirmed by independent witnesses as regards the telling and the details of the fulfilment.

E 4. Not told but percipient deeply impressed.

E 5. Mere narrative.

In addition to this classification I allotted a further index for the amount of detail involved and the degree of fulfilment.

D 1. Detailed impression, fulfilled and verified.

D 2. Detailed impression, partially fulfilled.

D 3. No details.

Thus a case possessing the highest evidential value would be one marked E 1, D 1, a case having only E 5, D 3 would be practically valueless, most probably would be rejected as vague.

I here reproduce the notes which I made for my own guidance after a preliminary survey, although they to some extent forestall the discussion on theory. I do so in order that readers may know exactly what was in my mind when classifying the cases, and may be helped thereby to criticise my methods.

I laid down the following conditions for an ideally perfect case.

(1) It—the precognition—must have been told or recorded before fulfilment, or acted upon in such a manner as to afford evidence of foreknowledge.

(2) It must include details so that chance fulfilment is rendered unlikely.

(3) The precognition must indicate fairly narrow limits of time for fulfilment, or else must contain such details as to fix the occasion of fulfilment, e.g. on visiting a certain place (see 8).

(4) It must be of such a nature that inference from considerably wider knowledge and with considerably enhanced powers of inference, could not reasonably have been held to have afforded the foreknowledge.

(5) It must be of such a nature that suggestion, whether auto- or hetero-, conscious or unconscious, could not have brought about the fulfilment.

(6) It must be of such a nature that the information was not in the possession of any other person so that it could have been conveyed telepathically to the percipient. The information in such cases would include the intention to perform certain actions.

(7) It must be of such a nature as to exclude hyperaesthesia of any extent which might reasonably be considered possible.

(8) The time interval between the prevision and the fulfilment must be sufficiently long to exclude the illusion of *déjà vu* and sufficiently short, on the other hand, to reduce the probability of chance fulfilment to negligible proportions. This latter limit is dependent on the circumstances of the prevision, such as the nature and amount of the details foreseen.

Note A. In the matter of time there must be taken into account the interval which has elapsed before the incident is reported. When documents are forthcoming this may not be of very much importance

but documentary evidence is rare. The lapse of a considerable time before reporting and investigation reduces the evidential value in cases where the account depends entirely on memory. People are liable to embroider their narratives and memory is unreliable. Confirmatory accounts should be independent. Action taken before fulfilment is, from some points of view, better evidence than confirmatory accounts.

Note B. Details are more easily given in visual premonitions, hence dreams, visual hallucinations and crystal visions furnish the best cases. In auditory cases, those where actual words are given in the premonition, approximate in evidential value to the above, but the number of details can never be so full. The number of details is important, the evidential value rising by a function of the product of the number and not by mere addition.

The trivial details are evidentially more valuable than broad general features.

Incorrect details do not necessarily deduct much from evidential value.

This concludes the preliminary discussion and I now propose to give samples of the various classes. I have summarised the accounts as briefly as possible in order to keep this paper within reasonable dimensions. For fuller information readers are referred to the volumes of *Proceedings* and *Journal*.

Dream. Natural death. Recurrent. E 3, D 1.

P. xi. 577. Lady A. dreams that she sees her uncle lying dead by the side of the road. The body is brought to the house in a wagon and two men, whom she recognises, carry it upstairs. In doing so the left hand struck the banisters. She tells the dream to her uncle who promised not to ride unaccompanied. The dream recurs after two years and again four years later. Fulfilled in all details.

Hallucination. Natural death. E 1, D 1.

P. v. 293. Mrs Alger sees phantasmal appearance of her mother-in-law, followed some hours later by hallucinatory voice saying, "Come, both of you, on the 22nd." The mother-in-law died four days later on the 22nd. Was not known to be ill at the time of the hallucination.

Mediumistic. Natural death. E 1, D 1.

P. xi. 433. Dr Suddick in a private circle for table tilting receives prediction of the death of a Mr Chris Varis, then very ill in a town some distance away and expected to live only a few days. His death is predicted as going to occur in 40 days' time. Fulfilled.

At another sitting in a different house with different sitters, one of the sitters is given information of the death of another friend, whom he had thought was alive at the time. This ostensible communicator confirms the prediction concerning Varis.

Neither Varis nor those about him knew of the prophecy.

Dream. Recurrent. Accidental death (Murder). E 1, D 1.

P. v. 324. Well known case of Mr J. Williams seeing in a dream the murder of Mr Perceval in the House of Commons before the event.

Dream. Accident. E 5, D 2.

J. iv. 327. This is a case of partial fulfilment. Mr H. dreams that his friend, Mr W. M. Evans is thrown from his trap by the horse rearing and is killed. The accident happened as foreseen in the dream only that Mr Evans escaped death. Other details correct.

Hallucination. Course of illness. E 5, D 1.

P. xi. 446. A lady staying with a relative in Paris, while in bed, sees hallucinatory figure resembling him but shrunken and partially paralysed and apparently imbecile, crossing the room. He was asleep at the time. Some time later he was attacked by creeping paralysis and softening of the brain, and became nearly as she had seen him in the hallucinatory vision.

Dream. Course of illness. E 1, D 1.

J. xii. 340. Mrs L. dreams that the doctor told her that her friend Mr. C., who was very ill with pneumonia, would die on Thursday at four o'clock. Her husband wakes her as she moans and cries in her sleep and she tells him her dream. The following Thursday Mr C. is better and continues to improve until the next Wednesday when he has a heart attack. He gets over this, but has another on Thursday and dies at a few minutes past four.

Dream. Accident. E 1, D 1.

P. xi. 489. Col. Coghill receives a letter from a friend telling him of a dream in which she saw him lying under his horse with several people trying to assist him. Fulfilled two days later. Letters preserved.

Dream. Accident. Warning case. E 2, D 1.

P. xi. 497. Lady Z. dreams of driving in a street near Piccadilly and her coachman falling off the box on to his head in the road, crushing in his hat. Next day, wishing to go to Woolwich but remembering her dream, she suggests to the coachman that she should go by train. He demurs at starting at the time proposed but

says that eleven o'clock will be all right. On the return drive Lady Z. notices the coachman leaning back on the box as she had seen him in her dream. Memory of the dream flashes across her and she calls to him to stop, and to a policeman standing by to catch him. The coachman at that moment faints and falls but is caught. Had not Lady Z. acted upon the warning of the dream, and called to the policeman, the coachman would have fallen on his head. Thus the warning conveyed by the dream averted the complete fulfilment.

Dream. Accident to material thing. Warning. E 2, D 1.

P. xi. 561. Mr Skelton, a railway engine driver, dreams of a collision in a station. Two days afterwards, while his train was on a siding at that station, a goods train, being late, was passing through on the express line. Mr Skelton heard the whistle of an express and recognised the scene of his dream. He succeeded in flagging the express and thus avoided the accident which he had foreseen.

Dream and Impression. Accident to material thing. Warning. E1, D 3.

P. v. 335. Frau K. dreams of a fire. On waking has presentiment of disaster. She urges her husband to have their securities removed from the safe in a brewery where they were being kept, and finally, after ten days, prevails on him to do so. About a month later the brewery, with the safe, was totally destroyed by fire. Frau K. dreamed of fire on the night when the brewery was burnt down.

Dream. Trivial incident. E 1, D 1.

P. v. 345. Dream of finding a brooch on a seat in Richmond Park. Told beforehand and fulfilled.

Dream. Trivial incident. E 1, D 1.

P. xi. 458. Dr Hy. Smith dreams of looking at a number of transparent photographic slides. Next day a man offers to show him some slides. He looks at them a week later and recognises those seen in his dream.

Dream. Trivial incident. E 1, D 1.

P. xi. 474. Maria Manzini, Dr. Ermacora's subject, dreams of a visit of a man trying to get subscriptions for a novel and offering prizes. Describes the man. Dream written down next day. Fulfilled in all details four days later.

Dream. Trivial incident. E 1, D 1.

P. xi. 487. Mrs Atley dreams of reading family prayers in the hall instead of in the Chapel as her husband, the Bishop, was away. After reading prayers she went into the dining room and saw a large

pig standing between the table and the sideboard. Tells her dream before prayers. Fulfilled. The pig escaped from its sty while prayers were being read.

Dream. Trivial incident. E 1, D 1.

P. xi. 488. Mrs C. dreams of seeing a monkey and being followed by it, which terrifies her. Tells her dream to the family and her husband recommends her to go for a walk. She does so with the children and sees a monkey which follows her. This took place in London where escaped monkeys are not common objects.

Hallucination. Arrival. E 1, D 1.

P. vi. 374. Anon. Hallucination of arrival of two men in a dogcart. Fulfilled shortly after with details correct. The arrival was unexpected and the visitors unknown. They were recognised as the persons seen in the hallucinatory vision.

Hallucination. Arrival. E 5, D 3.

P. vi. 375. Anon. Hallucination of extra guest at dinner. Fulfilled next day. Details correct.

Borderland. Winner of race. E 1, D 1.

P. xiv. 251. Prof. Haslam, between waking and sleeping, visualises a jockey in scarlet, riding horse to win. Repeated. He does not know anything about the horses, jockeys, or colours in the next day's race. Tells several people beforehand. Finds jockey in scarlet riding and backs the horse. The horse wins.

The foregoing are simply examples selected more or less at random to illustrate the various types of cases. It is unnecessary for my present purpose to weary the reader with the recital of a larger number, although it would be possible to give many instances of most of the types.

We must now turn to the consideration of the main purpose of this inquiry, viz. the discovery of hypotheses to account for the phenomena.

There are, first of all, four such possible hypotheses which may be held to be the most probable explanation in certain cases. These are, (1) Telepathy, (2) Auto-suggestion, (3) Subconscious knowledge, (4) Hyperaesthesia.

Before discussing these in detail I should like to point out that they are all based on mental phenomena which involve, as a general rule, some amount of dissociation, thus linking up with what was said earlier concerning the state of the percipient.

Cases of precognition where an explanation based on telepathy can plausibly be put forward are for the most part arrival cases. In this it may be held that some person connected with the arrival

which is to be, is thinking of the coming event at the time the percipient receives the impression and that this thought is conveyed telepathically.

It need not, of course, be only an arrival. Any event which is planned beforehand might be the subject matter of a preognition of this sort. The intention to act in a certain manner may be transmitted telepathically, and the percipient may receive it as an impression of something about to occur or as a dream or hallucination representing the scene as imagined. This hypotheses, while fairly covering the simpler arrival cases such as that of a letter or visitor, gets a little thin when stretched to include some of the more complicated.

As an example of this I quote a case told to me by the percipient some years ago. Unfortunately, it is not available for formal report, the percipient having died some time since, so it is only of use as an illustration and not as evidence.

The percipient, the wife of a minister, was expecting a visitor, a brother minister from a town some distance away, to lunch. She was sitting in a room on the first floor, overlooking the road, and about lunch time looked out of the window and saw her visitor walking up to the house. She recognised him perfectly and noticed that he was dressed in a fashion very unlike his usual attire. She rang the bell for the maid to set on the lunch and went downstairs to the front door to admit him. On opening the door she was surprised to find no one in sight. At the time of the occurrence the visitor was at a railway station some miles away, having missed his train. He arrived later in the day. When he arrived the percipient saw that he was dressed in the, for him, unusual fashion which she had noticed in the hallucinatory vision.

Now it is quite possible that while he was waiting on the station he might have thought of himself as arriving at the door of the house which he was to visit, but it requires some amount of stretching of the telepathic hypotheses that the projected phantasm should include details of his clothing. He might, of course, have been more than usually conscious of his attire owing to its not being his ordinary fashion, but one would expect that the phantasm would represent his more normal appearance.

I am aware that this particular case might be attacked on several grounds, such as that it is not a premonition at all, but a simple case of mistaken recognition. I am not going to trouble to answer any such objections as I do not cite the case as evidence, only as an illustration.

In the following case, *P.* xi, 503, E 5, D 1,¹ the telepathic hypothesis requires even more stretching. The facts are: Professor J.

¹ Though this has to be classed E 5, I think it really merits a higher marking.

Thoulet, waking from sleep, thinks that he has a telegram in his hand and goes into the room in which his friend is sleeping and says, "You have got a little girl, the telegram says . . ." He begins to read the imaginary telegram when it vanishes and he realises that he has been dreaming. He wrote down what he had read. Ten days later a telegram corresponding to the one seen in the dream actually arrives. M. Thoulet is able to recognise the words he had seen but not read aloud.

In order to make the telepathic hypothesis cover this case we must assume that the wording of the telegram had been decided upon ten days before the event which it was to announce had happened. This, though possible, seems rather unlikely, especially as the sex of the unborn child could not have been known.

Thus we get a graduated series of cases starting from those which the telepathic hypothesis will cover with ease, and working up to instances which require more and more stretching.

Now, although the canons of scientific methodology may compel us to give preference to this hypothesis on the grounds that telepathy is an already established phenomenon, yet the fact that its applicability tails off as it were rather tends to discredit it. If in the more complex cases we find that telepathy alone cannot satisfactorily explain the facts, and should we be successful in finding another explanation, then I think that even in the simpler cases we should have to bracket the two hypotheses together as alternatives. We cannot reject telepathy in cases where it fits, because we know, or at least most of us feel pretty well convinced, that telepathy exists.

The net result is to throw some doubt on the matter and restrain us in our endeavours to stretch the hypothesis to cover other types of cases.

I have made a list of the cases wherein I consider that the telepathic hypothesis may reasonably be held to apply. I have included those instances where a fair amount of stretching is necessary, such as the one just quoted.

The result is: Dream 9; Impression 9; Hallucination 9; Mediumistic 2; Crystal 1.

The next hypothesis to be discussed is that of auto-suggestion. It is possible, of course, that hetero-suggestion might also operate in bringing about the fulfilment of a precognition; but so far as I have been able to discover there is no case in those under review where this seems reasonably plausible.

As a typical case where auto-suggestion seems a possible explanation the following may be cited.

J. iv. 292-3. E 4, D 2. Mrs Edwards has an auditory hallucination warning her that she will die at a certain hour. As the clock was striking the hour she had a severe hæmorrhage and very nearly died.

It seems plausible to suggest that the hallucination acted by auto-suggestion to bring about the haemorrhage at that exact moment. I do not think that there is much that need be said on this point. We are aware of the very powerful influence which auto-suggestion can exert, particularly in cases where there is already some pathological state of the organism.

The number of the causes which I have extracted as being possibly due to this cause is as follows: Dream 3; Impression 2; Hallucination 7; Mediumistic 1.

It may perhaps be worthy of note that the percentage is higher with hallucinations than among the other types. This seems quite reasonable in that hallucination, being a more exciting and notable event than a dream or impression, would tend to be stronger in inciting to auto-suggestion.

The next hypothesis, viz. Subconscious knowledge, is perhaps the one which possesses the greatest possibilities of extension. On this hypothesis the apparently supernormal precognition is reduced to the normal type, that is to say, a prediction based on inference from known facts and causes, only in this case they are subliminally known. We have also to suppose in many cases a considerably greater power of inference than is possessed by the supraliminal mind.

It is obvious that the possible range of this hypothesis is extremely indefinite. What limits are we to place on the possibility of subliminal knowledge and inference? Is it true, as some determinist philosophers have held, that if we could know the exact state of affairs down to the minutest details at any given moment, and if we also possessed the necessary powers of calculation, we could predict the state of affairs at any future time with complete certainty? And if this be true, can we assume that any human being could possess this knowledge and these powers of calculation, whether supra- or subliminally?

If these suppositions be accepted, then all apparently supernormal precognitions are simply disguised cases of normal prediction. We have found an explanation which will cover the whole lot. As a matter of personal opinion I doubt very much whether any such view can be seriously entertained. I regard the necessary suppositions as being so extremely far-fetched and wildly improbable, in view of our knowledge of the human mind and its limitations, that the question hardly merits serious discussion.

Consider, for example, the case of Mrs Atlay, *P.* xi. 487, of which I have given a short summary in my list of specimen cases. If the determinist theory be correct, all the causes, or causes of causes, which produced the event of the pig escaping from its sty and wandering into the dining room during prayers were in existence at

the time Mrs Atlay was dreaming, and we must assume that she somehow became aware of them. How complex this interlocking system of causes must have been we can hardly imagine. It would include the state of the fastening of the door of the sty, the state of mind of the pig, if I may be allowed to speak of states of mind in connection with pigs, also the motives which induced it first to escape, and second to wander into the dining room. Besides all this there would be innumerable other factors such as the absence of anyone *en route* to head him off and lead him ignominiously back to captivity, the causes of a free passage, such as open doors or window in the dining room, etc.

Then, having acquired all these data, Mrs Atlay's subconscious mind must have engaged in a process of calculation as a result of which she foresaw the inevitable outcome of all the interacting factors.

Our normal knowledge of the psychology of our fellow human beings is insufficient to allow us to make accurately dated forecasts of their actions except in comparatively simple cases. Our knowledge of the psychology of pigs is far smaller than that of human beings. Can we reasonably suppose that our subliminal powers so greatly transcend those of our supraliminal minds?

There are, however, types of cases in which this hypothesis of inference from subliminal knowledge seems plausible. They are for the most part cases involving the precognition of states of the organism, either predictions of the course of an illness or of death from natural causes. They are, in short, a sort of supernormal medical prognosis.

It is clear that in such cases the cause of the precognition may be joined to or supplemented by one or more of the other causes mentioned. Thus, if by inference from subliminal knowledge the percipient forecasts the progress of his disease, auto-suggestion may operate to bring about some definite happening at the exact moment foretold. Where the prognostication concerns some other person it may be that the knowledge on which the inference is based is acquired telepathically from the patient, or possibly by hyperaesthesia, and in such case unconscious hetero-suggestion might come into play to produce definite results. Whether the latter phenomenon does actually occur I cannot say. I have not been able to find any clear indication of its presence.

As examples of cases in which this hypothesis seems plausible I would refer to *P.* xi. 446; *J.* xii. 340; *P.* v. 293; *P.* xi. 443, summarised above.

The number of cases in which I have considered that it might be reasonably held to apply is as follows: Dream 10, Borderland 2, Impression 5, Hallucination 19, Mediumistic 7, Crystal 1.

The last of these hypotheses, viz. hyperaesthesia, is applicable almost entirely in cases involving an accident to some material object. The following are typical examples.

P. xi. 418. E 2, D 3. Anon. A lady living in a wooden house in the Roekies, while sitting one evening in the porch, has an hallucinatory vision of a fire in the distance. After watching for about ten minutes she hears a faint crackling sound, and, being disturbed by the hallucination which at the time she takes to be real, she goes to investigate and finds her own house on fire. She is just in time to save her child.

As lending some support to the hypothesis of hyperaesthesia it is interesting that the same percipient experienced a second somewhat similar occurrence. While in England she was wakened one night from sleep by the impression of hearing her name called. On waking she found the nightlight blazing.

P. xi. 419. E 2, D 3. C. W. Moses, engineer in charge of a train, while driving his engine feels the impulse to stop the train when approaching a trestle bridge. He does so and goes ahead to investigate. He finds the bridge destroyed by fire, and a serious accident is thus avoided. This might have been due to hyperaesthesia of smell or else some difference in the noise of the wheels on the rails.

P. xi. 421. E 2, D 3. M. H. Gray has hallucinatory vision of flames. Investigates and finds the clothes in the laundry on fire. Might have been hyperaesthesia of smell or hearing, although two closed doors intervened. Where this hypothesis appears to be a reasonable explanation the case, of course, ceases to be a true pre-cognition, as hyperaesthesia presumes the actual existence at the time of the event supernormally cognised.

The number of such cases is, Borderland 1, Impression 6, Hallucination 4.

Taking together all the cases which have been selected as possibly explicable by one or other of these four hypotheses we get the following totals: Dream 22, Borderland 3, Impression 22, Hallucination 39, Mediumistic 10, Crystal-gazing 2.

In making this selection I have tended to stretch the hypotheses as far as they could reasonably be stretched. I have no doubt that in a good many cases some people would think that I had overdone it, but on the principle that an explanation by known causes is better than one that postulates the unknown, I thought it right to be as liberal as possible. If it should happen that an hypothesis is found which can account for pre-cognition in general, and is based on established fact, then these categories would be subject to revision.

After deducting the cases as enumerated above we have left a considerable residuum for which some other explanation must be

found. There remains 94 cases of Dream, 4 Borderland, 17 Impression, 23 Hallucination, 41 Mediumistic, 4 Crystal-gazing. This constitutes a mass of evidence far too weighty to be set aside.

There is, of course, one general hypothesis covering all possible types of precognition which so far has not been mentioned, viz. that they are all due to chance coincidence. This is the view that is generally held by the uninstructed and those who have not made an unbiassed study of the evidence.

I have had it said to me when discussing the matter with persons not acquainted with the subject, "Millions of people dream every night. It is, therefore, only what might be expected that some of the dreams should correspond with what happens during the next few days."

There is a superficial appearance of reasonableness about this argument when stated thus vaguely; but I think that if it be put into more definite form it will be seen to be quite untenable. It is true that millions of people dream every night, but the proportion of dreams which are remembered is comparatively small, and out of this fraction there is an even smaller proportion which exhibit the peculiarly impressive character which so often marks the premonitory dream and causes it to be recorded. We cannot have, unfortunately, any statistics of such dreams whereby we can arrive at an estimate of the numbers which turn out to be false premonitions. Still I do not think that this is essential to the argument.

Even supposing that there are millions of dreams remembered, the number of subjects which may be dreamed about is practically infinite. Thus the chances that any particular subject will enter into the dream on any particular night is very small. It is of course true that the subjects of any person's dreams are as a rule limited to a comparatively small section of the total of the possible subjects, being usually concerned with those things which enter his daily experience. For example, a fairly high proportion of my own dreams are connected with sailing and boats. But if I dream that I am sailing in my boat and actually do so during the next few days, I should not consider the dream premonitory. Should the dream contain details, say that the jib halliards carried away, and should this happen within a short time, it still would be hardly worthy of notice, for, although jib halliards do not often carry away, at least if you look after your gear as you should, I have known them to do so.

Should the dream contain a combination of independent details, none of which, perhaps, is very strikingly unusual or unlikely to happen, and should the fulfilling event consist of the same arrangement of details, then the chance hypothesis loses its plausibility, for even if, as remarked above, the number of subjects about which

anyone is likely to dream may be a comparatively small fraction of the practically infinite theoretical total, yet the number of possible combinations of these subjects is immensely great.

If then the chance that anyone will dream a particular dream on any particular night is very small, so also is the chance that any particular event will happen to that person during the next few days.

In saying this I refer, of course, to a complex event. For, as in the example quoted above, while it is quite likely that I should go sailing once or twice in a week, it is not so likely that while sailing my jib halliards should carry away and still more unlikely that that event should be combined with several other similar happenings. It is only when the apparent precognition refers to such a combination of details that anything supernatural is suggested.

The probability that the precognition is due to chance depends on both the separate probability of the dream being dreamed and that of the event occurring within a reasonably short interval. Where these separate probabilities are represented, as I have endeavoured to show, by very small fractions, the probability of chance fulfilment becomes infinitesimally small. The time element is, of course, important, and I have, therefore, rejected any cases where the lapse of time between the dream, impression, etc., and the fulfilment appears excessive.

What I have said of dreams applies equally to impressions, but when we come to consider hallucinations the argument is very much stronger owing to the rarity of these phenomena.

In this discussion we have been considering the probability of precognition of events of a usual and commonplace character, but in many cases the event foreseen is not usual.

Consider again the case quoted of Mrs Atlay and the pig in the dining room. How often does one dream of seeing so unusual a sight? It must be a very rare occurrence. On the other hand, how often does one find pigs—I refer, of course, to the non-human variety—in dining rooms? This again is not a usual happening. And when we combine the additional details of time, i.e. after reading prayers, and place, i.e. between the table and the sideboard, we get a combination of circumstances such that chance seems to be excluded for all practical purposes.

Now if this incident stood alone, we might perhaps say that it was an extremely curious coincidence and leave it at that. But it does not stand alone, we have a large number of cases to match it. Dreams of being followed by a monkey are surely very rare, at any rate for persons living in this country, and the event of being so followed must be almost unique in London where loose monkeys are, to say the least, unfamiliar objects.

I could go on citing cases but I think it is unnecessary. The hypothesis of chance cannot possibly be held to cover all the cases reported.

I have classed as non-precognitive a certain number of cases where it had appeared plausible, but there still remains so large a residuum where it is wildly improbable, that I have no hesitation in rejecting it as a possible explanation.

There is another hypothesis which is sometimes put forward, viz. that the precognitions are due to illusions of memory. In certain cases, where no records have been made and where no one has been told of the dream etc. before fulfilment, this explanation might be possible; but when records have been made or an account given before fulfilment, it seems inapplicable. We should have to suppose that there was illusion of memory not only as regards the precognition and its fulfilment but also as regards the times of making the records or giving the accounts. Not only must the percipient have experienced illusion of memory but also all those who give confirmatory evidence. In many of what I have called warning cases, of which there are some 34 all told, some action has been taken as a consequence of the premonition. It seems hardly likely that in so many cases the illusion of memory should apply to these actions as well as to the precognition itself.

Illusion of memory may possibly account for a few of the poorer cases, but it would be to discredit too profoundly the value of human testimony to suppose that it occurred in all the large number of well evidenced cases which we possess.

I do not think it necessary to spend time in discussing fraud, bad reporting, inaccurate observation and all the rest of the stock in trade of the sceptic of a certain type. Fraud is obviously out of the question in so large a mass of spontaneous phenomena. As regards the rest, the cases were all drawn from the *Proceedings* or *Journal* of the Society and this in itself is some guarantee that these points have not been overlooked. For a case to have been printed it must have reached the evidential standard laid down by the Society; there may have been a few lapses from this standard, but on the whole it may be taken to have been observed.

Reading the reports of the cases themselves together with the confirmatory evidence published amply supports this view.

After prolonged study I have no hesitation in affirming that precognitions do occur. In doing so I have the support of many authorities in psychical research. Should the reader feel any doubt remaining, I would refer him to Prof. Chas. Riehet's excellent little book, *L'Avenir et la Prémonition*.

We have now come to the end of the hypotheses which have from time to time been put forward to account for these facts, and we

find ourselves left with a very considerable mass which none of them will cover. Can we find a tentative hypothesis or must we leave it, as Prof. Riehet does, unexplained ?

It is, perhaps, a temerarious enterprise to venture in where he has abstained, yet in spite of this I propose to make the attempt. If I fail and my suggestion be held to be untenable, at least one more alternative will have been cleared out of the way and the path rendered less obstructed for future explorers.

Now what is the central basic fact common to all cases of precognition ?

When I observe an event which happens in my presence my mind somehow or other comes into conscious contact with that event. How it does so is a matter concerning which much controversy has arisen, and there are a number of different theories of perception. It would be out of place to attempt to discuss these here, even were I competent to do so. All we need say is that a cognitive relation is set up between the event and my mind.

The difference between normal cases and those of precognition is that in the latter the event does not happen in my presence, that is to say it is not an event in my *present* but in the future. Somehow or other a cognitive relation is set up between my mind and an event which, as we say, has not yet happened.

It is unfortunate that this enquiry leads us into some of the very worst morasses of metaphysical speculation. I would willingly have avoided adventuring into this perilous region could I have done so, but if the attempt to explain precognition is to be made, some excursion therein is inevitable. I will make it as short as possible.

Now it is held by some authorities that the future is non-existent. What exactly is meant by the word "exist" I will not consider, but in view of the evidence before us I suggest, although I feel considerable trepidation in doing so, that these authorities are mistaken, for how can I set up a cognitive relation with the non-existent ? It is equivalent to postulating a relation which contains only one term. Yet unless we dismiss the entire mass of evidence which remains uncovered by the alternative hypotheses discussed above, this relation actually occurs. I argue, therefore, that the future exists now in some way, and may be accessible to my consciousness under certain circumstances.

It has been held by some writers, Prof. Riehet for example, that the logical consequence of this position is that the entire future is completely determined. Such a consequence of complete determination would entail the surrender of freedom of choice with all its metaphysical and ethical implications. For, they argue, if we can foresee a future event, that event must exist in its entirety and cannot be altered by any action of ours in the present.

But even if we accept this argument (which, for reasons which I will try to explain later, I do not) the phenomena of precognition such as we have before us do not warrant us logically to infer complete universal determination.

All that we know is that a few future events have been subject to precognition. We are not entitled to argue therefrom that all events whatsoever could have been foreseen.

No one denies that some amount of determination of the future obtains ; if I release the trigger of a properly constructed and loaded Mills bomb, that action in those circumstances will determine an explosion at that place after a few seconds.

It may be that one of the indispensable conditions of the occurrence of a precognition is the complete determination of the event foreseen, that is to say, only those portions of the future which are already fixed can be foreseen.

In order to prove universal determination we should have to show that every event whatsoever could be the subject of precognition, and this, of course, we cannot do. It is true that those events which we have actually found to be the subject of precognition appear to differ in no observable single respect from any other events, that is to say they seem to be a fair sample (with the possible exception that death has rather more than its share, which, however, is probably susceptible of a perfectly simple reasonable explanation).

But although we can detect no difference in general character between the events which have been foreseen and those which have not been, so that we must admit that we can see no reason why, if the former have been the subject of supernormal precognition, the latter might not be so also, many essential characteristics are hidden from us and notably the characteristic of being completely determined or not. We can in some cases feel pretty sure that an event has been completely determined, though these are for the most part relatively simple and, as it were, self-centred events, but where all the multifarious conditions and influences of the environment impinge on it we can never be sure that we have allowed for all relevant factors. On the other hand, we can never say with certainty that an event was not completely determined.

Could these questions be settled definitely it would put an end to the age-old controversy between the upholders of free-will and the determinists. If observation carried on over a thousand years has been unable to settle the matter we may be fairly confident that it is because the fact is not observable.

Until, then, we can show that all events whatsoever are theoretically susceptible of becoming subjects of precognition the argument for universal determination fails.

There are two sides to the question of precognition. First, how

we can come to know a future event, and second, the mode in which the future can be held to exist.

In putting forward my suggested hypothesis I do not for a moment suppose that it is a complete explanation or even that it represents how things actually happen. It is merely a suggestion of a possible mode of looking at the phenomena based on an analogy with an established psychological doctrine.

We take our stand firmly on the empirically established fact that apparently supernormal precognitions do occur, and that they imply that conscious contact is sometimes made with future events. There may be an explanation of this phenomenon of which we have and can have no conception under the limitations imposed by the finiteness of our minds and the essential relativity of all our knowledge, but if we can find a tentative hypothesis based on other empirically established facts, that is all we can hope for; and it is a step worth the taking. We are always at liberty to retrace our steps.

It is clear that the crux of the whole thing is Time.

We must distinguish between two sorts of time, or rather between two different conceptions of it, viz. the mathematician's and physicist's time and psychological time. The former is admittedly an abstraction; it consists of point-instants having no magnitude but only temporal position; it has no more real existence than the mathematical point or line.

Psychological time is time as experienced.

Strictly speaking, of course, time is never actually experienced. What is really the object of experience is change, i.e. events taking place. Time is an abstraction from change.

With mathematician's time we have no concern. Its proper place is in physical science, and in its proper place it acts as a well behaved abstraction should act. Its conduct is not, perhaps, quite so faultless as is that of some other mathematical abstractions, but that is no concern of ours. Unfortunately the mathematician's concept of time has to some extent influenced our ordinary ideas, and these should, of course, be formed solely on our experience, i.e. psychological time.

Most people have very hazy notions on the subject; in fact, its inherent difficulty and familiarity renders the ideas of even the clearest thinkers rather nebulous. For the average person who has not thought about or studied time, I imagine that the definition implied in the hymn, "Time, like an ever-rolling stream, bears all its sons away," is more or less unconsciously adopted. As a definition it is obviously useless; as a description it is inaccurate. Time does not roll—abstractions don't. Events, if you like to put it that way, may roll.

Concomitantly with this idea of time as a flowing stream, another and conflicting one is ordinarily held. Time is thought of as a sort of pseudo space, stretching out before and behind, wherein events are located ; we pass along a path in this pseudo space, the point at which we are being the present moment.

It is over this idea of time that the mathematician's concept is apt to exercise a misleading influence.

Let us endeavour to thrust aside these notions, and rest solely on actual experience ; that is to say, experience of change or of an event taking place.

We must distinguish between two different types of experience of change ; first, that of an event actually taking place before our eyes as it were, and second, that of an event having taken place. In the first the experience is direct and immediate, as when we actually see an object move or change in some way ; in the second we infer change by comparing our immediate perception with our memory of another perception of the same object.

For example, look at a watch ; you see the second hand actually move, the hour hand appears to be stationary ; but if you look again in half an hour's time, you infer that the hour hand was moving because you see that it has changed its place.

We look at the watch in what we call our present.

Now if we perceive an event actually occurring, that present must have some duration. Were it a mathematical point-instant having no dimension, change could not occur within it. There is no room inside a point.

Perception, whatever else it may be, involves a state of consciousness, and a state of consciousness must possess some duration. If it vanished the moment it occurred, we could not be conscious. Moreover, it cannot be denied that our consciousness extends over a certain length of duration, and if this be so it cannot be made up of point-instants having no length. If our present moment has no length of duration, it must be separated from the present moment which just preceded it by a gap having length of duration, as otherwise the two moments would coincide ; thus all duration would be compressed into a single moment or point-instant.

The idea that our experienced duration is due to the persistence of memory of past point-instants is clearly untenable. We should have to picture the stream of consciousness as consisting of a series of flashes ; flashes, moreover, which are so rapid that they occupy no duration ; between the flashes are blank periods. If the experience of change actually occurring be psychologically the same as that of change having occurred, which is what the memory theory implies, each instantaneous flash must not only include an act of perception, but also an act of memory and an inference from the

two. This seems rather a lot to pack into a point-instant, wherein admittedly there is no room to pack anything.

It is true that we cannot understand perhaps how duration can come to us, as it were, in lumps, but neither can we understand how it can come in point-instants having no duration. When the choice between two inexplicables is presented, I think that we are bound to accept that one which is in accord with experience rather than that which rests on abstractions.

I need labour the point no further ; that our present moment has a definite duration is pretty generally accepted by psychologists. It is known as the doctrine of the specious present. There is a question which immediately presents itself, viz., does this specious present include past alone, future alone, or both past and future ? Some authorities have held the first view, others the third, but I do not know of any who have held the second. Were I called upon to choose, I should adopt the view that both past and future are included within the specious present, but I consider that the question is really meaningless, being based on the illegitimate hypostatisation of an abstraction.

If I talk of an event as being in the past or in the future, I require some " present " in reference to which that event is past or future. Now the only present of which I have any experience embraces a slab of duration. All the parts of it are present. I can, in imagination, cut it up into slices, and one slice may precede another, but it is quite an arbitrary matter which slice I take as the actual, or as I prefer to call it, the fictitious present. If I wish to call one particular slice past and another future, I must place my fictitious present somewhere between them. But what grounds can I have for selecting the slice which is to be my present moment ? The whole idea of a fictitious present is due, in my opinion, to the influence of that abstraction, mathematician's time.

Moreover, unless the slice of duration which I select as my fictitious present has no magnitude, i.e., is a point-instant, the question of past and future within its limits applies just as much to it as to the whole of the specious present. Yet point-instants never enter into my experience ; they are abstractions and exist only qua abstraction.

It may be objected that an event occupying a certain duration can be cut up into any number of slices and that each particular slice will occupy its own particular slice of duration and no other. Further, that there is no limit to the number of slices into which we can cut it up.

It is true that we can do so in imagination, yet even so we never arrive at a slice having no duration.

It is a form of the old paradox of the flying arrow. The paradox

arises only when we deal with abstractions, viz., a mathematical point on the arrow, a mathematical point in space and a mathematical point-instant in time. If we abstain from abstraction even in only one respect, say we consider the arrow as a whole, the paradox vanishes.

In making these abstractions does not imagination outrun possibility? The facts on which the quantum theory of physics is based show that so far as certain natural phenomena are concerned, there is a limit beyond which this cutting-up process cannot go.

Might I suggest, with the utmost deference as becomes the humble layman, that something like the quantum principle applies to space-time?

However this may be, it is a question for the physicist. As regards psychological time, experience presents it to us as a continuum, but as a continuum which will only cut up into lumps of a minimum size. How and why this is so I do not profess to understand; it is one of those matters which, like the quantum principle and many other things in modern physics, has to be accepted, as Prof. Alexander says, "with natural piety".

There is, of course, a mode in which I may reasonably speak of events as being past or future; my specious present covers a comparatively short length of duration, and events which are located outside of it are either past or future from its point of view. Also suppose that I have a specious present extending from t to t' , and another person has one from T to t to t' to T' , then from my point of view events in the period T to t in the other man's present would be past and those in t' to T' would be future.

There is a further point to be considered. All the events occupying the specious present are not presented with equal clearness.¹ This may be represented in a diagram. Let the horizontal line represent duration and the vertical coordinate the degree of clearness of



FIG. 1.

presentation. Then we should get a curve which would represent the consciousness during the specious present. It might take the form A (Fig. 1), in which case an event located near to t' would be presented with a smaller degree of clearness than one situated mid-

¹ I use the term "clearness" in a very special sense. It is by no means ideal for the purpose, but I cannot think of another. It is obvious that a sensuous presentation may be either clear or dim quite irrespective of its position in the specious present, but this has nothing to do with its clearness in the special sense in which I have used that word.

way between t and t' . In all succeeding diagrams t is to be understood as being towards the past and t' towards the future.

As the specious presents succeed one another the clearness of presentation in each will grow to a maximum and then gradually decrease to zero; the succession of specious presents would, in reality, be due to the fact that every event which is perceived is perceived throughout a finite time with continually varying degrees of clearness which eventually diminishes to zero.

Or the curve might take the form B (Fig. 2), which implies that when an event first comes within a specious present, it is perceived with maximum clearness which gradually diminishes to zero.

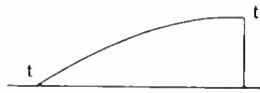


FIG. 2.

There are obviously other possible variants, such as

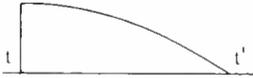


FIG. 3.

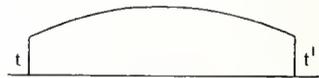


FIG. 4.

but for purposes of this enquiry these need not be discussed. The point at issue is whether the degree of clearness at t' is minimal, or whether the presentation to consciousness springs into being fully grown or at any rate with a substantial magnitude. I do not think that introspective observation will help us in this matter, and if we are to choose between the two, it can only be a tentative choice based on general grounds.

It might be argued that diagram B represents the act of perception as instantaneous, i.e., as occupying no duration and thus occurring with infinite speed. As a general rule, infinite speeds are looked upon with suspicion. We are much more ready to accept gradual growth.

Taken as a whole, the plan of nature seems to have been drawn with a soft pencil, or perhaps even with a brush. Mathematical lines and points are introduced only by the mind using its faculty of abstraction. We are more accustomed to soft curves and flowing lines than to sharp angles and hard edges.

It must be admitted, however, that these considerations are not decisive, but we can say that diagram A is, at least, as plausible as diagram B.

This being so, I propose to assume that the curve is somewhat in the form shown in diagram A, in so far, at any rate, that the angles

at t and t' are always less than right angles. There may be sharp fluctuations at any point of the curve, but with these I am not at present concerned. All that is necessary for my hypothesis is that as one approaches t' there is a gradual diminution of the coordinates, and not a sharp drop to zero.

I admit that this is an assumption, but I claim that it is legitimate. Though the theory of precognition which I am about to suggest requires that it be made, it could be stated on the basis of diagram B, but it would require the help of some rather cumbrous and ungainly expedients.

The diagrams above represent a single specious present. The actual fact is, of course, that there is a succession of specious presents, either as a continuous change, i.e., one merging into another by infinitesimal degrees, or else a discontinuous series. I do not attempt to decide which of these two alternatives is correct. To do so would involve the discussion of some of the most recondite and difficult problems of metaphysics. However, a decision on the point is fortunately not required for the present purpose, but as it is impossible to represent continuous change in diagrams, I shall, in what follows, show the series of specious presents as discontinuous, with the proviso that it is only to be taken as a symbolic representation for the sake of convenience, and not as implying any acceptance of the view that they are really discontinuous.

There is, however, one point on which I must insist, viz., that if it be a discontinuous series, the succeeding terms overlap to some extent. This is, I think, in accordance with experience. The representation in diagram of the series of specious presents will therefore take somewhat the following form



FIG. 5.

I can now state my tentative theory of supernormal precognition. I suggest that the specious present of the subliminal¹ mind is of

¹ I use the term subliminal mind in all that follows, because it is the one most familiar to students of psychical research and conveys the meaning I require. I wish, however, to make it quite clear that I do not intend thereby to express my opinion as to its nature or status. All that I postulate is that it is a region, normally cut off from ordinary consciousness by a threshold, wherein events take place, events which, even if they are not identical in character with those of the supraliminal mind, are of an analogous nature. Whether the subliminal can properly be termed a mind, I do not attempt to say. Possibly it is part of what Dr Broad in his *Mind and its Place in Nature* calls the psychical factor and to which he denies the term mind.

longer duration than that of the supraliminal, so that some events which are in the present of the subliminal would appear to be either past or future from the point of view of the supraliminal.

Dr C. D. Broad, to whose generous help and criticism I owe a great debt, which I hereby acknowledge, has pointed out to me that this is an inadequate statement of my theory, and has very kindly supplied the following accurate account.

“There is a one-to-one correlation between the series of supraliminal specious presents in a given mind and a series of subliminal specious presents in the same mind. Any term of the latter series overlaps in time the corresponding term of the former, extending further backwards into the past and further forwards into the future than the corresponding supraliminal specious present.”

(I wish to make it absolutely clear that the assistance which Dr Broad has so kindly given me does not imply that he endorses my suggestion or is at all prepared to accept my tentative theory.) This theory may be represented in diagram as follows :

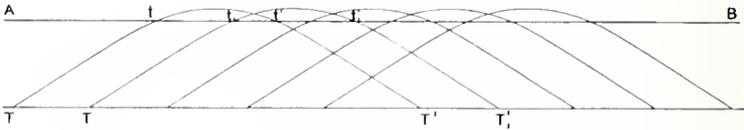


FIG. 6.

The line A-B represents the threshold between the supraliminal and the subliminal, the supraliminal specious present from t to t' has a corresponding subliminal specious present which extends from T to T' . Similarly, the succeeding specious present t, t' , corresponds with the subliminal T, T' .

If the flowing curve of diagram A (Fig. 1) be not allowed, and that of Fig. 2 insisted upon, I think the theory can still be represented in diagram. It would be somewhat as follows :

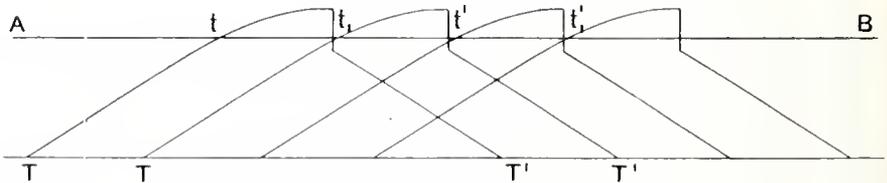


FIG. 7.

The ungainly features of which I spoke are the sharp angles where the vertical line from t' changes to the curve of the subliminal. However, as there is already one sharp angle necessitated by the

theory, I cannot see that anyone can reasonably object to another being inserted.

The contents of the supraliminal and subliminal minds, in so far as they are presented events, may be shown as follows :

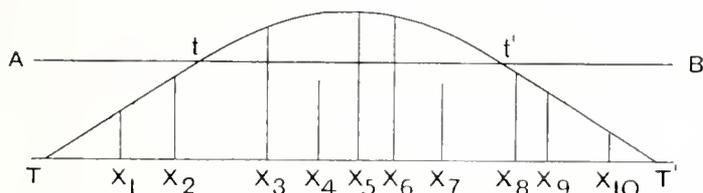


FIG. 8.

Here $x_1, x_2, x_4, x_7, x_8, x_9, x_{10}$ are presented to the subliminal only, x_3, x_5, x_6 are presented to the supraliminal. We may say, therefore, that for the supraliminal mind x_3, x_5, x_6 are present events, x_1 and x_2 are past, x_8, x_9 and x_{10} future. For the subliminal all the events from x_1 to x_{10} are present. I have in this diagram shown only one specious present, as it would have made it too confusing to have shown all the overlapping curves, but it is easy to see how it would look if they were filled in.

There is one point which must be mentioned, viz., that it is possible that an event, say x_8 , which was presented only to the subliminal in one specious present, might reach the supraliminal in a succeeding one.

Let us now see how we can represent a supernormal precognition, and for this purpose we will assume a fictitious case. At twelve noon, that is to say within the specious present of my supraliminal mind in which the striking of noon by my clock is presented to me, I have a supernormal precognition of an event which happens at 6 p.m. on the same day. The first diagram is as follows :

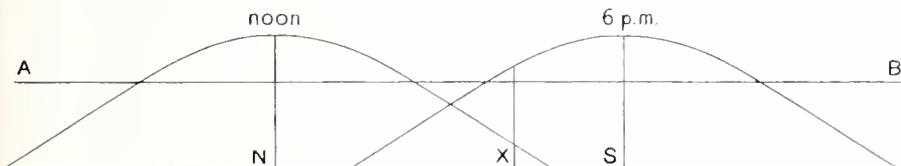


FIG. 9.

Here the first curve shows the specious present in which the striking of noon is presented. This is represented by the line from N. The second curve is the specious present in which the striking of 6 p.m. is presented, or would be presented if the spatial conditions are favourable. In the first curve the event x is present to the subliminal only, for obviously while that curve is in being the second

has not yet occurred. But in the second specious present, viz., that at 6 p.m., x is presented to the supraliminal. That is to say, it is experienced in the normal way.

Now my hypothesis suggests that the subliminal presentation of x in the first curve is somehow or other transmitted to the part of the curve above the line A, B.

This might be represented as follows. I show only the first curve, as this diagram represents the state of affairs at noon.

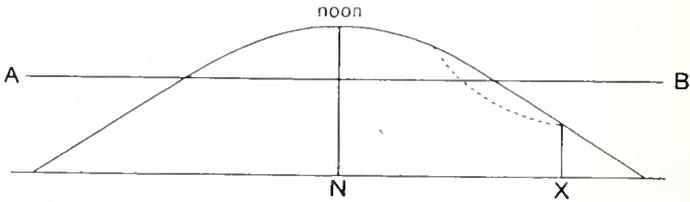


FIG. 10.

The dotted line represents the supernormal transmission of the subliminal presentation of x through the threshold to the supraliminal. When 6 p.m. arrives x is normally presented in the specious present at that moment, and this brings about the verification of the precognition.

All, therefore, that is required for my hypothesis to account for a supernormal precognition is that the specious present of the subliminal should extend sufficiently to cover the actual event, and that there should be a means of transmission to the supraliminal. That which is a precognition for the supraliminal is only a cognition for the subliminal.

Stated thus, these may not seem to be very extravagant demands for an hypothesis designed to account for so bizarre and puzzling a phenomenon as supernormal precognition. We know that the specious presents of the individual vary in length under the influence of fatigue, drugs, and probably concentration of attention. Moreover, it is likely that different individuals normally have specious presents of different lengths. It seems, therefore, not a very great leap to suppose that the supraliminal mind should differ from the subliminal in a similar way, but to a greater extent.

As regards the second requirement, viz., means of transmission through the threshold, we are in a similar position. That is to say, all we need to postulate is an extension of a process which we already know to exist. It is generally admitted that passage through the threshold does to some extent occur normally, as for example, in dreams, the mechanism employed frequently includes the use of symbolism; moreover there is ample evidence of supernormal

transmission; e.g., in the cases of hyperaesthesia. This is usually accompanied by, or occurs during, states of dissociation. It may not be correct to say that the subliminal perception itself is transmitted, though in some cases this appears to be so. It would, perhaps be better to restrict ourselves to the statement that the knowledge acquired by the subliminal perception is conveyed to the supraliminal.

In these non-precognitive cases of transmission through the threshold the events concerned lie in that portion of the subliminal specious present which coincides with that of the supraliminal. A diagram will make this clear.

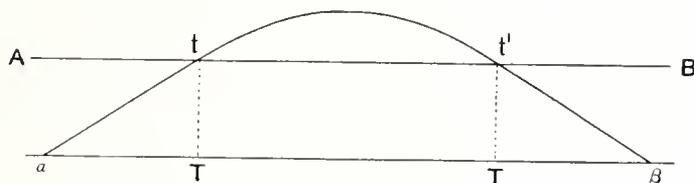


FIG. 11.

The events, subliminal perception of which may be transmitted to the supraliminal, lie between t and T' .

All that is required, therefore, for my hypothesis is that we should deny to these events any special privilege in the matter of passage through the threshold. All the events lying between a and β are *present* to the subliminal. Is there any obvious reason why a certain section of them should possess any special privilege simply from the fact of their position relative to the supraliminal specious present?

If we admit that the mathematical present moment is an abstraction which has no real existence, an admission which, it must be confessed, may be easy enough to make verbally, but is very difficult to embrace whole-heartedly with all its implications, I cannot see that there is any ontological difference between the various events which are present to the mind, whether supra- or subliminally, such as would confer this special privilege. I can offer no hypothesis as to how the subliminal mind establishes the cognitive relation, or, as I have put it, comes into conscious contact with the events within its specious present. It may be by an act of perception similar in character to ordinary perception, or it may be that the normal senses are specialised cases of a general faculty of knowing or presenting objects to consciousness. Each sense in operation would then be this general faculty working through and canalised by a physical mechanism whereby it takes on its own specific characteristics and becomes subject to its own peculiar limitations. On that level of the subliminal mind which lies furthest

away from the threshold the general faculty of knowing may operate directly, and not through any physical mechanism. It would be pure psychical knowing, but what it would be like I cannot conceive. Possible telepathy and clairvoyance may be examples of it.

In order that knowledge thus acquired may be transmitted to the supraliminal, it would have to be translated into terms which that part of the mind can understand; that is to say, visual, auditory, etc.

No weight, however, can be rested on this suggestion. Nevertheless, I think we may safely say that, whatever the real facts may be, the subliminal mind operates in some fashion other than through the normal senses.

When we turn to the mode of passage from the subliminal to the supraliminal we find an equally unsatisfactory state of ignorance. We do not know how the transfer is made even in normal cases; we only know that it occurs and that it tends to occur more freely in cases of dissociation.

There is one minor point which may be mentioned here. I noted earlier that there was a rather surprisingly low percentage of examples of symbolism in supernormal precognitions. We know from the work of the psycho-analysts that in cases of normal passage through the threshold symbolism is freely employed, and I suggest that the relatively low percentage of symbolism found in supernormal precognitions is due to the fact that the normal mechanism of passage through the threshold is not as a rule employed.

There is one final point to be considered, viz., what is the extent of this subliminal conscious contact with the future?

In Fig. 9 I showed the curve of consciousness meeting the line of duration at a point a little way ahead of the specious present. This would represent the degree of clearness of presentation as reduced to zero; that is to say, conscious contact as ceasing altogether.

There are, however, three other alternatives. First, the curve might end abruptly before meeting the line of duration thus

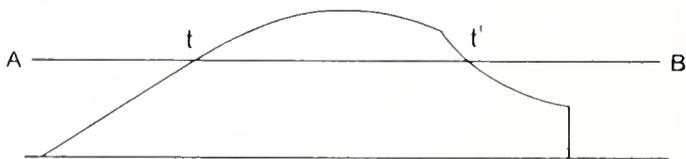


FIG. 12.

This I regard as highly unlikely. It does not seem reasonable to suppose that the causes, whatever they may be, which bring about a gradual approach of the two lines over the rest of their length should undergo so sudden and profound a change.

The second is that the line of consciousness approaches the line of duration asymptotically; that is to say, approaches ever nearer to it without ever actually meeting it.

The third alternative, which is the one which I am inclined to prefer, is that the line of consciousness is parallel to that of duration, and the curve is in the line representing the threshold. It would be thus:

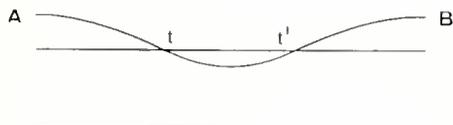


FIG. 13.

The coordinate of clearness as regards the subliminal mind would be constant throughout. The gradation of clearness of presentation which is actually observed in the specious present of the supra-liminal would then be shown by the vertical distance between the lines of consciousness and the threshold in the loop t to t' .

In all probability the actual facts are far more complex than anything which can be represented in a diagram. For example, the threshold is almost certainly not a sharp division between two homogeneous sections of the mind. There may be, I should say probably there are, several thresholds, or else there is one threshold which would be represented as a wide belt of gradually increasing density.

We have a little evidence to guide us in this matter of the length of the subliminal specious present. Precognitions of events weeks or even months ahead are fairly common. We have several quite good cases which stretch over years. Now if the conscious contact which the subliminal mind makes with the future can extend for a period of years, there seems to be no particular reason why it should not extend indefinitely. If the length of the contact is limited at all, I feel that it must be comparable in length with that of the supra-liminal mind, i.e., the normal specious present.

If the curve of consciousness approaches the line of duration asymptotically, then the degree of clearness of presentation would diminish rapidly at first, the rate of diminution getting slower and slower as we get further away from the present.

There is one fact in the evidence which may be cited in support of this view, viz., that the precognitions of trivial events are as a rule only a short time before the fulfilment. The longer the shots, the fewer the details.¹ However, this is susceptible of another explanation.

¹ Case *P.* xi. 577, summarised on page 60, is an exception to this rule, nor is it the only one.

It must be admitted that the implications of this hypothesis are very serious and may land us in considerable difficulties as regards our conception of time. However, there seems to be a conflict between the facts on which the doctrine of the specious present is based and our ordinary conception of time. It may be that the doctrine of the specious present may be rejected, but if so, some other means of accounting for the facts must be found.

I suggest, then, that we must revise our ordinary ideas of the nature of time, however difficult may be the task and however unsatisfying the result. That this is no novel demand the history of metaphysical thought since Kant clearly shows, and those who are familiar with MacTaggart's great work, *The Nature of Existence*, will already be prepared, if not to accept it, at least to regard it as within the sphere of reasonable discussion.

Briefly, my own personal ideas on the subject, for what they are worth, are as follows: I accept MacTaggart's view of the unreality of time and change in so far as sequences of completely determined events are concerned, but I hold, in disagreement with him, that there are non-determined events, such as events arising from free-will, which exhibit true change and therefore are subject to true time. But in all sequences of events, whether subject to true time or not, there is temporal order or location; that is to say, if A causes B, and B causes C, B will always lie between A and C. In other words, A is and always was and will be earlier than B, and C is, was and will be later. This conception of temporal location must be accepted as a fundamental feature of experience and held along with, but quite distinct from, the conception of true time. That it is a difficult conception to grasp and hold, I freely admit, but it seems to me to be necessitated by the facts, and mere difficulty of conception is, or should be, no bar.

This is not the place to enter upon any discussion of the grounds of this opinion. I may say, however, that I reached it from general metaphysical arguments and not from the consideration of the phenomena of the specious present and supernormal precognition. That these phenomena seem to require it, or something like it, for their explanation, appears to me to lend considerable support to the hypothesis.

It might seem at first sight that my theory implies that a future event, meaning thereby an event which does not yet exist, can exercise a causative influence on the present. This is obviously impossible, for non-existence cannot exercise a causative influence. But my suggestion is that the event which is the object of a precognition is not future in this sense; it already exists in the present of the subliminal. It is future only in the sense that it is temporally located later than events which are in the present of the supraliminal.

We must now turn to the other side of the matter.

I have suggested that the fact that precognitions occur shows that the subliminal mind is in conscious contact with a section of duration lying ahead of the normal specious present; that is to say, what we ordinarily call the future.

If this be so, the future exists now in some sense. Can we form any idea of the manner in which it exists?

I have already argued, unless we can show that all future events whatsoever are theoretically capable of being subjects of precognition, we cannot claim that the few scattered cases of which we have evidence prove the theory of universal determination.

But inability to prove a fact does not justify us in denying it. If all events are completely determined, that is to say, if freewill is an illusion, then, in my opinion, MacTaggart is right in denying reality to time. All events are coexistent in reality. We call one past and another future simply because of their temporal location with regard to another event which we call present, and our reason for selecting that particular event is that it is included within our specious present. That is to say, it is presented to my consciousness with sufficient clearness to pass the threshold dividing the subliminal from the supraliminal.¹ Now obviously, clearness of presentation is a completely non-temporal feature, so that it is really a very poor sort of reason, when one comes to think of it, for fixing the present moment.

I do not for a moment suppose that this theory of the unreality of time in determined sequences will be acceptable to everyone, but the matter can be considered apart from any theories. We say that an event happens and that it happens in the present. Well then, what is it that happens when a completely determined event happens? Being completely determined by events and conditions which already exist, and thus being incapable of being changed in any way, its actual happening seems to be reduced to the level of a mere formality. The happening adds nothing to it. It is true that it may be presented in my supraliminal specious present while it is happening, provided that conditions for observation are favourable. But if this be the only difference between present, past and future, when no observer is available, happening is reduced to a mere formal possibility of being observed. I admit that these considerations are not conclusive, because the formal possibility may depend in part upon a real time relation. The nature of time can be investigated only by metaphysical examination.

¹ I do not wish it to be assumed that I consider clearness of presentation to be the only condition necessary for the passage through the threshold. There may be, and if Freud is right there are, others. Passage may be denied by the endo-psyche censor on account of the inacceptability of the idea. But I think that as regards the presentation of *events*, clearness is probably the main factor.

I have mentioned the above merely to show that the thing is not so obvious and transparent as it may seem at first sight. So far we have been discussing completely determined sequences, but there are grounds, for example those based on ethical considerations, which render universal determinism unacceptable to many of us. Everybody assumes for practical purposes that some degree of freedom of choice exists. Many people, including philosophers of the highest rank, accept it theoretically as well. Let us assume that we possess some degree of freewill; that is to say, that we can intervene to some extent in the course of events and exercise a directional influence over them. I am not going to discuss the grounds for this assumption; they have been discussed *ad nauseam* for the past thousand years, so it is unlikely that I should be able to add anything of interest.

If we are able to intervene and guide the course of events, it follows that they must be to some extent what I may call plastic; that is to say, they are not frozen to rigidity.

Now the temporal locus of the operation of freewill appears, on the face of it, to be the present, that is the specious present, not the fictitious present.

It may be objected that the doctrine of the specious present is all very well for psychology and for knowing about things, but that when it comes to the actual doing of things, to the happening of the events themselves, it really "won't wash".

Can things happen in a time that is actually as the psychologist's time appears to be?

I freely admit that I cannot conceive how this may be so, but then I can no more easily conceive how the alternative, *i.e.*, mathematician's time, can be real. But I have actual experience of psychologist's time, while the other is admittedly an abstraction. Thus I prefer to accept the view based on experience, even if I cannot understand it. Moreover, arguments similar to those used in the case of the specious present may be adduced in this matter as well.

An act of freewill must have some duration, otherwise it would cease to exist at the very moment that it started. That is to say, it would not exist at all. Also, it must impinge on events, if I may put it that way, over a definite length of duration for the same reason. The cutting edge of a tool, however sharp, must have some thickness; it cannot be a mathematical line.

I think that I need labour the point no further, but will take it as established that when I exercise freewill it extends over a length of duration included within my specious present.

Now the only observable difference other than temporal location between the various slices of the specious present is the degree of

clearness with which they are presented to my consciousness. Ontologically, they appear to be otherwise similar.

It may be that there are also degrees of what I have called plasticity. In fact, I suggest that this is so. Still, however this may be, some part of the period is plastic and susceptible of being moulded to some extent by the operation of my freewill. If this is true for the supraliminal specious present, it may be, and my theory is that it is, true for the longer specious present of the subliminal.

The view which I would like to suggest may be pictured somewhat as follows :

Imagine a bundle of wires or threads all running roughly parallel to each other. You are looking at the bundle through a narrow slit across its length so that you can see only a short section at any one time. The slit moves along in the general direction in which the wires run.

Not only can you see through the slit, but you can also touch the wires through it. Some of the wires are flexible and elastic, others rigid and immovable. You are able, therefore, to exercise a limited control over their arrangement and relative positions.

The wires that are capable of being moved have this peculiar property, that after the slit has passed over them they become rigid, so that the whole bundle astern of the slit is fixed and immovable.¹

The edges of the slit are not sharply cut, but have fringes, so that your vision is clearest about the centre and diminishes in clearness at the edges.

The bundle of wires represents a strand of history, the slit the specious present of the observer. The screen in which the slit occurs is the threshold between the supraliminal and the subliminal. Those wires which are rigid throughout their whole length are those events over which we are not able to exercise an influence by the interposition of our freewill; they are completely determined. The flexible wires are those which we can affect by freewill; they are not completely flexible and elastic and can only be moved to a certain extent, thus symbolising the limitations of our power over them.

To make the picture complete, a further complication must be introduced. The eye which looks through the slit is, of course, the eye of the supraliminal, but somehow or other the subliminal mind is on the other side of the screen all the time and is in conscious contact with the wires. If my earlier suggestion be accepted, this conscious contact is effected, not by means of the ordinary senses, but by an undifferentiated faculty of knowing.

¹ This is not strictly correct, as some slight amount of movement is possible astern of the slit. See Dr Broad's *Mind and its Place in Nature*, p. 256. However, this point does not concern us for our present purpose.

But leaving this aside for the moment, let us try to imagine what happens when the position of one of the wires is altered by the exercise of a freewill act operating through the slit.

The arrangement of that part of the wires which is seen through the slit is disturbed; astern of the slit they are all frozen into rigidity, so no effect is produced: but ahead the whole length of the wire touched is moved, so that the disturbance stretches indefinitely in that direction.

Thus an action in the present immediately modifies the entire future. If we suppose that there are forces of attraction and repulsion between the wires, the slight movement of one of them occasioned by the freewill act operating on the small section covered by the slit may bring about an upsetting of the equilibrium at any part lying ahead, and thus cause profound and far-reaching changes.

Suppose that I exercise my freewill and move my chair. In its original position it was in certain spatial and gravitational relations with the rest of the universe. By moving it I have changed these relations. The relations are reciprocal. My chair stands in a certain spatial relation with, let us say, a particular electron on Sirius; the electron has a reciprocal relation with my chair. To describe that electron completely, all its relations must be included; they are part of the conditions which determine its present and future existence. I move my chair, thereby altering some of these relations and with them the conditions which determine the present and future existence of an electron on Sirius. The entire universe is changed by my freewill act; its future is modified; it can never be the same again.

We may say then that the future is completely determined by the present, but is not inexorably fixed, for the element of plasticity allows that which determines the future, *i.e.*, the present, to be altered.

This plasticity pervades all duration ahead of, and including, the specious present.

I cannot see that the ontological difficulties raised by regarding the future as determined yet plastic are any greater than those which arise from the ordinary view which we take of the present; that is to say, as existent yet subject to modification by freewill. That there are difficulties I do not deny, but then ontology is always difficult.

We must look, then, on the future as existing but as being subject to change, so that at the present moment the future, say this time next week, is so and so, call it A.B.C., but in half an hour's time it may be different, say D.E.F., having been changed by some freewill act which I have performed in the meantime.

If I were now to experience a visual precognition of an event a

week ahead I should get a picture of A.B.C., but in half an hour should the precognition occur, it would be a picture of D.E.F.

I have one case which illustrates this rather well. It is the warning case cited from *P.* xi. 497. The visual precognition included the detail of the coachman falling from a box and crushing in his hat on the pavement. All the events occurred as foreseen up to a certain point when, owing to the interposition of Lady Z in calling to the policeman, the course is altered. So far as one can see, had not Lady Z. interposed, things would have happened exactly as in the dream.

Although I quote this case as an illustration, I am by no means sure that it is a true example. In the first place it is somewhat doubtful whether Lady Z's action in calling to the policeman was really a freewill action. I am inclined to regard it as impulsive and determined. Moreover, it might be suggested that the detail of crushing in the hat was an embellishment to the story given by the precognition, an added detail, that is to say, derived from the dream factory itself and not from outside.

As we can never determine with any degree of certainty whether any action is the outcome of freewill, any cases cited will be open to doubt. The theory, however, does not rest on particular cases, but on general considerations.

We may say, therefore, that the future exists now in the sense that it is determined by the present. Yet it is not immutably fixed, but subject to modifications which are themselves determined by actions taking place in the current present.

My theory is that the deepest stratum of the subliminal mind is in permanent conscious contact with this ever-changing future and that precognitions occur when, for some reason which we do not understand, the knowledge acquired by the subliminal of events lying ahead of the supraliminal specious present is somehow transmitted through the threshold; such knowledge, can, of course, be only of the future as it then is, or, in other words, as it then is determined to be by the present. It will have been observed that I have been using terms which imply the reality of time, whereas I have admitted my adherence to the opposite view. But when doing so I particularly limited my remarks to completely determined sequences, while now we are discussing non-determined events. Although I agree with MacTaggart that time is unreal as regards determined sequences, I hold that it is real, or rather I ought to say that change is real, for time is only the common element abstracted from change, in cases of non-determined or freewill events. A non-determined event is the coming into existence of something entirely *new*. In so far as man possesses freedom of will he is subject to true time. Hence I claim that I was justified in the above use of terms implying its reality.

It is obvious, of course, that the theory which I have suggested will account only for precognitions of determined events. I see no reason, and have no evidence for believing, that precognitions of non-determined events ever occur. In fact, I regard them as impossible.

That this view implies a very large restriction of the scope of freewill, I am ready to admit, but I think that it can be shown on other grounds that such restriction is necessary. A discussion of the matter would be out of place here, so I will content myself with stating that, in my opinion, freewill should be regarded as limited to a very occasional non-determined change and not as an often exercised faculty.

I regard it as possible that, although a man may theoretically possess some degree of freedom, he may not, in the whole course of his life, ever actually exercise it.¹

I am perfectly aware that this hypothesis which I have outlined may seem fantastic and far-fetched.

It may seem plainly repugnant to common sense to suggest that a future event may exist in any sense and yet be subject to change, but I cannot see that this difficulty is any greater as regards the future than as regards the present. The present undoubtedly exists, and yet, if freewill be accepted, it can be changed.

My theory does not deny the reality of time, or rather I should say of change, for even if the whole past, present and future is already existent, changes occur in some parts thereof, but it is only as a result of non-determined events, such as freewill acts, that these changes occur; for determined events the reality of time is denied.

The position as regards time on this view may be briefly stated as follows. Determined sequences of events do not exhibit real change; they are inexorably fixed. Therefore true time does not apply to them. They are subject, however, to temporal location; that is to say, they stand in relations of the temporal order before and after. Temporal location must be accepted as a fundamental feature of existence quite distinct from true time.

¹ The two varieties of the determinist theory would be represented as follows on our wire model. On the fatalist theory all the wires are rigid. Though the observer may possess freewill he cannot move them, for, whatever course he may elect to pursue, the future is unalterable. I doubt if this theory is really tenable. On the ordinary determinist theory, he cannot exercise freewill. He is only an observer, all his actions are represented by wires, there is nothing outside the bundle of wires which is capable of affecting them; therefore, whether they be rigid or not, their relative positions never change. All sequences are causal. On the freewill theory some of his actions are uncaused and are thus, as it were, outside the bundle of wires. Some of the wires, being flexible, can be moved by this influence from outside.

Man, as possessing freedom of will, can initiate real change, i.e., can bring into existence an entirely new event. He is therefore, in so far as he does this, subject to true time. He is also, both as regards his freewill acts and his determined actions, subject to temporal location, or rather the events in which he is concerned are so subject.

A temporal location series will appear as a true time series when viewed by an observer who is subject to true time. Hence the difficulty experienced in conceiving temporal location apart from time.

But as I remarked above, I do not for a moment suggest that this is a true account of the way in which events actually happen ; at the best it is only a more or less convenient way of looking at the facts. However, there is no doubt whatsoever in my mind that there *is* a fact to be explained, viz., that on a few rare occasions the human mind does make conscious contact with future events. If a better explanation than the one I have suggested can be put forward, I will gladly jettison my own hypothesis.

Whatever hypothesis we adopt, however, I think that this fact compels us to the conclusion that the time conditions governing the subliminal mind are in some way different from those to which we are accustomed in our ordinary life.

There is one final remark which I feel bound to make in concluding this already too lengthy report. If either the view of the curve of consciousness as approaching the line of duration asymptotically, or that where it is shown as running parallel, be accepted, the subliminal mind must be regarded as being in contact with the future over an indefinite period. It would then seem that our ideas on the question of survival must be affected thereby.

In a sense, we are now, at this present moment, living in the indefinite future ; hence the question, " Do I survive bodily death ? " takes on an entirely different complexion.

If we could find a well authenticated case of a precognition of which the fulfilling event happened after the death of the percipient, then I think we should have evidence relevant to the question of survival. I cannot say that I have found any such case among those which I have collected, but it is quite likely that such may occur. Although it is not definitely stated in his account, I think that the case of M. Berteaux quoted by Prof. Richet in *L'Avenir et la Prémonition* is probably an instance. It would be interesting to learn, if possible, whether the medium who made the prediction predeceased or survived M. Berteaux. I do not say that precognition proves survival, for it may be that physical death involves the withdrawing of consciousness from a future ahead of the date of death, but I do think that it is a factor which has to be reckoned with in any discussion of the subject.

APPENDIX A

INDEX OF CASES

Acc. = Accident to person not involving death.	I. = Impression.
A.D. = Accidental Death.	Inc. = Incident, not trivial.
Arr. = Arrival.	M. = Mediumistic.
B. = Borderland.	M.Acc. = Accident to material things.
C. = Crystal.	N.D. = Natural Death.
C.Ill. = Course of illness, medical prognosis.	Rec. = Recurrent.
Coll. = Collective.	Sym. = Symbolic.
D. = Dream.	R. = Rejected as vague or non-precognitive.
H. = Hallucination.	T.I. = Trivial incident.
	W.N. = Winning number, etc.

<i>Page</i>		<i>Journal</i>	<i>Page</i>		<i>Journal</i>
i. 121.	H.	C.Ill.	v. 76.	H.	N.D.
283.	I.	M.Acc.	87.	M.	N.D.
299.	D.	N.D.	274.	D.(Sym.)	N.D.
311.	I.	C.Ill.	276.	D.	T.I.
467.	I.	C.Ill.	vi. 116.	D.	M.Acc.
ii. 65.	H.	A.D.	118.	D.	N.D.
175.	C.	T.I.	vii. 272.	D.	T.I.
iii. 296.	H.(Coll.)	N.D.	306.	D.	Arr.
iv. 9.	H.(Sym.)	N.D.	321.	H.	R.
12.	D.	N.D.	329.	I.	R.
188.	M.	W.N.	viii. 12.	M. Arr.	C.Ill.
212.	H.(Sym.)	N.D.	13.	H.(Sym.)	Inc.
213.	H.	R.	45.	I.(Rec.)	M.Acc.
223.	D.	Arr.	104.	M.	C.Ill.
224.	D.	N.D.	147.	D.	M.Acc.
225.	D.	T.I.	161.	D.	A.D.
226.	D.	W.N.	194.	D.	A.D.
228.	D.	W.N.	viii. 265.	D.	R.
229.	D.	T.I.	ix. 15.	C.	N.D.
241.	D.	R.	60.	M.	T.I.
242.	D.	T.I.	73.	M.	T.I. C.Ill.
iv. 256.	H.	N.D.	79.	I.	N.D.
257.	H.	N.D.	80.	H.	T.I.
292.	H.	N.D.	126.	H.	R.
327.	D.	A.D.	128.	D.	R.

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	135.	H.(Sym.)	M.Acc.	xv. 173.	D. N.D.
	154.	D.(Sym.)	N.D.	324.	M. R.
	195.	H.	R.	xvi. 3.	H. N.D.
	246.	H.	R.	29.	I. Acc.
	337.	M.	C.Ill.	41.	D. Arr.
x.	22.	D.	T.I.	51.	D. T.I.
	24.	D.	T.I.	68.	D.(Rec.) C.Ill.
	26.	I.	T.I.	68.	D. N.D.
	27.	D.	Arr.	149.	H. R.
	28.	D.	T.I.	217.	H.(Sym.) Acc.
	38.	M.	N.D.	297.	D.(Sym.) M.Acc.
	61.	I.	M.A.	xvi. 301.	D. M.Acc.
	61.	I.	M.A.	xvii. 127.	H. Arr.
x.	78.	D.	R.	127.	I. T.I.
	136.	H.	N.D.	128.	I. T.I.
	163.	D.	N.D.	129.	I. A.D.
	165.	D.	R.	135.	I. T.I.
	189.	I.	T.I.	136.	I. A.D.
	292.	M.	T.I.	137.	I. M.Acc.
	308.	H.(Coll.)	R.	137.	I. M.Acc.
xi.	49.	D.	C.Ill.	145.	D.(Sym.) N.D.
	58.	H.(Coll.)	N.D.	xviii. 239.	M. T.I.
	64.	D.(Rec.)	A.D.	xix. 161.	D. N.D.
	123.	D.	R.	167.	I. R.
	141.	D.	R.	168.	D. T.I.
	145.	D.	R.	170.	D. M.Acc.
	184.	D.	R.	171.	I. R.
	223.	D.	N.D.	173.	D. A.D.
xii.	62.	H.	R.	xx. 119.	D. T.I.
	101.	D.	R.	120.	D. N.D.
	184.	H.	N.D.	321.	M. W.N.
	312.	D.	T.I.	323.	M. W.N.
	328.	D.	T.I.	323.	M. W.N.
	340.	D.	N.D.	324.	M. W.N.
xiii.	118.	D.(Coll.)	N.D.	xxi. 50.	M. T.I.
	141.	I.	N.D.	157.	C. A.D.
	142.	D.	A.D.	343.	D. T.I.
	167.	I.	R.	xxii. 88.	H. W.N.
	237.	D.	T.I.	xxiii. 102.	D. Acc.
xiv.	68.	H.	R.	xxiv. 342.	H. N.D.
	106.	D.	T.I.	342.	H. Inc.
	106.	H.	C.Ill.	342.	H. A.D.
	358.	I.	R.	342.	I. Inc.
	382.	M.	R.	xxv. 66.	H. N.D.

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xxvi.	1/4.	M.(Rec.)	M.Acc.		343.	D.	T.I
	1/4.	M.	M.Acc.		344.	D.	M.Acc.
	117.	I.	M.Acc.		345.	D.	T.I.
	129.	D.	Acc.		346.	D.	T.I.
					346.	D.	T.I.
					348.	D.	T.I.
					351.	D.(Sym.)	N.D.
v.	291.	H.	N.D.		vi. 21.	H.(Coll.)	N.D.
	293.	H.	N.D.			(Sym.)	
	295.	B.	N.D.		22.	H.	R.
	295.	H.(Sym.)	N.D.		245.	H.	R.
	298.	C.(Sym.)	N.D.		293.	H. (Coll.)	N.D.
	299.	B.	N.D.			(Sym.)	
	301.	H.	R.		295.	H.	Arr.
	302.	H.	R.		345.	I.	R.
	303.	H.	R.		367.	I.	R.
	304.	H.	R.		372.	H.	R.
	305.	H.	R.		374.	H.	Arr.
	306.	H.	Arr.		375.	H.	Arr.
	306.	H.	R.		390.	I.	Arr.
	307.	H.	R.		392.	H.	Arr.
	309.	H.	R.		392.	I.	R.
	311.	M.	R.		vii. 207.	M.	W.N.
	313.	D.	M.Acc.		218.	M.	A.D.
	314.	D.	T.I.		viii. 17.	M.	Arr.
	316.	D.	T.I.		34.	M.	R.
	318.	D.	N.D.		34.	M.	R.
	319.	D.	N.D.		34.	M.	N.D.
	320.	D.	N.D.		61.	M.	C.III.
	322.	D.	A.D.		96.	M.	R.
	324.	D.	A.D.		112.	M.	T.I. N.D.
	326.	D.	A.D.		115.	M.	T.I.
	330.	D.	N.D.		120.	M.	N.D.
	332.	D.	N.D.		121.	M.	T.I.
	332.	D.	Acc.		188.	D.(Sym.)	N.D.
	333.	D.	Acc.		227.	M.	N.D.
	333.	D.	Acc.		233.	D.	R.
	335.	D.	M.Acc.		244.	M.	N.D.
	337.	D.	T.I.		245.	M.	T.I.
	338.	D.(Rec.)	T.I.		246.	M.	A.D.
	339.	D.	T.I.		339.	H.(Rec.)	Acc.
	340.	D.	M.Acc.		376.	D.	N.D.
	341.	D.	R.		377.	D.	R.
	341.	D.	M.Acc.		396.	D.	M.Acc.
	342.	D.(Rec.)	W.N.				

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400.	D.(Rec.) ^s M.Acc.	484.	H. Inc.
401.	D. M.Acc.	487.	D. T.I.
496.	H. T.I.	488.	D. T.I.
505.	C. T.I.	489.	D. Acc.
x. 156.	H. R.	491.	D. T.I.
232.	H. N.D.	493.	D. N.D.
332.	H. Acc.	495.	D. Inc.
334.	H. N.D.	497.	D. Acc.
335.	H. T.I.	498.	D.(Rec.) M.Acc.
335.	H. R.	499.	M.(Rec.) A.D.
335.	I. R.	500.	I. Arr.
337.	H. A.D.	500.	M. M.Acc.
382.	H. A.D.	500.	M. M.Acc.
xi. 416.	I. M.Acc.	501.	D. T.I.
417.	I. M.Acc.	503.	B. Arr.
418.	H. M.Acc.	503.	C. T.I.
418.	B. M.Acc.	505.	B. Acc.
419.	I. M.Acc.	507.	D. M.Acc.
421.	H.(Rec.) M.Acc.	509.	I.(Coll.) Acc.
422.	I. Acc.	513.	D. T.I.
423.	H. Acc.	514.	H.(Coll.) T.I.
424.	H.(Rec.) M.Acc.	515.	D. C.III.
428.	D. N.D.	517.	D. A.D.
429.	H. N.D.	518.	D. A.D.
431.	I. N.D.	520.	D. M.Acc.
433.	M. N.P.	521.	H. N.D.
437.	I. N.D.	522.	D. C.III. T.I.
440.	H.(Sym.) N.D.	527.	D. C.III.
442.	H. R.	528.	M. N.D.
443.	H.(Coll.) N.D.	530.	M. R.
446.	H. C.III.	532.	D. Arr. T.I.
448.	H. N.D.	533.	D. T.I.
448.	H.(Coll.) N.D.	534.	D. Inc.
	(Rec.)	535.	I. R.
455.	I. R.	536.	I. T.I.
458.	D. T.I.	537.	D. R.
459.	I. Acc.	538.	H. R.
461.	H. Arr.	543.	H.(Sym.) N.D.
462.	D. Arr.	545.	I. W.N.
463.	I. T.I.	547.	H. R.
466.	D. T.I.	561.	D. M.Acc.
474.	D. Arr. T.I.	563.	I. M.Acc.
477.	I. T.I.	564.	D. M.Acc.
481.	B. Inc.	564.	I. M.Acc.

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	565.	D. M.Acc.		303.	M. R.
	566.	D. Acc.	xviii.	258.	M. T.I.
	568.	D. M.Acc.	xx.	321.	M. T.I.
	571.	D. M.Acc.		322.	M. T.I.
	573.	H. A.D.		323.	M. T.I.
	577.	D.(Rec.) N.D.		324.	M. T.I.
	581.	H. N.D.		327.	M. Inc.
	582.	D. N.D. T.I.		328.	M. R.
xiv.	249.	I. W.N.		330.	M. T.I.
	251.	B. W.N.		331.	M. T.I.
	253.	D.(Coll.) W.N.		335.	M. T.I.
	254.	D. M.Acc.	xxi.	270.	M. T.I.
	258.	D. N.D.	xxvi.	296.	I. C.III.
	266.	I.(Coll.) A.D.		424.	M. A.D.
	318.	D. W.N. N.D.		424.	M. A.D.
	320.	D. W.N.		425.	M. Acc.
	322.	I. R.		425.	M. Acc.
xvi.	109.	M. R.	xxix.	111.	M. A.D.
	358.	I. N.D.	xxxiii.	439/600.	I. Inc.
xvii.	85.	M. T.I.	xxxv.	560/89.	M. T.I.
	89.	M. T.I.			

APPENDIX B

ON THE MATHEMATICAL ANALYSIS OF THE FOREGOING CLASSIFICATIONS

It has been suggested to me by Mr Whately Carington that some information might be derived from mathematical analysis of the classifications given in the text.

It is obvious that in this matter we are unable to make any direct comparison between the numbers of successes and failures, as we have no means of making even the roughest estimate of the latter. Frequently a precognition is discovered only when it has succeeded, that is to say it is recognised as such only after it has been fulfilled. There are, of course, quite a large number of cases where the dream, impression or whatever it may be is definitely recognised as purporting to be precognitive before fulfilment, and there must be a large number of failures of this type, but it is quite impossible to estimate the proportion of success to failure or the proportion of precognitions recognised as such before fulfilment to those unrecognised.

But this inability does not preclude entirely the application of mathematical treatment. If we take a number of successful cases and classify them under various categories it is sometimes possible to estimate the chance distribution under the different headings, then, if it is found that the actual distribution shows a divergence sufficiently large to be considered significant we are entitled to infer that some factor other than chance has been involved.

For example take the number of cases in which the event foreseen was death and compare it with that of the other cases. We have Deaths 100, Other cases 190. If we divide them into Hallucinatory cases and other types we find that the proportions are different, for we have

	Death.	Non-Death.	Total.
Hallucinatory - - -	35	28	63
Other types - - -	65	162	227
Total - - - - -	100	190	290

By inspection there is a marked discrepancy. Hallucinatory cases show 35 deaths to 28 others = 55.55% death, while other types show only 65 to 162 = 28.63%.

To test whether this is significant we apply the usual Contingency formula

$$\chi^2 = \frac{(ad - bc)^2 (a + b + c + d)}{(a + b)(c + d)(a + c)(b + d)}$$

where a , b , c , d , are the four observed numbers.

In this case the value of $\chi^2 = 15.82$ and reference to the appropriate tables¹ shows that the probability of this distribution being due to chance is very much less than .01 (1 in 100) so that the observed discrepancy may be taken as definitely significant.²

It follows therefore, either that the distribution of precognition of death in the types of case other than hallucinatory is that due to chance, in which case there must be some other factor operating in hallucinations to produce a significant increase, or the distribution in hallucinations is due to chance, when we must admit that some other factor has operated in the other types to bring about a significant decrease, or else of course the distribution in no case is due to chance. Whichever view we take we cannot avoid the conclusion that chance alone is not responsible for all the phenomena.

It may be that the suggestion which I made when discussing the figures simply in the light of inspection, viz., that the higher ratio of death cases in hallucinations is due to the greater emotional tone attached to such events is sufficient to account for this discrepancy. And if this be so, an important conclusion follows. If the cause of the predominance of death cases in hallucinations arises from a peculiar characteristic of the event foreseen, it seems to follow that that event, while still in what we call the future, exercises an influence and produces an effect in the present. This, if admitted, goes a long way towards confirming the reality of precognition and, I submit, lends colour to the hypothesis which I have put forward to account for the phenomena. For the effect which is produced is an effect on the mind of a percipient exactly similar in character to that which would be produced if the event were perceived in the normal way, i.e. as a present event. This is, however, somewhat of a digression. To return to the question of mathematical treatment of the data. In selecting the particular categories to be analysed

¹ Fisher's *Statistical Methods for Research Workers*, Table III.

² I am indebted to Mr Whately Carington for help in computing these significances.

there are certain points which must be considered and certain corrections which must be made.

The first subdivision of the cases is into "good", "ordinary", and "non-precognitive", and these are classified under the different headings such as dreams, impressions, etc.

Now as regards this classification the following considerations must be borne in mind.

(1) The classification simply represents my individual opinion and other judges might arrive at different results.

(2) The basis of the classification is twofold, (a) amount of detail, (b) quality of evidence.

As regards (a) this might in some cases be susceptible of mathematical treatment, but (b) stands on quite a different footing. A perfectly good case may be spoiled, either because the investigator neglected to obtain confirmatory evidence, or because the witnesses refused on quite irrelevant grounds to give that evidence. Or again the circumstances might have been such that no confirmation could have been possible, yet the precognition might have been genuine for all that.

The fact is that this classification of cases refers, not to the occurrence of precognition or the characteristics of the phenomenon, but solely to our grounds for believing that it occurs.

It seems possible at first sight that a useful comparison might be made between the ratios of good cases to ordinary of two different types, e.g. between dreams and impressions, as, although the figures are simply the expression of my opinion, I am not likely to have erred in favour of one type against the other. Any error may be assumed to be spread uniformly over the whole and would not, therefore, vitiate the result. Still, owing to the classification being based partly on quality of evidence, I cannot see that it can yield any information about the nature or cause of precognition.

As regards the classification into the various types of event foreseen this objection would not apply; there is little room for error and practically none for individual bias, the only possible loophole for differences of opinion would be those which I have mentioned when discussing the figures of these classes; they are, however, quite unimportant as regards number.

As following on the line of thought suggested by the example worked out above, I think that a fruitful comparison might be made between precognitions of events having high emotional tone, i.e. deaths, accidents and non-trivial incidents, and those not having this, viz., trivial incidents and winning numbers. I leave out the categories "course of illness" and "arrival", as being most likely due to other causes such as telepathy, auto-suggestion, etc. I also make a deduction from "accidents to material things" of

Borderland 1, Impression 6, Hallucination 4, to allow for possible hyperaesthesia. We then have the following :

	Dream.	Borderland.	Impression.	Hallucination.	Mediumistic.	Crystal.
Emotional - -	68	4	19	43	21	3
Non-Emotional -	41	1	8	6	27	—

Inspection clearly shows that the two types, Impression and Hallucination have a much higher ratio of emotional events than the other types collectively. Suppose that we take them together (throwing the two small types, Borderland and Crystal in with Dreams and Mediumistic as being naturally allied thereto), then we have

	Hallucination and Impression.	Dreams, etc.	Total.
Emotional - - - -	62	96	158
Non-Emotional - - -	14	69	83
	76	165	241

Testing for significance we have

$$\chi^2 = \frac{(62 \times 69 - 96 \times 14)^2 \times 241}{158 \times 83 \times 165 \times 76}$$

$$= 12.61 \quad P = < .01$$

which is significant.

I would suggest as an interpretation of this significance that in dreams and mediumistic cases we have a percipient already in a state of dissociation, so that messages from the subliminal meet with comparatively low resistance in getting through; hence events lightly charged with emotion may be the subject of precognition, whereas in the case of hallucination and impression the requisite state of dissociation does not exist ready to hand as it were but has to be created, so that events which are highly charged with emotion stand a better chance of getting their messages past the threshold.

There are two points which must be considered in this connection.

First, it might be objected that events of high emotional tone, being so much more striking than those lowly charged with emotion,

are more likely to be noticed and apparent instances of precognition recorded. This must be admitted, but as the error arising from this would presumably be evenly spread over the various types, that is to say would not apply with greater force, say, to dreams than to hallucinations, it will average out and not materially affect the result. Secondly, it might be said that the cases as to which we have data do not represent a fair sample of the whole number of precognitions actually occurring. I have given reasons for holding that in the mediumistic type the number in my collection is probably a small fraction of the whole ; I think that the same thing applies to impressions and, to a smaller extent, to dreams. But hallucination is itself so striking a phenomenon that the omissions here may reasonably be supposed to be considerably less than with the other types.

Further, the cases not recorded would most probably be mainly precognitions of trivial events as being less striking and thus more easily overlooked or forgotten ; the additions would therefore be chiefly to the figures for " other events ".

We therefore have an error of understatement in two types on the dream side and one type on the hallucination side, viz. impression. Assuming that the error is of about the same magnitude in all cases, the effect would be to increase the figures for non-emotional events, and this would make the significance of the discrepancy more marked. I think that the above is sufficient to indicate the possible scope of mathematical treatment and I will content myself for the present with the two examples worked out. I would suggest that if anyone was sufficiently interested in the matter there is a possibility of useful work in further analysis of the data by this method.

PROXY SITTINGS WITH MRS LEONARD

BY MRS LYDIA W. ALLISON

FOREWORD

READERS of the Rev. C. Drayton Thomas's paper in a recent Part of our *Proceedings* (xli. 139-185) will be aware of the nature and purpose of "proxy sittings." They have the advantage that the sitter, being ignorant of the circumstances to which the communications ostensibly refer, cannot give anything away consciously or unconsciously; nor can direct telepathy from the sitter be considered an explanation of any supernatural knowledge these communications may show. When therefore we come to weigh the claims of various possible hypotheses, we have one less to put into the scales.

Mrs Allison, the proxy sitter in this case, has herself described the circumstances in which she came to hold the sittings and no further statement is necessary on that point. There is, however, one matter to which it may be well to call her readers' attention at the outset.

By far the strongest evidence linking the trance-statements with the absent sitter, Mr John F. Thomas, and the purporting communicator, his wife, were obtained at the fourth and last sittings. The statements made in the three earlier sittings, though for the most part applicable to Mr and Mrs Thomas, were with a few exceptions of such a character as to be near the borderline of what may be accounted for by chance coincidence; upon which side of this border they lie is a matter for individual judgment. But the relevance and accuracy of the statements made at the fourth sitting are beyond what chance will easily account for, and since in this sitting we are ostensibly concerned with the same persons as in the first and third, we are justified in regarding these three sittings as forming a single whole. The case of the second sitting, to which objects provided by Mr Thomas were taken for "psychometry," is in my opinion more doubtful, because Feda distinctly stated (p. 114) that the persons here referred to were not the same as in the first and third sittings. This is quite possibly a mistake on Feda's part, but it has to be recognised that the second sitting is not entitled to the evidential advantage accruing to the first and third from their being definitely linked with the fourth. The incidents which seem to afford the strongest grounds for supposing that the second sitting falls into line with the others are the references to the mausoleum and the plant without flowers (pp. 115-6).

HELEN DE G. SALTER.

INTRODUCTION

IN the autumn of 1927, Mr John F. Thomas, of Detroit, Michigan, called on me one evening, introduced by Mrs Thomas Guinan, at that time secretary to Dr Walter Franklin Prince, of the Boston Society for Psychic Research. Mrs Guinan had previously written that Mr Thomas and his son had had some interesting sittings with mediums in England and America with his deceased wife as the purporting communicator. I did not attach any special significance to his visit because persons connected with psychical research have frequently called on me when passing through New York, at the suggestion of some mutual friend or acquaintance.

In the course of a casual conversation about the Boston Society, (of which we both are active members), and related topics, Mr Thomas told me that Dr J. B. Rhine of the psychology department at Duke University was editing the records of Mr Thomas's experiments. He did not mention any details concerning the sittings and said nothing that gave me the slightest hint of his personal affairs. I did not see Mr Thomas again until two years later, in November 1929, after the sittings on which this report is based had been held. When I recalled how at our first meeting he had avoided personal subjects and references to his sittings, he replied that he habitually refrained from allusions that might be informative.

Shortly after Mr Thomas's first visit I saw Dr and Mrs Rhine, who stopped in New York *en route* to Duke. Dr Rhine told me that he was working on a very interesting set of sittings collected by Mr Thomas, but mentioned no details. These facts were the only ones I could recall concerning Mr Thomas, when a letter arrived from him on 10 May 1929, one week before my departure for England, from which I quote: "Mrs Guinan writes me that you are going to London this summer and I have been wondering whether you would be interested in trying out an experiment with me there. I still have a London secretary (Mrs Hankey) who holds absent sittings with various mediums from time to time. It seems to me it would be quite good evidence if you would use this secretary for a few sittings with such mediums as Mrs Elliot and Mrs Garrett. If you were unknown to the secretary and any of my people participated in the sitting the material would at least serve to stretch the telepathic hypothesis in order to cover the circumstances."

In response to my request that he describe more fully what he had in mind, Mr Thomas replied that he would like me to take some articles to various mediums abroad for psychometry. I answered that I preferred Mrs Leonard to any other medium, enclosing my London address, and as my sailing date was only three days distant

I also wired him to forward the articles directly to the ship. They came with a note from Mr Thomas saying he would write me further to London. Shortly after my arrival there I received the following message: "Thank you for your letter and the telegram. I hope the articles reached you. How would it be to try a sitting with Mrs Garrett and Mrs Leonard before you get in touch with Mrs Hankey? It is possible that a skeptic might raise the point that the mediums associate Mrs Hankey with me and that material appropriate to me follows. Of course, I know there is nothing to the point but the most complete way to answer it is to avoid it. After a sitting or two before Mrs Hankey is brought in, there would be distinct advantages to working with her. [As will be seen later I did not follow these suggestions. There remains the possibility of normal leakage.] As to Mrs Leonard, she is, of course, without a peer. I had two personal sittings (anonymous) with her in April and May 1927, and Mrs Hankey (a professional recorder accustomed to taking sittings either personal or absent) held one for me using articles on 29 October 1928."

On this latter occasion Mrs Hankey told Mrs Leonard that the sitting was for the American who sat with her in 1927. This admission, because of the comparatively small number of American Leonard sitters and Mrs Hankey's having recorded Mr Thomas's personal sittings, may have immediately suggested an association to Mrs Leonard, a surmise which, however, does not affect this report. Mr Thomas added in his letter that he had not mentioned Mrs Leonard to me because he knew how she is importuned for appointments and he did not want to presume on my entrance there. With a single exception, I have always stenographically recorded my own Leonard sittings. I therefore decided to follow my usual procedure in this instance and avoid any suspicion that might arise from my being accompanied by a recorder. Mr Thomas did not know the dates of my sittings.

A passage from the record of an absent sitting held for Mr Thomas with Mrs Soule in Boston on 27 February 1928, Miss Lovewell, a professional stenographer, recording, and Mrs Thomas purporting to communicate, appears to have a bearing on this experiment, although its value is dubious. Mrs Soule, who is also known as Mrs Chenoweth, is the well-known Boston medium who figures so largely in the investigations of Dr James H. Hyslop and Dr Prince. It reads thus: "I have seen a friend of yours whom I did not know before I died and I think she will be a help to us. I wonder if you will not be glad to have me refer to a Mrs A. who has talked with you. Well, I have seen her husband over here and he feels as I do that we are very dependent on the dear ones on earth for our success in getting our messages over." My last sitting with Mrs

Soule was more than five years before this reference was received. I have no reason to think that she knows my name, as I had always gone to her under a pseudonym, not beginning with "A."

As stated above, Mr Thomas had called upon me for the first time in the autumn of 1927. Commenting later on the prediction, he writes: "I am sure I had never told anybody about my call upon Mrs Allison in New York. As a matter of fact, it was not in my own mind, because I hunted about mentally among the relatives and mutual friends of Mrs Thomas and myself to fit in this reference and finally gave it up as a lost cause. It did not occur to me that it was about Mrs Allison until after I wrote her about joining with me in the London experiments."

On the other hand, it never occurred to me not to mention Mr Thomas's visit. I recall two instances when I did so, both times to friends active in psychic research. The prospect of future work together never entered my mind. Our meeting might, of course, have reached the ears of Mrs Soule. Even had she been informed of our conversation, the point remains, however, that she predicted my participation fifteen months before it was actually considered. The following points in her statement are correct: "Mrs A." would apply to me; Mrs Thomas had not known me; Mr Thomas and I had talked together and my husband is dead.

Blanket Statement.

For purposes of brevity I have adopted certain abbreviations and designations for use in this report. Throughout I assume that the communicators are what they represent themselves to be and omit the qualifying word "purported." The most frequent communicators in my previously reported sittings, who again assist in these, are Dr James H. Hyslop, described by Fedá as "the 'fessor," "the test gentleman" or "the older gentleman" and my husband, Dr Edward Wood Allison, whom I refer to as E.W.A. and whom Fedá calls "your gentleman." The initials E.L.T. specify Mrs Thomas, who was the principal communicator in Mr Thomas's Leonard sittings, as she appears to be in mine. Fedá addresses me as "Mrs Lyddie"; my own initials are L.W.A. Mr Thomas's report, "Case Studies Bearing upon Survival," *Boston Society for Psychic Research*, 1929, is indicated by "Case Studies." His series of four Leonard sittings preceding those of this report were held on 26 April 1927; 12 May 1927; 29 October 1928; and 20 May 1929. At the first two Mr Thomas was present, with Mrs Hankey recording; the others were recorded by Mrs Hankey, who presented articles which Mr Thomas had sent to her for Mrs Leonard to psychometrise. These sittings are referred to as Sitting 1, present; Sitting 2, present; Sitting 3, absent, and Sitting 4, absent. Since Mr Thomas

was almost a complete stranger to me and I knew hardly anything about his family or his affairs, I have throughout this report stated the rare instances where I personally could definitely affirm or deny the truth of Feda's remarks. Otherwise, it may be taken for granted that I had no conscious knowledge of the degree of appropriateness of all Feda's other statements.

*Sitting with Mrs Osborne Leonard, Monday 3 June 1929,
Tankerton, Whitstable, Kent.*

Sitter: L.W.A.

It was not my intention in this first sitting of my 1929 series to invite failure by proposing an unusual experiment. Moreover I wanted the sitting for myself. I had left Mr Thomas's articles behind and hoped to revive the easy, cordial relations that have for some years existed between Feda and myself before venturing to suggest psychometry. Feda opened the sitting with several statements which were appropriate enough to me and a prediction which thus far has not been fulfilled. She then went on to give me a few further examples of her apparent faculty for acquiring knowledge of contemporary events concerning sitters. These are omitted from this report. All the annotations given below are by Mr Thomas, except those in square brackets, which are my own.¹

Record of the Sitting.

Mrs Lyddie, will you do some work in America with a man?

He is not an old man, only about forty. He [E.W.A.] feels this to be a very nice man, not an ordinary nice man, but an extraordinary nice man.

He is interested in psychic things and he's interested in a sort of scientific way and your gentleman says he has got the human way.

Someone you have already met but your gentleman thinks more to do later.

Annotations.

[Naturally, my mind at once jumped to Mr Thomas, but I did not reply.]

Fifty-five during the summer of 1929. [I should have thought Mr Thomas well under fifty.]

Correct. [This, of course, I knew.]

Correct as to first part, possibly as to second part. [At this time I had met Mr Thomas only once. Since then he has been to see me to discuss my preparation of these records.]

¹ As explained in the Foreword, this and the two following sittings have been considerably cut. The proportion of new and correct material to the whole amount given can be judged from the chart printed at the end of each sitting.—*Hon. Ed.*

Has "B" anything to do with him? He keeps seeing a "B."

(I don't know, I'll inquire.)

It seems as if you've already been in touch with this man, but not so close, I think, as you will be later.

Wait a bit. Does this man do writing? Because he gets a feeling of a good bit of writing. As if he has got some more writing to do and it will be about your work.

Your gentleman says he has already written about it.

(Feda, has he ever been to you?)

Mrs. Lyddie, I do not think it is anyone I know well; I do not get a strong feeling, but your gentleman says there is a slight link between him and Feda. Mrs Lyddie, I can't feel the man yet, but if he has it must be once or twice. Not like a sitter I know properly. It's *not* Mr Wallie. [Dr Walter Franklin Prince.]

[A reference to Dr Prince's sittings with Mrs Leonard follows, but is here omitted.]

This isn't someone I know closely. And your gentleman says of course it is a younger man than Mr Wallie, and I think he would be a little taller and a little narrower build than Mr Wallie. It doesn't feel like someone I

This "B" may indicate Boston. [While I had not thought of the connection of "B" with Boston, I knew Mr Thomas had had many sittings there. In Sitting 2, present, Feda mentioned a place beginning with B and then gave the name "Boston."]

[Which was also an admission that I placed this "man."]

This applies to a report written during the few months preceding these sittings and also to a thesis on psychic research that is in the future.

Correct.

I had had two personal sittings with Mrs Leonard, and Mrs Hankey had held two for me.

[Mr Thomas is younger, taller and narrower in build than Dr Prince. Both gentlemen had sat with Mrs Leonard.]

know properly. He hasn't got a very long name, your gentleman says. It isn't a long one, but he is rather important. Rather important to the work and so he wants you to keep your eyes on him ; to keep in touch with him.

(All right, Feda.)

[After a brief digression to her own affairs, Mrs Allison suggested that Feda should return to "this other gentleman."]

He said about forty, not a fat man ; in build about medium.

His face is not short ; a little longer but not sharp here. [Running hand around chin.] Not pointed ; rather rounded, firm, not a big fat jaw, but though thinnish, firm.

I get the impression that he wears glasses for something, sometimes, though he is not old.

Your gentleman says there is a reason for speaking of glasses just now. He says Feda got that from your gentleman because he had noticed that this man has been thinking very specially of new glasses just lately. He says perhaps you can verify that after.

It has been suggested to me that the name (John F. Thomas) was possibly emphasised by Feda as not very long to distinguish him again from Dr Walter Franklin Prinee, who is often referred to by his full name.

Both points describing build are correct. [Known to me and to Mrs Leonard if Mr Thomas is meant.]

Correct. [Known to me and to Mrs Leonard as before.]

I have used glasses for some years for reading, but I do not wear them otherwise. [On inquiry Mr Thomas wrote me that he did not wear glasses at Mrs Leonard's.]

I had been having my eyes retested for new glasses, partly as a result of a statement that I received in an absent sitting with Mrs Garrett, on 2 February 1929, recorded by Mrs Hankey, and sent to me in Detroit, as follows : "I do not think he wears glasses in the ordinary way but she is giving me that there is something wrong with one eye. I wonder if he has not got to attend to his eyes . . . in some way to the right eye." My visits to the oculist on 20 and 21 May 1929 were the first times I had consulted an oculist for several years. His diagnosis bore out Mrs Garrett's statement, the

But he has used glasses already. Only he happened to be thinking of something very special just now.

The test gentleman [Dr Hyslop] is interested in him and he is interested in the Professor.

He has, as a matter of fact, he has been doing something in connection with me and using my name [presumably Dr Hyslop's name] just recently. It was rather important to get that because he says he can corroborate it.

Has this gentleman lived in two parts of America ?

Mrs Lyddie, close to where he is now, do you know if there is a place with a name that would suggest water ?

(Do you mean the place is near water, or the name of the place suggests water ?)

I mean the name of the place he is thinking of now suggests water, like river.

right eye being more out of focus than the left. On 3 June I was still thinking "very specially of new glasses" because I had the specifications but as yet had not ordered the new ones, partly because I was not sure that they would be any improvement over the ones I am still using. I have not as yet (9.6.29) bought the new ones, but expect to do so.

Correct.

[I did not know that Mr Thomas was especially interested in Dr Hyslop. Fedra casually alluded to Dr Hyslop in Mr Thomas's first three Leonard sittings, with appropriate but not evidential remarks.]

Within three or four weeks I had been going through the *Proceedings* of the American Society in order to find a quotation from Dr Hyslop with which to close my report to the Boston Society, entitled "Case Studies." I found one and used Dr Hyslop's name in this report, which was mailed out from York, Pa., about six days preceding this sitting.

[Mr Thomas has resided in various parts of the United States.]

At the time of this sitting I was at my summer cottage at Orchard Lake, about twenty-five miles out from Detroit. [I naturally would have thought of Mr Thomas in Detroit. Perhaps it is well to emphasise again how little I knew about him. That he had a summer home never occurred to me.]

He may not be just where you would think he was. He may not be. So you see what I am feeling so strongly, when getting this name is that it *means* water.

As I got that the test gentleman said as a matter of fact, he is near water now, but it has nothing to do with me. He means close to the water now, perhaps at this moment. Is he packing up now? Because I get a strong feeling of his moving and packing.

(Fortunately, Fedra, I don't know.)

(Fedra, why are they giving me so much about him? I am interested, of course, but why?)

Because you already have a slight link and you are going to be more linked with him.

(Well, it interests me very much.)

Do you know, has he been living in a place where there are very tall trees with very large leaves, like a long straight trunk and the branches coming out with very large leaves?

(I will find out.)

The bottom hasn't branches. They grow rather like a pine tree. Like that kind of Gothic; no branches coming up from the bottom.

This sitting was held from six to seven o'clock in the morning, Detroit time. I find from my diary that at six a.m. I was at my cottage directly on the shore of Orchard Lake. I left there about six-thirty for my office. The reference to "packing" is an interesting point, because undoubtedly just at that time I was picking up and packing my bag and brief case to come into Detroit for the week. Florence, my daughter-in-law, and I have done a good deal of joking about ways to eliminate this picking up and packing, which comes so often that it bores us a little.

My home in the city is in a place of just this description. The section is called "Russell Woods" and has an unusual number of tall elms, which begin to branch out high up. "Like that kind of Gothic" is a vivid reference to the immediate surroundings of my home. Mixed in with these elms are a number of sycamores and some maples. Two of these sycamores are very close to my own lot and they meet the point of relatively large leaves. [I knew nothing of Mr Thomas's home surroundings.]

In the winter, last winter, things were happening to link you up with him. There were things happening then and the 'Fessor and your gentleman were rather helping it. . . .

[Feda, as is her custom, added her goodbyes and the control ceased.]

CHART

New, Correct Material.

Repetitions: Material common to both Mr Thomas's Leonard sittings and my own, with nothing of consequence added in mine. Frequently subjects only touched on in mine are developed at length in Mr Thomas's.

New Associations: Material enlarged on in my sittings from a source in Mr Thomas's Leonard series, with references which suggest associations in the mind of E.L.T.

Wrong Material: Including occasional unverifiable statements.

Contemporary Facts: Feda's knowledge of events in progress near the time of the sitting.

New, Correct Material	Repetitions	New Associations	Wrong Material	Contemporary Facts
Work with man Writing Glasses Mr Thomas's interest in Dr Hyslop Distance be- tween homes of Mr Tho- mas and L.W.A. Early link be- tween Mr Thomas and Dr Hyslop Name that sug- gests water Tall trees around Mr Thomas's home	" B " " M "		Journey be- tween New York and Detroit	Mr Thomas near water and packing to leave " Upsetting " experience and packing

Sitting with Mrs Osborne Leonard, Thursday 6 June 1929.
Sitter, L.W.A.

To the second sitting of this series I took the articles Mr Thomas had sent me for psychometry. There was, of course, no intimation that these articles had any relation to the previous sitting. This was the first time that I had ever asked Feda to psychometrise anything. Considering the difficulty of obtaining Leonard sittings there was more reason for Feda to suppose that I had a personal interest in these articles than that they were the property of a stranger.

[Almost the whole of this second sitting is omitted. The statements made, although many of them were correct as applied to Mr Thomas, were for the most part rather vague, and of such a kind as to leave too much scope for individual interpretation. Moreover, as stated in the Foreword, Feda never seems to have grasped the fact that the persons discussed in this sitting were the same as in the first and third sittings. Brief extracts, in which statements of a more distinctive character were made, are printed below. The whole amount of material given is again taken into account in the chart.—Hon. Ed.]

Record of Sitting.

[After references to two persons, one "on earth" and one "passed over":]

I am not sure which it would be, the one on the earth or the one passed over, but I get an ache in the side.

The person connected with these has had a fall.

(Do you know which one?)

Yes, the one that passed over.

Annotations.

One of the very few physical disturbances in my life has been an ache in the side and back, especially the back.

The ache was the direct result of a fall which took place at least seven years before E.L.T.'s passing.

Wrong. The fall in question applied to me. [My question, "Do you know which one?" seems particularly stupid, since Feda had introduced the subject of the fall with "I am not sure which it would be." Definite leading questions to elicit evidence are always a gamble. I find it difficult, however, to resist the temptation, as on a few occasions the results have warranted taking the chance.]

It wasn't dangerous, it was upsetting.

(What else, Fedda ?)

Grounds, 'mental grounds. Mrs Lyddie, would you know if they had lived near very ornamental grounds; not like a pretty garden, not like Gladys' [Mrs Leonard's name] but more with gravel paths, grass, laid out flowers in a sort of stiff way, what you call ornamental ?

I get a funny shaped feeling like a beehive. Glass in it as if it was made only of windows, like a beehive with windows.

I feel they would look at this place with windows and talk about it.

You go up and down stone steps, very like stone only the colour of marble. That is right, very nice steps, beautifully made steps. There are lumps of rock.

My fall did not prove to be dangerous, but it certainly was upsetting, both literally and in other ways. The word "upsetting" might be intended as a pun.

This description could apply to any modern cemetery. [It could also apply to a formal garden. Mr Thomas no doubt read the entire passage before making his annotation.] We had not lived particularly near any cemetery of this description. All the points apply to the cemetery in which the mausoleum where E.L.T. is interred is located. We had not lived nearer to this than seven or eight miles at any time, but we often drove by it.

Nearly every time E.L.T. and I came in sight of this building when driving we did look at this place and talk about it in a rather desultory way as a place for burial. As a matter of fact, the reason burial was made there was because of these more or less light-hearted references we had made to the building. This was the first time any member of the family was placed in the cemetery. We had and have no family lot.

To get to the particular crypt, one climbs quite striking stone steps made of marble. The "lumps of rock" are the marble slabs that seal the fronts of the crypts. [In Sitting 4, absent, 20 May 1929, Fedda described Mr Thomas as looking up at a big, white, gleaming stone.

(Is this building near the garden, or does it belong to another place?)

No, they belong to the same place; it is in the garden. I can't get more.

Mrs Lyddie, do you know if the one on the earth has had aches in the feet and legs lately?

(Why, Fedra?)

I'm getting an achy and tired feeling about the feet and legs. That comes rather strongly.

When one passes over you know how people gives them white flowers?

Well, do you know whether anything was done that was *not* white flowers but more like leaves? I have a feeling something symbolical, not any old leaves, something made of green leaves with symbolical meaning, especially made. Leaves, not flowers; it's leaves I'm getting.

Mr Thomas interpreted this passage as referring to the mausoleum, but no details were given.]

Correct.

I have been conscious of an achy feeling, noticeable only when going upstairs, therefore there may be some relationship between this statement and the following one.

This may refer to the mausoleum. The achy and tired feeling was due to too much golf and I noticed it only when I had occasion to go up a flight of stairs, such as the marble steps at the mausoleum. One striking thing about the remembrances in this mausoleum is the predominance of wreaths and other symbolical pieces made of leaves, a fact that is due to the impossibility of keeping flowering plants alive and fresh in a building of this kind. I had, myself, been looking for some kind of a plant of green leaves that would live and thrive there and I had finally before this sitting decided upon a fern, which was not placed until 31 July. I find in my diary that I stopped at the mausoleum on my way out to Orehard Lake on 2 June, four days before Mrs Allison's sitting.

I had made only one visit there since the preceding fall. Jerome [Mr Thomas's son] tells me that he and his wife visited the mausoleum on Decoration Day, 30 May, six days preceding Mrs Allison's sitting of 6 June, and at that time placed a plant there that had no flowers but "all leaves" and that he remembers the circumstances because they wondered whether a plant with just leaves and no flowers was suitable.

CHART

New, Correct Material	Repetitions	New Associations	Wrong Material	Contemporary Facts
Case not used in place where made Description suggesting Mr Thomas's mother Mr Thomas's brother Initial of mother's first name Middle-aged lady and gentleman connected with case. One here, one passed over Ache in side Fall—not dangerous "F" connected with case Lonny Fort or castle	Mr Thomas's psychic experiences E.L.T. not well before passing Lived in high place	Mausoleum	Foreign feeling Slippers Fall applied to E.L.T. "F" someone L.W.A. did not know "M" Arnold	Worried over business Head covering Going to the country "Achy" feeling Plant without flowers

Sitting with Mrs Osborne Leonard, Monday 10 June 1929.
Sitter, L.W.A.

I have always found Feda very suggestible. It therefore seems natural that the trend of this third sitting follows the thread coaxed along and encouraged by me in the first sitting and further developed in the second. Although, as will be noted, Feda did not connect the personalities, living or deceased, that she introduced in the first and third sittings with those of the articles I presented for psychometry in the second sitting, yet it is obvious that while she herself denied the connection, the content of all three sittings is fairly homogeneous.

Record of Sitting.

Good morning, good morning. I was just talking to them. They are both here and send their love. Your gentleman says, "I have constituted myself"—I wonder what that means, say messenger—such a funny word, "a messenger will do." Because lately a lot of people have been asking him to get in touch with people on the earth through you. People I never saw before in my life. But he say I told them in your sittings I have not got time to help them all. (Feda, I wish he could help the people he spoke of in my first sitting.)

He is going to help them. He was just approaching the woman. In the first sitting he told you about a lady who passed over not long ago, but a lady in the mid-dles. And he wanted to help her because she was a very good woman and also because the people that belong to her do miss her so much on earth.

This has nothing to do with the people he was speaking of at the last sitting. It was the *first* sitting.

This lady he is speaking of now

Annotations.

[Apparently I myself confused the content of the first and second sittings and Feda took up the confusion.]

E.L.T. passed over three years and two months before this sitting, at the age of forty-eight.

[My guess would have been anywhere between two and five years before and that E.L.T. was probably middle-aged.]

[See introductory note to this sitting.]

has got a gentleman she is very fond of and is trying to get back to. And it is to help the gentleman on the earth that I am taking part in this.

(Will she help me to place the gentleman?)

That is what she wants to do. It is your gentleman speaking really, but in a way he is speaking for her. The person on the earth is not someone who is always with you. He seems to be away from you. I do not mean in England, but that man in America isn't close to you, but in a different condition and place. I get the feeling that you know him more through writing than through seeing you. And it isn't only that he is in a different—but the kind of place and circumstances are very different.

Then he gives me a rather cut off feeling, as if he is seeing himself out of things. "It is not easy to get help, to get communication here." He gives me that feeling rather strongly. I do not think he is getting a lot of help where he is and he is thinking of you as being able to help him more than he can get help where he is.

Now I am seeing a big "B." I think that has got something to do with the man.

In a way he is rather important. He is not like many people in the place where he lives; he

Correct. [See Introduction.]

[Mr. Thomas had made a special point of holding sittings in places distant from his home, Detroit. One partially successful sitting with a mental medium, of which he made only a few notes, and four or five with a physical medium, which were unsatisfactory, constituted his total experience with Detroit mediums. It was true from his point of view that he was "cut off" from communication in his home city.]

"B" suggests Boston. [In Sitting 2, present, Feda had connected Mr Thomas with Boston. See note on Mrs Soule above and annotation of "B.R." below.]

These points apply in quite an apt way to my work in Detroit. I am assistant superintendent of

is rather well known, like people respect him and looks up to him. I do feel he has taken rather a prominent hold in semi-public things. He has had a good deal to do in that kind of way. His name would be known to people that isn't friends of his.

(Can my gentleman get more about his name ?)

He wants to. He said it isn't a long name. Is there an " R " in it ?

(What do you think ?)

There is a " B " but an " R " came up rather big. I know the " R " is rather important. You won't forget about the " B." It is rather important. That may be something you do not know. I feel he is getting that from the lady. It is a special name from the lady. I do not feel you would know it, but it is more important than a name you would know.

Did you know if he had been thinking of a newspaper or magazine ? There is something rather important just now about a magazine, or newspaper.

That's right, and he said this man had an ailment himself ; before the lady passed over he had a rather delicate state of health for a time and it looked then as if he might pass over. You know, that wouldn't have been so surprising if he had and the lady was very anxious then. She wasn't so anxious about herself ; she was more anxious about him, and afterwards it was *she* that went and *he* that stayed. Yes, she had been very anxious.

public schools in charge of the finances of the Board of Education.

[Obviously, there is neither a " B " nor an " R " in Mr. Thomas's name, but he says that " B.R." is the only living member of his father's family besides himself and that he refers to this relative more often by his initials than he does by his name, a fact that is true of all the family. E.L.T. knew him well.]

[Too vague for annotation.]

[Mr Thomas thinks this passage refers to an accidental fall he had about seven years before the passing of E.L.T. over which she was much more worried than he, as he did not consider the effects of the fall at all dangerous. This reference is another instance of Feda's giving similar material regarding E.L.T. and Mr Thomas in the psychometry sitting and this one, although she herself flatly stated that the people mentioned in the two had nothing to do

Do you know if she went rather quickly?

(I don't know.)

I get a feeling she went rather quickly. I think she got a nature that was trying to keep up and like making herself feel all right when she wasn't all right.

There was something here. [The medium's hands first touched the base of her chest and then moved towards her heart.] People had thought her heart was all right, but it wasn't. There was something the matter with her heart; it wasn't really strong.

Do you know that her heart had been made weaker by another condition? It was a bit complicated, her condition. Yes, it was complicated.

But I feel she had a bodily condition inside her that weakened the heart more than she knew herself.

I think she had once lived in a rather peculiar place, in a place almost like a mountain. She had been very fond of rather a peculiar place because it was many hundreds of feet above sea level and there was roads there.

You know what he told you about his being an important man?

(Yes.)

Well, the lady was important,

with each other. See note, page 114.]

Correct.

Characteristic of E.L.T.

E.L.T. had an examination of her heart, which was pronounced organically all right, but there was a good deal of functional trouble.

There is, of course, no way of verifying this statement, but I am strongly of the opinion that such was the case.

This general description strikes me as quite apt for the Upper Peninsula of Michigan, a high rugged country. We went to live there expecting to find farms, but did not, although there were roads through the wilderness. We did considerable driving over these roads, as a matter of picnicking and other recreation. The capital "P" given farther along comes back to this point, *i.e.* "P" for "peninsula."

E.L.T. had many outside in-

she wasn't a lady who did nothing.

(Feda, there must be something that would identify these people.)

Do you know what an "E" has to do with her? He keeps seeing an "E" with her; that it is rather important to her.

(I'll inquire.)

"P." There is a "P" comes up. The "P" is not for a person's name because he doesn't mean a person's name; it is part of the name of an address. A part she was very much connected with here.

They hadn't been stuck in one place much. They did travel about.

Do you know she has been trying to communicate in America, and your gentleman says she made a contact here apart from you.

(Does she mean with you, Feda, or only in England?)

Your gentleman says she has had a link in England that is not Feda, but she has also been through Feda. Perhaps they didn't mean me to know.

(I see.)

I get a feeling through Gladys [Mrs Leonard] that I know her.

Did you know her gentleman on the earth is interested in the professor? [Dr Hyslop.] Your gentleman said it may be rather important. He has been inter-

terests several of considerable magnitude.

E.L.T.'s name was Ethel. [Feda gave it as "Effel" in Sitting 1, present. For some reason I cannot explain, except the "M" given in my sitting of 3 June (see p. 113), I thought Mrs Thomas's name was Margaret.)

As far as I remember, this "P" as "part of name of an address" applies to only one place where we have lived, namely, the Upper Peninsula of Michigan. We have often spoken of it since as "when we lived in the Upper Peninsula." We were there for the first two years of our married life.

During our married life of twenty-seven years we lived in six different towns.

Correct.

Correct. [Known to me.]

This passage puts clearly into words my past and present attitude towards Professor Hyslop and his writings. My first interest in psychic research was

ested and there has been a reason.

(What reason?)

It was a sort of scientific interest that made him think of the professor.

Do you know James in connection with them, in a personal way?

(Why, Feda?)

It is very much connected with both in a personal way—a name they were very much mixed up with, but not the professor [Dr James H. Hyslop]. A personal friend, someone they both knew very well. And then your gentleman began to laugh and said he was a person you could *not think* of comparing with the professor; you wouldn't *dream* of comparing them. I do not know whether he has been naughty. The professor might not take it as a compliment if they had. Perhaps that wasn't right. He knows your gentleman will understand it.

Will you ask the gentleman whether he remembers someone that his lady tried to help very much, who gave a good deal of trouble because of [Pantomime of putting fingers to lips, holding something and drinking]. Something to do with a—he was showing me a bottle, firewater. Like that, as if he was drinking. It is a bottle of that nasty firewater and this person they knew on the earth she was trying very much to help.

Because it wasn't an ordinary case. It was a particular case.

due to a magazine article that Professor Hyslop wrote for Harper's about 1900.

[I knew of no James.]

The James connected in a personal way was called Jim. He was one of the vendors to the school lunchrooms of which E.L.T. was supervisor. [Feda had dwelt on Jim at some length in Sitting 1, present. See "Case Studies," pages 92, 93 and 125. Jim regularly presented Mrs Thomas and the family with a jug of home-made wine on holidays. In Sitting 1, present, Feda spoke of "Jim and the D," which Mr Thomas interpreted as "Drinks." This liquor association seems to be a forerunner of the following drinks incident, in connection with the husband of another lunchroom employee.]

E.L.T. tried very much to help a situation which involved the cook in one of her school lunchrooms and her husband. He was an inveterate drinker.

It was a particular case for E.L.T. because the brutality of

But she could not help as much as she wanted to.

Do you know if she got a girl who was related to her with her on the other side ?

The young girl passed over before she did.

It is a name rather like yours. It is not Lyddie—sounds like yours, awful like yours. Lily ? Lily ? I feel that is rather important. But I do not think it is the lady's own name. It is the name of someone very close. Someone she is very fond of.

There is someone called Charles she is very fond of.

The name I gave you before is Lily.

And then she gave me Charles. And then Anna or Annie.

He is trying to write something for me to show me. That is very interesting. She finished with three special words, and the

the husband of this employee kept her away from her work at times and it was a particular case for me because the two were making payments on a house I had had built for them.

E.L.T. spent a good deal of time on this case but finally helped the woman to get a divorce.

Correct.

Correct.

[The correct name would have been Laura. In Sitting 3, absent, Fedra described "a young lady with her [E.L.T.] who passed over before she did," with additional facts which applied to a cousin of E.L.T. of whom she was very fond. No name was given at that time. This cousin's name was not Lily; it was Laura. In Sitting 3, absent, Fedra referred to the name Lily, as one Mr Thomas would remember.]

The name of my older brother deceased was Charles. While they clashed considerably, I think E.L.T. had a certain fondness for him. [In Sitting 4, absent, Fedra asked, "Who is Charlie?" and then went on to another subject.]

My mother's name was Sophronia Ann, but she was generally called Ann.

[I give the following extract from an absent sitting held for Mr Thomas with Mrs Soule on 10 September 1926, which ap-

word heart comes near the bottom, and near it a short word beginning with "O," and the word heart and a message close—near—reassuring of her. In this particular message trying to say how close she was. As if this particular message was especially reassuring about proximity. Will you ask if anything was said in this writing that said "sun," "setting sun"? I think that is right. He said some awful word—solar luminaries. These words are not there but what they mean are there. That would describe something she had written in the writing.

She sent a message because of an important date. She sent a message this way. She specially chose to send it on an important date because the man was thinking of her very, very much at that time and she chose a date to send it on that she knew was important and that would make him see that she remembered it.

This message appears to correspond with Feda's statement in the following particulars:

- (1) "She finished with three special words": "Still your own."
- (2) "The word 'heart' comes near the bottom."
- (3) "Near it a short word beginning with O"; the word "O" occurs twice in the last sentence, also the words "one" and "own."

(4) There is no reference in the message to the setting sun, but it is stated that "it was almost dark, yet quite light enough for me to see you plainly."

"Maa, Mac." I am getting a name sounds like Machree.

(Spell it, will you?)

"M-M" not "Ma," it's "Me." It's like a surname beginning with "Me." "Me." A good bit more to it; it is not a short

appears to have some points in common with Feda's description:

"I was walking with you on the shore and it was almost dark and yet was quite light enough for me to see you plainly and as you turned to look back at the water, I knew that you were thinking of the old days, . . . and then it seemed to me that you said quite plainly aloud, 'Oh, well, I know she is right here,' and it was as if you had responded to my words, for I had been saying to you the same old words we used so often in fun, 'You can't lose me' . . . Oh my, oh my dearest one, I do feel my heart overflow with love to you and I am still your own

Ethel."]

["Me" followed by six or seven letters suggests McDougall, who was "important to these people." References to

name. Feels like six or seven more letters after the "Mc." As if it is rather important to these people.

Professor McDougall had been appropriately made in Mr Thomas's Leonard sittings. Also in a passage omitted from Sitting 1 (above) a name beginning with Mac was mentioned.]

[Passages are omitted here and earlier in the sitting relating to attempts to communicate through the well-known medium, Mrs Soule.]

This lady has been there quite lately.

(Could he tell me anything she said there?)

She had sent the gentleman rather a special message, with regard to flowers. She mentions flowers. Not flowers she knew when she was here, but something had been done recently near the time of sending the flowers. Something he had done at that time. He had wondered whether she had known about them, so she wrote that thing through that medium.

[Mr Thomas had two absent sittings with Mrs Soule on 27 May and on 10 June, the date of this sitting, five hours later than mine, allowing for the difference in time between London and Boston. During the course of the latter sitting this message was given: "Now I want to write something about water lilies, too, for they were always a very lovely flower to me. I recall some that we always called pond lilies and I think there was one place where we were never able to get them, but occasionally we did." "I am thinking of some little flowers that came up in the spring and grew in the woods. This is a very old memory but it is connected with a happy experience which I am sure you will recall, for as violets are now intimately connected with my contacts with you, it is but a continuation of those early flowers that we loved." About a week before this sitting Mr Thomas had placed some violets before his wife's portrait. Feda's words seem to combine an allusion to his action with an allusion to the message that was to be given at the Soule sitting about five hours later.]

CHART

New, Correct Material	Repetitions	New Associations	Wrong Material	Contemporary Facts
<p>E.L.T. never in Boston</p> <p>Mr Thomas "cut off" in Detroit</p> <p>Mr Thomas prominent in semi - public affairs</p> <p>Something published</p> <p>E.L.T., weak heart</p> <p>E.L.T. important</p> <p>"Not stuck in one place"</p> <p>Mr Thomas's special interest in Dr Hyslop in the past</p> <p>By proxy through "M"</p> <p>Mother's name Ann</p> <p>Boston nearer L.W.A. than Mr Thomas</p> <p>Three mediums</p> <p>Likes "S" best</p> <p>Communicated in flat</p> <p>"S"—four weeks ago</p> <p>Communication through younger woman</p> <p>Two ladies sitting</p> <p>Message late at night</p> <p>Journey with psychic conditions</p>	<p>"E"</p> <p>James Young girl with E.L.T.</p> <p>Charlie "Mc"</p>	<p>E.L.T. went quickly</p> <p>E.L.T. "not mixed up in sittings"</p> <p>Helping someone—a particular case</p>	<p>Lady in first sitting (?)</p> <p>"B" "R"</p> <p>Been to George</p> <p>Old black woman</p>	

Sitting with Mrs Osborne Leonard, Thursday 13 June 1929.

Sitter : L.W.A.

On 12 June, the day before this sitting, I had an anonymous sitting at the British College of Psychic Science for Mr Thomas with a medium named Miss Thomas, recommended to me by Mrs McKenzie, the Honorary Secretary of the College. The appointment was made over the telephone. My only stipulation was that I desired a sitting with any medium good at psychometry. The results were negative. I had presented the same articles as at sitting 2 above. After the sitting I saw Mrs McKenzie and we chatted about various subjects connected with psychical research. Mrs McKenzie, aware of my interest and connection with the Boston Society, told me that Mr Thomas's "Case Studies" had arrived a few days ago. She very kindly offered to lend me the volume. I refused, for obvious reasons, but I deliberately asked one question, "Do you know what Mr Thomas's wife's name was?" Mrs McKenzie said she thought it must be Ethel, although she did not think the complete name had been given in the record, only attempts to get it. My reason for that question was to see if Feda would give me the correct name *after* I knew it. I was attempting to get the name telepathically. It never occurred to me until much later that if Feda gave me the name under those circumstances she might only be repeating something she had already given in an earlier Thomas sitting.

Record of Sitting.

Annotations.

[About twelve lines of chatter concerning only myself are here omitted, which I interrupted as follows :]

(Feda, my gentleman promised he would try to find out more about the lady who has been coming to the gentleman. He wanted me to remind him of it.)

They have got her again. They wanted to give something about her. What they get about people you do *not* know is much more important than what you *do* know.

(Well, what *do* they know?)

Wait a minute. Who is Johnny? Johnny? Johnny?

[Of course I knew Mr Thomas's name is John. In Sitting 2,

"Helping Johnny." He suddenly said that.

(Well, stick to Johnny. What about him?)

Were you thinking about this yesterday? Because they were thinking of this yesterday, as if you were doing something, too, that should have reminded you of Johnny.

(What else?)

Did you do something? Two things could have reminded you of Johnny. One, I think, would be more what you call pronounced. But I feel it could be made in two things more.

(I think I understand that, Feda.)

Is there a letter, an initial "C" connected with Johnny more? Not you, but someone connected with Johnny has a name beginning with "C" very much in mind.

(Someone on the earth?)

Yes, on the earth would be thinking of Johnny and would be thinking of the "C" as well.

Messenger? Messenger? Something that has to do with messenger more. And that comes

present, Feda said, "I get the name John on the earth and then I get a feeling of getting through John, getting through to John." The name "Boston" followed in the next sentence, and Mr Thomas responded with, "Yes, that's fine." Feda then got "C—an initial C," to which Mr Thomas replied, "That's right." A little later Feda gave the name "Charlie." In my record, as will be noted a few paragraphs farther on, the "C" follows the "John" very shortly.]

[One thing that would have reminded me of "Johnny" was, of course, my sitting held for him. The other might have been my conversation with Mrs McKenzie concerning the arrival in England of Mr Thomas's "Case Studies."]

[Mr Thomas was in close touch with the widow of his deceased elder brother Charles. She naturally would have the name beginning with "C" very much in mind. As noted above, Feda had previously given the "C" connected with "John" in Mr Thomas's Leonard sittings.]

[“Messenger” has no significance here, but is repeated considerably farther along in this

more into yesterday. It all comes up with something you had to do yesterday. Johnny—"C"—and the word messenger.

Is there someone here in England that would be connected with Johnny? Because I get a feeling someone here or coming soon or very near, where that is linked up with Johnny more. Coming here, I mean London.

I get someone who passed over a few years ago who links up with this. Before the lady passed over, before the lady, not *just* before, like a week or two, but perhaps two or three years before the lady did. "C." "C." Why do I get the "C"? The "C" is someone on the other side. I am sure that is important.

Do you know the name Emma?

(What about that?)

Connected with them because there is someone the lady has met called Emma; that is important to her.

Do you know, was that lady very fond of music?

(Why?)

I get a feeling of music with her. I get a feeling she could do something in a sort of musical way herself, but I get strongly she appreciated music and would always pay attention.

sitting with details which identify it in a striking manner with the lives of Mr and Mrs Thomas. See page 141.]

[This passage may refer to an American lady connected with Mr Thomas in a professional way, who arrived in London on 11 July and held an anonymous sitting with a London medium arranged by Mr Thomas through Mrs Hankey in an interchange of letters that began late in May or early in June.]

My brother Charles died in 1917, nine years before E.L.T.

One of E.L.T.'s school friends in her youth who is now deceased was named Emma.

E.L.T. was fond of music and had been a church organist. She also sang. During the last fifteen years of her life E.L.T. did not participate herself, but did not lose her interest in music. [In *Sitting I*, present, Feda inquired, "Was she fond of music when she was here, because I get a feeling of music around her. . . . She likes music. I don't think she is always playing herself, but I feel she likes it." At this point

She had lived one time in a condition where she heard a good deal of music. I get a close relation of someone awful clever with music. Not one who is still on the earth; she's speaking now of one who has passed over.

(Feda, I want you to place these two people more closely in America. So that no one will be able to say, "How do you *know* these two people were meant?")

Mrs Lyddie, didn't they belong to a group, a little scientific group of people? I do not mean scientific like Sir Oliver, but people who investigated, but in quite a right scientific way.

(Where did they live?)

In a place not where you live. Isn't a letter "O" near it?

(I'm not sure about that.)

Can you look at a map to-day? Because I get a letter "O." I do not often get an "O."

It's a civilised place, not plains. I feel a town.

Isn't a good deal built of stone? I see lots of light stone. They have just added two important public buildings.

That place is well known.

It is a busy place. [To communicator.] What are they most busy about? You couldn't get that, professor? [Pause.] [To sitter.] Does they send things away from that place? Putting things on ships?

Feda changed the subject and Mr Thomas had not replied.]

Our next-door neighbour for a number of years was a professional musician who certainly did classify under "someone awful clever with music." He is deceased.

[Mr Thomas has been for many years a member of the A.S.P.R. and while E.L.T. had no special interest in psychic research, she was not antagonistic. Neither of them, however, belonged to what might be termed a "scientific group."]

[The state of Ohio is not far from Detroit.]

Two public buildings in Detroit, both comparatively new, are the Public Library and the Art Museum. They are both of marble.

[Detroit is one of the leading manufacturing cities in the United States.]

(What sort of things ?)

What do they pack in cases ?
They put something in cases.
And you can eat something that
comes from there ; that you can
eat.

(I don't know about that.)

When you think of it, it is the
hardest thing to get. A " B "
got something to do with making
something there. A " B."

(Feda, where did they live ?)

[Feda later modified this refer-
ence.]

[Not verified.]

[Feda's reply and her vivid de-
scription are one of the most
dramatic examples of medium-
ship in my experience. It would
have seemed so much easier for
Feda to say simply " Detroit "
in answer to my question,
" Where did they live ? " But
the giving of a name or a specific
word for which there is no alter-
native appears to create one of
the greatest difficulties in med-
iumship. It seems as if the
communicators convey their
meaning to Feda in the form of
thought which emerges in her
consciousness as hearing, sight or
impression and which accounts
for the roundabout descriptions.]

What do they cut ? Cutting
something. Chopping and cut-
ting. This isn't to eat at all.
Because I am getting the feeling
of steel and metal, then sharp-
ness—cutting. I do get a feeling
of metal. I think that must have
a good deal to do with metal. Is
there some factories there ? Be-
cause I feel noises. Because I—
clank, clank, clank—and fitting.
They fits things together, stamp-
ing and cutting out. More of
fitting. Some isn't fitted like
parts of them is made. I feel

some rather big pieees and what's the eireles, wheels, that I see? Because I see wheels and circles and all sorts of round and square things and hundreds and hundreds of men working. *No*, the professor says thousands and thousands; and the whole place is like a beehive of humanity working in these huge plaees. Do not forget this, they have added to, that is what I meant by the two plaees they have added and enlarged. It is noisy—it's hammering on metal—ringing noises. They runs along, they runs along. Like this. [Imitating whirring sounds.] The professor says, "We *hope* they do," but he is laughing. He *knows* they do. Some are sent out incomplete and some complete. Becaause some are assembled in other plaees. Wait a minute. [Sotto voce.] What did you say about "D"? [Aloud.] Detra—Detra. Something beginning with "D." Detra-Detro. He tried to say it. Enormous eases. But I said something a moment ago about an eatable. They want you to forget it. You wouldn't be thinking of it at all. Detra—he's trying to say the "D" word again. Detra—Detro—it is important but I can't get it.

[Feda's description of Detroit in answer to my question, "Where did they live," was impressive in the extreme and again leaves me in doubt as to the advisability of asking direct questions. On 1 August I wrote Mrs Leonard from Paris enclosing the following questions on a sheet of notepaper, leaving space for her answers, which she promptly returned.

"Have you heard of Detroit in Ameriea?' Yes.

'If so, do you know anything about Detroit?' No—not even in what part of Ameriea it is.

'Do you know if you have ever had a sitter from Detroit?' Not so far as I know. Among

so many anonymous sitters there may have been one from Detroit—but I do not know of it.”

(Signed) Gladys Leonard.

To an American it seems almost incredible that Detroit and the automobile industry should not be immediately associated. A dozen well-educated English people whom I questioned on this point had all heard of Detroit but not one of them knew it was the automobile manufacturing centre of America. However improbable this may seem, I should like to suggest trying a similar experiment. How many Americans know where the most popular English and French cars are manufactured in their respective countries? For instance, where is the famous Citroen made?

Mrs Leonard's mention of "Detroi" strengthens considerably the surmise that her allusions are meant to apply to Mr Thomas and E.L.T., making a definite and tangible link with them. No reference to Detroit by name or description occurs in Mr Thomas's Leonard sittings.]

He says I haven't quite explained something in connection with this, like what you call industry. There has been some extension lately and that is important, very important, but what *he* knows and what other people *don't* know he did, the people who have made the recent extension is contemplating another. Which you are bound to hear about in the course of a few months, even if you do not ask.

[The recent "extension" associated with the automobile industry Feda had just described might indicate the complete reorganisation of the Ford plant about two years previous to this sitting for making an entirely new type of car. As to the second "extension" which Feda said I was "bound to hear about in the course of a few months," whether I inquired or not, *The New York Times* for Monday,

Do you remember—[Sotto voce.] No, she doesn't know. [Aloud.] Well, that is what he says, a few years ago, perhaps ten to fifteen, there was something these people were very misjudged over. They are people who would never do what they knew to be wrong; they neither of them would.

I think the very thing that they were doing that people should have appreciated them for—it was the man specially—they were misjudged on for the time being.

Only for the time being. But it was rather bitter, hurtful at the time.

Wait a minute. It was a policy they were upholding, a principle which they were misjudged on. But they were right and they stuck to it.

Perhaps, Mrs Lyddie, it isn't right to say that.

(Oh, yes, it's all right. Do go on.)

Now, these people seem to Feda as if they were rather well off.

I get feeling of comfort and money, but they were connected with people who weren't well off; the man was and more than the lady. I don't know if it was polite to say so, but the man had

30 Dec. 1929, a little over five months after this sitting, carried a top centre, front page, double column article, announcing the Ford Company's proposed expenditure during the next year of more than \$30,000,000 for plant expansion.

This brought at once to my mind a quite serious difficulty that I had at one time with one of the then members of the Board of Education. I looked it up in the Proceedings of the Board and find that it was almost exactly fifteen years ago.

Correct, and from E.L.T.'s point of view.

Emphatically so.

Correct.

Correct. [I should have assumed so.]

Correct. This is very striking, particularly so because it selects my side of the family rather than that of E.L.T.

connections in his family that was not at all well off. I get a feeling of hard times. I'm sure he came of a family that one side would be rather poor.

Has he got any connection at all with Irish people?

(I don't know, Fedá.)

Because I get a connection; I am going back now to Ireland. A funny link. You do not often get mixed like a connection with Ireland and with Scotland, too. I think both that is in his family.

Does he cough? Has he got catarrh? (Why?)

While they were talking he gave me a feeling like an ordinary chest condition; it's not lungs, it's not throat, back of nose catarrh, a little to one ear. He has to be a bit careful, there—there. [Passing fingers over sinus along nose.] It is, but I wish he'd be careful near ear.

He feels like an energetic man; like a man with some goes in him.

I think a man that would wear himself out a bit; the lady used to say so.

She used to be far more afraid that something would happen to him than to her. She used to think it.

The professor say we touched on that in a previous sitting; that at one time she had been afraid when he had been ill and that he might pass over.

She little thought she would pass over; she didn't think she would. She got a great hope in her; she was a hopeful person.

I do not know whether you

[Not satisfactorily verified.]

I have a tendency in this direction but it seems to me this is over-emphasised.

[Mr Thomas is an energetic man.]

This is correct from E.L.T.'s point of view.

Correct.

See note on page 120.

While this is true in general I think E.L.T. had a premonition of death.

See note on page 120.

knew this, I think I told you the other day he had an illness that made her very anxious. He got over that and then he wasn't very well again and she had been a bit anxious about him a second time. And then she herself passed over, not long after.

Though they were well off and they lived in a nice place, they lived like a very, very simple life. They wasn't people that liked a terrible lot of luxuries, and could do without it.

Like doing something for themselves instead of asking a slave to do it.

(Pause.) Can't get that! Effie—Effie. Ettie. Do sound like Ettie. Haven't got it quite right. I am not sure I got it quite right. It is rather important, a name rather near to them. A little name very much like that. Ethi—Ethi—Ethi—Ettie—Ettie—sounds like Ettie. Awful much like that.

Ruben. Wait a minute. Do you know a name sound like Ruben connected with them? That is what it sounds like. That seems to be right because the 'fessor nodded his head.

Keep giving a funny name sounded like Hearst. Hirsch—say it again, 'fessor.

(Ask him to spell it, Feda.)

I asked him to spell it but he doesn't. Sounds like Hirsch—

Emphatically so.

[Mr Thomas says that E.L.T. had a very independent nature.]

[As already stated, Mrs McKenzic had told me she thought E.L.T.'s name was Ethel. See Introduction to this sitting, page 128. In Mr Thomas's Sitting 1, present, Feda, said, "Do you know a little name beginning with 'E,' too, that she is interested in, because I see 'E.' Maybe Eff—Effel," to which Mr Thomas replied, "Yes." In my third sitting Feda asked what an "E" had to do with the lady, to which I answered, "I'll inquire." See page 122.]

Ruben is a very suggestive name from E.L.T. Marks Ruben was a clothing merchant very well known to us in our earlier life. He died within the last year or so.

Ruben, a Jewish name, may have suggested the second Jewish name, Hersch. [Hersch is the name of a young Jewish man with whom Mr Thomas and Dr Rhine had come into contact at

Hirsch—Hirsch. A funny name, but that's what it sounds like. Someone they would both know. Not one of them, but both of them.

Who's Sadie? Someone the lady knew called Sadie. Someone she spent a good deal of time with.

Cornell, Cornell, Cornell. I do not know, I think he said Cornell or Connell.

Have they lived close to a railroad? Because I get a strong feeling being very near to a railway.

Not just beside it. I do not

Duke University. They had discussed this Mr Hersch a good deal. It is not true, however, that E.L.T. had known him. I am therefore charting the name as "Wrong" with a question mark.]

Sadie, also Jewish, was an office associate of E.L.T.

I know of no Cornell. [Mrs Cornell Woolley, who has been a regular Leonard sitter for many years and whose principal communicator is her deceased husband, Cornell, had had a sitting with Mrs Leonard two days previously. The giving of this name might be an association. I recorded a Leonard sitting for Mrs Woolley several years ago.]

Up to the time she was seventeen or eighteen E.L.T. lived alongside a railroad with her family. My home was three blocks down the street from E.L.T.'s. [This is, of course, charted "Wrong" because Feda inquired, "Have *they* lived close to a railroad?" It seems that some allowance should be made for Feda's confusing the pronouns. Suppose she was shown a house near a railroad, or received that impression telepathically. It would be quite understandable for her to conclude that it was the home of the two people concerned. Evidence has frequently been weakened by Feda using the wrong pronoun when the facts otherwise were correctly stated.]

E.L.T. did live just beside it.

get where you could walk in ten or fifteen minutes.

(Do you mean their home, or the town, is near a railroad?)

No, it's not like the town. As if this railroad is being thought a lot of.

Was there like two railroads, one big railroad, an ordinary railroad that everybody used, and then I get the feeling of a funny *little* railroad and I feel the people in this place would be very interested in this little railroad.

The professor said you might call it a *little* railroad but it was *remarkably efficient* in its own way.

The lady and gentleman were in some way interested in this little railroad. They were very interested, it interested them rather in some personal way. Not as if you were saying, "Oh, it is a little railroad," but as if there was some interest that they took in it.

In that place they were telling me about, this place that was the busy place, what he calls the beehive that is in America, did they do something in connection with England? Because I feel

This railroad has been all our lives very much in the thoughts of all of us. E.L.T.'s family has been connected in various capacities with this particular railroad ever since it was first built.

This is a striking statement because there was in our experience a "funny little railroad" about which we have laughed a great many times and in which we were very much interested. It is about one hundred miles from Detroit, but Jerome and I had been on an auto trip the Saturday and Sunday preceding this reference during which we were near this railroad and at which time we talked and laughed about it a good deal.

One point that always struck us was that, while it was a sort of a toy railroad, it did get its two trains along every day and quite noticeably on time and managed to make an annual profit.

The personal way in which we were interested was that before we had a car we used this railroad to get from Detroit to a cottage that we owned in southern Michigan and the family used to make two migrations on it annually for several years.

[The Ford Automobile Company of Detroit has its English branch at Manchester, in the north of England. I only learned this on subsequent inquiry.]

up and down in England and they got some important links with the North of England, not only in London where anybody might have them, but I'm going northward.

What did she say about his coats? The lady used to tease him about the shape and cut of his coats. Before she passed over he had a coat she didn't quite like, and she talked about this coat quite a lot.

The reason he is giving this to-day was because she was with him only a little while ago and he was thinking of that very coat that she made discouraging remarks—not, it isn't discouraging, it is disparaging—remarks.

Has he been, I think in the last two or three months, doing something in her name? It wasn't a gravestone, but it is something she is very pleased about which he has done during the last two or three months. And though it isn't a gravestone, like an important thing to her memory. That is right. It is something she is pleased about. And it is something that will last, that will live in people's memory. That is correct.

The professor says you might easily describe it as a memorial but it is not a memorial in a conventional sense, it's bequeath, that word fits. Building and bequeathing.

Correct and especially about a particular coat. This coat was one that was made by a Jewish tailor years ago when I was principal of a Jewish school. This tailor made the coat very long and of rather grotesque cut and when I protested said that it was a "coat good enough for a rabbi," or some similar remark. This "rabbi coat" became a stock joke in the family.

Jerome and I had been talking and laughing about this very coat not long before this reference was received.

During the last two or three months previous to this sitting I was having a baptismal font built as a memorial to E.L.T. to be placed in the church which had replaced the one where she had been organist.

It is not a bequest from E.L.T., but a project initiated and carried out by me in the spring of 1929, and dedicated on 9 June 1929. Jerome and I had made the trip, passing near the little

Margaret — Margaret, she's with Margaret. Then he suddenly said, "She is with Margaret."

Messenger—can't get this messenger. No, it isn't the name Messenger he's trying for but sounded like it. A Christian name, a surname. The professor said if you said the name Messenger indistinctly you would almost mistake it. It is awful much like Messenger. Every time he tries for it, it goes into Messenger. The professor said that name is important because it is someone they were both very interested in.

Did they go under a big archway where they lived? They used to go backward and forward under a big archway.

(You mean the lady and gentleman?)

Yes, somewhere they often went under a big archway.

Were they going to take or fetch money under that archway? This may be important, the professor says. They would naturally be thinking of money because of somewhere they were

railroad of which I spoke above, in order to be present at a dedicatory service for this baptismal font on 9 June, the Sunday previous to this reference.

The name Margaret is connected with one of the towns in which we lived, Hastings, Michigan. It is the most important town along the line of the little railroad discussed above. Margaret was a high school pupil when I was connected with the school there, who died later under rather distressing circumstances.

The name of two elderly gentlemen, who were the leading citizens in this same town that was on the little railroad and with which Margaret was connected, was "Messer." The Messers, Margaret and we lived on the same street.

We did go backward and forward under a big archway on an auto ride that we took very frequently, and it was the most striking feature of this ride.

For several years we took this ride practically every Monday night for the purpose of going to a branch bank where I had a number of monthly collections made for me on land contracts.

going to when they passed under it.

Close to archway is something making a trickling noise. Because I get the feeling of running water that isn't quite still but making a noise.

Do you know, have they ever been in an accident together ?

(I don't know.)

Not just before she passed over ; it would be several years ago. Yes, it is going back a good while. It had nothing to do with her passing. It was something they got out of alive, but I get a feeling it might have been a collapse. [Feda described this supposed accident in some detail.]

Do you know if for a time they had lived in a place, such a funny place, looks not like brick house. Looks like a wooden house.

I cannot place any running water by this archway. Every time it rained, however, the surface water ran down so that it was a couple of feet [inches] deep on the paving.

[Mr Thomas recalls a personal accident but none in which he and E.L.T. figures. Might not this description typical of any serious railroad accident be a logical sequence of association by Feda with her correct references to railroads a few minutes earlier in the sitting ?

Feda often spoils good evidence by her enlargement and interpretation of originally correct statements. One thing suggests another until she has built up a picture. Perhaps she is only keeping the pot boiling during the intervals between the efforts of the communicator.]

This applies to the house where E.L.T. lived as a child and young woman. It was a frame house and located on the railroad right-of-way, which might impress Feda as a "funny place."

[Again there is a confusion of pronouns. Feda says "they" and then continues to describe a very unusual house in which E.L.T. only had lived. The description is so appropriate as a memory of E.L.T.'s that I am discounting the wrong pronoun and charting the passage as "Correct," although I realise my decision is open to question.]

Such a dark brown, very near black.

The 'fessor says I am right in thinking it wood ; only he calls it timber.

I'm seeing side of it ; it's built like big pieces, one on top of other.

Poles—like poles, almost as if they were propping it up. Now, this building had the house of wood, the walls of wood, but instead of part of it having ordinary foundation it had like piles holding it up. The 'fessor says they call them piles, like heavy poles holding up the floors. Certainly one part of the house seemed held up between the floor and the ground underneath it.

The lady has some special reason for mentioning this place, I am sure.

The reason she mentions this place is because it happened to be in an important part of their lives. I get such a lot of plans being made just then. That is rather important. It wasn't that the building itself was important. It was that the time of their lives, the period, was important.

The 'fessor says this lady is wanting the gentleman on the

The colour was brown, blackened by the smoke from the trains.

Timber might apply more than wood, although in this section it would be described as a frame house.

It was a elapboard house. [A thin board used in covering wooden houses.]

Part of this house was on piles or posts instead of the ordinary foundation. I remember this distinctly and so does E.L.T.'s sister, because we have heard her tell of having to erawl under the house to bring out the next younger brother when he was a baby and had hidden himself there. The piles or posts were used instead of foundations, probably because it was felt that the location of the house on the right-of-way might be only temporary. Foundations and not posts or piles were the usual underpinning for houses all through that section.

The special reason E.L.T. may have had for mentioning the house was that it was located beside the railway she previously described.

This is an additional reason for mentioning the house. It was an important period in our lives, because E.L.T. lived there when we first met.

earth to know that she is happy. She wants him to know that, because he wonders sometimes is she happy, not being with him ; and they want you to say that she is happy because she *is* with him.

Oh, has he arranged three portraits close together lately ? I think I get this right. He has arranged three portraits together lately. I think they are portraits of her.

Mrs Lyddie, I don't know if you know this. It doesn't look like a hat, but something draped around her head. I think draped is wrong. It is almost as if something were fastened around her head.

He wishes to say this specially. Ask him whether he thinks in one portrait she has a slight look of the Madonna. He has thought so and she knew he had.

The 'fessor says her face is not of the typical Madonna shape but in one portrait she seems to have been caught in a way that gave her the expression that helps the idea. He says it has caught it in an extraordinary way more than any other. She had no other that caught it that way.

But he seems to be doing something lately with three portraits. I get the feeling of putting them close together like grouping them together.

There is one portrait very good but it has got a funny dress.

[Description followed, and the sitting came to an end.]

Shortly before this sitting I had arranged three photographs of E.L.T. along the bottom of the mirror on my dresser. There were originally five.

In one of these pictures E.L.T. wears a hat around which is draped what looks like a veil.

In this same picture E.L.T. has a motherly expression which her sister and I had spoken of as being most characteristic.

This is strikingly true of this picture as compared with any others.

See note above.

I know of no photographs of E.L.T. to which these points would apply. A portrait of her daughter-in-law more nearly answers the description.

(Perhaps you'd better leave it,
Feda.)

[Feda's fear that she was getting things wrong as the sitting petered out seemed quite justified. About two minutes later, after a few personal remarks to me, the sitting ended.]

CHART

New, Correct Material	Repetitions	New Associations	Wrong Material	Contemporary Facts
Emma "O" Well known, busy place Description of Detroit and attempt at name Extensions Misjudged Well off Hard times Ruben Sadie Railroad thought of Interested in little railroad Link in northern England Teased about coats Memorial to E.L.T. Margaret Messenger Archway connected with money Timber house on poles Three portraits	Johnny "C" Fond of music Ethie—Ettie	E.L.T. could do something musically Close relations with deceased musician	Hirsch (?) Near railroad (?) Trickling noise (?) Accident Mixed description of photographs	Two things connected with "Johnny" Disparaging remarks about coat

PROCEEDINGS

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PART 135

WHY I DO NOT BELIEVE IN SURVIVAL

BY PROFESSOR E. R. DODDS

THE composition of this paper was suggested to me by the following passage in the address delivered to the Society in 1932 by Dr Brown :

“ It seems to me that at the present day the evidence for survival which must have most weight with us is the evidence from philosophical and religious considerations, from the nature of the human mind, the nature of human values, and the extent to which survival itself would make life and the whole world process more rational. I do think that considerations such as these are sufficient to produce in our minds at any rate an active pragmatic conviction of survival.”

A little further on he adds :

“ It seems to me that the scientific researchers are concerned rather with getting sufficient evidence to *supplement* evidence that they all accept as really the fundamental groundwork in their religious and philosophical beliefs.”¹

On reading these remarks of Dr Brown's, the following reflections occurred to me. (1) If it is true, as Dr Brown says it is, that *all* the scientific researchers who are working on the interpretation of the supernormal evidence are from the start imbued with an antecedent conviction that survival is a fact, then it would be not only interesting but actually prudent to see how this evidence appears to less biassed observers or, failing that, to observers whose bias is in the opposite direction. (2) There should be no difficulty in obtaining such observers : for, so far as I can judge, the view of the antecedent probability of survival which Dr Brown attributes to “ all the scientific researchers ” is not that which would be taken by the majority of working scientists outside this Society ; it is also in strong conflict with the views of distinguished philosophers

¹ *Proc.*, xli. 86.

of various schools like Dr Broad, Earl Russell, and the late F. H. Bradley; and it conflicts no less with the impression formed by a large number of educated people who are neither scientists nor professional philosophers. Taking myself as an average representative of this last class, it occurred to me further that it would be useful to me, and might possibly be of interest to others, if I attempted first to present a brief reasoned statement on the probability of survival as it appears to me, and then to formulate in the light of this statement an attitude towards the supernormal evidence with the problems of interpretation which it presents. This seemed to me preferable to the usual practice of either considering the supernormal evidence in isolation from the antecedent evidence, or taking for granted a particular view of the latter without attempting a reasoned defence of it.

I was, however, well aware that the view which any man takes of the antecedent probability, or otherwise, of human survival must itself depend to a great extent upon his general philosophical and religious outlook, and that in turn upon his individual temperament and history. Hence any attempt at discussing the antecedent probability of survival runs the risk of raising issues so vast, so vague and so insoluble as to swallow up the entire discussion in a futile exchange of unproved generalisations. If this were to be avoided, it seemed necessary to confine myself to a statement of the case as it might appear to a person sharing the same general presuppositions as myself, and to indicate explicitly what these presuppositions are. The argument claims validity only in so far as this framework is accepted. That is why in the title of my paper I have violated the esteemed precept, beloved of girls' schools, which forbids to the writer on general topics the introduction of the pronoun "I". This paper does not pretend to be a complete statement of the case against survival: it is merely a statement of my own reasons, not for regarding survival as impossible, but for thinking the hypothesis unproved (and in some at least of its forms definitely improbable), and in that sense disbelieving it.

The presuppositions I have referred to are two in number. The first is that our sole means of reaching a conclusion on this question consist of observation and experiment together with the exercise of reason. This excludes the view that we have divine authority for the belief in survival. A complete statement of the case against survival would have to take account of this view, and investigate its claim to historical truth. I do not share the curious opinion that such investigation is bad form, nor the alternative opinion that it has been rendered unnecessary by the conclusion of a treaty of peace between science in the person of Professor Jeans and

religion in the person of Bishop Barnes. But it is evident that to attempt in the present paper any statement of my reasons for rejecting this view would carry the discussion too far from the immediate question at issue.

My second presupposition is that the validity of our apparent experience of mind and matter as two distinct modes of reality is accepted as a working hypothesis on the empirical level, without excluding the possibility that either may prove in the last analysis to be a mere appearance of the other. Both idealism (or "mentalism") and materialism seem to me to be *possible* ways of interpreting our experience; neither seems to be *required* by the facts, and a survey of their history suggests that the influence of each has depended less on its inherent convincingness than on its political or religious utility, on the variations of social demand, and on the dialectical dexterity of its exponents. This being so, an argument which is valid only on the idealist or only on the materialist hypothesis carries, for me, less weight than one which is valid without this limitation.

Within this very wide framework, then, before approaching the evidence from psychical research I ask myself what are the antecedent probabilities for or against survival. In its favour I find two distinct types of argument advanced. The first, which Dr Broad calls metaphysical, and which Dr Brown intends (I think) when he speaks of "the evidence from the nature of the human mind", asserts under varying forms that our mind has a quality in common with the ultimate stuff of Reality, or with the creator of Reality, and that the possession of this quality assures its continuance: in theological language, it asserts man's immortality as a consequence from his divinity. On this argument, two reflections occur to me. In the first place, it rests on a proposition about Reality (or about its creator) which may be true, but which I do not certainly know to be true. I do not know of what ultimate stuff the universe is constituted, nor how it came into being; and certainly I find nothing in my experience which assures me that my continued existence is indispensable to it. But secondly, if this type of argument proves anything at all, it appears to me to prove too much. As the late Dr McTaggart put it,

"I do not see how existence in future time could be shown to be necessary in the case of any being whose existence in past time is admitted not to be necessary. If the universe got on without me a hundred years ago, what reason could be given for denying that it might get on without me a hundred years hence? Or if it is consistent with my eternal nature that its temporal manifestation should begin at some point in time, could we find any reason for supposing that the cessation of that manifestation at some point

in time would be inconsistent with that nature? I do not see of what kind such a reason could be, nor do I know of any attempt that has been made to establish one.”¹

In other words, if we accept the metaphysical argument as a valid proof of survival, we must accept it also as a valid proof of pre-existence. This is, for me, unfortunate. For the doctrine of pre-existence is open to several objections² which I do not know how to meet. Until these have been resolved for me, I feel constrained to reject the theory of pre-existence, and with it the metaphysical argument for survival.

I think, however, that even if I could accept the metaphysical argument, with all its consequences, this would have but little logical bearing on the interpretation of mediumistic utterances. For the entity whose immortality and pre-existence the metaphysical argument has been held to certify is not, so far as we can judge, at all like the entities which converse with us at séances. It is a timeless, noumenal self, exempt from passion and withdrawn from change, and held to be imperishable either because of that very detachment or because of its capacity for pure impersonal thought. Now, whatever the “controls” and “communicators” of Mrs Piper or Mrs Leonard may be, it seems certain that they are not noumenal selves (if they were, they would give small satisfaction to the average sitter). Far from being exempt from passion, they are frequently emotional to the point of sentimentality; and pure impersonal thought is precisely what their communications conspicuously lack. Hence I do not think that the metaphysical argument, even if it were valid, could help us, at any rate directly, in the interpretation of these phenomena.

I turn to a brief consideration of the ethical arguments for survival (or, more exactly, for immortality), described by Dr Brown as “the evidence from the nature of human values, and the extent to which survival would make life and the whole world-process more rational.” It is argued (i) that an apprehension of timeless values in some way

¹ *Some Dogmas of Religion*, chap. iv.

² I have been asked what these are. In answer, I should mention the following difficulties among others. (a) If the doctrine means anything, it means that in addition to the recognised factors of heredity and environment a third, presumably far stronger factor, is operative in the formation of human character: is it not strange that no certain trace of the activity of this factor has been detected by psychologists? (b) It means, again, that the new-born infant possesses, or rather is, a mature and experienced mind: is it not strange that all the manifestations of that mind are precisely what we should expect them to be if it were neither mature nor experienced, but were in fact, what it appears to be, an infant mind in an infant body? (c) The doctrine involves the assumption of an act of incarnation whose mechanism is exceedingly difficult to picture and which lacks even the remotest analogue in our biological knowledge.

constitutes a claim to a permanent existence in time ; (ii) that the non-survivalist view is in some way incompatible with the good life for man, and should therefore be rejected ; (iii) that the non-survivalist view is incompatible with the goodness and rationality of the universe, and should therefore be rejected. I confess that these arguments appear to me completely nugatory. The first, so far as it is not a restatement of the old metaphysical argument, seems to rest on a mere confusion. If values are really "timeless", their timeless nature may be as fully actualised in an experience lasting thirty seconds as in one which lasts thirty thousand years. And observation does not suggest that there is in nature any correlation whatever between valuableness and temporal permanenee. As to the second argument, I discover little either in my personal experience or in my reading of history which tends to show that a belief in survival is morally necessary or even, for the average person, morally useful ; but were the reverse the case, I should still find it impossible to leap from an admission of the moral utility of the belief to Dr Brown's "active pragmatic conviction" of its objective truth. Finally, to consider the third argument, I find death no more irrational than copulation, birth, and a thousand other natural events ; while to the suggestion that our extinction amounts to a charge of immorality or insanity against the universe, I can only reply that the universe, so far as my limited knowledge of it extends, is like other fathers of families "capable de tout". Those who desire a more critical examination of this group of arguments may be referred to the eleventh chapter of *The Mind and its Place in Nature*, where Dr Broad breaks these butterflies upon the wheel of a remorseless and elegant logic. With his conclusion, that no valid philosophical grounds whatever have been produced for believing in human immortality (or in human survival either), I find myself regretfully obliged to agree.

My next question is, Are there any positive antecedent grounds for *rejecting* the belief in survival ? If this question means, "Can survival be shown on antecedent grounds to be impossible ?", the answer is, I think, that it cannot, save on the materialist pre-suppositions which, as I said at the outset, I do not feel constrained to accept although they may be correct. But secondly, Are there any valid grounds for considering survival *improbable* ? There are two considerations, each of which seems to me to raise a presumption, fairly strong though falling short of proof, not indeed against all forms of the survivalist hypothesis, but against the particular form of it which is required to account for the supernormal phenomena.

The first of these considerations is historical, and is in part an

argument from silence. Now arguments from silence are notoriously dangerous; and in this case Dr Broad holds that the absence of evidence for survival does not constitute evidence *against* it, since for all we know the conditions of existence after death may be such as to exclude the possibility of affirmative evidence being obtained.¹ This is a perfectly satisfactory answer from the point of view of the orthodox Christian, who believes the dead to be segregated in Heaven, Hell or Purgatory; it is also satisfactory from the point of view of the man who says "I believe that the dead survive, but I know nothing of their powers or their mode of existence or their location in space." It is less satisfactory from the point of view of the spiritualist. For if the spiritualist interpretation of the supernormal phenomena is correct, we know a good deal about the dead, and what we know is inconsistent with Dr Broad's defence. All spiritualists believe that the dead have both the will and the means to communicate with the living, either by controlling the hand or vocal organs of a medium or by influencing her mind telepathically. Most of them believe also that the dead can make their existence known by speaking to us directly, without the intervention of a human organism; by appearing in visible form; by the production of supernormal lights; by the supernormal movement of objects; and in various other ways. Now if the dead are really endowed with powers so varied and so remarkable; and if it is true, as they themselves tell us, that they are much occupied with the problem of comforting and assisting their surviving relatives; on these assumptions is it not matter for surprise that they refrained for so long from exercising their powers and making their existence known? During two and a half millenia of which we have fairly full written records—say from 650 B.C. to A.D. 1850—they failed so far as I know to produce satisfactory experimental evidence of their identity.² Why? During certain portions of this period they might have endangered their surviving friends by attempting to communicate with them; but there were long centuries during which action on their part would have been perfectly safe. Nor was there any lack of the necessary machinery or the necessary interest on the side of the living: the evidence collected in Oesterreich's book on Possession shows that the mediumistic trance is a fairly constant phenomenon in all ages and among all peoples; and curiosity about the state of

¹ *Op. cit.*, p. 524.

² I do not, of course, deny either that necromancy was occasionally practised, especially in later antiquity and the Middle Ages, or that the belief in the power of the dead to return in certain circumstances was general in the latter period. But the "spirits" appear to have made little or no attempt to establish their identity; and the ordinary mediaeval belief was founded much less on contemporary experiment than on the biblical story of the witch of Endor.

the dead has left its mark on the literature alike of Greece and Rome, of the Middle Ages, and of the Renaissance. But there is something more singular still. The two groups of pre-nineteenth-century mediums about whom we have most information, the *κᾶτοιχοι* of the late Graeco-Roman period¹ and the witches of the sixteenth and seventeenth centuries, while performing a number of the feats performed by modern mediums, perversely attributed them in the one case to the agency of non-human gods or demons, in the other to the agency of the devil. Once again, why? A satisfactory answer may one day be forthcoming; but until it is, I cannot but feel some doubt about the correctness of the spiritualist interpretation of the contemporary phenomena.

I come now to my second consideration, which is a very familiar one. It is drawn from certain apparent features of the relationship between the living mind and the living organism. Now it must be admitted that any attempt to interpret psycho-physical relations is in the present state of our knowledge fraught with danger and uncertainty, especially for the non-physiologist like myself. Keeping this warning before me, let me nevertheless consider the phenomena of old age. Anyone who has lived much in the society of the aged, and has observed them closely, will I think agree, whatever interpretation he may put upon the facts, that not only the human organism but the human mind, or that portion of it which expresses itself in thought, feeling and action, does appear to grow old. I am thinking here not merely of the grosser effects of time, those which a physician would classify as symptoms of senile decay; but also of the subtler psychological changes which come with advancing years, the gradually increasing imperviousness to new ideas, the gradually diminishing response to emotional stimuli, above all, the growing sense of finality, of fulfilment, of a destiny accomplished and accepted—in a word, the progressive encroachment upon the will to live of a new will to cease from living. Analogous changes appear, so far as we can judge, to attend the onset of old age in the higher animals, notably the dog and the horse.

Can these appearances—pathetic in some of their aspects, in others singularly beautiful—be interpreted in a manner consistent with survival? Many have thought so; but I cannot myself feel that the interpretations they have suggested are free from grave difficulty. It may be held that the old age of the mind is a temporary disease caused by the action of toxins produced by the ageing organism, and that on separation from the organism the mind will recover. But I find it hard to believe that growing old is really a reversible process—that mental changes so far-reaching as those associated with old age can be undone even when the supposed

¹ See *Journal*, xxvii. 216 ff.

originating cause is removed by death. Moreover, does not this line of argument assume a degree of dependence of mental states upon organic secretions which is sufficient in itself to create a strong presumption against the mind surviving after the dissolution of the organism? Another expedient is to assume that, besides the phenomenal self which experiences old age and its natural consummation in death, there is a remoter, ageless self which does not die because it does not grow old. But this hypothesis, apart from the total absence of any evidence in its favour, seems open to the same objection as the noumenal self of the metaphysical argument—for an ageless being can hardly be credited with an origin in time. In any case, like the noumenal self, it fails to throw much light on the supernormal phenomena: an element of the personality so remote from all ordinary observation and so strangely exempt from experience seems an unlikely carrier for the collection of small personal reminiscences which are the stock-in-trade of the average “communicator”.

The general situation as regards the connection between mind and body is I think reasonably and moderately stated by Dr Broad, who says: ¹

“The view that the mind is existentially dependent on the organism and on nothing else is compatible with all the normal facts, and is positively suggested by them, though they do not necessitate it. And it is the simplest possible view to take. The theory that the mind merely uses the body as an instrument is difficult to reconcile with the normal facts.”

Dr Broad's own theory is that the mind is a compound product of the organism *plus* what he calls a “psychic factor”, and that this psychic factor survives bodily death although the mind or personality does not. He claims that this hypothesis (*a*) covers all the observed facts, both normal and supernormal, and (*b*) is the minimum hypothesis which does so. Whether, if the first claim be granted, he is right in the second is a question to be discussed at a later stage. But I may venture to suggest here that the hypothesis in question does not seem to cover the supernormal facts very satisfactorily. Let us assume that when the late John Jones professes to communicate, and the communication is veridical, what has really happened is that a detached psychic factor, once an element in the mind of John Jones, has allied itself with the organism of the medium to form a new temporary mind, which is not the mind of John Jones but has one factor in common with that mind. Dr Broad supposes this alliance to result in the reinstatement of certain memories which were once present to the mind of John Jones, and the reproduction of certain modes of behaviour which were once

¹ *Op. cit.*, 549.

characteristic of that mind. So far, so good. But what if "John Jones" goes on to describe correctly some event which has occurred since John Jones's death, and is unknown to the medium and perhaps also to the sitter? There is nothing exceptional about such an occurrence at a good sitting. Mr Saltmarsh, in his valuable analysis of 142 sittings with Mrs Warren Elliott, found that in this series the total number of veridical statements about events subsequent to the death of the supposed communicator was actually *larger* than the total number of such statements about events prior to his death; and that the *percentage* of veridicality was also higher in the statements about events subsequent to death.¹ Now this is hardly what I should expect if the source of the communications were the surviving personality of John Jones; but still less is it what I should expect if the source were a psychic factor of the sort assumed by Dr Broad. "A psychic factor by itself", says Dr Broad, "is no more a mind than John Jones's corpse is a mind." What it is, or does, when it is "by itself" (*i.e.* unconnected with an organism), Dr Broad cannot tell us; but at any rate it is not a mind. From the day that John Jones's organism perished, his widowed psychic factor ceased to function as a mind, or element in a mind, until the day that it contracted an irregular union with the medium's organism. How, then, can it be a possible source of information about events which occurred in the intervening period? But if it is not a possible source, to what source shall we attribute such information? To the telepathic or clairvoyant powers of the medium, or to the agency of non-human spirits? I see no other alternative, since the agency of the dead is *ex hypothesi* excluded. Yet either step is surely fatal to the whole theory. For once we have granted that by the exercise of telepathic or clairvoyant powers, or by the agency of non-human spirits, the medium can supernormally apprehend an event which happened in 1916, the year after John Jones died, it appears wanton to invent a wholly different explanation for her supernormal apprehension of similar events in 1914 when he was still alive. The psychic factor has become, it seems to me, an entirely otiose hypothesis.²

But if I reject Dr Broad's interpretation of the evidence furnished by psychical research, what interpretation am I to adopt? In the first place, where a normal explanation appears possible I shall certainly accept it. I am, however, satisfied that neither chance nor cheating, nor any combination of the two, will suffice to account for the *whole* of the mental phenomena of mediumship. (About the physical phenomena I am less certain; but these do not in any case come much in question here, since the great majority of them do

¹ *Proc.*, xxxix. 91. A similar analysis of Leonard sittings is a *desideratum*.

² Cf. *ibid.* p. 173 ff.

not afford even *prima facie* evidence of survival.¹) As regards the mental phenomena, my choice is practically confined to three views :

(a) That which attributes them to the exercise by the living of supernormal faculties, viz. telepathy, or a combination of telepathy and clairvoyance.

(b) That which attributes them to the agency of non-human spirits.

(c) That which attributes them to the agency of the surviving dead. I will call these respectively the telepathic, the daemonist and the spiritualist hypothesis.

Now if my initial presuppositions are accepted, and my subsequent reasoning is valid, these three hypotheses vary widely in their antecedent probability. The telepathic hypothesis invokes no agency for whose existence there is not strong independent evidence : the independent evidence for regarding telepathy as a *vera causa* seems to me almost conclusive,² and that for clairvoyance very substantial. The daemonist hypothesis has the status of a bare unmotivated possibility : I know of no valid evidence against it, and I cannot agree with Mr Drayton Thomas in calling it "far-fetched and fanciful"³ ; but the only evidence in its favour is drawn from pre-scientific sources to which I can attach little weight. The spiritualist hypothesis seems to me, for the reasons I have given, to start under the heaviest handicap of the three ; not only is there no valid argument in its favour, but there is a definite antecedent presumption against it, although this does not amount to disproof. This being so, an elementary canon of scientific method requires me to give the preference to the telepathic theory, *provided that it adequately covers the phenomena to be explained*. But does it cover them adequately ? This is the crucial point of the whole enquiry, and to this I must now address myself.

I have made a list—doubtless not exhaustive—of ten objections which have been advanced against the telepathic hypothesis. Most of them were mentioned 12 years ago by Dr W. F. Prince in a paper which he read at the First International Congress of Psychical Research ;⁴ some of the others are taken from Dr Broad's book⁵

¹ Cf. Mr Saltmarsh in *Proc.*, xl. 106 ff. ; Prof. Richet, *ibid.* xxxiv. 111 f. For the same reason I have taken no account of "book-tests" or of prevision.

² The failure of recent experiments in "mass telepathy" in no way invalidates earlier evidence obtained under wholly different conditions : it merely proves (and the proof is valuable) that the conditions of these experiments are unsuitable for provoking the phenomenon.

³ *Proc.*, xli. 161.

⁴ *Compte Rendu Officiel*, Copenhagen 1922, p. 101 ff.

⁵ P. 546 ff.

and from a recent paper of Mr Drayton Thomas's.¹ I proceed to consider them *seriatim*.

(1) It is objected that the telepathic hypothesis does not account for the fact that communications invariably *claim* to come from the surviving dead.²

About this there are two things to be said. In the first place, the claim in question is by no means invariably made. Prior to the rise of the spiritualist movement in the nineteenth century the spirits of the dead were far from enjoying, if we can credit our documents, any monopoly of the control of mediums: the professed source of the communication was at least as often a non-human daemon or familiar, while in many cases no agency is alleged other than that of the "seeress" or "wise woman" herself. And even to-day the asserted monopoly is not without exceptions. Dr Osty, for example, has obtained numerous veridical communications both about the living and about the dead, comparable in range and accuracy with those of the best spirit mediums, from a subject, Mme Morel, who has no "controls" and no "communicators" and does not regard the dead as the source of her supernormal knowledge.³ It can hardly be doubted that were this lady imbued with the current spiritualist convictions her communications would emerge from the subconscious as orthodox "spirit messages".

For secondly, if we know anything about the working of the subconscious mind, we know (*a*) that it is addicted to dramatisation, and (*b*) that its dramas are usually if not always wish-fulfillments: both points are abundantly demonstrated by the study of dreams. Remembering further that the great majority of recent mediumships have been developed in a spiritualist environment, and that the great majority of sitters come to mediums not out of scientific curiosity but out of hunger for communion with the dead, I can find nothing in the facts here which I should not expect to find on the telepathic hypothesis.

(2) Dr Prince argued in 1921 that the wholesale ascription to mediums of telepathic powers was unjustified, since the experimental evidence suggested that only a very small minority of human beings possessed these powers in any recognisable degree, and there was no independent evidence that any medium possessed them.

The force of this argument has since been greatly weakened by the publication of a number of incidents which seem to have their origin in telepathy from sitter to medium. Perhaps the most striking of these is the John Ferguson case, published by Mr Soal

¹ *Proc.*, xli. 139 ff.

² Cf. *e.g.* Dr W. F. Prince in *Proc.*, xxxix. 299.

³ E. Osty, "Télépathie Spontanée et Transmission de Pensée Expérimentale," *Rev. Métapsychique*, 1932-3, pp. 80-3.

in *Proc.*, xxxv. The communicator, "John Ferguson", was eventually shown to be a fictitious personality; but before this happened Mr Soal (the sitter) had privately invented a number of hypotheses about "John Ferguson's" life and circumstances, which hypotheses were at subsequent sittings communicated to him as facts. This fictitious communicator also made a number of veridical references, the source of which could hardly have been any other than Mr Soal's mind. Mr Soal has himself mentioned the possibility¹ that the medium (Mrs Blanche Cooper) may have been assisted in her "mind-reading" by unconscious whispering on the sitter's part. The suggestion lacks proof and requires a deal of stretching to make it cover the facts²; yet if the case stood alone it might be not unreasonable to discount it on the ground of this suspicion. In fact, however, it by no means stands alone. Dr Osty has described numerous instances where different mediums have reproduced beliefs or hypotheses which were present in the minds of various sitters, but were subsequently proved to be erroneous.³ It is not easy to suppose that all these sitters (including Osty himself) gave themselves away by unconscious whispering or other means. Moreover, Osty finds that "quand on expérimente, en séances successives et périodiques, avec un même sujet sur un même être éloigné, il est aisé de se rendre compte que ce qu'on apprend sur cet être entre deux séances permet au sujet, dans la séance suivante, de préciser, de rectifier, de développer ses révélations antérieures."⁴ This agrees in principle with Mr Soal's experience; and it points clearly to telepathy from the sitter.

Nor are incidents of like import lacking in the records of Piper and Leonard sittings. Thus when Newbold was sitting for automatic writing with Mrs Piper, an irrelevant (and rare) name, which he had erroneously associated with the supposed communicator, was

¹ *Proc.*, xl. 177.

² It rests primarily on Mr Soal's statement that he possesses "an exceptional tendency to articulate words inaudibly" (*Proc.*, xxxv. 475). Did the tendency manifest itself at the Cooper sittings? Mr Soal thinks not, but it is hard to be certain (his *bona fides* is naturally not in question). Would his inaudible articulation convey anything to an experienced lip-reader? It seems quite possible. But what I cannot readily see is how on this hypothesis I am to explain such things as the very indirect yet quite convincing reference to the name "Paglesham" (p. 540). I can imagine Mr Soal unconsciously answering his own question by articulating "Paglesham": but would he articulate the ingeniously fantastic description "where cowslips grow in cockle beds", and the personal allusions which follow? I permit myself to doubt it.

³ *La Connaissance Supranormale*, p. 333 ff.; *Rev. Métapsychique*, 1932-3, p. 45 ff.

⁴ *La Conn. Sup.*, p. 206.

reproduced as a spirit utterance in Mrs Piper's script.¹ Again, when the experimenter (Hodgson) thinks about Sir Walter Scott, an obviously fictitious "Sir Walter Scott" communicates next day at the Piper sitting; when he thinks about D. D. Home, a similarly spurious "Home" presents himself next day.² In the same way Mrs Beadon (one of Mrs Leonard's sitters) meets in the street a lady whom she has not seen for twenty-four years, and is thus reminded of the lady's brother who died twenty-five years earlier; at Mrs Beadon's next Leonard sitting, a few days later, "Fedra" refers to this man by name and gives identifying details. Mrs Beadon states that she has observed parallel occurrences at many sittings.³ Again, Mrs Salter heard one evening in conversation an anecdote about a man who wore several pairs of trousers simultaneously; next day at a Leonard sitting the control purporting to be Professor Verrall remarked to Mrs Salter "It isn't given to many men to wear two pairs of trousers. Well, I did once, I think you'll remember." Mrs Salter did not remember.⁴ Here it looks as if the control mistook an anecdote telepathically derived from the sitter's mind for an incident in Professor Verrall's life. Again, in the White case Miss Nea Walker's erroneous belief as to the circumstances of the death of the presumed communicator is reflected by "Fedra."⁵

The evidence of such incidents is cumulative. If they are considered individually, possible alternative explanations can usually be devised. But collectively they seem to me to enforce the admission of telepathy from the sitter as a *vera causa*. The next step is to try to learn something of the conditions and limitations under which it operates; and for this purpose deliberate experiments in telepathically influencing the course of a sitting, on the lines of the Ferguson case, should in my view be undertaken.⁶

¹ *Proc.*, xiii. 24, 29 f. Cf. also p. 48 f.

² *Proc.*, xxviii. 85.

³ *Proc.*, xxxii. 69-72.

⁴ *Proc.*, xxxix. 320.

⁵ Nea Walker, *The Bridge*, p. 89. Cf. also Dr Jacks' Leonard sitting, *Proc.*, xxxii. 133 ff., where "Fedra" seems to have built up a fantasy about an imaginary "spirit" out of scraps of information telepathically derived from the sitter. It is worth noting also that Mrs Leonard claims to have experienced *in her normal personality* at least one veridical hallucination of a type usually assumed to be of telepathic origin (*My Life in Two Worlds*, p. 23).

⁶ The examples I have collected suggest on the whole that the ideas most likely to be transmitted are those which occupy not the foreground but the background of the sitter's consciousness. Cf. the difficulty which even the best communicators find in replying to a direct question; and the remark of Flournoy that "les idées des assistants qui ont le plus de chance de se transmettre au médium sont celles qui se trouvent en quelque sorte à l'état naissant ou évanescant, je veux dire sur la limite entre la conscience et l'inconscience, en train de passer de l'une à l'autre" (*Esprits et Médioms*, p. 481). This need not preclude deliberate experiment, but it should be borne in mind by experimenters.

(3) It is objected that if the telepathic hypothesis is to cover the facts we must credit mediums with the power of drawing on the contents of the minds of living persons quite unknown to them, who are not present at a sitting, and to whom their attention has not been in any normal way directed. Such an assumption, it is urged, goes far beyond any telepathic feats of which we have independent evidence; and if the phenomenon occurs only in spiritistic conditions we must assume that spiritistic conditions are requisite for its production.¹

This objection is more formidable than the last; and it has been brought into special prominence by the striking successes achieved by Mrs Leonard in "proxy" sittings. But, like the last, it loses a good deal of its force when we consider some of the incidents published by Dr Osty. These seem to show (a) that correct information outside the normal knowledge of all present at the sitting, concerning private details of the life of absent persons, may be given by sensitives who do not profess to be assisted by "spirits"; (b) that incorrect information may be given which corresponds to the belief of a third party who is unknown to the sensitive. Of the first type a number of instances are given in *La Connaissance Supra-normale*: e.g. Mme Peyroutet, on being asked by Osty to describe the past life of the person of whom he was thinking, furnished numerous details of a highly individual character which were quite unknown to Osty but were subsequently attested as correct by intimate friends of the person in question.² The circumstances here are analogous to those of a "proxy" sitting with Mrs Leonard, save that the being whose past is disinterred is a living being and not a dead one. The second type is exemplified in the case of the pretended Grand-Duchess Anastasia. Here the sensitive (Mme Morel), on being presented by Osty with a letter written by the real Grand-Duchess and a flower which had been handled by the pretender, had a series of visions which corresponded neither with the objective facts as subsequently verified nor with the impressions in Osty's mind, but with the fictitious story told by the pretender—a story which we are assured was at the time unknown to Mme Morel and only partially known to Osty.³

Osty is not the only investigator who has obtained results of this sort without the professed aid of spirits. Andrew Lang pointed out many years ago, à propos of the Piper controversy,⁴ that Miss Angus's crystal-gazing experiments afford strong *prima facie* evidence of an independent character that "third-party" telepathy (to use a con-

¹ Cf. Mr Thomas in *Proc.*, xli. 161; and Prince, *op. cit.*, p. 112 ff.

² p. 148 ff.

³ *Rev. Métapsychique*, 1932-3, pp. 52-7.

⁴ *Proc.*, xv. 48 ff.

venient phrase) really is a *vera causa*. On one occasion, for example, "Miss Angus described doings from three weeks to a fortnight old, of people in India, people whom she has never seen or heard of, but who were known to her 'sitter'," in circumstances which appear to preclude explanation by collusion or coincidence or telepathy from the sitter. Yet neither in this nor in any other of Miss Angus's experiments is there any suggestion of the direct or indirect agency of the dead. A spiritualist interpretation might, I suppose, be *forced* upon these cases and upon Osty's: it is certainly not the natural one.

Still less is a spiritualist interpretation applicable to the messages which Mr Soal received through Mrs Cooper from a supposedly deceased Gordon Davis—who subsequently proved to be alive and well at Southend.¹ Apart from the apparent element of prevision which it contains, this puzzling case seems to be susceptible of two, and only two, rational explanations. One is that the source of the messages was the subconscious mind of Mr Soal: this requires us to assume a considerable, but perhaps not impossible, amount of cryptomnesia on Mr Soal's part. The other is that the source was the subconscious mind of Mr Davis—a person who was quite unknown to the medium,² who was a good many miles distant at the time of the sittings, and to whom the medium's attention had not been in any normal way directed.

On the whole, I feel obliged to conclude that third-party telepathy, if it cannot be regarded as an established phenomenon, has considerably more than the status of a bare hypothesis. The independent evidence in its support is not very abundant, but it is not negligible. Its paucity need surprise no one who remembers (*a*) that in everyday life instances of long-distance telepathy from complete strangers, if they occur, are very unlikely to be recognised; (*b*) that in this country at any rate a "sitting" practically always means an attempt to obtain communications not from the absent living but from the dead. The remedy for the latter circumstance lies in our own hands.

(4) It is objected that the vivid presentation in trance of a personality normally unknown to the medium is not adequately explained save on the spiritualist hypothesis. Mr Salter has recently mentioned this along with the last objection, as two of the three chief stumbling blocks in the way of the telepathic theory; ³ and Mr Thomas lays great stress on it.

To this contention there are two possible rejoinders. In the first place, sceptics may doubt, and have in fact doubted, whether in

¹ *Proc.*, xxxv., 560 ff.

² If she had made secret inquiries about Mr Davis she could not have failed to learn that he was alive.

³ *Journ.*, xxvii. 331.

the circumstances of a sitting such "vivid presentation of a personality" ever has substantial value as evidence of identity. They point out that when the tones of the trance speech are recognised by the sitter as those of a familiar voice, or when certain mannerisms or *façons de parler* are felt by him to be characteristic of a certain person, it is very rarely possible to check the objectivity of the recognition, as can usually be done when a name, a date or an event is in question. The door is commonly left wide open to the insidious temptations of the will-to-believe—temptations whose potency in this context can be fully realised only by those who have either been sitters themselves or made an impartial study of the annotated records of sittings.

But secondly, whether evidence of this type has much or little value, there is some reason to think that certain mediums can on occasion "reproduce" the personalities of the unknown living with as much success as those of the unknown dead. At any rate, Dr Osty tells us that one of his sensitives, Mlle de Berly, is capable of "reproducing approximately the timbre and rhythm" of the voice of persons *living or dead* whom she has never heard speak, of "saying what they might say or might have said", and of "exhibiting their customary attitudes."¹ One would have liked more detailed evidence about these remarkable powers of Mlle de Berly—but in default thereof it is worth while to look again at the Davis case. The communicator in that case spoke, in Mr Soal's opinion, with a voice and accent closely resembling Davis's—so closely that early in the first sitting Mr Soal was impelled to cry out "By Jove, and it's like Gordon Davis!" Moreover, the communicator reproduced so successfully a number of Davis's mannerisms that Gordon Davis himself later agreed, when shown the record of the sittings, that it was very like Gordon Davis. Nevertheless it wasn't Gordon Davis. It wasn't Gordon Davis unless you are prepared to assume that he could simultaneously interview a client at Southend and converse with Soal in London through the lips of Mrs Cooper. Personally I find this assumption more staggering than any which is involved in the telepathic hypothesis; and I must therefore conclude that the latter hypothesis is *not* invalidated by the recognised reproduction of such personal characteristics as voice, accent and idiom.²

¹ *La Connaissance Supranormale*, 252.

² It is immaterial to the argument whether we suppose the impersonation to have been based on material telepathically derived, either from Mr Soal or from Mr Davis himself, or on the other hand simply assume that both Mr Soal and Mr Davis were victims of the will-to-believe. The point is that a vivid impression of Davis's personal presence was produced on a highly intelligent observer in circumstances which make it incredible that he was in any sense really present.

(5) A further objection to the telepathic hypothesis is that it appears to involve an otherwise unexampled *selective* action of the medium's mind, in supernormally deriving from other human minds precisely those remembered facts which are required for the building up of a particular trance personality. This was the spear-head of Hyslop's argument for the spiritualist interpretation of the Piper phenomena, and has often been urged since.

But I do not think that the available evidence in the least requires me to picture the subconscious mind of the medium hunting through the subconscious mind of the assumed agent, as through a lumber-room, until it finds precisely the bit of information which it needs in order to give verisimilitude to its impersonation of some deceased friend of the agent. I am equally free to imagine that when *rapport* is established between the medium's subconscious mind and that of the assumed agent, the nature of the material transmitted is determined by the relative emotive force of the agent's various complexes, or by the fact that the material belongs to an associative complex some elements of which are already in the medium's mind, or by any other cause that you like to suggest. Selection does undoubtedly operate at some stage before the material is presented in trance; but I see nothing to prevent its operating *after* the material has become part of the furniture of the medium's subconscious mind. I am free to imagine, in the first place, that the particular complex of feelings and images which underlies a particular trance personality attracts to itself only such elements of the newly acquired material as have some associative relevance to its existing content; and secondly, that the "control" who sits in the gateway of trance—Fedra or Topsy, Phinuit or Rector—operates on occasion, like the Freudian "censor", to prevent the emergence of irrelevant or disturbing matter which might interrupt the illusion and break the continuity of the medium's dream.¹ These are no more than guesses, although they derive a certain amount of support from the known mechanism of the normal dream. I claim no more for them than that they cover the observed facts as well as any other hypothesis, and better in one important respect than the hypothesis of possession. The degree of relevance and continuity to be observed in most trance communications is, to say the least, extremely limited. In Mr Saltmarsh's words, "one of the most striking features of communications received through trance

¹ Cf. a remark of the acting "control" in one of Mr Thomas's Leonard sittings: "If she [a certain communicator] tells me anything that disagrees with what she told me previously, I shall have to inform her that, in this confusing condition of the sitting, she is not recollecting clearly", *Proc.*, xli. 175.

mediums is their disjointedness".¹ This is what I should expect from the sort of psychological machinery I have suggested; it is not what I should expect if the communicators are what they say they are.

(6) A further objection to the telepathic hypothesis is that it fails to account for certain cases of "object-reading" (popularly called "psychometry"). The cases in question are those where a relic is submitted to an entranced medium, its ownership and history being *unknown both to her and to the sitter*, and she nevertheless furnishes correct details about its present or former owner. These puzzling occurrences have been most fully studied by Osty in *La Connaissance Supranormale*, by Prince and Pagenstecher in *Proc. Amer. S.P.R.*, xv. and xvi., and by Mr Saltmarsh in his report on Mrs Warren Elliott.

They are, it seems to me, puzzling occurrences on any hypothesis. The notion that the relic in some unimaginable way carries a permanent record of its own history, which the medium is able to read, is definitely put out of court by the fact that much of the information supernormally obtained in such cases refers to scenes in which the relic played no part whatever.² It apparently functions not as a record but as a signpost pointing to some mind, living or dead, whence the information is then supernormally derived. How is this function exercised? On the telepathic theory, the source of the information will be a living mind, usually and perhaps always that of the contributor of the relic; but I do not know how the presence of a watch or a purse creates a rapport between the medium's mind and that of the contributor. Equally, on the spiritualist view, I do not know how the presence of a material object causes the particular spirit which once owned that object to present itself to two strangers at a particular time and place. Are we to imagine the spirit, in the Warren Elliott experiments, following its relic from the house of the contributor to 31 Tavistock Square, and vigilantly haunting the S.P.R. rooms from the day of the relic's arrival until the day when chance determined that this particular relic should be removed, still in its wrappings, and without the contributor's knowledge, from the locked cupboard and conveyed to the sitting? or if not this, what *are* we to imagine, short of crediting the dead with omniscience? I can only echo Driesch's remark that object-reading is "the most mysterious and inexplicable of all supernormal phenomena".

It seems worth while, however, to mention an analogous incident which occurred in the Sinclair telepathy experiments, and to which Mrs Sidgwick called attention in her review of *Mental Radio*. The

¹ On the whole question of the *modus operandi* in trance communications compare Mr Saltmarsh's suggestive discussion, *Proc.*, xxxix. 119 ff.

² *Proc.*, xxxix. 156; *La Conn. Sup.*, 211. Other objections to this fantastic theory are briefly indicated by Prince, *Proc. Amer. S.P.R.*, xv. 314.

drawings used in these experiments were prepared in advance in batches, either by Mr Sinclair or by his secretary, and were then wrapped in green paper and placed in sealed envelopes. Mrs Sinclair later picked up the envelopes in succession and tried to reproduce the contents without opening them. Now on one occasion the secretary included in his batch two drawings which were not by him but by a person unknown to Mrs Sinclair. On picking up the envelope containing the first of these drawings, Mrs Sinclair gave a description which did not apply to the drawing in question, but did very accurately apply to the stranger's other drawing, which came seven places further on in the series. It looks on the face of it as if the handling of the stranger's first envelope had somehow served to establish telepathic rapport between Mrs Sinclair's mind and the stranger's. If so, we have here another case of the directive influence of the relic; only here the "relic" has no association with the dead, and the dead do not profess to co-operate in the achievement of the result. No solid argument can be based on a single instance; but I suggest that it is desirable to carry out further experiments on these lines both with Mrs Sinclair and with Mrs Elliott.

Another parallel in a non-spiritist context is furnished by certain of the feats attributed to the Polish amateur clairvoyant M. Ossowiecki, who makes no claim to the assistance of "spirits".¹ Handling, for example, a sealed letter whose authorship and contents were not normally known either to him or to Geley (the only other person present), he stated correctly, among other things, the age of the writer and also that of the writer's wife (who was mentioned in the letter).² These facts were not given or implied in the letter; they were not (apparently) known to Geley, who in any case could not be normally aware of their relevance to the letter; and the experiment had no connection with any deceased person. We seem reduced here to a choice between two explanations—an unlikely coincidence, or "third-party" telepathy mediated by a material object.

(7) It is further objected that the amount and quality of the veridical information given varies not with changes of sitter but with changes of communicator—which is the contrary of what we should expect on the telepathic hypothesis.

The evidence on this point is not so abundant as one could wish, since most sitters always evoke the same communicator, and most

¹ For his interpretation of his own powers see G. Geley, *L'Ectoplasmie et la Clairvoyance*, 72 ff.

² Geley, *op. cit.*, 35 f. It is not clear from Geley's account how far the facts apprehended by Ossowiecki were within the knowledge of Richet, from whom Geley had obtained the letter.

communicators always manifest themselves in response to the appeal of the same sitter or group of closely associated sitters. Mr Thomas claims that in a set of "proxy" sittings recently studied by him this difficulty is removed, since the actual sitter (Mr Thomas) was the same throughout the series, while there were 24 different communicators: he thinks that "it may therefore be assumed with some confidence" that variations in the veridicality of the communications depend on variations in the ability of the communicators. I cannot myself feel any such confidence. In 20 of these 24 cases the experiment was originated by the relatives of the alleged communicator, who were in many instances not personally known to Mr Thomas; and I gather that the bulk of the facts correctly given at these sittings were unknown to Mr Thomas at the time when they were given. It follows that on the telepathic hypothesis the ultimate source of the information about these facts is to be sought in the minds of the surviving relatives; and each communicator had apparently a distinct set of relatives. We are thus landed back in our old difficulty: when the communicator changes, the possible telepathic sources of relevant information change too, and variations in the veridicality of the communications can be equally well ascribed to the variation of either factor.

It is, however, a fact, as Hodgson pointed out, that some communicators, such as Mrs Piper's "George Pelham", have been successful with a number of different sitters, while others have consistently failed with a variety of sitters. This is perhaps most easily explained on the spiritualist hypothesis. But other explanations are not impossible: *e.g.* that the dream figure called "George Pelham" emerged from a deeper stratum of Mrs Piper's subconscious mind than the dream figure called "Stainton Moses", and was therefore more accessible to impressions telepathically received. And there is on the other side a fact which tells, so far as it goes, in favour of the telepathic hypothesis, namely that while some *sitters* consistently fail, others receive veridical information from a number of different communicators: Mr Soal and Mr Thomas himself are cases in point. My general conclusion is that little weight can be attached to objection 7.

(8) In the paper already mentioned Mr Thomas advances a series of objections which may be summarised in the statement that in these 24 cases there was no observable correlation between the success or failure of a sitting and the presence or absence of conditions which Mr Thomas considers favourable to telepathy.

I am afraid that the answer to this is that we know next to nothing about the conditions favourable to telepathy. I do not know which of Mr Thomas's cases were "likely cases" for telepathy, though Mr Thomas apparently does. There are only two things that I can say

(and those not very confidently). (a) Other things being equal, telepathy is probably more likely to occur when agent and percipient are in the same room than when they are miles apart.¹ This test cannot be applied to Mr Thomas's cases, since in none of them was the presumed agent present at the sitting. But in the Warren Elliott series studied by Mr Saltmarsh, which included 53 ordinary sittings and 89 of the "Absent Sitter" type, the percentage of veridicality in the former class was found to be quite double what it was in the latter; and a number of individual communicators who were successful when their surviving relatives were present at the sitting were complete failures at sittings where they were absent.² (b) Other things being equal, an "absent sitter" who is personally known to the actual sitter is probably better situated as a telepathic agent than one who is unknown both to the actual sitter and to the medium. This personal link existed in 12 out of Mr Thomas's 24 cases. The basis is far too narrow for confident generalisation; but it is at least noteworthy that the three best cases are "personal link" cases. The fact that five of the remaining "personal link" cases were inconclusive or failures does *not*, *pace* Mr Thomas, logically invalidate the possible inference that the existence of such a link is a condition required for the attainment of the highest degree of success; it merely proves that it is not the *sole* condition.

(9) It is further objected that the telepathic hypothesis does not satisfactorily account for cross-correspondences, in so far as these exhibit evidence of conscious design. This is Mr Salter's third stumbling-block.

This objection leads me on to the most thorny and difficult ground in the whole field of psychical research—ground which I could not possibly attempt to traverse in detail at the end of a long paper even if I felt myself adequately equipped for the task. All that I can do is to state the general impression which the evidence produces upon me, without demanding that others should acquiesce in my conclusion—or rather, in my absence of conclusions. For there are to my mind two points in which the evidence of cross-correspondences is inconclusive. In the first place, I cannot quite convince myself that in demonstrating pattern or coherence Mr Piddington and other investigators have conclusively demonstrated design. The patterns are there, and I do not suggest that their occurrence is in all or even in most cases due to chance. But suppose we posit that an undesigned telepathic infiltration from time to time takes place between the subconscious minds of certain automatists. What chiefly then

¹ Cf. the Brighton experiments (*Proc.*, vi. 156 f., viii. 544) and the Usher-Burt series (*Annales des Sciences Psychiques*, xx. 14-21, 40-54).

² *Proc.*, xxxix. 90 ff., 106 f. It is to be hoped that a similar comparison of Leonard sittings will one day be undertaken.

remains to be explained is (a) why the same idea frequently emerges in two automatists' scripts not in identical but in complementary forms; (b) why the emergence of a common idea is occasionally accompanied in the script by some such note as "seek elsewhere for this". I cannot feel that either of these peculiarities affords really clear evidence of design.

(a) As Pigou pointed out in 1909, "complementariness" occurred without design in a famous telepathic experiment in which Mrs Verrall was the percipient.¹ And, to quote a more recent example, when the agent tried to make Professor Murray think of Sir Francis Drake drinking the health of the Elizabethan mutineer Doughty, what emerged was "a faint feeling of Arabia or desert."² In a cross-correspondence this would have been called a very neat example of disguised allusion. Again, Osty has recently pointed out that in the majority of cases of spontaneous telepathy (phantasms of the dying, etc.) "la représentation mentale de l'événement se fait par images symboliques, allégoriques, et souvent elle est fragmentaire. Les choses se passent comme si le plan cryptique d'intelligence qui prend paranormalement connaissance de l'événement lointain ne voulait pas le présenter à l'ordinaire conscience."³ Does not this observation adequately account for the much admired obscurity of our cross-correspondences? We should expect them, it seems to me, on the telepathic theory, to be just what they are—oblique, allusive, and couched in phrases and images which are linked not by logical coherence but by emotional associations. These are in fact the common characteristics of subconscious products, as exemplified in many normal dreams.

(b) It seems not impossible that the consciousness which produces the script can sometimes distinguish from the general mass of

¹ The "One-Horse Dawn" case (*Proc.*, xx. 156-67, 387-94; xxv. 109-11). Mr Piddington subsequently endeavoured to show that this case involved something more than telepathy between the living (*Proc.*, xxx. 175-229, 296-305; xxxiv. 159-65); but I find the attempt more ingenious than convincing. I cannot feel that he has proved, or come within measurable distance of proving, that a reference to Jebb's note on *Oedipus Tyrannus*, 846, was intended by the intelligence responsible for the scripts. But secondly, if this were proved up to the hilt, there would be no reason, so far as I can see, for attributing the reference to any other source than the subconscious mind of Mrs Verrall. That she may have read, and probably had read, the note in question is not disputed (*Proc.*, xxx. 214). That upon subsequent study of her own scripts she failed to recognise the alleged reference does not surprise me at all; but it need not have surprised even Mr Piddington very much, in view of what psychoanalysis has taught us about the great strength and the apparent arbitrariness of subconscious resistances.

² *Proc.*, xxiv. 231.

³ *Rév. Métapsychique*, 1932-3, 96 f. Cf. Riecht, *Traité de Métapsychique*, 323 ff.

endogenous material those impressions which have been telepathically received, and may even recognise the particular source of the latter. This is supported by the case of Miss Samuels, who could usually distinguish the undesigned telepathic impressions received from Mr Wales both from those she received from Mr Fuller and from her own endogenous dreams or day dreams.¹

I am not wholly satisfied, therefore, that the cross-correspondences are the result of design. But, secondly, even if they are, I know of no conclusive answer to the suggestion put forward by Mangin, Broad and others, that the design of most of those hitherto published may have originated in the subconscious mind of Mrs Verrall. It is certainly true that more difficult intellectual feats than the construction of these puzzles have before now been performed subconsciously; that Mrs Verrall possessed just the sort of literary and linguistic knowledge which would be required for their construction; and that in all the more learned and elaborate of the published cross-correspondences her script played a prominent part. We shall be in a better position, however, to estimate the value of this theory when fuller information is available about the later work of the S.P.R. group of automatists.

(10) It remains to say a word about an objection which though demonstrably invalid is more frequent than any other on the lips of the uncritical. The spiritualist hypothesis, people say, is "so simple", the telepathic "so complicated".

If this means merely that the spiritualist explanation is more easily grasped by the unthinking, the statement is true, but irrelevant. We do not prefer Newton's picture of the physical world to Einstein's because it is more easily apprehended. If it is meant, on the other hand, that the spiritualist hypothesis is in the scientific sense simpler, the statement is relevant, but false. To a scientist, I take it, the simplest hypothesis is that which makes no assumption unsupported by independent evidence; the next simplest is that which makes the fewest and narrowest unsupported assumptions. Now the telepathic hypothesis assumes² that mediums possess a supernormal faculty for whose reality there is substantial independent evidence; and it assumes further that they possess it in a degree for which there is a *slight* amount of independent evidence. The spiritualist hypothesis assumes

- (1) that many, if not all, human personalities survive bodily death;
- (2) that they retain an accurate memory of many details in their past lives;

¹ *Proc.*, xxxi. 124 ff.

² In this comparison I have taken no account of the facts discussed in connection with objection 6, which seem equally obscure on either hypothesis.

- (3) that they have a detailed awareness of many physical events which have occurred among the living since their death ;
- (4) that they have in some cases access to the unspoken thoughts of the living ;
- (5) that they can at times communicate with the living, either by direct use of the organism of a medium or by telepathically influencing the medium's subconscious mind ;
- (6) that the unspoken wish of a living mind is in some cases sufficient to initiate this relationship between a particular deceased person and a particular medium.

These are I think the minimum assumptions which will cover the phenomena I have been considering. If the hypothesis is used, as most of its advocates use it, to explain also book tests, newspaper tests, and the whole range of physical phenomena, a large number of additional assumptions are involved. Thus, far from being simple, the spiritualist hypothesis is hydraheaded. It is in fact not one hypothesis at all, but a series of hypotheses, of such a character that no later member of the series is a necessary, or so far as our knowledge goes a probable, consequence of an earlier member. If the dead survive, there is no positive probability that they will remember details of their past lives ; if they survive and remember the past, there is no positive probability that they will be aware of terrestrial events after their death ; and so forth. Whatever advantages the spiritualist view possesses, simplicity in the scientific sense is not one of them.

I have now considered all the objections known to me which seem to me to have a *prima facie* claim to consideration. To sum up my conclusions, objections 1, 5, 7, 8 and 10 appear to me to have little or no cogency ; indeed, all of them are capable of being turned against their advocates. Objections, 2, 3 and 4 are to a considerable extent invalidated by evidence obtained under conditions which appear to exclude spirit agency. Objection 9 I am obliged to write off as inconclusive, for the reasons I have briefly mentioned. Finally, objection 6 calls attention to a set of phenomena which I cannot satisfactorily explain to myself on any theory.

Until, then, some stronger objection emerges, I must grant that the telepathic hypothesis covers the evidence as well on the whole as any other ; and since it is the minimum hypothesis which does so, it commands my provisional acceptance. Until this conclusion is upset, I must regard survival as unproved ; and I have stated my reasons for thinking that survival of the kind postulated by spiritualists, though not impossible, is antecedently improbable. It is in this sense that I do not believe in survival.

I must add that the two current forms of the spiritualist hypothesis—the theory of telepathy from the dead and the theory of

possession—seem to me to differ widely in their evidential status. Against the former no conclusive objection has been drawn, or is likely to be drawn, from the trance phenomena, for the excellent reason that we know nothing at all about the conditions which might govern this kind of telepathy. Against direct possession there is evidence which I find insuperable. It has been presented by Mrs Sidgwick in her two masterly papers on the Piper phenomena; ¹ and it is not necessary here to do more than recall its general character. The main points are the shiftiness displayed even by highly veridical communicators like “George Pelham”; ² their confident statements in cases where they can hardly fail to know that they are lying; the habitual lameness of their attempts to answer direct questions; and above all their acceptance of bogus personalities as genuine spirits (e.g. “George Pelham” guaranteed the authenticity of “Phinuit”, “Hodgson” upheld the objective reality of a “Bessie Beals” whom Hall had invented, “Frank Soal” described “John Ferguson” as a spirit, and none of Mrs Leonard’s communicators, so far as I know, has given “Feda” away). These facts are not incompatible with telepathy from the dead; but I do not know how to reconcile them with the theory of direct possession, although I have read many attempts to do so.

There are other considerations, of a sufficiently obvious kind, which *tend* to discredit all forms of the spiritualist hypothesis, but tell most definitely against the theory of possession. It is, I think, fair to say that the “spirits” have so far failed to convey to us any distinctive impression of their present mode of life, their occupations, or their state of mind; and that they have never explained this failure. How comes it that these countless Columbuses, returning to us (if but for an hour) from the supreme voyage of discovery, describe the life beyond the tomb in terms that are equally applicable to life in Putney, or alternatively, are borrowed from cheap theosophical literature? ³ Can the vivid literary talent of a Verrall or the philosophic insight of a Myers do no more than this? And why, in general, do the “spirits” of intellectually gifted persons produce no evidence that they retain their gifts in the other world? No single valuable contribution to art or science has been made, so far as I know, by an artist or scientist liberated from the material body: on the contrary, to study spirit communications in bulk, and

¹ *Proc.*, xv. 16 ff.; xxviii. 1 ff.

² A like shiftiness has been noted in subliminal personalities which exhibit supernormal faculty but make no claim to separate personal identity. For a good example cf. *Proc.*, iii. 14 f.

³ Sudre points out that French “spirits” commonly affirm reincarnation (as taught by Allan Kardec), whereas English “spirits”, more Christian in their outlook, usually know nothing of this heresy. Not even death, it appears, can resolve our theological differences.

without *parti pris*, is to echo the cry of Flournoy—"on ne sait s'il faut rire ou pleurer devant la trivialité, la niaiserie, l'incohérence de la plupart de leurs messages."¹ If there is an after-life, it would appear on the evidence so far available to be a life which kills all interest in intellectual pursuits, as living men understand them. This may be indeed the case; yet I cannot but think it surprising, as well as extremely unfortunate from an evidential point of view.

¹ *Esprits et Médiums*, 501.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 136

THE QUANTITATIVE STUDY OF TRANCE PERSONALITIES

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

I

PRELIMINARY STUDIES :

MRS GARRETT ; RUDI SCHNEIDER ; MRS LEONARD

But the best part of the soul is that which trusts to Measure and Calculation? Certainly.

And that which is opposed to them is one of the inferior parts of the soul? No doubt. PLATO.

The true Logic for this world is the Calculus of Probabilities, the only Mathematics for Practical Men. J. CLERK MAXWELL.

PART I.

INTRODUCTION : TECHNICAL METHODS

I

THE psychological status of Trance Personalities has been a matter of primary interest to this Society ever since its formation and has, indeed, engaged the attention of the curious from the earliest times.

Hitherto, however, our studies have been restricted, almost without exception, to what I may term "witness-box" methods. That is to say, we have listened (if we have not been too bigoted) to the accounts which these personalities have given of themselves ; we have applied (if we have been sufficiently critical) a certain amount of cross-examination, and we have sought to verify the items

of information which they have adduced in support of their claims to identity. In other words, we have treated them almost exactly as we would treat the claimant to a dukedom, if we had no fingerprints or Bertillon measurements of the true, but missing, heir. In particular, we have not hitherto been able to apply those processes of Measure and Calculation which alone characterise the transition from speculative enquiry to exact Science.¹

The object of the researches here described has been to explore the possibilities of substituting anthropometrical ² for legal methods and of bringing the personalities concerned out of the witness-box and into the laboratory.

2

The general method adopted has been that of the Word Association Test, use being made, according to circumstances, of the Psychogalvanic Reflex, the Reaction Time, and Disturbances in Reproduction, as indicators of the effects produced by the stimulus words.

Many readers are doubtless already familiar with this test; but I must devote a few paragraphs to it for the benefit of those who are not.

In principle, the procedure is simple enough. The experimenter takes a list of quite ordinary words, such as are met with in everyday life and calls them out, one by one, to the subject, who is preferably seated comfortably in such a position that he cannot see what is going on. The rules of the game are that the subject shall respond, *as quickly as possible*, with the *first* word suggested by the word called out. This sounds almost too easy to be informative, but certain complications are involved which may throw considerable light on the psychological make-up of the person under test.

For instance, the first word suggested may be too embarrassing to utter, so that it must be replaced by something innocuous; or what we may call purely intellectual difficulties may arise; or the subject may hesitate between one reply and another. In any of these cases the time elapsing between calling out the word and receiving the reply—known as the Reaction Time—will be longer than the normal, and this will be an indication that the word has some special significance for the subject.

More generally, we may say with Jung—the adoptive father of the technique—that the calling out of any word is equivalent to confronting the subject “in miniature” with all the situations in his experience in which the referent and connotations of that word have played a part.

This is so evidently true that I need not labour the point here; and it is, I think, equally clear that we have in this method, in

¹ See Appendix III, pp. 220–240, for Technical Notes, to which this and other numbers in the text refer.

principle at least, a means of distinguishing personalities by a substantially direct examination of their psychological content. For the experience of no two people is the same, and it is this experience which gives connotational significance to the words; so that a standard list will be differently accented, so to speak, for each individual and comparison of the accentuations should enable us to distinguish one individual from another.

Moreover, since reaction times and psychogalvanic reflexes can be measured, and disturbances in reproduction can be counted, we have, what is all important, a means of obtaining metrical attributes of the personalities—the psychological analogues of Bertillon measurements—to which mathematical treatment generally and the methods of statistical analysis in particular can readily be applied.

3

Now it has been shown up to the hilt—*e.g.* by Jung and Bleuler³—that the replies given and the reaction times, etc., observed in the course of the word association test are not determined only by those stored experiences, which we call memories, that are easily accessible to consciousness, but also by other factors—“complexes” and the like—lying deep in the subconscious or the unconscious.⁴ This, indeed, stands to reason and we have only to study dreams intelligently, or read such a book as Freud’s *Psychopathology of Everyday Life*, to realise how true it is that our most superficial actions, our most apparently inconsequent psychological formulations, are influenced by the deepest constituents of our beings and not only by those we can recognise.

This is, of course, very relevant to the whole subject of multiple personality in the ordinary pathological sense, and this in turn is closely connected with our present discussion.

In many such pathological cases we know, and in many others we have no reason to doubt, that the secondary personality shares a common subconscious with the primary and is, therefore, no more than the same “psyche”—if I may be allowed the term—masquerading in unfamiliar guise. It is, indeed, only this knowledge—or justifiable inference from analogy, as the case may be—that entitles us to speak of the two or more superficially different personalities as being “really” the same. Otherwise, we should be prejudging the whole central problem which has perplexed observers ever since the first hysteric was judged to be possessed of a devil.

Again, it has been shown—*e.g.* by Prideaux and others—that in cases of functional amnesia words which were of emotional significance in the forgotten part of life arouse abnormally large reactions when applied to the amnesic patient.

In view of these facts and others like them, we should confidently expect that the various personalities of any manifold—if I may adopt a convenient term—would show substantially identical, or at least significantly similar, reactions when tested in this way.

This view is supported by some experiments of Prince and Peterson in the BCA case as long ago as 1908.⁵ Using the psychogalvanic reflex as indicator, with the early technique due to Tarchanov, they succeeded in eliciting in personality A reactions of abnormal size, in response to test words relevant to one of B's dreams of which A had no conscious knowledge.

It follows from the foregoing that if the personalities of mediumistic trance are really no more than secondary personalities of the medium, as classical psychology would have us suppose, and if current theories of multiple personality are correct, then we ought to obtain from the supposed Control or "communicator" substantially the same reactions as those given by the medium in the normal state. If we do not, and if the difference is greater than can reasonably be attributed to chance, then we must conclude that one or other of these provisos needs revision; that is to say, either the Controls are not mere secondaries, or our psychological theories of multiple personality must be overhauled.

The general plan, then, is to take a number of sets of reactions to a given list of words from the normal personality and an equal number of sets from the trance personality, to compare the two aggregates of figures so obtained, and to calculate the probability that any resemblance or difference between them has arisen by chance alone—that is, by the operation of all those uncontrolled and unknown factors which, taken together, we call Error.

This last part of the procedure is, in an important sense, the most indispensable of all; for, strictly speaking, no result is worth anything until we know just how much it is worth.

4

I first suggested this method as far back as 1921, when I showed, using the psychogalvanic reflex and a correlational method of computation, that it was possible to distinguish normal people from each other and to identify them with themselves on different occasions by these means.⁶

But the honour (if honour it prove to be, as I hope it may) of having first attempted to apply the technique to the mediumistic trance goes to my near-namesake Mr Hereward Carrington, who is well known to students of *Psychical Research* and is now Director of the recently formed *American Psychical Institute*. In 1932 and 1933 he carried out, unknown to me, a series of tests with Mrs Eileen

Garrett and her Control "Uvani" (whom I have also studied) and the results were published in the first Bulletin of the Institute mentioned. The fact that these results were not at all conclusive, as was at first supposed by himself and by certain uncritical enthusiasts, has been in no way his fault but must be attributed entirely to ignorance of the psychological, physical and mathematical technicalities involved, on the part of those on whose assistance he relied. It is only fair to emphasise this here in view of the criticism to which the work in question must elsewhere be subjected. After all we do not blame, but rather praise, a pioneer aviator who succeeds in getting only to Newfoundland and not to Florida, if he has been provided with an unreliable compass, a twisted drift-sight and a misprinted Nautical Almanack.

This seems a convenient point at which to acknowledge most gratefully my own contrasting indebtedness to those who have helped me in the work I am about to describe.

First, of course, there are the three mediums, Mrs Garrett, Herr Rudi Schneider and Mrs Osborne Leonard, on whom the tests were made. Next the various Controls and "communicators" themselves, for, whatever their psychological status may ultimately prove to be, they have been uniformly obliging in this matter.

I am also greatly indebted to Mr Theodore Besterman for collecting the data for me in the cases of Mrs Garrett and of Rudi Schneider and, particularly, for suggesting that I should make a study of the latter as an integral part of the work being done on his mediumship as a whole.

In the case of Mrs Leonard I am under a deep obligation to Mr Drayton Thomas, not only for performing the same somewhat tiresome function, but also for most generously devoting a large number of his private sittings to the research and for enlisting the invaluable co-operation of his two "communicating controls." Mr Besterman, in his capacity of Investigation Officer to this Society, might almost be described as a professional chaser of the wild-goose eggs found in mares' nests, and is correspondingly inured to every form of thankless and wearisome investigation—though this, of course, in no way detracts from the value of his work here; but Mr Drayton Thomas is well known as a convinced, if critical, supporter of the spiritistic view of these phenomena, and I think, if I may say so without impertinence, that it speaks volumes for the genuineness of his scientific outlook that he should so willingly have begun and so enthusiastically have continued a long and tedious research—a research which he must have thought at best superfluous and one which might quite possibly produce results notably damaging to his most cherished convictions.

Above all, I wish to express and to emphasise my gratitude to

Professor Fisher, now occupying the Chair of Genetics at London University. Whatever success this research may be deemed to have achieved is—given the observations—almost entirely due to him; for, if he had not developed his elegant and powerful Analysis of Variance, the results could not, I think, have been obtained at all; while, if he had not personally guided my uncertain footsteps at every stage of the analysis, I should still be floundering in the morass of my own ineptitudes. The responsibility for the way in which I have applied his methods is, on the other hand, entirely mine.

5

I will now turn to a somewhat closer examination of the technique used, and will begin with the psychogalvanic reflex, to which I have already alluded more than once.

I want it clearly understood that there is no magic whatever about the psychogalvanic reflex. This formidable name refers merely to a decrease in the apparent⁷ electrical resistance of the subject's skin, and such a decrease takes place (in normal subjects, at any rate) whenever *any* stimulus is applied. It is true that, in spite of many years of study by numerous experimenters, there is still some difference of opinion as to precisely what causes it, as to the nervous mechanisms which control it, and as to what it is that it indicates. For some say that it indicates "emotion," while others say "conation" and yet others (of whom I myself was formerly one) prefer to speak of "affective tone." Mercifully, none of this matters for our present purpose; we are only interested in the undisputed facts that the decrease in apparent resistance is an easily measurable quantity and that it is positively correlated with the degree of psychological disturbance—of whatever kind this may be—that takes place when a stimulus is applied. We shall do best to think of it, in this context, merely as a measurable part of the total preparation for action which takes place in the subject as a result of confronting him "in miniature," as we have put it, with the situations recalled by the stimulus word.

Similarly there is nothing in the least remarkable about the apparatus we used for its measurement, except that it was specially designed for portability. It consists merely of an ordinary Wheatstone's bridge—the standard instrument for measuring electrical resistance—provided with the usual knobs and dials for reading the magnitude of the resistance to which it is applied (in this case the resistance of the subject) and it is fitted with a built-in galvanometer of a portable reflecting type, built-in batteries, sensitivity control and the usual switching arrangements. For a description of the apparatus, see Appendix I.

On the other hand, a few words may be of interest as to the way in which it is used. The standard method, so to speak, which I employed successfully in all my early work of 1920-21, consists in using a pair of circular zinc-plate electrodes, about two inches in diameter (in the Garrett-Uvani experiments they were 44 mm.), covered with chamois leather soaked in salt solution, and applied to the palm and back of the subject's left hand. The whole is padded with cotton wool and may conveniently be bandaged to a splint to minimize mechanical movement between the hand and the electrodes. Such mechanical movements tend to loosen one contact or the other and thereby to *increase* resistance, thus reducing or nullifying the effect we are seeking to observe and even producing, in many cases, a negative deflexion of the galvanometer.

This procedure works well for most ordinary people. So far as I can remember, I have met only about three, in a hundred or so tested, for whom it did not; but there are a few, who have a very high resistance and a tendency to erratic response, for whom more drastic methods are necessary. For these we now use "foot-bath" electrodes, such as are employed for work with electro-cardiographs. These consist of porcelain tanks containing the salt solution, in which the subject's feet are immersed, while special compartments contain zinc-plate electrodes, which accordingly do not make direct mechanical contact with the subject. This plan lowers resistance and practically eliminates the effects of muscular movement. It is the preferable procedure for all cases where there is any uncertainty as to the magnitude and reliability of the response obtainable with the simpler type of hand electrodes, which I should now use only in emergency or for specially good subjects. A still later plan, more convenient in many cases, is to use the foot-baths as hand-baths in which the hands and forearms of the subject are immersed, suitable transverse "grips" being provided to help in reducing casual movement.

It will be understood that what is actually measured is simply the movement of the spot of light given by the galvanometer along the scale attached to the latter, and that this movement is a measure of the resistance change produced in the subject's skin by the disturbance caused when the stimulus word is called out and answered.

Suitable corrections—*e.g.*, for sensitivity and for the voltage applied to the bridge—must, of course, be applied before the deflexions noted during the experiment are subjected to analysis.⁸

6

I pass to a consideration of the reaction times. These are, of course, easy enough to measure; for all that is needed is to use an

ordinary stop-watch, reading fifths of seconds, start it as the stimulus word is called out (preferably on the *accented* syllable of polysyllabic words), and stop it as the reply is given (again preferably on the *accented* syllable). The time thus recorded is the reaction time, and there is only one remark worth making here, namely, that any *constant* error on the part of the observer—or, indeed, any *constant* lag on the part of the subject—is automatically eliminated in the course of the subsequent analysis.

There are, however, two points connected with the *use* of reaction times, which are of very great importance, for ignorance of one might easily mislead and ignorance of the other might altogether vitiate otherwise valuable work.

The first is that differences in the actual mean magnitude of reaction time, taken over a series of words, do not necessarily imply, or even presumptively indicate, a difference of individuality between the personalities concerned. That is to say, if one personality gives an average reaction time of two seconds for a list of words and another gives an average of two seconds and a half, this is by no means necessarily an indication of non-identity in the sense that interests us here, even if the difference is shown by analysis not to be attributable to chance. Such a result could be brought about by many causes, such as drugs or fatigue or even a change of "mood" and it is, indeed, just the kind of difference which might reasonably be expected to accompany the trance state, as a part, perhaps, of the subconsciously determined impersonation undertaken by a secondary personality. We are interested only in the differential incidence, as regards personalities, of prolongation of reaction time among the various words of the list.

The second point may be explained as follows: In the case of the psychogalvanic reflex it is easy to see that there are plenty of physical and physiological reasons why the deflexion obtained cannot be indefinitely large. The subject's resistance cannot drop to zero (I am talking of ordinary deflexions and neglecting the not-impossible effects of the E.M.F. mentioned in Note 7) and even if it did, the resistance of the leads and electrodes, etc., would ensure a finite deflexion. To the first approximation, at any rate, we are accordingly entitled to take the deflexion as a definite numerical measure of the disturbance produced, which indeed, unless we call introspection to our aid, can only be stated in terms of the magnitudes we measure.

But there is nothing in principle to prevent a reaction time being prolonged indefinitely; there is only the moral effect of its being an experiment and not a child's game (and possibly the *force majeure* of the experimenter) to prevent the subject postponing the reply for ten minutes or till the week after next. Consequently, very long reaction times are apt not to be significant in proportion to their

length. If the average is one second, for example, a time of one second and a half probably means something; but a time of three seconds will mean little more, while one of ten will almost certainly mean no more at all than that of three.⁹

Now the effect of such meaninglessly long times, occurring at random, cannot be to produce spurious effects of similarity, for similarities depend, as is easily understood, on coincidences, as between the personalities, of long or short times on the same words, and the chance of this occurring by accident is taken care of in the analysis. But they might produce spurious effects of difference, as can be understood at once by imagining two idealised sets of identically similar reactions (which would, of course, give perfect similarity) and then considering the effect of adding random increases to the entries in one of them. We must, accordingly, be very much on our guard against this effect, if ever we meet with apparent differences between personalities based on reaction times. My own practice is to write double the mean value (or, better, double the median value¹⁰) for all values greater than this, and to compute on the basis of the figures thus obtained. My experience leads me to believe that this is just about right; but, pending further *ad hoc* research, I am not in a position to defend this procedure against anyone who likes to contend that one and a half times the mean, say, or two and a half times the mean, are better "cut-off" points.

In certain cases of critical importance, therefore, I have resorted to an over-drastring test by writing simply the value ONE for all values greater than the mean (or median, as the case may be) and ZERO for all values less than it. This procedure, of course, sacrifices all the information there may be in the gradations of the times and I accordingly use it only occasionally as a crucial qualitative test and rely on my "double the mean" plan for comparative work, where the error introduced, if any, will become what I may loosely term a second order effect depending on the differences between errors themselves already small.

Since writing the above I have begun a comprehensive study of the points involved and hope to be shortly in a position to propose a completely standardised procedure for dealing with both reaction time and psychogalvanic reflex data in this and other respects. Some such standardization is essential in order that results obtained in differing circumstances or by different workers may be truly comparable.

7

Thirdly, there is what is known as the reproduction test.

When the experimenter has gone through the list, noting the reaction times, the psychogalvanic reflexes and the words given in

reply to the stimuli, he goes through it a second time, asking the subject to reply, if possible, with the same word that was first given in association. Failure to do this, or prolonged hesitation in doing so, is counted as a "disturbance in reproduction," and the incidence of these disturbances among the various words of the list affords another basis for deciding whether the personalities concerned are similar or different.

This "complex indicator," to use the technical term, is one of exceptional merit,¹¹ mainly on account of the almost complete lack of extraneous error connected with it; and it has the additional advantage of being exceptionally easy to compute, since all the entries are necessarily either one or zero.

8

In addition to these three tests, I have done a considerable amount of work on other possible sources of information, notably on the Form of the Association; that is to say, on the grammatical form of the response and on the logical or grammatical relation between it and the stimulus word. But I have definitely come to the conclusion that, in the present state of the art, the method is substantially valueless.

It is easy enough, on the face of it, to classify the words or the relationships under different headings and to compute the chance that the numbers thus obtained for different personalities agree or disagree by accident to the observed extent. I have, indeed, obtained some most striking results by this method—for example, all the four personalities dealt with in the case of Mrs Leonard and described below appeared to be sharply separable by this means. But I have yet to find a system of classification which is both unambiguous and psychologically relevant, so that I was left with the uneasy feeling that I could quite well have produced entirely different results with almost equal plausibility.

I have accordingly abandoned the method, for the present, as an instrument of research, though I think it by no means impossible that special study might lead to a scheme capable of yielding very valuable results.

I have also tried other plans, such as the study of grammatical form alone, of the ways in which complex indicators are combined, and even of such preferences as are shown for initial letters for the response words.¹² But none of these have yielded any results of statistical significance one way or the other, and I have accordingly fallen back on the three classical indicators already described.

I have no time for more than a very few words on the subject of the beautiful and searching method, known as the Analysis of Variance, whereby the data obtained have been examined. (The general procedure is illustrated in Appendix II.)

If we inspect a mass of reactions obtained from two different personalities in response to the same set of stimulus words, we notice, of course, that, in general they differ one from another. These differences are due to a number of causes; one personality, for example, may be, on the whole, more sluggish in reply, or less sensitive as regards the reflex, than the other; the various words are responsible for marked differences of reaction; and there will be differences also corresponding to the several occasions on which the personalities have been tested—greater sensitivity on one day, less on another, and so forth. In addition to these clearly identifiable factors there operates a host of incalculable minor influences—ranging from distractions during the course of the experiment to the trivial incidents of the medium's daily life and thought—and these, collectively, make up error.

Now, what I may colloquially term the "overall differentness" of the hypothetical "population," of which the observations form a sample, is known as its Variance and to it contribute all the various factors I have mentioned. The function of the technique is to sort out the contributions made from the various sources, to express them in comparable form and, by comparing them with each other, to determine the probability of the similarities or differences existing between the reactions obtained from the different personalities being the result of chance alone.

It is impossible to exaggerate the importance of the last few words. Anyone can manipulate figures and get an answer of sorts; and all too many people do. But unless that answer is backed by a probability telling us what degree of reliance may be placed on it, it is likely to be worse than useless.¹³

In Science it is the Probability Integral, rather than Faith, that is "the assurance of things hoped for, the evidence of things not seen."

PART II

EXPERIMENTS AND RESULTS : INTERPRETATION

10

At last I come to the experiments conducted and the results obtained from them.

The first case to be investigated was that of Mrs Garrett and her control "Uvani," who, incidentally, purports to be an Arab and not to know English except, I gather, through the intermediacy of the medium.

These two personalities were tested at six double sittings,^{14c} and Mr Besterman, as already noted, was responsible for collecting the data, which comprised all three indicators.

Copies of these observations, as of all others mentioned in this paper, are preserved among the Society's archives.

The list of one hundred words used^{14a} was the same that I employed in my original work on the differences between normal persons. It was derived from that given by Dr Eder, in his Introduction to Jung's *Studies in Word Association*, by deleting the twenty words which I had previously found to give the largest psychogalvanic reflexes, on the average, when applied to about 100 normal subjects, and replacing them with twenty others which I judged likely to be less exciting.

The reason for doing this in the first instance was that I feared lest the inclusion of many words of great common interest—such as those bearing on erotic life and the like—might obscure individual differences in respect of other matters. In the present case I was again influenced by these considerations, but also by the knowledge that this list had already worked well. The order of the words was suitably varied from sitting to sitting.¹⁵

No difficulty was experienced in obtaining reaction times or verbal responses from either personality, each of whom, especially Mrs Garrett, had had previous practice with Mr Hereward Carrington in America.

11

On the other hand, Mrs Garrett proved to be by far the worst subject for the psychogalvanic reflex that I have ever worked with, while Uvani was little better, if at all. I have met, perhaps, so many as three or four persons of this high-resistance, erratic-response type before, but having previously had ample material to draw on, I discarded them at once in favour of more amenable subjects. We could not do this here, since mediums of Mrs Garrett's

calibre are not too easy to come by, so we struggled on doing the best we might and finally obtained some 520 positive deflexions of a sort, from the two personalities together, out of the 1200 aimed at. There were also 92 reasonably definite negative deflexions of no interest to us¹⁶ and ascribable to mechanical movement, while the remaining 588 attempts to obtain a reading failed to give anything worth writing down.

We used the hand electrodes throughout, hoping always that better results would be obtained next time; but I think now that I ought to have suspended the work after the first sitting and adopted foot-bath electrodes at once.¹⁷

These data (the 520 positive deflexions referred to) were far too fragmentary for treatment by the Analysis of Variance, which demands for its operation a compact array of figures without gaps. I was accordingly forced to employ the far cruder technique of correlation, which I had used in my early work, and this itself was reduced in value by the many gaps in the material.

I shall not trouble the reader with a detailed account¹⁸ of this work; for, although it occupied me for very many weeks, nothing unquestionably significant¹⁹ came of it.

None the less it is noteworthy that, no matter whether I dealt with the actual ohmic changes produced, or with the most elaborately corrected derivatives of these;²⁰ or whether I used the classical method of correlation, or that by "ranks" (which there is some reason for doing with material so bad as this²¹); or whether I eliminated or did not eliminate two words ("Frog" in the case of Garrett and "Dog" in the case of Uvani) which seemed unduly to influence the results,²² I always obtained a much stronger suggestion of *difference* than of *similarity*.

As a matter of fact, I obtained one result to the effect that the chance of the observed difference being accidental was no more than 1 in about 600; but in view of the general unsatisfactoriness of the material, I do not care to rely much on this figure.

12

The reaction times, however, told a far clearer story. In this case I was obliged to discard the first sitting altogether, because the official stop-watch broke down in the middle of it; but none the less Mr Besterman obtained for me a good compact mass of 91 words at each of five double sittings—a total of 910 observations. Such an aggregate is readily handled by the Analysis of Variance—once one knows how—and Professor Fisher himself very kindly dealt with the first fifty words for me in order to illustrate the procedure.

Actually, I worked these figures four times; first with the uncorrected data; second, writing double the mean for all values greater than this; third, writing 1 for all above the mean and 0 for all below it; and finally doing the same thing but using the median instead of the mean.²³

All four procedures led to the same result. There was found, in each case, either a wholly negligible similarity or none at all, together with a highly significant *difference*.

The chances of this difference being accidental were found to be about one in ten millions, one in one million, one in ten thousand, and one in ten million respectively.

Incidentally, this goes far to confirm the very approximate correctness of the plan of using double the mean for all greater values.

13

I also analysed the disturbances in reproduction in this case.

Unfortunately a great many replies took the form of "No" or "I don't know," and it is scarcely reasonable to maintain that the repeating of such negations constitutes undisturbed reproduction. I was accordingly obliged to omit these words, with the result that the data became nearly as fragmentary as those for the psychogalvanic reflex.

I obtained, however, an unbroken block of 26 words, which I treated by Analysis of Variance,²⁴ and I also applied another method, known as Contingency, to the comparable pairs of replies, working through the data sitting by sitting.²⁵

The two procedures agreed in showing a significant *difference* between the two personalities, the chance of this difference being accidental amounting to probably much less than one in one hundred.

14

Combining the results from these two sources of reaction time and reproduction test—which are quite independent—and allowing ourselves the modest make-weight of a one in ten chance from the psychogalvanic reflexes, we conclude that, in respect of the range of interests covered by the words of the list, these two personalities are significantly different—different, that is to say, to an extent that would arise by chance only once in some thousand million trials.²⁶

They show, therefore, no sign of sharing a common subconscience content in the way that the classical view of multiple personality would lead us to expect.

The next case to be studied was that of the well-known "physical" medium, Herr Rudi Schneider, and his Control "Olga," who purports to be, I understand, the surviving consciousness of one Lola Montez, once well known as an *artiste* and otherwise.

As already mentioned, this investigation was suggested to me by Mr Besterman with a view to its forming a part of a wider study of Herr Schneider's mediumship which was then in progress, but it developed, as will be seen, into a very valuable contribution to my own work.

The data were again collected for me by Mr Besterman²⁷ and consisted of the psychogalvanic reflexes and reaction times elicited in response to one hundred German words adapted by Mr Besterman from Jung's own list.^{28a} The words of this list largely resembled, either by actual equivalence or close affinity, those used for Mrs Garrett, but a number of special words were included which I shall discuss below.

Foot-bath electrodes were used for the psychogalvanic reflex with, on the whole, very satisfactory results. There was no trouble with negative deflexions, the subject's resistance was low^{28b} and deflexions of good size were easily elicited.

There was still a considerable number of readings spoiled for one reason or another and the first of the six double sittings had to be discarded altogether. We obtained, however, an unbroken block of 56 words for each of the other five, making a satisfactory total of 560 items for analysis.²⁹

This analysis disclosed a highly significant *similarity* between the two personalities, the chance of its being accidental amounting to something less than one in one hundred thousand. No more than a faint and wholly insignificant trace of difference was discernible.³⁰

I may say in parenthesis that once we get into these high figures (or, rather, these microscopic chances) the presence of a few zeros more or less may be disregarded. We obtain a measure of difference or similarity, as the case may be, the nature of which I shall discuss below, and the chances which I cite are measures not of these quantities but of our confidence in them. This confidence naturally increases with the number of data available, so that once it has become high we know that it is only a matter of going on long enough to obtain any specified chance we like, however small, assuming relevant conditions to remain unaltered. It would, therefore, be quite erroneous to conclude that Rudi and Olga are less definitely similar than Garrett and Uvani are different; for the results obtained from the latter pair are based on 910 items of information and

those for the former on only 560 ; and even this leaves out the question of the varying degree of error in the two cases. We are entitled to say that our *estimate* of the Garrett-Uvani difference is likely to be somewhat nearer the true value than our *estimate* of the Rudi-Olga similarity ; but the *fact* of significant difference and the *fact* of significant similarity may be regarded as coercively established in the two cases.

16

I mentioned just now that the list contained a number of special words. These were inserted with a view to throwing light, if possible, on Rudi's (and Olga's) attitude towards the genuineness of the phenomena alleged to occur in his presence.

When I analysed the data, I did not know which these words were ; but I selected ten, which seemed to be relevant and to have aroused especial interest to judge by an inspection of the reflexes they produced. I should add that no others appeared to have any bearing on the subject and that these ten did actually include all those chosen by Mr Besterman for this purpose.

These words were : *aufpassen, Gesetz, streiten, falsch, Angst, Geist, drohen, Schwindel, Vorsicht, vertrauen*. They may fairly be translated : *Beware, Law* (hence, rules, conditions, etc.), *Have a row with* (hence controversy, etc.), *False, Fear* (or anxiety), *Spirit, Threaten, Fraud, Caution, Trust* (hence distrust, suspicion, etc.). It is easy to see that all these words are likely to arouse, directly or indirectly, thoughts of fraud and suspicion, fear of exposure (if there is any) and the like.

In order to see what kind of importance these words had, I withdrew them from the aggregate and analysed afresh the data corresponding to the remaining 46 words. I still obtained a definitely significant similarity,³¹ the chance of its being accidental coming to no more than about 1 in 250, so that it cannot plausibly be maintained that the observed similarity is due to a strong common interest shared by two distinct individuals. On the other hand, the similarity is diminished to such an extent that this group of ideas, associated with fraud and suspicion, may fairly be said to dominate the psychic life of the joint personality, which we may term RUDI-OLGA. If I were pressed to desert purism for a moment in the interests of vividness, I would hazard the opinion that this group of ideas is responsible for something like one-third of the total similarity observed.

This is the more noteworthy in view of the fact that the list contained a good number of words connected with such almost universally interesting topics as love, sickness and death.³²

Even those with a weakness for illogical distinctions without

difference must, I think, admit that if Rudi and Olga are—in some mystical and transcendental sense—not “the same” they are, at any rate, “two minds with but a single thought, two hearts that beat as one”.

17

The reaction times given by Rudi were of peculiar interest.

In the first place they were very short, averaging about 1.9 second, as compared with about 2.5 for Mrs Garrett, 3.8 for Uvani and 4.5 for the whole Leonard group to be discussed later. (*N.B.* Olga was very much longer, with a mean of no less than 8.4 seconds, but this may be attributed to the peculiarities of the “trance” state and is not relevant to the present issue.)

Secondly, they maintained a most remarkable dead level of magnitude, showing extraordinarily little variation from word to word.³³ This is of importance for the following reasons :

My imperfect acquaintance with the German language has prevented me making as thorough a study of the replies given as I could wish ; but two things are quite clear to me even under this handicap. First, there is a very high proportion indeed of replies which seem to have no connection whatever with the stimulus word ; second, of these irrelevant replies, a considerable number—of the order of fifteen or twenty per cent, I should judge—appear to be determined by the immediately preceding word, or by one only two or three places removed from it.

When we find the first of these features—irrelevance—coupled with an abnormally *long* reaction time (as in some of the early Leonard sittings) we may confidently attribute it to “mind-wandering.” But when we find it coupled with an abnormally *short* time we cannot but suspect that the subject has been making up his mind what to reply *before* he hears the stimulus word. And this view is strengthened by the apparent borrowing of associations from near-by words. I have counted some fifty cases of this apparent borrowing in the first three Rudi sittings, whereas, in the whole of the rest of the material examined, I have noticed only some half-dozen examples of this happening.

I necessarily speak with diffidence here ; but the strong impression I gather from the data as a whole is that Rudi, far from co-operating properly in the test, was attempting rather to follow a plan of campaign well calculated to obscure the issues involved.

18

When these times were compared with those of Olga and analysed, further points of some interest emerged.

I first treated the raw data, without applying corrections of any kind. This gave me a highly significant *difference* between Rudi and Olga—a surprising and almost unbelievable contradiction of the even stronger similarity I had obtained from the psychogalvanic reflexes. When, however, I wrote double the mean for every value greater than this (*cf.* 6 above) and re-analysed, I found negligible indications of difference and similarity alike—the latter being, if anything the less feeble of the two. When I applied the still more drastic “one or zero” method, which I have already mentioned (using the mean as the critical value), I obtained a difference of rather less than nothing, if I may loosely put it so, and a similarity which, though scarcely significant statistically, was quite well marked.³⁴

It is clear that this procedure, by eliminating the cause of spurious difference already noted, has yielded a result conforming both to the decisive answer given by the reflexes and to the known inferiority of the reaction time data.

These points are not without an interest of their own, but the real value of the RUDOLGA investigation to the main structure of this paper is that it shows clearly that, when we are confronted with two apparently different personalities which are really one and the same, the method proclaims them to be so with no uncertain voice.

From the methodological standpoint, in which I am far more interested here than in demonstrating any particular conclusion, the importance of this can hardly be over-estimated. We should look with grave suspicion on our fine new telescope if it showed every star in the heavens as double!

19

I think that having gone so far, almost anyone might be excused for supposing that all we had to do in future was to sit back in our chairs and sort out pairs of personalities into similar sheep and dissimilar goats. Such, however, is far from being the case, and when I came to deal with the Leonard group of personalities, which I shall now discuss, I was faced with intricacies of relationships involving corresponding difficulties in analysis.

I must accordingly ask for close attention while I develop an argument which cannot fail to be complicated; and I can only hope that the reader will not find it so hard to follow as I have found it to prepare.

Through the great kindness of Mr. Drayton Thomas, as already mentioned, I was able to study, in the case of Mrs Leonard, no fewer than four personalities. These were: first, Mrs. Leonard in her

normal state (but see section 29 below); second, her Control "Feda," who—as most readers probably know—usually takes charge of the trance and purports to act as a kind of intermediary between the would-be communicators and the sitter; third, the late Reverend John Wesley Thomas—father of Mr Drayton Thomas; and, fourth, Mr Drayton Thomas' sister, *née* Miss Etta Thomas and daughter, therefore, of the Reverend John. These two appear to be able to control the medium—if I may be allowed spiritistic terms for a moment—almost as well as Feda herself; and it was, of course, while they were doing so that their reactions were taken.

I shall refer to these four personalities, for the sake of brevity, as L, F, J and E respectively.

20

Most unfortunately, we were unable to use the psychogalvanic reflex in this case. Mrs Leonard herself, I hasten to add, was very willing; but there were external obstacles which could not be surmounted and we were accordingly forced to rely on the reaction times, reproduction test and response words only. This, as may be imagined, was a heavy handicap, especially as all four personalities gave very long times all round and I was by no means sure that the variations in these would be sufficient to give definite results.

In any event, the task of determining the relationships within a fourfold personality with the aid of nothing more elaborate than a common stop-watch may reasonably be described as formidable.

I may say here, in parenthesis, that the response given to the words of the list showed a most striking concordance with the professed characters of the personalities concerned. Thus J was rich in scriptural allusions and references to church matters, E in associations constellated by reminiscences of ill health and F in words having a more or less Oriental flavour.

As I have already explained, I have not yet been able to devise a satisfactory method of subjecting responses to mathematical treatment, but it is greatly to be hoped that Mr Drayton Thomas—who is far better qualified to do so than I—will see his way in discussing this aspect of the research in a special communication.

The main experiments consisted of six quadruple sittings³⁵ and the same list of words was used that did duty in the Garrett-Uvani work. But it was found that we could not count on getting more than 75 reactions from each personality, because the work seemed to exhaust somewhat rapidly the "power"—whatever that may be—on which the process of "control" depends. For this reason, also, we obtained no reactions at all from E for the second sitting, because the power gave out after taking a hundred each from L, F and J.

Thus the comparisons discussed below are based on six sets of seventy-five words for each personality, except when E is involved, and then on five sets of seventy-five.

Extra reactions, up to a total of one hundred, were actually taken on a number of occasions, in the hope that the responses might be used statistically; but this hope proved illusory, as already explained.

21

Now in studying a quadruple manifold of this kind, the most natural thing to do, perhaps, would be to throw all four personalities into hotch-potch—as the lawyers say—and apply a test to the total mass of data to see whether, taken as a whole, it is substantially homogeneous or not.

Unfortunately, we cannot allow ourselves so simple a procedure as this; because, although the result would be extremely striking if the mass were found *not* to be homogeneous, we might easily be misled if it were.

Suppose, for example, that Feda were a clearly distinguishable personality, bearing the same relation to Mrs Leonard that Uvani bears to Mrs Garrett, and suppose also that she were capable of assuming diverse superficially different forms while remaining fundamentally the same—as a well-behaved secondary personality should do. Then the sameness between F, J and E might easily so obscure the difference between F and L, if all were treated together, that we should conclude that the whole manifold was homogeneous, whereas actually it would not be.

It is accordingly necessary to take the personalities by pairs and ascertain what relationship exists between the members of each. There are, of course, six such pairs to be studied, namely L and F, L and J, L and E; F and J, F and E; and J and E.

I accordingly analysed each of these pairs in turn and obtained the following results for *similarity*³⁶ based on the reaction times.³⁷

	<i>z</i>	P
LF - - -	.2740	Less than .01, say 1 in 110
LJ - - -	.3445	„ „ .002, „ 1 „ 600
LE - - -	.1977	„ „ .05, „ 1 „ 25
FJ - - -	.1271	„ „ .15, „ 1 „ 7
FE - - -	—	No similarity.
JE - - -	.2702	Less than .01, „ 1 „ 100

Now I cannot discuss the mathematics of the subject here; but it will not be misleading to regard the figures in the column headed *z*

as measures of the similarity between the personalities concerned—though somewhat clouded, of course, by error—and those in the column headed P as the chances that these apparent similarities have arisen by accident. Thus the smaller the value of P the greater our confidence in the measure of similarity to which it belongs.

It will be noticed that the closest similarity is between L and J—not between L and F as we might, perhaps, expect—and that we may feel practically certain that this is genuine; L and F, with J and E, come next and we may feel quite confident about them also; the similarities between L and E and between F and J are rather small and their associated P's rather high, so that we have not much confidence in them; while there is no similarity at all between F and E, so that the question of confidence does not arise.

22

The corresponding figures for the reproduction test ³⁸ are :

		<i>z</i>	P	
LF	- -	.2287	About .02,	say 1 in 50
LJ	- -	.3289	„ .002,	„ 1 „ 500
LE	- -	.2192	„ .03,	„ 1 „ 30
FJ	- -	.38965	„ .000,4,	„ 1 „ 2,500
FE	- -	.3314	„ .002,	„ 1 „ 500
JE	- -	.4951	„ .000,01,	„ 1 „ 100,000

These figures naturally do not correspond exactly with those obtained from the reaction times; and it would be very odd if they did, for the two tests clearly involve appreciably different processes. The reaction time test is a matter of finding a word with which to reply, while the reproduction test is one of remembering which word you found. The results of the two should give somewhat similar results (and they do), but there is scarcely more reason why they should be identical than those obtained by examining school children in Mathematics and in French.

The relatively very great similarity between J and E should be noticed here—a similarity consistent with their supposed relationship of father and daughter. This similarity is well above the average for reaction times also.

Another point of considerable interest may be brought out as follows :

Let us take the average similarity of each personality with the other three and arrange these averages in order of magnitude.

This gives us :

FOR REACTION TIMES.				FOR REPRODUCTIONS.			
L	-	-	·2731	J	-	-	·4045
J	-	-	·2473	E	-	-	·3486
E	-	-	·1227	F	-	-	·3166
F	-	-	·1037	L	-	-	·2589

Notice here how J and E keep together in each case and particularly how the J factor, so to speak, dominates the scene in the reproduction test and is second in the case of the reaction times. On the whole it is markedly *J* whom the others most resemble—not L as one might expect. We shall find this tendency again and again, and I have no hesitation in saying that, if the four sets of figures had simply been marked W, X, Y and Z and given to someone to analyse who did not know which was which, he would have come to the conclusion that the one corresponding to J was the “ highest common factor ” in the group and would have surmised that this was the normal medium.

It is also interesting to note—though I do not want anyone to attach a grain more importance to it than he wishes—that F least resembles the others in the matter of reaction time, where some element of word-selection is involved and likeness of vocabulary is calculated to promote similarity. This is curiously consistent with Feda’s claim to be of Indian birth, so that English would be an acquired language to her, and with her habit of talking somewhat childishly. If F is a secondary personality of L, in the classical sense, she certainly seems to play her part with an extraordinary attention to detail; but, on the other hand, it might be rash to wager very heavily on the validity of the indication obtained.

I must now ask my readers’ indulgence while I digress to discuss a point which must already have been perplexing some of them, especially those who are accustomed to dealing with statistics.

“ Why, ”—it has probably been wondered—“ having discovered pretty strong similarities in five cases out of six, is it not frankly admitted that we are dealing with a straightforward case of quadruple personality and have done with it? Is not all this talk about

differences of similarity and the dominance of one factor rather than another, and all the rest of it, no more than a somewhat disingenuous attempt to discover differences where none exist and so pave the way for a fanciful interpretation of facts which could be simply explained?"

With this feeling, if it exists, I have every sympathy; for it is not unnatural on the facts so far presented in the manner I have so far presented them.

None the less, I propose to reserve my main defence for a moment, and I will ask the reader to follow me in an argument which I believe to be in some degree novel, which is certainly important, and likely, I think, to convince him that the differences I have been discussing are not artifacts, but genuine differences worthy of examination.

I want those who are conversant with statistical practice to forget, and the others not to think of, the ideas of "populations" and "samples" which form, so to say, the current coin of statistical small talk.

I want to discuss the matter rather in terms of such everyday objects as Automatic Machines—the kind of things one finds on railway stations, where one puts in a penny and takes out a packet of buttered toast or a good cigar. The resemblance of such a machine to a medium under test is very close; there is only the trifling difference that instead of a coin one puts in a stimulus word and instead of a packet of buttered toast one takes out a reaction time. It would, indeed, be easy enough, in principle, to design a machine which in answer to a word called out to it would reply with another after an interval determined by the nature of the word called out.

Consider two of these machines and imagine that each has slots for a half-penny, a penny, a three-penny bit, a sixpence and a shilling—or as many other coins as you like. Let us suppose also that both machines deliver bars of buttered toast of different weights according to the coins inserted, and that we want to determine experimentally whether the machines are similarly constructed or not. We put our five coins into the first machine and receive, let us say, packets containing respectively one, two, six, twelve and twenty-four ounces of the commodity. We do the same with the second machine and receive—somewhat to our surprise—two, six, one, twelve and twenty-four ounces for the respective coins. We conclude that the machines are to some extent similar (because the two biggest coins produced the same results) and to some extent different (because the three smaller ones did not).

Now it is quite easy to calculate a measure of similarity from these data—one of the z 's, in fact, that I have been talking about—and also the chance that this result is accidental—for example, that one of the machines is out of order. But we need not bother about the

actual values in this rather fantastic case: What I want to emphasize is that, unless one of the machines goes wrong, we can go on repeating the test indefinitely without ever getting a different measure of similarity—because the similarity is, as it were, *built into the machine*. But we will, of course, get a constantly increasing measure of confidence; for the more often we get the same result the less likely it is to be due to some passing derangement and our P the chance of accidental occurrence—is a measure of this likelihood.

Now, in machines there may be said to be no error; but if there were—as there is in obtaining reaction times from human subjects—or if we weighed our butterscotch inaccurately, we would get a false measure of similarity; but the longer we went on the more closely would our estimate approximate to the true value and the decreasing magnitude of P would be an indication of this.

It follows that, unless we are prepared to deny the propriety of regarding a subject under test as a response-producing mechanism, it is no use talking in an all-or-none sort of way (protopathically, as the late Dr Rivers would have put it) about significant *versus* insignificant similarities. We must recognise that these figures we have calculated—these *z*'s—are genuinely measures of the relationships between the personalities, somewhat deformed by error, it is true, but accurate and reliable to the extent indicated by the associated chance. And in so far as they differ between themselves they indicate corresponding differentiations within the psychological manifold considered.

25

I do not know whether the reasoning in which I have just indulged will have seemed convincing. If it has not, the reason, I venture to believe, is to be found only in my imperfect exposition.

Fortunately, it does not particularly matter; for I am in a position to adduce further facts which I think will be found substantially conclusive.

I have already mentioned that the technique of Analysis of Variance affords a test of difference as well as one of similarity. When we apply this test to the Leonard reaction time data we have: ³⁹

	<i>z</i>	P
LF	- -	No difference observable (<i>z</i> is -·0531)
LJ	- -	” ” ” (<i>z</i> is -·1773)
LE	- -	” ” ” (<i>z</i> is -·1088)
FJ	- -	·2266 ·005 v.n., say 1 in 200
FE	- -	·2063 ·01 ” ” 1 ” 96
JE	- -	No difference observable (<i>z</i> is -·1946)

Taking the average differences in the way we did before we have :

F	-	-	-	-	-	·1266
E	-	-	-	-	-	-.0324
J	-	-	-	-	-	-.0484
L	-	-	-	-	-	-.1131

It will be seen that this order for difference is precisely opposite to that for similarity ; which is as it should be, if—as seems natural—the most markedly similar personalities are the least markedly different. The chance of this effect arising by accident is, of course, 1 in 24—a comforting, if not compulsive, reassurance that the measures of similarity and of difference which we are studying are not a “fortuitous concourse” of chance determined figures, but an ordered assembly with, so to speak, a definite structure behind it.

The figures for reproduction test differences are somewhat curious : for, although the differences shown are all feeble, they are all positive. I do not know what the chance is of this occurring by accident among six partially interdependent quantities of this kind ; but it is clearly, I think, not more than 1 in 8, because we can choose (in several different ways, as it happens) three *independent* comparisons—such as LF, LJ and LE—the probability of any one of which being positive is one-half, on the assumption that chance alone is at work. Combining this with the above-mentioned chance of 1 in 24 we get a combined chance of about 1 in 160, which makes us feel pretty certain that there is some method in this madness.

The actual figures for the differences are :

	z	P
LF	- - ·0437	About ·31
LJ	- - ·0836	„ ·23
LE	- - ·1542	„ ·045
FJ	- - ·0712	„ ·41
FE	- - ·1504	„ ·05
JE	- - ·0390	„ ·33

The average differences for personalities are :

E	-	-	-	-	-	·1145
L	-	-	-	-	-	·0938
F	-	-	-	-	-	·0851
J	-	-	-	-	-	·0646

and these show only a trifling correlation in the expected sense with the corresponding figures for similarities. On the other hand, among the similarities and differences themselves we observe no fewer than three coincidences in rank (in the expected sense, of course) and the chance of this happening by accident is 1 in 18.

Thus we see that from all sides small contributions conspire to

give us a strong assurance that we are dealing with an ordered system of differing relationships and not with a casual collocation of artifacts arising from the fortuitous incidence of error.

26

The coercive evidence, however, is to be found in the reaction time differences for FJ and FE. The chances of these being accidental were about 1 in 200 and 1 in 100 respectively, and it will be remembered that these comparisons gave also the smallest measures of similarity. I have, of course, checked these by the "one or zero" method mentioned in section 6 above and obtained confirmatory results.⁴⁰

Now these two comparisons are independent, in the sense that there is no sort of reason, other than the existence of an inherent structure such as we are interested in, why a difference between F and J should involve one between F and E.

We may accordingly combine them by the usual rules and we then have a combined chance of only about 1 in 19,000 of there being no heterogeneity at all within the total mass of data.

It follows from this, to a very high order of probability, that the Leonard Quartet, if I may so describe it, is *not*—like Minkowski's Space-Time—"a homogeneous four-dimensional manifold" but that there are highly significant heterogeneities within it.

This should dispose of any lingering objections that may still be felt as to the propriety of treating the differences between similarities as validly informative results, even if my *ad hoc* argument by analogy with automatic machines has failed to carry conviction.

27

I must mention here that, in addition to the six quadruple sittings just described, we carried out an entirely separate set of experiments with the Leonard personalities, dictated by my early fear that the reaction times were too long and too obscured by error to give reliable results.⁴¹

For this second set of experiments,⁴² I used a different and—if I may put it so—a much "stronger" list of words, containing many more of marked general interest, and this fact alone would prevent us from expecting precisely parallel results.⁴³ I also asked Mr Drayton Thomas to do his best to speed up the reactions; and this he did effectively, for the mean time for all personalities dropped from 4.5 to 3.1 seconds.⁴⁴ On the other hand, it proved possible to use only fifty words instead of seventy-five and to obtain only four instead of six sittings.

The results obtained accordingly bear but little resemblance to those from the main set of experiments,⁴⁵ and these results are of low reliability, so that this work is mainly of interest in showing that a total of only 200 observations from each subject is not likely to afford a firm basis for future investigations.

None the less, one or two points of interest emerge, which seem worth mentioning. For example, J and E still show the strongest similarity, not only on the reproduction test⁴⁶ as formerly but also on the reaction times. J's dominance of the group asserts itself here in respect of reaction times and not of reproductions, where he drops to third place; and F retains her position at the bottom of the reaction-time list, thus tending to confirm the suspicion that there is something aberrant about her when an element of word-selection is involved.

28

I should like to suggest, at this point, somewhat parenthetically, that it should be possible to extract yet more information from these facts, and those yet to be given, by a process of systematic inter-comparison. We might develop an argument, or series of arguments, somewhat on the lines "If the similarity between A and B is greater than that between B and C, and if the difference between A and C is less than that between B and D, then . . . etc." Such a procedure systematically applied would presumably disclose certain inconsistencies within the nexus of relationships considered and it should be possible to decide what scheme of similarities and differences calls for a minimum distortion of the figures given, allowing for the fact that they are of differing reliability as indicated by their associated P's (or the ratios of the z's to their standard errors which determine these).

This appears to call for a mixture of logic and mathematics beyond my present powers; but I fancy it might prove a valuable line of investigation.

29

The foregoing pretty well clinches, I think, the case for the existence of marked and genuine differences between the components of the Leonard group; but I have yet to present what are, perhaps, the most striking of all the results I have obtained. The circumstances which led to them were as follows:

When I first began work on the analysis of the Leonard data I was much perplexed to find so many resemblances as those I have described. I had not then formulated the technique which later enabled me to distinguish the personalities, and the contrast between this considerable measure of similarity and the marked difference observed between Garrett and Uvani was very puzzling.

It was an almost casual, but none the less valuable, remark by Mr Drayton Thomas that set me on what appears to be substantially the right track. In one of his letters, he mentioned what he termed Mrs Leonard's "personal preparation for a sitting" and suggested that this might result in the reactions taken in what was supposed to be her normal state being not, in fact truly normal.

I at once asked that a further six sets of Leonard reactions should be collected in circumstances calculated, so far as possible, to ensure a genuinely normal condition and, in particular, as far removed as practicable from any of Mr Thomas' own sittings. Mr Drayton Thomas most kindly concurred in this proposal and further enlisted the services of two friends, Mr W. H. Wilson and the Rev. G. C. Batten, to assist in the work. We are very much indebted to them for their good offices in this matter, which could scarcely have been managed in the time available without their help.⁴⁷

I shall speak of Mrs Leonard thus tested as "Normal Leonard"—L(N) or LN—and of Mrs Leonard previously tested in a supposedly normal, but actually prepared, state as "Prepared Leonard"—L(P) or LP.

I had in mind, of course, the idea that the process of preparation would presumably be intended to promote in advance of the sitting some measure of preliminary *rapport* with the expected communicators and that, on any hypothesis, this might lead to reactions taken in this state being coloured, as it were, by whatever influences are at work during the sitting itself.

30

I accordingly examined first the reaction time relationship between Normal Leonard and Prepared Leonard, and then those between Normal Leonard, F, J and E as before, with the following interesting results :

There is only the slightest of similarities between the two Leonards ; but equally there is no difference.

The figures are :

SIMILARITY				DIFFERENCE			
z		P		z		P	
·1085	-	-	·18	-·0253	-	-	—

The two states may be regarded as quite neutral, so to speak, with respect to each other.⁴⁸

On the other hand I found no similarity, but a very marked *difference* between Normal Leonard and Fedra, whereas there had been a definitely significant *similarity* in the case of Prepared Leonard.

The comparative figures are :

		SIMILARITY		DIFFERENCE	
		<i>z</i>	P	<i>z</i>	P
L(N) & F	-	-0050	—	.2834	.0008 v.n.
L(P) & F	-	.2740	.009	-.0531	—

This tremendous reversal of relationship cannot rationally be attributed to chance. We conclude, therefore, that some highly significant change is wrought in Mrs Leonard's psychical constitution by the process of preparation referred to.

When we compare Normal Leonard with J, it is interesting to note that there is still a considerable measure of similarity and only an insignificant trace of difference, as compared with an overwhelming similarity and no difference at all in the case of Prepared Leonard. The comparative figures are :

		SIMILARITY		DIFFERENCE	
		<i>z</i>	P	<i>z</i>	P
L(N) & J	-	.2546	.013	.0444	.32
L(P) & J	-	.3445	.002 v.n.	-.1773	—

In the case of E we find a negligible similarity and an equally negligible difference ; the two personalities are even more neutral to each other than the two varieties of Leonard. Previously there had been a very modest degree of similarity and no difference at all.

Comparative figures :

		SIMILARITY		DIFFERENCE	
		<i>z</i>	P	<i>z</i>	P
L(N) & E	-	.0463	.34	.0401	.33
L(P) & E	-	.1977	.05	-.1138	—

This is the only relationship which is substantially unaffected. I shall discuss the possible significance of the other changes below.

We may now re-write our tables using Normal Leonard instead of Prepared Leonard and obtaining :

NORMAL LEONARD REACTION TIMES

		SIMILARITIES		DIFFERENCES	
		<i>z</i>	P	<i>z</i>	P
L(N) & F	-	-0050	—	.2834	.0008
L(N) & J	-	.2546	.013	.0444	.32
L(N) & E	-	.0463	.34	.04	.33

with the other relationships as before, namely

F & J	-	.1271	.14	.2266	.005
F & E	-	.0999	—	.2063	.01
J & E	-	.2702	.01	-.1946	—

The mean figures for similarity by personalities are :

J	-	-	-	-	-	·2173
L	-	-	-	-	-	·0986
E	-	-	-	-	-	·0722
F	-	-	-	-	-	·0074

(Notice how J goes at once to the top of the list as the evidently dominant factor.)

31

In the case of the reproduction test data I obtained the following results :

NORMAL LEONARD REPRODUCTIONS

	SIMILARITIES		DIFFERENCES	
	<i>z</i>	P	<i>z</i>	P
L(N) & L(P)	·3303	·0025	-·0136	—
L(N) & F	-·2597	·013	·1294	·13
L(N) & J	-·1151	·16	·2082	·01
L(N) & E	-·1092	·18	·3144	·00025 ⁴⁹

with the other relationships as before, namely

F & J	-	·3896	·0004	·0712	·41
F & E	-	·3314	·002	·1504	·050 v.n.
J & E	-	·4915	·00001	·0390	·33

The mean values of *z* for personalities are :

L (N)	-	·1613	·2173
F	-	·3119	·1679
E	-	·3269	·1170
J	-	·3329	·1061

It will be noticed that the two orders show a one to one (inverse) correspondene in accordance with expectation.

Note how, with these data, Normal Leonard differs violently from E and significantly from J.

After this there can be no excuse whatever for regarding the Leonard manifold as homogeneous.

32

I will do my best to take a bold sweep and straighten out this tangled tale in a few dogmatic words.

The four factors, L, F, J and E are clearly distinguishable, if not by one test then by another.

Of these four factors J is dominant, particularly as regards the trance itself and the prepared state of L. The influence of the J factor extends even to normal Leonard states; but this is not

unreasonable on any hypothesis. Mediums are often described as "sensitives" and it is certainly likely that they owe their gifts, whatever the explanation of these may be, to a susceptibility which more ordinary people do not share. It seems to me inevitable that the mere fact of doing a test, even in her normal state, after having done so many with Mr Drayton Thomas, would arouse in Mrs Leonard's mind, or attract to it, whatever influences are operating in the Thomas sittings.

The E factor is closely linked with J, and it seems to me that the resemblances noted between L and E, such as they are, arise more because E is like J and J dominates L than because E and L resemble each other. Note how E escapes from L as it were when J's influence is weak, as in the reproduction test with Normal Leonard.

F seems to occupy a somewhat intermediate position. Her allegiance appears to oscillate between L and J according to circumstances and her connection with E seems to follow her connection with J. But the reaction times distinguish her very sharply from Normal Leonard.

33

Unfortunately, I cannot very well end this paper without a few words on the thorny subject of interpretation, even though I am much more interested in illustrating a method than in drawing conclusions from these applications of it.

Thus, although I am above all things anxious that we should maintain a conservative policy and develop this kind of work on a basis of reasoning from present knowledge rather than of guessing from present ignorance, it would be mere cowardice if I did not point out some of the difficulties which would embarrass any attempt to explain these facts in the light of current psychological conceptions.

Let us see what we can make of it on orthodox lines.

We might start by saying that the work of Prince and Peterson was on too small a scale to be very weighty, and we might go on by saying that the condition under which Rudi appears to play the part of Olga is not a trance at all. Both these points are debatable, but we will not bother about that now.

This procedure enables us to suggest that the yielding of significantly dissimilar reactions is a regular feature of multiple personality. I do not in the least know how we could reconcile such a contention, and all that it implies, with the rest of our knowledge of how subconscious content constantly influences behaviour. But I doubt not that there would be found, let us say, here or there a psychoanalyst hardy enough to undertake the task, for are not some of these true sons of Habbakuk—*capable de tout*?

But I think that even the hardiest and most successful expositor might find himself in a quandary if he were then asked, "Well then, by what criterion *do* you define individual minds as different?" If he replies, "By the criterion of having different bodies," he begs the whole question at issue and is *ipso facto* unfitted for human intercourse, while, if he says, "By the criterion of having differing sub-conscious content," he will be reminded that this is just what he has been attempting to explain away. I think his only chance would be to maintain that—in the case of Mrs Garrett, for example—there are "really," if we only knew it, duplicate brains and nervous systems which are switched into and out of action by the process of going into trance.

Personally, I consider this sort of thing to be fantastic to the point of absurdity and calculated to arouse only the ribald laughter of instructed persons; but I suppose that as a most desperate *pis aller* it might satisfy the prejudiced and the ignorant for a time. But I cannot imagine what our supposed apologist would do when he came to the Leonard material and found himself confronted with this strange medley of personalities, resembling each other in some respects, differing in others, in varying degrees and according to circumstances—for all the world as if they were really four different consciousnesses each interpenetrating the other to some extent. Above all, he will find it awkward to explain how it comes about that a personality which gives so perfect a rendering of a distinguished Wesleyan Divine, contrives to dominate the scene and to be, as it were, *plus royaliste que le Roi*—more normal than Normal Leonard herself.

I fear he will be driven to the conclusion that I must have calculated the whole thing backwards like a cinema-film reversed.

34

On the other hand, for anyone who likes to be bold enough to entertain the hypothesis that the personalities are what they purport to be, the whole thing becomes simple enough. And it is simpler still if we rid our minds of the antiquated notion that consciousnesses are some sort of discrete entities which can be isolated in psychical preserving jars and neatly labelled L and F and J and E.

Once our thought is freed from this kind of limitation, it is easy enough to imagine the temporary formation of a corporate or composite personality or consciousness in which each member affects the others though each is uppermost when in control of the medium. On such lines as these, I find, everything falls readily into place; the dominance of J—a man of strong character in life; the affinity with E, his daughter and for many years his close

companion ; the consistent least similarity of F to the others when word-selection is involved ; the influence of the strong-minded and interested J on the normal but always sensitive L ; and the negligible effect of the less interested F in the same circumstances—all fit together into a coherent pattern as neatly as the pieces of a well made puzzle.

35

None the less, although what I have just said seems to me scarcely to be denied. I want to make it perfectly clear that nothing whatever in the facts I have presented entitles anyone to claim—as some enthusiasts are sure to do, however plainly I warn them—that this work “proves human survival” or even “demonstrates the existence of discarnate entities.” Let it be clearly understood that it does neither the one nor the other, and any claim that it does will constitute a grossly unwarrantable extrapolation from the facts.

It is perfectly true that the facts are easier to explain if we make certain tremendous assumptions of a spiritistic nature ; but this does not constitute proof. It may be merely an indication of our own psychological ignorance, just as at one time it was found easiest to explain simple astronomical occurrences by supposing the sun to be swallowed by a monster every night and regurgitated every morning.

Discarnate consciousnesses may exist : I think that they do. Human survival of death may be a fact in Nature : I think that it is. It may even become strictly demonstrable : I believe that it will. But however true these propositions may be, not one of them follows from the facts I have described in this paper.

All that I claim that these facts show is, first, that the Word Association test—properly applied and its data properly analysed—affords a most powerful instrument for the study of the problems which confront us ; and, second, that in order to cope with the facts elicited we may well be obliged to revise somewhat extensively our theories concerning multiple personality.

And I want to insist that, despite the difficulties I have indicated, it is from this starting point of multiple personality—of which as psychologists we know a good deal—and not from that of discarnate entities—of which as psychologists we know nothing at all—that the study of these phenomena must be developed.

When, if ever, we succeed in obtaining significantly similar reactions from the same ostensible communicator through two different mediums, we may begin to talk about discarnate entities. And when, if ever, we obtain from a supposed communicator, through a medium, reactions significantly similar to those he gave

before his death, we may reasonably begin to talk about proving survival.

In the meantime we must suffer ourselves to be guided by the relentless Law of Parsimony, which bids us abhor superfluous hypotheses ; but we may well take for our motto the words of Terence :

Nil tam difficile est quin quaerendo investigari possit.

“ There is no limit to the powers of Research.”

PREPARATORY NOTE TO APPENDICES

THE objects of the following appendices are : First, to lighten the text, which would otherwise be unreadable ; Second, to afford detailed support to the statements made therein ; and, Third, to provide such information as to the method of procedure and the reasons for it as will enable others, if they so desire, to repeat the experiments in a manner resembling the original work sufficiently closely to afford valid confirmation of it (or the reverse) ; or at least to enable usefully comparable results to be obtained.

If, as is only too possible where so much detail is involved, I have failed in either of these last two objects, I shall be very pleased to remedy the defect to the best of my ability, either by publication or by correspondence ; especially if—as I greatly hope may be the case—others are moved to undertake parallel researches on these lines.

Questions of interpretation will best be decided by doing more work.

W. W. C.

ROTTERDAM, 17. iv. 34.

APPENDIX I

THE PSYCHOGALVANIC REFLEX APPARATUS

THE apparatus used for observing the Psychogalvanic reflexes in the Garrett-Uvani and Rudi-Olga investigations was built to specification by Research Engineers, Ltd., of Canonbury, who were also responsible for the detail design.

The circuit used was the ordinary Wheatstone's bridge (too well known to need illustration or description here), having one fixed arm of 1,000 ohms, one arm variable in the usual way from 0 to 9,999 ohms by three dials, and the third providing alternatives of 1,000 or 100 ohms—the latter being used, of course, for external resistances greater than 9,999 ohms and giving a range up to 99,990 ohms if desired.

The galvanometer was a Gambrell portable reflecting type (Type No. 10716) using a 100 ohm coil and having a sensitivity of 5 mm. on the scale per microampere. (*N.B.* These instruments are very compact, being self-contained and fitted with a translucent scale only some 6" or so from the mirror.)

Since, in this class of work, we are negligibly interested in the actual resistance of the subject, but only in the *relative* sizes of changes of resistance, the bridge was built up of "commercial" grade resistance units, costing only a shilling or two apiece, instead of using expensive precision resistances.

Dry batteries were fitted giving, by means of a two-way switch, a nominal three or six volts across the bridge. The actual voltage applied at any moment could be read by means of a simple voltmeter mounted on the operating panel. This precaution is important.

These batteries also supplied the galvanometer lamp and an additional small lamp for the use of the operator.

The electrode leads (of ordinary "flex") were arranged to plug into sockets in the side of the containing case.

The sensitivity control of the galvanometer consisted of a 1000 ohm resistance connected in parallel with it, one lead to the galvanometer traversing this resistance; thus sensitivity was a maximum when the galvanometer was shunted by the whole 1000 ohms.

The sensitivity indications on the control dial were, accordingly, correct in general only for the external resistance (4000 ohms) at which the bridge was calibrated for sensitivity. The following table shows the sensitivity factors for other external resistances.

TABLE I
SETTING ON CONTROL DIAL

External Resistance. (ohms)	3/4	1,2	1/3	1/5	1/8	1/10	1/20
1,000	1.7	2.6	3.8	6.3	9.5	11	20
2,000	1.5	2.3	3.4	5.6	8.7	10	20
4,000	1.33	2.0	3.33	5.0	8.0	10	20
7,000	1.33	1.7	2.6	4.4	7.1	9.0	20
9,000	1.33	1.6	2.5	4.2	6.9	8.7	20

etc.

etc.

These and similar factors were, of course, used in reducing the deflexions observed to standard form. See Appendix III, 20.

The following table shows the approximate changes in external resistance necessary to produce one mm. scale deflexion on the

galvanometer, using 3.0 volts on the bridge, for different external resistances :

TABLE II

External Resistance (ohms)	Change for 1 mm. deflexion
500	0.16
1,000	0.33
2,000	0.77
3,000	1.32
5,000	2.77
7,000	4.70
10,000	8.50

The whole apparatus was built, for portability, into a case measuring about 9" wide by 15" high by 10" deep. This was fitted with a recessed and detachable door, or lid, in which the hand electrodes, leads, etc., were housed, and by an ingenious arrangement due to Messrs. Research Engineers this door could not be closed for transport until the safety lever of the galvanometer was pushed over—a procedure which also automatically switched off the galvanometer lamp.

APPENDIX II

NOTES ON THE ANALYSIS OF VARIANCE OF WORD ASSOCIATION TEST OBSERVATIONS

1

The following notes are not a mathematical exposition of the subject, for which Professor Fisher's *Statistical Methods for Research Workers* and the original papers cited therein, should be consulted.

They are intended merely to show, in a purely empirical way, how the method is applied in practice to the particular problems here considered, so that anyone acquainted with simple arithmetic and possessed of a calculating machine will be able to use it in this class of work. I hope, however, that the elementary illustrations given will serve to indicate, roughly at least, the "why and wherefore" of the processes involved.

2

Suppose that we are dealing with any pair of personalities, X and Y, and that we have tested them with n words on n' occasions each.

In the illustrations which follow, I am supposing that n' is 5 and n is some small number. In practice, n' will often be 5 or 6, whereas n will usually be 50, 75, 100, or some other relatively large number.

We will start by writing down the observations in tabular form in the manner indicated below, keeping to letters for the moment instead of actual figures, for the sake of generality.

TABLE I

X							Y							
Wd.	I	II	III	IV	V	T	I'	II'	III'	IV'	V'	T'	Sum	Diffce.
1	a_1	a_2	a_3	a_4	a_5	A	a'_1	a'_2	a'_3	a'_4	a'_5	A'	A - A'	A - A'
2	b_1	b_2	b_3	b_4	b_5	B	b'_1	b'_2	b'_3	b'_4	b'_5	B'	B - B'	B - B'
3	c_1	c_2	c_3	c_4	c_5	C	c'_1	c'_2	c'_3	c'_4	c'_5	C'	C - C'	C - C'
4	d_1	d_2	d_3	d_4	d_5	D	d'_1	d'_2	d'_3	d'_4	d'_5	D'	D - D'	D - D'
5	e_1	e_2	e_3	e_4	e_5	E	e'_1	e'_2	e'_3	e'_4	e'_5	E'	E - E'	E - E'
6	f_1	f_2	f_3	f_4	f_5	F	f'_1	f'_2	f'_3	f'_4	f'_5	F'	F - F'	F - F'
7	g_1	g_2	g_3	g_4	g_5	G	g'_1	g'_2	g'_3	g'_4	g'_5	G'	G - G'	G - G'
			etc.		etc.				etc.		etc.			
Totals:	t_1	t_2	t_3	t_4	t_5	T	t'_1	t'_2	t'_3	t'_4	t'_5	T'		

It will be understood that the entries in any row have to do with the same *word*, the entries in any (pair of) column(s), such as III (and III') have to do with the same *occasion*, and the entries in any "block" have to do with the same *personality*.

Now if there were no error whatever, of any kind—physical, psychological or observational—and if all the words were of exactly equal significance on every occasion to each subject, then all the entries would have the same value. This is an absurd case, of course, but will serve as a starting point.

Next, suppose that the two personalities differ in the sole respect that one is, say, twice as "reactive" as the other; but that, again, there is no kind of error and that the words have all the same equal significance for both subjects. In this all the entries in either block will be the same, but all those in one will be double those in the other. This case does not particularly interest us and we will neglect it here.

Now, suppose that there is still no error, and that the personalities are identically similar; but that the different *words* have a varying significance (*ex hypothesi*, for both subjects alike). Then all the entries in any one row will be the same, but those in any column will vary. We might have the kind of thing illustrated in Table II, where I show four words only in order to save trouble.

TABLE II

X							Y							Sum	Diffee.
Wd.	I	II	III	IV	V	T	I'	II'	III'	IV'	V'	T'			
1	10	10	10	10	10	50	10	10	10	10	10	50	100	0	
2	19	19	19	19	19	95	19	19	19	19	19	95	190	0	
3	17	17	17	17	17	85	17	17	17	17	17	85	170	0	
4	12	12	12	12	12	60	12	12	12	12	12	60	120	0	
Totals :	58	58	58	58	58	290	58	58	58	58	58	290	580	0	

This is the idealised case of two identically similar personalities, free from error, not varying from occasion to occasion, but varying considerably in their reactions to different words. Notice that the entries under T and T' (which are the totals of their respective rows) vary a good deal, as do those under "Sum," which are obtained by adding T to T'; while those under "Difference," obtained by subtracting T' from T for each row, are all zero. The first fact indicates the varying effects of the different words, the second the similarity (identity) of the two personalities.

If, on the other hand, the two personalities were somewhat different (there being still no error and no variation from one occasion to another) we should get something like Table III :

TABLE III

Wd.	X						Y							
	I	II	III	IV	V	T	I'	II'	III'	IV'	V'	T'	Sum	Diffee.
1	10	10	10	10	10	50	12	12	12	12	12	60	110	-10
2	19	19	19	19	19	95	17	17	17	17	17	85	180	10
3	17	17	17	17	17	85	19	19	19	19	19	95	180	-10
4	12	12	12	12	12	60	10	10	10	10	10	50	110	10
Totals :	58	58	58	58	58	290	58	58	58	58	58	290	580	0

Notice here that the entries under "Difference" are not zero and vary through a range of 20 units.

It should not now be very difficult to realise that the *variation* of the entries under "sum" is a measure of the differing significance of the words for the two personalities taken together; while the *variation* of the entries under "difference" is due partly to this and partly to the extent to which the personalities differ with respect to the different words. This last is the quantity in which we are interested.

In order, therefore, to judge the extent to which the two personalities are similar we must compare the variation of the entries under "Sum" with that of the entries under "Difference"; because, if the former is large compared with the latter, we shall know that the differentness of the words, rather than that of the personalities, is mainly responsible for whatever variation is found in the "Difference" column.

Now, in statistical practice, for reasons which I shall not attempt to explain, variations of this kind are always measured by taking the *sum of the squares of the quantities concerned from their mean value* (or some derivative of this, such as its value divided by the number of quantities or the square root of this last—known as the "standard error" or "standard deviation").

The quickest way of calculating this quantity is to use the formula

$$S(x - \bar{x})^2 = Sx^2 - \bar{x}S(x)$$

where $S(x - \bar{x})^2$ stands for "the sum of the squares of the quantities from their mean," $S(x^2)$ for "the sum of the squares of the quantities" and $\bar{x}S(x)$ for "the sum of the quantities multiplied by their mean." (N.B. Since \bar{x} equals the sum of the quantities divided by

their number, it follows that the quickest and most accurate way of computing this quantity is to take the sum of the quantities, square it, and divide by the number of quantities.)

This procedure is used for calculating all "sums of squares." (*N.B.* The abbreviation "sum of squares" is habitually used for the expanded expression "sum of squares of differences from the mean.")

In order, then, to compare two personalities for similarity, we start by working the quantity $S(x - \bar{x})^2$ first for the "Sum" column and then for the "Difference" column.

In the infantile example given in Table III we have :

	SUMS		DIFFERENCES
$S(x)$ -	580	$S(x)$ -	0
$S(x^2)$ -	89,000	$S(x^2)$ -	400
$\bar{x}S(x)$ -	84,100	$\bar{x}S(x)$ -	0
	<hr style="width: 50%; margin: 0 auto;"/>		<hr style="width: 50%; margin: 0 auto;"/>
$S(x - \bar{x})^2$ -	4,900	$S(x - \bar{x})^2$ -	400

In order to reduce these values to a "standard form," so to speak, independent of the number of observations on which they are based, we must divide each by the number of contributing entries—in this case 10, because there are 5 entries in each of the two rows which go to build up any given Sum or Difference. In general, we divide by $2n'$, where n' is the number of occasions (sittings) involved.

Further, we must obtain a magnitude which is not at the mercy of the number of words used. To get this we do not divide by n , the number of words, but by $n - 1$, because there are only $n - 1$ independent comparisons which can be made between words.

The result of the first process is to give us *the* "sum of squares" for the entries considered, and of the second to give us the "Mean Square."

Thus, by dividing by 10 in the above case, we obtain 490 and 40 for the "sums of squares" for the Sums and Differences respectively; while a further division by 3 (*N.B.* There are only four words, so that $n - 1$ is 3) gives us 163.3333 and 13.3333 respectively.

Now the first of these, as already indicated, depends on the varying effect produced by the differing significance of the *words*, and we may therefore speak of it as the Sum of Squares for Words, denoting it by W , and the corresponding Mean Square by $MS.W$. The second depends on the differential effect of words on the personalities, so that we may call it the Sum of Squares for Word-cum-Personality, denoting it by WP ; the Mean Square will similarly be $MS.WP$.

The next step is to divide MS.W by MS.WP, which gives, in this case 12.25.

For mathematical reasons given in Professor Fisher's book we next look up the natural logarithm (the logarithm to base e) of this and halve it. Thus :

$$\begin{array}{r} \text{Log}_e \quad 12.25 \qquad \qquad 2.5055 \\ \frac{1}{2} \quad ,, \qquad \qquad \qquad 1.25275 \end{array}$$

This quantity is known as z and is the measure of the similarity between the two personalities.

But we now want to know how much it is worth, and inasmuch as in this case it is based on only four words we shall not expect it to be worth very much, although "as z 's go," so to speak, it is a high value.

Now it can be shown (*loc. cit.*) that the standard error of such a z is equal to the square root of the reciprocal of the number of independent comparisons (technically known as "degrees of freedom") which can be made between the quantities from which it is derived ; that is to say, in this kind of case, between the numbers of words. As there are only four words in this case the standard error will be the square root of $1/3$ which is .5774 very nearly.

We accordingly divide z by this standard error (commonly known as σ_z) and look up the value of P corresponding to it in a table giving the deviation of the normal distribution¹ in terms of the standard deviation (*e.g.*, *loc. cit.*, Table I).

In this case we have :

$$z/\sigma_z = 2.1695.$$

Reference to the table shows us that the corresponding value of P is almost exactly .03. But this must be halved, because it only represents the chance of getting a z of this magnitude by accident, whereas we want to know the chance of getting one not only of this magnitude but also of this sign ; for there is nothing in the mathematics to prevent z being negative.

So we end up with

$$z = 1.25275, \quad P = .015.$$

We may interpret this result as meaning that there is a strong similarity between the two personalities, which is fairly—but not overwhelmingly—reliable, for there are about 15 chances in 1000 of its being accidental.

As an alternative to the last part of the procedure, we may use Professor Fisher's Tables, which give the values of z , for various numbers of degrees of freedom for the two Mean Squares concerned at the two points $P \cdot 05$ and $P \cdot 01$. (*N.B.* It is a more or less arbitrary

¹ *N.B.* z is normally distributed.

convention that anything yielding a chance of .01 or less is considered definitely significant, anything between .01 and .05 as semi-significant, and anything greater than .05 as non-significant.)

In actual word-association test practice, of course, where the numbers of degrees of freedom for W and WP are always equal, it is quicker (and more accurate) not to work the mean squares but to divide one $2n'W$ by $2n'WP$ straight away. In the example we should divide 4,900 by 400 without reducing them to 163.3333 and 13.3333 first.

In practice, also, where 50, 75 or more words are used, the figures are very much heavier. The following is an example from actual work.

Case : L(P) and F. 75 words, 6 sittings.

W		WP	
S(x)	87,074	S(x)	562
S(x ²)	103,645,700	S(x)	1,480,780
$\bar{x}S(x)$	<u>101,901,753</u>	xS(x)	<u>4,211</u>
S(x - \bar{x}) ²	2,553,947	S(x - x)	1,476,569
Do/12	212,829 = W	Do/12	123,047 = WP
		Do/74	1,662.8 = MS.WP
W/WP	1.7297		
z	.2740		
σ_z	.11625		
z/ σ_z	2.35704.	P is more than .01 and less than .02.	

To recapitulate : In order to test two personalities for similarity, proceed as follows :

Write down the observations in rows and columns as shown in the illustrative tables above. Add the entries in each row for each personality, thus obtaining the " Personality Word Totals " given under T and T'. Sum these by words for the two personalities, obtaining the figures under " Sum." Subtract the right-hand word total (say) from the left, writing down the (algebraic) difference under " Difference ". Check all this by the ordinary methods of summing columns, etc.

Work the sum of the squares of the differences from their mean of the entries under " Sum ". Do the same for entries under " Difference " (algebraic mean, of course).

Divide the result for Sums by that for Differences (this is short-circuiting the process of reducing to Mean Squares). Find the natural logarithm of this quotient. Halve it. This gives z, the measure of similarity you want.

Subtract one from the number of words used. Find the reciprocal of the resulting number. Find the square root of this. Divide z by

it. Look up the value of P in a table of normal deviates. Halve this. The result is the chance that the observed similarity is accidental.

The process of testing for difference is not so easy as the foregoing test for similarity; nor is it so easy to describe or explain.

We will start by saying, very colloquially, that "Error is the extent to which the same things don't happen under (apparently) the same conditions", and will add that, if there is *no* error (as in the idealised examples given above) any difference observed is absolute and cannot be attributed to chance, accident or error at all, because there is no error to which to attribute it.

In these examples, it will be noted, each personality is shown as giving precisely the same reaction to a given word on every occasion. In practice, this is far from being the case, however carefully we try to ensure that experimental conditions are the same. The result is that, instead of such figures as

19 19 19 19 19 and 17 17 17 17 17

for personalities X and Y respectively, we get something like

19 23 15 18 20 and 16 18 15 21 15

The point to notice here is that although each entry for X in the first case differs from the corresponding entry for Y, this difference is constant, namely 2; whereas in the second case the differences vary being, in fact,

3 5 0 -3 5

In the first case, the *constant* difference is due to the fact that the word has a different exciting power on the two personalities; in the second, the varying difference is due to the distortion of this effect (if any) by error and whatever systematic difference of effect may be produced on the personalities by the fact that the occasion, as such, is different for the several pairs of observations.

It is, therefore, the magnitude of this variation in difference between corresponding pairs of entries that interests us when we are trying to determine whether the difference between personalities (indicated by the magnitude of the Mean Square for WP calculated above) is attributable to Error or not.

In principle, then, the first step of the procedure is to take the corresponding pairs of entries for each word, subtract (algebraically) the one from the other, as just shown, and calculate the sum of squares of deviations from their mean of these differences. This sum of squares must then be divided by 2, because two entries contribute to each variate—just as in calculating W and WP we divided by $2n'$. Thus in the case just indicated, the (algebraic)

mean of the differences obtained by subtraction is 2 and the sum of the squares of their differences from it is 48, which divided by 2 gives 24. This is the contribution, so to speak, made by this word to the total variation due to change of occasion and personality together with error.

We must therefore repeat the process with each word, when, by summing all these contributions we shall obtain the total variation due to these causes.

In practice this is intolerably laborious and very liable to error; the following is the best procedure I have been able to devise.

Taking the analogue of Table III (in which, of course, the entries for each word for either personality will, in general, vary), write down the algebraic differences between corresponding pairs for the various occasions by inspection as we did in the elementary example just worked. We thus obtain figures such as these (taken from actual work):

Sitting:	I	II	III	IV	V	Total
Word						
1	-92	-53	-9	-77	59	-172
2	26	-82	38	-41	-19	-78
3	111	0	-65	-62	-41	-57
4	-43	55	3	69	61	+145
	etc.	etc.	etc.	etc.	etc.	etc.

The totals on the right should agree with the corresponding entries in the "Difference" column (see Table III) which were used in computing WP, and this fact affords a most valuable check both on the subtractions by inspection here involved and the original summing of the data for W and WP working.

When these figures have been written down for all words, find the sum of the squares of the lot. This is best done by columns direct on the machine, either using it to do the squaring for one (in the case of heavy figures) or working by a mixture of tables and memory for smaller figures.

From the sum of squares thus obtained subtract the value of $S(x^2)$ which was used in computing WP, *divided by the number of occasions (sittings) used (n')*. Divide the result by 2. Application of elementary algebra will show that this gives precisely the same answer as the formal ritual previously described where the sum of squares of differences from the mean is computed for each word separately. It is incomparably quicker and less liable to mistakes.

We thus obtain a new $S(x - \bar{x})^2$ to the treatment of which we will return after a short digression.

As already indicated, the variations we have just been considering include any systematic variations due to the differential effects of occasion on personality. For example, a warmer day might stimulate one personality with respect to all words and depress the other. The variation thus introduced must be subtracted from the quantity which we have just found before we can decide how much of the latter consists of true Error of the kind that interests us.

The occasion-cum-personality contribution in question may be denoted by OP and calculated as follows :

Sum the columns headed I, II, III, etc. (that is to say, the "occasion columns") for each personality and subtract the corresponding totals one from another, just as we did for the corresponding pairs of entries in the case of the individual words. Thus we might have :

	X					Y				
Occasion :	I	II	III	IV	V	I	II	III	IV	V
Total :	257	129	365	238	164	264	132	368	246	180

and the differences between corresponding totals would be

$$-7 \quad -3 \quad -3 \quad -8 \quad -16.$$

Work the sum of the squares of the differences of these from their mean, which here comes to 113.2. But to the formation of each difference there contribute $2n$ entries, where n is the number of words as before. Consequently we must divide by $2n$ in order to get the proper value of OP. If, as was often the case in my work, we are using 75 words, we divide by 150 and in this case we should obtain for OP the fantastically low value of $113.2/150$ or .7547.

This quantity, then, whatever it may be, represents the contribution made by occasion-cum-personality to the total discrepancies between corresponding pairs of entries. We therefore subtract it from the $S(x - \bar{x})$ last calculated (from the differences of corresponding pairs), thus obtaining a new quantity, called OWP (occasion-cum-word-cum-personality) which is Error for our purpose.

But in order to obtain from this a quantity comparable with WP (which, it will be remembered, is our index to the extent to which the personalities differ) we must again divide by the appropriate number of degrees of freedom so as to obtain a Mean Square for OWP for comparison with MS.WP.

This number is given by $(n - 1)(n' - 1)$ where n and n' , as previously, are the numbers of words and occasions respectively. Thus, if there are 75 words and 6 sittings we divide by 370. This division gives MS.OWP. We divide MS.WP by it, just as we divided (in the strict procedure) MS.W by MS.WP, and derive a z and a P in precisely the same manner as before.

The following is an example from actual work :

Case : F and J (Reaction Times) : 75 words ; six sittings.

WP	OWP
$S(x) = -525$	$S(x^2) = 1,581,419$
$S(x^2) = 2,273,241$ divided by six	$= 378,873.5$
$\bar{x}S(x) = 3,675$	$S(x - \bar{x})^2 = 1,202,545.5$
$S(x - \bar{x})^2 = 2,269,566$	Do/2 = 601,272.7
Do/12 = 189,130 = WP	OP = 226.3
Do/74 = 2,555.8 = MS.WP	OWP = 601,046.4
	Do/370 = 1,624.4
	MS.WP/MS.OWP = 1.5734
	$z = .2266$
	$\sigma_z = .09$ very nearly.
	$z/\sigma_z = 2.5188$

Whence P is found to be .005, very nearly.

(Note. In these cases, where n is fairly large, σ_z may be taken as

$$\sqrt{\frac{n'}{2(n-1)(n'-1)}}$$

The foregoing, though highly imperfect from a mathematician's point of view, will, I trust, be sufficient to enable the comparative amateur to perform the actual calculations necessary to obtain valid results from his observations, and to form some idea of what he is doing and why.

One word of warning may be added. Unless it is actively desired to qualify for the sweet security of a mad-house, it is futile to attempt such computations on any considerable scale without a *suitable* calculating machine.

I did about two-thirds or more of my own work on a hand-driven machine and would never do so again. An electrically driven model is virtually essential.

It is hopelessly slow and very error-liaible to look up large squares in tables and add them on the machine ; the big summings of squares for W (and usually for WP also) should be done direct on the machine, which must accordingly accumulate products—as practically all do. The machine should also “accumulate multipliers”, so that at the end of such a summation of squares the sum of one set of factors is available as a check against omissions. This, however, necessitates double dials (as in the Monroe KA 163 or MA. 3. 173) because without them the individual squarings cannot be safely done. Best of all is a machine such as the Rheinmetal SA. IIe with full automatic multiplication in addition to accumulation of multipliers. This greatly reduces the strain on the operator and correspondingly minimizes liability to error and necessity of repeated checking.

APPENDIX III

TECHNICAL NOTES

N.B. It would be almost miraculous if, in the single-handed computation of so formidable a mass of material, offering many thousands of opportunities for error, no minor slips should remain undetected.

I have, however, applied the ordinary checks to all calculations and special checks to those of critical importance; and I am confident that, although a complete and faultless re-working might possibly lead to alterations in, say, the second or third place of decimals in some of the values given, no substantial change in the results would be found necessary.

W. W. C.

1. This is not to say that no work has been done on the psychological characteristics of trance personalities; Mrs Sidgwick's monumental study of the psychology of Mrs Piper's Trance (*Proc. S.P.R.*, xxviii.) is an outstanding example, while the particular observation that "Phinuit", though claiming to be a French doctor, had a negligible knowledge of either French or Medicine, belongs to the same category. But no such work, that I know of, contains the all-important *quantitative* element.

2. The proper word is clearly "psychometrical"; but this has unfortunately long been used to refer to a class of paragnostical phenomena into which no metrical element enters. It is for consideration whether it would not be well boldly to adopt the term for the kind of method here discussed, which does consist in obtaining metrical attributes of the "psyche", and to find a more appropriate term for the variety of paragnosis concerned.

3. Cf. Jung, *Studies in Word Association*, 1918.

4. "... subconscious or unconscious..." I use the words interchangeably except that "unconscious" indicates a lesser accessibility and a greater difficulty of exhumation. There is no essential difference of kind, but only of degree, between conscious, subconscious and unconscious; the distinctions, though convenient, are arbitrary; there are no hard and fast water-tight compartments. Or, if there are, it is futile to talk about their contents, because—*by definition*—we can never tell what is in them.

5. *Journal of Abnormal Psychology*, 1908, vol. iii. pp. 114 ff.

6. *Proc. S.P.R.*, vol. xxxi. p. 401. Cf. also my *Measurement of Emotion*, chapter v.

7. "... apparent ..." An electromotive force may also be generated, such as could be detected by a sufficiently sensitive galvanometer without the aid of a Wheatstone's bridge or any E.M.F. applied externally. This may reinforce or diminish the effect of the resistance change proper, but we cannot tell this by looking at the galvanometer. Hence the use of the word "apparent".

8. We may take the deflexions as being proportional to the voltage on the bridge, for any given change of resistance, and correct by dividing the deflexion observed by the quotient of the voltage used by that at which the bridge was calibrated—in this case 3 volts. The sensitivity correction depends on the actual construction of the apparatus; see Appendix I.

9. Actually the same kind of consideration does apply to some extent to psychogalvanic reflexes.

This is not due, as might at first be supposed, to the deflexion being an "unknown function" of the magnitude of psychical disturbance in which we are interested; for it does not seem possible to define the extent of the disturbance except in terms of the measurements we make on it—any more than we can define physical objects in any terms other than those of the measurements which, directly or indirectly, we make on them.

But introspection shows that the psychogalvanic reflex may be misleadingly increased by a process best described as *regeneration*, e.g. the subject may first be embarrassed (say) by the word and then much more so by his own embarrassment. This latter effect may be determined by local circumstances, such as the person of the experimenter, and is not therefore necessarily a measure of the intrinsic excitingness of the word.

Special research on the optimum "cut off point" for psychogalvanic reflexes is accordingly needed; but fortunately no significant *difference* dependent on them arises in the course of this work.

10. The "median" is that value of the variate above and below which values occur in equal numbers. It is identical with the mean only in symmetrical distributions. Thus in the distribution 14, 11, 8, 12, 10, 9, 13, the value 11 is both mean and median; while for the series 4, 12, 11, 12, 14, 9, 13, 10, 6, the median is 11, but the mean 10.101 . . . etc. The median is also known as the "probable mean".

11. Compare my *Measurement of Emotion*, p. 64, where I conclude that disturbance in reproduction "is not only a complex-indicator, . . . but *the* complex-indicator *par excellence*".

12. "... preferences for different initial letters ... for the response words ..."

In the case of Garrett and Uvani I counted the number of replies beginning with A, B, C, etc., for each. I also counted the corresponding frequencies for the first and last fifty words in each case. The correlation between the frequencies for the whole of G with the whole of U was .7309; and for G (1-50) with G (51-100) it was .7158. But this high correlation between G and U is largely due to the restrictions imposed by the numbers of words available beginning with the different letters. I accordingly took the average frequencies of these as indicated by three different dictionaries and obtained correlations with these dictionary values of .6434 for G (1-50), .7764 for G (51-100), .6807 for G and .8467 for U. Eliminating the "dictionary effect" by the usual method of partial correlation, I obtained a figure of .4482 for G (1-50) and G (51-100) and one of .3955 for all G with all U. The former represents the consistency of Garrett in this matter, apart from the influence of the language, and the latter the agreement of G with U. Neither is highly significant, while the difference between them is negligibly so. The test accordingly fails, but seems worthy of a mention on grounds of general interest.

13. It seems convenient to interpolate here a note of warning against a mistake to which some might be liable and a fallacy in which many frequently indulge.

First: Any test which does, in fact, significantly distinguish personalities, is *ipso facto* a valid test, and over-rides any number which fail to do so. The test of counting legs, for example, would seldom enable us to distinguish normal persons, though it might work for senile millipedes; and the same applies (with reservations) to the teeth of young healthy persons, as also to fingers and toes. But if these tests failed we should none the less accept the results given by comparing Bertillon measurements, supposing these to indicate difference.

Second: It is commonly but erroneously maintained that "no number of negative results can outweigh one positive result." This is not true. When I say that the chance of a phenomenon occurring accidentally is p , this is equivalent to saying that *if* pure chance alone is at work the phenomenon will occur, on the average, once in p trials. And it *may* be that we have, to speak colloquially, struck just that p th occasion at the first shot. Hence the importance of knowing the value of p ; for, if this is fairly large, such as .2 for example, our assurance that we have not done so is correspondingly slight.

14. (a) The list consisted of the following words :

1. Head.	26. Blue.	51. Frog.	76. Wait.
2. Green.	27. Lamp.	52. Try.	77. Cow.
3. Water.	28. Carry.	53. Hunger.	78. Waste.
4. Sing.	29. Bread.	54. White.	79. Luek.
5. Dead.	30. Rich.	55. Brown.	80. Horse.
6. Long.	31. Tree.	56. Speak.	81. Table.
7. Ship.	32. Jump.	57. Pencil.	82. Work.
8. Make.	33. Pity.	58. Sad.	83. Brother.
9. Window.	34. Yellow.	59. Plum.	84. Fast.
10. Friend.	35. Street.	60. Dog.	85. Purpose.
11. Cook.	36. Bury.	61. Home.	86. Chair.
12. Ask.	37. Salt.	62. Nasty.	87. Worry.
13. Cold.	38. Dress.	63. Glass.	88. Knife.
14. Stalk.	39. Justice.	64. Help.	89. Motor.
15. Pay.	40. Hat.	65. Wine.	90. Clean.
16. Village.	41. Paint.	66. Big.	91. Bag.
17. Pond.	42. Silly.	67. Carrot.	92. Choice.
18. Sick.	43. Book.	68. Give.	93. Bed.
19. Mountain.	44. Wild.	69. Doctor.	94. House.
20. Bring.	45. Finger.	70. Travel.	95. Coal.
21. Ink.	46. Month.	71. Flower.	96. Shut.
22. Angry.	47. Bird.	72. Beat.	97. Fire.
23. Needle.	48. Walk.	73. Box.	98. Evil.
24. Swim.	49. Paper.	74. Cold.	99. Hotel.
25. Go.	50. Wicked.	75. Apple.	100. Insult.

(b) Half a dozen preliminary words, or thereabouts, taken from an auxiliary list were given at the beginning of each test of each personality, and before starting on this main list, in order to make sure that everything was working properly and to give the subject a chance of "settling down". If this precaution is omitted the early words of the list are apt to give unduly large effects, notably as regards psychogalvanic reflexes. The mere fact of being tested renders the subject especially susceptible until he gets used to it.

It was presumably a failure to realise this fact and to allow for it that led Wechsler and Jones (*cf. Psychological Bulletin*, 1927, pp. 173-4), using eight of the most exciting and eight of the least exciting words taken from my own earlier results, to conclude that position in the list was of great importance. As the starting effect, so to speak, may well continue for just about six or eight words, it is not surprising that working with a list of only 16 words, without preliminaries, they should have reached this conclusion.

In these early experiments I used only five preliminary words (*cf. The Measurement of Emotion*, p. 31, which Wechsler and Jones presumably have not read); but that these were enough for my contemporary purpose is shown by the fact that if the order in which the words were given is correlated with the order of magnitude of the mean corrected deflexion for my whole 50 subjects (which Wechsler and Jones clearly did not trouble to do), we obtain a coefficient of only $-.08$, which shows that order had, on the whole, an utterly insignificant effect on the results obtained.

The value of the criticism may be estimated from the following flat contradiction (*Psy. Bull.*, *loc. cit.*):

“The difference between earlier and later position is much *greater* in the case of emotionally toned (critical) than in the non-emotionally toned (non-critical) words. For example, when the word ‘table’ occurs at the head of the list, the deflexion it elicits is on the average eight times that when placed at the end; but the word ‘kiss’ has a deflexion *only* 20 per cent. greater when placed at the beginning than when placed at the end.” (My italics. W. W. C.)

Or can it be that American kisses are even less exciting than American tables?

(c) The dates of the sittings were: 24. vii., 31. vii., 14. viii., 21. viii., 28. viii. and 2. ix., 1933.

15. In order to localise, so far as possible, the effect of any breakdowns in the tranee or the experimental technique, the words were handled in blocks of 25 as above and the changes of order were made within the block. Six orders determined by a simple plan were used. No personality received the words in the same order twice; but each received them in each of the orders once. The same order was not used twice at a double sitting.

The object of this varying of the order was primarily to prevent subjects getting warning from one word of which was coming next, but also to minimise the effects of “perseveration” where the effect produced by one word “carries over” to the word or words following.

16. I conducted, however, a short *ad hoc* study of these negative deflexions in connection with a point which arose in preparing my review (*q.v.*) of Mr Hereward Carrington’s work on this medium. I should like to emphasise here that the occurrence of these numerous negative deflexions is no kind of reflection on Mrs Garrett’s *bona fides*.

17. The figures given in the following table, showing maximum,

minimum and mean resistances, deflexions and actual ohms changes, etc., may be of interest to other workers :

TABLE I

MISCELLANEOUS DATA FOR GARRETT-UVANI PSYCHOGALVANIC
REFLEXES

GARRETT

Sitting.	I.	II.	III.	IV.	V.	Means of values given.
Initial resistance (ohms) - -	9,219	21,190	7,300	10,986	10,565	11,852
Final resistance -	8,672	28,294	15,159	14,437	12,874	15,887
Maximum resistance	9,702	28,862	15,159	15,924	13,099	13,364
Minimum resistance (unstimulated) -	7,394	21,099	7,300	9,392	6,410	10,319
Mean resistance -	9,056	25,994	12,419	13,616	10,041	14,225
Maximum change (ohms) - -	986	3,053	558	878	2,695	1,634
Maximum change as % of mean resist- ance - -	10.9	11.7	4.7	6.5	27	12.2
Mean change (ohms)	133	452	69	80	166	180
Mean change as % of mean resistance -	1.5	1.7	0.7	0.6	1.7	1.24

UVANI

Initial resistance (ohms) - -	3,900	14,016	16,063	10,510	—	8,897
Final resistance -	3,612	12,010	13,970	17,670	—	9,452
Maximum resistance	4,032	14,016	17,023	19,050	—	10,824
Minimum resistance	3,199	11,833	13,970	10,510	—	7,902
Mean resistance -	3,591	12,700	16,281	15,065	—	9,527
Maximum change (ohms) - -	338	322	103	2,510	—	655
Maximum change as % of mean re- sistance - -	9.4	2.5	0.6	16.6	—	5.8
Mean change (ohms)	126	114	14	183	—	87
Mean change as % of mean resistance	3.5	0.9	0.1	1.2	—	1.14

(N.B. No worth-while reflexes were obtained from Uvani on the fifth occasion, or from either personality on the sixth. Uvani was always tested after Garrett.)

18. The following table shows the coefficients of correlation actually obtained by the various procedures.

The columns headed GG and UU show the coefficients resulting from working the means of odd-numbered with even-numbered sittings for Garrett and Uvani respectively, and are thus measures of their self-consistency. Those headed GU give the results of working the means for all Garrett sittings against all Uvani sittings and therefore give a measure of their agreement with each other.

TABLE II
COEFFICIENTS OF CORRELATION : GARRETT-UVANI
PSYCHOGALVANIC REFLEXES

	All words included.			FROG and DOG eliminated.		
	GG	UU	GU	GG	UU	GU
Actual ohms change (uncorrected)						
Usual method -	.1784	.0923	.0022	.0593	.2406	-.0730
Percentages of means (fully corrected)						
Usual method -	.4198	.1502	-.0279	.0604	.3260	-.1206
Percentages of means (fully corrected)						
Method of Ranks	.1772	.4430	-.0160	.1428*	—	—

(* FROG only eliminated in this case.)

(N.B. It will be noticed that although the consistencies of the two personalities fluctuate violently we never get anything approaching a noteworthy agreement between them.)

19. To test the difference between *any two* of the above coefficients for significance we may proceed as follows (*cf.* Fisher, *Statistical Methods for Research Workers*, p. 182).

Convert the coefficients into the quantities called “*z*”s by the formula

$$z = \frac{1}{2} \{ \log_e(1+r) - \log_e(1-r) \}$$

where *r* is the coefficient of correlation concerned and *e* the base of natural logarithms, as usual. Find the standard error of each *z* by taking the reciprocal of three less than the number of pairs of

variates concerned. Subtract the lesser from the greater z and find the standard error of this difference by summing the two reciprocals just obtained and taking the square root of this sum. Divide the difference of the z 's by this and look up the chance of getting such a result by accident in a suitable table, *e.g. loc. cit.*, Table I.

Example : Take GG and GU for the second entry in the left hand half of Table II above.

Applying the above formula we get

	r	z	$n - 3$	Reciprocal
GG	.4198	.44735	76	.01316
GU	-.0279	-.02785	92	.01087
Difference		.47520	Sum	.02403
			Sq. Root	.155
			Diff Sq. Root	3.065

Whence the chance of this being accidental is found to be less than one in a hundred though greater than one in a thousand. We should take this to be significant and conclude that G and U are "different" were it not for the very low self-consistency of Uvani, *i.e.* the low value of the coefficient for UU.

When we take the mean value of the z 's derived from GG and UU as a measure of the total self-consistency, so to speak, and a corresponding value for the standard error of this, we obtain a probability of accidental occurrence of just about 1 in fifty. It needs a certain amount of optimism to regard this as significant.

The figures obtained by the method of ranks gives much better results, namely a chance of no more than about 1 in 600. But the method of ranks is itself somewhat discredited and the process of "stepping up" from the value ρ first calculated to the corresponding r involves assumptions which may not be warranted here.

Consequently, though I have little personal doubt that the result is genuine, I do not care to claim it as such.

20. Correction of Garrett-Uvani Psychogalvanic reflex data :

The data actually entered on the working sheets were the scale readings of the galvanometer at the beginning and end of each deflexion, together with the relevant balancing resistance, sensitivity adjustment and the voltage actually applied to the bridge.

The first step in reducing these data to the form required was the construction of a tripartite Reduction Factor, consisting of (a) the sensitivity factor, (b) the voltage factor and (c) the resistance factor.

The first of these was obtained from a table (see Appendix I) giving the true change in sensitivity at any setting of the sensitivity

control for a given external resistance. The second was the quotient of the voltage at which the bridge was calibrated (3.0 v.) by that actually in use. The third was derived from calibration curves showing the change in resistance necessary to produce a deflexion of 100 scale divisions, at full sensitivity and with three volts on the bridge, for any given external resistance.

The next step was to multiply each of the two scale readings just mentioned by this composite Reduction Factor, thus obtaining the amounts (in ohms) by which the external resistance had changed, from the value to which the bridge was balanced, at the beginning and end respectively of the reaction.

The difference between these quantities is evidently the actual ohms change constituting the reaction.

The resistance to which the bridge was balanced, less the first of the two ohmic changes just mentioned, is the true external resistance at the beginning of the reaction, and I expressed, in all cases, the actual-ohms-change calculated as a percentage of this "initial resistance"; for I argued that a change of, say, 10 ohms in 10,000 could not reasonably be regarded as equivalent to one of 10 ohms in 5,000 or 20,000.

The percentages thus obtained were further corrected for the possible effects of fatigue, or like secular changes within the period of the sitting, as follows :

Assuming the change to be linear in time, I grouped the percentages, for each sitting, by tens of words (1 to 10, 11 to 20, 21 to 30, etc.) and calculated (by the usual method of least squares) the equation to the straight line most nearly passing the points whose coördinates were the means of these groups of percentages and the numbers 5.5, 15.5, 25.5, etc.¹

From this equation I derived a new factor for each percentage change, such that, dividing by it, I turned each into a percentage of the mean (percentage) change for the sitting concerned.

These were the figures used in the correlations.

It appears, however, in the light of further study, that much of this very laborious technique is redundant, although I think it the nearest practicable approach to a truly rigid treatment.

In the first place, the use of the "resistance factor" (*c*) mentioned above is largely nullified by the subsequent expression of the actual ohms change as a percentage of the "initial resistance"; for the first process consists in multiplying by a quantity which, for high resistances at any rate, is very nearly a linear function of the initial resistance, and the second consists of dividing by the initial resistance itself; so that the *relative* values of the figures ultimately obtained

¹ *N.B.* This saves much labour, but introduces a small error, negligible in all ordinary cases.

for the various words (in which alone we are interested in this work) are little altered from those of the deflexions corrected for voltage and sensitivity only.

Secondly, the use of the "initial resistance" instead of the "balanced resistance" is a minor refinement which could be omitted without serious results.

Thirdly and most importantly, in comparing two personalities, it is clear that whatever error is allowed to remain by omitting these correctional procedures applies to both; so that the final error affecting the results is no more than differential.

Given reasonably good raw material, I should only use this very elaborate technique in cases where (a) the issue was of the first importance and (b) the less elaborate treatment yielded ambiguous results; or (c) where it was desired to obtain the nicest quantitative information possible.

The same applies to the correction for fatigue, etc., which seems seldom if ever necessary. Its desirability in any particular case can be judged by inspection or by some simple test such as that indicated in Note 29 below.

21. The reason is as follows: Despite what I said above (Note 9) about the impossibility of defining the disturbance in which we are interested except in terms of the measurements we make, it seems none the less true that it is absurd to allow a whole set of reactions to be dominated by one or two "freak" words. FROG, for example, in the case of Garrett, gave a change of 1,084% (10.84 times) the mean change on one occasion and 1,018% on another, while DOG in the case of Uvani went as high as 1,178%. The method of ranks, whatever its limitations, does at least obviate absurdities of this kind; and I think it worthy of consideration for very erratic material although, of course, it wastes whatever niceties of gradation there may be. The point, however, has no importance for the results described in this paper.

22. See note above. I am inclined to think that these two words have a genuine significance for the two personalities respectively, but that the effects produced by them have been greatly exaggerated. This may be due to the regenerative effect mentioned in Note 9, but also—I am inclined to suspect—to the personalities half-wittingly "playing up" to what they knew was expected of them. (N.B. The word FROG occurred in Mr Hereward Carrington's list and the marked effect received comment.) It is not so easy as it sounds to be a good test subject and so to adjust one's mind that each word comes freshly and without reference to previous tests.

23. The results are given in Table III below. For notation see Appendix II.

TABLE III

GARRETT-UVANI REACTION TIMES BY ANALYSIS OF VARIANCE

SIMILARITIES

	W/WP	z	P
1. Uncorrected data - -	1.1961	.0896	About .20
2. $2\bar{x}$ written for all greater than $2\bar{x}$ - - -	1.1729	.0798	„ .24
3. “One or zero”, using mean - - -	1.0791	.0382	„ .36
4. “One or zero”, using median - - -	1.0552	.0269	„ .40

DIFFERENCES

	WP/OWP	z	P
1. Uncorrected data - -	2.5481	.4677	Less than 10^{-7} More than 10^{-8}
2. $2\bar{x}$ written for all greater than $2\bar{x}$ - - -	2.3159	.4293	Less than 10^{-6} More than 10^{-7}
3. “One or zero”, using mean - - -	1.8916	.3187	Less than 10^{-3}
4. “One or zero”, using median - - -	2.3274	.4224	Nearly 10^{-4} Less than 10^{-6} More than 10^{-7}

(N.B. In order to equalise sittings the “uncorrected data” were written throughout as percentages of the mean reaction time of the sitting to which they belonged.)

24. The disturbed reproductions for Garrett and Uvani were distributed among the different sittings as indicated below. The first column gives the total number of failures, while the second shows this as a percentage of the number of stimuli to which a useful reply was given in the first instance; *i.e.* replies such as “No”, “I don’t know”, etc., are excluded, also the inarticulate noises sometimes emitted by Uvani.

Sitting.	GARRETT		UVANI	
	No.	Percentage.	No.	Percentage.
I	53	62.4	31	24.3
II	27	30.3	35	48.6
III	36	37.9	23	31.1
IV	28	28.6	37	53.6
V	20	20.8	29	38.7
VI	17	20.6	31	41.3
Totals -	181		186	

The figures for the analysis of the 26 words are :

WP/OWP	z	P
2.0923	.3692	About .008 (say 1 in 120)

25. Given the number of disturbances in reproduction for the two personalities at a given sitting, it is easy to work out the expectation of coincidences between these if chance alone is at work. The number of actual coincidences is counted and compared with the expectation. The chance that the difference between the expected and observed number is fortuitous is calculated by the usual method. (Cf. Fisher, *loc. cit.*, p. 80 *et seq.*)

The same procedure can be applied to two successive sittings of the same personality. As a result of five such tests of each kind I get a mean value for χ^2 (*cf. loc. cit.*) of 0.355 for comparisons of G with U and the chance of this being accidental is between .5 and .7. For comparisons of G with G, and U with U, at different sittings, I get a mean value of 11.613 with a corresponding chance of about .0005 (by extrapolation from the table given). This result, supporting that obtained by analysis of the 26 unbroken words, appears to justify the statement that the chance of the difference between G and U being accidental is probably much less than 1 in 100.

26. This figure (though of no real interest—*vide supra*, p. 187) is justifiable even if the contention of Note 25 is refuted, because the most conservative reasonable estimate from the reaction times is less than 1 in 1,000,000, and Note 24 shows a chance of 1 in 120, while the reflexes are certainly good for one of 1 in 10.

27. The dates of the sittings were : 19. x, 15. xi, 22. xi, 6. xii, 13. xii. 33, and 7. ii. 34.

28a. The list given was as follows :

1. Buch.*	26. Kuss.	51. drohen.	76. Wirtschaft.
2. Hunger.*	27. Türe.	52. Licht.	77. Spiegel.
3. weiss.*	28. wählen.*	53. reich.*	78. strafen.
4. aufpassen.	29. still.	54. Baum.*	79. Knochen.
5. trüb.*	30. Spott.	55. Berg.*	80. Tot.*
6. treffen.*	31. schlafen.	56. spielen.	81. lieben.
7. Gesetz.	32. Geist.	57. Schwindel.	82. Wechsl.
8. lieb.	33. Kopf.*	58. neu.	83. Pflicht.
9. Glas.*	34. grün.*	59. Wand.	84. Schlange.
10. streiten.	35. Wasser.*	60. dumm.*	85. Feuer.*
11. Ziege.	36. Engel.	61. Zahn.	86. Messer.*
12. gross.*	37. lang.*	62. richtig.*	87. Arbeit.*
13. malen.*	38. Schiff.*	63. stinken.	88. Automobil.*
14. Teil.	39. freundlich.*	64. sterben.	89. Apfel.*
15. Blume.*	40. tragen.*	65. Monat.*	90. Arzt.*
16. hell.	41. See.	66. Hund.*	91. Pflaume.*
17. Vater.	42. krank.*	67. reden.*	92. Frau.
18. waschen.	43. stolz.	68. faul.	93. schwarz.
19. fremd.	44. kochen.*	69. lachen.	94. Strasse.*
20. Glück.	45. Tinte.	70. Blut.	95. Lampe.*
21. erzählen.	46. böse.*	71. Vorsicht.	96. Fenster.*
22. eng.	47. Nadel.*	72. vornehm.	97. Dorf.*
23. Bruder.*	48. Reise.*	73. drehen.	98. Gebirge.
24. falsch.	49. blau.*	74. Vertrauen.	99. bezahlen.*
25. Angst.	50. Brot.*	75. bringen.*	100. kochen.

This list also was given in differing orders on different occasions.

It will be noticed that by misadventure the word *kochen* appears twice. This does not affect the results obtained, since one of the sets of reactions obtained was spoiled by gaps, so that the word appears only once in the block of 56 unspoiled words on which the conclusions are based.

A total of 47 words (marked *) are exactly or closely paralleled in my list as used for Garrett-Uvani and the Leonard group (first series).

28b. The approximate figures in this connexion are :

TABLE IV

MISCELLANEOUS DATA FOR RUDI-OLGA PSYCHO GALVANIC
REFLEXES

RUDI

Sitting.	II.	III.	IV.	V.	VI.	Means of values given.
Initial resistance (ohms) - -	1,060	1,380	800	1,240	1,160	1,128
Final resistance -	1,200	1,330	940	1,260	1,110	1,168
Maximum resistance	1,200	1,380	940	1,280	1,160	1,192
Minimum resistance (unstimulated) -	1,030	1,160	800	1,330	1,060	1,054
Mean resistance -	1,132	1,245	894	1,242	1,114	1,125
Maximum change (ohms) - -	128	126	62	85	142	109
Maximum change as percentage of mean resistance -	11.3	10.1	6.9	6.8	12.7	9.6
Mean change (ohms)	26	50	15	31	39	32
Mean change as percentage of mean resistance - -	2.3	4.0	1.7	2.5	3.5	2.8

OLGA

Initial resistance (ohms) - -	2,000	2,100	940	1,100	1,050	1,438
Final resistance -	2,200	3,100	1,500	1,500	1,100	1,880
Maximum resistance	2,400	3,100	1,500	1,500	1,350	1,970
Minimum resistance (unstimulated) -	2,000	2,100	940	1,100	900	1,408
Mean resistance -	2,270	2,692	1,375	1,363	1,113	1,762
Maximum change (ohms) - -	367	365	151	178	228	258
Maximum change as percentage of mean resistance -	16.1	13.6	11.0	13.1	20.5	14.9
Mean change (ohms)	82	70	52	58	67	66
Mean change as percentage of mean resistance - -	3.6	2.6	3.8	4.3	6.0	4.1

(N.B. The first sitting had to be discarded.)

TABLE V
(COMPARISON OF METHODS OF CORRECTION : RUDI I)

	Words.	No. in Group.	Mean (S and V only).	Mean actual ohms.	Mean fully corrected.
<i>a</i>	1-10 Ratio <i>a</i> : <i>b</i>	10	328.7 3.023	118.8 3.031	273.2 2.976
<i>b</i>	11-20 Ratio <i>b</i> : <i>c</i>	10	108.7 1.276	39.2 1.380	91.8 1.280
<i>c</i>	21-30 Ratio <i>c</i> : <i>d</i>	10	85.2 .715	28.4 .634	71.7 .751
<i>d</i>	31-40 Ratio <i>d</i> : <i>e</i>	9	119.2 2.468	44.8 2.240	95.5 1.957
<i>e</i>	41-50 Ratio <i>e</i> : <i>f</i>	10	48.3 .589	20.0 .545	48.8 .522
<i>f</i>	51-60 Ratio <i>f</i> : <i>g</i>	8	82.0 1.427	36.7 1.486	93.5 1.355
<i>g</i>	61-70 Ratio <i>g</i> : <i>h</i>	10	57.5 .821	24.7 .823	69.0 .795
<i>h</i>	71-80 Ratio <i>h</i> : <i>k</i>	7	70.0 1.280	30.0 1.176	86.8 1.135
<i>k</i>	81-90 Ratio <i>k</i> : <i>l</i>	6	54.7 1.287	25.5 1.275	76.5 1.159
<i>l</i>	91-100	7	42.4	20.0	66.0

29. Partly for the reasons given at the end of Note 20 above and partly because the material was so much better, I did not apply to these data the elaborate ritual of correction which I used in the case of Garrett-Uvani. Each deflexion was multiplied only by the sensitivity factor and the voltage factor, thus giving what it would have been at full sensitivity and with three volts on the bridge.

As an indicative test of the adequacy of this procedure I worked the figures given in Table V above for the Rudi material for the first sitting. (*N.B.* The first ten words give an abnormally high mean due to insufficient preliminary words having been given, but this

does not affect the issue here. It was partly for this reason and partly because Olga broke down after only 25 words that the sitting was discarded.)

The words were grouped by tens from the list and the means, allowing for gaps, were computed for each group (a) correcting for sensitivity and voltage only, as above (b) using actual ohms change, and (c) taking the fully corrected data as for Garrett-Uvani.

If the different methods of treatment made no difference at all, the ratios interlineated in the table would be identical in any one row. The fact that they differ very slightly shows that the more elaborate treatment makes little difference as compared with the less.

(N.B. It will save repetition if I mention here that a somewhat analogous test was applied before computation to all reaction time data. These tests did not disclose any appreciable systematic tendency towards change of time with the progress of the experiment.)

30. The actual figures are :

SIMILARITY				
W/WP	-	-	-	3.5511
<i>z</i>	-	-	-	.6336
P is less than 10^{-5} .				

31. The corresponding figures for the 46 words are :

SIMILARITY				DIFFERENCE			
W/WP	-	-	2.2063	WP/OWP	-	-	1.0366
<i>z</i>	-	-	.3957	<i>z</i>	-	-	.0180
P	-	-	.004 v.n.	P is very nearly .5.			

(N.B. The difference, if any, would show up here rather than in the 56 words first dealt with.)

32. Lest any trace of difference should by chance escape notice, I also applied to the fragmentary data for the 44 words, left after taking the unbroken block of 56 for analysis, the same correlational procedure that I had used for the Garrett-Uvani psychogalvanic reflexes. The coefficients obtained were :

Rudi-Rudi	-	-	-.0707
Olga-Olga	-	-	.0607
Rudi-Olga	-	-	.0045

This residual material is, accordingly, entirely uninformative.

33. Analysis of Rudi's times shows a Mean Square for Error of 5.87, while that for Word Variance is only 3.71.

The mean for his word totals is 5.66 times their standard deviation, while in the case of Garrett this figure is only 4.22.

34. The figures are :

	SIMILARITY		DIFFERENCE	
	(1) Raw data - -	W/WP 1.0767		WP/OWP 1.8117
	<i>z</i> .0370		<i>z</i> .2972	
	P .36 v.n.		P .00005 v.n.	
(2) Writing double the mean for all greater	W/WP 1.0886		WP/OWP 1.1958	
	<i>z</i> .0425		<i>z</i> .0894	
	P .34		P —	
(3) "One or Zero" method	W/WP 1.4429		WP/OWP .7743	
	<i>z</i> .1833		<i>z</i> -.1292	
	P .035 v.n.		P —	

Note how the Similarity increases and the Difference diminishes and becomes inverted as the treatment is made more rigorous.

35. The dates of the sittings were : 3. xi., 24. xi., 8. xii. and 15. xii. 33; 13. i. and 26. i. 34.

36. The values of *z* given were obtained from those of *W/WP* in the usual way. In the case of FE the value of *z* was -.0999.

37. Mean reaction times, in seconds, for sittings (75 words) were :

Sitting.	L.	F.	J.	E.
I	5.84	1.75	3.74	4.22
II	4.30	5.42	6.06	—
III	4.23	5.18	3.86	3.99
IV	3.45	4.15	4.87	6.75
V	3.49	5.35	5.30	4.30
VI	3.71	4.02	4.55	4.63
Means of Means	4.17	4.31	4.72	4.78

N.B. J and E were always tested after L and F, and F always after L; so that the tendency for the mean time to increase from left to right may be partly a fatigue effect.

38. The distribution of failures or disturbances in reproduction among the personalities and sittings (75 words each sitting) were :

Sitting.	L.	F.	J.	E.
I	75	74	42	34
II	70	39	—	—
III	48	33	49	37
IV	50	31	31	35
V	51	31	57	30
VI	57	28	36	41
Totals	351	236	215	177
Percentages	78.2	52.4	57.3	47.2

N.B. No reproductions were obtained from J or E at the second sitting. See text, § 20.

It is convenient, if not strictly relevant, to note here that there is considerable doubt whether Fedra reacted properly in the first sitting. Apparently she often thought of a reply before hearing the stimulus word. Notice the relatively very short reaction time for this sitting and compare with the remarks on this subject in connexion with Rudi Schneider. Text, § 16.

The effect, of course, is merely to dilute the useful material with a certain amount which is useless and does not affect the validity of the results obtained, except by making them less marked.

Suitable admonishment, together with practice, led to a normal performance by Fedra in this respect.

39. We need not trouble about the P's corresponding to small negative z 's. A big negative z would indicate that the two personalities were "contravariant"; that is to say that there was a strong tendency for one to give a small reaction whenever the other gave a large one, and *vice versa*. This would indicate a causal connexion between the two, just as a significantly large negative correlation would do; but it would, I think, be quite unintelligible psychologically.

40. The results are :

	SIMILARITY		DIFFERENCE	
	z	P	z	P
FJ	-0.171	—	.1960	.015
FE	-0.1740	—	.2253	.006

In view of the over-drastring nature of the test, these results afford good confirmation that the differences obtained by the usual method are not illusory artifacts.

41. See § 6 of text.

42. The dates of the experiments were 9. ii. and 23. ii., 5. iii. and 18. iii. 34.

43. The 50 words actually used were :

1. Light.	11. Cash.	21. Drive.	31. Boat.	41. Hill.
2. Town.	12. Take.	22. Child.	32. Build.	42. Shoot.
3. Bath.	13. Young.	23. Bet.	33. Door.	43. Egg.
4. Love.	14. Proud.	24. Sleep.	34. Dance.	44. Kill.
5. Read.	15. Black.	25. Fear.	35. Eat.	45. Fetch.
6. Mad.	16. War.	26. Foot.	36. Cry.	46. Noise.
7. Bald.	17. Stoop.	27. Red.	37. Hot.	47. Pen.
8. Name.	18. Ball.	28. Land.	38. Girl.	48. Take.
9. Pin.	19. Pray.	29. Call.	39. Town.	49. Poor.
10. Say.	20. Cat.	30. Life.	40. Lake.	50. Run.

N.B. It will be seen that by a regrettable oversight on my part, two words—Town and Take—appear twice in the list. In dealing with the data I have left these in, since their only effect, presumably, would be to increase similarities between the personalities concerned.

44. The mean reaction times, in seconds, for personalities and sittings were :

Sitting.	L.	F.	J.	E.	
I	4.04	1.88	3.45	2.81	General Mean 3.135
II	2.71	2.86	3.84	3.33	
III	2.56	3.26	4.06	3.89	
IV	2.48	2.76	3.20	3.10	
Means of Means	2.95	2.69	3.64	3.26	

45. The reaction time results were :

	SIMILARITIES		DIFFERENCES	
	z	P	z	P
LF	-.0462	.37	-.0047	—
LJ	-.0320	.41 v.n.	-.1026	—
LE	-.0974	—	.1202	.15
FJ	-.0939	—	.1350	.12 v.n.
FE	-.1672	—	.1166	.03
JE	.1416	.16	.1993	.045

Taking means for personalities and arranging in order as before gives :

J	-	-	.0266	E	-	-	.2807
L	-	-	-.0067	F	-	-	.1176
E	-	-	-.0410	J	-	-	.0771
F	-	-	-.0716	L	-	-	.0043

46. The distribution of disturbances in reproductions among personalities and sittings was :

Sitting.	L.	F.	J.	E.
I	33	19	11	8
II	21	7	10	11
III	25	13	16	22
IV	21	17	18	24
Totals -	100	56	55	65
Percentages	50	28	27.5	32.5

The results obtained from the reproduction test were :

	SIMILARITIES		DIFFERENCES	
	z	P	z	P
LF	.2526	.04	.0328	.39
LJ	.2137	.14	.2322	.02
LE	.1668	.12	.0615	.30
FJ	.1602	.115	.1239	.145
FE	.3525	.008	-.0980	—
JE	.3599	.005	.0151	.45

Means for personalities arranged in order of magnitude are :

E	-	-	.2931	J	-	-	.1237
F	-	-	.2551	L	-	-	.1088
J	-	-	.2446	F	-	-	.0196
L	-	-	.2110	E	-	-	.0071

N.B. I attach little or no weight to these results, but they are worth noting for purposes of comparison.

47. These tests were made at various times between 18. iii. and 4. iv. 34. They are thus separated from the first series of sittings, from which the figures for supposedly normal but actually "prepared" Leonard were obtained, by intervals of from seven to

twenty weeks, while the first series itself extended over twelve weeks. We can, therefore, hardly attribute the change to lapse of time ; for if Mrs Leonard remains substantially the same during twelve weeks (and if she did not we could scarcely obtain significant results from the first series of experiments) she is not likely to change radically in the next seven or so.

48. Mean reaction times for Normal Leonard were :

Sitting.	I.	II.	III.	IV.	V.	VI.
Mean R.T. (secs.)	2·29	1·49	2·43	1·74	1·74	1·43
	General Mean, 1·85.					

Note the considerable speeding up of Leonard's reactions, even as compared with the Second Series figures (Note 37). This may be partly due to systematic differences in the *modus operandi* of the timers, but such differences provided they are constant for the individual operator make no difference to the computed results, while if they are not they only increase error without altering the sense of the results.

49. Normal Leonard gave disturbances in reproduction as follows :

Sitting.	I.	II.	III.	IV.	V.	VI.
No. - - -	32	35	39	21	18	34
Percentages -	42·7	46·7	52·0	28·0	24·0	45·3
	Mean, 28·83.			Percentage, 38·44.		

REVIEW.

HEREWARD CARRINGTON, *An Instrumental Test of the Independence of a "Spirit Control"*. (American Psychical Institute: Bulletin I: pp. 96. Price \$2.00. New York: 20 West 58th Street.)

The Bulletin in which this paper appears is the first issued by the recently formed American Psychical Institute, of which Mr Hereward Carrington is Director, and we may feel confident that if the spirit of objective enquiry which has inspired the work reported is maintained, the Institute will make many notable contributions to psychical research in the future.

In addition to the strictly instrumental tests, the Bulletin contains a few pages dealing with the formation of the Institute, accounts of a variety of collateral experiments of various kinds, and discussions of the results. I shall, however, confine myself almost exclusively to the quantitative work with the psychogalvanic reflex and reaction times, considering this to a great extent in the light of my own recent studies in the same field, which are described elsewhere in this issue.

Inasmuch as I was the first to suggest the method used (as Mr Carrington handsomely acknowledges) the task of reviewing this first attempt to apply it places me in a peculiarly invidious position. For if I praise (and much praise is merited) I shall be suspect of wishing to exalt my own offspring; while if I criticise (and many criticisms leap to the instructed eye) I shall be thought envious of Mr Carrington for being before me in applying my suggestion. And the worst of it is that both accusations will be true! Between this Scylla of jealousy and Charybdis of opportunism I must steer as best I can, but the difficulties are doubled by the fact that although Mr Carrington has given, in effect, what appears to be the right answer, it is the easiest thing in the world to drive the proverbial coach and four through the procedure whereby he arrived at it. And it is necessary to point out the joints in the methodological harness lest prospective workers in this field should be misled, or the uncritical induced to mistake a valuable reconnaissance for a sweeping victory.

In briefest outline, what Mr Hereward Carrington did was this: he applied a word association test to the well-known medium, Mrs Eileen Garrett, and to her "Control" known as "Uvani" (purporting to function through her body, of course) on various occasions;

the measured reflexes and times corresponding to the words used in the test were then compared by correlation, and the conclusion was drawn that those from Mrs Garrett and Uvani respectively showed much less agreement than those from Mrs Garrett on different groups of occasions. From this conclusion Mr Carrington, very reasonably, felt himself entitled to infer that the two personalities do not share a common subconscious mental content and should therefore be regarded as "different" individuals. This argument is perfectly sound; because, without begging the whole question at issue, there seems to be no other way of defining what we mean by "same" or "different" in this context. But the validity of the inference, like that of any other argument, depends on that of the data to which it is applied, so that, if these are unreliable, the inference is equally so.

Now, the conclusion that the Garrett and Uvani deflexions (or times) differ significantly, which is the starting point of Mr Carrington's argument, is itself the end product of a complex process in which three sciences, namely psychology, physics (in the case of psychogalvanic reflex) and mathematics play a part. These three aspects will be considered *seriatim* below.

But Mr Hereward Carrington, although he has spent many years in the active pursuit of psychical research, is not and does not profess to be a trained psychologist, physicist or mathematician. He therefore had recourse (in principle very wisely) to those who did. Unfortunately, with one or two honourable exceptions, these coadjutors did not serve him well, but omitted almost every precaution and committed almost every error that could find place in the work.

To account for this fall from grace we may charitably invoke the theory of the Enchanted Boundary—so trenchantly developed by Dr Franklin Prince¹—which separates psychical research from other sciences and is such that so soon as the sceptical "scientist" crosses it "his weapons become like rotten wood, the joints of his armour begin to gape widely, and his proud steed alters to a sorry jade, stumbling at every pebble in the way". Otherwise we should be compelled to infer a degree of levity and incompetence which it would be distressing to associate with Transatlantic science.

I will now discuss various specific points which have occurred to me in the course of reading the Bulletin:

PSYCHOLOGICAL.

(1) To anyone accustomed to using the word-association test Mr Carrington's psychological advisers brand themselves with amateurishness by permitting the use of the infinitive forms "to

¹ Cf. *Psyche*, iii. 298, and his book of that name.

sing", "to die", etc., instead of simply "sing", "die", etc. The use of these forms is, in principle, objectionable because the essence of the test is that it should be conducted crisply, so that the subject receives in a clear-cut manner the unheralded impact of the stimulus word. Imagine—to take the extreme case—an experimenter saying, "Now I wonder whether you would be so kind as to give me the pleasure of hearing your reaction to the interesting little word 'pusillanimously'?" This is ridiculous, of course, but any premonitory irrelevance is apt to take the edge off the word given.

But the *reason* why these forms were used is simple. Most of the original work on the word-association test was done in German, notably by Jung and his co-workers and in *their* lists we naturally find the infinitives "singen", "sterben", etc. When such lists are translated into English, the translator rightly indicates that an infinitive was used; but no man familiar with either the theory or practice of the test would dream of using such infinitives in English working.

I am well aware that these forms are given in the supposedly authoritative work of a well-known American psychoanalyst; but this merely shows that he inserted the matter concerned more "for the sake of completeness" than because he knew anything about the subject.

I do not say that the use of these forms made the slightest difference to the result: I do not for a moment suppose they did. But I do say that their use indicates that Mr Carrington's psychological advisers were unfamiliar with the practice and ignorant of the theory of the word-association test.

(2) Conformably with this conclusion, I find no indication that the order of the words was varied as between one experiment and another (*cf. supra*, p. 224, note 15). This precaution is desirable because every word should be presented to the subject so far as possible *de novo*, whereas, if the order be not altered, some words will soon give warning of those that follow them and the effect of these latter will be altered.

(3) No account seems to have been taken of the possible effect of fatigue in producing spurious resemblances or differences, by prolonging unnaturally the reaction times towards the end of a sitting or increasing or diminishing reflexes. These effects seem to be negligible, but the possibility should have been noted, if only to be dismissed. To allow it to go by default, so to speak, suggests that it has not been grasped at all.¹

(4) Similarly, the likelihood of over-long reaction times engendering artificial differences² has been overlooked, and the same applies

¹ Cf. *supra*, pp. 228, 234.

² *Supra*, p. 181.

to reflexes, though here the oversight is more pardonable, for only an intimate personal experience with the method would lead one to realise how the effect can come about here.¹

(5) When correlational methods are to be used, *faute de mieux*, it is important that allowance should be made for the fact that the psychological or physiological "mood" of the subject may vary from one occasion to another; for otherwise a sitting conspicuous for very long times or very large deflexions may dominate the others and distort the result. This may best be done by expressing each time or deflexion as a percentage of the mean for the sitting to which it belongs. But this was not done, leaving room for unnecessary error, though inspection does not suggest that this was serious.

These last three points are admittedly semi-mathematical, inasmuch as some notion of how figures are compared is needed for their apprehension. But it is the business of the psychologist to supply his statistical colleague with figures from which systematic errors have been eliminated, or to collaborate with him in their removal; and every competent experimental psychologist possesses (or should possess) at least a smattering of statistical theory, which seems to have been lacking here.

In this connection I may point out that the figure of *minus* .4 given for the coefficient of correlation between G and U for the first series of experiments conducted in 1932 (p. 32) indicates just as close a relationship between them, though inverted, as would *plus* .4; either figure is highly significant when 100 pairs are concerned. I am told that this is a misprint for $-.04$, but it has strangely escaped the notice of the enthusiastic "critics" who have hailed the work as conclusive.

PHYSICAL.

Reverting to the experiments themselves the following points are noticeable in connection with the psychogalvanic reflex technique.

(1) No circuit diagram or equivalent account of the electrical arrangements is given, nor can I find any details regarding sensitivity of galvanometer, average resistance of the subject, average change of resistance per stimulus, etc.

(2) So far as I can ascertain by personal enquiry, the ordinary Wheatstone's bridge method was used in the second series of tests (1933) and the Godefroy transformer technique in the first. Now the latter procedure, while possessing the advantage of an invariable zero point for the galvanometer, does not give a measure of the

¹ *Supra*, p. 221, note 9.

resistance change in the skin but of the first differential of this with respect to time. One might fairly say that the "bridge" method measures the effect produced by the stimulus and the "transformer" method the rapidity of its onset. It was rash to assume that these two are even qualitatively—much less quantitatively—equivalent.

(3) No allowance is made for, nor any relevant data given concerning, the possibly great effect on deflexions of changes in the subject's resistance during the experiment apart from the transient effects of the stimuli (*supra*, p. 228). This, again, is a point which (it would seem) may, in fact, be legitimately ignored. But in pioneer work of this kind, where technique is not yet standardised, every such point should be considered and the reasons (if any) for ignoring it should be given. Otherwise, the suspicion that it has merely not been thought of, with the corollary that the work is superficial, is almost irresistible.

(4) Much more serious than any of these is the failure to understand what is involved in the production of "negative deflexions" (*supra*, p. 179) indicating an increase as opposed to the normal decrease of resistance on stimulation. These (as I think that everyone who has worked at all extensively with the reflex will agree) are almost invariably due to mechanical movement between the hand and the electrodes. (*N.B.* It is theoretically possible that they might be generated by a back E.M.F. induced by the stimulus; if so, reversal of the electrode leads would abolish them.) As such, they *may* indicate the kind of psychic disturbance which it is desired to measure; but this needs demonstration before they can be used as indicators with safety. Using my own material (Garrett-Uvani) I obtained a positive correlation of .17 for Garrett and of .28 for Uvani between the positive and negative correlations observed (neglecting the sign of the latter). This is about what one would expect; but it means that, in general, to treat negative deflexions as if they were positive is to dilute informative material with much less informative and thus obscure the answer.

Yet this was done, I understand, in computing the results of the 1932 experiments—and that without (presumably) such feeble justification as I have just given.

(5) No account seems to have been taken of day-to-day variation of resistance. See PSYCHOLOGICAL (5) above.

STATISTICAL.

(1) Mr Carrington would have done better to have sought the advice of a competent statistician, if possible, before planning his experiments. He would then have been told that to base a research of this kind on no more than two sets of readings from one of the

personalities (Uvani) is asking for trouble. It is true that to have arranged for more Uvani sittings would have necessitated abandoning others of considerable intrinsic interest. Possibly Mr Carrington was wise in adopting the policy he did—on the whole, I think he was—but it should have been made clear to him that this policy was almost certain to preclude the obtaining of statistically significant results.

(2) The plan adopted for comparing individual words (p. 33) is antiquated and tells us nothing reliable when it has been applied; for who shall estimate the chance of the medians of pairs of variates differing significantly from those of octets eleven times in a hundred? Moreover, although it may still be “customary to consider differences as large as four times their probable error . . . as significant”, this is a matter of pious homage to tradition rather than modern statistical practice.¹ The proper way of treating such problems is by using the sums of squares of their deviations from their means as described by Professor Fisher.²

(3) The use of the Spearman-Brown formula (p. 34, lines 9 and 36, lines 1 and 2) is at least equally archaic. When first devised it often served a useful purpose (though not wholly reliably) in enabling one to pass, as we should now say, from an *inter*-class to an *intra*-class correlation. It would be better, now that we know how to do it, to calculate an *intra*-class correlation straight away.³

(4) *Mutatis mutandis*, the above criticisms apply to the reaction time data.

(5) It would have been better to compare the odd-numbered Garrett sittings with the even-numbered, rather than the first four with the last four, in order to compensate for any systematic change with time in Mrs Garrett's reactions to the words.

(6) On the all-important question of the *significance* of the difference in the correlation coefficients for G compared with U, as against G with G and U with U, the method adopted is not satisfactory. The proper procedure is to convert the coefficients into “z's” by the formula $z = \frac{1}{2}(\log_e(1+r) - \log_e(1-r))$, take the mean of the z's for the GG and UU correlations, subtract that for the GU correlation and divide the difference by its standard error, which is the square root of the sum of the reciprocals of the number of pairs of variates respectively concerned less three.

This procedure gives a value of 1.88, in the case of the reaction times (see top of page 36) corresponding to a chance of accidental occurrence of about 1 in 16. This cannot be judged significant, apart from the reservations made under the heading PSYCHOLOGICAL

¹ Cf. Fisher, *Statistical Methods for Research Workers*, 4th Edn., p. 46.

² *Loc. cit.*, 114-115.

³ *Loc. cit.*, chap. vii.

above and from the fact that the two correlations are of very unequal value.¹

The corresponding result from the reflex data is, as will be seen by inspection, quite insignificant.

Thus it will be seen that from the total experiments emerges but one promising result and that of very dubious significance—dubious in the sense that unless it were confirmed (as I am happy to say that it amply is by my later work²) no statistician would accept it as convincing, though actually it happens to be the right answer. For this comparative failure Mr Carrington himself is in no sense responsible; for there can be no doubt that the care he expended on the work would have led to results as convincing as my own if he had not been misdirected on so many points.

He may well feel inclined to echo Mr Chesterton's lines:

“The New World's wisest did surround
Me, and I'm sorry to record
I did not find their views profound,
Or their conclusions well assured.”

For the credit of American Science it is to be hoped that more effective help for him will be forthcoming next time he undertakes experiments on these lines. And the sooner the better; for, in my partial judgment at least, we need an immense amount of such work at the present stage of the science.

I am not quite clear whether Mr Carrington supposes that a marked difference in the *mean* reaction time or deflexion indicates a difference of personality of the kind he is seeking. On p. 38 he rightly discounts the value of this and insists on the paramount importance of the differential reaction as between individual words; but in his footnote to p. 34 and his textual note on p. 36 he appears to take it seriously, for otherwise his disagreement with the “fairness” of Dr Zubin's comparisons lacks motive. Actually, difference in the values of the means is of no importance, for such an overall change could easily be brought about by drugs, by fatigue, or even

¹ The immediately preceding comments were made on the assumption that the correlation coefficients given are correct; but they do not appear to be so.

Making allowance for the fact that the figures given in the published sheet for the 1933 sittings awkwardly show .1, .2 . . . etc., where one *fifth*, two *fifths* . . . etc., are intended, I obtain for the UU correlation the value .3283 instead of .4565, for the 59 pairs of variates available. This is derived from the use of the full Bravais-Pearson Product Moment Formula. Dr Zubin seems to have worked with a Correlation Table, grouping all values greater than 6.0 secs. together in the 6 sec. class. This may account for the discrepancy and the procedure seems to counteract the misleading effects of “too long” times. But, again, it should have been mentioned and justified.

² *Supra*, p. 186.

by a heavy meal. Atropine, for example, properly applied, abolished the psychogalvanic reflex, while a large lunch would, I think, appreciably slow the reaction time. These are all-round physiological changes having nothing to do with psychical individuality, and if anyone likes to maintain that they are "really" psychological, he will have to prove it first and find that it makes no difference afterwards. At best, a change of mean time or of mean reflex would indicate a change from, say, an excitable to a sluggish *type* and this seems just the kind of thing—no more than an exaggerated change of mood—which a true secondary personality could compass.

The test of individuality depends on the transvaluation, so to speak, of associations revealed by the *differential* response to words.

Turning to the brighter aspects of the paper, there can be no doubt that what Mr Carrington has lost on the swings of significance he has gained on the roundabouts of variety.

Particularly interesting, for example, are some of the personality tests (Benneuter, Page, Thurstone, etc.) applied by Miss Frances Strakosch, and the Rorschach test given by Dr Levy. Some of these (attitude towards the Church, attitude towards Birth Control) may not be worth very much, but the general method ought to prove extremely fruitful in this field and is very well worth development.

Most of all I like, in idea though not in its particular application, the test on what might be called "interference of learning" carried out by Miss Strakosch and Dr Shipley. Briefly: Mrs Garrett and Uvani were caused to learn lists of paired numbers in such a way that, if there were any connection between the personalities, the learning by one might be expected to facilitate learning by the other in one set of circumstances and to retard in another. Indications of such an interconnection were found; but this is exactly the kind of test, as Mr Hereward Carrington points out, which would show it on any hypothesis, for any kind of learning by verbal repetition is bound to establish nerve paths of lowered resistance in Mrs Garrett's *body*, which is involved in each case. Such learning leads to an automatism, so to speak, in which the higher centres or "pure mind"—if this means anything—are not involved, as anyone can testify who has learned a poem well enough to recite it while reading a newspaper understandingly, or to work a calculating machine while thinking of something else.

But the general plan of applying "mental tests" is excellent and should be followed up. The right kind of test will not be easy to select; but from the hundreds available it should be possible to choose a dozen or so which would enable quantitative methods to be applied to the comparison of the results.

Another very striking part of the work is to be found in the highly identificatory replies sometimes given by "third entities" (supposed communicating controls other than Uvani) of whom several were tested. For example, the associations "Glass—Alaska" and "Needle—Niagara" given by "Will" are superficially extremely far-fetched, but apparently quite relevant to the supposed communicator. Our own experience with Mrs Leonard and her controls suggests strongly that this method of "free association" is likely to prove a far quicker way of eliciting evidential reminiscences (especially if the subject is not required to answer against time) than direct questioning or urging the communicator to think of something evidential. This line of research should also be further developed.

To sum up: Taking the paper as a whole, Mr Hereward Carrington is to be congratulated not only on his courage and enterprise in undertaking so formidable a research under so heavy a handicap in respect of material resources, and on his determination to import the essential element of quantitative measurement into the cloudy waste of psychical speculation, but on having produced a very notable contribution to the subject—perhaps the most important single contribution ever made.

But will scientists in other fields please note, for their future guidance, that nothing short of a little better than their best is good enough for psychical research?

W. W. C.

PROCEEDINGS

OF THE

Society for Psychical Research

PART 137

REPORT OF AN INVESTIGATION INTO THE MEDIUMSHIP OF RUDI SCHNEIDER¹

By THEODORE BESTERMAN AND OLIVER GATTY

“The man who cannot occasionally imagine events and conditions of existence that are contrary to the causal principle as he knows it, will never enrich his science by the addition of a new idea.”

MAX PLANCK.

INTRODUCTION

THE history of Rudi Schneider's mediumship up to the end of 1931 is given, in a summary form, in *Proceedings*, xl. 428-436. In the following year the only sittings of any importance were those organised in London by Mr H. Price in February-May² and by Lord Charles Hope in October-December.³ The main points emerging from these two series are: (1) An accusation of fraud on a single occasion by Mr Price, an accusation which is open to criticism and has been dealt with elsewhere⁴; and (2) the apparent confirmation in Lord Charles Hope's sittings, though to a lesser extent, of the infra-red absorptions described by Osty.

¹ An informal report of this research was given at a Private Meeting of the Society on 30 May, and a preliminary account appeared in *Nature* (1934), cxxxiii. 569.

² H. Price, *An Account of Some Further Experiments with Rudi Schneider* (1933).

³ Lord Charles Hope and others, “Report of a Series of Sittings with Rudi Schneider”, *Proceedings S.P.R.*, xli. 255-330.

⁴ E. Osty, *Revue Métapsychique* (1933), pp. 110 ff.; W. F. Prince, *Bulletin XX* of the Boston S.P.R., pp. 86 ff.; Lord C. Hope, *Proceedings S.P.R.*, xli. 284 ff.

The Council of the Society regarded Dr Osty's reports¹ as presenting a strong *prima facie* case for the infra-red phenomena described by him. As the resources of the *Institut Métapsychique* had been strained by the earlier investigations, the Council made a grant and an offer of collaboration to the *Institut*, which was accepted. One of us (Th. B.) accordingly proceeded to Paris and took part in seventeen sittings at the *Institut* during February-March 1933. These were totally negative (except for a very slight curtain movement during an informal sitting at a moment when Th. B. was not present). Rudi accordingly returned to his home at Braunau, where Th. B. followed him in the hope that the atmosphere of the medium's home might be conducive to the production of paranormal phenomena. The four sittings at which he was present there were negative, except for two very slight curtain movements under conditions making a judgment impossible.

At this stage it was arranged that the medium should have a long rest before attempting to give further formal sittings, and that, after an interval, a party of members of the Society should go out to Weyer, in Upper Austria, where the medium now lives since his marriage with Fräulein Mitzi Mangl. Miss Reutiner, Lord Charles Hope, Mr Evelyn, Captain Cochrane-Baillie and Th. B. accordingly went to Weyer in September 1933 and took part in several sittings (Th. B. in three). Seemingly paranormal phenomena, in the form of considerable telekineses and curtain movements, were observed, but under non-evidential conditions. As a consequence Rudi was invited to the S.P.R. He gave a series of sittings in the Society's *séance*-room between October 1933 and March 1934 inclusive.

The sittings were organised with a fourfold objective in view. (1) To look for confirmation of the infra-red phenomena previously reported, (2) to test the alleged telekinetic powers of the medium, (3) to record any phenomena observed instrumentally, excluding human testimony so far as possible, and (4) to make the conditions of the sittings such as to inspire confidence in any results that might be obtained.

In the event no phenomena clearly of a paranormal kind were obtained. Nevertheless sufficient experimental details will be given to enable the reader to judge of the methods employed.

THE SÉANCE-ROOM

The Society's *séance*-room was specially built for the purpose of mediumistic investigations. It is supported on pillars, the space below being used as a storeroom. On its south and east side the

¹ *Les pouvoirs inconnus de l'esprit sur la matière* (1932); *Supernormal Aspects of Energy and Matter* (1933).

room projects freely, the south wall being pierced by a large window, supplied with a pair of solid sliding shutters which are completely light-tight. The north side is a party wall adjoining the next house. The east side is pierced by a door, automatically locked and also

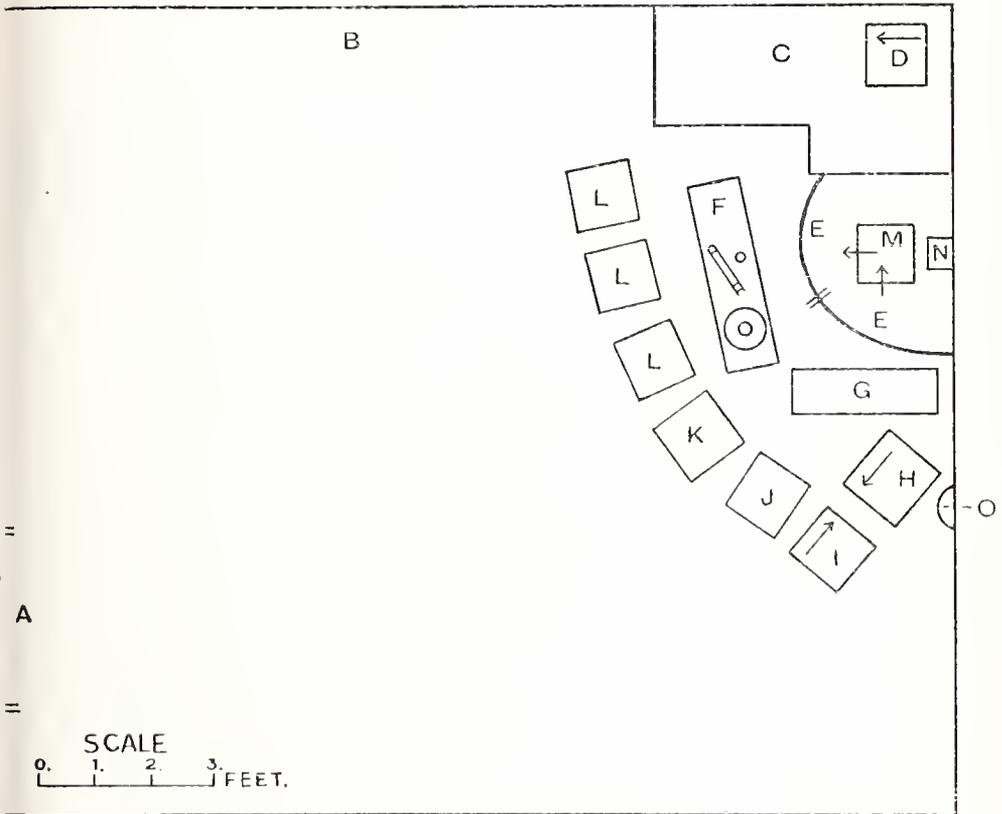


FIGURE 1. PLAN OF SÉANCE-ROOM.

- | | |
|--|---|
| A. Door. | I. Controller's chair. |
| B. Shutters. | J. Sub-controller's chair. |
| C. Galvanometer, etc. enclosure. | K. Th. B.'s chair. |
| D. O. G.'s chair. | L. Sitters' chairs, as arranged
for a circle of six. |
| E. Curtains forming cabinet. | M. Lead box. |
| F. Thermopile and light source box,
with basket, etc. standing on it. | N. Temperature reading boxes. |
| G. Photo-cell and light source "box". | O. Pillow for medium's head. |
| H. Medium's chair (screwed to the
floor). | |

double bolted on the inside during sittings, leading to a small ante-room, which in turn leads to the main part of the house.

The space C (see plan, Fig. 1) is divided from the cabinet by a solid partition, reaching from floor to ceiling. It contained a shelf, later two shelves, stretching from wall to partition, on which stood a

Moll galvanometer, with its lamp and scale, a cardiograph embodying an Einthoven string galvanometer, a voltmeter and a switch-board. This apparatus was observed by O. G., who had to crawl under the lower shelf in order to get to and from his chair. The apparatus enclosure was for all practical purposes light-tight.

THE CABINET

The larger enclosure formed the so-called cabinet, into which the mediumistic "force" is said to enter, there concentrating itself and issuing through the opening to produce phenomena. This cabinet contained, at the request of *Olga*, the medium's control, a bowl of water (on one occasion of ice), a lead box, and other things detailed below. The history of this box is as follows. In sitting No. 20 *Olga* alleged that the force was harmed by counter-vibrations. An inverted tin box was therefore introduced as a temporary measure, and it was proposed to *Olga* that a permanent box impervious to all radiation¹ should be made into which the "force" could enter and collect itself. *Olga* agreed with enthusiasm, and accordingly a box was made (at the expense of one of the sitters, who wishes to remain anonymous) of 2-inch chemical lead. The overall dimensions are 12" × 12" × 15". At the side nearest to the medium is a circular hole of diameter 3", into which the force could enter, and on the side facing the cabinet curtains was an oval of size 6" × 9", through which the force was to issue. The justification for using this box is clear, for *Olga's* statement, though improbable, might have been true. If, on the other hand, *Olga's* notion about harmful radiations were purely subjective, then the suggestive effect of accepting it and taking appropriate steps could presumably only be beneficial to the production of paranormal phenomena.

CURTAIN MOVEMENTS

A phenomenon alleged to occur in sittings with Rudi Schneider is the movement of the curtains forming the cabinet. These curtains usually bear vertical strips of luminous ribbon, and in the present sittings there were placed, in addition, luminous spots in four places near the bottom edges of the curtains. Thus if a decided movement occurred it was unmistakable, particularly as a record was always dictated as to the spots visible to each sitter. Smaller movements, however, would necessarily be uncertain, owing to the optical and psychological difficulties of accurately perceiving and localising small luminous areas in the dark. Moreover, it was obviously desir-

¹ Actually, of course, it is impervious to all known forms of radiation except cosmic rays, at all points of its surface other than the holes left as an entrance and exit for the force.

able that the exact extent of such curtain movements, together with the amount of energy exerted to produce them, should be measured. A simple method of achieving this was accordingly devised. A strip of leather was wound on a roller operating on an adjustable ratchet, the roller being fixed against the wall forming the back of the cabinet, and the farther end of the strip to the curtain. Hence a movement of the curtain towards the sitters (the usual direction), at the point where it was attached to the dynamometer, would force the attached strip to turn the roller, which would then remain in the position of furthest extension, thus enabling the *distance* pulled to be measured. The force exerted could be tested with an ordinary spring balance. Actually the most sensitive adjustment of the ratchet enabled a force of about 3 oz. to be measured.

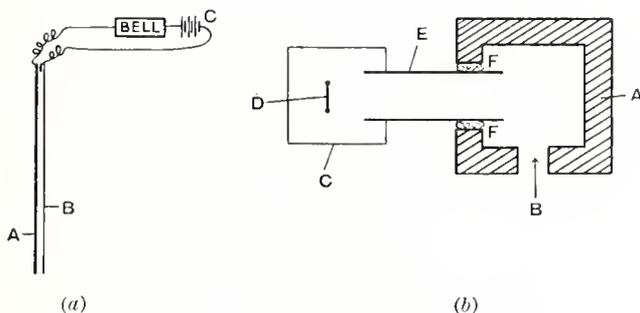


FIGURE 2.

(a) The circuit and section of apparatus. A=Brass strip. B=Tinfoil sheet. C=Battery.

(b) Plan. A=the lead box. B=entry for the force. C=cardboard box on scale $\frac{1}{16}$ ". D=the brass strip and tinfoil. E=connecting tube. FF=cotton wool lagging.

No movement was ever recorded by this appliance and it was accordingly dropped in favour of another and still simpler device of a nature likely, if successful, to be more stimulating psychologically to the medium. Nevertheless no response was obtained, and finally, shortly before the end of the sittings, a still more sensitive arrangement was installed. From the ceiling of a cardboard box measuring $8'' \times 8'' \times 18''$, were suspended a strip of brass and a strip of tinfoil $13\frac{1}{2}''$ in effective length, and separated at the top by an insulating block of ebonite, $\frac{1}{4}''$ in thickness (see Fig. 2). The two strips were placed in a bell circuit in such a way that if they were brought into contact a bell was rung. On the tinfoil side of the box containing this arrangement there was made an oval hole, which was connected by means of a light tube to the oval hole of the lead box, into which, after being lagged with cotton wool, it exactly fitted. Thus, the "force" on leaving the lead box would be obliged to enter the con-

tact box and could hardly help exerting pressure on the tinfoil and thus ringing the bell, for the tinfoil was so sensitively hung that the slightest puff brought it into contact with the metal. At the same time even vigorous blowing at the entrance of the lead box could not affect the tinfoil. Under these conditions no response was obtained, although the bottom of the curtain continued to move slightly.¹

These circumstances do not completely dispose of all the curtain movements, some of which seemed to extend to as much as 25 cms. The need for further investigation of these movements was constantly in mind, but it could not be done in a conclusive manner without interfering with the more important tasks in hand. The investigation of these movements was therefore limited to the methods described, which at least make it *probable* that all the curtain movements were due to stray draughts which were from time to time stronger than usual. This can hardly be said to be proved, but six or more people talking and singing in a room, combined with an entry of cold air through a grid, may be capable of producing considerable effects now and again during a period of some 200 hours.

CINÉ CAMERA, LIGHTING, ETC.

On occasion the cabinet also contained other things which will be described below in connection with the temperature and mitogenetic tests.

In front of the cabinet stood the box containing the thermopile, etc., details of which will be given below in the section devoted to the infra-red investigation.

On the thermopile box stood an inverted waste-paper basket, under the edge of which was caught the corner of a handkerchief which hung down the side of the box nearest to the cabinet curtains. On the basket stood a small luminous hand-bell. Other objects on the box were, at various times, a small celluloid ball, a black ruler, and a partly luminous xylophone. These things were intended to be the objects of the medium's telekinetic forces. In the hope of photographing these objects in paranormal movement a cinematographic camera was installed with a film sensitive to the infra-red, supplied by Messrs Ilford, and it was eventually found possible to obtain sharply defined moving pictures in a feeble red light. By increasing the infra-red flood lighting, and using horn or ebonite filters, it is confidently expected that cinema films could be taken in *total*

¹The simple expedient of excluding the draughts could not be adopted, for this would have made the room uninhabitable during the long sittings. The source of the draughts is a small ventilation grating in the ceiling, through which fresh air is pumped into the room through a conduit, by a motor-driven fan in the ante-room. It is certainly a pity that a more convenient system of ventilation was not adopted when the *séance*-room was built.

absence of visible light. By this means motion pictures could be obtained of telekinetic phenomena in a light that is innocuous to the medium. No telekineses of the objects provided for the purpose were, however, obtained, and consequently the camera was not used during the actual sittings.

Over the table hung a lamp, the position of which was adjustable vertically and laterally. This lamp was specially designed and made for these sittings. It consists of a hood of black-painted metal $13\frac{1}{2}$ " long, with a diameter of 12", inside which hung eight small electric lamps, each in its own compartment and controlled by its own switch, and each supplied with a red filter of different thickness. In this way it was possible to obtain a large range of intensities of red light and to know what amount of light was in use at any given moment, for any change was at once recorded. The lamp as a whole was also controlled by a rheostat, but the use of this was avoided.

THE CONTROL OF THE MEDIUM

The method of controlling the medium is simple and effective. He is seated in position H (Fig. 1), facing in the direction of the arrow. Directly in front of him sits the first controller, who takes the medium's hands in his own and puts his knees and the lower part of his legs firmly against and outside the medium's. Moreover, frequently throughout the sitting *Olga* requests the sitters to "hold tight", that is, to make sure that the circle is completely formed. This request has the natural consequence of making the sitters, including the controllers, hand-conscious, and thus serves to prevent any lack of continuity in the control of the medium. Next to the first controller sits the second controller, who is nearly always a woman. She places her right hand round the medium's right wrist, thus supplementing the first controller's guard of that side of the medium nearest to the apparatus.

A detailed account of the method of control and of the precautions taken to preclude fraud is on record, but in view of the negative results it does not seem necessary to give this account in full. It should be stated that luminous strips¹ were sewn to the sleeves of the medium's pyjamas, which were provided by the Society.

GENERAL ARRANGEMENTS

There was no notetaker in the *séance*-room. The records were dictated by Th. B. into a telephone suspended round his neck, to the secretary, who was seated in the Society's office, some distance from the *séance*-room. In this way a complete record (extending in all to some 400 typewritten pages) was made of all conversation with *Olga*

¹The luminous paint used did not require to be refreshed, being made on a mesothorium basis, and hence being relatively long-lived.

(except occasional trivial remarks), all observations made in the room (the instrumental ones being called out by O. G.). Needless to say, the task of the secretary, Mrs C. Wright, was an arduous one, and we take this opportunity of expressing our cordial thanks to her for the efficient way in which she carried out her work. Occasionally, when the sitting was particularly long, the secretary left a little before the end, and the last few minutes were taken down by the medium's wife, Mitzi Schneider. Throughout every sitting held in the *séance*-room, Frau Schneider was in the Society's office with Mrs Wright or at the telephone in the manner described.

Such were the personal precautions taken to prevent fraud on the part of the medium (apart, of course, from the direct precautions taken to prevent interference with the apparatus). These precautions were all of a simple kind, which were not in the least troublesome to the medium, who, indeed, welcomed them. Naturally, if paranormal phenomena had been forthcoming these precautions would have been tightened up and elaborated; as described they were merely a first step.

When the sitters had taken their seats and the controllers had taken charge of the medium, which was always done, at the beginning of each sitting, in a bright white light, the light was extinguished, generally by means of a rheostat placed in the observation corner. The required amount of red light had already been switched on and then remained. After an interval of about four to seven minutes, the medium's hands and arms usually begin to tremble, this being regarded, for purposes of record, as the beginning of the trance. These tremblings fairly rapidly increase in strength until they are interrupted by several violent clonic spasms; usually the medium suddenly falls quite limp for a moment or two during these preliminary efforts. Then, after one of the clonic spasms, the medium relaxes a little, though persisting in a moderately violent, jerky movement of his trunk, and begins to change the rate and depth of his respiration. Until this moment his breathing is more or less normal, allowing for his violent movements. Now, however, the medium half opens his mouth, and begins to breathe rapidly, noisily and wetly. This goes on, interrupted by frequent clonic spasms, moments during which the medium falls quite limp and does not breathe at all, brief commands and jerky fragmentary conversation, until *Olga* asks for an interval. The process of coming out of trance is an almost exact inversion of that of falling into trance. These processes are gone through at the beginning and end of each part of the sitting.

THE TRANCE AND RESPIRATION

The object of the present research as a whole was primarily to investigate the problem whether Rudi Schneider possesses paranormal

powers, and, if so, to inquire into the nature and capacities of those powers. Other problems arising incidentally, however interesting in themselves, had to be subordinated to this primary aim, at any rate so long as the main question remained open. Consequently no steps could be taken to study the psychological aspects of the medium's trance which might have adversely affected the subject's mediumistic activities. Hence we restricted deliberate experiments on these lines to word-association and psychogalvanic reflex sittings, the results of which must now be set out.

As we have seen, when Rudi goes into trance his personality changes. A strictly objective account of the change would be: when in trance Rudi behaves as if he were a woman answering to the name of *Olga*, who declares herself to be the surviving spirit of *Lola Montez*.

Before we turn to the consideration, so far as our fragmentary material permits, of the nature of *Olga*, a few words ought to be said about the trance itself. On this subject we have little of value to offer. We are unable to offer definite evidence as to the genuineness of the trance, for the not inadequate reason that no conclusive methods of determining this point exist. The method, quite inadequate moreover, of testing the subject's anaesthesia, could not be tried without greatly endangering the object of the sittings. So far as it goes evidence of this kind incidentally obtained tends to suggest that the trance is genuine. During the violent movements in which the subject indulges in his trance he frequently administered tolerably severe blows on various parts of his anatomy, without ever giving much sign that he was even conscious of pain. But as the subject in his normal state (as when playing football), has been seen to experience without complaint even harder blows, this evidence is not particularly convincing.

There remains the problem of the subject's abnormal breathing when in trance. This breathing is an invariable concomitant of the trance, and has the appearance of beginning quite suddenly at a rate of about 60 to 100 cycles per minute. Our impression is, however, that this sudden beginning is an illusion due to the noisiness of the breathing when it reaches this point. What actually happens appears to be this: the subject settles down and puts himself into a favourable attitude¹ of body and mind; after about four to seven minutes, tremors begin in his extremities; at the same time the rate of breathing slowly increases with the increasing violence of his muscular movements; at a given moment the rate of breathing becomes too great to be conveniently carried out by normal means; and at this moment the subject opens his mouth and begins the noisy breathing so characteristic of his trance.

¹ What this is is not known, as the medium is unable to say.

From this point the rate of respiration increases and varies up to a maximum of 240 cycles per minute in the present sittings, though higher rates have been recorded elsewhere. The question therefore arises whether this breathing is in itself an indication of the genuineness of the trance, on the ground that it is impossible to breathe normally at this rate without losing consciousness. This statement, at least, can be answered definitely in the negative.

Dr C. G. Douglas kindly assisted in the collecting of samples of the medium's breath in Douglas bags and their subsequent analysis.

Four samples of expired air were collected as follows :

Case I. 15.5°C. and barometer at 751 mm. The medium sitting resting in light and out of trance. This was three-quarters of an hour before Case II.

Case II. 15°C. and 756 mm. Collected 2 minutes 52 seconds after the beginning of the panting.

Case III. 15°C. and 756 mm. 7 mins. 23 secs. after beginning of panting.

Case IV. 15°C. and 756 mm. 13 mins. 30 secs. after beginning of panting.

In Cases II-IV inclusive the room was dark and the medium was in trance.

Case II shows slight over-breathing. Case III shows a moderately large rate of oxygen consumption if the time 0'46" instead of 1'46" is correct. This figure corresponds to a man walking at some 4 miles per hour, and since the medium was in fairly violent motion at the time this high figure is in no way excessive.

TABLE I

Case	Vol. in litres	Vol. at S.T.P. in litres	Time	No. of Breaths	Breaths per minute	cc. of room air per breath	ccO ₂ at S.T.P. per min.	$\frac{\text{CO}_2}{\text{O}_2}$ at S.T.P.
I	48.5	44.9	7'33"	111	14.7	436.9	274	0.80
II	55.35	51.3	2'8"	?	?	?	340	1.14
III	52.92	49.9	0'46"	215	?280	246.2	?1410	0.87
			?1'46"	215	?122	246.2	?611.9	0.87
IV	71.74	66.5	4'34"	831	182	86.32	223	0.93

Case IV is normal for ordinary mild exercise as far as the respiratory coefficient is concerned and the breathing is definitely shallow.

No measurements were ever secured while the "force" was doing work because it never was observed to do work. The above figures suggest that the medium's breathing is one that gets shallower as the

rate of respiration increases. During a sitting short periods of apnoea are observed, but these do not alternate regularly with the breathing and so there is no Cheyne-Stokes breathing. Moreover, the medium shows no signs of distress at the end of a sitting.

A still more conclusive proof of the normal nature of the subject's breathing in trance is the fact that we have both succeeded in producing exactly similar effects during similar periods of time.

The figures for the total quantity of air breathed may be slightly too small since there may be some leakage of the valves at the more rapid rates of respiration recorded. This error, however, would not affect the composition of the exhaled gases. Even a 50% leakage through the valves would still leave the figures at perfectly normal values, so it must be concluded that there is nothing paranormal about the respiration of the medium in trance.

This is a convenient point at which to insert a medical report kindly made for us by Dr Mauriee B. Wright :

Age : 26.	Height : 5 ft. 5 $\frac{1}{4}$ in.
	Weight : 10st. 6 $\frac{1}{2}$ lbs.
Head Measurements :	Length : 15.4 cms.
	Breadth : 11.4 cms.
Hand Grips :	Right hand, 280 lbs.
	Left hand, 180 lbs.
Blood Pressure :	143/85 mm. of Hg.
Pulse :	89 per minute.
Chest Expansion :	2 $\frac{1}{2}$ ins.

Healthy appearance, stocky build, good musculature. No stigmata of degeneration. Good profile. Ear shape good. Heart and lungs normal. No evidence of nervous disease. Deep Reflexes rather exaggerated. Eyes normal. In every way a normal-looking individual. Rather nervous under examination which would account for rather high Blood Pressure and pulse rate, also the increased reflexes.

Finally, the trance cannot be compared with any known form of *e.g.* hypnosis, since there are no hard and fast categories of hypnotic states, whatever the early workers in this field may have supposed. Even if there were, any useful comparison would be impossible, since (*a*) the trance never takes place otherwise than in approximate darkness, thus making visual inspection impossible, and (*b*) since the tonic movements of the subject and his noisy breathing mask other less obtrusive features that may be characteristic of the trance.

RUDI AND OLGA

There remains one possible indication as to the genuineness or otherwise of the trance, which leads us directly to the consideration

of the personalities themselves. This is the question of memory : are the memories of Rudi and *Olga* co-extensive ? Obviously a definite answer is impossible, since, as Mrs Salter has well said in a somewhat similar context, " It is very much easier to discover that a man does not know a language when he is pretending he does, than to discover that he does know it when he is pretending he does not." ¹ At the same time we *believe* that Rudi's amnesia in regard to *Olga* is complete. This belief is based on Rudi's behaviour when in his normal state, in which he never gives the slightest indication of any direct knowledge of what goes on when he is in trance. This being so we are also inclined to believe that the subject's trance represents a genuine loss of normal consciousness ; but we are neither able nor do we wish to put this any higher than a mere belief, though it is admittedly a strong one.

We do not, however, feel confident of *Olga's* amnesia in regard to Rudi's experiences. *Olga* herself, to say nothing of Rudi, behaves as if there were no " correspondence " between them. This is manifestly illogical, for if *Olga* is a spirit who knows what is going on in the physical world, as she claims, why should she not be aware of, at least, the subject's physical activities ? The same difficulty applies to *Olga's* alleged ignorance of Rudi's mental activity, since she frequently claims to know what others are thinking of. Nor is evidence wanting that *Olga* does in fact know of Rudi's doings and thoughts. Thus, to take only one example, *Olga* sent messages to Rudi not to be unhappy at the death of his father. Again, it is frequently noticeable that *Olga* insists on asking for special sittings when these will interfere with arrangements already made by Rudi ; this is less direct evidence, but none the less convincing.² In short, we have little doubt that there is no amnesia by *Olga* regarding Rudi.

This kind of one-sided amnesia is by no means uncommon, and it unfortunately solves no problem. For Rudi's amnesia may be, and very probably is, no more than the expression of a belief. If Rudi believes that he cannot or ought not to remember *Olga's* experiences, this belief is quite sufficient to account for the fact that in effect he does not remember them. This naïve causality of the mental life increases by a hundred-fold the difficulty of most of the tasks of psychical research.

We then arrive at the heart of our present problem, the true nature of *Olga*. If Rudi loses consciousness on going into trance and if he does not afterwards remember what he did in trance, then clearly *Olga* and Rudi cannot be regarded as the same personality unless we give a quite unjustified extension to our terms. There are thus two

¹ Mrs W. H. Salter, " The History of George Valiantine," *Proc. S.P.R.* (1932), xl. 404.

² This kind of antagonism is fairly characteristic of dual personalities.

main possibilities : (1) that *Olga* is, as she claims to be, the spirit of Lola Montez ; (2) that she is a more or less discrete part of the total Rudi personality.

(1) The first of these possibilities it is difficult not to dismiss out of hand. The full spiritualistic theory of the survival of the " whole " personality is certainly not acceptable, for *Olga* clearly does not behave in a manner characteristic of Lola Montez. Thus, to take only one minor example, she does not speak English although the Montez did so. Even if we hold a minimum theory of survival, *e.g.* the persistence of a small number of characteristic memories, it would be hardly possible to accept *Olga* as Lola Montez, for she never in our presence gave the slightest indication of the possession of any such memory.

(2) Thus we can only inquire whether *Olga* is *different* from Rudi, or at any rate more different than a mere secondary personality can be expected to be. This point, obviously, can only be settled by the inspection of various kinds of *behaviour*, and this we have done by applying the word-association and the psychogalvanic reflex tests to each personality separately and by comparing the results.

The psychogalvanic reflex portion of this experiment has already been reported on by Mr Carington (*Proc. S.P.R.*, xlii.), who shows that Rudi and *Olga* are similar to such a degree that they must be regarded as being the " same ". As Mr Carington also points out, it seems that Rudi did not frankly co-operate in regard to the word-associations. There is evidence to show that he systematically prepared responses instead of uttering the first word that came into his head. This strongly suggests that he was afraid that free responses might betray something he wanted to keep to himself. A clue to the nature of this " something " is possibly to be found in the stimulus-response Schwindel-schön (Fraud-good) which occurred about half-way through the first session ; too much weight must not, however, be placed on a single response of this kind, nor should it be forgotten that this response may have been motivated by an old and superseded state of mind. At any rate, after this point there is practically nothing in the nature of a genuine response. It is with regret that we are obliged to draw attention to this fact, for it is impossible to praise too highly Rudi's straightforward behaviour throughout the ordinary sittings and his willingness to submit to every suggested test.

In these circumstances the word-association test is almost useless for our present purpose, and there only remains one feature of the *Olga* responses to be discussed. In the ordinary sittings *Olga* does not speak much ; indeed, the medium's rapid breathing makes normal conversation impossible. Her remarks are limited to instructions regarding the conduct of the sittings, and the vocabulary used for this purpose is naturally limited. Now from the word-association

sessions with *Olga* the curious fact emerged that *Olga's* vocabulary seems to be wholly limited to the words used by her in the ordinary sittings (her responses, like Rudi's, had no relevance to the stimuli). The extent of her vocabulary is only a little more than half the normal.¹

From the presence of such frequent repetitions of particular responses as 14 (Acht, Achtung), 25 (alle), 18 (Freund, etc.), 14 (Licht), 11 (lustig), 19 (nicht), 15 (schliessen), 25 (sprechen), 13 (Strahlen), 15 (zählen), and from the large number (70) of words occurring only once, it may reasonably be inferred that there is no consistency or definiteness in the word-responses. For such consistency would produce results showing a few words occurring only once or twice, very few, if any, occurring as often as those enumerated above, and a large number occurring between three and six times. Analysis of the lists confirms this inference, for the fact emerges that there is no correlation between given stimuli and given responses. This must necessarily be the case for the words that occur most frequently; but the following table shows that it is equally true of the few words occurring between three and six times.

TABLE II

Olga's responses given between 3 and 6 times.

<i>Response</i>	<i>Stimuli</i>
Berührung	wählen, Engel, bringen, Kopf
Bewegung	Spott, richtig, kochen, Kopf, Schiff
Ende	neu, Automobil, Spott
gut	Knochen, stolz, böse, Hund, Licht
helfen	lang, vornehm, Kuss, Wasser
Himmel	Berg, Gebirge, Gebirge
kabinett	Türe, Blume, waschen, Strasse
Kreis	Schiff, kochen, malen, kochen, kochen
Nähe	Strasse, Angst, falsch, drehen, Frau
rot	drohen, Brot, drehen, Brot, Lampe
schwarz	weiss, schwarz, Frau
Streifen	Nadel, Buch, hell, Teil, Türe, Knochen
tief	spielen, Teil, Pflaume
viel	Frau, schlafen, Vorsicht, trüb
Vorhang	Baum, Pflicht, krank, Schwindel, Tinte, Monat
zeigen	wählen, Schlange, krank, Apfel

It is thus clear that the limited vocabulary produced by *Olga* in her responses is not due to her possession of a consistent personality, that is, one frequently giving the same or similar responses to given stimuli. The responses to only two of the above words, indeed, Himmel and Kreis, show any signs of consistency. Berg and Gebirge (the use of these synonyms was due to an oversight in the compilation

¹ A complete table of the words used by *Olga* in her responses is on record at the Society's rooms.

of the lists), elicited the following responses in addition to Himmel : hell, frei, laut, besser, Höhe. It is thus a reasonable inference that the notion of mountains produces a pleasurable reaction in *Olga*. Kochen, which three times elicited Kreis, was also duplicated by accident in drawing up the lists, and produced the following responses : zergangen, Abteilung, bewegen, bisjen sprechen, ja, Freund. Hence the recurrence of Kreis may be simply due to the fact that kochen was presented twelve times instead of six. If there is any significance it is possibly due to a mishearing of Kreis as Reis (rice), a favourite dish of the medium's.

Berg-Gebirge, then, being the only stimulus-word which elicited a characteristic response from *Olga*, it is interesting to compare the results obtained with Rudi. These were as follows : Garten, Kopf, bewegen, weinen, kalt, Himmel, grausam, fein, Glaube, Bild, Baum, Himmel. These responses are about equally similar and dissimilar to *Olga's*, the two occurrences of Himmel being particularly notable. However, as we have already decided that the Rudi responses cannot be taken too seriously it will be wiser not to press this point.

All these facts, fragmentary as they are, point to the conclusion that the trance is genuine and that *Olga* is a personality artificially induced or suggested for the purpose of the sittings and hence limited in its scope to its function as a trance-personality.

THE INFORMATION AVAILABLE TO OLGA

It has been alleged that the trance personality is aware of what goes on in the dark *séance*-room. In these sittings the medium in trance still claimed these powers and was put to the following test. One of the sitters (O. G.) put his hand in one of three positions, "Up," "Forward," or "Down", these positions being checked by the touch of the sitter next to him. The medium in trance in the darkened room made twelve guesses at the 38th sitting on 6 February 1934 at this one in three chance and scored four successes. The result was not considered good enough to make it worth a more detailed investigation. As recorded below *Olga* was entirely unaware of the strength of the infra-red beam in the thermopile apparatus. On the other hand she said when first questioned about *Feda*, who was asked, at a sitting with Mrs Leonard, to get in touch with *Olga*, "She has been here tonight." This tallied with what *Feda* had said. All attempts at a cross-correspondence, however, failed.

THE INFRA-RED INVESTIGATION

No evidence of absorptions of a beam of infra-red light of the type recorded by Osty¹ and Herbert² could be obtained, notwithstanding

¹ E. and M. Osty, *Les pouvoirs inconnus de l'esprit sur la matière* (1932).

² *Proc. S.P.R.* (1933), xli. 259.

frequent announcements by the trance personality that the "force" had entered the ray.¹

The intensity of the infra-red light was measured by means of thermopiles and a photo-electric cell. The former were used to detect possible absorptions in the far infra-red beyond the range of the photo-cell and the latter was used in order to look for absorptions of short period. Since no effects were observed the instruments were not calibrated and their exact sensitivity is therefore unknown. Two thermopiles were used, (a) a Moll linear thermopile of approximate resistance 22 ohms which is advertised to give an electromotive force of 24 microvolts to a candle placed 50 centimetres from the plane of

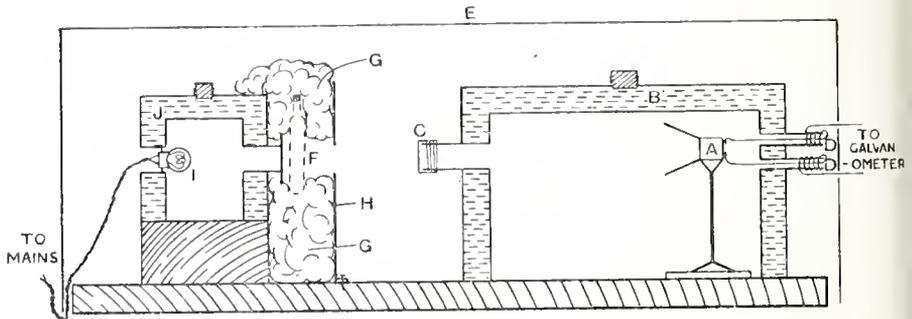


FIGURE 3.

- A. Thermopile.
- B. Double-walled water filled tin box.
- C. Red gelatin filter spliced on to box.
- DD. Entry to thermopile box with wires spliced on to tubes. The wires are encased in rubber tubing.
- E. Box covering whole system.
- F. Filters.
- G. Cotton wool.
- H. Screening plate.
- I. Source of light.
- J. Water-jacketed copper oven.

the thermopile with a 1 mm. slit. The corresponding electromotive force for the blackened surface of a Leslie cube $10 \times 10 \times 10$ cms. at 100°C . is 22 microvolts, both figures referring to the thermopile at 20°C . (b) A large surface Moll thermopile of approximate resistance 50 ohms which operated without a slit and is advertised as giving under the above conditions 2200 microvolts to the candle and 2000 microvolts to the black surface. The instruments were connected in series to a Moll galvanometer of undamped period of 1.3 sec., which gave approximate deflections at one metre of 200 mm. per micro ampere and 4 mm. per microvolt, having an approximate resistance of 50 ohms and a factor of merit of 2,500.

¹ This statement rests on the unsupported statement of O. G., who visually observed the recording apparatus.

One of the thermopiles (A, Fig. 3) was placed inside a double-walled tin box (B) which was filled with water, so as to constitute a water-jacket, and was lagged on the inside with a layer of felt padding 2 cms. thick. The radiation was allowed to enter this box by a red gelatin window (C). In this way the thermopile was protected from stray heating effects and draughts and its two brass terminals were given a maximum opportunity of coming to the same temperature or to a steady difference of temperature. The latter condition is important to minimise stray thermo-electromotive forces due to inequalities in their temperature. The thermopiles stood on a plate of glass, coated with paraffin wax to prevent the appearance of a conducting film of moisture; and the latter stood on an earthed sheet of metal. The water-jacketing box (B) was also connected to earth. The leads from the thermopile were all single strands of copper wire from double cotton insulation and these were encased in rubber tubing. They entered the box B by two special holes (D D¹) which were plugged with cotton wool to prevent the entry of draughts and the wires were firmly spliced to the tin tube filling these holes. This precaution was to make it impossible to move the thermopile by pulling on these wires since they were not strong enough to move the whole box B. This procedure ensured that any stray vibrations in the room set up by the medium's breathing or for other reasons could not produce deflections in the galvanometer. This precaution was not sufficient, however, to render the galvanometer reading insensible to someone deliberately pulling the leads. Since the wires were on the far side of the apparatus to the medium this was deemed adequate as a preliminary precaution, and the maximum effect observed during control experiments when the leads were pulled deliberately was only 4 mm. on the galvanometer scale.

The leads were carried on an equipotential railway, of the type advocated by White¹ and by Maass,² so that they came in contact only with glass rods which were supported on bits of metal that were all connected to earth. As a further precaution the rods were fitted with a solution of rubber and paraffin wax in carbon tetrachloride to prevent a conducting film of moisture appearing on them. The leads were twisted round several of the glass rods so as to prevent mechanical shocks to the wires from transmitting forces to the galvanometer which was directly attached to one lead. The other lead came to an anti-thermo-electric switch of the type used by one of us (O. G.)³ in work on adiabatic calorimetry.

The principle is to make contact by dipping two spirals of copper

¹ White, *Jour. Amer. Chem. Soc.*, 1914, 36, 1862.

² Lipsett, Johnson and Maass, *Jour. Amer. Chem. Soc.*, 1917, 49, 935, 1940.

³ Gatty, Wolfenden and others, *Jour. Chem. Soc.*, 1934, 561.

wire into a cup containing mercury. The cup is drilled in a sheet of ebonite that stood upon an earthed sheet of metal, the metallic ends of the spirals were amalgamated by dipping into a saturated solution of mercury chloride to ensure a contact where the mercury wetted the copper, and the upper ends of the spirals were shellaced over thin cotton insulation so as to ensure thermal equilibrium with the mercury before electrical contact was made. A finger inserted into one of these mercury cups produced a deflection of 1 cm. disappearing in 10 seconds to zero, so they can be said to be adequately immune from stray heating effects and also from bad contacts. At the other side of this switch the lead was carried on an equipotential railway and inside its rubber tube to the galvanometer.

The galvanometer box was a double box packed with cotton wool between its two walls and the leads were led into it through glass tubes shut in with sealing wax and so forming a draught-proof entry. The front of the box was a plain sheet of glass. The two brass terminals of the galvanometer were therefore in a box that was thoroughly well shielded, and so stray thermo-electric effects were reduced. The galvanometer itself stood on a 10 cm. pile of alternate sheets of glass (2 mm.) and layers of paper approximately 1 cm. thick. In this way insulation and freedom from vibration was secured. The whole box was placed on a sheet of earthed metal and the two were nailed to the table in the observer's cabinet. In this way deflections due to a person jumping in the *séance*-room were found to be dead-beat and of the order of 8 mm., falling in 2 seconds to zero. The galvanometer reading was observed on a scale at approximately 1 metre, the exact position being adjusted each time the observer (O. G.) set the apparatus going. This procedure was necessary in order to allow room for the observer to enter the observation cabinet, which had to occupy a minimum space in order to allow the trance personality to have her cabinet of suitable shape and sufficient size.

The source of light used in the thermopile system was a 240 watt lamp run through a rheostat off the 100 volt direct current mains. This powerful source was put in with a view to a possibility of wishing to do spectrographic work with the linear thermopile. It was enclosed in a double-walled water-jacketed copper oven which was connected to earth. In the second series of experiments the voltage across this lamp was read on a Cambridge Paul Universal Unipivot galvanometer and by use of a calibration curve all readings could be corrected for the voltage across the lamp. The energy falling on the thermopile after passing through the filters was found to vary approximately as the square of the voltage. Data for one calibration curve are given in Table III.

TABLE III

Calibration of Ray against Voltage

Thermopile Reading	Voltmeter
172	100.2
148	90
122.5	80
97	70
76	60
55	50
38	40

All visible light was excluded from the beam by a sheet of ebonite of thickness 0.005 em. or by a filter of 1 em. of a saturated solution of iodine in carbon disulphide in a glass vessel, or by both. The latter filter was used because the photographic work of Rayleigh¹ and Herbert¹ indicated that absorptions, unless of extreme rapidity, did not occur at wave lengths shorter than 1μ while Herbert and Osty using photo-cells both recorded absorptions. No alternating current circuits ran close to the thermopile system, and therefore screening of the circuit was unnecessary. The same holds also for the Moll galvanometer, as it is a moving coil instrument.

Osty, doubtless relying on the early work of Lange on copper oxide photo-electric cells, claimed that his cells were sensitive up to 6μ and the iodine solution was therefore selected because Coblenz gives for it the following transmissions :

TABLE IV

Wave Length	Transmission per cent.
0.75μ	0
1.0μ	80
$1.2.5\mu$	90
3μ	60
4μ	10
5μ	0

Since only the ebonite filter was used, the range of incident radiation must have been extended much farther into the infra-red.

It is evident, however, that the range up to 5μ given with the solution filter alone is adequate, since a careful calibration at the National Physical Laboratory of one of Osty's cells taken over by one of us (Th. B.) from Paris gave the following figures. The sensitivity at a series of wave lengths is given in Table V expressed in milliamperes per watt of incident radiation falling on a circular patch, 2 em. in

¹ *Proc. S.P.R.*, Lord Charles Hope and others, *loc. cit.*

diameter, centred with respect to the ruled guide on the surface of the cell, which is connected in an external circuit whose resistance is of the order of 30 ohms. The sensitivity becomes negligible at a wave length of 1.800μ and the spectrum, though explored down to 3μ , showed no discoverable recurrence of sensitivity with the energy available in the spectroscopic system. This is to be contrasted with a lower limit of 1.4μ in the case of the best caesium photo-electric cells.

TABLE V

Wave length (in μ)	Sensitivity (in milliamps. per Watt)
0.586	0.001
0.590	0.007
0.595	0.021
0.600	0.065
0.605	0.180
0.610	0.306
0.615	0.433
0.620	0.481
0.625	0.492
0.630	0.488
0.635	0.420
0.640	0.285
0.650	0.206
0.660	0.176
0.680	0.142
0.700	0.115
0.725	0.096
0.760	0.085
0.800	0.092
0.850	0.114
0.900	0.131
0.985	0.141
1.050	0.136
1.100	0.123
1.200	0.090
1.300	0.054
1.400	0.025
1.500	0.011
1.600	0.005
1.700	0.001
1.800	0.0005

The thermopile system described above gave a 40% drop in its total deflection for an absorption occupying $\frac{1}{2}$ second and was frequently steady enough for deflections of less than $\frac{1}{2}\%$ to have been detected. It is therefore claimed that the thermopile system was both sensitive

enough and rapid enough to detect absorptions of the type previously recorded and the surface density of illumination was kept low, as this is supposed¹ to increase the chance of observing absorptions. Full records of the actual results obtained at all the sittings are available at the Society and specimen records are given below. In certain cases the fact that all possibility of absorption was less than 2% has to be left to the second by second observations by one of us (O. G.). In some cases the force is said to enter the ray and the observations given whenever the spot moved by 1 mm. are such as to show the reader that on that occasion any deflection corresponded to less than $\frac{1}{4}\%$. In one case and only one case there appeared to be a genuine absorption > 3% and it seemed to amount to a 16% absorption. On this occasion it was subsequently discovered (1) that water was leaking out of the water-jacketed box surrounding the thermopile, (2) that one of the thermopile terminals was touching a moist piece of felt inside the box. In view of the failure to repeat this one "absorption" on any other occasion throughout the whole series of sittings this one result is attributed to experimental error. Thus the result when contrasted with 80% absorptions alleged to occur during 10 minute intervals² are seen to be *definitely negative*. In view of the absence of positive results and the trouble of simultaneously recording the voltage of the mains and the galvanometer readings on a photographic plate all these observations were dictated through to the secretary in the office. Whenever the spot shifted across one of the millimetre lines on the scale the change if observed was recorded, though occasional exceptions were made to this rule when the readings seemed of trivial importance as compared with alleged movements of curtains.

¹ E. Osty, *Supernormal Aspects of Energy and Matter* (1933), pp. 18, 30.

² Osty, *loc. cit.*

TABLE VI
Twentieth Sitting, 21 November 1933

Time	Thermopile	Zero	Remarks by <i>Olga</i>
6.10	6	-35	
6.32	15		
6.34			
6.35	15 steady		
6.37	17		
6.40	18		
6.42	18		
6.43			
6.43½	18½ steady		
6.46	19		
6.56	21		
7.1	21		
7.1½			
7.2	21 steady		
	21½		
7.4	22		
7.5	22		
7.5½			
7.6	22¼ steady		
7.8	23		
7.9	23		
7.11		-36	

In this case a steady warming-up of the linear thermopile is evident. A slit of approximately 1 cm. diameter was used and the solution of "iodine" in carbon bisulphide as filter. The time when the ray was used last before this sitting both filters were being used simultaneously and the beam was therefore less intense. Hence *Olga* was in error in stating at 7.6 that the beam was hotter than on the last occasion when correspondingly lower thermopile deflections were obtained.

TABLE VII
Forty-fifth Sitting, 27 February 1934

Time	Thermopile uncorrected	Voltmeter	Thermopile corrected	Zero	Cold reading
6.15	128	99.2	127.8	- 50	- 42
6.16½	125				
6.18	124				
6.19½	125				
6.23	124	98	128		
6.25	126				
6.27	125				
6.28	126				
6.29	125				
6.30	124				
6.32	123	97	129½		
6.37	123 steady		"Look out"		
6.41	125	97.5	129½		
6.43	127	98	130		
6.48	128	98.5	130		
6.53	129	99	129		
6.54½	130				
6.55½	131				"I'm going into the ray."
6.56	131				
	130				
6.57½	131				
6.58	132	100	"Does my friend see anything?" 129		"No"
6.59½	131				
7.0	132				
	131				
	132				
7.1	130				
	131				
7.2	131				
7.3					"I have taken the force, it is in the cabinet."
7.8½	130	100.2			
7.9	131				
7.10	126	98.2			
7.14	129	99.2			
7.18	127				

The corrected thermopile readings correspond to a voltage of 99 and are equal to $\frac{(99)^2}{(\text{voltage})^2} \times (\text{uncorrected reading})$.

TABLE VIII

Fifty-first Sitting, 20 March 1934

Time	Thermopile uncorrected	Voltmeter	Thermopile corrected to V = 109	Cold reading	Zero
8.42 $\frac{1}{2}$	120	?	?	-31	-41
8.44 $\frac{1}{2}$	121	106	129 $\frac{1}{2}$		
8.46 $\frac{1}{2}$	125	107	131		
8.53 $\frac{1}{2}$	128	108	131		
9.6	128 and steady				
9.12	129	109	129		
9.25	128	108.9	128		
9.29	127				
9.30	127	108.7	127		
9.34	127				

SECOND PART corrected to V = 108

9.54	139				-23
9.56		107			
10.1	135				
10.3	134	105	142		
10.11 $\frac{1}{2}$	139 steady	"All count to five"			
10.13		107			
10.23	139	108	139		
10.30	139	"It is going into the ray"			
10.31	139 steady				
10.32	139 steady	"Look out, the power is going into the cabinet"			
10.46	139 steady				
10.50 $\frac{1}{2}$	139 steady				

The figures for the 51st sitting show that if the power on that occasion was exerting an absorbing action on the ray it was less than $\frac{1}{2}\%$. At this point it is interesting to point out that the alleged transparency of the "force" to visible radiation since it cannot be photographed can hardly be reconciled conventionally with the view that this radiation is capable of harming the force. Any harmful effect would be expected to become apparent only when some radiation was absorbed. Perhaps the radiation is to be supposed injurious only to the medium in tranee and not the force; but this alternative is hard to reconcile with (a) the statements made by the medium in tranee, and (b) what would be expected on general physiological grounds.

TABLE IX

The apparent absorption of the 3rd part of the Twenty-ninth Sitting, 12 December 1933.

Time	Thermopile	Zero
9.22 $\frac{1}{2}$	—	-78
10.36	60	
10.37	"The power is now going into the table. Look out."	
10.39	60-59 Kicks repeated seven times. Then to 61 and then 60	
10.40	61	
10.41	61 kicked to 60.	"Something is coming in"
	60	"Now"
	61	
10.42	61 $\frac{1}{2}$	"Hold tight"
	62	
	62	
10.43		"Hold tight"
10.44	62 and unsteady	
10.46	62-63, a kick and then back to 62	
10.46 $\frac{1}{2}$	63	
	63	
10.47	64 "The force is now going again into the ray"	
	63 $\frac{1}{2}$	
	63 $\frac{1}{2}$	
10.48	63	
10.49	63	
10.49 $\frac{1}{2}$	63-62, a kick	
	62	
	63	
	62	
	63	
10.50	63-61 in a single kick	
	63	
	62	
	62	
	62 $\frac{1}{2}$	
	62	
	63	
	62	
	63	
	63 $\frac{1}{4}$	
	63 $\frac{1}{2}$	
	63	
	63 $\frac{1}{2}$	
10.51	64	
	65	

Time	Thermopile	Zero
	64	
	65	
10.53	64½	“ Look out. The power is now going into the curtain ”
10.54	65-58, a quick drop	
	59	
	59	
	60 steady	
	59½	
	59	
10.55	59	
	60	
	59	
	58	
	57½	
	57	
	57	
	58	
10.56	57	“ The force is going back ”
	58	in a kick it reached this value
	58	
	57	“ Look out ”
10.57	57	
	56	
	57	
10.58	57	
	58	
10.59	58	
	59	
11.0	59	
	57	
	58	
	59	
11.1	59	
	60	
	59	
	60	“ Olga says the force has been in the ray all the time ”
11.4	60	
11.5½	61	
	61½	
	62	
	61	
11.6½	61, kicked to 60 and back to 61.	“ Hold tight ”
	61	
	62	

Time	Thermopile	Zero
11.7	62 62 $\frac{1}{2}$ 62	“ Hold tight ”
11.8	62 $\frac{1}{2}$ 62 63	
11.10	62	“ The power is now all out ”
11.11	61	“ Olga is going in again ”
11.11 $\frac{1}{2}$	62-61,	a kick and now steady
11.12 $\frac{1}{2}$	61	“ Hold tight ”
11.13	62	“ Hold tight ”
11.14	62	
11.15	61 62 62 61	
11.18	62-63	a kick. “ Olga says have a pause ”
11.19	63-66	a kick
11.20	67	
11.22	63	End of breathing
	63-66	in one kick
11.23	63	End of trance
	62 63	
11.25		Zero = -78

In this sitting exact timing of the rapidly fluctuating spot was impossible since all records were dictated first by O. G. to Th. B. and then, at once, by the latter to the office by telephone. Movements such as were obtained on this evening were never observed at any of the other sittings. It is concluded that they are due to faulty insulating owing to water escaping into the thermopile box and one of the terminals of the latter touching a moist piece of felt that was glued to the metal box. This source of error was due to faulty soldering of the walls of the box, which was moved during the week by the caretaker. The error, however, was detected and corrected before the 30th sitting in January. Owing to the results of this evening a voltmeter to measure the voltage of the mains was installed for the next series of sittings to check independently the energy being emitted by the source. The observed fluctuations of voltage in the next series of sittings were neither large enough nor rapid enough to account for the results of the 29th sitting. These are therefore attributed to faulty insulation and are highly significant in showing the errors liable to arise from minor flaws in the set up of the apparatus.

The actual arrangement of the beam, which was entirely unfocussed, is illustrated in Figure 3. The whole apparatus including the source and the thermopile was enclosed by placing a wooden box E over it. This box and the stand supporting the thermopile and the source were nailed to the floor. They were both covered with a black cloth that was screwed to the floor by pegs through which was run a fine steel cable that was also earthed. In this way the only access to the ray was a hole opposite the opening between the two portions of the cabinet curtain. The diameter of their aperture, which was loosely closed with a piece of muslin cut into four pieces that could each be moved back with the lightest pressure, was varied from 20 cms. to about 5. At times the muslin was completely removed and in all cases the medium in trance announced that the "force" had no difficulty in getting into the ray. The ray itself was feeble enough to be suitable, according to the trance personality, which however was found to be in error when tested on one occasion, to see if it was aware of the relative strength of the beam at two different sittings (see the 20th sitting above). An electric stove placed near the thermopile outfit but unable to shine directly in through the window of the thermopile box produced no appreciable deflection of the galvanometer in a period of two minutes.

The photo-electric cell used was a copper-copper oxide cell of type PA₁ as sold by the Westinghouse Brake and Saxby Signal Co., generating a current that is given in milliamperes per Watt of incident radiation in Table X.

TABLE X

Wave length (in μ)	Short circuit, milliamperes per Watt of radiation
0.6	0.04
0.625	0.55
0.635	1.36
0.65	0.91
0.70	0.77
0.75	0.73
0.80	0.73
0.85	0.71
0.90	0.66
0.95	0.60
1.0	0.53
1.1	0.39
1.2	0.27
1.3	0.18
1.4	0.10
1.5	0.05
1.6	0.02
1.7	0.005

The cell has an approximate resistance of 1800 ohms and was connected in series to a Cambridge Instrument Co. portable electrocardiograph Einthoven galvanometer having a 1400 ohms gilt glass fibre. With this external resistance the cell should be working near its maximum efficiency, since 1400 is not very different from 1800 ohms. The source of light was a torch bulb run off a 6 volt accumulator and the filter used was a 1 cm. saturated solution of iodine in carbon disulphide in a glass vessel. The cell was rested on glass and paper to insulate it and free it from vibrations, and was only some 15-20 cms. from the source of light. Both were enclosed in a rectangular muslin box whose edges were ribs of wood. The box was nailed to the floor and a hole 10 cms. in diameter in the muslin was made on the side of the box next to the cabinet. This box was close to the medium's chair, which was also screwed to the floor, and only rose some 10 cms. from the floor. The force apparently had no difficulty in entering this ray either, if the words of the medium in trance are to be believed. Photographic records were not taken since nothing was seen when the naked eye of the observer watched the shadow of the fibre. It is estimated that an absorption lasting only some 0.04 seconds would have been observed however in this way. The string galvanometer cannot be kept as steady as the Moll galvanometer spot because the tension of the fibre is a function of the temperature of the fibre and its carrier. If used for long periods at a time the instrument warms up and the tension of the fibre varies and has constantly to be readjusted. The total deflections amounted in some cases to 81 mm. and an absorption of 1% should have been detected with certainty by this method. The precision and the steadiness could have been increased by being more careful to protect the Einthoven galvanometer from vibrations. The latter from time to time caused small deflections which occurred when the force was not in the ray according to the trance personality. A number of figures are given below to show that no absorptions were observed in this ray.

TABLE XI

Cell Readings for the second part of 34th Sitting on Friday, 26 January 1934.

Cell zero - 26

8.53 p.m.	cell = 18 $\frac{1}{4}$	and "Force is about to go into ray"
8.54	cell = 18	"Look out"
8.54 $\frac{1}{2}$	cell still = 18	
8.55	cell = 18	
8.56		Olga "I have been in twice"
8.57	cell = 17 $\frac{1}{2}$	
9.0 $\frac{1}{2}$	cell = 17	Olga "Now I am going in again"
9.1 $\frac{1}{2}$	cell = 17	

TABLE XII

Cell Readings, 36th Sitting, 30 January 1934

Cell zero—plus 1

8.22	“ The force is coming out to move the curtain and to enter the new ray ”
8.24	Cell steady at 22
8.25	Cell steady at 22
8.29	Cell steady at 23

Similar results occurred on the other occasions when the “ force ” was said to enter the ray, and occasionally a kick when the observer said *fünf* owing to the vibrations he himself set up. These kicks could be set up without the presence of the medium. One observed in the 36th sitting was :

TABLE XIII

9.32	cell = 11½
9.33½	cell = 12
9.35	cell = 12
9.35½	“ Count to 5 ”
	At the word <i>fünf</i> a kick from 12-13
	Everyone said <i>fünf</i> together without <i>Olga's</i> instructions
	The reading kicked from 12-13 1/4

The maximum deflection ever obtained with the cell was 81 units different from its zero. Thus a deflection of 1% should have been detectable but was never observed in conditions that made the deflection seem to have its origin with an absorption owing to the “ force ” from the medium.

TEMPERATURE TESTS

Two small wooden boxes were fixed to the wall of the cabinet where *Olga* collected the “ force ” and an ordinary glass drinking tumbler was put into each of them and was tightly packed into the box with cotton wool. A piece of 32 gauge constantan wire was soldered to the ends of two copper leads and one junction was put in one tumbler and the other junction in the other tumbler. The leads and also the constantan wire were encased in rubber tubing except at the thermo-junctions. They were carried on an equipotential to an antithermo-electric switch of the type described before and thence to the same Moll galvanometer as was used in the thermopile circuit. The circuit contained only three single strands of copper wire, the switch, the galvanometer, and the constantan wire. The insertion of a hand into the upper part of either tumbler rapidly produced a deflection of three or four centimetres on the galvanometer scale.

The sensitivity of the apparatus was calculated from the thermo-electric force per degree centigrade of a copper constantan thermojunction which is about 38×10^{-6} volts per degree centigrade and the resistance of the circuit which was roughly 80 ohms. In consequence 1 mm. on the galvanometer scale should correspond to some 0.01°C . During a sitting the lower box slowly became cooler, but this occurred during control experiments as well. The force on several occasions was announced by the medium in trance to have gone into the lower box and it stayed there for several periods of 20 minutes. On no occasion was any change of temperature detected. These results, some of which are given below, agree with the fact that measurements with the thermopile when the source of infra-red light was turned off also showed no heating or cooling effect when the "force" was said to be where the beam of infra-red light usually was.

TEMPERATURE EFFECTS. Zero - 44, Reading - 12

The electric stove was put in the cabinet and turned on.

After 1 minute reading was - 14.

After 2 minutes reading was - 16.

After 3 minutes reading was - 19.

A fall in reading corresponds to an increase in temperature of the lower box relative to the upper box, as was shown by inserting a hand into the tumbler.

TABLE XIV

Temperature Effects

Nineteenth Sitting, 17 November 1933

Time	Reading	Zero, and remarks by Olga
6.10	-44	-48
6.12	-45	
6.13	-45	
6.15	-45	
6.16	-44½	
6.17	-44	
6.19	-43½	
6.21	-43	
6.24	-43	
6.27	-43	
6.27½	-42½	
6.28	-42	
6.29	-42	
6.30		"Force enters lower box"
6.31	-42	
6.33	-41¾	
6.34½	-41	

Time	Reading	Zero, and remarks by Olga
6.35½	- 41	
6.37	- 41	“ Power is getting stronger in the box ”
6.39	- 40	
6.40	- 40	
6.44	- 40	
6.45	- 40	
6.48	- 39½	
6.49	- 39	
6.51	- 39	
6.53	- 38½	
6.54	- 39	- 47
6.56	- 38	

Throughout there is slight fall in temperature of the lower box. There is no significant effect when the force enters the box, while it is there, or as it leaves. This is also so on the many other occasions that this apparatus was used.

A search of mitogenetic properties of the “force”.—It was thought that the “force”, though unable to affect the ray, might conceivably have a mitogenic power owing to some telepathic mechanism. Recent work by Gray and Ouillet¹ shows, for instance, that even if the biological evidence of Gurwitseh² for mitogenic rays is accepted they cannot be supposed to be ultra-violet radiation. The actual experiments were performed on two strains of *Bacillus fluorescens* and on samples of dormant yeast. No result of the action of the force was observed, but none of the experiments were of a high order of accuracy. Thus, on 10 November 1933, eight tubes of *Bacillus fluorescens* were taken into the *séance*-room in two different boxes. At each stage of their history the tubes were redistributed in a random manner between two boxes so that the controls should be adequate. The force was said by the medium in trance to have been in the box containing tubes A, C, F and H from 9.34½ till 9.56 and again from 11.46-11.57, making 32½ minutes in all. The bacteria were then counted in a sample, the count extending over ten cells. The results given are the mean of ten such counts and the results are as follows:

TABLE XV

“ Force ” by the tubes				Control tubes	
A	-	-	14.7	B	- - 18.9
C	-	-	17.1	D	- - 19.2
F	-	-	18.1	E	- - 21.5
H	-	-	16.4	G	- - 13.2
Mean = 16.6				Mean = 18.2	

¹ *Proc. Roy. Soc. B.* 1934.² *Die Mitogenetische Strahlung*, Berlin, 1932.

The ten counts for A gave values that varied from 10-18 with a standard deviation of 2.35. For the mean of four tubes would therefore expect a standard error of $\frac{2.35}{2}$ and a difference of 2.35 between the means would be regarded as worthy of inspection by the *t*-distribution formula of "Student". Since $18.2 - 16.6 = 1.6$, the difference is not significant. A more careful test is to calculate

$$\bar{x} = \frac{1}{n_1 + 1} \sum_1^{n_1+1} x_r \quad (\text{mean of } n_2 + 1 \text{ observations})$$

$$\bar{x}' = \frac{1}{n_2 + 1} \sum_1^{n_2+1} x'_r \quad (\text{mean of } n_2 + 1 \text{ observations})$$

$$s^2 = \frac{1}{n_1 + n_2} \left\{ \sum_1^{n_1+1} (x - \bar{x})^2 + \sum_1^{n_2+1} (x' - \bar{x}')^2 \right\}$$

$$t = \frac{\bar{x} - \bar{x}'}{s} \sqrt{\frac{(n_1 + 1)(n_2 + 1)}{n_1 + n_2 + 2}}$$

and look up in Student's tables of *t* for $n = n_1 + n_2$.

This procedure gives $t = 0.34$ for $n = 6$. There is thus approximately a chance of 0.75 that a homogeneous population would be found to exhibit the observed discrepancy owing to pure chance. The results are not significant.

On 12 December 1933 a larger number of tubes containing cultures of *Bacillus fluorescens liquefaciens* was put into each of the two temperature-measuring tumblers and the "force" was announced to have gone into the lower box from 8.31-9.5, which amounts to 34 minutes in all. Two counts, each in 10 cells, was made on five tubes from each box. The remaining tubes were not counted since they would not have produced a great increase in accuracy. The results were:

TABLE XVI

Lower box	16 and 20
	16 and 17
	12 and 15
	31 and 28
	13 and 18
	88 95
Upper box	30 and 30
	23 and 16
	17 and 15
	12 and 17
	26 and 17
	108 95

The results give $\bar{x}_1 = 18.6$
 $\bar{x} = 20.3$

$t = 0.6$ and $n = 18$

For $n=18$ there is a probability of about 0.65 that t should exceed 0.6, so that the observed discrepancy between the upper and lower boxes probably arose from errors of random sampling.

A $t=2$ would be regarded as significant, so that if $x_u - \bar{x}_1$ which equals 1.7 or 8.5% had been $\frac{20}{6}$ times as great the result would have been significant. Thus this method would have detected a 28% difference in the counts of the two series. Alternatively the number of counts should have been multiplied by ten and refer to 100 tubes in all in order to achieve an accuracy of 8%.

On Tuesday, 4 November 1933, three tubes of dormant yeast were put in one temperature box and three in the other. The force was announced to have gone into the box containing tubes 3, 4 and 5, while the controls 2, 6 and 7 were not influenced by it. The force was in the box from 11.12 to 11.20, making 8 minutes in all. The CO_2 evolved by these tubes was subsequently measured for $1\frac{3}{4}$ hours. The results for the CO_2 evolved in the last $1\frac{1}{2}$ hours were as follows :

TABLE XVII

<i>In the force</i>	<i>Not in the force</i>
Tube 4 = 4.18	Tube 2 = 4.06
Tube 3 = 4.46	Tube 6 = 4.77
Tube 5 = 4.20	Tube 7 = 4.49
-----	-----
Mean = 4.28	Mean = 4.44
Standard error = 0.158	Standard error = 0.358

For the standard error of the mean divide by $\sqrt{3}$ and obtain $\hat{S}_{in}=0.09$ and $\hat{S}_{out}=0.21$.

Since $4.44 - 4.28 = 0.16$, the difference between the two samples is *not* significant. It is therefore concluded that no significant difference in the rate of growth of two strains of *Baeillus fluorescens* or in the fermenting power of yeast was observed owing to the alleged presence of the force.

SURVEY OF THE SITTINGS

Number of sittings : 55, of which 38 were full-dress sittings in the Society's *séance*-room (type A) ; four were informal sittings, three held in O. G.'s house, and one in a small room at the S.P.R., in the hope that a less official atmosphere might induce paranormal phenomena (type B) ; six were short sittings for " gathering power " or for conversations, specially asked for by *Olga* (type C) ; six were word-association and psychogalvanic reflex sittings (type D) ; and one was a special sitting held in the Physiological and Biochemical

Laboratories, Oxford, to enable the trance respiration to be tested with the kind help of Dr C. G. Douglas (type E).

Key to Sitters

a = Sat throughout the sittings.

b = Sat in 1933 series only.

c = Sat in 1934 series only.

Name	No. of attendances
<i>The main group of Sitters</i>	
Mrs Theodore (Henrietta) Besterman - <i>a</i>	41
Theodore Besterman - - - - <i>a</i>	53
Miss Helen Carruthers - - - - <i>c</i>	20
Capt. the Hon. Victor A. Cochrane-Baillie <i>a</i>	27
C. J. A. Evelyn - - - - - <i>b</i>	10
G. W. Fisk - - - - - <i>c</i>	10
Oliver Gatty - - - - - <i>a</i>	45
C. V. C. Herbert - - - - - <i>b</i>	7
Lord Charles Hope - - - - - <i>a</i>	36
Miss A. Reutiner - - - - - <i>a</i>	35
Mrs W. H. Salter - - - - - <i>b</i>	9
<i>Occasional Sitters</i>	
Frau R. Schneider - - - - - <i>b</i>	1
Dr E. R. Holliday - - - - - <i>c</i>	2
Miss Mary Clive - - - - - <i>c</i>	2
Miss Jac Castellani - - - - - <i>c</i>	4
Gerald Heard - - - - - <i>c</i>	3
Miss Jean Goschen - - - - - <i>c</i>	2
Anthony Powell - - - - - <i>c</i>	2

In addition the following persons attended one sitting each in the 1934 series : Felix Greene, Miss Ursula Brocklebank, Richard Gatty, Ulich Verney, Miss Peggy Wake, Miss Peggie Robb-Smith, Miss Miriam Rothschild, Rev. C. Drayton Thomas, C. E. Bechhofer Roberts, Miss Elizabeth Capel-Cure, Miss Nea Walker, The Hon. Margaret Thesiger, John Foster, Geoffrey Faber, Mrs Geoffrey Faber, Miss Ina Jephson, C. C. L. Gregory, Henry Whitehead, Miss I. Newton, Miss Marguerita Barran, Lieut.-Col. L. P. Winby, Dr Pryn Hopkins, Mme M. Horska-Szpyrkowna,

REVIEW

C. G. JUNG : *Modern Man in Search of a Soul*, translated by W. S. Dell and Cary F. Baynes. London : Kegan Paul, Trench, Trubner and Co., 1933. Pp. ix + 282. 10s. 6d.

This book is not the systematic examination of contemporary spiritual adventures which its English title might suggest ; nor on the other hand is it, like so much psycho-analytic literature, a mere digest of clinical material. Of the eleven lectures which compose it, the majority were first published in German in 1931, as part of a volume entitled *Seelenprobleme der Gegenwart* ; the remainder are now collected for the first time from scattered publications. They deal discursively with various wider aspects and implications of " analytical psychology "—its postulates and aims, its bearing upon literature, anthropology, religion and ethics, above all its therapeutic value in the disordered world of to-day. They are addressed in the main to that section of the educated lay public which looks to the psychologist for the sort of practical guidance that was once expected of the clergy. While they are less systematic than, for example, Freud's *New Introductory Lectures*, they are also much less technical ; they are refreshingly free from vanity and dogmatism ; and they raise questions which are of fundamental importance for western civilisation. Jung has here left the consulting room for the market place—one might almost say, for the pulpit.

Although the book contains one or two references to parapsychology, its interest for the psychical researcher is marginal rather than direct. Jung's *Weltanschauung* is much more favourable to " occultism " in general than is Freud's : in the growth of spiritualism, theosophy, and even astrology, he sees a necessary reassertion of the intuitive or religious side of man's life against the dominance of the conceptual thinking associated with modern science. This necessity is not, however, the same thing as validity : he quotes spiritualism, along with the hallucinations of the insane and certain experiences of primitive man, as an example of the law that relatively independent psychic contents always tend to be personified. His own view is that " we cannot know whether anything happens to a person after he is dead." But the idea of life after death is for him one of the " primordial thoughts ", which though " incommensurable " with science are " indispensable conditions of the imagination " and may have to be revived and encouraged for therapeutic purposes. The physician's ultimate task, he says elsewhere, is to convey to the

patient "the healing fiction, the meaning that quickens—for it is this that the patient longs for, over and above all that reason and science can give him." This of course raises two issues, one ethical, the other practical: is it intellectually honest to "convey" opinions which one does not share; and how can such a procedure be successful with educated patients who are capable of distinguishing the truth of a belief from its usefulness and are on their guard against suggestion? To neither question does Jung give a satisfactory answer. When he falls back, as he does, upon the statement that "all psychic happenings are real", it is difficult not to suspect him of an elementary confusion. For the reality which belongs to every judgement *qua* "psychic happening" is not, even for a pragmatist, the same thing as its truth. And while the psychologist as physician may plead that questions of truth are irrelevant to his work, if he aspires to be a teacher he can no longer equate "this judgement is true" either with "this judgement is made" or with "this judgement is mentally hygienic."

Jung remarks that "every psychology—my own included—has the character of a subjective confession." And it is extremely interesting to observe how widely the path of analytical psychology as outlined by Jung has diverged from the psycho-analysis which the Freudian school, using the same fundamental concepts and faced with the same fundamental problems, has developed under the influence of a very different temperament. While rightly recognising in Freud "a great idealist", and paying generous tribute to the importance of his discoveries, Jung mocks (not without justification) at the Freudian "nursery-tales about the terrible old man of the tribe"; he also (more questionably) regards Freud's teaching as "hostile to spiritual values", since it is a psychology without the soul. In contrast, Jung's system might be described as a religion without a creed (he himself compares it with the discipline of Yoga). It has a minister, a ritual, a sacrament of confession and absolution; above all, it has as its central mystery a process of conversion or re-birth which "cannot be compassed by the rational concepts of consciousness," because it is no mere re-education of wayward instincts but "the bringing into being of a new centre of equilibrium." "The archetypes come to independent life and serve as spiritual guides for the personality. . . . This spontaneous activity of the psyche often becomes so intense that visionary pictures are seen or inner voices heard." These are the classical symptoms of religious conversion. But to what is the patient converted? His spiritual director does not know or (apparently) very much care. The psychotherapist "must have no fixed ideas as to what is right, nor must he pretend to know what is right and what not—otherwise he takes something from the richness of the experience." "Whether energy

is God, or God is energy, concerns me very little, for how, in any case, can I know such things ? ” It is as though the psychological machinery of conversion were set working *in vacuo*, as a wholesome spiritual exercise with no purpose beyond itself. What sets it working seems to be in the end simply the powerful and sympathetic personality of the spiritual healer. That is why “ the attitude of the psychotherapist is infinitely more important than the theories and methods of psychotherapy. ” And that is also why Jung, with all his imaginative insight and breadth of vision, is unlikely to leave behind him any such school of scientific therapists as that founded by his master and rival. Jung is a great personality and a penetrating observer ; he is not a great scientific or philosophical thinker.

E. R. DODDS.

OBITUARY

DR WALTER FRANKLIN PRINCE

(Died, 7th August, 1934)

ALTHOUGH Dr Walter Prince's many friends in England had been aware that he had not enjoyed the best of health for several years, the news of his death in August came to them as a great shock.

Dr Prince's career in psychical research was a very remarkable one. It was not till 1909, when he was already middle-aged, that he was first brought in touch with the subject through his position as clergyman of an Episcopalian Church in New York. One day there walked into his church a young woman, whose obvious mental and physical ill-health made demands on his sympathy. She was the "Doris Fischer" of the now famous case of multiple personality, and later on was adopted by Dr and Mrs Prince, and as Miss Theodosia Prince looked after Dr Prince during his latter years.

Considering the nature of his previous training, Dr Prince's treatment of the Doris Fischer case and his presentation of it in vols. ix.-xi. of the *Proceedings* of the American Society for Psychical Research must be reckoned among the most remarkable feats in modern psychology. Some of the incidents arising in the treatment of the case turned Dr Prince's ever alert mind towards a closer study of supernormal phenomena, and he rapidly became known as one of the leading figures in the psychical research of our generation. His deafness prevented him, as he was fully aware, from ever becoming a first-rate observer of the physical phenomena of the séance room, but except in that limited field, his critical ability, open-mindedness and philosophic grasp enabled him to make contributions to psychical research of very great importance. Besides the Doris Fischer case one may mention his report on Senora Maria Reyes de Z., which together with the earlier investigations of the same medium by Dr Pagenstecher, constitutes the most important evidence on the debated subject of psychometry; his examination of the automatic writings of Patience Worth, and various spirited but fruitless attempts to reduce to order the chaotic condition of the terminology of psychical research.

Dr Prince had been Research Officer of the American S.P.R. for several years before the disruption in that Society in 1925, which led to the formation of the Boston S.P.R. Of the new Society he was at once appointed Research Officer and held that post till his death.

He was elected a Corresponding Member of our own Society in 1921, and was President for the years 1930, 1931.

Our Society was privileged to hear him deliver two papers of outstanding interest. His Presidential Address to the S.P.R. is an important pronouncement, by one fully qualified by experience to make it, on the progress made and the degree of certainty attained in different branches of psychical research, and the factors which conduce to or hinder further development. The other paper was an account (afterwards printed by the Boston S.P.R.) of a tour of investigation on the Continent. For substance and style this paper would on its own lines be hard to beat.

Attention should also be drawn to the short but valuable summary printed in the *Transactions* of the Congress at Copenhagen of the bearing of the evidence for telepathy on the problem of survival. Certain phenomena since reported would very likely have induced Dr Prince to modify his statement in detail, though not the general tenor of his argument.

Dr Prince held the view, and stated it frequently and with great emphasis, that the evidence of psychical research supported a belief in survival, but his keen criticism of certain of the phenomena upon which the case for spiritualism is often based, prevented his ever becoming a champion entirely acceptable to the spiritualists. It is a position that is very well understood in this country.

But it would not be proper to conclude an obituary notice of Dr Prince without laying emphatic stress on the degree to which qualities not only of intellect, but of personal character—his friendly candour and his absolute trustworthiness—contributed to the importance and permanent value of his work.

IN MEMORY OF WALTER FRANKLIN PRINCE

WITH the death of Walter Franklin Prince, which occurred on 7th August, 1934, Psychical Research has lost one of its outstanding figures.

Born at Detroit on 22nd April, 1863, Mr Prince graduated in 1881, taking his A.B. at Yale in 1896 and his Ph.D. a few years later. Attracted to the religious life, he became a B.D. of Drew Theological Seminary and then followed a number of Church appointments in Brooklyn and elsewhere.

As early as 1900, Dr Prince exhibited characteristic traits which were afterwards to dominate his whole life. He became an Assistant Secretary to the Law and Order League of Connecticut and his interest in legal methods were shown by his papers on the First Criminal Code of Virginia, on the Blue Laws, and above all by his paper on the "Great Slave Conspiracy Delusion," which he published in the *Saturday Chronicle* of New Haven, Conn. in 1902. In

this paper he examined the evidence as seen in the documents and summed up his findings in the way so familiar to readers of his later papers on psychical research. It was, he wrote, "the crowning judicial atrocity of American History." On 25th May, 1934, in a letter to the present writer, he described another work which had just been published as "the worst printed crime in the history of Psychic Research."

It was in dealing with documentary material that Dr Prince exhibited his greatest powers. During his work with the American Society for Psychical Research (1917-1920) and latterly with the Boston Society his principal achievements have been in his analyses of various cases, although in practical work the investigation of the Doris Fischer Case will always be remembered. Although he fully recognized the importance of skilled field work and did his utmost to acquire knowledge of the psychological and mechanical aids to fraud he was never entirely at home in the laboratory, for he was great enough to know that without a long and laborious training it would be impossible for him to observe in the way that he knew was necessary.

During the tenure of office of the writer with the American Society, at the time that Dr Prince was still with it, we were in almost daily consultation. A deep friendship and rare understanding grew up between us, which was never marred even by later differences of opinion. We continued a long and confidential correspondence almost to the day of his death. In his last letter he expressed the wish that "so many miles did not intervene, so that we might often meet face to face." Had he not left us we should have met this autumn in his beloved home at Hingham, Mass.

E. J. D.

THE FIRE WALK

BY E. S. THOMAS

“MR FRAZER”, says Mr Andrew Lang (in *Magic and Religion*) “is interested mainly in the religious, magical, or ritual significance of (the fire walk) which varies in different places. To me on the other hand the immunity of the performers appears a subject worthy of physiological enquiry.” Sir James Frazer on his side notes that “eyewitnesses who have described the rite in Oceania have said little or nothing as to its meaning and purpose: their whole attention having been apparently concentrated on the heat of the furnace and the state of the performers’ legs before and after passing through it.”

In preparing this paper I have studied some 27 cases of what I shall call the ember walk as practised by Asiatics, and 10 of the stone walk of Oceania, and my verdict would be that Mr Andrew Lang has the better claim on our sympathy. For the purposes of this paper the object of the rite or ceremony has been of secondary importance. Yet a few words regarding the antiquity, significance and geographical distribution of the performance will not be out of place.

The oldest reference to it comes from India,¹ in the story of a contest as to which of two priests was the better Brahmin. It was decided by walking a fire of red-hot embers. The story is as old as 800 B.C., but it is referred to in one of the Samhitas of the Taittiriya 400 years before this. About 300 B.C. it was enacted that the fire-walk or the holding of red-hot iron should be the test of the veracity of a witness. Its connection with the story of Sita, wife of Rama, who proved her innocence and virginity by walking the fire, is too well known to be more than referred to. In modern times it is performed by Hindus as an expiation for sin, as a demonstration of the power of mind over matter, in fulfilment of a vow-bargain made in illness or misfortune, and as an obligation for the tenure of a post in the temple. In the background there is in some cases the suggestion that it is also implicitly a vernal or fertility rite, with a dominant idea of its purificatory power. In China it is certainly a solar or vernal fête, and it is probably the same in Japan, where it is also performed as a sign of spiritual power, and with a purificatory object. In the South Seas on the other hand, only members of a certain family are said to have the power to walk the hot stones, descendants of a man upon whom it was bestowed by a mythological eel in return for its life, although as the huge oven is also used to roast the first fruits and early fruits of the Ti or *Dracaena* root-

¹ *Encyclopaedia of Religion and Ethics.*

harvest, a vernal significance seems to be present both in this part of the ceremony and the season of year when it is usually performed.

The famous classical examples of the ember walk,¹ at Mount Soracte, near Rome, practised, it is interesting to note, by a member of a particular family, and at Castabala in S. Cappadocia, are well authenticated as to the fact by Virgil, Strabo, and Pliny. Iamblichus² the Philosopher mentions the piercing of the bodies of walkers with skewers, a practice followed, as we shall see, by Hindu walkers to-day.

An arresting sentence in Ezekiel 28, 14 describing the King of Tyre walking up and down amidst the stones of fire, which Sir James Frazer accepts as evidence for the stone-walk in antiquity, appears, unfortunately, to be a corrupt passage misconstrued; Dr Moffat's translation has "flashing jewels" in place of "stones of fire"; so that we are left with the story of the Three Children as the sole possible reference to an analogous experience in the Bible.

Andrew Lang³ quotes the Berserker's boast from the Icelandic "Flatey Book"—"I can walk through the burning fire with my bare feet" as a possible reference to the practice. In a legend of a pupil of St Brigid, who filled her shoes with cinders to purge her soul of fleshly desires, Professor R. A. S. MacAllister⁴ sees a tradition of it also in pre-Christian Ireland. But Spain and Bulgaria are the only two regions in Europe where the ember-walk is definitely recorded.

The antiquity of the records of the ember walk point to a source among Aryan peoples. Its present distribution is as follows: Hindus and Hindu emigrants practise it to-day in India, the Straits Settlements, Mauritius, Trinidad and S. Africa. Hindu influence in China and Japan in the past is very evident in the Buddhism of both countries and the practice may well have reached them therefore from India. Although in India Moslems walk the fire, particularly at the Moharrem festival, no cases appear to be reported from other Islamic countries. In North America frenzied Cheyenne⁵ Indians used to rush into fires and trample them out, in Yucatan⁶ men walk or rush over red-hot cinders to avert expected calamity, and in Australia⁷ treading out or treading on fire is practised in initiation ceremonies. We are told that in Yucatan they are sometimes unscathed, but it is clearly not done in any of these cases as a demonstration of immunity from burns.

In Oceania alone are hot stones walked, and the distribution there

¹ *Encyclopædia of Religion and Ethics*, and Lang, *Modern Mythology*.

² Frazer, *The Golden Bough*, vol. vi.

³ Lang, *Modern Mythology*.

⁴ *Man*, 1919, 63.

⁵ *Handbook of American Indians* (Smithsonian Inst.), i. 495.

⁶ Mackenzie, D. A., *Myths and Traditions of the South Sea Islands*.

⁷ Mathew, J., *Eagle Hawk and Crow*.

coincides with the peregrinations of the hereditary Fijian fire-walkers. The ceremony may well have reached them through Indian traders and have been transferred from the bed of hot embers to the *dracaena* oven.

In Spain and Bulgaria it is singular that, like the classical fire-walkers of Mt Soraete, some cases in India, and as generally reported in the South Sea Islands, the fire-walkers should belong to a particular family or caste. In Spain they were reported in the nineteenth century to have the hereditary power of going into the flames and, by dint of charms, permitted by the Inquisition, of extinguishing fires.¹ In Bulgaria the family are called Nistinares and the power is attributed to Sts Constantine and Helena. About 21st May, the day of these Saints, the desire to walk the fire comes upon them, especially when certain music is made. After processions and sacrifices of oxen and rams, fires are lit, and, filled with ecstasy by the music, they dance in the flame in turn until, the power leaving them, they feel the heat and withdraw.² No details are given as to the state of their feet before or afterwards.

I now turn to the evidence proper, beginning with China. The only detailed report³ of the ceremony is 73 years old. It took place amidst scenes of frenzied excitement. Round an area of red-hot charcoal, 20 feet in diameter, a Taoist priest walked, throwing on rice and salt to conjure the flames and assure a propitious year. Two exorcists facing one another across the area then beat the surface with whips representing snakes. Then they walked across and two of the peasant performers, who had practised fasting and continence for a week, crossed and recrossed until it was trodden down. The Great God's image was then carried across and back three times by twenty similarly trained peasants. The witness, Herr Sehlegel, a German, was not impressed. He attributed their immunity from burns to their horny foot-soles placed flat. He had seen the protagonists of one of these ceremonies flogged as impostors next day by Government officials. The salt and beating of the embers would certainly tend to lower the temperature and hasten the production of ash. It will be remembered that the men with the whips faced each other and we may guess that the path followed was this beaten one.

In Japan the throwing on of salt is also recorded, and the feet are dipped in salt before walking. The surface too is beaten and to greater effect, with long poles. Mr H. G. Ponting, one of the recorders of the Japanese ceremony,⁴ walked the 20 foot length after dipping his feet in salt, and feeling only a comfortable warmth, repeated it at a slower pace. He halted, moreover, in the middle and trod on the hottest ember he could see (the only indication he gives

¹ Lang, *Magic and Religion*.

² Lang, *Modern Mythology*.

³ *Internat. Archiv. f. Ethnog.*, 1896, x. (supplement).

⁴ *London Times*, 23.2.34.

of the state of the furnace) and kept his foot there for a second. He felt a slight sensation of a burn, and a blister formed which gave no trouble. It seems clear from this very summary account, in which the details relate to the frenzied excitement into which the priests worked themselves and the cult accessories, that the coals were not very hot when Mr Ponting walked. The embers were of charcoal which had been brought to a glow with long fans over an area 20 ft. \times 12 \times 1 ft. deep, and they had clearly cooled and probably deposited a good thick coating of ash. He considers that the beating smooth of the embers had much to do with it, the ostensible effect of which was doubtless, as in the Chinese rite, to drive out the inimical spirit of fire.

In a case reported to Sir James Frazer by two eye-witnesses, however, the conditions seem to have been more severe. One of them, Mr Foxwell, said "the sweat ran down him" as he stood near the furnace and he was unable to explain the immunity of the performers. Perceval Lowell, the American writer, was, he said, laid up for three weeks with burns after walking, while a Scottish engineer named Hillhouse was unscathed. The probable reason for the immunity of the engineer, his hardened foot-soles, does not seem to have occurred to this observer, and this weakens our faith in the value of his impressions.

In a third case from Japan, reported in 1903 by the wife of an American naval officer,¹ salt was rubbed on the legs before walking. The heat appears to have been intense as the lady spectators held up fans to screen their faces from it, and an English lady shrieked with alarm² when the first of the walkers entered the furnace, of whom one is reported to have strolled over the 18 feet distance with no apparent discomfort. The furnace was 18 ft. \times 6 \times 2 ft. deep. Straw was laid on charcoal and ignited and the charcoal fanned into a glow. The ceremony took place at 6 o'clock, when nearly dark, with a full moon. No details of time intervals are given. The great heat of the furnace is not incompatible with a good covering of ash, invisible in the moonlight, to which the burnt straw would contribute. Many people followed the protagonists, including boys and a woman carrying a babe, but we are not told after what interval of time.

Lastly, in an ember-walk witnessed at Tokio in 1899 by Mr Andrew Haggard,³ those who were to walk sat stripped in a room by big tubs of cold water, with which they frequently drenched themselves, working themselves into excitement meanwhile with long prayers, chants and gesticulations. He too records that the fire was beaten with long poles before the walk. Before passing on to the Indian cases two observations may be made. First, that we are not justified in

¹ Frazer, *Golden Bough*, vol. vii. ² *American Anthropologist*, v. 1903, 377.

³ Lang (9) and *The Field*, 1899.

assuming, if there is no reference to beating the surface in such summary accounts from China and Japan as those we have to be satisfied with, or to douching with cold water, that these incidents did not take place. The second observation is that nothing is said, in any of the above accounts, of the ash which must have formed on the surface. Ash is such a bad conductor of heat that even a thin layer would effectively shield a calloused foot from the red-hot embers during a short contact.

In two cases of the Indian walk the ash is specially mentioned. Mr W. E. Beeching¹ witnessed the ceremony in 1933, a Moslem performance which took place on a dark night about 3.30 a.m. The fire was lit at midnight and contained a good deal of cork. It was spread out with iron rakes which were cooled with water. This formed puddles round the embers which lay about 12 inches thick over an area, one gathers, about 9 feet in diameter. The priest came decked with garlands from the bath and, after gyrating round the edge, pranced into the glowing mass. His feet covered to the ankles were slowly withdrawn and as he moved forward the *black* impresses filled in as the embers slipped into place. He turned and returned several times. Mr Beeching who desired to walk was told to wait until he could do so with priestly blessing. He entered in due course with bare feet, turning up his trousers, and dashed to the other side and back unhurt. He attributes the priest's immunity to the ash, his calloused feet, and the mud with which he cleverly coated his legs when gyrating through the puddles, and his own to the fresh ash which formed during the 15 minutes he was kept waiting.

Mr R. U. Sayce² has described an ember-walk by Indians in 1926 at Pietermaritzburg, S. Africa, with a wealth of anthropological detail covering the daily life and food for ten days prior to the ceremony of the performers, upon whom strict sexual abstinence and thought-control were enjoined, as well as the ritual perambulations of a procession with the image of the goddess Draupadi about the furnace, the slaughter of a goat and other matters which are not of much concern to us here. The pit was 30 feet long \times 12 feet wide and the walkers came up from the river, many with silver hooks in their backs and skewers through tongue and cheeks. They dipped their feet in turmeric water before, and milk and water after, the walk.

Many of the ritual details were obtained three years later from prominent local Hindus and not from Mr Sayce's personal observation. He was good enough to write to me on some doubtful points: namely the nature of the "ashes" over which they walked and the method of their preparation. The furnace was a mixture of ash and embers some 9 inches thick, prepared by raking bonfires as the fuel was consumed. This implies iron rakes and puddles from their cool-

¹ *Chambers' Journal*, March 1934.

² *Man*, 1933, 33.

ing as in Mr Becching's case. The walkers are stated in his account to have been summoned from the river *when all was ready*. There is internal evidence that this was at least 200 yards away, but he does not remember the distance. He did not apparently witness the perambulations of the idol ($2\frac{1}{2}$ times round the furnace) which, with the sacrifice, are said to have taken place after the return from the river, all of which would have given time for a considerable deposit of ash. Most of the performers appeared to collapse after the walk, for reasons which are obvious enough—the long period of training on light food, the walk to and from the river, and emotion. A number of them were accustomed to wear boots, he says; their immunity was not due to thick foot-soles. But it is clear that the thickness of the ash may have compensated for this.

An interesting performance of the ember-walk at Suva in the Fiji Islands was witnessed by Mr S. M. White¹ in 1932. The performers approached the furnace (45 ft. \times 15 ft.) of wood ashes (presumably embers and ash) from which all burning sticks had been carefully removed, supported in a state of prostration by friends after fasting for fourteen days and a three mile walk from the sea. Continence had also been observed. Headed by the priest they walked the length quite slowly, some three or four times, with no sign of discomfort, their feet sinking in the ashes, and one ploughing his feet deep on the return journey. Many had skewers through their cheeks and fleshy parts of their bodies. The surface was white with ash after the walk, but the heat up wind was too great for the hand to bear for long at a height of nearly 6 feet. Paper thrown on burst into flame at once. The heat down wind was like a furnace. The single crossing, he says, took at least half a minute, the longest time recorded. This suggests a short (probably dancing) step of 18 inches every second. There was no sign of blistering or burning even on the delicate upper surface between the toes, and the soles were whitish and soft after walking a distance through rivers and (an important point when coupled with a reference to washing after the fire walk) along muddy tracks. Their legs were therefore in all probability coated with wet mud and their feet soft, which would not lessen the protective virtue of the callus. The ash was clearly deep at the start of the walk; while porous it would let the heat radiate through; when compacted by the foot it would, as before stated, act as a shield to the heat. It is clearly the "premier pas qui coute": the return journey along the compacted path would be far cooler. I defer consideration of the great heat of the furnace in this and other cases until the whole evidence is before us, noting however that we are not told in what direction they walked as regards the wind. I would call attention to the common tendency in these accounts to ignore details (the wind, the tracks, and the mud, for instance)

¹ *Journal S.P.R.*, Jan. 1934.

which while of great scientific importance are not realised to be so by the observer.

A simple case described by Capt. G. R. Hearn¹ in which the panda or priest of a small temple had to plunge knee-deep through burning cowdung at the Holy festival as a duty attached to his office, is interesting from the fact that the secret of its successful performance is said only to be revealed by the priest to his successor on his deathbed, and because having duly prepared himself by fasting and prayer, he is said to know when he is ready for the ceremony by the insensibility of his fingers to the flames of a candle. In any case Capt. Hearn says, the flames are not very severe and he does not seem surprised to have found his legs unhurt and apparently the hairs thereon unsinged. The cowdung cakes covered apparently an area of 10 ft. × 8 ft. and were three feet deep, so that the ash was probably very thick. In a case witnessed by Mr H. K. Beauchamp² near Madras in 1901, water was sprinkled all round the area of embers, 15 feet square and 3 or 4 inches deep, and we are at liberty to assume that the walkers gyrated in it as in the observant Mr Beeching's account. The walk took place at 6 p.m., *i.e.* after dark, the fire of a ton of jungle wood and two cartloads of charcoal having been lit at noon. It is not surprising therefore to read that their feet sank into the loose bed of fire, as it would have looked and is described. And the apparent wading rather than walking was a variant which was probably introduced with impunity. A glutinous decoction of aloe was reported to produce immunity to burns, but the performers rejected the suggestion with indignation. Other decoctions are variously reported but without verification.

In Mr J. G. D. Partridge's³ account of the ceremony performed in 1902 in India, the priest after frenzied dancing sat in a swing-seat full of 3 inch thorns without relieving his weight on the ropes. The thorns pierced his clothes but did not seem to hurt him and there was no blood on his body. He constantly inhaled incense fumes. There were two 9-foot pits, one filled with embers and the other with ashes from a wood-fire. He took four steps in four seconds in each pit, but this part of the performance did not impress Mr Partridge.

The case I reserve to the last⁴ is the most arresting: a Moslem performance in which some supernormal influence of the fakir or dervish seems to operate and wane at intervals. It was described by Mr V. E. Stowell in a letter to his father which the latter sent to Sir Oliver Lodge. The case was corroborated by his older friend Mr Quinn. The account was published in the *Journal* for June 1928. It apparently took place on 22nd November, 1927. The performance

¹ *Man*, 1905, 83.

² Thurston, E., *Ethnog. Notes in S. India*.

³ *Ibid.* with a similar case (S. P. Rice); *v.* also *Journal S.P.R.* Oct. 1930 (Millar).

⁴ *Journal S.P.R.*, June 1928.

was in aid of a hospital. The fire trench full of embers was 12 feet long, narrow and shallow. The writer and Quinn were told they could walk without getting burned. They did so in their socks and felt nothing. Afterwards they walked again, through, in addition, flames two or three feet high of rushes thrown on and ignited, apparently with paraffin oil. After stepping out, an adhering cinder burnt a hole in Stowell's sock. While people walked, he says, the fakir was "worked up into a terrific state of excitement". At intervals of about a minute the power seemed to leave him and he shouted, "Bass, bass!" (Enough!). Once Quinn failed to hop out at the cry and one of his feet was slightly scorched, but without burning his sock, which, he says, showed it was genuine. (But in this Stowell was mistaken: Quinn states in a letter to Mrs Salter that his foot was blistered and his sock scorched). The fire, Quinn states, (probably meaning the heat), was fierce and the embers red hot. Stowell says the fire was too hot to sit close to. His woollen socks were "not even frizzled".

A native friend told Quinn that this fakir alone had this power and that it was hereditary. He had fasted all day and collapsed rolling his eyes at the end. Many people walked, including children led by the hand, and finally the band with their instruments. An Indian friend's account was also sent to Mrs Salter. He had walked in his socks and only felt warmth. He related that the loin-cloth of a Hindu caught fire as he stepped out of the trench and that he was not even scorched, although a doctor present said he should have been severely burned. Many people of note attested to the walk over glowing embers and through flames under the aegis of this fakir Syed Hussein Atashi, including Lord Sydenham.¹

In order to come to some decision regarding the hypernormality or otherwise of this account I made a shallow trench 9 feet long and 12 inches wide and filled it with hay. On this I laid folded sheets of newspaper and a dozen bundles of fire sticks evenly spread, which just filled it. I lit the hay quickly all along and as soon as the wood flames died down I walked across in thin slippers, taking five steps on the embers. My shoes were quite unscorched, and here I may say that for this reason I have omitted from this paper all accounts of walking the fire in shoes.

I then walked in my socks, treading five times with four steps as in ordinary walking, taking three seconds in the process. I did this four times, making the return journey at once the last time. By then the glow was deeper in colour, probably due in part to deposit of ash. I could feel it warm under my feet but there was no discomfort. My socks were not even singed except where small embers clung to them, and I felt these burn through the holes they made as I brushed them off on the bank. (My sock soles (an old pair) were very thin,

¹ *Journal S.P.R.*, October 1928.

not to say threadbare in places). This was the cause of my delay. Clearly then the foot leaves the embers before they have time to burn. Yet my feet tingled and burned, but not painfully, for several hours afterwards. If the attention of the walker is distracted by a call or startling sound, the distraction would lead probably to a momentary halt upon one foot with consequent burning. In a large furnace, too, it is not surprising that a fall should, as in an early case in India related by Mr Stokes,¹ lead to such severe burns on the body as to cause death: although the walkers passed through unscathed.

As regards Mr Stowell's immunity from flames knee-high, experimenting with a 6 foot stretch of blazing hay with flames more than knee-high I found I could walk it comfortably as before in four seconds without feeling any heat whatever. I did it three times, but did not turn in the flames. I waved a piece of cotton cloth to and fro in the flames, and it caught fire in from 3 to 4 seconds.

My small experiments then lead me to the conviction that there is nothing supernatural, as far as the evidence goes, in the immunity of persons who do the fire-walk in shoes or socks alone, with their legs protected by clothing. And calloused foot-soles are thicker than socks. I defer, as I have said, consideration of the possibly paranormal aspects of the performance until we have considered the other fire-walk, over heated stones in Oceania (which I call the stone-walk), and to this I now turn.

The stone-walk in the South Seas presents us at first sight with several new and baffling factors. It is clear from many of the accounts that the dramatic and picturesque aspects of the performance greatly impressed the witnesses, and had inevitably impaired their critical faculties. The walkers are primarily members of a family, the Nga Ngalita tribe of Fiji who, they say, inherit the gift from an ancestor who received it from a deity in eel form. They and their assistants are arrayed for the ceremony in their picturesque native costume of garlands and coloured dried leaf-strip skirts and other ornaments. The ceremony usually takes place close to the seashore in a coconut-grove, and all is animation and excitement. The furnace is a pit 2 to 4 or more feet deep, and from 9 to 15 or 20 to 30 feet in diameter. In it poles are sometimes, if not always, set radially to hold a pile of some 200 stones from 40 to 80 lbs. in weight, according to one observant witness. These are always of the same rock, a very vesicular augite-auesite basalt. The fire is lit and blazes 4 to 8 hours or more, but usually 36 to 48 hours. Unfortunately no account tells us how the fire is fed: one can only surmise that fresh wood is thrown on, which obviously would not impart anything like as much heat to the stones as the logs below. When the spectators gather, the stones glow white and red hot and

¹ *Indian Antiquary*, ii. 1873, 190.

are exploding and firing off flakes. Then with loud yells the picturesquely garbed assistants rush in with long green poles having tourniquet loops of creeper at the end and haul out the blazing logs. The butts of the poles are next set in line along the surface and hauled like a rake this way and that with long creeper stems until the whole is fairly levelled, and sometimes, if not always, a surface is arranged of fire-split stones turned flat side uppermost. When all is ready the leader emerges from the grove, usually in deep silence (in contrast with the Indian ceremony), tall and dignified, garlanded with flowers, carrying a branch of the Ti plant (the *dracaena*, whose root is afterwards cooked in the oven), and walks across or round the furnace or both. Others may follow, and finally the *dracaena* roots, wrapped in succulent hibiscus or other leaves, are laid on with a quantity of leaves over them. Clouds of steam rise as they pile earth on the top. Then it is left for four days, by which time it is cooked. The walk is thus a manifestation of inherited power, as well apparently as a prelude to the *dracaena* season, and so a vernal ceremony.

Two observers have attempted a scientific account of the stone-walk and these I will consider first.

Dr Robert Fulton, M.B.¹ observed it in June 1902 at Bega Island in the Fiji Group, where it is usually held. The fire, lit 48 hours before, was glowing fiercely on his arrival, and there was discomfort at a distance of 10 feet. The oven (*lovo*) was 20 feet in diameter and 2 feet deep. The stones, he says, fairly glowed and there was little smoke when after a further two hours the burning embers were removed and the stones turned over and levelled. By this time, he says, they had undoubtedly cooled, turning from white to red, then to grey and black. The whole process of preparation, he thinks, took nearer two hours than one. The heat at the centre was still very great. The chief walker appeared from the grove and was examined by Dr Fulton and Dr Smith. His pulse was just over 90, his hands were cool, his feet cold, perceptibly cooler than the rest of the body, clean and odourless. The soles were yellow-white, smooth like soft kid, as in fact they would be after the customary bathe, which is not mentioned. In addition to the native skirt he wore anklets of dry braeken. There were black hairs on his legs. He then, followed by some twelve others, walked deliberately across and twice round the stones: Dr Fulton thought they walked round only, but he was assured by others that they crossed the centre. One of the men was examined by Dr Duncan after the walk. His feet, the hairs on his legs, and the dry fern anklet showed no signs of scorching or burning. His pulse was 120, and (an interesting point) his feet still cool, if not cold, although the temperature of his legs was that of a person in high fever. People with cameras, accustomed to timing, said the walk

¹ *Transactions of the N.Z. Institute*, xxxv. 187.

lasted 15 seconds : 25 to 30 steps were taken, so that the soles were not in contact with the stones for more than half a second at a time.

A stone 10 inches by 5 by 2 was removed for him from the centre of the oven. It was carried in a leaf basket to the shore, where it steamed and fizzled for several minutes in water : even then it was too hot to hold. He lost it going on board when it slipped from its basket and he found it too hot to save from falling into the sea.

Dr Fulton thinks the secret lies not in the calloused feet of the walker but in the peculiar properties of the rock, which has a very low specific heat and very slow conductivity. He does not think (but he offers no evidence for this) that the stones were heated through, and concludes therefore that the split surfaces were comparatively cool. Unfortunately, he does not say whether the surface stones were glowing hot on his arrival. The walk, he further says, did not begin till most of the stones were split, and he noted the pile had not collapsed, inferring that the logs had not burned through. He contends finally that the heat imparted to a foot in contact would be a fraction of that given out by many other rocks in the same time, and that in spite of the high temperature the heat given off would not burn the feet.

Incidentally it should be observed that if 25 to 30 steps measure circumference and diameter, then the diameter of the prepared surface must have been about seven steps. In a case to be considered presently the step appears to have been about 12 inches.

Mr S. P. Langley¹ witnessed the stone-walk in Tahiti in 1901 and was the first to give scientific attention to it. The pit was $9 \times 21 \times 2$ feet deep. He ascertained the composition of the rock, weighed an average stone, and estimated the number at 200, with an average weight at 40 to 80 lbs. The fire was lit at midday and burned till 4.40, when the logs were removed. After such a short period it is not surprising that the outer stones did not burn his hands, although the lowest layers of stones, which lay four deep in the middle, were red hot. The upper central stones also gave out considerable heat. The upper stones looked unsafe for naked feet, but an engineer told him he had seen natives standing on steam pipes at 300° F. The chief walker, Papa Ita, said he was able to walk through spells and the aid of a goddess, after two or three days spent in prayer. He and two disciples in native dress walked when all was ready, the former over the centre with hurried steps, the latter half-way to the edge. He turned and walked back with deliberate confidence accompanied by new disciples who clearly picked a cooler path. After a third and fourth passage Europeans accompanied him in thin shoes which were not burnt, but the trousers of one were scorched, doubtless by the flames which were still occasionally licking up between the stones. He was now allowed to remove a stone from

¹ *Nature*, 22nd August, 1901.

the centre of the pile upon which, he says, somewhat inconsistently, every foot had rested. It was ovoid and the large end had been in the hottest part of the fire.

He attempted to estimate its temperature in a bucket, but the ebullition was so violent that a great deal boiled over. It continued to boil for ten minutes. The stone weighed 65 lbs., and the estimated temperature (for what it is worth) was 1200°. This rock, it was found, glowed red hot between 1300° and 1400°, but the temperature on the surface, he says, must have been indefinitely lower. He found that after standing 8 to 10 seconds in his shoes on the hottest part of the pile the heat became unpleasant. He could therefore have walked in his shoes for 16 to 20 paces. It is unfortunate that he did not note the time taken on the walk by the performers, or spent by the assistants in preparing the surface.

One can hardly agree with Mr Langley when, while regretfully deciding that there was nothing abnormal in the performance he witnessed, he says he is convinced that he witnessed a typical stone walk. The difference between his 4½ hours and the 36 to 48 hours recorded by others for the heating process, is considerable. His information regarding the physical and petrological characters of the rock used is of great value, but his suspicion based on the use of this stone and the reported refusal of Papa Ita to use a marble is ill-founded. A siliceous rock of high specific heat would certainly slag and marble be calcined in the oven.

I obtained a piece of olivine basalt about 4 inches square and about 3 inches at its thickest, weighing nearly 4 lbs., of much the same specific gravity (2.5 instead of 2.4), but not so vesicular, and I heated it in a coal fire all over for 3 hours and then removed it to the side of the grate. In three minutes it had lost its red glow. On another occasion I left it on the coals in an anthracite stove for 5 hours and then cracked off an inch-wide piece in the dark. It was only red hot half through. The small piece I heated all day on a gas ring and it was only red hot underneath. The rest of the stone I subsequently heated all over with red coals for 4 hours. On removing the covering coals and turning it over hottest side up, *on the fire*, it again lost its glow in about three minutes.

It is possible to calculate that a spherical stone of 60 lbs. weight of the same unit weight (specific gravity) as mine would take about thrice the time to cool; other shapes, such as oval, not so long. So we may, I think, feel fairly sure that in 15 minutes any red-hot stones on the surface would have lost their glow, that is, would have cooled below red heat. Although I found, like Mr Langley, that the rock retains its heat afterwards for a very long time, from my experiment it seems probable that after the logs are removed, the tops of the upper stones would cool as fast or as slowly near the edge of the furnace as in the hotter middle region. If so, it may be worth

noting, although we have no information as to the probable internal state of the hot stones, that I was satisfied by experiment with my stone that I could have taken three steps barefooted with each (uncalloused) foot on such rocks, heated all over and cooled in the hearth for an hour and a quarter, which is about the time taken to prepare the arena in Dr Fulton's case. After the same period of time I could have taken 18 steps on the stone heated on the underside only and cooled outside the grate. Mr Langley thought the upper stones were not heated through and were therefore much cooler on the flat surfaces when they were split, and this may well have been true in his case at any rate.

If the stone Dr Fulton experimented with, which was about the thickness of mine, was a typical specimen, then the surface stones in his furnace would have cooled at about the same rate as my stone. In spite of the 48 hours heating a great deal of heat must have radiated away in the (say) $1\frac{1}{2}$ hours spent in the preparation. In the one of these two cases, then, we have a short period of heating and an unknown time spent in preparation; in the other a long period of heating (but no information as to how the fire is fed) and a long time spent in making ready for the walk—which may well have been *intentionally* protracted.

Before passing to further cases the probability should be borne in mind that when the logs burn at the inner end and drop, the cooler peripheral stones would tend to roll over the hotter inner stones. It is these that would be raked to and fro and form the upper layer. If this does not happen the levelling would bring hotter stones to the surface in the centre, which several observers have noticed to be the hottest part, avoided or hurried over by the walkers.

But in the absence of details regarding the fuelling of the pile and its behaviour during the long hours of heating, speculation about these points, important as they are, cannot be very fruitful.

Passing on to consider further cases, in one that took place in 1904, witnessed by Mr W. L. Allardyce¹ at Bega Island, Fiji, the pit was 30 feet in diameter and 3 feet deep, filled with stones and wood in alternate layers. The fire burned for 12 hours before being cleared of logs and levelled in the usual way. The heat, he says, was considerable. A handkerchief laid on the stones (he does not say *where* in the pit) was charred. Twelve or fourteen men walked round and across, taking not more than a minute walking quite slowly. This pace suggests short quick steps of a foot in half a second. He examined the feet of the performers and there was, he felt sure, no preparation applied before and no sign of burning after. He attributes their immunity to walking barefooted on the very hot wind-sheltered sunbaked rocks. In this case we have clear indication, I think, that the stones walked on were not red hot, but the

¹ *Man*, 1904, 45.

important detail of the time taken for the preparation is not given.

In 1933 Mr Kingsley Roth¹ saw the ceremony at the same island with a smaller pit 15 feet across and 3 feet deep. He tells us the stones were laid on logs which in his photograph are set radially. He does not say how long they were left to burn, but there was definite discomfort 6 feet away. The clearing and levelling only took 20 minutes. From a second photograph the stones seem to have all collapsed into the pit and are shown covered with logs. The clearing, he says, goes on until the stones are completely uncovered. A special log was laid on the top during the clearing and left until the walkers arrived. This was the path of the Power and no one might cross between it and the path by which the walkers were to arrive. Continence was observed beforehand. They walked deliberately over the stones with a firm tread, gripping them with the feet, and there was no sign of burning afterwards.

Mr Basil Thomson's² experience at Bega is undated. The pit was 19 feet wide and the fire had burned 48 hours. It was, he says, a white-hot mass shooting out little tongues of flame. The flattening (and presumably clearing of embers) took half an hour, and tongues of flame still played among the stones. Then with cries of "Vutu, Vutu", fifteen men rushed out of the grove in pairs, and with apprehensive looks stepped on to the stones, led by one Jonathan. But it is important and instructive to notice that, in this case, planting their feet firmly and squarely (the best way, incidentally, to distribute the heat over calloused foot-soles), they walked *round* the pit and not across it.

A handkerchief was dropped on a large stone from the pit as Jonathan stepped down, and what remained was picked up as the last man left. In the 15 or 20 seconds [*sic*] which elapsed it was charred where it touched the stone and seared elsewhere; clear evidence that it was not red hot or near it. A more instructive test would have been to lower and raise a handkerchief on to and off the stone in time to the steps of one foot of one of the walkers. The fact that the dried fern anklets of the walkers were not burnt, which Mr Thomson notes, is not so significant as it would be had they crossed a diameter of the oven. Mr Thomson is surprised that their insteps were not burnt, but an instep which touched the ground would be calloused to some extent, and not as soft as Mr Thomson thinks. He also noticed the interesting point that their feet were cool after the walk.

Dr Hoeken's³ account of the stone-walk witnessed at the same island in 1898 is one of the best known and is interesting for detailed observations of doubtful accuracy in places, as well as for

¹ *Man*, 1933, 49.

² Lang, *Modern Mythology*.

³ *Transactions of the N.Z. Institute*, xxxi. 67.

unwarranted inferences. The stones were heated from 36 to 48 hours, the pit was 25 to 30 feet in diameter and 8 feet deep at the deepest, filled, he says, with white-hot stones. The temperature was 114 degrees on the windward side at the edge. When the pit was prepared—the time taken is not given—the area of stones was only 15 feet across. The walkers were greeted with yells from the crowd (in most accounts they approach in silence) and marched, as he thought, leisurely, but others thought quickly, across and around the stones, leaving where they entered. The leader was just 30 seconds in it, a rate of 2 feet a second as in Mr Allardyce's case; short quick steps would account for the difference of opinion as to their manner of walking.

Dr Hocken had examined one of the walkers with the leader's (Jonathan's) permission and satisfied himself by touch, smell and taste that their legs and feet had not been treated with any application. The soles were soft and flexible. He says nothing of the temperature of the feet. The same examination followed the walk. Jonathan said he felt no heat or other sensation when walking. "The stones", Dr Hocken writes, "must have been white hot", but not visibly so, owing, he suggests, to the brilliance of the day. Hypnotism and anaesthesia may produce insensibility, he concludes, but do not interfere with cautery.

It is clear, I think, that this account is not a very reliable one. The pit could not have been full of white-hot stones, if they only covered an area of about half the diameter of the pit after clearing. The probability also is, for reasons shown above, that the stones were as they looked, neither white nor red hot on the surface.

Mr Haswell's ¹ account refers to a ceremony at Raiatea in 1885. The fire burned for a day, the furnace was 30 feet across and 3 to 4 feet deep. The flames poured up between the stones which were red and white hot. When all was ready and the furnace, he says, still pouring out its intense heat, the natives marched up and walked leisurely across, repeating it five times. Here the strategic part of the performance is dismissed with the words "when all was ready" and the general reader would assume that they walked over red and white-hot stones, blazing logs and all.

I give this account as a good example of bad reporting. Also the following :

A Boston journalist, Mr Gorten ² saw the performance at Honolulu, arranged by Papa Ita, in 1895. All day the fire burned until the stones were white hot. "Then", he says (outdoing Mr Haswell) after reference to incantations and gestures, "the Tahitian calmly and deliberately walked the length of the pit". On his return he stopped in the middle to be photographed. Mr Gorten, however, after speaking of the fiery path, and white-hot stones, remembers the

¹ Lang, *Modern Mythology*.

² Lang, *Magic and Religion*.

preparations, but explains their object in unexpected fashion: in fact it is something of an anti-climax to read, "The stones were undoubtedly hot and were turned by long poles just before walking to have the hottest side up."

At the end of it we are told that Papa Ita was touring the Islands in the hands of a manager who, it was rumoured, was going to take him to the Pan-American Exhibition at Buffalo. With that we take leave of Papa Ita and Jonathan (who may be the same person), unless one of them figures anonymously in the last and most sensational case I shall describe, namely that witnessed at Rarotonga in 1899 by Col. Gudgeon,¹ who walked the hot stones with three other Englishmen barefooted. It is a full account, but I only give the salient details.

The oven was lit at dawn and his party went to the scene at 2 p.m., when things were being got ready and the stones levelled. The *Tohunga* and his pupil, after striking the oven in the customary way with their bunches of *ti* leaves and muttering spells, walked slowly and deliberately over the 12 feet of hot stones. The *Tohunga* then handed his bunch to Mr Godwin (who owned the site) with the words, "I hand my *mana* to you: lead your friends across!" They started, and "I can only say", he writes, "we stepped out boldly. I got across unscathed; only one of the party was badly burned, and he, it is said, was spoken to and looked behind, a thing against the rules". . . . "I knew quite well", he continues, "that I was walking over red-hot stones and could feel the heat, yet I was not burned". He "felt something like electric shocks then and afterwards for about 7 hours. . . . A man must have *mana* to do it: if he has not, it will be too late when he is on the hot stones of Taira-ahi-roa."

Half an hour later the priest threw his green branch on the stones and in 15 seconds it was blazing. Col. Gudgeon's feet being naturally tender, he walked slowly for fear of tripping. His soles did not peel and were not hardened. A troupe of 200 Maoris followed, of whom "many thought they were burnt but none were severely".

In this dramatic description several useful details are lacking: the rate of walking, the number of steps, the shape of the oven and its size, whether they walked across the centre or near the edge over the 12 feet of hot stones, and how long the clearing took. If dawn is at 5 o'clock the fire had burned only 9 hours, and it is unlikely that the upper stones would be heated red hot through in that time. Other accounts also speak of small flames lying up between the stones, as one might expect, after the large burning logs were withdrawn. These, with a hot region selected by the *Tohunga* from experience, would suffice to parch and ignite the green leaves very rapidly, and give quite a mistaken idea of the heat of the stones they had walked on.

¹ Lang, *Magic and Religion*.

It is clear from his account that Col. Gudgeon was greatly impressed by the scene and elated by his experience. The heat radiating from between the stones gave him the impression that the stones he trod were of the same heat. If he took 12 steps over the 12 feet distance, stepping 12 inches in half a second, each foot would have touched the hot stones 6 times for half a second. How hot were they? In default of necessary details the presumption must be that there was nothing abnormal in the immunity of Col. Gudgeon in his courageous performance.

Clearly the subject cannot be left without considering the great heat of the furnace both in the ember- and the stone-walk, as distinct from the temperature and nature of the surface trodden. We are, I think, justified in assuming the following points, except in the more trivial examples of the ember-walk:

1. That there was more or less severe psycho-physical preparation calculated to train the performer in auto-hypnosis, which would not only give him faith, but render him more resistant and insensible than ordinarily to the heat, and as well-known experiments with hypnotised subjects have shown, even to burning.¹ We have evidence of its actual operation in the self-torture inflicted on themselves by Indian walkers, as well as in the test, described by Captain Hearn, in which the performer passed his fingers to and from a lamp flame until they were insensible and he felt ready for the ordeal. Col. Gudgeon was clearly in a state of exaltation which would in some degree have conduced to the same result.

2. That a bath was taken before the walk which, cooling the limbs, would contract the blood vessels and reinforce the effect produced by auto-hypnosis, supplemented in the Asiatic performances by the excitement into which they worked themselves.² The coldness of the feet after as well as before the walk, reported in two of the cases from Oceania, is of very great interest, and deserves scientific investigation. In Mr Haggard's Japanese case alone do we hear of repeated applications of cold water indoors as distinct from the bathe. The softening and soaking of the calloused foot-sole by water would rather be an advantage than otherwise to the walker. It is highly probable, I think, that Col. Gudgeon would have had a customary afternoon bathe not long before the walk. As the *Tohunga* knew he was going to walk, it is not unlikely that he prescribed it.

3. In some other Indian cases observers have noted the muddy state of the legs of the walkers, others have referred to the water used to cool the rakes, or splashed about,³ and others again to small tanks into which the walkers stepped before walking.

4. In India, at any rate, cases of fakirs and their pupils who inure

¹ *The Lancet*, November 3, 1917 (and Dr. Seligman in *The Listener*, 14.2.34).

² Dr Seligman, *loc. cit.*

³ *Journal S.P.R.*, Oct. 1928.

their bodies to the heat of fires as well as to pain are too well known to need quoting. Walking sun-baked stones in the sheltered island of Bega may well have inured the soles and legs to heat, as Mr Allardyce suggests.

The heat endurable by a hardened foot-sole is greater than many people realise. We have Mr Langley's instance of natives standing barefoot on superheated steam pipes. The Editor of *Man* speaks¹ of a young acquaintance, a ship's engineer accustomed to walking barefoot, who could hold a lighted match under his heel until it burned out, feeling nothing and showing no signs of burning.

As far as the evidence goes, then, the quick formation of protective ash in the ember-walk, and the low conductivity of the basaltic rock in the stone-walk, the time taken to prepare the oven in both respects, the effects of exaltation and self-suggestion, strengthened by ascetic training, in reducing the temperature and increasing the insensibility of the body, aided by cold bathing, and lastly the calloused foot-soles of the walkers and their short quick steps, seem to relegate the fire-walk to the class of phenomena whose abnormal element does not transcend that of cases reported as of ordinary occurrence with good hypnotic subjects. The cases where the abnormal element seems strongest suffer evidentially through the omission of important details, and, apparently, malobservation of a serious kind.

One cannot conclude a study of the fire-walk without reference to the striking classical examples in fairly recent times of immunity to fire, namely the experiences of Sir William Crookes and others with D. D. Home,² too well known to relate, which seem to rest on such excellent testimony. But until more crucial evidence of the supernatural in the fire-walk is forthcoming there is, I think, little justification for corroborating the evidence for the supernatural in the fire-walk by reference to them, and still less *their* cases by reference to the fire-walk.

¹ *Man*, 1904, 31a.

² I am indebted to Father Thurston for drawing my attention to his article on Human Salamanders in *The Month* (Feb.-March 1932), in which these and other cases of similar immunity are described in detail. Father Thurston also describes an ember-walk from Mysore (c. 1922). Great emphasis is laid on the tremendous heat of the furnace (c. 13 × 16 × 1 ft.) on which several cartloads of dry palm-leaves were thrown, through which people walked quietly.

A NOTE ON THE RECENT EXPERIMENTS WITH RUDI SCHNEIDER

BY LORD CHARLES HOPE

I HAVE read with much interest the report written by Mr. Th. Besterman and Mr O. Gatty on their recent experiments with the medium Rudi Schneider (*Proc. S.P.R.*, xlii.) and their letter to *Nature*, 14th April, 1934.

Having attended a large number of sittings held with this medium during each of his five visits to London, I feel that a few comments from me would not come amiss and might be of interest to those readers of the *Proceedings* who are interested in the question of the present position of the Rudi Schneider mediumship. The series of sittings were all held in London, April 1929 ; Oct. 1929 to Jan. 1930 ; Feb. 1932 to May 1932 ; Oct. to Dec. 1932 ; Oct. 1933 to April 1934. The first three series were held at the National Laboratory of Psychical Research under the direction of Mr Harry Price,¹ the fourth was arranged privately by me and some friends of mine (*Proc. S.P.R.*, xli.) ; the fifth being his recent visit to the S.P.R.

While in no way wishing to criticise the patient and conscientious experiments conducted by Messrs. Besterman and Gatty during the recent sittings with Rudi Schneider, I consider that there are several points raised by this report and by their letter to *Nature* (14th April), which should be elucidated ; otherwise, I fear, such readers as have not closely followed the whole case and read the reports issued after former series of sittings might, inadvertently, be misled as to the sum total of available evidence for the production of paranormal phenomena by this medium.

Firstly, while it is nowhere stated in the Besterman-Gatty report that these experiments with infra-red light apparatus disprove the results reported by Dr Osty (" Les Pouvoirs Inconnus de l'Esprit sur la Matière, by E. and M. Osty), or those reported by Mr Herbert (*Proc. S.P.R.*, xli.), it is possible that some readers might come to that conclusion.

It should be noted that absorptions of an infra-red light beam have been reported only at those series of sittings in which the telekinetic phenomena noted have at times been of considerable range

¹ Harry Price, *Rudi Schneider*.

Bulletin IV. " An Account of Further Experiments with Rudi Schneider ", by Harry Price.

Lord Charles Hope, and others, *Proc. S.P.R.*, xli.

and frequency. In other words, although at sittings when absorptions have been noted there have often been no other phenomena, yet the two kinds of phenomena have alternated at fairly frequent intervals and on many occasions both kinds of phenomena have been reported as happening at the same sitting.

It is true that at only seven out of the seventy-seven sittings, conducted by Dr Osty in 1931, were any telekinetic phenomena noted (whereas at the majority of the sittings absorptions were observed), but it must be remembered that Dr Osty, for the greater part of his experiments, concentrated on trying to obtain absorptions of infra-red beams rather than telekinetic movement of material objects. Also, it is worth noting that during the majority of sittings Dr Osty dispensed with the usual "cabinet", and so the movement of the curtains, which is much the most frequent and nearly always the initial telekinetic phenomenon with this medium, was rendered impossible.

It would seem to be a significant fact that at both those series of sittings at which no absorptions were reported (see Th. Besterman's report, *Proc. S.P.R.*, xl. 428-436, on sittings in Paris, Feb.-March 1933, and the recent S.P.R. sittings, *Proc. S.P.R.*, xlii.), no telekinetic movements of any importance were observed either. This would seem to show that although it may be, as conjectured by Dr Osty, usually easier for the medium to effect with his "force" an absorption of an infra-red beam rather than to move a material object by telekinetic means, yet when telekinetic movements are absent from many consecutive sittings there is no reason either to expect absorptions.

Unfortunately, with the possible exception of some small curtain movements and some rather indefinite "touches", no phenomenon of any kind was reported during the recent series of sittings at the S.P.R.

In Messrs. Besterman and Gatty's letter to *Nature*, and also in their full Report in *Proceedings*, the point is somewhat stressed that small curtain movements, such as had already been noted from time to time, continued after a "check" apparatus had been devised which registered nothing paranormal. "In order to determine", runs this letter, "whether these (movements of curtains) were due to draughts, a strip of tinfoil about a foot in length was so hung about 5 mm. from a vertical metal plate that a slight draught brought them into contact and rang an electric bell. The whole was so placed that the 'force' had ready access to it and that draughts could not affect it. Under these conditions the bell did not ring, though the curtain continued to move."

It seems to be quite probable that these small curtain movements were indeed caused by draughts. It was not, however, made clear in the above letter or, I consider, in the full Report, that the "force",

to effect the desired contact, would first have had to get behind the curtains, enter a circular hole of 3" diameter in the lead box, and then turn at right-angles and advance about 8" before impinging on the tinfoil which was only about 4" in width.

It has frequently been noticed at former series of sittings with Rudi, when really impressive phenomena have occurred, that it has, almost without exception, been at the sittings at which the phenomena have been both frequent and varied that any degree of delicacy of aim or accomplishment has been achieved by "Olga". To move a curtain even violently indicates no very great sense of direction. To pick up objects, tie knots in handkerchiefs, touch sitters with the edge of a curtain after a warning to that effect has been already given by "Olga" and occasionally to place small objects into or take them away from the hands of sitters, does need a very considerable accuracy of aim and control of the "foree".

The alternative theory, that when such movements have been noted they have been achieved by means of the medium freeing a hand or foot, must of course be duly considered, but those who have had the experience of controlling this medium while successful sittings have been held are almost unanimous in thinking that theory untenable owing to the variety and continuity of the phenomena observed while the controllers were satisfied the control of the medium was perfect.

Although to effect the desired contact in Mr Gatty's above-mentioned apparatus might have required even less strength than to move the curtains ever so slightly, yet it would certainly have needed a much more complicated use of the "foree".

In their letter to *Nature*, Messrs. Besterman and Gatty say, "It has been alleged that the trance personality is aware of what goes on in the dark *séance*-room; in these sittings this was not found to be the case." In their full Report (*Proc. S.P.R.*, xlii.) they give details of unsuccessful attempts made by "Olga" to describe the position of a sitter's hand. This might be read as constituting a refutation of the observations made by sitters at previous series of sittings, whereas in point of fact no such disproof was obtained. Personally I have always doubted the supposed ability of "Olga" to "see" in the dark what the sitters are doing, although I think "she" has shown, on occasions, a certain hypersensitiveness of hearing and awareness to any unheralded experiment on the part of any of the sitters. But it is true that during the more successful sittings at which I have been present "Olga" has on several occasions displayed a very accurate idea of where such objects as have been seen or heard by the sitters to move during the sitting would be found when the white light was turned on at the end.

The red light used at the sittings was usually too faint and too restricted in its area of illumination to explain how this could have

been achieved by "Olga", but a careful record of "Olga's" successes and failures in that direction would have to be kept over a long series of successful sittings before any conclusion could be come to. The experiment done in conjunction with Dr C. G. Douglas of Oxford, described in the Report and also in the letter to *Nature*, in connection with the fast trance breathing hitherto so much commented on, is of great interest. If this single experiment can be relied on, and no doubt it was carried out with care, and we are assured the breathing of the medium while undergoing the tests was typical of his trance breathing during ordinary sittings, then it would certainly seem to disprove the result of Dr Osty's experiment ("Les Pouvoirs Inconnus de l'Esprit sur la Matière, p. 117) done by means of external measurements of the medium's chest while the trance breathing was in progress. Dr Osty's experiment had seemed to show that the volume of air breathed by the medium in trance was greatly above the normal.

The medium's brother, Willi Schneider, also a noted medium, when he was in a similar trance state, is said to have breathed in almost as rapid a manner as Rudi. This rapid breathing of the Schneider brothers has been held to be unique among physical mediums, and the discovery that Rudi after all only inhales about a normal amount of air into his lungs, brings their ease more into line with other impressive cases of physical mediumship, and supports the theory that the fast breathing is an acquired habit and is not really necessary for the production of paranormal phenomena. Both authors of the Report claim to have duplicated Rudi's fast breathing for considerable periods of time. I myself heard Mr Gatty do a successful imitation of Rudi's trance breathing for over half an hour at a time during a sitting; an excellent method of comparison although one, perhaps, unlikely to appeal to "Olga's" sense of uniqueness.

The result of the word-association and psychogalvanic reflex tests with Rudi and "Olga" is of great interest and will strengthen the opinion of those observers of the mediumship (and they form a majority) who had already come to the conclusion that "Olga" is a secondary personality of the medium's. It is indeed unfortunate that so much careful work and such a large financial outlay should have gone unrewarded by conclusive results as to the chief phenomena associated with this mediumship, and disappointing to all concerned that Messrs. Besterman and Gatty, after all their trouble, should not have had an opportunity of judging of the value of the phenomena in question such as a single good sitting would have given them.

At the end of March 1934, thinking it would be of considerable interest from a psychological point of view if the medium were asked to sit in a room in which he had previously produced impressive

phenomena (in 1929 and in Nov.-Dec. 1932), and to restrict the sitters as much as possible to those in whose presence he had given really successful sittings, I invited Rudi to stay on in London for an extra fortnight after completing his visit to the S.P.R. As phenomena were observed at the second and third of these extra sittings, he eventually remained for three weeks, and gave in all seven sittings from April 4th to 20th. No apparatus to detect the presence of the "force" was employed, as there was not sufficient time in which to arrange this, and in any case the medium appears to think that when his "force" is weak the use of such apparatus tends to inhibit his powers.

Owing to illness I was myself only able to attend the first sitting, which was a blank. Fortunately, however, among the sitters who attended most of these sittings were Professor A. F. C. Pollard, Professor D. F. Fraser-Harris, Mr C. C. L. Gregory and Captain the Hon. Victor Cochrane-Baillie, all of whom had had considerable experience with the medium during at least two of his former successful series of sittings, and the last named had also attended a large number of the S.P.R. series of sittings.

Out of the seven sittings held, four at least were positive (at a fifth one sitter thought there had been a small movement of the curtain, but this was not corroborated by the other sitters and therefore was discounted). The phenomena observed consisted chiefly of movements of the cabinet curtain, which was not hung with such delicate adjustment as those at the S.P.R. had been, and therefore required more "force" to move them. While these movements were at no time as large as have been reported at former sittings, they were large enough on occasions to be seen by all the sitters and the small bells attached to one curtain were fairly frequently heard to ring.

The sixth sitting was the best, and the movements of the curtains were quite frequent and considerable. Mr Gregory was asked by "Olga" to hold a waste-paper basket near to the centre of the two curtains. Accordingly, keeping contact with his neighbours, he balanced the basket on his hands. He felt several tugs on the end of the basket nearest to the curtains and eventually it was pulled off his hand on to the floor.

A technique, new as far as I know, was developed by "Olga" at these sittings. At the first sitting I was asked to hold the centre edge of the curtain nearest to the medium. I was subsequently asked by "Olga" if I could feel "her" pulling at it. I did not feel anything, but at later sittings Professor Fraser-Harris, Mr Gregory and Mr C. W. Fisk all had the experience of feeling the curtain tugged and when held somewhat lightly it was several times pulled out of the sitter's hand.

While in no way equal in variety or strength to the phenomena

displayed at earlier sittings in 1929 and 1932, the result of these extra sittings tend to show that Rudi Schneider has not lost his power of producing paranormal phenomena, and it is to be hoped that either in Austria or elsewhere it will be found possible to continue to experiment with him and see if the reported absorptions of infra-red light beams can be repeated and more closely examined and the production of telekinetic phenomena proved to the satisfaction of all.

A Comment on the above Note by Mr Oliver Gatty

I HAVE read with great interest Lord Charles Hope's "Note on the recent experiments with Rudi Schneider," and am very grateful to him for elucidating a number of potential misunderstandings.

The report on the recent investigations of Rudi Schneider at the Society for Psychical Research was confined almost exclusively to facts observed during those sittings, both for the sake of brevity and in the interest of impartiality. If this has led to any misunderstandings, I should like to apologise.

Lord Charles Hope raises the question as to the accuracy of the single experiment carried out on the respiration of the medium in trance. Four samples were taken and also one of the medium in a normal resting state. All five sets gave figures of substantially the same order of magnitude, and this may be said to be a sort of check on the figures. Further, the values for the respiratory coefficient depend only on the composition and not on the bulk of the exhaled gases. Thus the possibility of error due to leakage of the valves at high speeds is not likely seriously to affect the conclusions drawn (see *Proc. S.P.R.*, xlii. p. 261) from the data. The method of measuring total exhaled gases in a Douglas bag is superior to measurements of chest expansion (used by Dr Osty), since it also includes the respiration due to movements of the diaphragm. It therefore seems safe to conclude that there is nothing necessarily paranormal in the breathing of the medium, as it was also found possible to reproduce it at will.

REVIEW

The Case for Faith-Healing, by J. D. BERESFORD. With a Preface by the Very Reverend H. R. L. Sheppard. London: George Allen and Unwin. Pp. 186. Price 5s. net.

The case for faith-healing can be made incontrovertible if care be taken to indicate quite clearly the kinds of illness than can be cured by faith. It may be necessary to say what we mean by faith, what we mean by illness and what we mean by cure, but in the everyday meaning of these words it may be readily admitted that many conditions which are obvious departures from health may be relieved if the sick person has a deep-seated conviction, however achieved, that such relief will take place. The truth of this is most apparent when we are dealing with those forms of illness which are sometimes referred to as functional nervous disorders. When, however, we have to deal with illness due to or accompanied by gross organic changes the effects of faith as a healing agent are much more obscure. We are not justified in denying that physical changes may gradually be brought about by mental influences, but all our knowledge of the physical universe makes us sceptical when we are told that gross physical changes, such as the lengthening of a bone, may take place instantaneously as a result of faith.

Yet it is faith-healing of this kind which Mr. Beresford defends in this book, and it is because he accepts as already proven cures of this kind that his work is of little interest to men of science. For example, he says: "I have good authority for the instantaneous healing in February this year of a boy, with a retracted Achilles tendon and a malformation of the foot, due to infantile paralysis at the age of nineteen months." Yet he very rightly adds: "We can imagine a malignant growth changing its nature, dying out and becoming finally absorbed by healthy tissue. In such cases we can postulate a conceivable process that does not too greatly shock our sense of causation. But there is no conceivable natural process known to us that will account for the instantaneous creation of material necessary to lengthen the muscles, tendons and, in all probability, bone of that child's leg by nearly two inches." Nevertheless he believes in the truth of this story, for which he says he has good authority, and he believes in the miracles of Lourdes—"cures vouched for by medical and ecclesiastical authority". Being con-

vinced he says : " I . . . am now content to try and find some more or less rational justification for the facts, rather than to spend my energies in obstinate and unreasoning denial."

The aim of his book is the somewhat ambitious one of framing " a cosmology that would admit the possibility of miracles ". He seeks a rational justification of the facts, although he realises that there is no conceivable natural process known to us that will account for them. For the production of miracles of healing he does not invoke any external power. The power that acts in faith-healing is a power within the self, and it is merely because we do not know all the laws of nature that we regard ' miracles ' as a contravention of natural law. This power is attributed to a deeply hidden element of the self which is " able to influence the rhythm of the organism, even to upset and rebuild the arrangement of the body cells ".

Mr Beresford's conception of the nature of this element of the self is not easily grasped, and in some ways his different accounts of it seem contradictory. He begins by insisting that man is not a single but a multiple personality. He takes up the point of view adopted by Professor McDougall in his presidential address to our society and maintains that " the thinking ' I ' of Descartes is the indifferently capable leader of a mixed and at times unmanageable team of other ' I's ', some of which may remain throughout life beyond the reach of our conscious understanding ". He records that he came under the influence of Myers' teaching on the subliminal self and he quotes a passage from Maeterlinck about the " Unknown Guest ". He gets the impression from both these writers that the subconscious self is a separate entity, and he repudiates this suggestion of duality although he admits that " man is not a single but a multiple personality ". In the end he says there is no such separate entity as a subconscious self and no separated strata or levels of consciousness. Rather is he inclined to assume that consciousness is present in every cell of the body ; and the totality of consciousness is the sum of the consciousnesses of all the individual cells. Any group of cells that, as it were, think alike create a small superconsciousness within the confederation that we know as the personality ; and to bring about any faith-cure of the kind that works apparent miracles, *every element of the consciousness must be temporarily in harmony*. To bring this about there must be " a complete submission of the intellect and critical judgment ". Only thus can the integration of the personality necessary for faith-healing be achieved.

But, even with such complete integration of the personality as is implied in the " thinking alike " of all the individual cells, we have no grounds for supposing that such integration carries with it the power to work miracles of healing. Behind the psychological changes underlying integration of the whole personality Mr Beresford is forced to postulate some spiritual immortal principle in man,

which is developed by persistent effort through many incarnations and is related to that universal spirit which is the source of all life. "All matter is a temporal, spatial expression of spirit", and "this book is addressed without prejudice to all those who, however formally, subscribe to the idealistic account of the universe".

T. W. MITCHELL.

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L. P. Jacks, LL.D., D.D., Shotover Edge, Headington, Oxford.

Sir Lawrence J. Jones, Bart., 39 Harrington Gardens, London, S.W. 7.

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Sydney C. Scott, 92 Westbourne Terrace, London, W. 2.

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Professor Dr Freud, Berggasse 19, Vienna, Austria.

Professor Pierre Janet, rue de Varenne 54, Paris, France.

Dr C. G. Jung, Seestrasse 228, Kusnacht, E. Zurich, Switzerland.

Count Carl von Klinckowstroem, Agnesstrasse 44/°, Munich, Germany.

Maurice Maeterlinck, Villa des Abeilles, Nice, France.

Professor T. K. Oesterreich, Nauklerstrasse 23, Tübingen, Germany.

Dr Eugène Osty, Avenue Niel 89, Paris, France.
 Professor Charles Riehet, rue de l'Université 15, Paris, France.
 Dr Rudolf Tischner, Ditlindenstrasse 18, Munich, Germany.
 Carl Vett, 13 Kongen's Nytorv, Copenhagen, Denmark.
 Dr Elwood Worcester, 186 Marlborough Street, Boston, Mass., U.S.A.

HONORARY ASSOCIATES.

Dallas, Miss H. A., 9 Perryfield Road, Crawley, Sussex.
 Fryer, Rev. A. T., 63 Newbridge Road, Bath.
 Gow, David, 16 Queensberry Place, London, S.W. 7.
 Hill, J. Arthur, Claremont, Thornton, Bradford.
 Hoernlé, Professor R. F. A., University of the Witwatersrand,
 Johannesburg, S. Africa.
 Irving, Rev. W. S., Oxenhall Vicarage, Newent, Glos.
 Muirhead, Professor J. H., Dyke End, Rotherfield, Sussex.
 Sage, Prof. Charles M., rue de Coulmiers 33, Paris XIV^e, France.
 Stawell, Miss F. Melian, Littledore, 64 Sandfield Road, Headington,
 Oxon.
 Tanagra, Dr A., Odos Aristotelous 53, Athens, Greece.
 Thouless, Professor R. H., The University, Glasgow.
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MEMBERS AND ASSOCIATES.

An asterisk is prefixed to the names of Members.

- *Adams, B. A., Kilrea, Lindsay Road, Hampton Hill, Middlesex.
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- *Albemarle, Countess of, Quidenham, Norwich.
 Albree, John, Swampscott, Mass., U.S.A.
 Alexander, Professor S., 24 Brunswick Road, Withington, Manchester.
- Allan, Miss J., Invergloy House, Invergloy, Inverness-shire.
- *Allen, Professor H. J., Marley Orchard, Kingsley Green, Haslemere.
- *Allison, Mrs E. W., The Beverley, 125 East 50th Street, New York,
 U.S.A.
- *Alvarez, Dr Juan, 25 de Diciembre, 804, Rosario, Argentina.
- *Anderson, Lady, Ballydavid, Woodstown, Waterford.
 Anderson, Mrs Henry C., 20 Hermitage Drive, Edinburgh.
- *Andrae, Frau Edith, Cronbergerstrasse 7-9, Berlin-Grünwald,
 Germany.
 Andrews, Dr Marion B., 73 Shrubbery Avenue, Worcester.
- Anstey, Brigadier E. C., R.A., 14 Lytton Road, Quetta, Baluchistan,
 India.
- *Archer-Hind, Mrs, Little Newnham, Cambridge.
- *Ardron, G. H., e/o Hongkong and Shanghai Banking Co., 9 Gracechurch
 Street, London, E.C. 3.
- *Arnold-Forster, Mrs. H. C., Orchards, Dover Park Drive, Roehampton,
 London, S.W. 15.
- *Assheton-Smith, Lady, 30 Queen Anne's Gate, London, S.W. 1.

- *Auden, Harold A., The Research Institute, Great Burgh, Epsom, Surrey.
- Austen, H. W. C., M.D., Dormy Pool, Saxmundham Road, Aldeburgh.
- Bacon, Mrs Sewell, Gate House, Northwood, Middlesex.
- Bailey, Cyril, Balliol College, Oxford.
- *Baker, Miss Mabel, 17 Ivydale Grove, London, S.W. 16.
- *Balfour, Right Hon. the Earl of, P.C., LL.D., Fishers Hill, Woking.
- *Balfour, Miss, Whittingehame, Haddington.
- *Balfour, Dr Margaret I., 14 Sylvan Road, London, S.E. 19.
- *Balfour, The Lady Ruth, Balbirnie, Markinch, Fife.
- *Ball, Mrs J. H., 11 Nevill Park, Tunbridge Wells, Kent.
- *Barlow, Fred, Drakeford, Pool Head Lane, Wood End, Tanworth-in-Arden.
- Barrett, Lady, M.D., 31 Devonshire Place, London, W. 1.
- *Barrow, J. R., e/o Grindlay & Co., 54 Parliament Street, London, S.W. 1.
- *Bateheler, E., 3 Glyn Mansions, London, W. 14.
- *Bax, Clifford, G. 2, The Albany, London, W. 1.
- *Beadon, Mrs, 11 Cheyne Walk, London, S.W. 3.
- *Beattie, Ivor Hamilton.
- *Beaty, Amos L., 30 Sutton Place, New York City, U.S.A.
- *Behrens, Major Clive, Swinton Grange, Malton, Yorks.
- *Bell, Mrs A. H., Backwoods, Lindfield, Sussex.
- *Benediet, Professor H. Y., 3401 Guadalupe Street, Austin, Texas, U.S.A.
- *Bennett, Sir Ernest, M.P., 12 Prince Arthur Road, London, N.W. 3.
- Benson, Miss A. M., M.D., 32 Wood Lane, London, N. 6.
- *Bentley, W. P., 4214 Swiss Avenue, Dallas, Texas, U.S.A.
- *Besterman, Theodore, e/o Westminster Bank, Tavistock Square, London, W. C. 1.
- *Besterman, Mrs Theodore, 22 Woburn Square, London, W.C. 1.
- Bidder, George P., Cavendish Corner, Hills Road, Cambridge.
- *Birley, Mrs Oswald, Corner House, 62 Wellington Road, London, N.W. 8.
- Blathwayt, W., Eagle House, Batheaston, Bath.
- *Blennerhassett, Mrs Richard, 52 Hans Place, London, S.W. 1.
- Bligh, Stanley M., 19 Clareville Grove, London, S.W. 7.
- Bois, H. G., Billesley Manor, Alcester, Warwickshire.
- *Bolton, Edward J., Rossett Hall, nr. Wrexham.
- *Bond, F. Bligh, 15 Lexington Avenue, New York, U.S.A.
- Bond, Mrs W. C. Shalesbrook, Forest Row, Sussex.
- Bosanquet, Miss Theodora, 1B Bay Tree Lodge, Frognaal, London, N.W. 3.
- *Bousfield, W. R., K.C., F.R.S., St Swithins, Northwood, Middlesex.
- Bouwens, B. G., Old Manor House, Littleton, Shepperton, Middlesex.
- Bowen, Miss Anna C., Hotel Richmond, Batavia, N.Y., U.S.A.
- Boxer, Miss Cécile F., Firwood, Alum Chine, Bournemouth.
- *Brandt, Rudolph E., 36 Fenchurch Street, London, E.C. 3.
- *Branson, J. R. B., Headley Mill Farm, Bordon, Hants.

- Brewster, Bertram, 59 Madeley Road, London, W. 5.
- *Brinton, R. S., Croft House, Kidderminster.
- Bristowe, The Hon. L. S.
- *Broad, Professor C. D., Litt.D., Trinity College, Cambridge.
- *Broch, Dr Leon, 76 Cuba Street, Habana, Cuba.
- *Brooke, Anthony W., The Priory, Redbourn, Herts.
- *Brown, B. H. Inness, 120 Broadway, New York City, U.S.A.
- Brown, Charles, M.B., Tinto Bank, Kirkcaldy.
- *Brown, William, M.A., M.D., D.Sc., F.R.C.P., 88 Harley Street, London, W. 1.
- *Browne, O. H., Fort House, Eastern Parade, Southsea, Hants.
- *Bruce, Rev. W. J. Wallace, Meath Park, Coleraine, Co. Londonderry.
- *Bruck, Dr Carl, Prenzlauer Allee 25, Berlin N.O. 55, Germany.
- *Bulford, Staveley, 20 Eaton Terrace, London, S.W. 1.
- Buller, Professor A. H. Reginald, University of Manitoba, Winnipeg, Canada.
- *Bulley, Mrs Arthur, Ness, Neston, Wirral, Cheshire.
- Burnham, Mrs.
- *Burrett, Mrs F. J., Moorland Cottage, Linkside, Hindhead, Surrey.
- *Bury, Henry, The Gate House, Alumdale Road, Bournemouth.
- Bury, Mrs Henry, The Gate House, Alumdale Road, Bournemouth.
- Butler, Mrs Charles, Evendale, Falmouth, Cornwall.
- *Butt, Lady, c/o Midland Bank, Gloucester Road, London, S.W. 7.
- *Butterfield, Mrs W. J. A., Ebor House, 71 Crystal Palace Park Road, London, S.E. 26.
- Cairns, Rev. David S., D.D., 62 Hamilton Place, Aberdeen.
- *Campbell, Mrs, Arduaine, Lochgilphead.
- Campbell, Colin E., 35 Ebury Street, London, S.W. 1.
- *Campbell, Mrs E. Kenneth, Millfield, Wittersham, Kent.
- Campbell, Rev. Canon R. J., D.D., Heatherdene, Fairwarp, nr Uckfield, Sussex.
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- Candler, H. A. W., 43 Gondar Gardens, London, N.W. 6.
- Candler, Miss M. L., 43 Gondar Gardens, London, N.W. 6.
- *Cannon, Alexander, K.C.A., M.D., Ph.D., etc., L.C.C. Mental Hospital, Bexley, Kent.
- *Carden, W. A., M.R.C.S., Korea, Trappes Street, Worcester, C.P., S. Africa.
- *Carington, W. Whately, Calandstraat 64, Rotterdam, Holland.
- *Carpenter, Mrs, Withleigh, West Heath Road, London, N.W. 3.
- Carrington, Hereward, Ph.D., 12 East 88th Street, New York City, U.S.A.
- *Carruthers, Miss Helen, 10 King's Bench Walk, London, E.C. 4.
- *Carter, Mrs H. A., 190 Mount Vale, York.
- *Carthew, Lieut.-Colonel T., D.S.O., 2 Mitre Court Buildings, London, E.C. 4.
- Case, Miss A. J., 17 Bailbrook Road, Batheaston, nr Bath, Somerset.
- *Cave, Charles J. P., Stoner Hill, Petersfield, Hants.
- *Chambers, John, Mokopeka, Hastings, Hawkes Bay, New Zealand.

- *Chant, Stephen, Spring Cottage, Sanderstead, Surrey.
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Chitty, Hubert, M.S., F.R.C.S., 46 Pembroke Road, Clifton, Bristol.
- Clapham, J. H., King's College, Cambridge.
- Clarkson, Mrs St John, 27 Ovington Street, London, S.W. 3.
- Clay, The Hon. Mrs, 11 Tite Street, London, S.W. 3.
- *Clements, Mrs P. H., 26 St Andrews Road, Earlsdon, Coventry.
- Closson, Miss Olivia T., 206 The Shawmut, 2200 Nineteenth Street,
N.W., Washington, D.C., U.S.A.
- Clwyd, Lady, Bryngwenallt, Abergele, N. Wales.
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London, N.W. 1.
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- *Cole, The Lady Eleanor, Fishers Hill, Woking.
- *Collins, George E., 4 Hawthorn Avenue, Wilmslow, Manchester.
- *Collins, Seward, 375 Park Avenue, New York City, U.S.A.
- *Collins, H. S., Leicester Court Hotel, Queen's Gate Gardens, London,
S.W. 7.
- *Conybeare, Miss I., 349 Latymer Court, London, W. 6.
- *Cooke, P. A., 7 Foulser Road, London, S.W. 17.
Coombe-Tennant, A. H. S., 73 Portland Place, London, W. 1.
- Cooper, Rev. Canon F. W., Prestwich Rectory, Manchester.
- Cornish, J. Easton, Cutley House, Moustapha Pasha, Alexandria,
Egypt.
- *Corry, Miss Luey, 13 Argyll Road, London, W. 8.
Cort van der Linden, P. W. A., Minister of State, 59 Koninginnegraecht,
The Hague, Holland.
- Coxe, Henry R. H., 6 Queen's Parade, Bath.
- *Craig, Mrs John Diekey, Bankers Trust Co., 501 Fifth Avenue, New
York City, U.S.A.
- *Crandon, L. R. G., M.D., 366 Commonwealth Avenue, Boston, Mass.,
U.S.A.
- *Critehley, Maedonald, M.D., F.R.C.P., 137 Harley Street, London,
W. 1.
- *Crocker, Lieut.-Colonel H. E., C.M.G., 7 Wrights Lane House, Ken-
sington High Street, London, W. 8.
- *Crosfield, Miss M. C., Greensand, 78 Doods Road, Reigate, Surrey.
- *Crowther, Miss A. G., 64 Wembley Park Drive, Wembley Park, Middx.
- *Crunden, Mrs W. M., e/o Crunden & Martin Mfg. Co., St Louis, Mo.,
U.S.A.
- *Crunden, Miss Edwina, e/o Crunden & Martin Mfg. Co., St Louis,
Mo., U.S.A.
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Daniell, Mrs Averell.
- D'Arej, Rev. G. J. A., The Viearage, Worksop, Notts.
- *Davidson, Miss A. M. Campbell, Villa Mont Jean, Cavalaire (Var),
France.

- *Davies, Benjamin, 6 South Marine Terrace, Aberystwyth, Cardigan-shire.
- *Davies, Powys, 9 Lansdowne Road, Bedford.
- Davys, Lieut.-Colonel G. I., O.B.E., I.M.S., c/o Grindlay & Co., 54 Parliament Street, London, S.W. 1.
- Day, Miss Mabel K., 15 Elgin Court, London, W. 9.
- Dearmer, Rev. Dr Percy, 4 Little Cloisters, Westminster Abbey, London, S.W. 1.
- *de Gernon, Mrs, 16 Cheniston Gardens, London, W. 8.
- *de Janasz, Robert, 40 Ennismore Gardens, London, S.W. 7.
- De Jong, Dr K. H. E., Beeklaar, 356, The Hague, Holland.
- *De Koven, Mrs Reginald, 1025 Park Avenue, New York City, U.S.A.
- Deland, Mrs Lorin F., Kennebunkport, Maine, U.S.A.
- *Dewar, Lady, Brookhill House, Cowfold, Sussex.
- *Dewar, Rev. Canon Lindsay, 9 Precentor's Court, York.
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- De Wyckoff, Joseph, Arlena Towers, Ramsey, N.J., U.S.A.
- *Dhar, Bansi, Government Intermediate College, Fyzabad, U.P., India.
- *Dick, Lieut.-Colonel Maxwell, M.B., B.S., LL.B., 13 Bedford Park Mansions, The Orchard, Chiswick, London, W. 4.
- Dickinson, Miss, Trebrea Lodge, Tintagel, Cornwall.
- *Dickson, Mrs B. W. A., Little Bridgen, Bexley, Kent.
- *Dingwall, E. J., D.Sc., 14 Selwyn Gardens, Cambridge.
- *Dixon, Edward T.
- Dixon, Hugh N., 17 St Matthew's Parade, Northampton.
- *Dixon, Professor W. Macneile, LL.B., Litt.D., 2 South Park Terrace, Hillhead, Glasgow.
- *Dodds, Professor E. R., 6 Sir Harry's Road, Edgbaston, Birmingham.
- Donne, Mrs, c/o Messrs Holt & Co., 3 Whitehall Place, London, S.W. 1.
- *Dowdall, Hon. Mrs, Melfort Cottage, Boar's Hill, Oxford.
- *Doxford, W. R. Dallas, 43 Swan Court, London, S.W. 3.
- Drew, Ringrose C., Muar, Johore, Malay States.
- Driesch, Professor Dr Hans, Zöllnerstrasse 1, Leipzig, Germany.
- *Duckworth, Mrs Arthur, 43 Catherine Street, London, S.W. 1.
- *Due-Petersen, Jens, Aabyllille, Aabyhof, Denmark.
- *Duff, J. R. K., 100 Sunningfields, London, N.W. 4.
- *Dundas, Mrs. R. W., 39 Hill Street, London, W. 1.
- Dunne, D. P., 137 Victoria Street, London, S.W. 1.
- *Dupré, Pierre V. C. W., The Firs, Spaniards Road, Hampstead Heath, London, N.W. 3.
- *Duveen, Mrs Henry, 5 Stanhope Place, London, W. 2.
- *Earl, Dr C. J. C., Caterham Mental Hospital, Caterham, Surrey.
- *Earle, Arthur B., Puddington, Wirral, Cheshire.
- *Edgeworth, F. H., M.D., 20 Combe Park, Bath.
- *Edwards, Francis H., 3B Fulwood Park, Liverpool.
- *Elder, Frederick, 21 Essex Villas, London, W. 8.
- Elliot, Mrs Gilbert, 60 Wynnstay Gardens, London, W. 8.

- *Enger, G., 6 Rue Chérif Pacha, Alexandria, Egypt.
- *Enthoven, Mrs F. V., 44 Carlton Hill, London, N.W. 8.
- *Evans, Laurence A., Mitre House, Chelsea, London, S.W. 3.
- *Evans, S. I. J., 3 Chancery Lane, Port of Spain, Trinidad, B.W.I.
- *Evelyn, C. J. A., Wotton House, Dorking, Surrey.
- *Ezra, Alwyn, 143 Esplanade Road, Fort, Bombay, India.
- *Feilding, Hon. Everard, 5*n* Montagu Mansions, London, W. 1.
Felkin, Mrs, 119 Grosvenor Road, London, S.W. 1.
- *Fermor-Hesketh, Lord, Easton Neston, Towcester, Northamptonshire.
- *Findlater, J. W., Aydon, Cutenhoe Road, Luton, Beds.
- *Fischer, S., 305 Broadway, New York, U.S.A.
- *Fisk, George W., Ditton Grange, Ditton Hill, Surbiton, Surrey.
- *Fitch, Hugh B.
- *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.
Flugel, Professor J. C., 11 Albert Road, London, N.W. 1.
- *Forman, Josef, c/o American Express Co., Via Tornabuoni, Florence,
Italy.
- *Foot, Miss Katharine, 31 Brookfield Mansions, London, N. 6.
Footner, Mrs, 22 Meadway, London, N.W. 11.
- *Francis, Mrs Francis.
- *Fraser-Harris, Professor D. F., M.D., D.Sc., The Athenaeum Club, Pall
Mall, London, S.W. 1.
- *Fraser-Harris, Mrs D. F., 3 Grove Park Lodge, London, W. 4.
Frecman, Miss Adelaide C., Queen Anne's Mansions, London, S.W. 1.
- *Frecman, Rev. Canon, Bentley, Clifton Hill, Bristol.
- *Frith, Mrs W., Rutherglen, Clarendon Road, Watford, Herts.
- *Fry, Miss Agnes, Kilve Court, Bridgwater, Somerset.
- *Gantz, Mrs W. L., 10 Bywater Road, London, S.W. 3.
- *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, Khaiban Arbab Jamshid, Teheran,
Iran.
- *Gibbes, Miss E. B., 25 Jubilee Place, London, S.W. 3.
- *Giglio, E., 21 Mincing Lane, London, E.C. 3.
Giles, Mrs, 19 Highfield Road, Edgbaston, Birmingham.
- Gilson, R. Cary, Quilters, West Chilmington Common, by Pulborough,
Sussex.
Glasson, J. W., I.C.S., Glanusk, Soy, Kenya Colony.
- *Godsal, W. C., Haines Hill, Twyford, Berks.
- *Goldney, Mrs K. M., Stuart Hotel, Richmond Hill, Surrey.
- *Goldschmidt, Mrs de, Lane End, Burchetts Green, Berks.
Gooch, Dr G. P. South Villa, Campden Hill Road, London, W. 8.
- *Goossens, Mrs F. W.

- *Gore, Miss K. C., Peacock Hall, Little Cornard, Sudbury, Suffolk.
Gough, A. B., Ph.D., Briar Cliff, Sevenoaks, Kent.
- *Grant, Malcolm, The Mall House, Castle Townshend, Skibbereen, Co. Cork.
- *Grant-Suttie, Major H. F., 19 Midmar Gardens, Morningside, Edinburgh.
- *Green, Lady, Gotwick Manor, East Grinstead, Sussex.
Green, Mrs Alan B., Acton Castle, Marazion, Cornwall.
- *Greenwood, L. H. G., Emmanuel College, Cambridge.
- *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 de l'Intendant 193, Brussels-Molenbeck, Belgium.
- *Grubbe, Hubert H., 22 Park Lane, Southwold, Suffolk.
- *Grugeon, C. L., The Chestnuts, Henley-on-Thames.
- *Grundy, Professor W. W., Lerryn, Rhiwbina, nr Cardiff.
- *Gurdon, John, c/o Midland Bank Ltd., 20 Eastcheap, London, E.C. 3.
- *Gwyn, W. J., 8 Netherhall Gardens, London, N.W. 3.
Haldar, Professor Hiralal, 12 Duff Street, Calcutta, India.
Hall, Wilfred, 9 Priors Terrace, Tynemouth, Northumberland.
- *Hamilton and Brandon, The Duchess of, 25 St Edmund's Terrace, London, N.W. 8.
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, Mrs B., Great Surries, East Grinstead, Sussex.
- *Hannen, E. C., Ouseleys, Wargrave, Berks.
- *Harding, Norman, 18 North Common Road, London, W. 5.
- *Hare, Dr A. W., 59 York Road, Birkdale, Lancs.
- *Hargreaves, J. N. D., 78 Buckingham Gate, London, S.W. 1.
Harley, Dudley, 324 West John Street, Martinsburgh, West Virginia, U.S.A.
Harriman, T. G., M.D., 3768 American Avenue, Long Beach, Cal., U.S.A.
- *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, William, Downs View, Tattenham Corner, Epsom.
Hart, Mrs H. H., 29 Abercorn Place, London, N.W. 8.
- *Hart, Hornell, Ph.D., Hartford Theological Seminary, Hartford, Conn., U.S.A.
- *Haslam, Oliver H., Cairngill, nr Dalbeattie, Kirkcudbrightshire.
- *Hawkins, J. Gordon, 915 Franklin 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, Gerald, 28 Portman Court, London, W. 1.

- Heaton, Guy, 51 Westeliff Road, Bournemouth.
 Hemenway, Mrs Augustus, Readville, Mass., U.S.A.
 *Henderson, Miss Hester M., M.B., 25 Ferndale, Tunbridge Wells, Kent.
 Henderson, J. J., 28 Washington Avenue, West Caldwell, N.J., U.S.A.
 Henderson, Miss Lilian, The Sesame Club, 49 Grosvenor Street, London, W. 1.
 *Herbert, A. S., Collingtree, Elgood Avenue, Northwood, Middx.
 *Herbert, Benson, Dunelm, Lostock Park, Bolton, Lanes.
 *Herbert, C. V. C., 3 King's Mansions, Lawrence Street, London, S.W. 3.
 *Hereod, Ernest, 38 Avenue de Rumine, Lausanne, Switzerland.
 *Herrman, Osear, 527 West 34th Street, New York, U.S.A.
 *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, St James's Lodge, London, S.W. 6.
 *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.
 Home, Miss Mary M.
 Hookham, Philip, Shottery Cottage, Shottery, Stratford-on-Avon.
 *Hope, Lord Charles M., Swinley Hurst, Ascot, Berks.
 *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.
 *Howden, Richard A., 27A Cornwall Gardens, London, S.W. 7.
 *Howell, Mrs Philip, 5 Carlyle Square, London, S.W. 3.
 Howgrave-Graham, Captain A., Randfontein, Transvaal, S. Africa.
 *Hume, Lieut.-Colonel W. J. P., C.M.G., Brinksway, Lynchemere, Sussex.
 Hume-Rothery, J. H., Mendip House, Headington Hill, Oxford.
 Humphreys, T. H. A., 4 Park Place, London, S.W. 1.
 Hurwitz, W. A., Ph.D., White Hall 8, Ithaca, N.Y., U.S.A.
 Hutcheson, F. W. H., Grove Lawns, St Albans, Herts.
 *Hutcheson, Mrs, Spelmonden, Goudhurst, Kent.
 *Huxley, Professor Julian S., King's College, London, W.C. 2.
 *Hyland, C. W., Ph.C., 300 Commissioner Street, Johannesburg East, Transvaal, S. Africa.
 *Hynes, Miss G., 41 Haverstock Hill, London, N.W. 3.
 *Hyslop, George H., M.D., 3447 80th Street, Jackson Heights, Long Island, New York, U.S.A.
 Imamura, Prof. Shinkiehi, 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.
Jaye, William R., Beldornie Tower, Pelham Field, Ryde, I.W.
- Jenkinson, W. E., Willow Hall, Sowerby Bridge, Yorks.
- *Jephson, Miss Ina, Ladyoak, Flimwell, Kent.
- *Jerome, Adrian, Rose Bank, Shanklin, I.W.
- Johnson, Miss F. C., 25 York Street Chambers, London, W. 1.
- *Johnson, Miss G. M., 6 Carlton Hill, London, N.W. 8.
- *Johnson, James MacNeill, Aberdeen, North Carolina, U.S.A.
- *Johnston, Sir Reginald F., C.M.G., C.B.E., K.C.M.G., 18 Mortlake Road, Kew Gardens, Surrey.
- *Johnston, Dr William B., Giverny, par Vernon, Eure, France.
- *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, S. L., Rangpur, North Bengal, India.
- *Kakucs, Baroness de, The Estate House, Heytesbury, Wilts.
- *Kellogg, F. R., 120 Broadway, New York City, U.S.A.
Kelly, Miss Eleanor B., The Lookout, Peacehaven, Sussex.
- *Kennedy, Miss H. E., 56 Westwood Avenue, S. Harrow, Middx.
- *Kingsley, Mrs, Lee Ness, Channel Way, Fairlight, Sussex.
- *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, Arthur H., Woodcroft, Oxted, Surrey.
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., 70 N. Judd Street, Honolulu, T.H.
- *Lebrecht, Miss L., Mosbacherstr. 14, Wiesbaden, Germany.
Lee, Blewett, P.O. Box 152, Station C., Georgia, U.S.A.
- *Lee, Dr H. D. C., 32 New North Road, Huddersfield, Yorks.
- *Lees, Norman D., A.I.C., Norton Hall, Norton-on-Tees, Co. Durham.
- Leon, Mrs Philip, 4M Montagu Mansions, London, W. 1.

*Leonard, Miss Margaret.

Leopold, Dr H. M., Oranjelaan 7, Hilversum, Holland.

*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, Amsterdam Free Library, Amsterdam, N.Y., U.S.A.

*Librarian, Studievereniging voor "Psychical Research," Universiteits-Bibliotheek, Amsterdam, Holland.

*Librarian, Prussian State Library, Berlin, Germany.

Librarian, The City Public Libraries, Birmingham, 1.

*Librarian, The University, Birmingham.

Librarian, Jamsetjee Nesserwanjee Pctit Institute, Bombay, India.

Librarian, Boston Athenaeum, Beacon Street, Boston, Mass., U.S.A.

Librarian, Public Library, Brighton.

*Librarian, The University, Bristol.

Librarian, Bowdoin College Library, Brunswick, Maine, U.S.A.

Librarian, Grosvenor Library, Buffalo, N.Y., U.S.A.

Librarian, Harvard College Library, Cambridge, Mass., U.S.A.

Librarian, Meadville Theological School, 5707 Woodlawn Avenue, Chicago, Ill., U.S.A.

*Librarian, University of Cincinnati, Cincinnati, Ohio, U.S.A.

Librarian, Adelbert College of Western Reserve University, Cleveland, Ohio, U.S.A.

*Librarian, Public Library, Cleveland, Ohio, U.S.A.

Librarian, New Hampshire State Library, Concord, N.H., U.S.A.

Librarian, Selskabet for Psykisk Forskning, Ny Vestergade 7, Copenhagen, Denmark.

*Librarian, Iowa State Library, Des Moines, Iowa, U.S.A.

*Librarian, Glasgow Society for Psychological Research, 102 Bath Street, Glasgow.

Librarian, The University, Glasgow.

Librarian, Pennsylvania State Library, Harrisburg, Pa., U.S.A.

*Librarian, Case Memorial Library of the Hartford Seminary Foundation, Hartford, Conn., U.S.A.

*Librarian, Haverford College Library, Haverford, Pa., U.S.A.

Librarian, Leeds Library, Leeds.

Librarian, Hon., Constitutional Club, London, W.C. 2.

Librarian, Dr Williams's Library, Gordon Square, London, W.C. 1.

Librarian, Guildhall Library, London, E.C. 2.

Librarian, Theosophical Society, 12 Gloucester Place, London, W. 1.

*Librarian, Public Library, Los Angeles, Cal., U.S.A.

*Librarian, John Rylands Library, Manchester.

Librarian, Public Library, Melbourne, Australia.

Librarian, The University of Minnesota, Minneapolis, Minn., U.S.A.

*Librarian, Bavarian State Library, Munich, Germany.

Librarian, Literary and Philosophical Society, Newcastle-upon-Tyne.

Librarian, Public Library, Newcastle-upon-Tyne.

Librarian, Yale University, New Haven, Conn., U.S.A.

Librarian, Public Library, Jersey City, New Jersey, U.S.A.

- Librarian, Public Library, New York, U.S.A.
- *Librarian, General Theological Seminary, Chelsea Square, New York, U.S.A.
- *Librarian, Public Library, Omaha, Nebraska, U.S.A.
- Librarian, Norsk Selskap for Psykisk Forskning, Lille Ekeberg gaard, O. Aker, 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.
- Librarian, Theological Seminary, Rochester, N.Y., U.S.A.
- Librarian, Mercantile Library Association, St Louis, Mo., U.S.A.
- *Librarian, James Jerome Hill Reference Library, St Paul, Minn., U.S.A.
- Librarian, Public Library, Salt Lake City, Utah, U.S.A.
- Librarian, Public Library, Seattle, Washington, U.S.A.
- *Librarian, Swarthmore College, Swarthmore, Pa., U.S.A.
- 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.
- *Llewellyn, Mrs W., Upton House, nr Poole, Dorset.
- *Lloyd, Miss Edyth M.
- Lloyd, Miss Julia, Hillside, Balsall Common, nr Coventry.
- *Lloyd-Jones, Mrs, 104 Draycott Avenue, Kenton, Middlesex.
- Lodge, Dr Eleanor C., C.B.E., 5 Fyfield Road, Oxford.
- *Lodge, F. Brodie, The Lawn, Rugby.
- *Lodge, Mrs F. Brodie, The Lawn, Rugby.
- *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. Cceil, 3 Rawlinson Road, Oxford.
- Macdonald, Miss Isabelle M., M.B., 47 Seymour Street, London, W. 1.
- *Machin, Mrs H. A. C., Kenora, Ontario, Canada.
- *Mackay, A. G., M.B., 10 Spence Street, Newington, Edinburgh.
- Maekay, N. Douglas, M.D., Dall-Avon, Aberfeldy, Perth.
- *Mackenzie, Mrs J. O., 15 Lyndhurst Gardens, London, N.W. 3.
- *Mackeson, Mrs Peyton, 1 Eldon Road, London, W. 8.
- Maeklin, Miss H. E., White Wood Corner, Sandy, Beds.
- *Maeky, Mrs, 64 Wembley Park Drive, Wembley Park, Middlesex.
- Madders, Mrs H. F., 87 Hampstead Way, London, N.W. 4.
- *Magranc, Mrs Vietor, Dockyard House, Sheerness.
- *Mahony-Jones, Mrs, M.B., 1 Culverden Gardens, Tunbridge Wells.
- *Mallet, E. Hugo, 14 St. James's Square, Bath.

- *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.
- Marston, Sir Charles, 4 Camden Park, Tunbridge Wells, Kent.
- Marten, Miss A. R., Osbornes, Liphook, Hants.
- *Marten, Ven. G. H., 11 Percival Road, East Sheen, London, S.W. 14.
- *Mather, Rev. Herbert, Royal Automobile Club, Pall Mall, London, S.W. 1.
- Mather, L. G., 15 Mitcham Park, Mitcham, Surrey.
- *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.
- *McCombe, John, M.D., Shaughnessy Bldg., 407 McGill Street, Montreal, Canada.
- 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.
- McKeever, Buell, The Chicago Club, Michigan Ave. and Van Buren Street, Chicago, Ill., U.S.A.
- *McLagan, P. W., 4378 Western Avenue, Westmount, Quebec, Canada.
- McLauchlan, G. M., c/o Dr J. J. Dunne, Port Alfred, C.P., S. Africa.
- Meck, Maximilian de, 27 Boulevard de Ccssole, Nice, France.
- Meebold, Alfred, Heidenheim, Wurtemberg, Germany.
- Mehrji, M. H., M.D., Yusuf Building, Esplanade Road, Fort, Bombay, India.
- *Merritt, O. K., Mt. Airy, North Carolina, U.S.A.
- *Micklethwait, Richard K.
- *Millard, Mrs Almira B., c/o The F. W. Faxon Co., 83 Francis Street, Boston, Mass., U.S.A.
- *Miller, G. B., Brentry, Romsey, Hants.
- *Minns, Christopher, 32 Parkway, Welwyn Garden City, Herts.
- *Minns, Mrs C., 32 Parkway, Welwyn Garden City, Herts.
- *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., 224 Hamlet Court Road, Westcliff-on-Sea, Essex.
- *Mounsey-Wood, Mrs, 58 Elmhurst Road, Reading, Berks.
- *Muir, Mrs W. E., Rowallan, Haslemere, Surrey.
- *Mullins, Colonel W. B., Ambersham House, Midhurst, Sussex.

- *Mure, Lady Georgiana, 19 Cliveden Place, London, S.W. 1.
- *Murphy, Professor Gardner, Ph.D., Columbia University, N.Y., U.S.A.
- *Murray, Lady, Cleveland House, St. James's Square, London, S.W. 1.
- Murray, Professor Gilbert, LL.D., Litt.D., Yatscombe, Boars Hill, Oxford.
- *Myers, Mrs F. W. H., 12 Cleveland Row, St James's, London, S.W. 1.
- *Myers, Harold H., Ovington House, Ovington Square, London, S.W. 3.
- *Myers, L. H., Leckhampton House, Cambridge.
- *Nash, Miss Diana, Point of Pines, Tryon, N. Carolina, U.S.A.
- *Naumburg, Miss Margaret, c/o Messrs Straus & Kenyon, 51 East 42nd Street, 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, Ilyd B., 68 Holland Park, London, W. 11.
- *Nicholson, Dr C. R., 37 Greystoke Lodge, Hanger Hill, London, W. 5.
- *Nicol, J. Fraser, 2 Observatory Road, Edinburgh, 9.
- *Nicoll, Mrs De Lancey.
- *Nijinsky, Madame, c/o Barclays Bank, 1 Pall Mall, London, S.W. 1.
- *North, Sidney V., Poste Restante, Southwick, Sussex.
- *Nunn, Miss M., 2 Leinster Gardens, London, W. 2.
- *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.
- *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, C. G., C.I.E., Duncan, Vancouver Island, British Columbia.
- *Palmer, John W. G., 13 New Road, Brighton, Sussex.
- *Palmsteirna, 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., Old Farm House, 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, Hon. Mrs Dudley, Old Priory, Brightwell, nr Wallingford, Berks.
- Perkins, Miss S. R., Holly Bank, Bracknell, Berks.
- *Perrott, F. D., Longford, Horley, Surrey.
- 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/o Mrs Colin Gow, 42 Drayton Gardens, London, S.W.10.
- *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.
Pigou, Professor Arthur Cecil, King's College, Cambridge.
- Piper, John E., LL.B., 10 Herondale Avenue, London, S.W. 18.
- Pithapuram, The Maharajah of, Pithapuram, Madras Presidency, India.
- *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, Rother Cliff, Rye, Sussex.
- *Prebble, Mrs, Fairhaven, Upper Strickland Avenue, Hobart, Tasmania.
- *Preedy, Kenelm, 1 Ashburn Place, London, S.W. 7.
- *Price, Harry, Arun Bank, Pulborough, Sussex.
- *Price, H. H., Trinity College, Oxford.
- *Putman, Miss Irene, 328 Walnut Avenue, San Diego, Cal., U.S.A.
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.
- *Rampling-Rosc, Major W., The Llyndir, Rossett, Denbighshire.
Ramsden, Miss H., Sesame Club, 49 Grosvenor Street, London, W. 1.
- *Ransom, Francis, Newlands, Hitchin, Herts.
- *Rashleigh, John C. S., M.D., Throwleigh, Okehampton, Devon.
- *Rathey, 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.
Richardson, Frederick W., County Analyst's Office, Hustlergate, Bradford, Yorkshire.
- *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.
- *Ritchie, A. J., Oriental Club, Hanover Square, London, W. 1.
- *Ritson, F. A., 4 Bon-Accord Crescent, Aberdeen.
Riviere, Mrs Evelyn, 3 Stanhope Terrace, London, W. 2.
- Rix, Rev. A. H., 37 St Paul's Road, Manningham, Bradford.

- *Roberts, C. E. Bechhofer, Leylands Farm, Abinger Common, nr Dorking, Surrey.
- *Robson, Major J. S., Hales Place, Tenterden, Kent.
- *Rogers, George F., M.D., 4 King's Parade, Cambridge.
- *Romanes, F. J., The Brick House, Duton Hill, Dunmow, Essex.
- *Rothschild, Miss Miriam L., 4 Palace Green, London, W. 8.
- *Röthy, C., 1 Zuhatag-Gasse 5, Budapest, Hungary.
- *Rowtree, W. S., 15 Chatsworth Road, Brighton, Sussex.
- *Russell, Dr A. V., 4 Oaks Creseent, 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.
- *Schiller, Professor F. C. S., D.Sc., F.B.A., Corpus Christi College, Oxford.
- *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.
- *Scott, Sydney C., 92 Westbourne Terrace, London, W. 2.
- *Selborne, The Earl of, K.G., Blackmoor, Liss, Hants.
- Shand, Alexander F., 1 Edwardes Place, London, W. 8.
- Shastri, B. G., Kala Mehta's Street, Sagrampura, Surat, India.
- *Shaw, Mrs Bernard, 4 Whitehall Court, London, S.W. 1.
- *Sidgwick, Mrs Henry, Fishers Hill, Woking.
- *Siepmann, Mrs, c/o C. A. Siepmann, B.B.C., Portland Place, London, W.1.
- Simpson, Miss E. C. Price, Beech Barns, Alton, Hants.
- *Sinclair, Miss May.
- Singh, Amar, Hoshiarpur, Punjab, India.
- Smith, G. Albert, Rosedene, 7 Melville Road, Hove, Sussex.
- *Smith, Harrison Bowne, Jr., c/o The George Washington Life Insurance Co., Charleston, W. Va., U.S.A.
- *Smith, Marion, 80-11th Street, N.E., Atlanta, Georgia, U.S.A.
- *Smith, Mrs Newton, 33 Dale View Crescent, London, E. 4.
- *Smith, The Lady Sybil, Mitford House, Lennox Gardens, London, S.W. 1.
- Smith, Rev. William J., The Manse, Bo'ness, West Lothian.
- *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 Creseent Road, Beckenham, Kent.
- *Sowrey, Wing-Commander J., R.A.F., Yeoveney, Staines, Middlesex.
- *Spears, John, L.R.C.S., 60 Prinees Road, London, S.W. 19.
- 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., 29A Clumber Street, Nottingham
- *Stansfield, C. E., 70 Northeourt Avenue, Reading, Berks.
- *Steane, G. A., 5 Queen Victoria Road, Coventry.
- Stephens, Mrs, 41 Harrington Gardens, London, S.W. 7.
- Stephens, W. F., Mahé, Seychelles, Indian Ocean.
- *Sterling, Miss F. M., Home Wood, Hartfield, Sussex.
- *Stevens, Rev. W. H., Allendale, Kellet Road, Carnforth, Lanes.
- Stevenson, A. Creery, Bervie, Wood Lane, Fleet, Hants.
- Stewart, Miss M.A., Queen Anne's Mansions, London, S.W. 1.
- Stoehr, Miss, Alexandra Club, Cape Town, S. Africa.
- *Story, Mrs, 33E Hogarth Road, London, S.W. 5.
- Stout, Professor George F., Craigard, St Andrews.
- *Strachey, Mrs J. St Loe, 39 St Leonard's Terrace, London, S.W. 3.
- *Strange, T. A., Pearcroft Road, Stonehouse, Glos.
- *Stratton, Professor F. J. M., Gonville and Caius College, Cambridge.
- *Strawson, A. H., 27 Norfolk Road, London, N.W. 8.
- *Strutt, Rear-Admiral the Hon. A. C., R.N., 65 Cadogan Gardens, London, S.W. 3.
- *Strutt, Hon. Mrs A. C., 65 Cadogan Gardens, London, S.W. 3.
- *Strutt, Hon. Charles R., Terling Place, Chelmsford, Essex.
- *Sturt, H. H., e/o Asiatic Petroleum Co., Singapore, Straits Settlements.
- Swainson, Miss F. J.
- *Swainson, Miss M., Forty Five, Manor Way, Ruislip, Middx.
- Swinburne, Mrs, 22 Queen's Gate Gardens, London, S.W. 7.
- *Sydenham, F., F.R.C.S., M.D., 379 Harborne Road, Edgbaston, Birmingham.
- *Taylor, Miss E. W., Forty Five, Manor Way, Ruislip, Middx.
- *Telling, W. H. Maxwell, M.D., 29 Park Square, Leeds.
- *Temple, Mrs, Meadows Farm, Barnham, Sussex.
- *Tennant, Mrs B. V., Hams Plot, Beaminster, Dorset.
- *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., Mynydd Ednyfed, Criccieth, N. Wales.
- Thomas, Ernest Seymour, Pitt Rivers Museum, Oxford.
- *Thomas, Gilbert M., 6 Priory Road, London, W. 4.
- *Thomas, John F., 4375 Buena Vista, West Detroit, Michigan, U.S.A.
- *Thompson, Dr R. B., Fellside, Brixham, S. Devon.
- *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.

- *Thurn and Taxis, H.I.H. The Prince Alexander of, Loucen, Nimburg, Czecho-Slovakia.
 Thurston, Rev. Herbert, S.J., 114 Mount Street, London, W. 1.
 Tinnevely, Rt. Rev. The Lord Bishop of, Bishopstowe, Palamcottah, S. India.
 Tipping, Miss K., 7 Lansdowne Circus, Leamington.
 Tottenham, Miss Mary T. A., Ballycurry, Ashford, Co. Wicklow, I.F.S.
 *Traprain, The Viscountess, Whittingehame House, Whittingehame, Haddington.
 *Tretthewy, A. W., Artillery Mansions, London, S.W. 1.
 *Troubridge, Una, Lady, The Black Boy, Rye, Sussex.
 *Tuckey, Mrs C. Lloyd, Ingarsby, Silverdale Road, Eastbourne, Sussex.
 Tuson, K. H., Lieut., R.E., G.H.Q., British Troops, Cairo, Egypt.
 *Tyrrell, G. N. M., 6 Carlton Hill, London, N.W. 8.
 *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.
 *Vatcher, Mrs, Lancaster Gate Hotel, London, W. 2.
 *Vaughan, E. L., 8 Arlington Road, Eastbourne.
 *Vincent, Miss G. H. M. M., Carisbrooke Road, Leicester.
 *Voss, Dr Vivian, Transvaal University College, Pretoria, S. Africa.
 Vyvyan, Mrs T. C., Poldhu, Richmond, Natal, South Africa.
 *Wagstaff, Mrs, 19 Princess Court, London, W. 1.
 *Wales, Hubert, Homewood Heath, Hindhead, Surrey.
 *Walker, Mrs J. F., 26 St. Andrew's Mansions, London, W. 1.
 Walker, Miss May C., c/o National Provincial Bank, Piccadilly, London, W. 1.
 *Walker, Miss Nea, 13 York Road, Edgbaston, Birmingham.
 Walsh, Correa M., Belleport, Long Island, N.Y., U.S.A.
 Wanderley, F. M., Corumba, Matto Grosso, Brazil.
 Wang, C. Y., 63 Szeming Street, S.D.A., Hankow, China.
 Ward, Hon. Kathleen, Moorings, Menai Bridge, N. Wales.
 *Warner, Hon. Mrs W. W., 4 Whitehall Court, 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.
 Watson, Lady Milne-, 73 Eccleston Square, London, S.W. 1.
 *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., 20 St Loo Mansions, London, S.W. 3.
 *West, M., Cornerways, Baughurst, nr Basingstoke, Hants.
 Whitaker, Joseph J. S., Villa Malfitano, Palermo, Sicily.
 *Whitehead, Miss Mercia D.
 Wigan, W. L., Clare Cottage, East Malling, Maidstone, Kent.
 Wilkins, Mrs, 12 Braemar Avenue, Thornton Heath, Surrey.
 Wilkins, C. F., Three Trees, Hillingdon, Middx.

- *Wilkins, Rev. H. J., D.D., Redland Green, Bristol, Glos.
- *Wilkinson, Mrs C., c/o Mrs E. S. Dumsday, 9 Pine Close, Pinelands, C.P., S. Africa.
Williamson, Mrs R. W.
- *Willock, Mrs C. J., Lampool, nr Uckfield, Sussex.
- *Wilson, Mrs C. Stuart, c/o Barclays Bank, 120 Broadway, New York, U.S.A.
Wilson, Percy.
- *Wilson, S. R. W., Lodge Farm, Wigginton, nr Tring, Herts.
- *Wilson, W. R., Yamachiche, Que., Canada.
- *Wilson-Wright, L. A., Meer Hill, Loxley, Warwick.
- *Winby, Lieut.-Col. L. P., 11 Trevor Square, London, S.W. 7.
- *Winchilea and Nottingham, Edith, Countess of, Dower House, Ewerby, Sleaford, Lincs.
- *Winterstein, Dr Baron Alfred von, Wattmanngasse 38, Vienna xiii, Austria.
- *Wodehouse, The Lady Isabel, Old Mill House, Milford-on-Sea, Hants.
- *Wood, Christopher, 28 Portman Court, London, W. 1.
- *Wood, T. Eugène, Redcliffe, 14 Chine Crescent Road, Bournemouth.
Woodhull, Miss Zula M., Norton Park, Bredon's Norton, nr Tewkesbury, Glos.
Woods, Miss Alice, St Ives, Radlett, Herts.
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.
- *Yerburgh, Rev. W. H. B., The Rectory, Bredon, Tewkesbury, Glos.
Younghusband, Sir Francis, Currant Hill, Westerham, Kent.
- *Zachystal, Dr Dominik, Havl. nám 6, Prague, Czecho-Slovakia.
- *Zeigler, Major C. H., Springfield, Breinton, Hereford.

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